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ABSTRACT

Concerns about negative impacts of student phone use have led to calls around the world for tighter restrictions on phones in schools. This paper evaluates the impact of a 2023 policy that banned non-pedagogical uses of phones within schools in Rio de Janeiro, Brazil. To isolate the causal effects of the policy, we contrast middle schools that already had strict rules on phone use prior to the policy ("control schools") to similar schools that did not have strict rules ("treatment schools"), before and after the ban. While restrictions were imperfectly implemented both before and after the ban, we show that in-school phone use fell substantially in treatment schools relative to control. We then show that test scores, which were trending similarly in the two groups prior to the ban, improved by 0.06 s.d. in treatment schools relative to control.

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1 Introduction

In the nearly twenty years since modern smartphones were first introduced, they have profoundly reshaped the way young people use time, consume information and entertainment, and connect with others. These changes have been met with a chorus of concerns about potential harms and calls for policy change (Capraro et al., 2025; Haidt, 2024).

Among the policies that have been most widely adopted in response are restrictions on phone use in schools. Scholars have argued that these restrictions can impact attention, learning, discipline, mental health, and social interactions (Montag and Elhai, 2023; Böttger and Zierer, 2024). Others have disputed these claims, emphasizing the lack of robust causal evidence and highlighting potential benefits of phone use (Odgers, 2024; Vuorre and Przybylski, 2023; Miller et al., 2023). While the debate among scientists remains contentious, policy has moved rapidly (Wikipedia, 2025; D'Addio, 2025).

Robustly estimating the causal impact of phone bans in K–12 schools is hard. Large-scale randomized experiments that exogenously vary policies at the school level have so far proven infeasible. Correlational studies that simply compare outcomes at a point in time across schools with different policies can be severely confounded by unobserved characteristics of schools or students that are correlated with the policies. Quasi-experimental studies looking at changes over time around significant phone policy changes are more compelling, but this body of work, which we review in more detail below, remains small.

In this paper, we present new evidence on the learning impacts of one of the largest city-level phone bans implemented to date, in the municipality of Rio de Janeiro, Brazil – the largest municipal school district in Latin America, with more than 1,000 schools. In August 2023, Rio was the first municipality in the country to outlaw phone use in schools, first for non-pedagogical uses within classrooms, and later (in February 2024) school-wide. While all schools in Rio were subject to the ban, its impact on usage differed substantially across schools depending on whether or not they already had a strict phone policy in place.

Our main analysis exploits this variation in a differences-in-differences (DiD) framework that contrasts changes in standardized test scores between schools that did not yet have strict rules prior to the policy change and similar schools that did. We refer to the former throughout as the *treatment* group (since they were exposed to a larger policy change) and the latter as the *control* group. We combine administrative data from the Rio school district with a new survey of school principals that provides information on pre-ban policies and phone use, as well as a set of detailed qualitative interviews. We focus on middle school students (grades 6-9), as our test score data for high school students is limited and usage of phones in elementary grades remains minimal.

We begin by characterizing the impact of the ban on phone use as reported in our principal survey. Compliance with stated phone policies was imperfect both before and after the ban. Before the ban, 40% of control schools reported that most or all students were on their phones during class across grades 6 to 9, compared with 52% of treatment schools. These shares were higher in older grades (44%, 67%, and 69% in grades 7, 8, and 9 for treatment schools; 37%, 47%, and 47% for control schools). In the post-period, these shares fall substantially in both groups—to 15% in control schools and 19% in treatment schools—closing about 67% of the baseline gap between them.

Our main result shows that an index of math and Portuguese standardized test scores improved by 0.06 standard deviations in treatment schools relative to control schools after the school-wide ban.[1] Effects are positive for both math and Portuguese, with somewhat larger and more precisely estimated effects in math. Because phone use also declined in control schools, these reduced-form estimates may understate the total gains associated with stricter restrictions. A back-of-the-envelope calibration based on the estimated first-stage change in phone use suggests that, in schools where the ban reduced phone use, the total gains may have been on the order of 0.2 standard deviations relative to a no-ban counterfactual.[2]

Several pieces of evidence support the validity of our research design. First, we leverage two years of quarterly standardized test scores prior to the policy change to show that learning outcomes were not evolving differentially over time in treatment and control schools. Event-study estimates are flat and statistically indistinguishable from zero before the ban, and then rise only after the policy is introduced, with larger effects after the school-wide phase. Second, because baseline phone rules were not randomly assigned, we show our results are robust to using matching techniques to restrict attention to a set of treatment and control schools that were statistically identical in observable characteristics over the baseline period. Third, we show that our estimates are robust to rich controls that allow time-varying effects of baseline characteristics including average academic achievement, student-teacher ratios, attendance, student composition, and student sociodemographic characteristics. Fourth, results remain stable when we use alternative definitions of treatment based on schools' self-reported phone use. Finally, we document that selective test-score attrition does not change differentially across groups after the policy, reducing concerns that composition effects drive the results.

[1]This is equivalent to roughly one fifth of the typical annual progression in these test scores.

[2]For comparison, other well-known education interventions produce effects in a similar order of magnitude: experimental evidence from Project STAR shows that reducing class size increased early-grade test scores by about 0.2 standard deviations (Krueger, 1999), while Chetty, Friedman and Rockoff (2014) find that a one-standard-deviation increase in teacher value added raises student test scores by around 0.10-0.14 standard deviations.

Nevertheless, we emphasize several important caveats in interpreting our results. Our design does not identify the effect of a phone ban relative to no ban. Instead, it identifies a relative contrast in the effects of the municipal ban across schools with different baseline exposure to the policy. While evidence supports parallel trends, treatment and control schools do differ in the levels of observable characteristics at baseline. Some concurrent policies could in principle have had heterogeneous effects across schools.[3] Moreover, mapping the relative changes we observe to treatment effects of the ban generally requires assuming homogeneity of treatment effects across groups (Chaisemartin and D'Haultfeuille, 2017). Our treatment-on-complier estimates could be over-stated if treatment effects are larger in treatment schools than in control schools, or under-stated in the reverse scenario. Also, estimates are less precise when inference allows for arbitrary serial correlation within schools. A final limitation is that our evidence on changes in phone use comes from principals' retrospective categorical reports, which are coarse, noisy, and potentially affected by social-desirability bias. We therefore use these measures primarily to validate differential treatment intensity and to characterize mechanisms, rather than as a precise measure of first-stage dosage.

Our paper contributes to a small but growing quasi-experimental literature on phone restrictions in K–12 schools, which has yielded mixed results.[4] Contrasting changes in test scores across schools in regions that banned phones to those in regions that did not, Beland and Murphy (2016) document a 0.07 s.d. gain in high-stakes exams, concentrated among low-achieving students in England; Abrahamsson (2026) finds that local phone bans reduced bullying and improved grades and mental health for girls in Norway; Beneito and Vicente-Chirivella (2022) find a positive effect of about 0.10 s.d. in PISA scores and reductions in bullying incidence in Spain; Kessel, Hardardottir and Tyrefors (2020) find no impacts in Sweden, rejecting even small improvements.[5] A recent paper using nationwide U.S. data finds that lockable phone pouches reduced phone use but had mixed academic effects, with

[3] As a leading example, standardized tests became more demanding in 2024, as the municipality aligned them to the national common core curricular standards. While performance temporarily dropped for all schools after that, the change applied to all schools just the same (if anything, it should have hurt treatment schools more, as their students had lower baseline proficiency than those in control schools).

[4] A large body of correlational studies also finds mixed patterns for the relationship between phone use and learning outcomes. Synthesizing this literature, Campbell et al. (2024) reviews 22 studies and concludes that the evidence is inconclusive on whether bans improve academic outcomes or wellbeing, or reduce cyberbullying. A meta-analysis of five studies by Böttger and Zierer (2024) reports small positive effects of bans, largely attributable to reductions in bullying rather than gains in achievement. At the policy level, Goodyear et al. (2025) compares schools with permissive and restrictive rules and finds no differences in wellbeing, mental health, or academic outcomes, even though higher individual phone use strongly predicts worse outcomes.

[5] Beneito and Vicente-Chirivella (2022) use a slightly different identification strategy than the others, contrasting two regions that banned phones with others, using synthetic control and DiD methods. Another related study with a different strategy is Shi and Villarroel (2026), which uses within-school variation induced by teacher discretion over classroom rules in Chile and finds no effects on test scores.

modest gains in high schools, and small negative effects in middle schools (Allcott et al., 2026). Most closely related to our study, Figlio and Özek (2025) also contrast schools with different baseline exposure to a district-wide phone ban in a single district in Florida, finding increases in disciplinary incidents and improvements in test scores.

Our study differs from this literature in several important dimensions. First, it extends evidence on school phone bans beyond high-income settings to a large middle-income country, where school infrastructure, enforcement capacity, and baseline phone use patterns may differ in ways that matter for policy effectiveness. Second, we leverage quarterly student-level test-score data over a two-year pre-period—substantially higher frequency than the data available in most prior studies—which allows us to rigorously assess pre-trends and trace the dynamics of treatment effects across the classroom and school-wide phases of the ban. Third, our design helps address a central challenge in much of this literature: the timing and content of phone restrictions chosen independently by individual schools may be correlated with broader changes in school leadership, discipline, or pedagogy. We instead exploit variation in schools' baseline phone rules around a centralized municipal ban that applied to all schools at once, rather than relying on individual schools' endogenous decisions to adopt restrictions.

Our findings also connect to a broader literature on the cognitive and behavioral effects of digital-device use beyond K–12. Experimental and quasi-experimental studies with older students and adults show that phones can impair attention and performance even when not actively used, and that reducing access to phones or social media can improve outcomes in some settings (Ward et al., 2017; Allcott et al., 2020; Allcott, Gentzkow and Song, 2022; Braghieri, Levy and Makarin, 2022; Bursztyn et al., 2025; Allcott et al., Forthcoming). Particularly related is a recent randomized study showing that removing phones from college classrooms in India improves academic performance, especially among students with lower baseline achievement (Sungu, Choudhury and Bjerre-Nielsen, 2025).

2 Background

Rio's school district is the municipal public school system operated by the Secretaria Municipal de Educação (SME) of the City of Rio de Janeiro. It serves the municipality of Rio de Janeiro, not including the broader metropolitan region, and is the city government's main provider of public early-childhood, elementary and middle education. As of the 2025 school year, the network enrolled more than 650,000 students across 1,557 schools, making it the largest municipal school system in Latin America. The system is governed centrally by the SME and implemented through regional coordinating offices (Coordenadorias Regionais de

Educação, CREs), which oversee school operations and pedagogy across the city.

The Rio school district implemented its school phone ban in two phases. First, in August 2023, it banned phone use inside the classroom – except for educational purposes, under teachers' guidance – but did not regulate usage during breaks or in other school environments (e.g., libraries, computer labs or sports facilities). [6] Before then, there was no municipality-wide decree in force imposing a common rule on student phone use; accordingly, any limits would have arisen through ordinary school management and classroom discipline, leaving principals and teachers room to adopt stricter or looser norms. After roughly four months under this classroom-only regime, the district launched a public consultation in December 2023 on extending the restriction to the full school day. Following more than 10,000 submissions, the large majority of them favorable, the city adopted a second decree on February 1, 2024. This made the ban *school-wide*, extending it to any phone use during school hours (once again, except for teacher-guided activities). To this day, students are still allowed to use devices before the school day begins and after it ends. At all other times, devices must remain stored (in dedicated container boxes, where available, or in students' backpacks), turned off or silent, with violations subject to warnings or confiscation by teachers or school staff. The policy was not anticipated; in fact, it preceded a similar national law by 1.5 years, and later inspired its enactment.

We note several other policy changes that occurred around the time of the municipal phone ban.[7] First, in 2024, Rio changed the structure of their standardized math and Portuguese tests. Before the COVID-19 pandemic, Brazil's Ministry of Education planned to align national standardized assessments with the common core. Because of the pandemic, adoption of common-core-aligned assessments was postponed; instead, most school districts resorted to simplified standards to track minimum proficiency. In 2024, a few school districts finally started to implement standardized assessments aligned with the common core; Rio was among them. This shift made exams more demanding – covering higher-order skills and competencies – which led to a sharp decline in test scores across *all* municipal schools (see Figure A1 and Figure A2). Second, Rio made a number of changes to its staffing policy, including updated rules for performance pay implemented in mid-2023, and hiring of additional teachers for underserved schools in mid-2024. These changes are unlikely to confound our estimates because uniform policy changes that affected all schools equally are netted out by design, and our results are robust to allowing different baseline characteristics to have differential effects after the ban. We discuss this further in Section 6.4 below.

[6] Additional exceptions include medical or accessibility needs, emergency protocols, and operational contingencies. See Decreto 53,019/2023 and Decreto 53,918/2024 for details.

[7] These changes are detailed in Appendix F.

3 Data

3.1 Administrative data

The main data source for analysis is administrative data from the Rio school district. These include data on student enrollment, student demographic characteristics, and quarterly test scores in both math and Portuguese. The test scores are based on assessments designed to match the instructional content of each school quarter, with items generated and graded by out-of-state teachers. As such, these assessments are different from both school exams (not standardized and graded by the teachers themselves) and from national standardized tests (conducted every two years, only with terminal grades of elementary, middle and high school, and focused on the curricular content of all previous grades).

We estimate impacts on math and Portuguese standardized test scores; Figure A1 and Figure A2 in the Appendix compile average raw test scores by subject and grade level over time. We normalize raw scores to z-scores separately for each grade, always relative to control group test scores in Q1/2022 (the first quarter for which we have data).[8] Lastly, to prevent inflated p-values due to family-wise error rates from multiple comparisons, we compute a summary measure that averages across math and Portuguese z-scores (Kling, Liebman and Katz, 2007).

The data also identifies the school in which each student was enrolled in each quarter, along with demographic characteristics such as date of birth, parental education, gender, participation in the conditional cash transfer program Bolsa Família, and race.

Additionally, we build school-level covariates for our matching procedure using data from the 2022 and 2023 school censuses (annual administrative data reported by the universe of K–12 schools in the country). Our data is primarily from 2022; we use 2023 data only for the 0.25% of schools that started operating since the previous census. We compute an infrastructure score based on the presence of key facilities (such as labs, libraries, and sports courts). We capture student demographics by computing the proportion of underrepresented minority students (black, brown and indigenous). These data also include a school management complexity index (ICG).[9]

[8] To prevent the change in reference matrices in 2024 from visually confounding time trends in learning outcomes, our normalization also adds a *constant* to each student Q1/2024 math and Portuguese test scores – such that average test scores by subject at the time match those predicted by a linear interpolation between Q4/2023 and Q2/2024. Doing so is irrelevant for treatment effect estimates (since it leaves group differences unchanged), but avoids a mechanical drop in test scores in Q1/2024 imposed by the Item Response Theory (IRT) scoring model.

[9] The ICG is a composite index calculated by the government, based on how complex it is to manage a school based on factors like enrollment size, number of shifts, and the range of education levels and programs it offers.

Our primary sample is students who were enrolled in middle-school (grades 6–9) at some point between 2022 and 2024, excluding students who changed schools during this period.[10] This encompasses 196,757 students across 325 municipal schools, yielding 1,116,689 student-quarter observations. In turn, the matched sample encompasses 97,380 students across 154 schools, for a total of 549,969 observations. In both samples, longitudinal data are unbalanced panels; i.e., we do not observe every student in all school quarters, due to a variety of reasons (from graduation to migration to dropouts or deaths). We assess the robustness of our findings to restricting the analyses to the subsample of students for whom we have a balanced panel.

3.2 Survey data

In May 2025, we collaborated with the Rio school district to survey school principals on the existence and nature of pre-ban rules regulating phone use, as well as on the perceived impacts of the municipal ban.

The survey was distributed directly by the district administration to principals in all elementary and middle schools between April 29 and May 20, 2025, excluding special education and pre-kindergarten schools. There were 897 responses, corresponding to a response rate of 89%. Among middle schools (i.e. those offering grades 6 to 9), the focus of our paper, there were 327 responses (90% response rate).

It comprised 15 questions about student reactions to the phone ban, disciplinary measures, enforcement challenges, perceived impacts on student behavior, and perceived classroom phone use before and after the ban (full script in Appendix G). Crucially, it collected information on each school's phone rules prior to the municipal ban, which we use for treatment assignment. Specifically, the survey asked: "Before the official municipal ban in 2024, did your school already have rules about phone use?." Principals had to choose one of the following alternatives: (a) "Throughout the entire school day (students were prohibited from using phones throughout the school day, except for educational purposes in the classroom)"; (b) "Restriction during classes (students could only use phones during recess or lunchtime)"; or (c) "No general restriction (each teacher and staff member decided when students could use phones)." Schools featuring strict rules before the ban, wherein students were forbidden to use phones throughout the day, were assigned to the control group, and those featuring partial rules (pre-ban rules only enforced in the classroom) or no specific rules, to the treatment group. We explore alternative treatment definitions in robustness exercises.

The survey also collected data on perceived phone use, which we use in our first-stage

[10] Around 1.5% of students in our sample changed schools. In robustness tests, we verify that the probability of changing schools is not affected by the treatment.

analysis. Specifically, principals were asked: "Before the 2024 ban, how many students used phones for personal purposes during class (e.g., messaging, social media)? Indicate your perception for each grade (*none*, *few*, *some*, *most*, or *all*)."

4 Empirical strategy

We estimate a differences-in-differences model, contrasting changes in standardized test scores across schools that had no or only partial phone restrictions prior to the municipal ban (the *treatment* group) to those with strict rules regulating in-school phone use even before the municipal policy (the *control* group), before and after the ban. The rationale for this identification strategy is that the centralized ban had much larger scope to affect students' phone use in schools where strict limits were not already in place. As we show below in Section 6, this is consistent with survey data, based on principals' categorical reports about school-wide pre-ban usage.

After defining the treatment and control groups based on the pre ban rules, we estimate Ordinary Least Squares (OLS) regressions as follows:

$$Y_{igst} = \beta_1 \big(\text{Treat}_s \times \text{Post}_t^{\text{classroom}}\big) + \beta_2 \big(\text{Treat}_s \times \text{Post}_t^{\text{school}}\big) + \delta_t + \theta_{gs} + \lambda X_{igst} + \varepsilon_{igst}, \quad (1)$$

where Y_{igst} is the standardized test score of student i enrolled in grade g in school s at quarter t; $Treat_s = 1$ if school s had only partial or no phone rules prior to the municipal ban, and 0 otherwise; $\text{Post}_t^{\text{classroom}} = 1$ for Q3-Q4/2023, and 0 otherwise; $\text{Post}_t^{\text{school}} = 1$ from Q1/2024 onwards, and 0 otherwise; δ_t are quarter fixed effects; θ_{gs}, grade $\times$ school fixed effects; X_{igst} are controls (including school baseline characteristics $\times$ Post); and ε_{igst} is an error term. We are interested in β_1, β_2, which capture the differential effects of the ban in treatment schools relative to control schools. We cluster standard errors at the grade $\times$ school level, allowing for arbitrary serial correlation within each school and grade.

We also estimate an event study specification, whereby we allow treatment and control outcomes to vary differentially for all quarters, relative to Q2/2023:

$$Y_{igst} = \sum_{\substack{p=1 \\ p\neq 6}}^{12} \gamma_p \left(\text{Treat}_s \times \mathbf{1}\{t = p\}\right) + \delta_t + \theta_{gs} + \lambda X_{igst} + \varepsilon_{igst}, \quad (2)$$

The main assumption for this estimator to identify the causal reduced-form effects of the policy is that, in the absence of the ban, counterfactual outcomes in treatment and control schools would have followed parallel trends; i.e., pre-existing differences in test scores across schools that already had strict phone rules prior to the ban and those that did not would have otherwise remained identical. This also implies the absence of other contemporaneous shocks differentially correlated with prior strictness.

Conditional on these assumptions, we estimate the reduced-form impacts of the school phone ban on learning outcomes. Because phone use also decreased in control schools after the ban, our estimates likely reflect a lower bound for the causal effects of this policy on learning. We discuss implications for the interpretation of our findings in Section 7.

Besides assessing dynamic treatment effects, the event-study specification also allows us to test whether learning outcomes were evolving differentially across treatment and control schools before the ban. While the parallel trends assumption cannot be directly tested, this analysis provides corroborating evidence for it over the two years before the ban for which we have test score data. We also assess the sensitivity of our findings to alternative treatment definitions, restricting attention to schools with no pre-ban rules rather than pooling those with partial restrictions.

To further address the possibility of unobserved confounds, we implement a matching estimator. As we show in Section 6.1 below, baseline phone rules correlate with several school characteristics, from infrastructure, to student profiles to administrative complexity. While differences in levels do not invalidate the identification assumption, we assess the robustness of our findings by applying matching techniques that restrict comparisons to treatment and control schools that are similar in observable characteristics in the baseline period. Matching prior to treatment enforces common support and strengthens the credibility of the parallel trends assumption. Because treatment is assigned at the school level, we match observations at the school level rather than at the student level (Pimentel et al., 2018; Cohodes, Eren and Ozturk, 2026).

Concretely, we match schools using administrative data on covariates from the 2022 school census (*Censo Escolar*): infrastructure (library, labs, sports court, etc.), student race shares, Bolsa Família coverage, region (*subprefeitura*) and the school management complexity index (ICG). We estimate propensity scores through a logistic regression. Schools are paired via optimal matching on the logit distance (Ho et al., 2011; Hansen and Klopfer, 2006), with exact matching on ICG and region, ensuring no schools are paired across administrative lines. For 99.75% of schools, matching relies exclusively on 2022 data; as such, event study estimates for Q1-Q2/2023 (prior to the classroom ban) provide a rigorous test of differential pre-trends outside of the procedure.

Since treatment assignment is not random, however, it could still be the case that baseline differences in phone rules conflate other differences in baseline characteristics or policies, if concurrent policy changes had differential effects for treatment and control schools after the municipal ban that vary by those characteristics. To address this concern, our main specification controls for interactions that allow for differential trends after the ban based on baseline school characteristics – average test scores and student-teacher ratio. We also run additional robustness tests that assess the sensitivity of our estimates to these and alternative controls.

5 Qualitative evidence on school rules and phone use

We start with a qualitative account that contrasts school rules and phone use across the treatment and control groups to shed light on what the categories reported by principals actually mean. In December 2025, we interviewed five school principals (two from schools that had strict rules prior to the municipal ban, two from schools that had partial rules, and one from a school with no rules) to better understand their answers to the quantitative survey a few months prior.

First, we use these interviews to characterize what rules and usage were like before the ban. Schools that reported to be strict even before the ban did not allow students to use phones in the classroom except for strictly pedagogical uses under teachers' supervision. Isabel, a principal at one of these schools, stated that "students were *never* allowed to have free access to their phones. They were advised not to leave it on the desk but, rather, to keep it in their backpacks." Nivania, a principal at another strict school, said "students could not use their phones at all (...) teachers were discouraged to resort to them even for pedagogical uses because it was hard to enforce that students were only using them for their intended purpose." In contrast, schools with partial rules had much weaker policies. Debora, a principal at one such school, said that students could "use it during breaks, e.g., to play online." Lidia, a principal at another school with partial rules, said rules existed, "but were not as strict." Marize, a principal at a school with no rules restricting phone use prior to the ban, said that "teachers were free to restrict phone use or not" back then.

These differences in rules were reflected in differences in phone use before the municipal ban. Usage was lower in control schools, although not nil. In the quantitative survey, both Isabel and Nivania reported that only a few 6th and 7th graders used their phones for personal use (messaging, social media) before the municipal ban. Among older students, however, enforcing restrictions was hard even for strict schools: both said that most 8th and 9th graders were on their phones for personal use, similar to schools with partial or no rules. While that might seem to suggest that rules were completely ineffective, the qualitative interviews revealed that the intensive margin was responsive.

Nivania stated that "the great majority of students were not at all on their phones during class time", and Isabel stated that "students spent at most 10% of a typical math class on their phones." The contrast to schools with partial or no rules is clear. Lidia said the school had problems with students "recording teachers and sharing footage out of context on social media." Debora, also at a school with partial rules, when describing 8th graders (most of whom used their phone for messaging or social media in that school), pointed out that 30-40% of them were on their phone during class "100% of the time." Marize, a principal at a school with no rules prior to the ban, stated that figure was 60-80%, and that "students had their phones constantly, some on their desks, some in their hands (...) even those without the phones visible often would wear headphones during class."

However, enforcing restrictions unilaterally was costly, and usage remained high after the ban. Isabel said that "except for some 5% of students who did not bring phones to school, all others wanted to be connected all the time (...) Many kept their phones in their pockets rather than their backpacks. They wanted to be on them constantly, and would even ask to go to the restroom to access their phones." She stated that the ban helped because "students and parents no longer associated the rules with an 'annoying' principal." Debora and Lidia, principals at schools with previous partial rules, said that, after the ban, the school adopted container boxes to better enforce restrictions, which helped further decrease usage. Marize said that things got better after the law because "it was difficult for teachers when there were no rules (...) since students tried to circumvent teacher-specific restrictions and it was only up to them to enforce those." These reports help paint the picture that implementation remained highly imperfect after the ban, but changes were bigger in treatment schools than in control schools.

Similar accounts of implementation issues were observed following the nationwide ban in Brazil, enacted about a year after the Rio school-wide ban.[11] Data from nationally representative surveys following the January 2025 federal ban document students' struggles with managing screen use, multiple implementation challenges, as well as the perceived impacts in its aftermath.[12] Despite frictions, respondents widely reported declines in phone

[11] Law No. 15,100/2025, published in the *Diário Oficial da União* on January 13, 2025, prohibits the use of smartphones by students in public and private schools during class time, recess, and other school activities. Additionally, it sought to harmonize rules that previously varied across states and municipalities, such as those already implemented in Rio de Janeiro. Schools retain discretion over implementation details – i.e., whether devices must remain stored in dedicated spaces or whether they can remain in students' possession – but are required to enforce restrictions consistently throughout the school day.

[12] Two out of three secondary students in the country found it challenging to reduce their screen time (Congresso em Foco, 2025*b*). That figure aligns with the 45% of Brazilian 15-year-olds who reported to be distracted by screens in most math classes (OECD, 2024) – one of the highest shares among the countries assessed in the 2022 PISA and a key justification for the national policy. Even though most teachers and school staff were aware of the ban only a few months after it was enacted, one in three students still were completely oblivious even 5 months into it (Congresso em Foco, 2025*a*). Most students in the country kept their phones in their backpacks or pockets, raising the challenge of self-managing compliance with the ban;

use and improvements in classroom attention – eight out of ten students perceived positive impacts on learning outcomes (Congresso em Foco, 2025*b*). Taken together, these findings on the federal-level implementation highlight possible mechanisms and enforcement challenges that help contextualize our evidence from Rio.

6 Results

6.1 Descriptive statistics and covariate balance

We start by documenting schools' and students' baseline characteristics, differences between treatment and control schools, and the extent to which our matching procedure effectively approximates the two groups when it comes to observable characteristics.

Schools with strict rules even before the ban tend to be more densely located in more well-off neighborhoods (the distribution of schools within the city is shown in Figure A3) and to display higher baseline average test scores (Figure 1). Table B1 further documents that, at baseline, treatment schools featured a larger share of poor and non-white students, and were significantly smaller (based on grade size) than control schools. While the former's students had significantly lower baseline test scores – a 0.1 s.d. difference in z-scores, relative to the control group – most importantly, there were no significant differences in *changes* in test scores over the course of the school year, consistent with the absence of differential pre-trends.

Figure B1 and Panel B in Table B1 document that matching reduces standardized differences in means for all matched covariates, which become nearly zero post-matching, even for covariates not included in the matching algorithm such as parental education and class size. If average test scores remain lower for the treatment group, relative to control schools, the difference is much smaller than in the full sample, and changes in test scores over the course of the school year across the two groups remain small and not statistically significant. Density plots confirm similar distributions for both matched variables (Figure B2) and other unmatched covariates (Figure B3).

6.2 Effects on phone use

Consistent with imperfect enforcement, phone use as reported by the principals in the quantitative survey was high before the ban and remained well above zero after the ban. This is true even in control schools, where 40% of principals reported that most or all students

only about a quarter of schools stored phones on their behalf (Congresso em Foco, 2025*a*).

were on their phones during class across grades 6 to 9 before the ban, and 15% after the ban (see Figure A4 for the overall distribution of responses). In treatment schools, usage was higher both before (52%) and after the ban (19%). Our empirical strategy is based on the hypothesis that initially permissive schools would experience larger post-ban declines in phone use than those that were already strict before the ban.

These patterns are shown in Figure C1 and Figure C2, which break down principals' responses by prevalence category and grade level. Schools reporting no students using phones at baseline were rare even among those with already strict rules before the ban. While the ban increased the prevalence of this category across both groups, it typically remained below 20% (especially for higher grade levels) and it did not systematically vary across groups either before or after the ban. In contrast, differences in the baseline prevalence of high usage were much more notable across groups, especially in grades 8 and 9. Post-ban, the prevalence of this category converged across all schools and grade levels.

We can test this hypothesis by estimating first-stage treatment effects of the municipal phone ban on in-school phone use, through the differences-in-differences strategy, with principals' categorical responses as dependent variable.

In Table 1, columns (1)–(3) investigate treatment effects separately for different prevalence levels using the matched sample: any use (i.e., any category other than 'none'; column 1), some+ use (i.e., *some*, *most* or *all*; column 2), or most+ use (i.e., *most* or *all*; column 3). Treatment effects are concentrated in the latter: while the share of schools reporting that most or all middle-school students used phones decreased for all schools (23.9 p.p., nearly 60% of the baseline control mean), the effect size was 10.3 p.p. larger (significant at the 5% level) for treatment schools – enough to eliminate 2/3 of the baseline differences in the share of high-usage schools across the two groups. Table D1 shows results are similar for the full sample.

[Table 1]

Appendix E conducts robustness tests to measurement error in principals' retrospective reports, particularly in light of known problems with the use of ordinal scales to express underlying latent variables (Bond and Lang, 2019). Results corroborate our claims that phone use was higher in schools without strict rules prior to the municipal ban.

Other questions in the survey help contextualize these first-stage results. First, we document that students were more dissatisfied with the ban in treatment schools than in control schools (12.1 pp difference, $p = 0.033$), validating that the shock was larger in the former. Second, in most schools students continue bringing their phones to school after the ban (72%

report students bringing phones most days or every day in treatment schools, and 62% in control). When students do bring their phones, they usually keep them in their backpacks or pockets (95.1% of treatment schools, 87.3% of control). Finally, 83.4% of principals report implementation difficulties, especially during breaks, with student resistance, lack of parental support, and difficulty monitoring students all mentioned as common challenges. Taken together, these results help explain the finding that usage remains relatively high.

6.3 Effects on learning outcomes

We start by plotting quarterly standardized test scores for control and treatment schools in Figure 1, separately for the full sample (Panel A) and the matched sample (Panel B). In both cases, prior to the municipal ban, there was a gap in test scores between the groups, but this gap remained roughly constant over the two years for which we have data before the municipal ban. With the introduction of the classroom ban, in Q3/2023, this gap began to tighten, and it was greatly reduced after the school-wide ban, from Q1/2024 – more so within the matched sample, for which baseline differences in test scores disappeared after the policy.

[Figure 1]

To formally test the hypothesis that the municipal phone ban improved learning outcomes, Figure 2 displays 95% confidence intervals for event study estimates based on the differences-in-differences strategy, restricting attention to the matched sample and using Q2/2023 as the reference period. Consistent with the raw data, there were no systematic differences in test scores trends across the two groups before the ban. While effect sizes start trending upwards after the classroom ban, no coefficient is statistically significant until the onset of the school-wide ban. Importantly, effect sizes keep trending upwards over the course of the school year, approaching 0.1 s.d. by the end of the 2024 school year.

[Figure 2]

To help benchmark effect sizes, Table D2 in the Appendix estimates the differences-in-differences model with the summary measure z-score as dependent variable, as well as separately for math and Portuguese test scores, for the matched sample. Column (1) shows that the ban improved standardized test scores by 0.061 s.d. (significant at the 5% level) in treatment schools, relative to control. The effect size is very robust to replacing student covariates with student fixed effects (column 2), adding grade × period fixed effects (column 3), or adding sub-school-district (CRE) × Post interactions (column 4).[13] Across the four columns, effect sizes associated with the classroom ban are all small and not statistically significant. Columns (5-6) break down estimates by subject. The school-wide ban improved math test scores by 0.077 s.d. (significant at the 5% level, column 5), and Portuguese test scores by 0.045 s.d. (significant at the 10% level, column 6).

We study distributional impacts in two ways. Table D3 documents that treatment effects are concentrated at the bottom of the pre-ban achievement distribution.[14] Figure 3 then compares the rank distribution of students within the sample, before and after the ban. The left-hand side panel compares treatment and control schools in 2022 (baseline) and 2024 (post-treatment), while the right-hand side panel conducts a placebo test, comparing the same groups in 2022 and 2023 (both before the ban). While mean reversion is apparent in all panels, there is clear evidence that the lower-end of the treatment distribution saw higher gains, relative to control, after the municipal ban. In turn, there is no such difference between the two groups in the placebo year. The patterns are not only informative about who benefits the most from the policy, but also allow us to assess the robustness of our findings to the change in assessment structure introduced in Q1/2024. If we documented an erosion of the initial advantage of high-achievers in control schools (which featured higher average test scores before the ban) relative to those in treatment schools, that would be consistent with treatment effects being driven by the increase in exam difficulty; conversely, documenting catching up of initially low-achievers in treatment schools, relative to those in control schools – despite more challenging assessments – is more consistent with treatment effects being driven by the phone ban.

[Figure 3]

Because our estimates are always relative to control schools, it is unreasonable to compute treatment effects on the treated while ignoring the fact that phone use also fell substantially in the former. As previously discussed, most first-stage impacts of the ban transpired through a larger decrease in the share of treatment schools for which principals reported that *most* or *all* students used phones during class time, relative to control schools. Based on Table 1, the additional decline in most+ use in treatment schools relative to controls was 10.3 percentage points, while control schools experienced a 23.9 percentage point decline over the same period

[13] *Coordenadorias Regionais de Educação* (CREs) are the 10 administrative sub-divisions of the SME/Rio school district.

[14] Because this analysis requires observing student outcomes both before and after the ban, the sample here is restricted to students who were already observed in 2022.

(column 3). Under a constant-slope interpretation, this implies a gain of about 0.14 standard deviations associated with the phone use decline in control schools, which is therefore netted out by the DiD estimator. Applying the same calibration to the total decline implied for treatment schools, 34.2 percentage points, yields an implied total treatment-school effect of about 0.20 standard deviations. Table D4 reports these calibration results for the full and matched samples. These calculations should be interpreted cautiously: they rely on a coarse measure of phone use and on the assumption that the relationship between reductions in phone use and test scores is linear and homogeneous across settings.[15] We therefore view them as illustrative calibrations of magnitude rather than as precise causal estimates. Moreover, qualitative survey evidence suggests that intensive margin effects – which are not entirely reflected in principals' survey responses – would further attenuate these estimates as phone use significantly converged across treatment and control schools.

Lastly, heterogeneity patterns of treatment effects match those for the reduction in phone use – corroborating the causal attribution of learning gains to the school phone ban. First, Table D5 documents that effect sizes are much larger among 7th and 8th graders. The policy had limited room to affect grade 6, where baseline phone use was comparatively low (Figure C1), while among the oldest students enforcement appears to have been more difficult, as reported in the qualitative interviews. Grades 7 and 8 seem to be the margin where exposure was large enough for the ban to matter and compliance responsive enough for it to change behavior. Second, Figure D1 documents that treatment effects were indeed concentrated in schools whose principals reported that phone use decreased after the ban; in contrast, effect sizes are much smaller and not statistically significant where phone use did *not* decrease after the ban.

Such findings corroborate school principals' survey responses on their perceived impacts of the ban. In both treatment and control groups, the three most commonly cited impacts were more face-to-face interaction, fewer interruptions during class, and improved focus (see Figure C3). These reports are in line with the quantitative results we find on test scores.

6.4 Robustness

Figure 4 documents that our findings are robust to alternative treatment definitions. If anything, restricting attention to schools without prior rules as the treatment group leads to even larger effect sizes – even though treatment effects are less precisely estimated due to

[15] These results can be viewed as a Wald-style calibration. A causal interpretation requires assumptions beyond parallel trends, including monotonicity, stability and homogeneity of treatment effects (Chaisemartin and D'Haultfeuille, 2017). We do not require already-strict schools to be unaffected by the ban, nor that phone use falls to zero after the ban.

the small number of schools in that category.

[Figure 4]

When it comes to the concurrent enactment of policies that could differentially affect schools with and without strict pre-ban phone rules, the differences-in-differences estimator nets out common shocks, contrasting differences in learning outcomes over time across schools with different baseline phone use rules. As such, uniform policy changes across schools – such as those in assessment structure or in teacher pay – do not confound estimates of differential effects across schools over time.

Nonetheless, since baseline phone rules were not randomly assigned, it could still be that some baseline school or student characteristics respond differentially to those uniform policy changes. In particular, if revised reference matrices made assessments more difficult in 2024, these could have affected treatment and control schools differentially – precisely because control schools had higher baseline test scores. Nevertheless, the patterns in Table D3 and Figure 3 are inconsistent with the claim that treatment effects are driven by top control students losing ground to treatment students in face of harder assessments.

More broadly, other concurrent policy changes that are not uniform across schools (such as new teacher hires targeted at those with the highest student-teacher ratios) could confound our treatment effect estimates. To deal with these concerns, we allow baseline school characteristics to influence test scores differentially after the ban, controlling for interactions of the school-wide ban indicator with school baseline average test scores and student-teacher ratio. We assess the robustness of our findings to the inclusion of these and other controls. Table D6 assesses the sensitivity of our results to the inclusion of the interaction of the school-wide ban with baseline average test scores (column 2), baseline student-teacher ratio (column 3), and baseline average student attendance (column 4). Our treatment effects estimates are extremely robust across all columns. Table D7 replicates these robustness tests for the full sample. Once again, all estimates are extremely stable across columns. Similarly, Table D8 and Table D9 document that our estimates are robust to allowing baseline student characteristics to matter differentially after the ban.

Since schools that had different baseline phone rules also differed in a range of observable characteristics, our matching strategy is also an important ingredient of credibly isolating the treatment effects of phone bans from those of other variables. Appendix B documents that the matching procedure successfully eliminates differences in the covariates we can measure over the baseline period. We further document that our findings are robust to alternative matching specifications. In Supplementary Materials, Table D10 replicates our main results with three alternative matching strategies (which yield slightly different control samples), as well as using inverse probability weighting with the full sample. Treatment effects remain large and precisely estimated across all columns. While effect sizes of the school-wide ban for the full sample are somewhat smaller, they remain statistically significant and quantitatively relevant; meanwhile, alternative specifications yield statistically significant effects also for the classroom ban (Figure D2 and Table D11).

Our baseline specification clusters standard errors at the school-grade level because the outcome data come from grade-specific exams and implementation intensity varies substantially across grades within the same school. In Figure D3, we report alternative specifications that cluster at the school level and at the school-cohort level (keeping students in a fixed cluster as they progress across grades). Results are broadly similar under school-cohort clustering, but standard errors are larger under school clustering. The event-study coefficient for the first period after the school-wide ban remains statistically significant, but the coefficients in other periods are no longer significant at conventional levels due to larger variance in the later periods. We view this as an important caveat to our results, as it shows the estimated effects are less precisely estimated when inference allows for arbitrary correlation across grades within schools.

Lastly, to deal with potential selection concerns, we document that our findings are robust to restricting attention to a balanced panel of students (Table D12). We also show that baseline phone rules do not predict changes in availability of standardized test scores data after the municipal phone ban (Table D13): all effect sizes are extremely small, and no coefficient is statistically significant for the summary measure. While all schools have to go through these standardized assessments, these findings alleviate the concern that effects on scores are driven by the ban differentially affecting student absences on exam days or drop outs. We similarly show that the probability of switching schools is not affected by the treatment in Table D14. Results are also robust to omitting the two quarters between the classroom and school-wide bans from the analysis (Table D15).

7 Conclusion

We add an important new data point to the growing body of quasi-experimental evidence on the impact of phone restrictions in K–12 schools. We move this literature beyond the developed world to a large middle-income country, and we offer one of the largest district-level studies in any country to date – focusing on Rio de Janeiro, the largest municipal school district in Latin America. We combine detailed administrative records with survey data and qualitative evidence to provide a rich picture of the way students and educators experienced

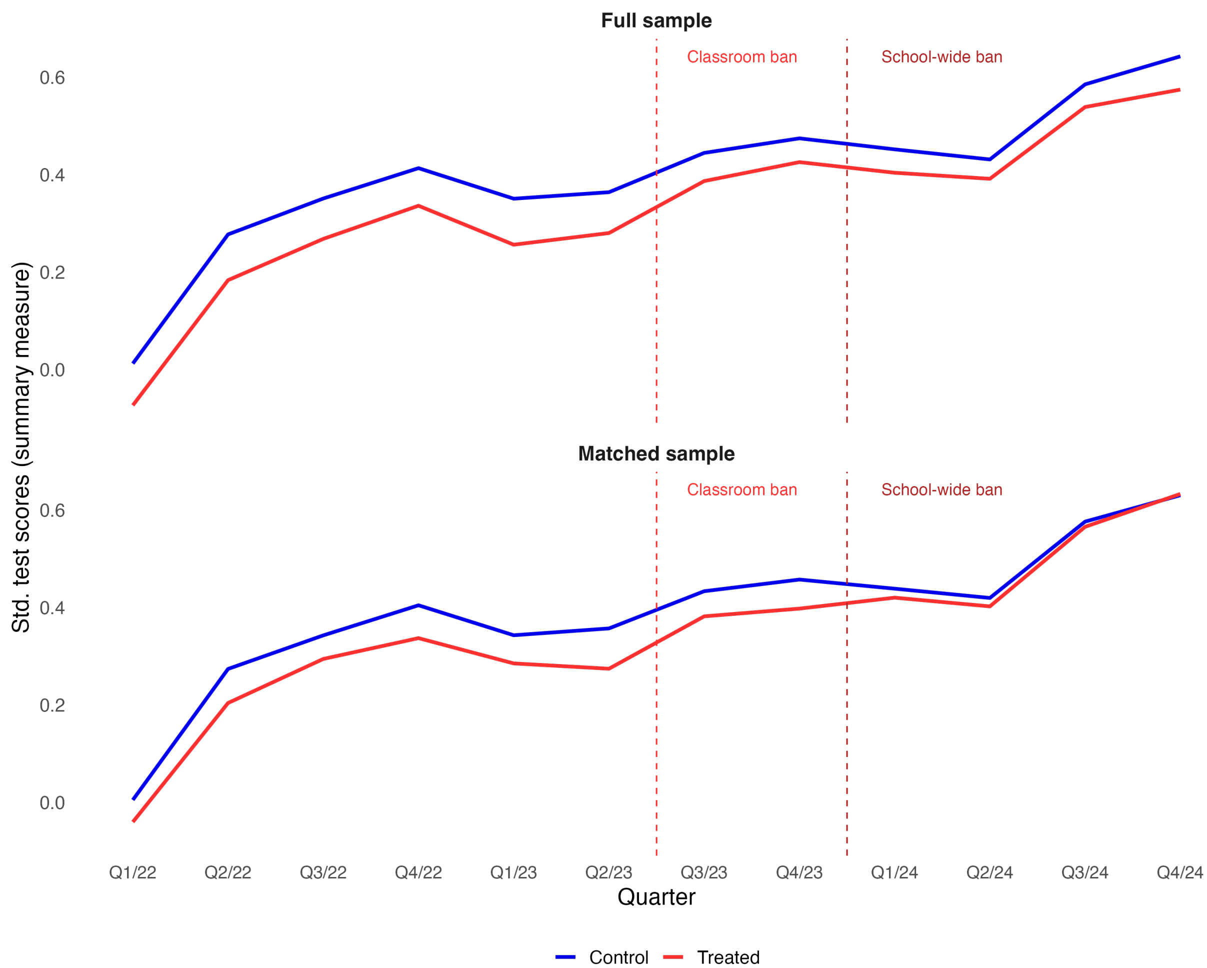
Rio's phone ban.

Our findings suggest that the ban, although imperfectly enforced, led to substantial reductions in in-class phone use, closing most of the gap in use between schools that had previously had strict policies (control schools) and those that had not (treatment schools). Our main finding is an improvement in middle school math and Portuguese standardized test scores of 0.06 standard deviations in treatment schools relative to control. This pattern is consistent with substantial gains in learning, and it implies that the total effect of the ban may have been an improvement in test scores as large as 0.2 standard deviations.

We reiterate several important caveats. First, treatment and control schools differ in the levels of observable characteristics at baseline, and some concurrent policies could in principle have had heterogeneous effects across schools. Second, mapping relative changes to treatment effects of the ban generally requires assuming homogeneous treatment effects. Our estimates could be over-stated if treatment effects are larger in treatment schools or under-stated if the reverse is true. Our results are also less precisely estimated when inference allows for arbitrary correlation across grades within schools. A final limitation is that our evidence on changes in phone use comes from principals' self-reports which are subject to measurement error. Despite these limitations, our findings point toward significant learning gains from Rio's phone ban, and suggest that additional research on the impacts and implementation of school phone restrictions in middle-income and developing countries is an important area for future research.

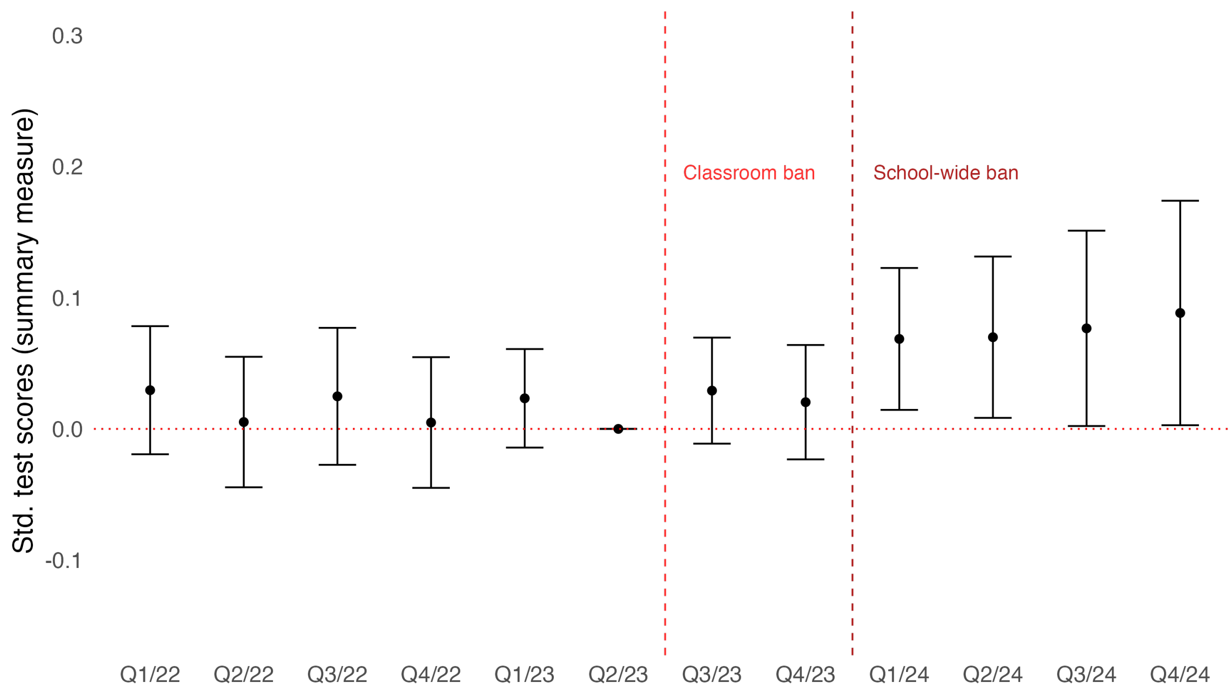
Figures and tables

Figure 1: Trends in standardized test scores



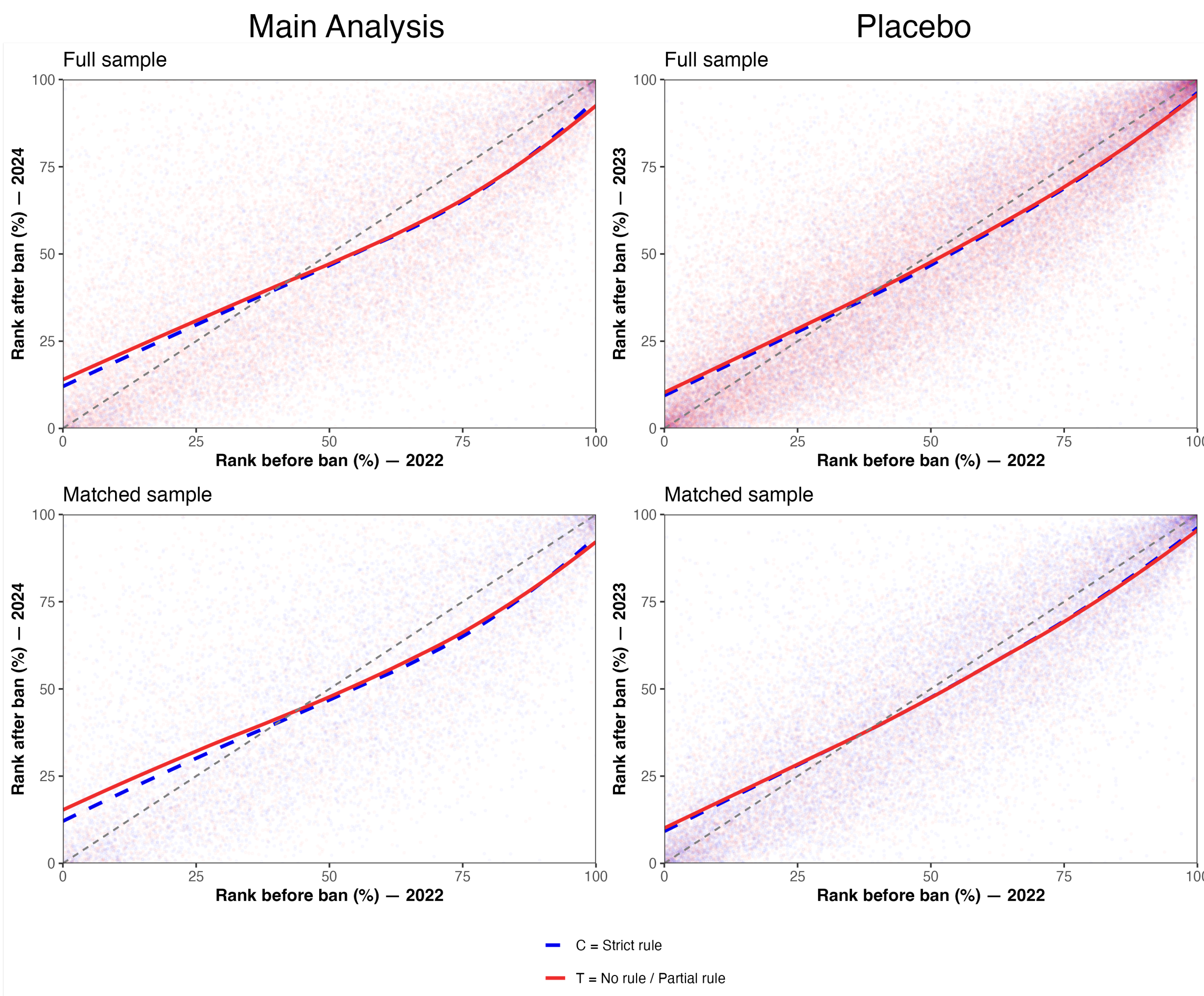
Note: This plot shows the summary measure of Portuguese and math quarterly test scores, standardized relative to control schools in Q1/2022. The normalization also adds a constant to each student Q1/2024 math and Portuguese test scores – such that average test scores by subject at the time match those predicted by a linear interpolation between Q4/2023 and Q2/2024. Treated = 1 for schools with only partial or no rule on phone use prior to the municipal ban, and 0 otherwise.

Figure 2: Event study effect sizes on math and Portuguese standardized test scores (Matched sample)



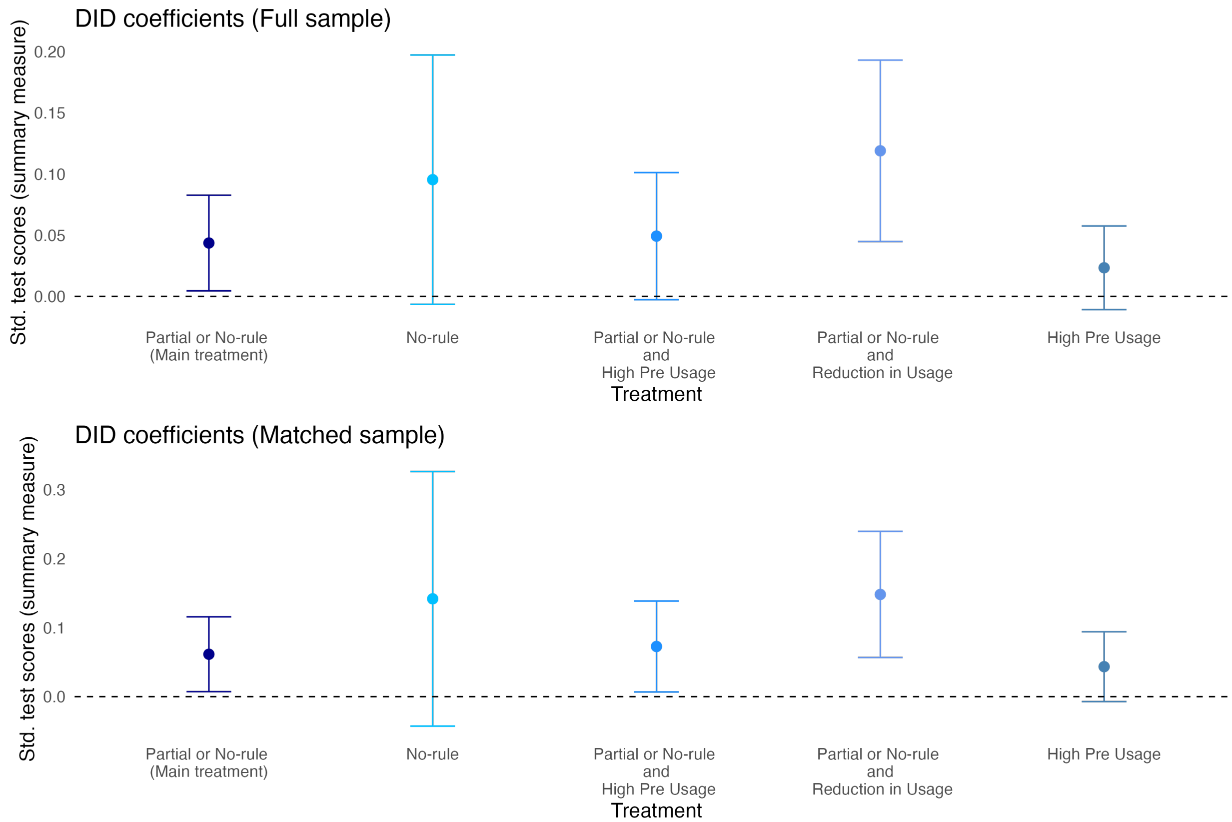
Note: This plot shows estimates from an event-study specification using a differences-in-differences model, with the summary measure of Portuguese and math quarterly test scores (standardized relative to control schools in Q1/2022) as the dependent variable. Student × quarter data from Q1/2022–Q4/2024. Bars are 95% confidence intervals. Specifications include school × grade and quarter fixed effects, interactions of *Post* with baseline (Q1/2022) school-average test score and student–teacher ratio, and control for age, gender, race, Bolsa Família beneficiary status, and parental education; standard errors are clustered at the school × grade level.

Figure 3: Rank–rank plots by treatment status



Note: Each panel plots locally-estimated scatterplot smoothing (LOESS) fitted curves between student percentile rank in 2022 (x-axis) and the subsequent percentile rank (y-axis). The dashed 45° line indicates no change. Treatment schools (solid red; no/partial pre-ban rule) and control schools (dashed blue; strict pre-ban rule) are shown separately. The left column uses 2024 as the post period; the right column uses 2023 as the post period (Placebo). Top row uses the full sample; bottom row restricts to the matched sample. Points are individual students (highly transparent). Ranks are computed within grade using the mean z-score in the relevant period across the entire student distribution in each sample.

Figure 4: Robustness tests: Alternative treatment definitions



Note: This plot shows versions of the main treatment coefficient from Equation 1, using alternative definitions of $Treat$. The first value shown corresponds to the main specification. In the second, we consider only schools with no previous rule as the treatment group, and exclude schools with partial rules from the sample. The last three explore treatment definitions that use the perceived phone use as reported by the principals. We define average usage as a simple mean taken over the panel of students in each school, which uses the pre-policy values reported by principals for each school and grade converted from categorical responses to a numerical scale (None = 0, Few = 0.25, Some = 0.5, Most = 0.75, All = 1). In the third column, we define a school as treated only if it had no strict rule and had usage above 50%, as defined above. In the fourth, we define a school as treated only if it had no strict rule and had an average reduction in usage. Finally, the last column defines a school as treated if its reported average usage was high, regardless of what the previous phone rule was. Specifications include school $\times$ grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, and control for age, gender, race, Bolsa Família beneficiary status, and parental education; standard errors are clustered at the school $\times$ grade level.

Table 1: First stage: Impact of phone ban on phone usage during class by treatment status (Matched sample)

	Dependent variable:		
	Any use	Some+ use	Most+ use
	(1)	(2)	(3)
School-wide ban	-0.126^{***}	-0.306^{***}	-0.239^{***}
	(0.021)	(0.034)	(0.031)
School-wide ban × Treated	-0.050	-0.023	-0.103^{**}
	(0.031)	(0.045)	(0.042)
DV mean (control, pre-ban)	0.934	0.708	0.399
DV mean (treated, pre-ban)	0.977	0.867	0.561
School × Grade FE	Yes	Yes	Yes
Observations	1,206	1,206	1,206
R^2	0.671	0.680	0.705

The table reports the first-stage estimates of the impact of Rio de Janeiro's municipal phone ban on phone usage during class for the matched sample of schools. The dependent variables are indicators for different levels of phone usage: any use (1), some, most or all use (2), and most or all use (3), based on principals' categorical reports. School-wide ban = 1 for the quarters after the implementation of the school-wide phone ban (Q1-Q4/2024), and 0 otherwise. Treated = 1 for schools with only partial or no rule on phone use prior to the municipal ban, and 0 otherwise. All regressions include school and grade fixed effects. The sample comprises school × grade observations over two period (pre and post). Standard errors are clustered at the school-grade level. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

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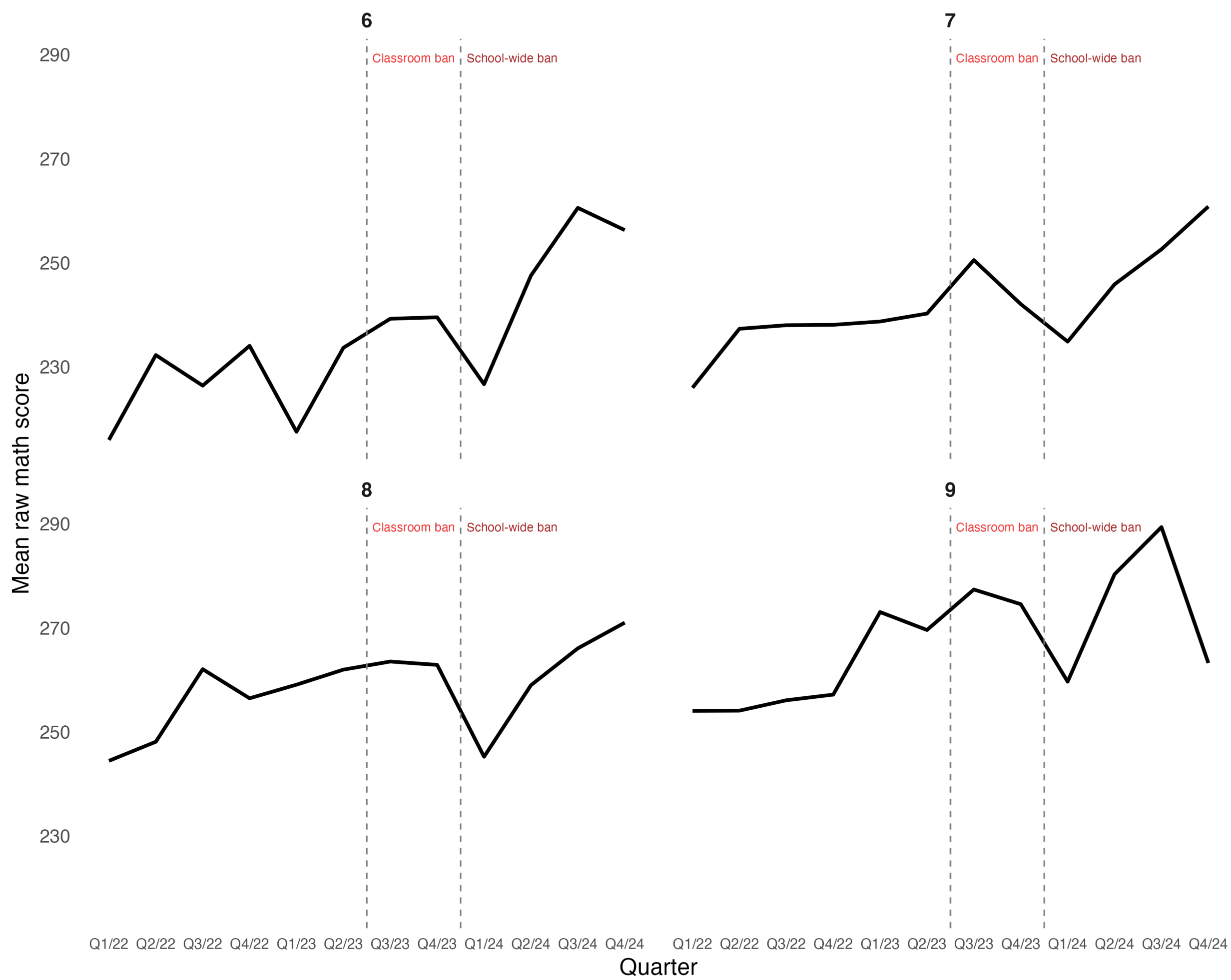
Appendix

Table of contents

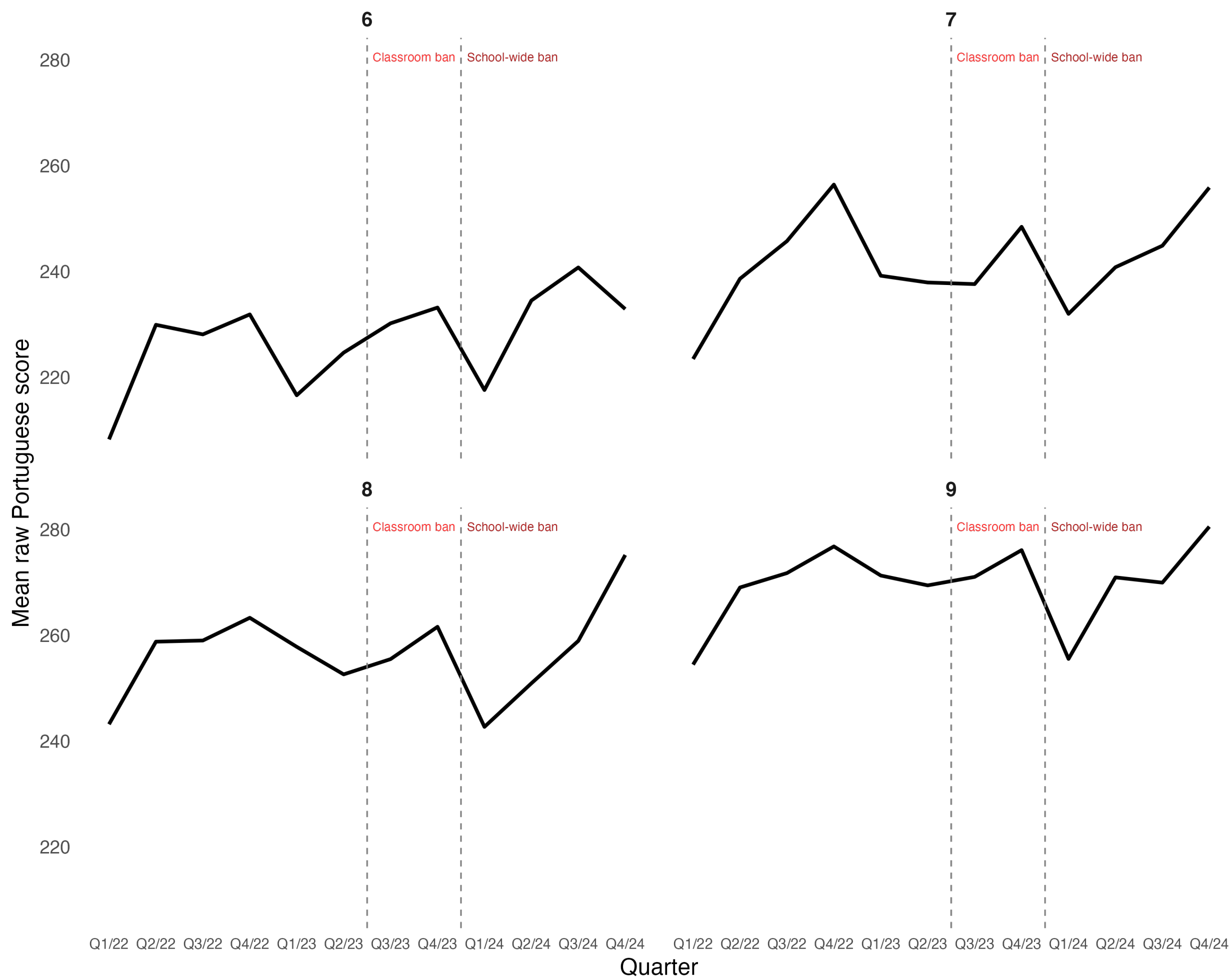
A Descriptive statistics and context

Figure A1: Raw math scores by grade (Full sample)



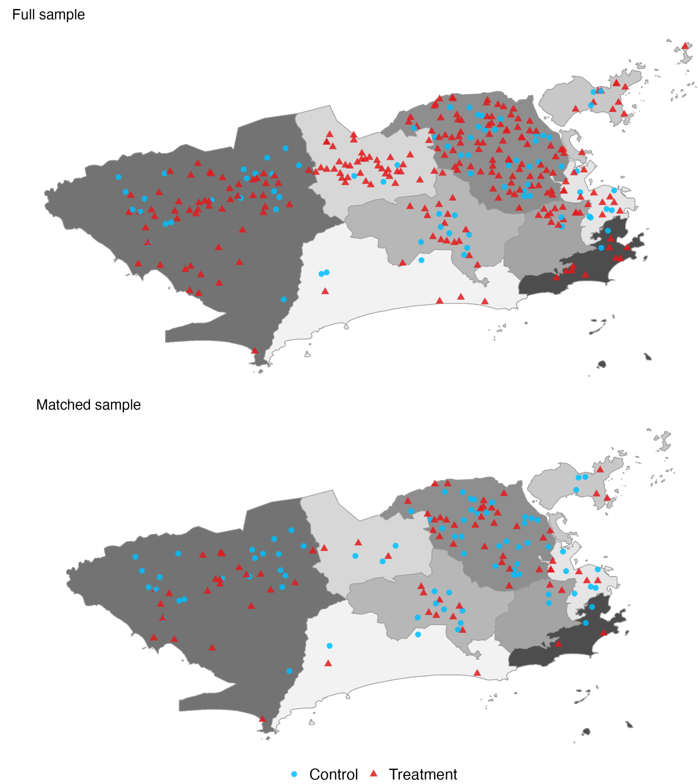
Note: This plot shows average quarterly math test scores by grade. A simple mean is taken over the distribution of students in the full sample for each grade, without any normalization.

Figure A2: Raw Portuguese scores by grade (Full sample)



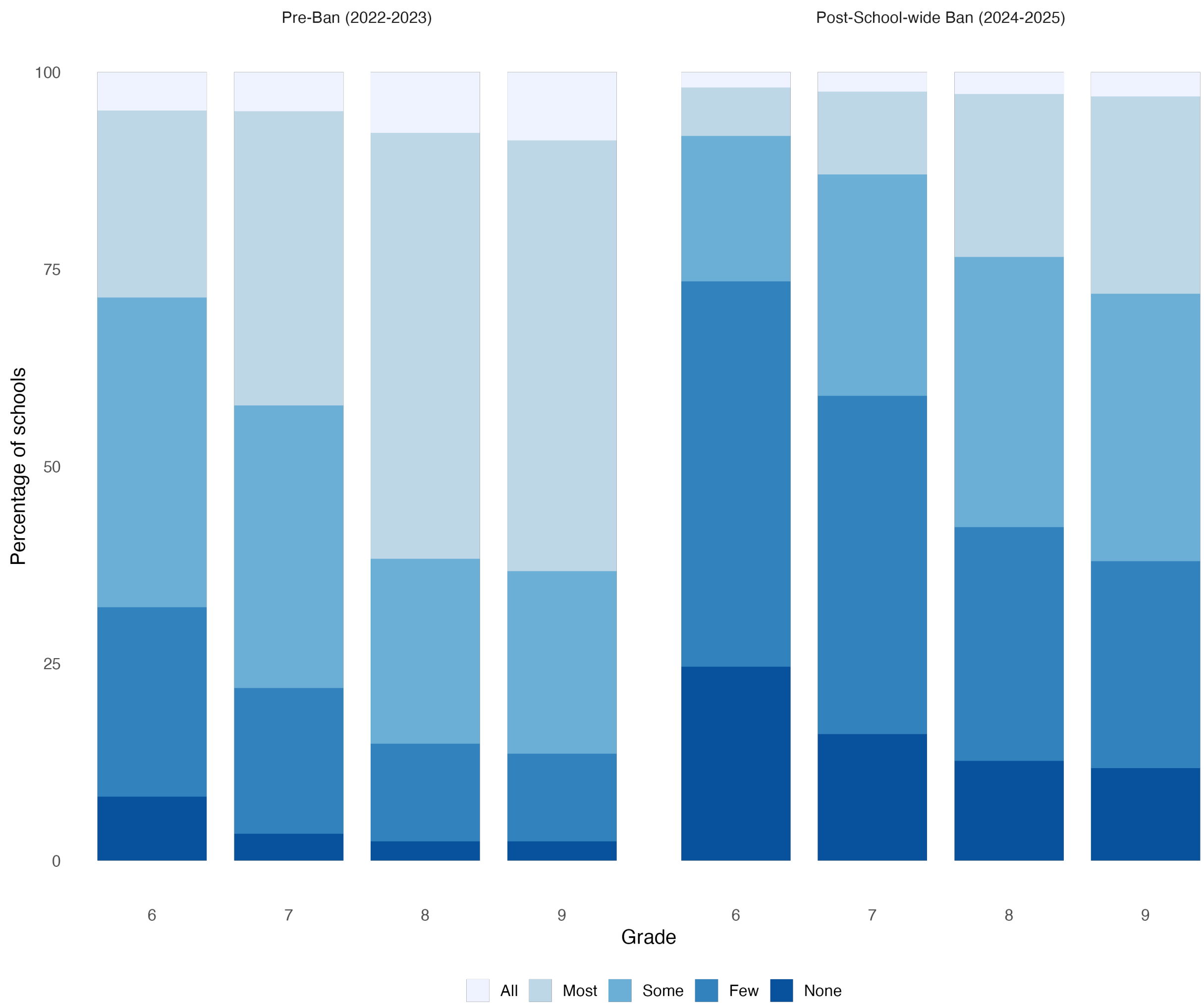
Note: This plot shows average quarterly Portuguese test scores by grade. A simple mean is taken over the distribution of students in the full sample for each grade, without any normalization.

Figure A3: Geographic distribution of the matched sample



Note: This figure shows the distribution of schools in the city of Rio de Janeiro. The administrative limits of each "subprefeitura" are shown shaded.

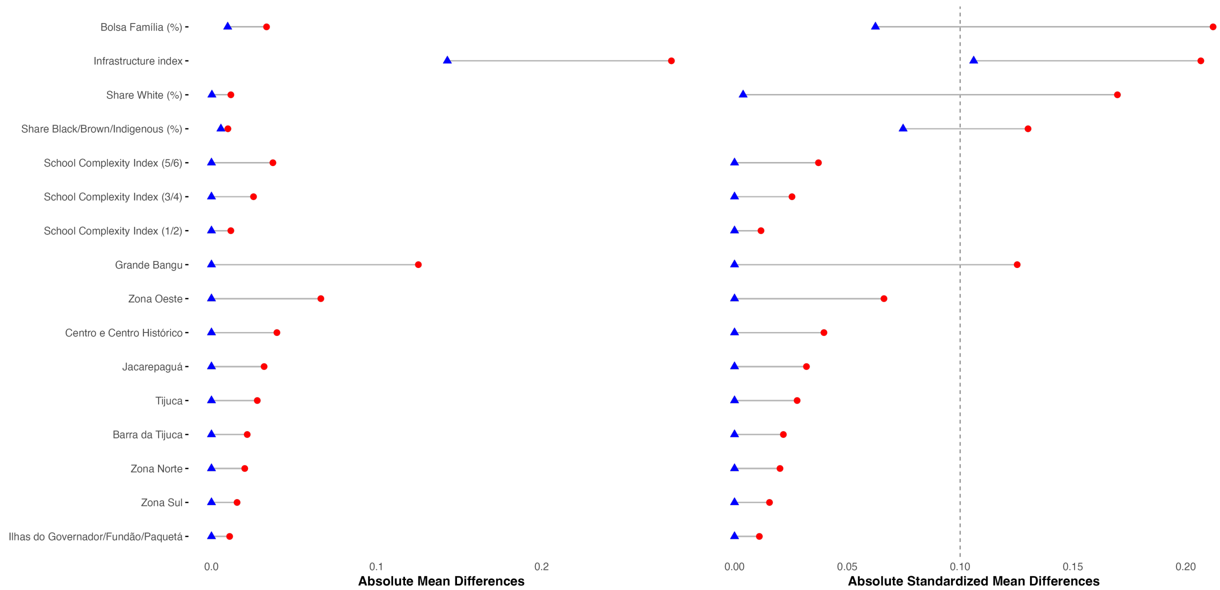
Figure A4: Distribution of schools by reported student phone use before and after the ban, by grade



Note: Bars show the share of schools reporting how many students use phones during class, before (2022–2023) and after (2025) the ban. Categories are ordered from *All* to *None*. Percentages are calculated within grade.

B Matching and covariate balance

Figure B1: Covariate balance before and after matching



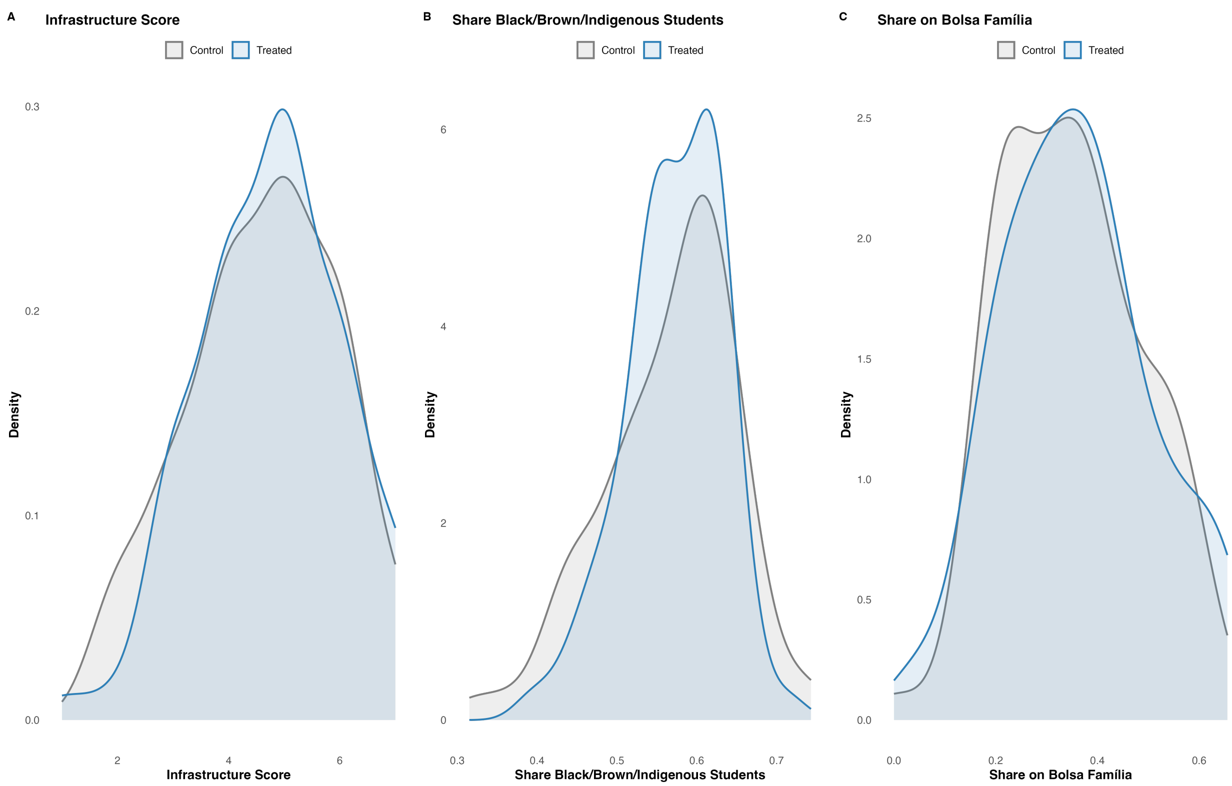
Note: This figure shows covariate balance before (red circles) and after (blue triangles) matching for the covariates included in the matching algorithm. The left panel reports absolute mean differences and the right panel reports absolute standardized mean differences, with the vertical dashed line at 0.1 indicating the target threshold used for acceptable balance.

Table B1: Descriptive statistics and balance in selected variables

Variable	Treatment	Control	Diff	p-value
Panel A: Full sample				
Number of classes	954	306		
Number of schools	245	79		
Mean age	13.46 (0.04)	13.49 (0.06)	0.02 (0.07)	0.735
Bolsa Família (%)	0.38 (0.01)	0.35 (0.01)	-0.03 (0.01)	0.010**
Disabled (%)	0.03 (0.00)	0.03 (0.00)	0.00 (0.00)	0.094*
Mother $\geq$ HS (%)	0.03 (0.00)	0.04 (0.00)	0.01 (0.00)	0.033**
Male (%)	0.51 (0.00)	0.50 (0.00)	-0.00 (0.00)	0.476
White (%)	0.33 (0.00)	0.34 (0.01)	0.01 (0.01)	0.047**
Student–teacher ratio	22.88 (0.20)	23.01 (0.29)	0.13 (0.36)	0.714
Infrastructure index	4.86 (0.04)	4.59 (0.08)	-0.27 (0.09)	0.003***
Grade size	79.04 (1.48)	86.15 (2.73)	7.10 (3.11)	0.023**
Avg. proficiency z-score	0.18 (0.01)	0.28 (0.02)	0.10 (0.02)	0.000***
Avg. Δ proficiency Q2	0.26 (0.01)	0.28 (0.01)	0.01 (0.02)	0.439
Avg. Δ proficiency Q3	0.07 (0.01)	0.07 (0.02)	-0.00 (0.02)	0.986
Avg. Δ proficiency Q4	0.08 (0.01)	0.07 (0.01)	-0.00 (0.02)	0.828
Avg. attendance (%)	94.62 (0.09)	94.45 (0.15)	-0.17 (0.17)	0.337
Attendance $\geq$ 75% (Passing Threshold)	0.99 (0.00)	0.99 (0.00)	-0.00 (0.00)	0.129
Avg. phone share pre-ban (%)	0.60 (0.01)	0.53 (0.02)	-0.07 (0.02)	0.000***
Panel B: Matched sample				
Number of classes	297	298		
Number of schools	77	77		
Mean age	13.48 (0.06)	13.49 (0.06)	0.01 (0.09)	0.888
Bolsa Família (%)	0.35 (0.01)	0.36 (0.01)	0.00 (0.01)	0.945
Disabled (%)	0.03 (0.00)	0.03 (0.00)	0.00 (0.00)	0.293
Mother $\geq$ HS (%)	0.03 (0.00)	0.04 (0.00)	0.00 (0.00)	0.160
Male (%)	0.51 (0.00)	0.50 (0.00)	-0.01 (0.01)	0.097*
White (%)	0.33 (0.01)	0.34 (0.01)	0.01 (0.01)	0.445
Student–teacher ratio	22.73 (0.33)	23.11 (0.30)	0.39 (0.45)	0.388
Infrastructure index	4.74 (0.07)	4.62 (0.08)	-0.11 (0.11)	0.294
Grade size	84.24 (2.87)	86.30 (2.79)	2.06 (4.00)	0.607
Avg. proficiency z-score	0.21 (0.01)	0.27 (0.02)	0.06 (0.02)	0.006***
Avg. Δ proficiency Q2	0.24 (0.01)	0.28 (0.01)	0.04 (0.02)	0.054*
Avg. Δ proficiency Q3	0.07 (0.01)	0.06 (0.02)	-0.00 (0.02)	0.816
Avg. Δ proficiency Q4	0.06 (0.01)	0.07 (0.01)	0.01 (0.02)	0.470
Avg. attendance (%)	95.02 (0.17)	94.34 (0.15)	-0.68 (0.22)	0.002***
Attendance $\geq$ 75% (Passing Threshold)	0.99 (0.00)	0.99 (0.00)	-0.00 (0.00)	0.007***
Avg. phone share pre-ban (%)	0.62 (0.01)	0.53 (0.02)	-0.09 (0.02)	0.000***

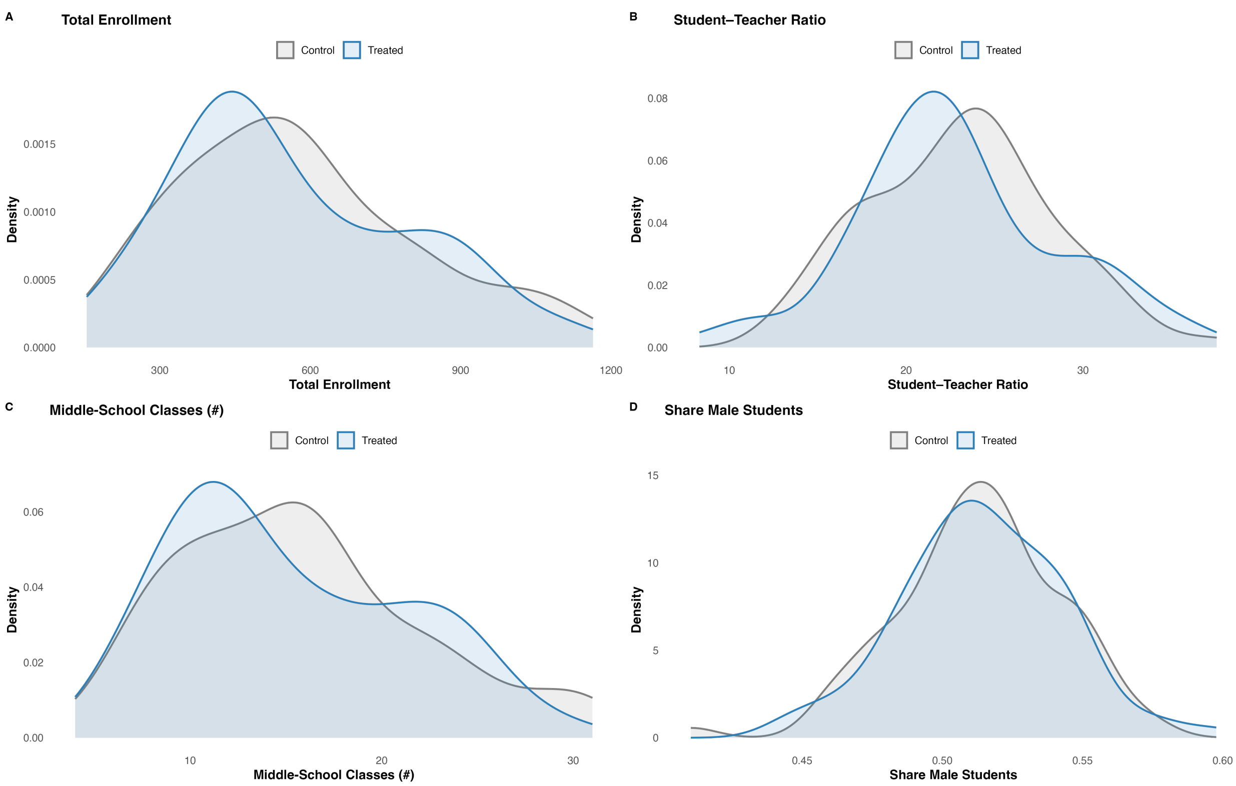
Note:
This table reports mean characteristics for treatment and control groups; standard errors are in parentheses. All variables are measured in the pre-ban period (2022) and means are computed over school–grade cells. "Diff" is Control minus Treatment. Panel A uses the full sample; Panel B restricts to the matched sample. The first two rows report counts of classes and distinct schools. Δ Proficiency refers to the average change in z-scores in each quarter relative to the previous quarter. The average phone usage is calculated from recoded categorical survey responses (None = 0, Few = .25, Some = .5, Most = .75, All = 1). p-values come from two-sided t-tests comparing group means. Variables labeled "(%)" are proportions reported on a 0–1 scale. $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

Figure B2: Kernel density of matched covariates, treatment and control



Note: This figure shows kernel density overlays for the matched covariates in the distribution of schools in the treatment and control groups, after matching.

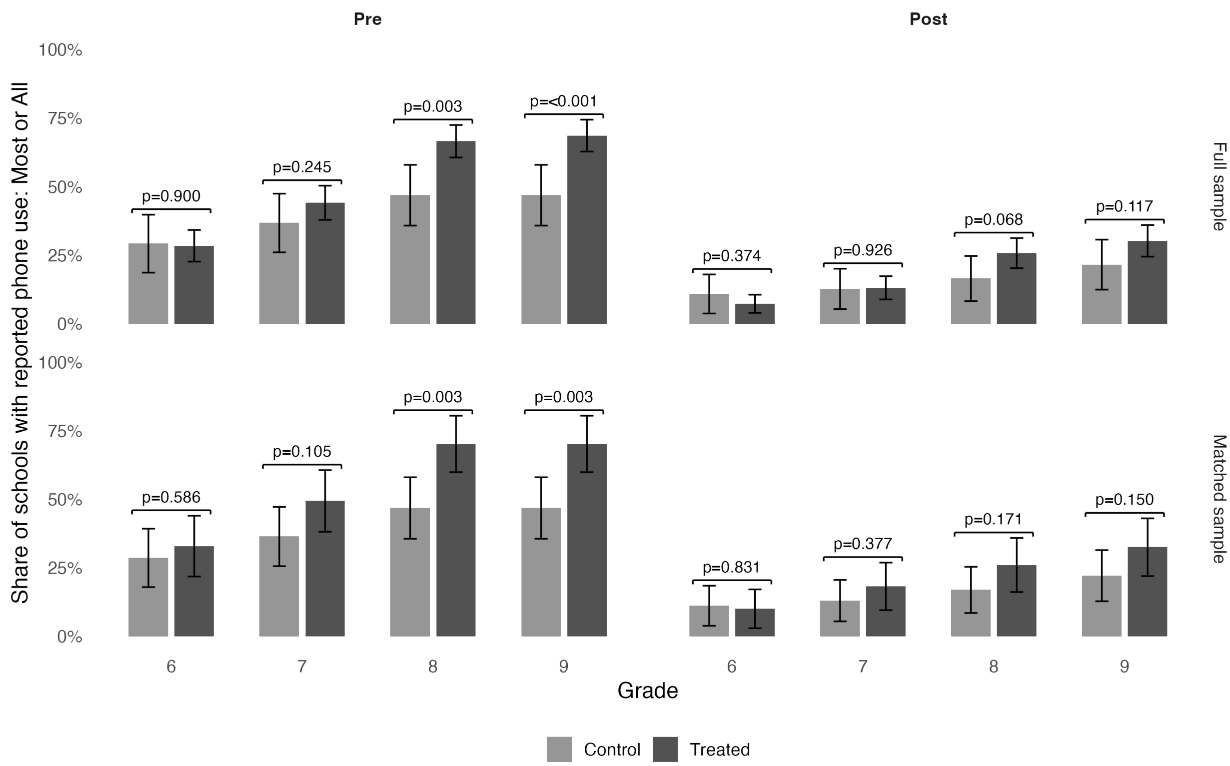
Figure B3: Kernel density of covariates not used in matching, treatment and control



Note: This figure shows kernel density overlays for the covariates not used in the matching algorithm, in the distribution of schools in the treatment and control groups, after matching.

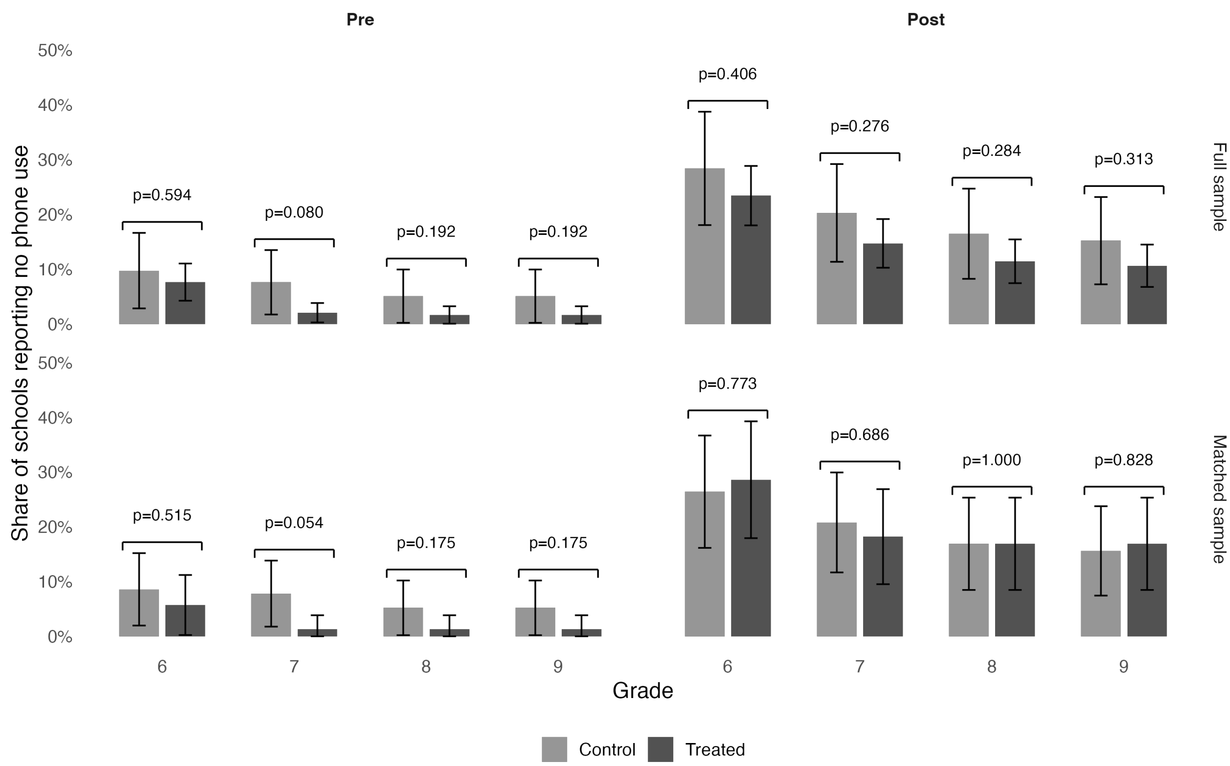
C Mechanisms and perceived effects

Figure C1: Convergence of treatment and control schools in reporting most or all student phone use



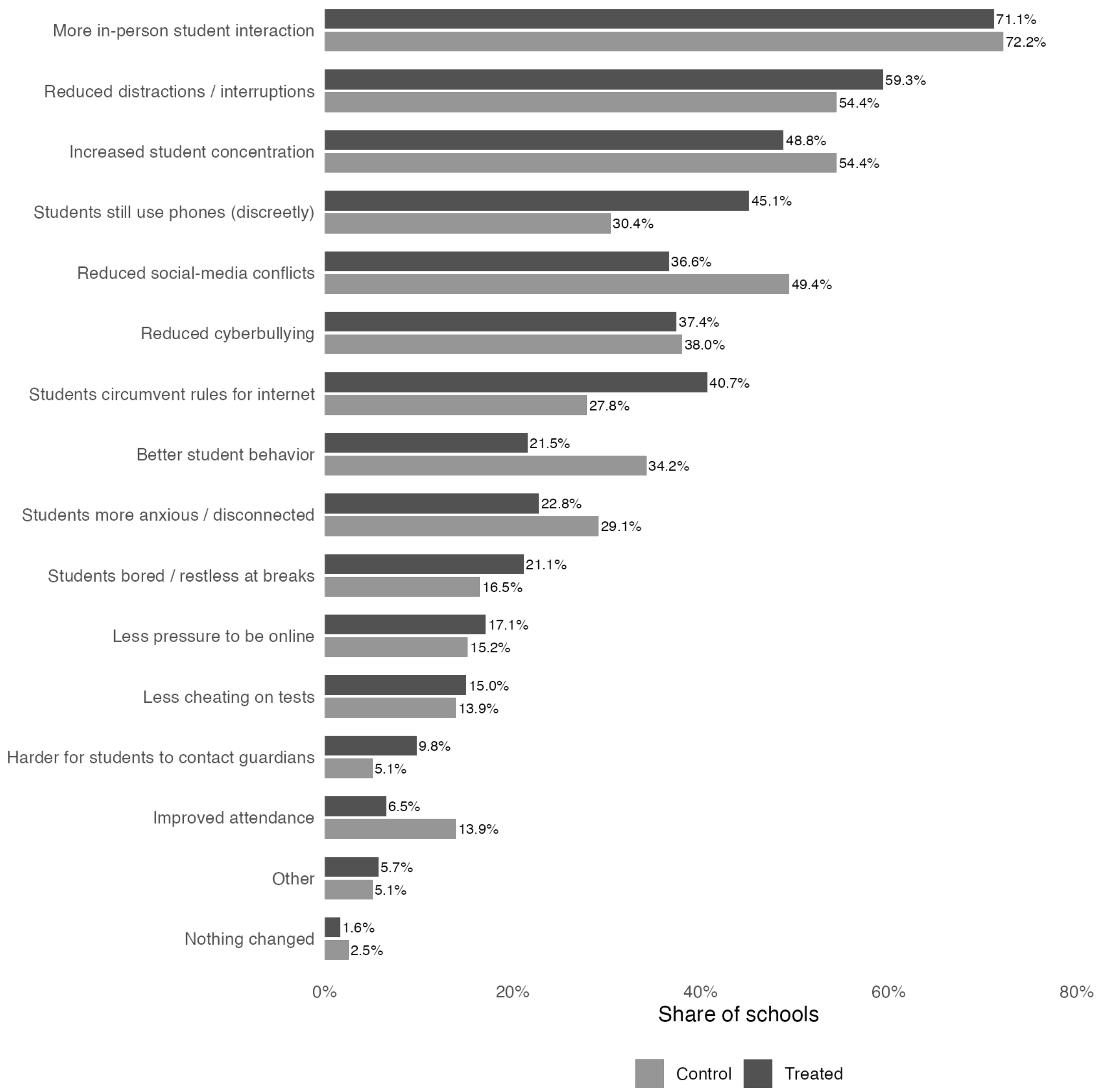
Note: Bars report the share of schools where principals reported that most or all students use phones, by grade (6–9). Facets separate full and matched samples and pre- and post-ban periods. Error bars show 95% confidence intervals. Brackets display p-values from Welch's t-tests comparing treated and control schools within each cell.

Figure C2: Convergence of treated and control schools in reporting no phone use



Note: Bars report the share of schools where principals reported no student phone use, by grade (6–9). Facets separate full and matched samples and pre- and post-ban periods. Error bars show 95% confidence intervals. Brackets display p-values from Welch's t-tests comparing treatment and control schools within each cell.

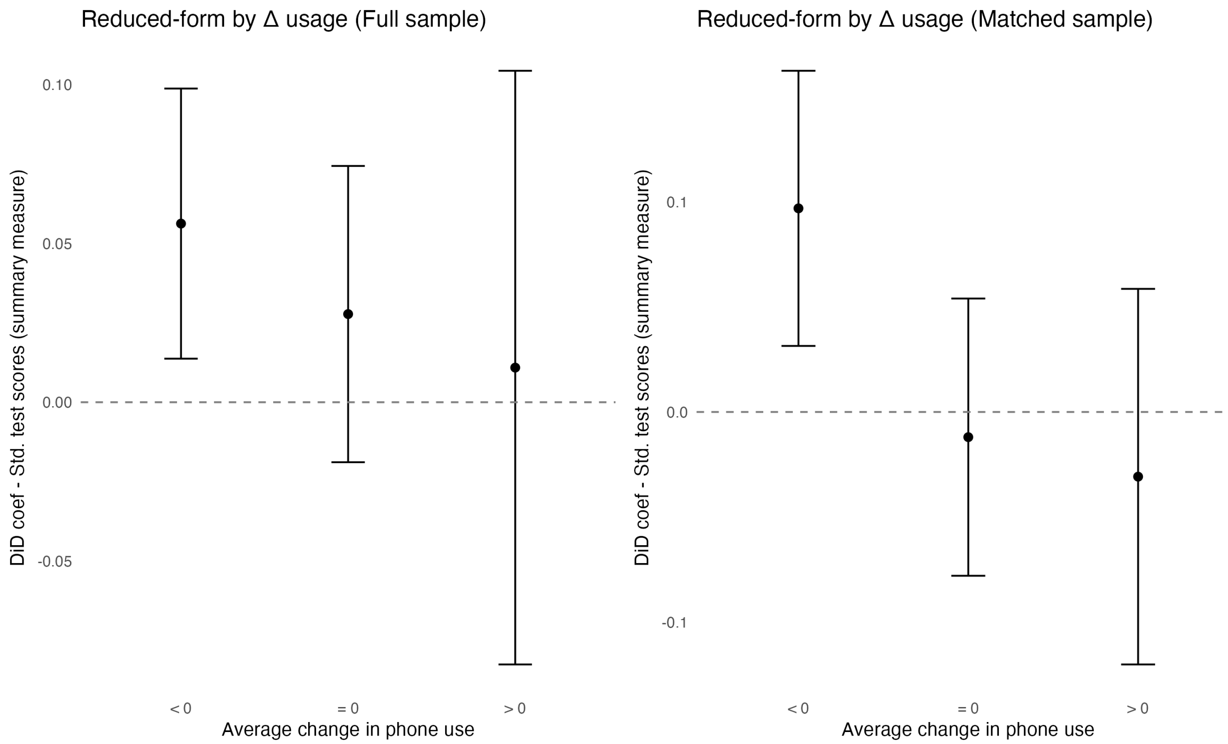
Figure C3: Perceived effects of the phone ban on students



Note: Bars show the share of school principals that reported each effect within each group.

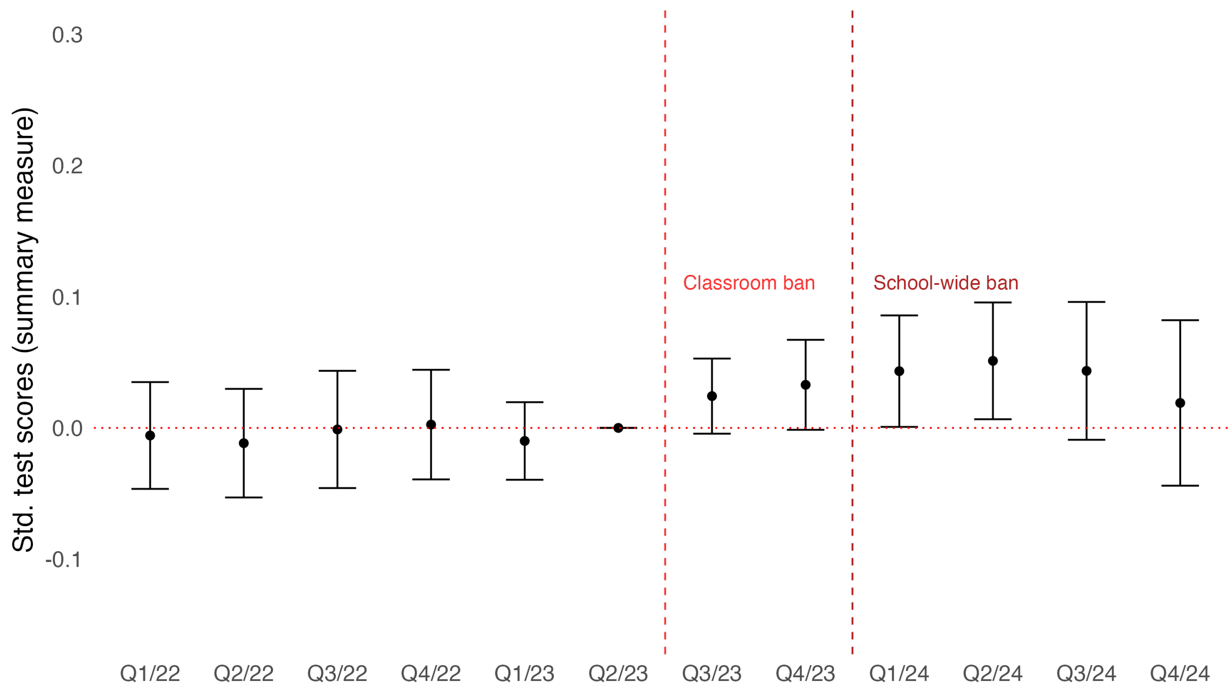
D Additional results

Figure D1: Treatment effect by perceived change in phone use



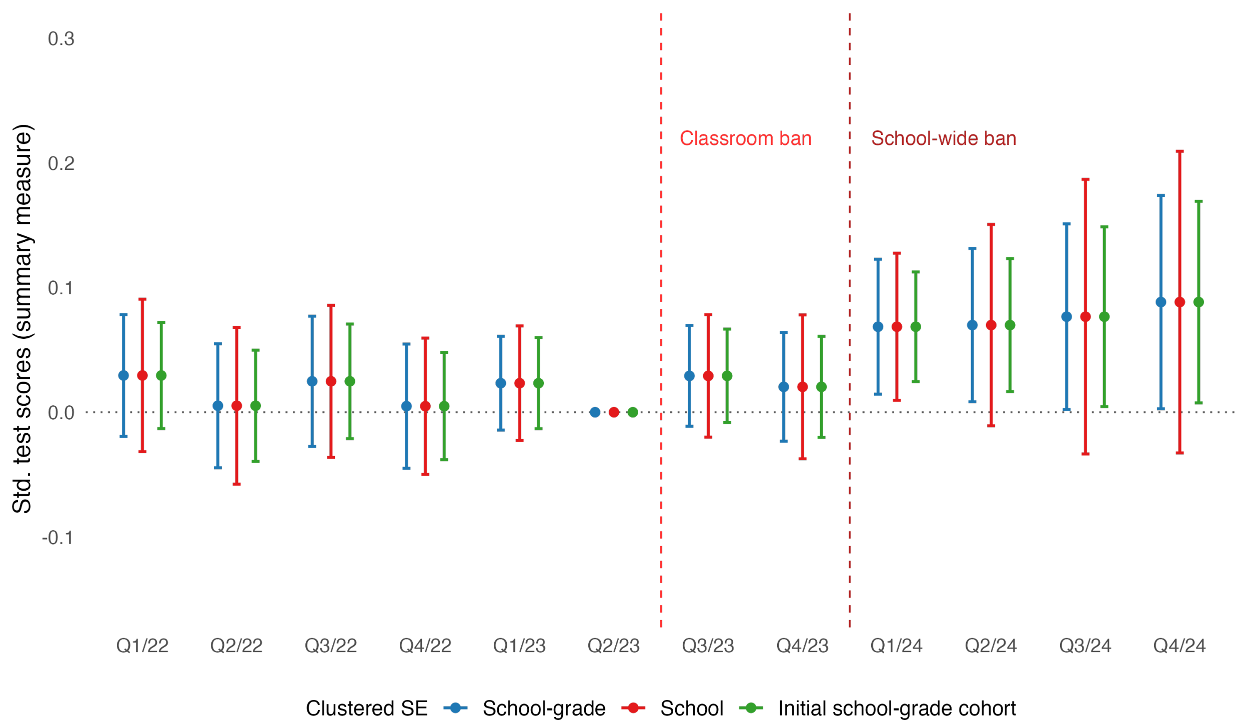
Note: This plot shows estimated values of the main treatment effect from Equation 1 estimated in three subsamples of treatment schools in the full and matched samples, based on their average reported change in phone use. The treatment schools in each of the two samples are grouped into three mutually exclusive subsamples, and one is used in each of the three models. The control group includes all control schools in each sample and is the same in the three models for each sample. The average share used to group them is calculated as a simple mean taken over the panel of students in each school, which uses the pre and post values reported by principals for each school and grade converted from categorical responses to a numerical scale (None = 0, Few = 0.25, Some = 0.5, Most = 0.75, All = 1). Specifications include school × grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, and control for age, gender, race, Bolsa Família beneficiary status, and parental education; standard errors are clustered at the school × grade level.

Figure D2: Event study effect sizes on math and Portuguese standardized test scores (Full sample)



Note: This plot shows estimates from an event-study specification using a differences-in-differences model student–quarter data from Q1/2022–Q4/2024, using the full sample of schools (no matching). Bars are 95% confidence intervals. Outcomes are grade-standardized z-scores normalized to control schools in Q1/2022. Specifications include school $\times$ grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, and control for age, gender, race, Bolsa Família beneficiary status, and parental education; standard errors are clustered at the school $\times$ grade level.

Figure D3: Event study on math and Portuguese standardized test scores with alternative clustering (Matched sample)



Note: This plot shows estimates from an event-study specification using a differences-in-differences model student–quarter data from Q1/2022–Q4/2024, using the matched sample of schools. Bars are 95% confidence intervals. Outcomes are grade-standardized z-scores normalized to control schools in Q1/2022. Specifications include school × grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, and control for age, gender, race, Bolsa Família beneficiary status, and parental education; standard errors are clustered at the school × grade level, school level, and cohort level (where a cohort is the school and grade in which a student first appears in the sample.).

Table D1: First stage: Impact of phone ban on phone usage during class by treatment status (Full sample)

	Dependent variable:		
	Any use	Some+ use	Most+ use
	(1)	(2)	(3)
School-wide ban	-0.126^{***}	-0.314^{***}	-0.246^{***}
	(0.021)	(0.033)	(0.031)
School-wide ban × Treated	0.009	-0.014	-0.084^{**}
	(0.024)	(0.037)	(0.035)
DV mean (control, pre-ban)	0.932	0.706	0.401
DV mean (treated, pre-ban)	0.968	0.824	0.521
School × Grade FE	Yes	Yes	Yes
Observations	2,561	2,561	2,561
R^2	0.681	0.699	0.710

The table reports the first-stage estimates of the impact of Rio de Janeiro's municipal phone ban on phone usage during class for the full sample of schools. The dependent variables are indicators for different levels of phone usage: any use (1), some, most or all use (2), and most or all use (3), based on principals' categorical reports. School-wide ban = 1 for the quarters after the implementation of the school-wide phone ban (Q1-Q4/2024), and 0 otherwise. Treated = 1 for schools with only partial or no rule on phone use prior to the municipal ban, and 0 otherwise. All regressions include school and grade fixed effects. The sample comprises school × grade observations over two period (pre and post). Standard errors are clustered at the school-grade level. $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

Table D2: Difference-in-differences estimates of Rio's school phone ban on student test scores (Matched sample)

	Dependent variable: z-score					
	Summary measure				Math	Portuguese
	(1)	(2)	(3)	(4)	(5)	(6)
School-wide ban × Treated	0.061**	0.073**	0.061**	0.084***	0.077**	0.045*
	(0.028)	(0.035)	(0.027)	(0.028)	(0.037)	(0.024)
Classroom ban × Treated	0.010	0.014	0.008	0.010	0.009	0.012
	(0.021)	(0.019)	(0.020)	(0.021)	(0.030)	(0.016)
School-grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Grade-period FE	No	No	Yes	No	No	No
Student FE	No	Yes	No	No	No	No
CRE x School-wide ban FE	No	No	No	Yes	No	No
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Student covariates	Yes	No	Yes	Yes	Yes	Yes
Number of Schools	154	154	154	154	154	154
Number of Students	97,380	97,380	97,380	97,380	97,380	97,380
Number of Classes	601	601	601	601	601	601
Δ Q4 Std. Progression (Control)	0.33	0.33	0.33	0.33	0.3	0.35
Observations	549,969	549,969	549,969	549,969	549,969	549,969
R^2	0.158	0.752	0.166	0.159	0.149	0.132

The table reports difference-in-differences estimates of the impact of Rio de Janeiro's school phone ban on standardized test scores using the matched sample. Specifications vary in the inclusion of student fixed effects, grade-period fixed effects, and CRE×School-wide ban fixed effects. The dependent variable is the standardized z-score in a summary measure (math and portuguese combined), math , and portuguese, normalized to the control group in 2022/Q1.Classroom ban = 1 for the quarters after the implementation of the classroom phone ban but before the school-wide ban (Q3-Q4/2023), and 0 otherwise; School-wide ban = 1 for the quarters after the implementation of the school-wide phone ban (Q1-Q4/2024), and 0 otherwise. Treated = 1 for schools with only partial or no rule on phone use prior to the municipal ban, and 0 otherwise. All regressions include the fixed effects indicated and, unless otherwise noted, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, along with controls for age, gender, race, Bolsa Família participation, and parental education. Standard errors are clustered at the school–grade level. The sample comprises student–quarter observations from 2022Q1 to 2024Q4. $* p < 0.10$, $** p < 0.05$, $* * * p < 0.01$.

Table D3: Difference-in-differences estimates by baseline student grade quantiles

	Dependent variable: z-score (summary measure)							
	Full sample				Matched sample			
	p25	p50	p75	p100	p25	p50	p75	p100
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School-wide ban × Treated	0.080*	0.040	0.010	0.003	0.099	0.078	0.044	0.040
	(0.046)	(0.034)	(0.031)	(0.034)	(0.063)	(0.052)	(0.046)	(0.044)
Classroom ban × Treated	0.062**	0.031	0.026	0.008	0.038	0.005	0.015	0.003
	(0.025)	(0.023)	(0.022)	(0.022)	(0.032)	(0.032)	(0.030)	(0.029)
School×Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Schools	324	324	323	323	154	154	154	154
Number of Students	25,443	25,442	25,442	25,442	12,037	12,571	12,858	13,269
Number of Clusters	1,251	1,260	1,259	1,255	589	595	596	596
Observations	147,754	166,654	174,079	181,924	68,917	80,883	86,321	93,670
R^2	0.187	0.131	0.103	0.093	0.180	0.129	0.101	0.098

The table reports difference-in-differences estimates of the impact of Rio de Janeiro's school phone ban on standardized test scores, estimated separately by quartiles of baseline student performance. Results are shown for both the Full Sample and the matched sample. Quantiles are defined based on students' average test scores in 2022, ranked within each grade. The treated group comprises schools with no prior phone restrictions or only partial bans, while the control group consists of schools that had strict pre-existing bans. The dependent variable is the standardized z-score in a summary measure (math and portuguese combined) normalized to the control group in 2022/Q1. Classroom ban = 1 for the quarters after the implementation of the classroom phone ban but before the school-wide ban (Q3-Q4/2023), and 0 otherwise; School-wide ban = 1 for the quarters after the implementation of the school-wide phone ban (Q1-Q4/2024), and 0 otherwise. Treated = 1 for schools with only partial or no rule on phone use prior to the municipal ban, and 0 otherwise. All regressions include school–grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, along with controls for age, gender, race, Bolsa Família participation, and parental education. Standard errors are clustered at the school–grade level. The sample comprises student–quarter observations from 2022Q1 to 2024Q4. $*\ p < 0.10$, $**\ p < 0.05$, $***\ p < 0.01$.

Table D4: Calibration of test-score effects using survey-based first stage

	Full Panel		Matched Panel	
	(1) Reduced Form	(2) High Usage	(3) Reduced Form	(4) High Usage
Panel A: Reduced Form and Implied Effects				
Reduced-form estimate	0.044**		0.061**	
	(0.020)		(0.028)	
Implied effect from control-group decline		0.128		0.143
		(0.089)		(0.097)
Implied total effect in treated schools		0.171*		0.204*
		(0.103)		(0.117)
N	1,116,689	1,116,689	549,969	549,969
Control Mean	0.400	0.400	0.390	0.390
R^2	0.156		0.158	
Panel B: First-Stage Components and Calibration				
Control-group decline in phone use		0.246***		0.239***
		(0.031)		(0.031)
Additional decline in treated schools		0.084**		0.103**
		(0.035)		(0.042)
Total decline in treated schools		0.330***		0.342***
		(0.016)		(0.028)
Implied slope (RF / additional decline)		0.520		0.597*
		(0.319)		(0.362)
First-stage F-statistic		5.90		6.04
N (First Stage)		2,561		1,206

Column groups report results for the full panel and matched panel, respectively. Reduced-form columns report the difference-in-differences estimate of the phone ban on the standardized test scores (summary measure) and are equivalent to the estimates shown in Column (3) of Table D11 and Table D2. High-usage columns use first-stage estimates to calibrate implied score effects using an indicator equal to 1 if principals report that "Most" or "All" students use phones during class. Panel B reports the building blocks of this calibration: (i) the post-ban decline in phone use in control schools, (ii) the additional decline in treated schools relative to controls, and (iii) the total decline in treated schools. These numbers are equivalent to those shown in Column (3) of Table 1 for the matched sample. The implied slope equals the reduced-form estimate divided by the additional decline in treated schools. It can be interpreted as the change in test scores, in standard deviation units, associated with a one-unit reduction in the share of observations classified as high usage. The first calibrated row in Panel A applies this slope to the decline in phone use observed in control schools, yielding the improvement in test scores that is differenced out by the DiD design. The second calibrated row in Panel A applies the same slope to the total decline in treated schools, yielding the implied total treatment effect relative to a no-ban counterfactual. Standard errors for calibrated quantities are computed using the delta method. $*\ p < 0.10$, $**\ p < 0.05$, $***\ p < 0.01$.

Table D5: Difference-in-differences estimates by grade

	Dependent variable: z-score (summary measure)							
	Full sample				Matched sample			
	Grade 6	Grade 7	Grade 8	Grade 9	Grade 6	Grade 7	Grade 8	Grade 9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School-wide ban × Treated	−0.008	0.084***	0.070*	0.003	0.011	0.092**	0.096*	0.036
	(0.040)	(0.030)	(0.036)	(0.045)	(0.056)	(0.040)	(0.053)	(0.063)
Classroom ban × Treated	−0.007	0.022	0.056*	0.039	−0.041	0.002	0.019	0.035
	(0.031)	(0.023)	(0.029)	(0.038)	(0.037)	(0.024)	(0.031)	(0.056)
Observations	206,334	314,672	305,172	290,511	92,195	156,288	153,853	147,633
R^2	0.197	0.171	0.154	0.159	0.207	0.168	0.158	0.163

The table reports difference-in-differences estimates of the impact of Rio de Janeiro's school phone ban on standardized test scores by grade. Columns (1)–(4) use the full sample, and Columns (5)–(8) use the matched sample. The dependent variable is the standardized z-score of the summary measure combining math and Portuguese test scores, normalized relative to the control group in 2022/Q1. Classroom ban = 1 for the quarters after the implementation of the classroom phone ban but before the school-wide ban (Q3–Q4/2023), and 0 otherwise; School-wide ban = 1 for the quarters after the implementation of the school-wide phone ban (Q1–Q4/2024), and 0 otherwise. Treated = 1 for schools with only partial or no restrictions on phone use prior to the municipal ban, and 0 otherwise. All regressions include school–grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, and controls for age, gender, race, Bolsa Família participation, and parental education. Standard errors are clustered at the school–grade level. The sample comprises student–quarter observations from 2022Q1 to 2024Q4. $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

Table D6: Robustness tests: Differential trends by baseline school characteristics (Matched sample)

	Dependent variable:						
	Summary measure (z-score) (Matched sample)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
School-wide ban × Treated	0.061**	0.061**	0.061**	0.059**	0.058**	0.062**	0.053*
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.029)
Classroom ban × Treated	0.011	0.011	0.010	0.011	0.011	0.011	0.011
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
School-wide ban × Baseline avg. std. test score		−0.050					0.027
		(0.035)					(0.041)
School-wide ban × Baseline student-teacher ratio			−0.004*				−0.007**
			(0.002)				(0.003)
School-wide ban × Baseline avg. attendance				0.002			0.002
				(0.004)			(0.005)
School-wide ban × Baseline share non-white					0.419**		0.611**
					(0.202)		(0.239)
School-wide ban × Baseline share Bolsa Família						−0.073	−0.125
						(0.084)	(0.095)
N Students	97,380	97,380	97,380	97,380	97,380	97,380	97,162
N Schools	154	154	154	154	154	154	154
N Clusters	601	601	601	601	601	601	595
School×Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline decile shares × Post	No	No	No	No	No	No	Yes
Observations	549,969	549,969	549,969	549,969	549,969	549,969	548,693
R^2	0.157	0.158	0.158	0.158	0.158	0.158	0.158

The table reports robustness checks for the impact of Rio de Janeiro's school phone bans on standardized test scores using the matched sample. Each column augments the baseline difference-in-differences specification with interactions between the post-ban indicators and baseline (Q1/2022) school–grade characteristics. Column (1) includes no baseline interactions. Column (2) adds interactions with the baseline school–grade average test score. Column (3) adds interactions with the baseline student–teacher ratio. Column (4) adds interactions with baseline average attendance. Column (5) adds interactions with the baseline share of non-white students. Column (6) adds interactions with the baseline share of students enrolled in Bolsa Família. Column (7) includes all baseline school–grade interactions simultaneously, and additionally allows for differential trends by the full baseline test-score decile distribution at the school–grade level (coefficients omitted from the table for readability). The dependent variable is the standardized z-score of the summary measure combining math and Portuguese test scores, normalized relative to the control group in Q1/2022. Classroom ban equals 1 for quarters after the implementation of the classroom phone ban but before the school-wide ban (Q3–Q4/2023), and 0 otherwise. School-wide ban equals 1 for quarters after the implementation of the school-wide phone ban (Q1–Q4/2024), and 0 otherwise. Treated equals 1 for schools with only partial or no restrictions on phone use prior to the municipal ban, and 0 otherwise. All regressions include school–grade and quarter fixed effects, as well as controls for student age, gender, race, Bolsa Família enrollment status, and parental education. Standard errors are clustered at the school *imes* grade level. The sample consists of student *imes* quarter observations from Q1/2022 through Q4/2024. $* p < 0.10$, $** p < 0.05$, $* * * p < 0.01$.

Table D7: Robustness tests: Differential trends by baseline school characteristics (Full sample)

	Dependent variable:						
	Summary measure (z-score) (Full sample)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
School-wide ban × Treated	0.047**	0.042**	0.049**	0.046**	0.044**	0.048**	0.035*
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.021)
Classroom ban × Treated	0.034**	0.033**	0.034**	0.033**	0.034**	0.034**	0.033**
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
School-wide ban × Baseline avg. std. test score		−0.106***					−0.010
		(0.030)					(0.034)
School-wide ban × Baseline student-teacher ratio			−0.002*				−0.004***
			(0.001)				(0.002)
School-wide ban × Baseline avg. attendance				0.002			0.004
				(0.003)			(0.003)
School-wide ban × Baseline share non-white					0.259**		0.168
					(0.122)		(0.153)
School-wide ban × Baseline share Bolsa Família						−0.019	−0.068
						(0.055)	(0.063)
N Students	196,757	195,485	196,757	195,485	196,757	196,757	194,880
N Schools	325	323	325	323	325	325	323
N Clusters	1,277	1,269	1,277	1,269	1,277	1,277	1,256
School×Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline decile shares × Post	No	No	No	No	No	No	Yes
Observations	1,122,250	1,116,689	1,122,250	1,116,689	1,122,250	1,122,250	1,113,387
R^2	0.156	0.156	0.156	0.156	0.156	0.156	0.156

The table reports robustness checks for the impact of Rio de Janeiro's school phone bans on standardized test scores using the full sample. Each column augments the baseline difference-in-differences specification with interactions between the post-ban indicators and baseline (Q1/2022) school–grade characteristics. Column (1) includes no baseline interactions. Column (2) adds interactions with the baseline school–grade average test score. Column (3) adds interactions with the baseline student–teacher ratio. Column (4) adds interactions with baseline average attendance. Column (5) adds interactions with the baseline share of non-white students. Column (6) adds interactions with the baseline share of students enrolled in Bolsa Família. Column (7) includes all baseline school–grade interactions simultaneously, and additionally allows for differential trends by the full baseline test-score decile distribution at the school–grade level (coefficients omitted from the table for readability). The dependent variable is the standardized z-score of the summary measure combining math and Portuguese test scores, normalized relative to the control group in Q1/2022. Classroom ban equals 1 for quarters after the implementation of the classroom phone ban but before the school-wide ban (Q3–Q4/2023), and 0 otherwise. School-wide ban equals 1 for quarters after the implementation of the school-wide phone ban (Q1–Q4/2024), and 0 otherwise. Treated equals 1 for schools with only partial or no restrictions on phone use prior to the municipal ban, and 0 otherwise. All regressions include school–grade and quarter fixed effects, as well as controls for student age, gender, race, Bolsa Família enrollment status, and parental education. Standard errors are clustered at the school × grade level. The sample consists of student × quarter observations from Q1/2022 through Q4/2024. $* p < 0.10$, $** p < 0.05$, $* * * p < 0.01$.

Table D8: Robustness tests: Differential trends by baseline individual characteristics (Matched sample)

	Dependent variable:					
	Summary measure (z-score) (Matched sample)					
	(1)	(2)	(3)	(4)	(5)	(6)
School-wide ban × Treated	0.062**	0.061**	0.061**	0.062**	0.117**	0.110**
	(0.028)	(0.028)	(0.028)	(0.028)	(0.048)	(0.048)
Classroom ban × Treated	0.010	0.010	0.010	0.010	0.024	0.024
	(0.021)	(0.021)	(0.021)	(0.021)	(0.030)	(0.030)
N Students	97,380	97,380	97,380	97,380	50,735	50,735
N Schools	154	154	154	154	154	154
N Clusters	601	601	601	601	596	596
School×Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes
Baseline school controls × Post	Yes	Yes	Yes	Yes	Yes	Yes
Bolsa Famíilia × Post	Yes	No	No	No	No	Yes
Race × Post	No	Yes	No	No	No	Yes
Gender × Post	No	No	Yes	No	No	Yes
Parental education × Post	No	No	No	Yes	No	Yes
Student decile × Post	No	No	No	No	Yes	Yes
Observations	549,969	549,969	549,969	549,969	329,791	329,791
R^2	0.158	0.158	0.158	0.158	0.206	0.208

The table reports robustness checks for the impact of Rio de Janeiro's school phone bans on standardized test scores using the matched sample. Each column augments the baseline difference-in-differences specification with interactions between the ban-period indicators and baseline individual characteristics. School-wide ban equals 1 for quarters after the implementation of the school-wide phone ban (Q1–Q4/2024), and 0 otherwise; Classroom ban equals 1 for quarters after the classroom phone ban but before the school-wide ban (Q3–Q4/2023), and 0 otherwise. Treated equals 1 for schools with only partial or no restrictions on phone use prior to the municipal ban, and 0 otherwise. Across all specifications, the regression includes school–grade and quarter fixed effects, student covariates (age, gender, race, Bolsa Família status, and parental education), and interactions of the School-wide ban indicator with baseline (Q1/2022) school–grade average test score and student–teacher ratio. Columns (1)–(5) add one type of baseline-individual interaction at a time; column (6) includes all of them. Standard errors are clustered at the school × grade level. The sample consists of student × quarter observations from Q1/2022 through Q4/2024. $*\,p < 0.10$, $**\,p < 0.05$, $***\,p < 0.01$.

Table D9: Robustness tests: Differential trends by baseline individual characteristics (Full sample)

	Dependent variable:					
	Summary measure (z-score) (Full sample)					
	(1)	(2)	(3)	(4)	(5)	(6)
School-wide ban × Treated	0.044**	0.043**	0.044**	0.045**	0.076**	0.066*
	(0.020)	(0.020)	(0.020)	(0.020)	(0.037)	(0.037)
Classroom ban × Treated	0.033**	0.033**	0.033**	0.033**	0.037*	0.037*
	(0.016)	(0.016)	(0.016)	(0.016)	(0.022)	(0.022)
N Students	195,485	195,485	195,485	195,485	101,224	101,224
N Schools	323	323	323	323	323	323
N Clusters	1,269	1,269	1,269	1,269	1,257	1,257
School×Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes
Baseline school controls × Post	Yes	Yes	Yes	Yes	Yes	Yes
Bolsa Famíilia × Post	Yes	No	No	No	No	Yes
Race × Post	No	Yes	No	No	No	Yes
Gender × Post	No	No	Yes	No	No	Yes
Parental education × Post	No	No	No	Yes	No	Yes
Student decile × Post	No	No	No	No	Yes	Yes
Observations	1,116,689	1,116,689	1,116,689	1,116,689	670,411	670,411
R^2	0.156	0.156	0.156	0.156	0.208	0.211

The table reports robustness checks for the impact of Rio de Janeiro's school phone bans on standardized test scores using the full sample. Each column augments the baseline difference-in-differences specification with interactions between the ban-period indicators and baseline individual characteristics. School-wide ban equals 1 for quarters after the implementation of the school-wide phone ban (Q1–Q4/2024), and 0 otherwise; Classroom ban equals 1 for quarters after the classroom phone ban but before the school-wide ban (Q3–Q4/2023), and 0 otherwise. Treated equals 1 for schools with only partial or no restrictions on phone use prior to the municipal ban, and 0 otherwise. Across all specifications, the regression includes school–grade and quarter fixed effects, student covariates (age, gender, race, Bolsa Família status, and parental education), and interactions of the School-wide ban indicator with baseline (Q1/2022) school–grade average test score and student–teacher ratio. Columns (1)–(5) add one type of baseline-individual interaction at a time; column (6) includes all of them. Standard errors are clustered at the school × grade level. The sample consists of student × quarter observations from Q1/2022 through Q4/2024. $* p < 0.10$, $** p < 0.05$, $*** p < 0.01$.

Table D10: Robustness tests: Alternative matching specifications

	Dependent variable:				
	Summary measure (z-score)				
	PS match Subpref + ICG	PS match CRE	PS match CRE + ICG	Mahalanobis Add. covariates	IPW
	(1)	(2)	(3)	(4)	(5)
Treated x School-wide ban	0.061**	0.081***	0.096***	0.079***	0.053**
	(0.028)	(0.028)	(0.028)	(0.027)	(0.022)
Treated x Classroom ban	0.010	0.007	0.042*	0.033	0.036*
	(0.021)	(0.021)	(0.023)	(0.021)	(0.019)
N Students	97,380	98,935	96,461	99,571	196,487
N Schools	154	158	152	158	324
N Clusters	601	622	596	621	1,273
School×Grade FE	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes
Observations	549,969	556,622	542,090	562,207	1,116,689
R^2	0.158	0.164	0.160	0.161	0.158

The table reports difference-in-differences estimates of the impact of Rio de Janeiro's school phone bans on student achievement under alternative matching strategies. Column (1) reports the main specification, using propensity-score matching with exact matches on subprefeitura and the School Management Complexity Index (ICG). Columns (2) and (3) instead exact-match on administrative regional office (CRE), without and with ICG, respectively. Column (4) uses nearest-neighbor Mahalanobis matching (without replacement) on a richer covariate set: infrastructure items, internet access, enrollment, student–teacher ratio, grade offer, number of middle-school classes, gender and racial composition, and Bolsa Família coverage. Column (5) uses Inverse Probability Weighting (IPW) on the full sample, using the same matching covariates as column (1). The dependent variable is the overall test-score z-score, normalized to the control group in 2022Q1. Classroom ban = 1 in the quarters after the classroom ban but before the school-wide ban (2023Q3–2023Q4); School-wide ban = 1 after the school-wide ban (2024Q1–2024Q4). Treated = 1 for schools with only partial or no phone rule prior to the municipal ban. All regressions include school–grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, along with controls for age, gender, race, Bolsa Família participation, and parental education. Standard errors are clustered at the school–grade level. The sample comprises student–quarter observations from 2022Q1 to 2024Q4. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table D11: Difference-in-differences estimates (Full sample)

	Dependent variable: z-score		
	Summary measure (1)	Math (2)	Portuguese (3)
School-wide ban × Treated	0.044**	0.055**	0.032*
	(0.020)	(0.028)	(0.018)
Classroom ban × Treated	0.033**	0.035	0.030**
	(0.016)	(0.023)	(0.013)
N Students	196,757	196,757	196,757
N Schools	325	325	325
N Clusters	1,277	1,277	1,277
School×Grade FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Δ Q4 Std. Progression (Control)	0.33	0.3	0.35
Observations	1,116,689	1,116,689	1,116,689
R^2	0.156	0.148	0.133

The table reports difference-in-differences estimates of the impact of Rio de Janeiro's municipal phone bans on student achievement. The dependent variable is the standardized z-score in a summary measure (math and portuguese combined), math , and portuguese, normalized to the control group in 2022/Q1. Classroom ban = 1 for the quarters after the implementation of the classroom phone ban but before the school-wide ban (Q3-Q4/2023), and 0 otherwise; School-wide ban = 1 for the quarters after the implementation of the school-wide phone ban (Q1-Q4/2024), and 0 otherwise. Treated = 1 for schools with only partial or no rule on phone use prior to the municipal ban, and 0 otherwise. All regressions include school–grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, and controls for age, gender, race, Bolsa Família participation, and parental education. Standard errors are clustered at the school–grade level. The sample comprises student–quarter observations from 2022Q1 to 2024Q4. $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

Table D12: Robustness tests: Difference-in-differences estimates, balanced panel (Matched sample)

	Dependent variable: z-score		
	Summary measure	Math	Portuguese
	(1)	(2)	(3)
School-wide ban × Treated	0.185***	0.208**	0.161***
	(0.067)	(0.087)	(0.060)
Classroom ban × Treated	0.026	0.014	0.039*
	(0.030)	(0.045)	(0.023)
School-grade FE	Yes	Yes	Yes
Student FE	No	No	No
Period FE	Yes	Yes	Yes
Student covariates	Yes	Yes	Yes
Number of Schools	123	123	123
Number of Students	6,496	6,496	6,496
Number of Classes	472	472	472
Observations	77,952	77,952	77,952
R^2	0.191	0.201	0.155

The table reports difference-in-differences estimates of the impact of Rio de Janeiro's school phone ban on standardized test scores using a balanced student panel. The dependent variable is the standardized z-score in a summary measure (math and portuguese combined), math , and portuguese, normalized to the control group in 2022/Q1. Classroom ban = 1 for the quarters after the implementation of the classroom phone ban but before the school-wide ban (Q3-Q4/2023), and 0 otherwise; School-wide ban = 1 for the quarters after the implementation of the school-wide phone ban (Q1-Q4/2024), and 0 otherwise. Treated = 1 for schools with only partial or no rule on phone use prior to the municipal ban, and 0 otherwise. All regressions include school–grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, along with controls for age, gender, race, Bolsa Família participation, and parental education. Standard errors are clustered at the school–grade level. The sample comprises student–quarter observations from 2022Q1 to 2024Q4. $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

Table D13: Robustness tests: Difference-in-differences estimates on probability of non-missing test scores

	Dependent variable: Indicator for non-missing test score (1 = Yes)					
	Summary measure	Math	Portuguese	Summary measure	Math	Portuguese
		Full sample			Matched sample	
	(1)	(2)	(3)	(4)	(5)	(6)
School-wide ban × Treated	−0.006	−0.004	−0.003	−0.006	−0.005	−0.002
	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)	(0.004)
Classroom ban × Treated	−0.001	−0.002	−0.001	−0.006	−0.006*	−0.005
	(0.004)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)
School-grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Student FE	No	No	No	No	No	No
Student covariates	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. (control group)	0.90	0.92	0.93	0.90	0.92	0.93
Number of Schools	325	325	325	154	154	154
Number of Students	210,885	210,885	210,885	104,485	104,485	104,485
Number of Classes	1,277	1,277	1,277	601	601	601
Observations	1,241,665	1,241,665	1,241,665	610,510	610,510	610,510
R^2	0.055	0.046	0.047	0.055	0.046	0.047

The table reports difference-in-differences estimates of the impact of Rio de Janeiro's school phone ban on the probability that student test scores are observed (non-missing). The dependent variable is an indicator equal to 1 if a test score is present and 0 otherwise, estimated separately for a summary measure (math and portuguese combined), math , and portuguese scores. Classroom ban = 1 for the quarters after the implementation of the classroom phone ban but before the school-wide ban (Q3-Q4/2023), and 0 otherwise; School-wide ban = 1 for the quarters after the implementation of the school-wide phone ban (Q1-Q4/2024), and 0 otherwise. Treated = 1 for schools with only partial or no rule on phone use prior to the municipal ban, and 0 otherwise. All regressions include school–grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, along with controls for age, gender, race, Bolsa Família participation, and parental education. Standard errors are clustered at the school–grade level. The sample comprises student–quarter observations from 2022Q1 to 2024Q4. $* p < 0.10$, $** p < 0.05$, $* * * p < 0.01$.

Table D14: Robustness tests: Difference-in-differences estimates on probability of switching schools

	Dependent variable:	
	1{moved school}	
	Full sample	Matched sample
	(1)	(2)
School-wide ban × Treated	0.0024	0.0016
	(0.0030)	(0.0037)
N Students	214,019	105,939
N Schools	326	154
N Clusters	1,281	601
School×Grade FE	Yes	Yes
Year FE	Yes	Yes
Student covariates	Yes	Yes
DV mean (control, post-ban)	0.0236	0.0241
Observations	330,199	162,456
R^2	0.0497	0.0340

The table reports difference-in-differences estimates of the impact of Rio de Janeiro's municipal phone ban on the probability that a student attends a school different from their first observed school. The dependent variable is an indicator equal to 1 if the student's current school differs from their first observed school, and 0 otherwise. School-wide ban = 1 for years after the implementation of the school-wide phone ban (2024), and 0 otherwise. Treated = 1 for schools with only partial or no rule on phone use prior to the municipal ban, and 0 otherwise; treatment status and baseline controls are evaluated at the student's first observed school to avoid mechanical correlation with the act of moving. The unit of observation is the student-year, since school assignment varies annually. All regressions include school–grade and year fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, along with controls for age, gender, race, Bolsa Família participation, and parental education. Standard errors are clustered at the school–grade level. The sample comprises student-year observations from 2022 to 2024. Column (1) uses the full sample; Column (2) restricts to the matched sample. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table D15: Robustness tests: Difference-in-differences estimates with separate ban periods (Matched sample)

	Dependent variable: z-score					
	Summary measure	Math	Portuguese	Summary measure	Math	Portuguese
	Excluding (Q3,Q4) 2023			2022–2023		
	(1)	(2)	(3)	(4)	(5)	(6)
School-wide ban × Treated	0.062**	0.077**	0.046*			
	(0.028)	(0.038)	(0.024)			
Classroom ban × Treated				0.008	0.005	0.011
				(0.021)	(0.031)	(0.016)
N Students	96,961	96,961	96,961	76,360	76,360	76,360
N Schools	154	154	154	154	154	154
N Clusters	601	601	601	598	598	598
School×Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Δ Q4 Std. Progression (Control)	0.33	0.3	0.35	0.33	0.3	0.35
Observations	451,640	451,640	451,640	376,712	376,712	376,712
R^2	0.160	0.151	0.135	0.147	0.139	0.131

The table reports difference-in-differences estimates of the impact of Rio de Janeiro's two sequential school phone bans on standardized test scores using the matched sample. Columns 1–3 exclude Q3–Q4 of 2023, capturing the school-wide ban only (phones prohibited throughout the school day). Columns 4–6 restrict the sample to 2022–2023, capturing the classroom-only ban (phones prohibited during class hours). The dependent variable is the standardized z-score in a summary measure (math and portuguese combined), math , and portuguese, normalized to the control group in 2022/Q1. Classroom ban = 1 for the quarters after the implementation of the classroom phone ban but before the school-wide ban (Q3-Q4/2023), and 0 otherwise; School-wide ban = 1 for the quarters after the implementation of the school-wide phone ban (Q1-Q4/2024), and 0 otherwise. Treated = 1 for schools with only partial or no rule on phone use prior to the municipal ban, and 0 otherwise. All regressions include school–grade and quarter fixed effects, interactions of the post dummy with baseline (Q1/2022) school-average test score and student–teacher ratio, and controls for age, gender, race, Bolsa Família participation, and parental education. Standard errors are clustered at the school–grade level. $p < 0.1$, $p < 0.05$, $p < 0.01$.

E Robustness of ordinal scales of phone use

Bond and Lang (2019) shows that, in a wide range of circumstances, categorical variables coded as ordinal scales (as in the case of our phone use variable) might not preserve properties of their underlying latent variables. In particular, monotone recodings of those categorical variables (e.g., converting categories of phone use into usage shares, so that one could estimate an OLS regression of the effects of phone bans on such shares) might not preserve the rankings of the latent variables, rendering regression estimates meaningless and potentially even reversing the sign of true effects.

While we refrain from converting our categorical phone use variable into numerical usage shares, one could still worry that measurement error in principals' retrospective reports might preclude one from drawing any conclusions from those variables. Informed by those concerns, a recent literature has developed statistical tests to assess the extent to which inference based on ordinal scales is still preserved. Following Kaiser and Vendrik (2023), we conduct two such critical tests: estimating cumulative 'cut' regressions; and quantifying how extreme a monotone rescaling would have to be for the sign of treatment effects to reverse.

First, for each cut $k \in \{1, \dots, 4\}$ of the baseline ordinal phone-use distribution, we define

$$h_{d,k} \equiv \mathbf{1}\{\text{resp}_d \leq k\}$$

and regress $h_{d,k}$ on the treatment indicator and grade fixed effects. A negative coefficient at cut k implies that treatment schools are shifted to the right of control schools in the phone-use distribution up to that cut.

Table E1 reports these regressions (for the full and matched samples). For both samples, cuts 1–3 yield negative and statistically significant coefficients, while the coefficient at cut 4 is small and statistically indistinguishable from zero. This pattern is consistent with stochastic dominance of treatment over control in the lower and middle parts of the scale, with any crossing occurring only at the top category ("All").

Second, we consider the parametric family of monotone codings:

$$\text{None} = 0, \quad \text{Few} = \frac{m}{3}, \quad \text{Some} = \frac{2m}{3}, \quad \text{Most} = m, \quad \text{All} = 1, \qquad m \in (0, 1),$$

and, for each m, estimate the OLS coefficient on the treatment indicator using this recoded outcome. We then search for a threshold m^* such that $\hat{\beta}(m^*) = 0$. This implements the Kaiser–Vendrik sign reversal test: even allowing for any strictly increasing transformation within this family, a sign reversal occurs only when Most $= 0.17$. Thus, for baseline phone use to be higher in control schools than in treatment schools, principals would have to interpret

Table E1: Cumulative cut regressions for phone usage under monotone mappings

Panel A: All schools

Cut	Coef.	Std. error	p-value	95% CI
1	-0.037	0.013	0.004	[-0.062, -0.012]
2	-0.120	0.026	0.000	[-0.170, -0.069]
3	-0.123	0.031	0.000	[-0.184, -0.061]
4	0.020	0.016	0.218	[-0.012, 0.052]

Panel B: Matched sample

Cut	Coef.	Std. error	p-value	95% CI
1	-0.043	0.017	0.010	[-0.076, -0.010]
2	-0.159	0.033	0.000	[-0.223, -0.096]
3	-0.163	0.039	0.000	[-0.240, -0.086]
4	0.003	0.022	0.882	[-0.041, 0.047]

Each row labelled by a cut k reports the coefficient on Treated from a regression of $h_{d,k} = 1\{\text{resp} \leq k\}$ on the treatment indicator and grade fixed effects, using only pre-ban observations (post = 0).

"most students use their phones" as meaning that 17% or less students did so – which we consider extremely unlikely.

Together, the tests corroborate our claim that baseline phone use was higher in treatment schools than in control schools, a core element of our empirical strategy to identify the causal effects of phone bans by contrasting schools with different treatment intensities.

F Concurrent Rio policy changes

We reviewed all policy acts issued by the Rio school district between 2023 and 2024 that could plausibly affect student outcomes in grades 6–9. Two reforms are discussed in the main text and treated in robustness checks: (i) the reformulation of the performance bonus framework (*Resolução SME nº 395/2023*) and (ii) new teacher appointments to meet classroom demand (*Diário Oficial do Município do Rio de Janeiro*, 18/07/2024). Below we list other policies that touched grades 6–9.

- **Assessment and grading regime (*Resoluções SME nº 378/2023* and *406/2023*; *Circular E/SUBE/CAV nº 19/2023*).** These acts clarified minimum numbers of evaluation instruments per reporting period and codified recovery procedures through the Academic Management System (*SGA, "Sistema de Gestão Acadêmica"*) and the individualized recovery plan (*PPI*). The rules apply uniformly across the network and largely formalize existing practice; because they do not differentially affect treatment and control schools, they are not confounders.

- **Curricular organization (*Resolução SME nº 417/2023*; *Decreto Rio nº 53939/2024*).** These provisions updated curricular matrices and subject allocations systemwide (including grades 6–9). The rollout is gradual and does not introduce discrete breaks in grading rules that coincide with the phone-ban timing.

- **Calendar adjustments (*Resolução SME nº 475/2024*; *Resolução SME nº 508/2025*).** Calendar resolutions shifted deadlines for bimonthly reporting and school council meetings while preserving the statutory number of school days.

- **Rio de Janeiro Math Olympiad (*Olimpíada Carioca de Matemática, OCM*; *Resoluções SME nº 382/2023*, *414/2023*, *424/2023*).** Regulations for a citywide, extracurricular mathematics competition that reaches only a subset of students. There is no direct rule change in regular grading, so average bimonthly grades are unlikely to be systematically affected.

- **Teacher professional development (multiple circulars in 2023–2024 for Portuguese and Mathematics, grades 6–9).** Short routine training cycles (some in person, some distance) aimed at long-run pedagogy.

G Survey instrument

Introduction

A Secretaria Municipal de Educação do Rio de Janeiro está realizando uma pesquisa, em parceria com a Universidade de Stanford, com o objetivo de entender como a proibição do uso dos celulares, exceto para finalidades pedagógicas, está sendo aplicada nas unidades escolares da rede municipal. Sua participação é fundamental para que possamos analisar os impactos da norma e identificar oportunidades de aprimoramento. Na rede municipal, a proibição foi estabelecida pelo Decreto 53.918, de 1 de fevereiro de 2024. No Brasil, ela foi normatizada pela Lei 15.100 de 13 de janeiro de 2025. O questionário leva menos de 15 minutos para ser respondido. O prazo para preenchimento é até 18/05. Agradecemos a colaboração.

The Municipal Department of Education of Rio de Janeiro is conducting a survey, in partnership with Stanford University, with the goal of understanding how the ban on cell phone use—except for educational purposes—is being implemented in municipal schools. Your participation is essential for us to assess the impacts of the policy and identify opportunities for improvement. In the municipal school network, the ban was established by Decree 53,918 on February 1, 2024. In Brazil more broadly, it was formalized by Law 15,100 on January 13, 2025. The questionnaire takes less than 15 minutes to complete. The deadline to respond is May 18. Thank you for your cooperation.

Questionnaire

1. Na sua opinião, como os alunos reagiram à proibição do uso dos celulares nas escolas?
   In your opinion, how did students react to the ban on cell phone use in schools?

   - Ficaram extremamente insatisfeitos — *Extremely dissatisfied*
   - Ficaram muito insatisfeitos — *Very dissatisfied*
   - Ficaram pouco insatisfeitos — *Somewhat dissatisfied*
   - Foram indiferentes — *Indifferent*
   - Não ficaram insatisfeitos — *Not dissatisfied*

3. Atualmente, os alunos estão trazendo algum aparelho de celular para a escola?
   Are students currently bringing phones to school?

   - Não — *No*

- Sim, mas poucos dias — *Yes, but only a few days*
- Sim, na maioria dos dias — *Yes, most days*
- Sim, todos os dias — *Yes, every day*
- Não sei — *I don't know*

4. Onde os alunos normalmente deixam os celulares quando trazem os aparelhos para a escola?
   Where do students usually keep their phones when they bring them to school?
   (multiple choice check box)

   - Ficam com ele na mão — *They keep it in their hands*
   - No bolso da calça — *In their pants pocket*
   - Guardam na mochila — *In their backpack*
   - Guardam no armário da sala de aula ou na do diretor — *In a classroom or principal's office locker*
   - Deixam em uma caixa na entrada da escola — *In a box at the school entrance*
   - Guardam em outro lugar — *Somewhere else*

5. Após o início das aulas de 2025, com que frequência foi necessário efetuar alguma advertência aos alunos por utilizarem o celular na escola sem autorização?
   Since the beginning of the 2025 school year, how often have students been disciplined for unauthorized cell phone use?
   (Choose Never, Rarely, Sometimes, Frequently, Always for each one below)

   - Advertência verbal — *Verbal warning*
   - Retirando de sala de aula — *Student removed from classroom*
   - Entregando celular para o professor(a), diretor(a) ou coordenador(a) — *Phone was handed over to teacher/principal/coordinator*
   - Outras formas (especifique) — *In other ways (specify)*

6. No último mês, com que frequência os responsáveis foram avisados do uso inadequado dos celulares pelos alunos?
   How often were guardians notified about improper phone use in the past month?

- Nunca — *Never*
- Raramente — *Rarely*
- Às vezes — *Sometimes*
- Frequentemente — *Frequently*
- Sempre — *Always*

7. Durante as aulas, atualmente, aproximadamente quantos alunos hoje usam o celular para fins pessoais (ex.: mensagens, redes sociais)? Indique sua percepção para cada ano escolar (1º ao 9º ano):
   Currently, during class time, approximately how many students use phones for personal purposes (e.g., messaging, social media)? Indicate your perception for each grade (1st to 9th):
   - Nenhum — *None*
   - Poucos — *Few*
   - Alguns — *Some*
   - A maioria — *Most*
   - Todos — *All*
   - Nossa escola não atende essa série — *Our school does not serve this grade*

8. ANTES da proibição oficial do município em 2024, sua escola já aplicava alguma regra sobre o uso de celulares?
   BEFORE the official municipal ban in 2024, did your school already have a rule about phone use?
   - Durante todo o período escolar (os alunos são proibidos de usar o celular durante todo o dia letivo, exceto para fins educacionais em sala de aula) — *Throughout the school day (students are prohibited from using their cell phone during the entire school day, except for educational purposes in the classroom)*
   - Restrição durante as aulas (os alunos podem usar o celular apenas durante o intervalo ou no horário do almoço) — *Restricted during classes (students may use their cell phone only during recess or at lunchtime)*
   - Sem restrição geral (cada professor e funcionário decide quando os alunos podem usar o celular). Isso também se aplica a escolas que atendem apenas alunos mais novos, onde o uso do celular não é uma preocupação, caso não haja uma regra

oficial da escola. — *No general restriction (each teacher and staff member decides when students may use their cell phone). This also applies to schools that serve only younger students, where cell phone use is not a concern, if there is no official school-wide rule.*

9. Durante as aulas, aproximadamente quantos alunos usavam o celular para fins pessoais (ex.: mensagens, redes sociais) ANTES da proibição municipal de 2024? Indique sua percepção para cada ano escolar (1º ao 9º ano):
   BEFORE the 2024 ban, how many students used phones for personal purposes during class (e.g., messaging, social media)? Indicate your perception for each grade (1st to 9th):

   - Nenhum — *None*
   - Poucos — *Few*
   - Alguns — *Some*
   - A maioria — *Most*
   - Todos — *All*
   - Nossa escola não atende essa série — *Our school does not serve this grade*

10. Desde a implementação da proibição nacional em 2025, houve mudanças na forma como a política de uso de celulares é aplicada na sua escola? Se sim, quais foram as principais mudanças?
    Since the national ban in 2025, have there been changes in how phone policy is enforced in your school? If so, what were the main changes?
    (Open ended textbox)

11. Pensando na implementação da medida na sua escola, os desafios têm sido maiores:
    Thinking about the implementation of the measure in your school, the challenges have been greater:

    - Nos intervalos de aula — *During recess*
    - Durante as aulas — *During class*
    - Ambos igualmente — *Equally in both*
    - Não tenho tido dificuldades — *I haven't had difficulties*

12. Quais são os principais desafios atuais para a aplicação da política de uso de celulares na sua escola? (Marque todas as opções aplicáveis)
    What are the main current challenges in enforcing phone use policy at your school? (Select all that apply)
    (Multiple choice checkbox)
    - Falta de apoio dos pais — *Lack of parental support*
    - Dificuldade em monitorar os alunos — *Difficulty monitoring students*
    - Falta de clareza na política da escola — *Lack of clarity in school policy*
    - Resistência dos alunos — *Student resistance*
    - Outro (especifique) — *Other (specify)*
13. Na sua percepção, quais foram os principais impactos da proibição do uso de celulares nas escolas? (Marque todas as opções aplicáveis)
    In your opinion, what were the main impacts of the cell phone ban in schools? (Select all that apply)
    (Multiple choice checkbox)
    - Aumento da concentração dos alunos em sala de aula — *Students focus better in class*
    - Redução de distrações e interrupções — *Distractions and interruptions are reduced*
    - Melhora no comportamento dos alunos — *Students' behavior has improved*
    - Maior interação presencial entre os alunos — *Students have more face-to-face interactions*
    - Menor pressão para estarem online — *Students feel less pressure to be online*
    - Redução de casos de cyberbullying — *Reduction in cyberbullying at school*
    - Menos cola durante provas ou tarefas — *Students cheat less during tests or assignments*
    - Alunos se sentem mais ansiosos ou desconectados sem o celular — *Students feel more anxious or disconnected without their phone*
    - Alunos têm mais dificuldade para contatar os responsáveis durante o dia — *Students find it harder to contact their parents or caregivers during the day*
    - Alunos burlam a regra para acessar a internet — *Students get around the ban to go online*

- Alunos ficam entediados ou inquietos nos intervalos — *Students feel bored or restless during breaks or recess*
- Alunos continuam usando o celular, mas com mais cuidado para não serem pegos — *Students use their phone just as much, but they are more careful not to get caught*
- Redução de conflitos relacionados às redes sociais — *Fewer social media-related conflicts*
- Melhora na frequência escolar — *Improved school attendance*
- Nada mudou para os alunos — *Nothing really changed for students*
- Outro (especifique) — *Other (please specify):*

14. Quando você tem conflitos com seus alunos, com que frequência eles envolvem o uso de celulares?
    When you have conflicts with your students, how much of the time does it involve their cell phone use?
    - Nunca — *Never*
    - Raramente — *Rarely*
    - Às vezes — *Sometimes*
    - Frequentemente — *Frequently*
    - Quase todas as vezes — *Almost always*

15. Houve outras mudanças importantes na sua escola entre 2023 e 2024, ou entre 2024 e 2025, que possam ter impactado o comportamento ou o desempenho dos alunos? Se sim, quais?
    Were there any other major changes in your school between 2023 and 2024, or between 2024 and 2025, that could have affected student behavior or performance? If so, what were they?
    (Open-ended text box)

16. Deixe comentários adicionais, caso deseje.
    Leave additional comments if you'd like.
    (Open-ended text box)