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The Consequences of Cellphone Restrictions in Classrooms

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Abstract

Schools are increasingly restricting cellphones worldwide amid concerns about achievement and mental health, yet causal evidence on school-level bans remains mixed. We examine cellphone restrictions in Chile before the pandemic, where teacher discretion over cellphone use generated classroom-level variation. Using administrative and survey data, we exploit cross-cohort, within-teacher, and within-student cross-subject variation in cellphone policies. Restrictions modestly reduce eighth graders' in-class recreational cellphone use but not for tenth graders, suggesting uneven compliance. They also lower eighth graders' perceived academic capability without affecting test scores. Our findings best extrapolate to decentralized policy contexts and contexts with uneven enforcement within schools.

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Keywords

cellphone bans, student outcomes

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1 Introduction

Children and adolescents are increasingly using digital technologies in the classroom. One device receiving ample attention is cellphones. Phones can facilitate pedagogy by enabling information retrieval and improving access to learning applications, particularly in settings with limited digital infrastructure. On the other hand, cellphones may impair attention and self-control by diverting scarce cognitive resources and impairing sustained concentration (Chen, Yan, Moeyaert, & Bangert-Drowns, 2025; Ward, Duke, Gneezy, & Bos, 2017). Smartphone use is also associated with poorer youth mental health (Office of the Surgeon General, 2023), with heavy social media use predicting lower psychological well-being (Twenge, 2019; Twenge & Campbell, 2018).[1] These heightened concerns about cellphones' effects on learning and mental health are prompting policymakers around the world to restrict access.[2]

Meanwhile, recent evidence on the causal effect of phone bans shows mixed results. School-level phone bans yield sizable test score gains in England and Norway (Abrahamsson, 2024; Beland & Murphy, 2016) and lead to delayed test score increases in Florida (Figlio & Özek, 2025), while a Swedish study finds no impact and can reject even small-sized improvements (Kessel, Hardardottir, & Tyrefors, 2020).[3] A recent study using UK data found that adolescents in schools implementing restrictive phone policies banning recreational phone use had similar self-reported mental wellbeing as those in schools with more permissive policies (Goodyear et al., 2025). Importantly, restrictive policies were not meaningfully associated with phone or social media use. There is also substantial variability in the amount of time adolescents spent on their phones within both restrictive and permissive schools, suggesting a wide range of implementation fidelity and compliance. This underscores the need for better understanding student responses to phone policies, as well as variation in how teachers adopt phone policies, both across and within schools.

This paper studies *classroom*-level phone policies and their consequences for student phone use and academic outcomes. We use administrative records from Chile merged with student and teacher responses to survey questions on phone policy and usage during class. The resulting dataset links classroom phone policies to students' in-class phone use for entertainment, test scores, and self-reported academic capability across subjects for eighth-grade students in 2017 and 2019 and tenth-grade students in 2017 and 2018. We identify the effect of cellphone policies using two complementary strategies: (i) within-teacher shifts in policies over time, and (ii) within-student, cross-subject variation in exposure to phone restrictions.

[1] Braghieri, Levy, and Makarin (2022) shows that the rollout of Facebook negatively impacted college student mental health and impaired academic performance.

[2] U.S. data from a December 2024 School Pulse Panel shows that over three-quarters of surveyed public schools prohibit students from using phones during any class, a reflection of state- and jurisdiction-led efforts to regulate students' cellphone use (De La Rosa, 2025). Florida was the first state to regulate cellphone use in 2023. The majority of states in 2025 have passed laws banning or regulating phones in schools (Amy, 2025).

[3] Outside of the causal literature, a large volume of studies relate social media use and mobile phone use with academic performance. See Liu, Kirschner, and Karpinski (2017) and Kates, Wu, and Coryn (2018) for a review.

We find that restricting phone access decreases the likelihood of students using cellphones in class for entertainment by 3 percentage points among eighth graders, a change of 4% relative to the sample mean. The corresponding behavioral change is larger when reframing the outcome to accommodate a higher intensity of phone disengagement—*strongly* disagreeing to using phones in class for entertainment. In contrast, there were no behavioral responses among tenth-grade students. We also show null effects on test scores under both identification strategies. Despite the absence of achievement effects, eighth graders report reduced academic self-efficacy (3%), defined as feeling capable in a given subject. This suggests a response rooted in perceived rather than demonstrated ability.

We find no evidence of heterogeneous impacts across a range of student, teacher, and school characteristics. An exception is that a relatively small group of eighth grade students taught by teachers who did not believe that phones were useful for pedagogical purposes in the baseline year were more likely to curtail in-class phone use for entertainment and exhibited larger decreases in feelings of academic capability, compared to peers taught by other teachers. These results suggest nuanced interactions between classroom context, such as teachers' attitudes towards mobile devices as instructional tools, and subsequent fidelity of implementation. Moreover, our findings are applicable to a wide set of contexts involving variation in cellphone policies across teachers within schools.

This paper makes several contributions to the existing literature. First, it builds on a small causal literature in economics examining how *school*-level cellphone bans affect student outcomes.[4] We instead focus on a common setting in which phone restrictions are not enforced very strictly or uniformly within schools. Beland and Murphy (2016) exploit the staggered adoption of high school phone bans in four English cities from 2001-2011 to show that test scores rose by about 0.07σ. Subsequent studies from Nordic countries paint a varied picture. Kessel et al. (2020) examined cellphone bans among Swedish lower secondary schools in 1997-2017 and find no detectable change in ninth-grade achievement. In contrast, Abrahamsson (2024) finds significant benefits of middle school phone bans for student well-being in Norway, including reduced bullying.[5] A recent paper using Florida administrative student records and smartphone activity data at the school-level shows that students in schools with higher smartphone activity before a 2023 statewide ban had higher test scores two years later relative to peers in schools with lower smartphone activity (Figlio & Özek, 2025). This bump in test scores is accompanied by an increase in disciplinary incidents in the first year. These nuanced and sometimes divergent conclusions may arise due to a host of reasons, including differences in enforcement policies across these school systems, different aggregation of achievement data (e.g., at the school-level in Sweden), and contrasts in how cellphones are used across these contexts and time periods.[6] Our paper finds that exposure to

[4] A much larger literature use experimental evidence to examine the effect of phones on learning. A recent review finds that distractions from cellphones worsens immediate recall (Chen et al., 2025).

[5] There are pronounced gender differences, with girls experiencing improved GPA, math scores, likelihood of pursuing an academic high school track, and reduced number of consultations for psychological symptoms.

[6] In particular, Kessel et al. (2020) emphasize that smartphones are more ubiquitous during their setting and

restrictive classroom-level phone policies has null effects on test scores, accompanied by the result that students report reduced academic capability despite no discernible change in performance.

Second, we are able to examine effects on student phone use and behavior in addition to academic outcomes. Results show that classroom phone restrictions induce a relatively small degree of behavioral change: eighth graders' in-class phone use for entertainment decreases by 4% of the baseline mean in response to phone restrictions, while tenth graders exhibit no discernible change. This mismatch between teacher intentions and actual student phone use may contribute to the null effects observed in schools with weaker compliance (Beland & Murphy, 2016) and smaller academic gains for girls in schools with lenient enforcement guidelines (Abrahamsson, 2024).[7]

Finally, our study informs a literature on the pedagogical use of technology. Studies focusing on mobile devices recognize both their potential for diverting attention and scope for facilitating learning. While findings on mobile learning applications are often mixed, evidence suggests that there is potential to improve student outcomes when embedded in well-designed and aligned curriculum with appropriate teacher support. Students randomly assigned to phones for targeted instructional activities can experience learning gains relative to peers without phones (Deng, Cheng, Ferreira, & Pavlou, 2025). In contrast, students assigned to unfettered access in the absence of instructor guidance showed decreased academic performance (Deng et al., 2025), consistent with evidence on phones' capacity to distract and negatively affect recall (Chen et al., 2025). We are able to evaluate the consequences of cellphone restrictions in a large-scale study involving diverse instructor practices towards the use of cellphones as learning aids. Findings of heterogeneous effects along this margin suggest that compliance with cellphone restrictions and subsequent effects on learning outcomes can interact in nuanced ways with the existing pedagogical context.

2 Background

2.1 Chilean school system

Chile's K-12 education system serves over 3.6 million students. In 2024, there were a total of 11,048 schools for an average school size of just under 325 pupils (Arias, Figueroa, Iturrieta, & Pérez, 2025). The schooling system is structured into three levels: preschool, primary (grades 1-8), and secondary (grades 9-12) (Ministerio de Educación, 2024).

Students typically progress through a common core curriculum through grade 10, with limited tracking or ability grouping. Students grouped into the same homeroom usually take all classes

Swedish schools have integrated education technologies like smartphones into classroom instruction at greater rates than the U.K. Figlio and Özek (2025) is also unique in that it estimates the causal effect of school cellphone bans post-pandemic.

[7] Low compliance bans in Beland and Murphy (2016) are defined by headteachers responding to the extent to which the ban was adhered to by students on a seven-point scale (1 meaning "not at all" and 7 meaning "completely") with a three or below. More lenient policies in Abrahamsson (2024) are defined as schools allowing phone use during breaks instead of collected before classes or banned outright from campus.

together, ensuring stable peer exposure even if teachers change across subjects. Starting grade 11, students take electives and formally choose between an academic, university-preparatory track and a technical-professional track focused on vocational skills.

The national curriculum requires eighth-grade students to take Language, Mathematics, Natural Science, History/Social Science, English, Physical Education, Arts, Technology, and (optionally) Religion. In tenth grade, Natural Science is divided into Biology, Chemistry, and Physics, and students receive instruction in Philosophy. Mathematics and Language typically receive the most instructional time.

Governance in this K-12 education system combines centralized with local decision-making. The Ministry of Education establishes national curriculum standards, teacher certification requirements, and education financing policies. The Education Quality Agency monitors student learning through the Education Quality Measurement System (SIMCE), standardized tests administered to students in grades 2, 4, 6, 8, 10, and 11 on a rotating basis across subjects. These assessments provide information to parents and policymakers about school performance. Despite centralization in some domains, individual schools retain substantial autonomy over pedagogy, disciplinary procedures, rules governing student behavior, and other internal policies such as the use of digital devices for pedagogical and personal use.

2.2 Cellphone usage and regulation in Chilean schools

Smartphone ownership among Chilean students is pervasive. By adolescence, nearly nine in ten students own a smartphone and most bring it to school daily (Leiva & Kimber, 2018; Ministerio de Desarrollo Social y Familia, 2017). Daily cellphone use among Chilean adolescents also varied notably by socioeconomic status: 8 hours for low-income and 5 hours for high-income students (Leiva & Kimber, 2018). Teenagers use cellphones primarily for WhatsApp, social media, and listening to music.

Teachers frequently use cellphones in class for pedagogical purposes. Sixty-six percent of students report in-class activities that require cellphone use (UNICEF-MINEDUC, 2023). Students report using phones to search for class-related information, participate in teacher-organized activities, and photograph the board or presentations. These figures are consistent with our data: 20% of teachers report using cellphones for pedagogical purposes many times or always, and more than half of teachers use them a few times. Yet despite their instructional role, 85% of teachers agree that cellphones distract students during class.

School policies on cellphone usage during the period we analyze (2017-2019) were highly decentralized, with considerable discretion left to individual schools—and often to individual teachers—over whether and how to regulate cellphone use (Ministerio de Educación, 2019). Our data confirm this decentralized pattern, as we observe variation in cellphone policies, even within schools. It is only after the pandemic that we observe more systematic discussion and regulation regarding

cellphones.[8]

Qualitative evidence suggests that teachers face substantial difficulties enforcing classroom cellphone policies. Teachers report that some students do not comply, partly because they perceive teachers lack authority to confiscate cellphones. Teachers also cite legal ambiguity surrounding cellphone confiscation and that limited parental support can further undermine enforcement efforts (La Tercera, 2023). As a result, cellphone use has become a frequent source of conflict between teachers and students. Indeed, a school coordinator notes that classroom cellphone use is the most common reason for disciplinary notes among students (La Tercera, 2023). Consistent with this qualitative evidence, Leiva and Kimber (2018) find that while 88% of students report that their schools have rules governing cellphone use, 71% report using phones in class, and 22% report their cellphone has been confiscated at school—only slightly below the share reporting confiscation at home (28%).

3 Data and Descriptive Statistics

3.1 Data

We leverage administrative data from two sources, focusing on eighth-grade students in 2017 and 2019 and tenth-grade students in 2017 and 2018. The first source is the Education Quality Agency, which administers standardized tests at the end of each academic year (December). Only selected grades and subjects are tested each year. Accordingly, we use Math scores for eighth graders and Language scores for tenth graders when exploiting variation over time, as those are the subjects observed over time for each grade (see Figure A1). When exploiting cross-subject variation, we focus on 2019 for eighth-grade students and 2018 for tenth-grade students as we observe both Math and Reading scores.

The Agency also distributes surveys to teachers and students during test administration. Teachers of each tested subject respond to them, which allows us to exploit cross-subject variation in cellphone policy. Our key treatment variable is teachers' self-reported frequency of allowing cellphone use in class, measured on a four-point scale: (1) Never or almost never, (2) A few times, (3) Several times, and (4) Always or almost always. In the analysis, we report results both combining and separating the first two categories, using the latter two as the comparison group. Teachers are also asked about the extent to which they use cellphones in class to support pedagogical activities, measured on the same four-point Likert scale as described above, as well as their gender, years of experience in the school, and degree attainment. Response rates are higher for tenth-grade teachers than eighth-grade teachers (70% vs. 55%).

Student surveys provide additional outcomes beyond test scores, including self-reported cellphone use in class for entertainment and self-assessed subject capability, each measured on a four-

[8]For instance, there were seven motions to regulate cellphone usage in educational contexts at the national level. Only two were filed during the pre-pandemic period, while the rest between 2023 and early 2024.

point Likert scale.[9] While the academic capability measure is at the subject-year level, students' self-reported in-class use of cellphones is not. As such, students may consider their cellphone use across all courses when answering this question, rather than pertaining to a specific subject or class. The content of the teacher and student questionnaires changes across years. The absence of items on teachers' cellphone restrictions and students' in-class cellphone use in particular limits the set of grades and periods available for analysis.

The second source of data is the Ministry of Education, which provides student demographics, including age, gender, and indicators of socioeconomic vulnerability. Students in the bottom tercile of the national socioeconomic distribution are classified as "priority," while those not qualifying as a priority but who are below the 80th percentile are classified as "preferential."[10] Administrative teacher records allow us to link students to their subject teachers based on classroom assignments. After merging the Ministry of Education data with the Education Quality Agency data, the sample includes 172,865 eighth-grade students taught by 4,984 teachers and 187,343 tenth-grade students taught by 5,685 teachers.

3.2 Descriptive statistics

Table 1 shows sociodemographics, phone use, and academic outcomes for the student sample and describes the gender and experience profiles of teachers, alongside their cellphone policies and attitudes towards phones as an instructional aid. The mean ages for students are 13 and 15 for the eighth- and tenth-grade samples, respectively, and approximately two-fifths are priority students. 70% of eighth-grade students report not using phones in class for entertainment, compared to 56% in the tenth-grade sample, likely reflecting different patterns of cellphone use across age groups. Average test scores of 0.1-0.2σ show that students with non-missing listed attributes are slightly positively selected, with three-quarters or more expressing that they feel capable in a given subject.

Panel B shows a higher share of male teachers in eighth grade math (40%) compared to tenth grade language (26%). The distribution of teacher experience is comparable across grades. The vast majority of teachers impose some restrictions on cellphones, with 94% of eighth grade teachers prohibit phones entirely or allow only a few times, compared to 88% among tenth grade teachers. Most teachers used cellphones at least a few times to support pedagogical activities in the baseline year, underscoring the broad interest in incorporating phones as a low-cost instructional tool.

[9] Students are asked how much they agree or disagree with the statement "I use my cellphone in class to entertain myself." Possible responses are: 1) Strongly disagree, 2) Disagree, 3) Agree, and 4) Strongly agree. They are also asked how capable they feel in learning each tested subject, with possible answers as: 1) Not at all capable, 2) Not very capable, 3) Quite capable, and 4) Very capable. Response rates are slightly higher for eighth-grade than tenth-grade students (90% vs. 85%) and vary little across questions.

[10] Alternative criteria to be considered a priority student are belonging to the Security and Opportunities Subsystem (a component of the Social Protection System) or FONASA (public health insurance) bracket A. If families do not meet any of these criteria, they may be eligible based on their income, parents' schooling, or the rurality and poverty of the municipality in which they live.

4 Empirical Approach

4.1 Empirical specifications

We rely on two complementary sources of variation in cellphone policies to identify their effects on student outcomes. The first leverages policy changes for the same teacher over time. This within-teacher approach requires sufficient variation in the extent of in-class phone restrictions within a period of one to two years. Table A1 shows that 10-15% of teachers across both grades recorded different responses to the question on how often they allow the use of cellphones in class. Teachers are similarly likely to tighten or relax their policies on student phone use, such that there is no systematic shift towards a more restrictive or lax stance during this pre-pandemic period in Chile.[11]

The second approach relies on the fact that each student is tested in two subjects annually. We are able to observe the cellphone policies of each subject teacher. This sets up a within-student, cross-subject design that allows us to account for individual characteristics such as academic motivation and family background alongside school factors that commonly influence student outcomes across subjects. 11-16% of students experience in-class phone policies characterized as different by their teachers (Table A2). Within this group, there are no marked differences in the likelihood of restricting cellphones between math and reading teachers.

A key distinction between these two approaches is that the within-student approach demands cellphone policy variation across classrooms, such that these findings inform contexts with non-uniform cellphone policies within schools. While this rules out settings with strict school cellphone bans enforced uniformly across classrooms, our findings are still informative if enforcement is uneven. In contrast, the within-teacher model can accommodate strict school cellphone bans, since changes over time may reflect either individual teacher discretion or shifts in school-wide policy.

Within-teacher, over time. Teacher-student linkages in the data permit tracing individual teachers' classroom policies to student academic outcomes. We focus on test scores and self-reported academic capability for student i assigned to teacher j in subject k (Φ_{ijkst}). $\textit{RestrictPhone}_{jt}$ indicates whether the teacher allows cellphones in class "never or almost never" or "a few times", relative to "lots of times" or "always or almost always."

$$\Phi_{ijkst} = \boldsymbol{\beta}' \textit{RestrictPhone}_{jt} + \mathbf{X}'_{it}\boldsymbol{\omega} + \mathbf{Q}'_{jt}\boldsymbol{\Gamma} + \kappa_{js} + \pi_t + \epsilon_{ijkst}, \tag{1}$$

We account for student and teacher characteristics including student age, gender, indicators for

[11] Observable characteristics such as teacher experience are not predictive of the type of classroom cellphone policies. For example, distributions of experience are similar across teachers switching to both strict and lax policies, with no significant differences in propensity for a given policy within each experience bin.

socioeconomic status, lagged standardized test scores, teacher gender, and experience (X_{it}, Q_{jt}). κ_{js} are teacher–school fixed effects that absorb pedagogical style and other teacher attributes that are constant within a given school. To avoid conflating these with changes in school environments, we rely on teachers who remain in the same school across cohorts. Year fixed effects π_t control for factors affecting all schools, namely national curricular changes. Identification hinges on eighth-grade teachers changing policies between 2017 and 2019 and tenth-grade teachers doing so between 2017 and 2018.

Within-student, cross-subject. The second approach compares outcomes corresponding to contemporaneous classes with different phone policies taken by the same student:

$$\Theta_{ijkst} = \boldsymbol{\beta}' \mathit{RestrictPhone}jt + \mathbf{X}'_{it}\boldsymbol{\omega} + \mathbf{Q}'_{jt}\boldsymbol{\Gamma} + \varphi_{it} + \eta_k + \xi_{ijkst}, \tag{2}$$

Student-year fixed effects – φ_{it} – absorb attributes that are constant across two academic subjects in a given year. These can include but are not limited to common individual characteristics, household shocks, and shared teaching practices. The model also includes subject indicators (η_k) to account for systematic differences in testing and self-reported academic capability across subjects. For robustness, we augment the model with subject-by-school district fixed effects to absorb localized factors that can shape subject-specific learning. We estimate all models separately for eighth grade in 2019 and tenth grade in 2018, since those are the two years for which we have adequate information on classroom-level cellphone policies, math and language test scores, and self-assessed academic capability (Figure A2). Note that the presence of any spillover effects in this setting generates ambiguous net impacts. Students may be inclined to either curb (or extend) their phone use in the relatively lax classroom in response to restrictions in the other classroom, and this could lead to underestimates (or overestimates) of the policy effects. Despite this limitation, later we show that our findings are consistent between our two approaches.

4.2 Assumptions and validity

Our identification strategy rests on the assumption that variation in classroom phone policies are not systematically correlated with unobserved determinants of student outcomes. For the within-teacher design, this requires that classroom policy changes over time are not in response to shifting student composition or other unmeasured factors that also affect learning. For the within-student, cross-subject design, validity hinges on students not sorting into particular classrooms based on teachers' phone policies or correlated teaching practices. Specifically, high-achieving students or those self-reporting high levels of academic capability cannot disproportionately enroll in classrooms featuring particular restrictive or lax policies on phones.

Features of Chilean schools that alleviate some of these concerns include the standard organization of students around homerooms in which the same group of students take classes together,

ensuring peer stability. The general curriculum continues through grade 10, thereby reducing the scope of elective opportunities. Still, we control for lagged subject-specific achievement in the case of ability grouping, and rerun our results by incorporating student attributes such as lagged attendance rate and whether the student was retained in the previous academic year, alongside peer characteristics such as average lagged test scores and shares from economically disadvantaged backgrounds to account for any variation in student composition. We also include covariates on classroom pedagogical approaches from teacher surveys to ensure that results are driven by changes in phone policies rather than contemporaneous shifts in instruction, such as the frequency with which teachers evaluate student learning. Next, we test whether observable baseline characteristics predict the subsequent adoption of more restrictive or more permissive policies in the within-teacher model. The final validity check probes for differences in student composition leading up to changes in cellphone policies using an event-study framework. For this, we focus on lagged test scores in elementary school and socioeconomic status given data limitations in our outcome variables.

5 Results

5.1 Effects on student phone use

We first assess whether phone restrictions alter students' self-reported in-class phone use. Table 2 shows that allowing phones at most a few times decreases the likelihood that eighth graders use their phones for entertainment during class. The probability of disagreeing with the statement "I use my cellphone in class to entertain myself" rises by 3 percentage points (p.p.), or 4% of baseline phone use. In contrast, effects among tenth graders are small and statistically indistinguishable from zero.

Several factors may account for differential responses across grade levels. Eighth graders are younger, and potentially more compliant with classroom rules. Teachers are more likely to impose restrictive phone policies towards this group, compared to their tenth grade counterparts, such that using phones for fun during class is perceived as less acceptable for eighth graders. There are also sample differences, including restricting only to students in math classes among eighth graders and language classes for tenth graders.

We then verify our results using more intensive definitions of both treatment and outcome variables. When separating phone restrictions into never allowing phones vs. allowing a few times, we find that both lowered self-reported phone use at similar magnitudes, relative to classrooms where phones are used lots of times or almost always. An alternative binary outcome—whether students *strongly disagree* with in-class phone use—shows that phone restrictions increase this margin by 5 p.p. for eighth graders and 2 p.p. for tenth graders (Table A3), corresponding to over 15% and 8% of the mean. These behavioral responses are modest in magnitude, but the absence of a stronger relationship is likely based on how the outcome variable is defined: it is not subject-

specific. Students may be responding based on perceptions of phone restrictions across all classes, which is less responsive to policy changes in a single math or language class.[12]

5.2 Effects on student academic outcomes

Next, we examine whether phone restrictions affect student performance. Cellphone use can depress academic achievement by serving as a distraction (Beland & Murphy, 2016; Chen et al., 2025), suggesting attention costs as a key mechanism (Loewenstein & Wojtowicz, 2025). Across both within-teacher and within-student specifications, Table 3 shows no detectable effects of phone restrictions on standardized test scores. The estimates are close to zero and we can rule out gains of 0.03-0.06σ.

Beyond test scores, we also observe students' perceptions of their academic capabilities for each tested subject. Table 4 shows that phone restrictions reduce the extent to which students feel capable, particularly in grade 8. Restrictive policies lower the probability that students report feeling "quite capable" or "very capable" by over 2 percentage points, or 3% relative to the sample mean. These effects are somewhat weaker among tenth graders. Our results indicate that cellphone restrictions can erode students' subjective confidence in their academic ability even when there are no measurable effects on actual performance.

Interpretation of these results requires taking into context the norms and policy landscape surrounding in-class phone use. Our setting crucially involves classroom phone policies that may vary across subjects within schools. This sets the study apart from those focused on school-level bans that implicitly are more uniformly imposed across classrooms. Since our within-student specification requires a juxtaposition of classes with more restrictive and lax policies, the experience of having used phones in another class may shape the subjective feeling about academic abilities. Another consideration is the high level of in-class phone use for pedagogical purposes in Chile, where 50-65% of teachers believe that cellphones are a useful tool for pedagogy. Our null test score finding echoes the conclusions of Kessel et al. (2020) using Swedish data, which may have deviated from the higher test scores found in English secondary schools (Beland & Murphy, 2016) because schoolwork-related phone usage made phone bans ineffective.

5.3 Robustness

We undertake a number of additional analyses to probe the possibility that variation in classroom phone policies is related systematically to other determinants of student academic outcomes. First, we control for lagged student attendance and whether they were retained in the previous year in consideration of the possibility that classroom phone policies are endogenous to students' baseline behavioral and attainment attributes. We then fold in peer characteristics including mean

[12]Because student responses on in-class cellphone use are not subject-specific, we cannot analyze them within the student fixed effects model.

lagged standardized test scores and shares of male peers, peers who are from economically disadvantaged backgrounds (priority or preferential). Results on student phone use, test scores, and academic capability are highly stable to the inclusion of these covariates (Tables A4-A6). We furthermore include classroom-level variables on pedagogical approaches to account for other instructional dimensions that may be co-determined with cellphone access. Point estimates remain very similar.

In the baseline specification, the within-student, cross-subject strategy includes subject fixed effects. Even though schools in our sample use the same national curriculum and standardized tests, there may be variation across municipalities in subject-specific curriculum and teaching. Therefore, we run a specification using subject-municipality fixed effects. Table A7 shows qualitatively similar results for test scores and stronger evidence of lower perceived academic capability, particularly among tenth graders.

Next, we examine whether observable characteristics predict changes in cellphone policy in the within-teacher model. An association could suggest that teachers' phone policies are changing in response to evolving student composition or other determinants of student outcomes. Table A8 incorporates a vector of variables on baseline mean classroom achievement and sociodemographic student composition, as well as changes along these dimensions. Low F-statistics show little evidence that policy shifts are systematically associated with observable student or teacher attributes across both grades.

Finally, we assess pre-trends in Figure A3 and Figure A4. Because we do not observe students' test scores or self-assessed academic capability far enough back in time for 8^{th}- and 10^{th}-grade teachers, we instead use two teacher-level outcomes that are available up to four periods prior to the policy change: the mean lagged 4^{th}-grade subject score (Math for 8^{th} grade and Reading for 10^{th} grade) and the fraction of priority students. These variables are stable pre-treatment characteristics and provide informative proxies for trends in students' academic preparation and socioeconomic composition. We compute the outcomes by averaging each teacher's classroom characteristics across all the classrooms they teach. We then compare trends between teachers who change their cellphone policy and those who do not in an event-study framework. We find no evidence of differential pre-trends across grades and outcomes. This pattern supports the identifying assumption that, absent the policy change, teachers who eventually change their cellphone policy and those who do not would have followed similar trajectories in these characteristics prior to the policy change.

5.4 Heterogeneity

The results may disguise more nuanced patterns across school, teacher, and student attributes. We first evaluate whether phone bans induce differential behavioral and academic responses across types of teachers and schools. Since the within-teacher model leverages variation from shifts in phone policies that are either more lax or restrictive, we assess whether the response is asymmetric

according to the direction of policy shift. We also stratify by the teacher's use of cellphones for pedagogical purposes in the baseline year (2017), since the fidelity of implementing phone bans might vary by the extent to which phones are embedded as an instructional technology. Finally, we test for differential effects by lower vs. higher-achieving schools.

Figure 1 shows the effects separately estimated for each subgroup across grades and three outcomes: whether students report using their phones in class for entertainment, test scores, and feeling capable in a subject.[13] We find no evidence that changes in phone use or self-reported academic capability were driven by a shift towards either more lax or restrictive policies. Effects across schools of different average performance levels were also indistinguishable from each other. The only difference was that students taught by teachers who never or almost never deployed cellphones for pedagogical purposes appeared less likely to use phones for entertainment in response to phone restrictions, relative to those taught by teachers more amenable to using phones for instruction. This larger behavioral change was also reflected in feelings about academic capability: these students also exhibited larger decreases in this form of self-efficacy compared to those taught by teachers who used phones more frequently for pedagogical purposes. These findings suggest that teachers' habits and attitudes towards pedagogical phone use may matter for how faithfully they implement phone restrictions and for downstream effects. Next, we examine whether results depend on the distinctiveness of teachers' cellphone policies relative to peers'. Table A9 shows no evidence that the effects of phone restrictions vary significantly by whether the majority of same school and grade teachers contemporaneously restrict access, although we are constrained by limited variation due to only observing the phone policies of a subset of teachers.

Finally, we examine whether results vary by student attributes, specifically stratifying by gender, socioeconomic background, and prior achievement. Panel A in Figure A5 shows no evidence of differential responses in in-class phone use along these dimensions. Correspondingly, there also lacks systematic evidence of any differential effects along these attributes for both academic outcomes.

6 Conclusion

This paper provides new evidence on the impact of *classroom-level* cellphone restrictions on student outcomes. Phone policies during the late 2010s were highly decentralized in Chilean schools, leading to cross-classroom variation in approaches to mobile devices. A novel combination of administrative and survey data enables two complementary strategies that leverage within-teacher, over time and within-student, cross-subject variation in phone policies. This contrasts with changes in school-level measures of cellphone restrictiveness common in the literature. We show that cell-

[13] We cannot estimate effects on in-class cellphone use leveraging the within-student approach because our measure is not subject-specific. Thus, estimates shown for this outcome rely on within-teacher variation over time. We also limit to the teacher FE model when stratifying by pedagogical cellphone use in order to use responses from the baseline year of 2017.

phone restrictions have no discernible effects on test scores, although students report modest reductions in perceived learning capabilities. The effects are concentrated among eighth grade students, whose self-reported use of cellphones for in-class entertainment also declines when more restrictive phone policies are adopted.

Several implications follow from these findings. First, the modest behavioral responses we observe—particularly among older students—highlight the challenges of enforcing *classroom*-level restrictions. This limited compliance likely explains the lack of test score gains, in contrast to schoolwide bans with stricter enforcement studied in the existing literature. Also notable is the pervasive use of phones as a pedagogical tool, which further complicates compliance and sets our context apart from classrooms in higher-income countries that may readily shift to other forms of classroom technology.

The results on lower academic capability show an unintended consequence of cellphone restrictions. The experience of differentiated phone access across classrooms and subjects may shape one's perceived academic capability. More research can shed light on whether this result is replicable in settings with stricter and more uniform enforcement of phone curbs. Future work can also investigate persistent effects on academic and behavioral outcomes.

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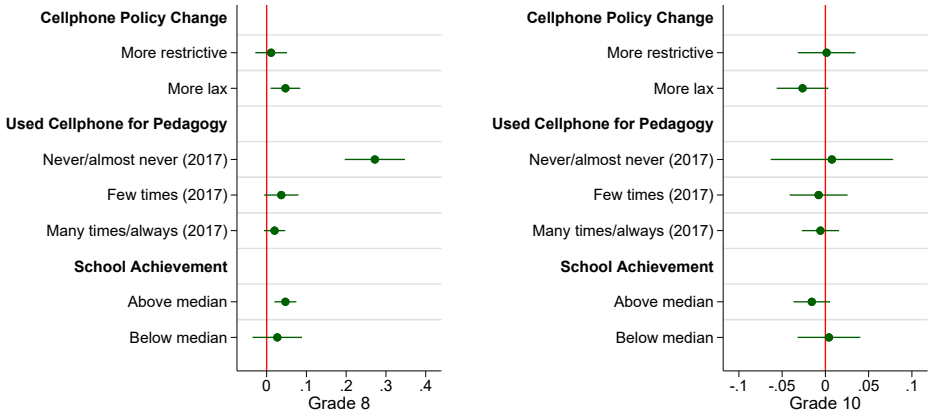
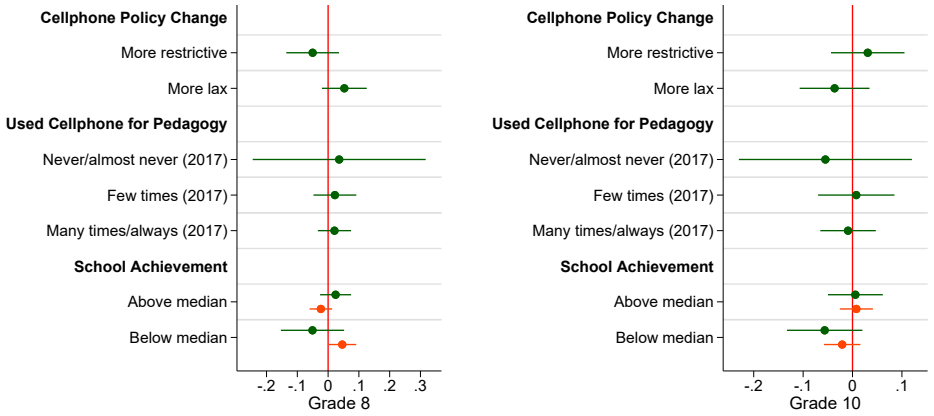
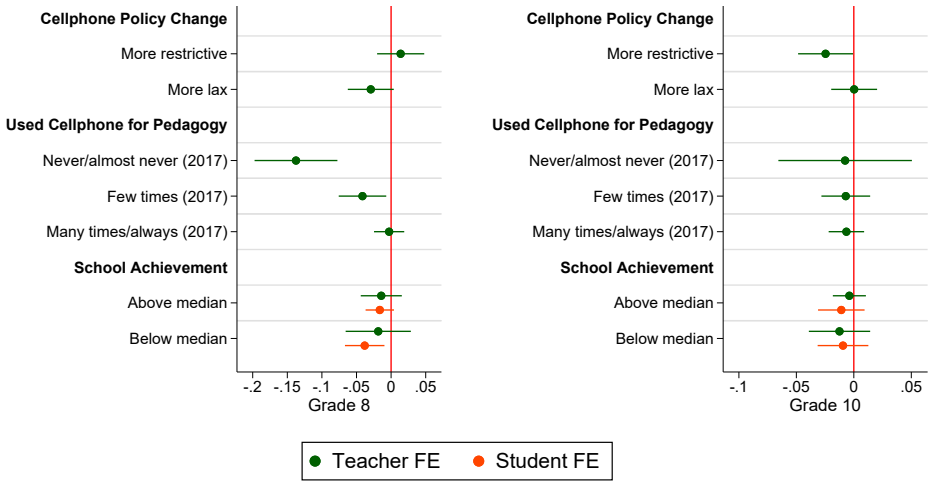
Figures



(a) Use Phone in Class to Entertain Oneself: Disagree



(b) Test scores



(c) Feeling Capable

Figure 1: Heterogeneity by Teacher and School Characteristics

Tables

Table 1: Descriptive Statistics

	(1)	(2)
Panel A. Student characteristics		
Male	0.48 (0.50)	0.49 (0.50)
Age	13.2 (0.43)	15.2 (0.46)
Priority student	0.38 (0.49)	0.41 (0.49)
Preferential student	0.30 (0.46)	0.31 (0.46)
Using cellphones in class to entertain oneself:		
Disagree or strongly disagree	0.70 (0.46)	0.56 (0.50)
Strongly disagree	0.34 (0.47)	0.18 (0.38)
Test scores	0.24 (0.98)	0.13 (0.99)
Feeling capable in subject	0.73 (0.44)	0.85 (0.35)
N	172,865	187,343
Panel B. Teacher characteristics		
Male	0.40 (0.49)	0.26 (0.44)
Teaching experience:		
<1 year	0.07 (0.26)	0.12 (0.33)
1-2 years	0.06 (0.24)	0.05 (0.22)
3-4 years	0.09 (0.29)	0.10 (0.30)
5-9 years	0.26 (0.44)	0.24 (0.43)
10-14 years	0.18 (0.38)	0.15 (0.35)
15-19 years	0.08 (0.27)	0.09 (0.28)
20+ years	0.26 (0.44)	0.26 (0.44)
Cellphone policy:		
Allow cellphones: Never	0.62 (0.48)	0.35 (0.48)
Allow cellphones: A few times	0.32 (0.47)	0.53 (0.50)
Used cellphone to support pedagogy (2017):		
Never or almost never	0.32 (0.47)	0.12 (0.32)
A few times	0.50 (0.50)	0.57 (0.50)
Many times or always	0.17 (0.38)	0.32 (0.46)
N	4,984	5,685
Sample	Grade 8	Grade 10

Notes: Student-year sample limited to those with non-missing listed attributes. It includes students in math classes in 8th grade and students in reading classes in 10th grade. Teacher-year sample limited to teachers with non-missing responses across attributes. Grade 8 sample includes math teachers only, while grade 10 sample includes reading teachers only.

Table 2: Effects of Classroom Phone Restrictions on Student Phone Use

	I use my cellphone in class to entertain myself: Disagree			
	(1)	(2)	(3)	(4)
Never Allow or Allow a Few Times	0.030**	-	-0.006	-
	(0.014)	-	(0.011)	-
Never Allow	-	0.028*	-	-0.002
	-	(0.015)	-	(0.012)
Allow a Few Times	-	0.030**	-	-0.008
	-	(0.015)	-	(0.011)
Teacher-school fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
N	173,146	173,146	199,787	199,787
Sample	Grade 8		Grade 10	

Notes: Outcome is an indicator for disagreeing or strongly disagreeing with the statement "I use my cell phone in class to entertain myself." Omitted category is lots of times or always/almost always. All specifications include student age, gender, socioeconomic attributes, lagged test scores, teacher gender and experience. Standard errors are clustered at the teacher-school level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Effects of Classroom Phone Restrictions on Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Teacher FE Model				Student FE Model			
Never Allow or Allow a Few Times	0.007	-	-0.003	-	-0.012	-	0.005	-
	(0.026)	-	(0.027)	-	(0.018)	-	(0.016)	-
Never Allow	-	0.007	-	0.002	-	0.005	-	0.005
	-	(0.028)	-	(0.029)	-	(0.019)	-	(0.018)
Allow a Few Times	-	0.006	-	-0.005	-	-0.026	-	0.005
	-	(0.027)	-	(0.027)	-	(0.019)	-	(0.017)
Teacher-school fixed effects	✓	✓	✓	✓				
Year fixed effects	✓	✓	✓	✓				
Student-year fixed effects					✓	✓	✓	✓
Subject fixed effects					✓	✓	✓	✓
N	178,396	178,396	199,604	199,604	148,340	148,340	169,618	169,618
Sample	Grade 8		Grade 10		Grade 8		Grade 10	

Notes: Outcome is test scores standardized at the subject-grade-year level. Omitted category is lots of times or always/almost always. All specifications include student age, gender, socioeconomic attributes, lagged test scores, teacher gender and experience. Standard errors are clustered at the teacher-school level for the teacher FE model, and school-year for the student FE model. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effects of Phone Restrictions on Students Feeling Capable in Subject

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Teacher FE Model				Student FE Model			
Never Allow or Allow a Few Times	-0.023*	-	-0.007	-	-0.022**	-	-0.011	-
	(0.014)	-	(0.007)	-	(0.010)	-	(0.009)	-
Never Allow	-	-0.024	-	-0.006	-	-0.026**	-	-0.024**
	-	(0.015)	-	(0.008)	-	(0.010)	-	(0.009)
Allow a Few Times	-	-0.022	-	-0.008	-	-0.019*	-	-0.003
	-	(0.014)	-	(0.008)	-	(0.011)	-	(0.009)
Teacher-school fixed effects	✓	✓	✓	✓				
Year fixed effects	✓	✓	✓	✓				
Student-year fixed effects					✓	✓	✓	✓
Subject fixed effects					✓	✓	✓	✓
N	178,882	178,882	202,058	202,058	153,236	153,236	178,550	178,550
Sample	Grade 8		Grade 10		Grade 8		Grade 10	

Notes: Outcome is an indicator for student responding "quite capable" and "very capable" to the question "How capable do you feel when it comes to learning in [subject]?". Other possible responses are "not very capable" and "not capable." All specifications include student age, gender, socioeconomic attributes, lagged test scores, teacher gender and experience. Standard errors are clustered at the teacher-school level for the teacher FE model, and school-year for the student FE model. *** p<0.01, ** p<0.05, * p<0.1.

Appendix

		Reading	Math
Grade 8	2017		✓
	2019	✓	✓

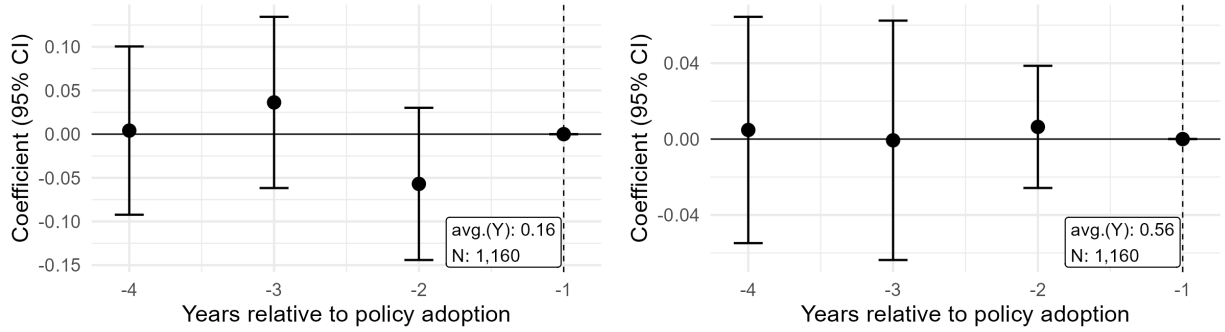
		Reading	Math
Grade 10	2017	✓	
	2018	✓	✓

Figure A1: Source of variation for teacher FE model

		Reading	Math
Grade 8	2017		✓
	2019	✓	✓

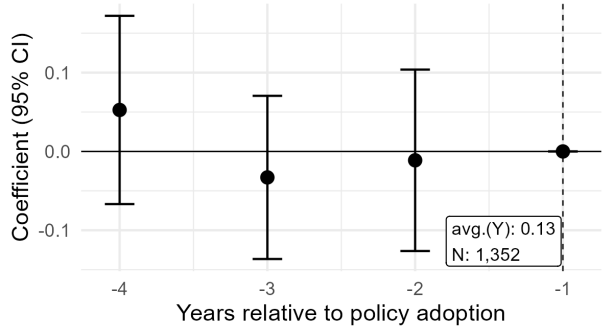
		Reading	Math
Grade 10	2017	✓	
	2018	✓	✓

Figure A2: Source of variation for student FE model

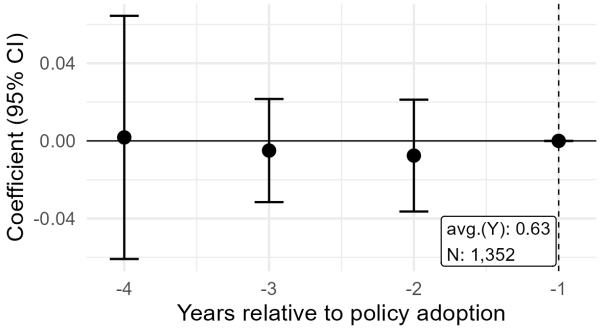


(a) Lagged 4^{th}-grade scores – 2018 as treatment year

(b) Priority student – 2018 as treatment year



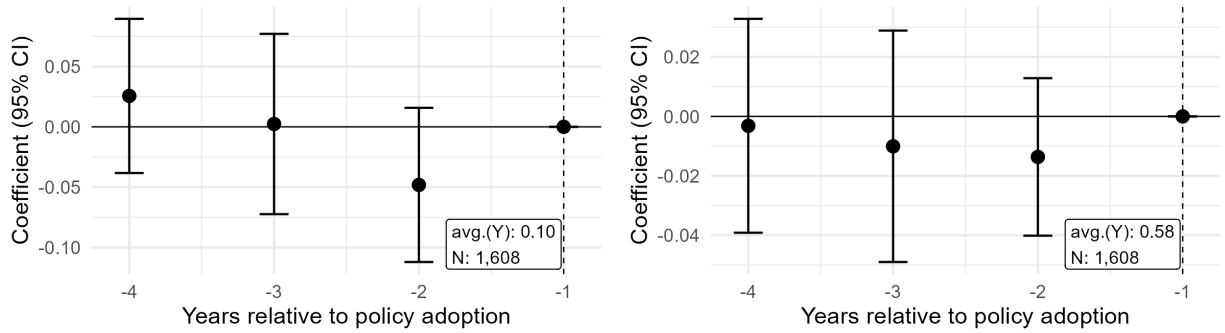
(c) Lagged 4^{th}-grade scores – 2019 as treatment year



(d) Priority student – 2019 as treatment year

Figure A3: Pre-trends test in Grade 8

Notes: This figure presents the pre-trends analysis. Given that we observe 8th teacher policies in 2017 and 2019, we show results assuming that the policy was adopted in 2018 and 2019. The sample includes teachers who report their cellphone policy in both periods of analysis. For these teachers, we track their classrooms up to four years prior to the policy change and compute teacher-level outcomes by averaging each teacher's classroom characteristics across all the classrooms they teach. Teachers who change their cellphone policy between periods constitute the treatment group; those who do not change their policy form the control group. To increase statistical power, we pool teachers moving toward a more restrictive policy with those moving toward a more flexible policy. We run the following regression separately by grade: $Y_{jkst} = \sum_{p\in\{-4,-3,-2\}} \beta_p \, 1\{E_{jkst} = p\} + \kappa_{js} + \pi_t + \epsilon_{jkst}$, where Y_{jkst} is the mean lagged 4th-grade score (Math for 8th grade and Reading for 10th grade) or the fraction of priority students in teacher j's classrooms for subject k, school s, and year t. The variable p indexes years relative to policy adoption. The specification includes teacher-school fixed effects κ_{js} and year fixed effects π_t. Standard errors are clustered at the teacher level, and 95% confidence intervals are reported. We additionally report the mean of the dependent variable and the number of teacher-year observations.

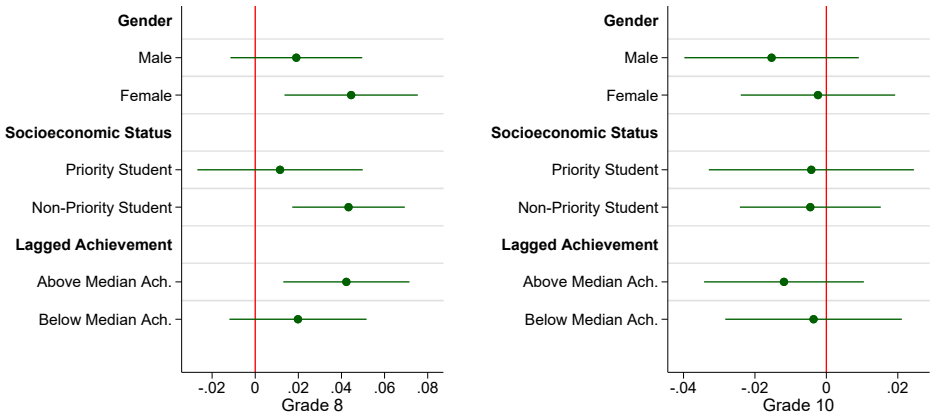


(a) Lagged 4^{th}-grade scores

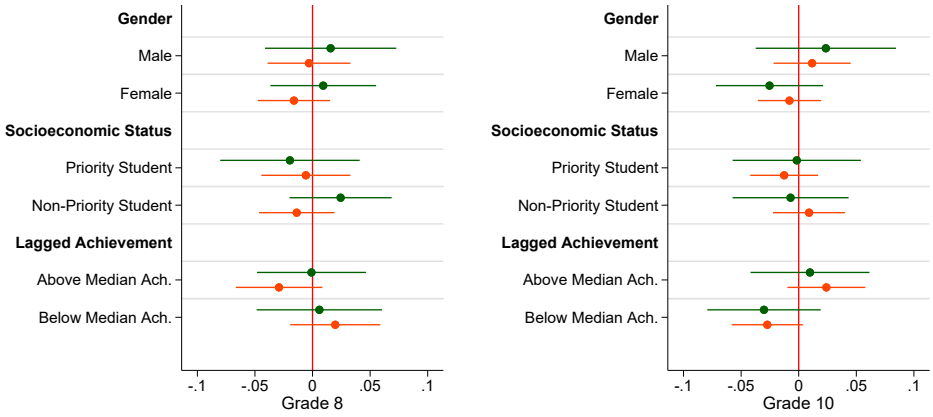
(b) Priority student

Figure A4: Pre-trends test in Grade 10

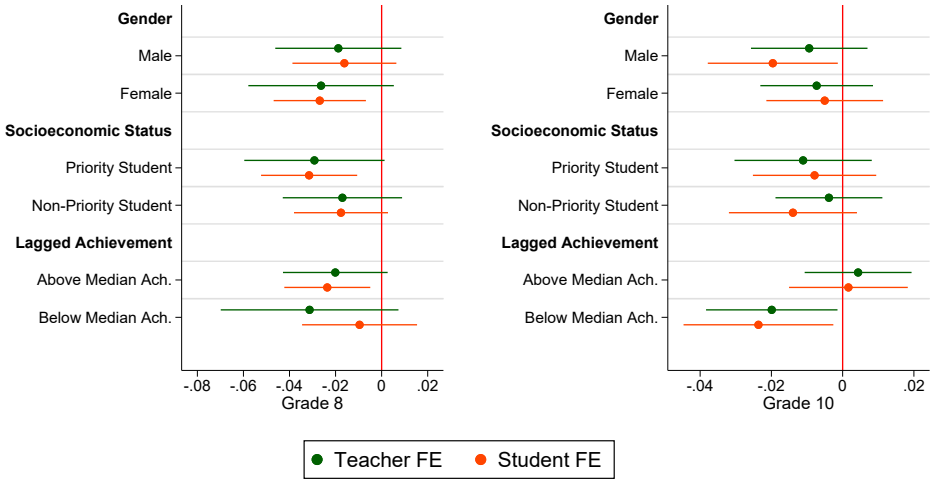
Notes: This figure presents the pre-trends analysis. The sample includes teachers who report their cellphone policy in both periods of analysis. For these teachers, we track their classrooms up to four years prior to the policy change and compute teacher-level outcomes by averaging each teacher's classroom characteristics across all the classrooms they teach. Teachers who change their cellphone policy between periods constitute the treatment group; those who do not change their policy form the control group. To increase statistical power, we pool teachers moving toward a more restrictive policy with those moving toward a more flexible policy. We run the following regression separately by grade: $Y_{jkst} = \sum_{p \in \{-4,-3,-2\}} \beta_p \, 1\{E_{jkst} = p\} + \kappa_{js} + \pi_t + \epsilon_{jkst}$, where Y_{jkst} is the mean lagged 4$^{\text{th}}$-grade score (Math for 8$^{\text{th}}$ grade and Reading for 10$^{\text{th}}$ grade) or the fraction of priority students in teacher j's classrooms for subject k, school s, and year t. The variable p indexes years relative to policy adoption. The specification includes teacher-school fixed effects κ_{js} and year fixed effects π_t. Standard errors are clustered at the teacher level, and 95% confidence intervals are reported. We additionally report the mean of the dependent variable and the number of teacher-year observations.



(a) Use Phone in Class to Entertain Oneself: Disagree



(b) Test scores



(c) Feeling Capable

Figure A5: Heterogeneity by Student Characteristics

Table A1: Within-Teacher Variation in Cellphone Policy

	(1)	(2)
Switch to a stricter policy	0.04	0.07
No switch	0.91	0.85
Switch to a more flexible policy	0.05	0.08
N	946	1,282
Sample	Grade 8	Grade 10

Notes: Sample of unique teachers constructed using teachers who had non-missing responses to the survey question on classroom cellphone policy in 2017 and 2019 for the grade 8 sample, and 2017 and 2018 for the grade 10 sample. A stricter policy denotes when teachers shift from allowing students to use phones in class lots of times or always to allowing phone use a few times or never in class. Switching to a more flexible policy involves the reverse direction.

Table A2: Within-Student and Year, Cross-Subject Variation in Cellphone Policy

	(1)	(2)
Same policy in both subjects	0.89	0.84
Math is more restrictive than reading	0.06	0.09
Reading is more restrictive than math	0.05	0.06
N	77,040	90,196
Sample	Grade 8	Grade 10

Notes: Sample of unique students is limited to 2019 for 8th graders and 2018 for 10th graders. A stricter policy entails a classroom that allows students to use their phones never or only a few times, compared to a classroom that allows phone use lots of times or always.

Table A3: Effects of Classroom Phone Restrictions on Student Phone Use

	I use my cellphone in class to entertain myself: Strongly disagree			
	(1)	(2)	(3)	(4)
Never Allow or Allow a Few Times	0.053***	-	0.015*	-
	(0.013)	-	(0.008)	-
Never Allow	-	0.065***	-	0.019**
	-	(0.015)	-	(0.009)
Allow a Few Times	-	0.046***	-	0.014*
	-	(0.014)	-	(0.008)
Teacher-school fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
N	173,146	173,146	199,787	199,787
Sample	Grade 8		Grade 10	

Notes: Outcome is an indicator for strongly disagreeing with the statement "I use my cell phone in class to entertain myself." Omitted category is lots of times or always/almost always. All specifications include student age, gender, socioeconomic attributes, lagged test scores, teacher gender and experience. Standard errors are clustered at the teacher-school level. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Robustness: Effects on Student Phone Use with Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Teacher FE Model					
Never Allow or	0.030**	0.030**	0.032**	-0.006	-0.007	-0.005
Allow a Few Times	(0.014)	(0.015)	(0.015)	(0.011)	(0.011)	(0.011)
Behavioral and peer characteristics		✓	✓		✓	✓
Frequency of pedagogical activities			✓			✓
Teacher-school FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
N	173,146	172,895	172,123	199,787	199,526	198,427
Sample	Grade 8			Grade 10		

Notes: This table includes two sets of additional covariates. Behavioral characteristics include a quadratic of the lagged attendance rate (0-100 percent) and an indicator for whether the student was retained in the previous year. Peer characteristics include average peer lagged standardized test scores, the shares of peers in the classroom that are male, priority students, or preferential students, respectively. Classroom pedagogical attributes include indicators for teacher responses on the frequency with which they rely on activities to assess whether students understand the content, to allow students to evaluate their own learning process, and for students to evaluate the learning of their classmates, with responses on a Likert scale (1: none or almost none of the classes, 2) in a few classes, 3) in most classes, and 4) in all classes). All specifications furthermore include student age, gender, socioeconomic attributes, lagged test scores, teacher gender and experience. Standard errors are clustered at the teacher-school level for the teacher FE model, and school-year for the student FE model. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5: Robustness: Test Score Effects with Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Teacher FE Model						Student FE Model					
Never Allow or	0.007	0.010	0.015	-0.003	-0.006	-0.005	-0.012	-0.013	-0.013	0.005	-0.002	-0.002
Allow a Few Times	(0.026)	(0.025)	(0.025)	(0.027)	(0.027)	(0.027)	(0.018)	(0.018)	(0.018)	(0.016)	(0.015)	(0.015)
Behavioral and peer characteristics		✓	✓		✓	✓		✓	✓		✓	✓
Frequency of pedagogical activities			✓			✓			✓			✓
Teacher-school FE	✓	✓	✓	✓	✓	✓						
Year FE	✓	✓	✓	✓	✓	✓						
Student-year FE							✓	✓	✓	✓	✓	✓
Subject FE							✓	✓	✓	✓	✓	✓
N	178,396	178,134	177,334	199,604	199,363	198,302	148,340	148,182	147,274	169,618	169,382	168,454
Sample	Grade 8			Grade 10			Grade 8			Grade 10		

Notes: This table includes two sets of additional covariates. Behavioral characteristics include a quadratic of the lagged attendance rate (0-100 percent) and an indicator for whether the student was retained in the previous year. Peer characteristics include average peer lagged standardized test scores, the shares of peers in the classroom that are male, priority students, or preferential students, respectively. Classroom pedagogical attributes include indicators for teacher responses on the frequency with which they rely on activities to assess whether students understand the content, to allow students to evaluate their own learning process, and for students to evaluate the learning of their classmates, with responses on a Likert scale (1: none or almost none of the classes, 2) in a few classes, 3) in most classes, and 4) in all classes). All specifications furthermore include student age, gender, socioeconomic attributes, lagged test scores, teacher gender and experience. Standard errors are clustered at the teacher-school level for the teacher FE model, and school-year for the student FE model. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A6: Robustness: Effects on Students Feeling Capable with Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Teacher FE Model						Student FE Model					
Never Allow or	-0.023*	-0.022*	-0.020	-0.007	-0.006	-0.007	-0.022**	-0.022**	-0.021**	-0.011	-0.011	-0.011
Allow a Few Times	(0.014)	(0.013)	(0.013)	(0.007)	(0.007)	(0.008)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
Behavioral and peer characteristics		✓	✓		✓	✓		✓	✓		✓	✓
Frequency of pedagogical activities			✓			✓			✓			✓
Teacher-school FE	✓	✓	✓	✓	✓	✓						
Year FE	✓	✓	✓	✓	✓	✓						
Student-year FE							✓	✓	✓	✓	✓	✓
Subject FE							✓	✓	✓	✓	✓	✓
N	178,882	178,618	177,819	202,058	201,798	200,692	153,236	153,058	152,086	178,550	178,274	177,292
Sample	Grade 8			Grade 10			Grade 8			Grade 10		

Notes: This table includes two sets of additional covariates. Behavioral characteristics include a quadratic of the lagged attendance rate (0-100 percent) and an indicator for whether the student was retained in the previous year. Peer characteristics include average peer lagged standardized test scores, the shares of peers in the classroom that are male, priority students, or preferential students, respectively. Classroom pedagogical attributes include indicators for teacher responses on the frequency with which they rely on activities to assess whether students understand the content, to allow students to evaluate their own learning process, and for students to evaluate the learning of their classmates, with responses on a Likert scale (1: none or almost none of the classes, 2) in a few classes, 3) in most classes, and 4) in all classes). All specifications furthermore include student age, gender, socioeconomic attributes, lagged test scores, teacher gender and experience. Standard errors are clustered at the teacher-school level for the teacher FE model, and school-year for the student FE model. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A7: Robustness: Effects on Academic Outcomes using Subject-District Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Test Scores				Feeling Capable			
Never Allow or	-0.013	-0.024	-0.002	0.001	-0.021**	-0.026***	-0.011	-0.018**
Allow a Few Times	(0.018)	(0.017)	(0.015)	(0.015)	(0.010)	(0.010)	(0.009)	(0.009)
Student-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Subject FE	✓		✓		✓		✓	
Subject-District FE		✓		✓		✓		✓
N	147,274	147,274	168,454	168,454	152,086	152,086	177,292	177,292
Sample	Grade 8		Grade 10		Grade 8		Grade 10	

Notes: All specifications use within-student, cross-subject variation. They furthermore include 1) behavioral characteristics: a quadratic of the lagged attendance rate (0-100 percent) and an indicator for whether the student was retained in the previous year, 2) peer characteristics including average peer lagged standardized test scores, the shares of peers in the classroom that are male, priority students, or preferential students, respectively, and 3) classroom pedagogical attributes including indicators for teacher responses on the frequency with which they rely on activities to assess whether students understand the content, to allow students to evaluate their own learning process, and for students to evaluate the learning of their classmates, with responses on a Likert scale (1: none or almost none of the classes, 2) in a few classes, 3) in most classes, and 4) in all classes). All specifications also include student age, gender, socioeconomic attributes, lagged test scores, teacher gender and experience. Standard errors are clustered at the school-year level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A8: Predictors of Changes in Phone Policy

	Changed phone policy	
	(1)	(2)
Average class lagged test scores (2017)	0.027	-0.094*
	(0.058)	(0.056)
Share of male students in class (2017)	-0.001	-0.025
	(0.097)	(0.070)
Share of priority students in class (2017)	-0.249	-0.390**
	(0.205)	(0.180)
Share of preferential students in class (2017)	-0.167	0.022
	(0.155)	(0.130)
Share of indigenous students in class (2017)	0.306	0.004
	(0.275)	(0.222)
Share of students with fathers who did not complete HS (2017)	0.058	-0.055
	(0.275)	(0.216)
Share of students with mothers who did not complete HS (2017)	0.309	0.273
	(0.276)	(0.222)
Diff. in average class lagged scores	-0.000	-0.022
	(0.053)	(0.049)
Diff. in share of male students	0.035	-0.124
	(0.106)	(0.095)
Diff. in share of priority students	-0.308	-0.106
	(0.194)	(0.167)
Diff. in share of preferential students	-0.009	0.107
	(0.186)	(0.152)
Diff. in share of indigenous students	0.284	-0.151
	(0.205)	(0.182)
Diff. in share of students with fathers who did not complete HS	0.051	-0.025
	(0.217)	(0.161)
Diff. in share of students with mothers who did not complete HS	0.125	0.084
	(0.227)	(0.164)
Teaching experience (2017):		
1 year	-0.021	0.009
	(0.138)	(0.131)
2 years	-0.066	-0.025
	(0.116)	(0.090)
3-4 years	0.029	-0.105
	(0.080)	(0.064)
5-9 years	-0.037	-0.070
	(0.062)	(0.053)
10-14 years	-0.056	-0.025
	(0.068)	(0.058)
15-19 years	0.064	-0.069
	(0.082)	(0.063)
20+ years	-0.093	-0.019
	(0.061)	(0.052)
N	992	1,322
F-statistic	0.886	0.796
Sample	Grade 8	Grade 10

Notes: Outcome is an indicator for any changes in mobile phone policy over the sample period for a given teacher. Regressors include both baseline variables in 2017 as well as changes between baseline and either 2018 or 2019. All specifications furthermore include school district fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Effects of Classroom Phone Restrictions by Teacher Peer Phone Use

	Test Scores		Feeling Capable	
	(1)	(2)	(3)	(4)
Never Allow or Allow a Few Times ×	0.034	0.050	0.017	0.015
Majority of Teachers Restrict Phones	(0.048)	(0.051)	(0.031)	(0.023)
Never Allow or Allow a Few Times	-0.036	-0.047	-0.035	-0.011
	(0.044)	(0.046)	(0.028)	(0.020)
Student-year fixed effects	✓	✓	✓	✓
Subject fixed effects	✓	✓	✓	✓
N	142,108	160,580	146,802	169,000
Sample	Grade 8	Grade 10	Grade 8	Grade 10

Notes: Majority of teachers restricting phones takes a value of 1 when more than half of teachers in the same school-year-grade restrict phones by never allowing or only allowing a few times. All specifications include student age, gender, socioeconomic attributes, lagged test scores, teacher gender and experience. Standard errors are clustered at the school-year level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.