Removing Phones from Classrooms Improves Academic Performance

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Widespread smartphone bans are being implemented in classrooms worldwide, yet their causal effects on student outcomes remain unclear. In a randomized controlled trial involving nearly 17,000 students, we find that mandatory in-class phone collection led to higher grades — particularly among lower-performing, first-year, and non-STEM students — with an average increase of 0.086 standard deviations. Importantly, students exposed to the ban were substantially more supportive of phone-use restrictions, perceiving greater benefits from these policies and displaying reduced preferences for unrestricted access. This enhanced student receptivity to restrictive digital policies may create a self-reinforcing cycle, where positive firsthand experiences strengthen support for continued implementation. Despite a mild rise in reported fear of missing out, there were no significant changes in overall student well-being, academic motivation, digital usage, or experiences of online harassment. Random classroom spot checks revealed fewer instances of student chatter and dis-

ruptive behaviors, along with reduced phone usage and increased engagement among teachers in phone-ban classrooms, suggesting a classroom environment more conducive to learning. Spot checks also revealed that students appear more distracted, possibly due to withdrawal from habitual phone checking, yet, students did not report being more distracted. These results suggest that in-class phone bans represent a low-cost, effective policy to modestly improve academic outcomes, especially for vulnerable student groups, while enhancing student receptivity to digital policy interventions.

Mobile phones are now nearly universal among school-aged children and adolescents, fueling growing concern about their impact on learning (*1–3*) as well as mental health and peer relationships (*4, 5*). In response, more than 40% of the world's education systems have enacted or proposed bans on smartphone use in schools–a trend strongly endorsed by UNESCO as a means to reduce distraction, digital addiction, and cyberbullying (*6*). Across the United States, nearly half of all states are currently considering similar legislation (*7, 8*). These policies aim to protect students from the negative academic and social consequences of in-class device use.

Despite the surge in policy measures, there is a marked *absence* of large-scale, experimental evidence that evaluates the broader impacts of mobile device use on student learning, well-being, digital use and overall educational experience (*9*). While some quasi-experimental studies from European countries report academic gains—particularly for low-achieving students (*1, 10*)—others find negligible effects in more digitally integrated classrooms (*11*). Similarly, evidence for the impact on well-being is also mixed (*12, 13*). Given the prevalence of phones among the youth worldwide, understanding the impact of removing them from classrooms is crucial for policymakers, administrators, and educators alike.

We report on a randomized controlled trial involving 16,955 students across 10 higher education institutions, conducted in Spring 2024. Student cohorts were randomly assigned at the department-grade level (i.e., a cohort) to either a mandatory in-class phone collection—requiring students to deposit their devices at the start of each lecture—or to a business-as-usual control group, where phone use remained unrestricted. Because assignment was at the department-grade level, students experienced the same treatment condition consistently across nearly all their courses throughout the semester.

1 Results - impact of phone ban

1.1 Academic performance

We estimate the following linear regression model to estimate the impact of random assignment to a phone-ban policy on students grade:

$$GPA_{i,t} = \beta \text{PhoneBan}_{i,t} + \delta GPA_{i,t-1} + \varepsilon_{i,t}, \tag{1}$$

As shown in Fig.1A, restricting student access to smartphones is associated with significantly higher academic performance. In a cross-sectional model controlling for prior achievement, the policy yields a 0.086 standard deviation increase in GPA ($\hat{\beta} = 0.086$, $\text{CI}_{95\%} = [0.015, 0.157]$, $t = 2.39$, $p_{\beta>0} = 0.009$). Baseline academic performance is balanced across treatment and control groups, with no statistically significant differences in prior GPA levels under any specification (SI Table B.1).

Our estimated improvement of 0.086 standard deviations in grades following a classroom phone ban is meaningful when benchmarked against both pedagogical and non-pedagogical interventions in education. For example, the difference between having an average teacher and a very good teacher for one academic year is roughly 0.20 SD – an effect size that is considered large (*14*). Successful teacher professional development programs and large-scale curriculum reforms generally yield achievement gains in the range of 0.05–0.20 SD (*15, 16*). Targeted growth-mindset programs show similar effect sizes (*17, 18*). Consequently, the impact

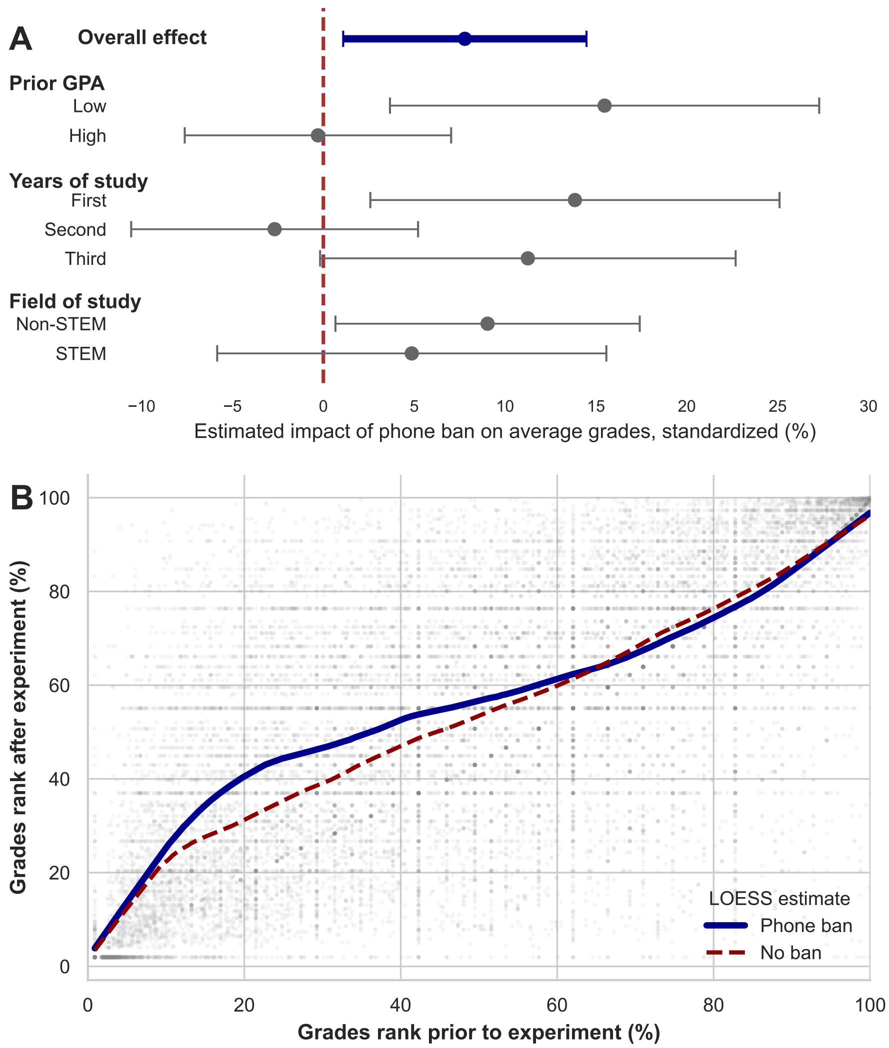

Figure 1: **Phone ban impact on student grades.** Panel A. Estimated treatment effect of smartphone ban on student grades from model (1). Error bars denote 95% CIs. The top estimate is the average treatment effect across the full population. The next estimates contain heterogeneous treatment effects for subgroups split by Prior average grades (above or below pre-experiment median); years of study; and, field of study. Panel B. Locally weighted scatterplot smoothing (LOWESS) curves show non-parametric estimates of the outcome as a function of the predictor, stratified by treatment. Shaded areas represent local trends without assuming a global functional form.

of a straightforward classroom phone ban aligns with other established, scalable educational interventions that boost student achievement.

Fig. 1A also presents estimates of heterogeneity in treatment effects across pre-specified subgroups, relative to the overall average treatment effect (reported in the first row). The estimated gains vary substantially, with larger effects concentrated among students with lower prior academic performance, first-year undergraduates, and those enrolled in non-STEM majors. Within these groups, the effects of the smartphone ban are consistently positive and statistically significant. By contrast, no meaningful effect is detected among students with above-median prior achievement, second-year students, or those in STEM fields.

These findings suggest the intervention particularly benefited initially disadvantaged students. Such redistributive effects position the policy as a tool for advancing educational equity by disproportionately aiding those at greater risk of falling behind. By narrowing achievement gaps, phone bans could support more equitable educational outcomes across student populations. Our findings also suggest that improved academic performance due to classroom phone bans could have downstream effects on graduation rates. By lifting grades for lower-performing and first-year students, these policies may help more students meet key progression and graduation thresholds. Even modest GPA increases, as documented here, can significantly help academically vulnerable students stay on track, potentially reducing dropout rates and increasing graduation rates over time.

The ban's statistically significant GPA increase is robust across model specifications, outcomes, and aggregation levels: student- and course-level estimates agree (Fig. 1A; SI Tables B.2, B.3), survive alternative covariate and fixed-effects choices (SI Tables B.4, B.5), and remain intact under four standardization schemes (SI Tables B.6–B.9).

The phone-ban policy also reduced GPA inequality among treated students (SI Table B.10). At the student level, GPA variation increased post-policy for both groups, but more sharply for control students (Δ Pre/Post = +10.83) than treated students (Δ = +5.16), resulting in a net reduction in dispersion (Δ Control–Treat = –5.67). GPA concentration, measured by the

Herfindahl–Hirschman Index, decreased among treated students (–3.02) but rose among controls (+22.64), yielding a large relative drop in concentration (Δ Control–Treat = –25.66). These results indicate that the policy was associated with greater GPA equalization among treated students.

Fig. 1.B visualizes the relationship between students' baseline academic rank and their performance following the intervention, separately by treatment group. The LOWESS curves provide non-parametric estimates of post-treatment rank as a function of pre-treatment rank, allowing for flexible trend detection. Students in the phone ban schools (treatment group) systematically outperformed their peers in control schools across most of the rank distribution, with the largest divergence among lower-ranked students. This pattern is consistent with the subgroup analysis, reinforcing that the intervention had disproportionately positive effects on lower-performing students. These findings are robust to alternative specifications of the LOWESS model. SI Fig A2 uses a 20% LOWESS bandwidth and shows the estimate for different subgroups and level of aggregation. These patterns hold with 10% and 50% bandwidths (see SI Figs.A3, A4) and when fitting within colleges (SI Fig.A5).

We also assess whether the observed academic improvements are mirrored by changes in student attendance. Across specifications using both average attendance (SI Table B.12) and course-level attendance measures (SI Table B.13), we find no consistent evidence that the phone ban affected student attendance. These results suggest that the academic gains were not primarily driven by increased classroom presence, but more likely reflect changes to in-class attention and dynamics.

1.2 Self-reported outcomes

One major concern for enacting bans in classrooms is the lack of clarity on how students perceive these policies; confiscating their "valuables" may inadvertently exacerbate their disengagement from school, especially for those already facing challenges (*19*). Therefore, examining students perspective is critical for successful deployment. We surveyed a subset of students (N=2,557) to examine their perception.[1]

We find that students exposed to the phone ban substantially *improved* their attitudes towards ban policies (Table 1, Panel A). Students who experienced the in-class phone-bans became significantly more supportive of such restrictions ($\hat{\beta} = 0.174$, $\text{CI}_{95\%} = [0.059, 0.289]$, $t = 2.961$) and perceived them as beneficial for them ($\hat{\beta} = 0.196$, $\text{CI}_{95\%} = [0.080, 0.311]$, $t = 3.323$), relative to peers in classrooms without a ban. Importantly, treated students' preference for a "no ban" classroom (i.e. no phone-use restrictions) has diminished ($\hat{\beta} = -0.186$, $\text{CI}_{95\%} = [-0.292, -0.080]$, $t = -3.434$). Given five policy options (full-day bans, lecture-only bans, phone-allowed zones, app-based limits, or no ban), those assigned to ban treatment during lecture hours were significantly less likely to choose the option of no ban. Directionally, the heightened preference toward the ban increases with the stringency of the policy, with the largest increase observed for the all-day ban, relative to the no-ban condition (SI Table C.21). Although we test only one policy (lecture-only ban), this pattern is consistent with the hypothesis suggesting that once students experience the potential value of phone bans, the exact form of the phone policy matters less to them than the presence of some restriction.

From a policy perspective, our results suggest that the initial *enforcement* of phone-free classrooms can become self-reinforcing. The convergence of improved academic performance and increased student receptivity underscores the potential effectiveness of such policies, as the phone ban can cultivate student buy-in when learners personally experience its benefits through enhanced attention and academic performance.

Notably, in the absence of phones in classrooms, we find a mild increase in students' perceived fear-of-missing-out ($\hat{\beta} = 0.179$, $\text{CI}_{95\%} = [0.007, 0.351]$, $t = 2.041$; Table 1, Panel B).

[1] See SI Section A and C for details.

Table 1: Regression results on student survey outcome. We estimate the following model: $Y_i = \alpha + \beta \times \text{PhoneBan}_i + \varepsilon_i$, where Y_i denotes the survey outcome variable and PhoneBan_i is the binary in-class phone-ban treatment status for student i. All outcome measures are standardized using the pooled sample standard deviation. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables and report sharpened FDR q-values to correct for multiple-hypothesis testing, following (*20*). N= 2,557. Significance at $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Outcome	Phone Ban (1)	Sharpened Q-value (2)
Panel A. Policy Preference and Attitudes Toward Phone Ban		
Supports Phone Ban	0.1741*** (0.0588)	0.0086
Preference Towards No Phone Ban	-0.1861*** (0.0542)	0.0038
Perceived Benefits of Phone Ban	0.1957*** (0.0589)	0.0038
Panel B. Impact on Digitally Pertinent Outcomes		
Fear of Missing Out Index	0.1790** (0.0877)	0.0865
Mean Total Phone Screen Time	0.0826 (0.0837)	0.2720
Bring Phone to School	-0.0441 (0.0568)	0.3336
Experienced Any Online Harassment	0.0610 (0.0585)	0.2720
Panel C. Impact on Perceived Educational Experience		
Student Subjective Well-being Index	0.0817 (0.0585)	0.2271
Perceived In-class Distraction Frequency	0.0668 (0.0551)	0.2711
Academic Thriving Index	0.0564 (0.0528)	0.2720
Perceived Learning	0.0765 (0.0500)	0.2119

This could partly be driven by the inability among students in the ban group to keep up with social media during lecture hours. Since no-ban classrooms remained active, students in phone-ban classrooms might perceive that their peers in control classrooms continued engaging on social media, resulting in a sense of fear-of-missing-out (FOMO). Under this model, a school-wide or nationwide ban could eliminate some of these network effects—and the associated mild increase in FOMO (*21*). Moreover, we observe no aggregate effects on average phone-use time, the frequency with which students bring phones to the school or online harassment. Taken together, these results indicate that the phone ban has limited spillover effects on the digital lives of students.

The support for phone ban policies appears to be strongest among 'front benches', who also report greater perceived benefits from restricting phone use (SI Table D.23).[2] Similarly, students with an above median GPA report higher perceived benefits from the ban (SI Table D.24). These results indicate that, despite low-performing students benefiting more from the ban, more motivated students exhibit a stronger preference for it. One possible explanation is that if the classroom becomes a more learning-conducive environment, motivated students might appreciate it more, even though the actual learning gains are larger among lower performers. Turning to gender, female students show a stronger inclination to support phone bans and perceive greater associated benefits. In contrast, heightened fear of missing out (FOMO) effects predominantly affect male students, coinciding with increased phone screen time and perceived distraction (SI Table D.25). These findings suggest that female students may be more receptive to the phone-ban policy. We observe no heterogeneity based on the field of study (SI Table D.26).

In the context of behavioral and educational sciences, our findings of increased grades can be theorized to be driven by reduced distraction. Prior research indicates that merely having

[2] We define "front-benchers" as students who self-reported sitting in the front section of the classroom when asked to choose among front, middle, or back seating options.

a smartphone nearby can erode attention and cognitive capacity (*22*). Removing phones can reduce extraneous cognitive load, freeing working-memory resources for deeper cognitive processing of course material, and providing the external structure needed to support learners with limited executive control in their self-regulated learning (*23, 24*). In the context of behavioral economics, the ban might operate as a commitment device that curbs behavioral tendencies to prioritize short-run (digital) temptations over long-run (academic) returns (*25*). However, we find no effect on students perceived distraction (Table 1, Panel C). Moreover, we find no significant effects on perceived learning, students subjective well-being, or their academic motivation – suggesting that students' overall perceived educational experience is unlikely to be substantially affected.

1.3 Classroom Dynamics and Environment

Our research assistants' (RA) random classroom checks provide an external perspective on classroom dynamics, complementing students' self-reported responses (Figure 2). It is important to note that RAs were blinded to the treatment condition; however, they could infer it by observing students using phones or noting the placement of phone boxes relative to their observation spot. Thus, spot checks reflect external perceptions rather than purely objective measurements. Interestingly, we find that students appear more distracted in the spot checks.[3] The discrepancy could be explained by the attention allocation model (*26*). Removing a primary source of interference (i.e., phones) may heighten sensitivity to alternative, less salient stimuli (e.g., ambient noise). Thus, the phone ban, by eliminating a key source of self-distraction (i.e., in-class phone use), may prompt students to direct their attention toward alternative distractions that could be more easily noticeable to an external observer. Consistent with this hypothesis, students in the ban group reported directionally higher distraction across a battery of 13 ex-

[3] Albeit insignificant, directionally they also ask less questions to teachers.

ternal factors, environmental noise being most affected (SI Table C). Similarly, students in the treatment group consistently expressed directionally higher, statistically insignificant, concerns about external factors affecting their focus across a 10-item scale (SI Table C.16). However consistent with theory, our data does not unravel this distraction paradox further.

Our findings on other in-class behavior reveals a more consistent pattern. We find that teachers in phone-free classrooms appear significantly busier maintaining order and student focus. They are also more occupied with educational materials. Importantly, despite teachers themselves not being subject to a phone ban, we observe a reduction in teachers in-class phone use in treated classrooms. Echoing more engaging teacher behavior, students exhibited significantly fewer instances of disruptive behavior, and less peer-to-peer conversation unrelated to course material. Additionally, we observe directionally fewer negative teacher-student interactions, although this effect is not statistically significance. It is also noteworthy that students observe a mild decrease in their use of other electronic devices during lectures, potentially indicating a complementarity among in-class use of digital technologies.

Viewed jointly, these results provide suggestive evidence that classroom phone bans can substitute students and teachers' time away from their phones to enhancing classroom dynamics, potentially translating into a "healthier" classroom environment and the subsequent improved student performance.

2 Discussion

Our study sheds light on a central policy question: what is the impact of banning phones in classrooms? Our findings suggest that phone bans can substantially improve student grades, with particularly pronounced benefits for low-performing students. This group is critical to advancing educational equity, as improving outcomes among students who are already lagging academically could help narrow achievement gaps. We also find that students who experience

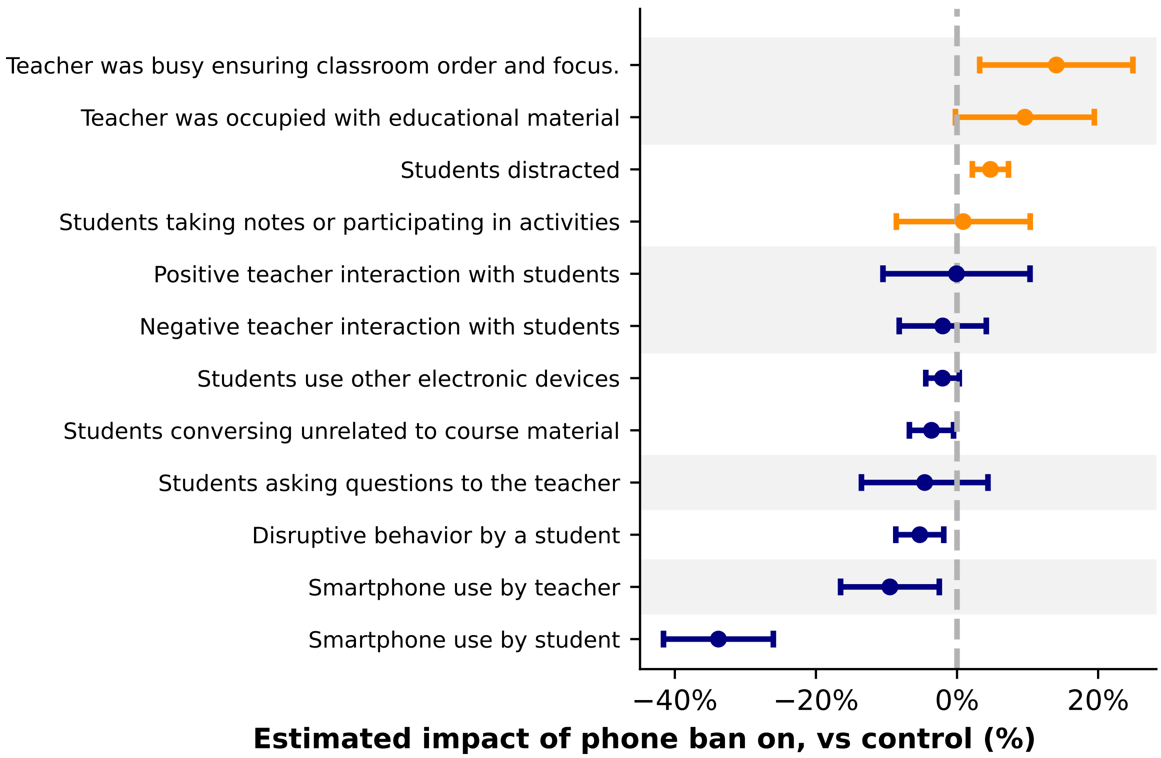

Figure 2: Impact of phone ban Results are based on unannounced *in-class* spot checks by independent enumerators. During each visit, an enumerator randomly selected a classroom, observed the session unobtrusively for under a minute (e.g. through a window or doorway), and coded a binary indicator for every behavior listed above. We estimate the linear-probability model $Y_i = \alpha + \beta \times \text{PhoneBan}_i + \varepsilon_i$, where Y_i is a binary outcome and $\text{PhoneBan}_i = 1$ if student i was subject to the in-class phone ban. To guard against over-representing HEIs with more frequent visits, we apply weights that give each participating school equal total influence on the estimates. Standard Errors are clustered at the same level as the treatment assignment: department-grade-institute level. Bars show 95% confidence intervals. $N = 7{,}797$ enumerator spot checks. Gray shaded areas reflect teacher-involved behaviors.

the ban exhibits an increased preference towards the ban, that is important for the policy support. Lastly, our results indicate a potentially more supportive learning environment with less disruptive student behavior and increased teacher's effort. A key limitation of this study is that it was conducted in higher-education institutions, where students are already academically selected. Given the stronger effects observed among first-year students, impacts might be more pronounced for lower grade levels—an avenue for future research. Moreover, the effects may also compound over time and longer exposure, e.g., over the entire course of studies could be higher. Additional research is also necessary to understand how this generalizes to other geographic contexts, in particular more developed countries where students often possess other digital devices.

The impact of digital technologies extends far beyond classrooms. As these technologies have become fundamentally entrenched in people's lives worldwide, their effects on cognition and learning are increasingly important. Our findings—that removing phones from classrooms positively influences objective performance and aligns with students' preferences—corroborate the argument that phone interference might disrupt various cognitive processes. This disruption may particularly affect tasks requiring sustained engagement or uninterrupted attention, such as productivity at work, enjoyment at social gatherings, safety while driving, or sleep quality. Removing digital devices when they interfere with such activities could represent a broader solution, highlighting an avenue for future research. From an educational policy perspective, our results indicate that restricting phone use in classrooms can support learning.

3 Materials and Methods

This study was approved by the Institutional Review Board at the University of Copenhagen and the University of Pennsylvania. Pre-registration details can be found at https://aspredicted.org/8dv5-qd78.pdf Further details of the study is available in SI Appendix Section A.

We commissioned the manufacturing of approximately one thousand wooden phone deposit boxes using recycled materials and installed them in classrooms across 10 partner higher education institutes (HEIs) in Odisha, India. At each partner institute, we stationed a full-time research assistant – with undergraduate degree or above – and collaborated with a designated teacher, who served as the project coordinator. We randomized academic departments within each HEIs, stratified by semester, into treatment and control courses with equal probability. In the treatment courses, teachers enforced a phone ban by requiring all students to deposit their phones in a designated phone-box at the start of each lecture. Note that this is not an opt-in policy, and unless they had an approved exception (e.g., documented emergencies or caregiving obligations), in which case they informed the teacher prior to the class session.[4] This protocol was applied in all classes for the treatment arm throughout the Spring 2024 semester, while students in the control departments maintained their usual access to phones. Notably, many classrooms were shared by both treatment and control arms. Therefore, we placed phone-boxes in all classrooms. The key difference between the two arms lies not in the presence of the boxes themselves, but in the enforcement protocol requiring students in the treatment arm to deposit their phones.

Our pre-registered[5] randomized controlled trial protocol comprised three avenues of data collection. First, we obtained each student's objective final grades and attendance records from both before and during the study period. Importantly, no technology was allowed during the exams for both conditions. Second, we administered a survey in two waves through the semester, sharing the survey link at the start of the term and sending reminders via study coordinators of

[4]In the case of an exception, students needed to ask for an approval by informing teachers to keep their phones with them.

[5]Due to logistical constraints, we were unable to install a usage-tracking app on students' devices or administer the baseline survey prior to the experiment; instead, we employed two survey waves during the experiment period. Therefore, we omit the heterogeneity analyses requiring pre-intervention data – that are baseline phone usage intensity, well-being, and FOMO. Additionally, we initially aimed the project with 18 HEIs, but the experiment protocol implementation and the data collection were fully completed at 10 institutes. See SI Appendix A for details.

each partner institute.[6]

Lastly, our research assistants conducted random classroom spot checks – with consent – by briefly watching each classroom through the window or the doorway, without interrupting the lecture. During these visits, they checked each classroom against a predefined list of observable behaviors (e.g., whether students were using their phones or asking questions), noting whether or not each behavior occurred. This procedure was carried out throughout the semester, enabling us to examine how banning or allowing phone use influenced classroom interactions and engagement from an external observer's perspective.

Author Contributions A.S. and A.B.N. jointly developed the initial research concept and study design. All authors contributed to the planning of field implementation. A.S. and P.K.C. led field operations and coordination of the research assistant team. A.S. and A.B.N. conducted the data analysis and drafted the manuscript. All authors provided input on the final version of the paper.

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A Experiment Protocol Details

A.1 Recruitment and Partner Institution Enrollment

Between November and December 2023, the project team sent out formal invitations to 52 higher education institutions (HEIs) in Odisha, India. The invitation letter was addressed to the heads of the institutes – Principals or Vice Chancellors – with a detailed project protocol, including the research team, background and objectives of the study, methodology, collaboration items, and academic engagements, as well as anticipated collaborations and incentives. The HEIs were selected from four different geographical and administrative areas, covering 12 of the total 30 districts. Of the 52 institutions invited, 25 exhibited an interest in collaboration. Each of these institutions offers undergraduate (UG) and/or postgraduate (PG) programs across disciplines such as sciences, humanities, social sciences, and languages.

Following multiple rounds of online and in-person discussions with principals, vice chancellors, department heads, teachers, and selected student representatives, 18 higher education institutions (HEIs) initially recruited to participate in the in-class phone ban experiment study. The project team then conducted onsite visits to verify that each institution's infrastructure and logistical capabilities aligned with the study protocol. During this assessment phase and in-person visits during the study period, 2 HEIs ultimately withdrew from the project, 2 more were found to lack sufficient resources or time to implement the intervention (e.g., producing phone boxes, informing teachers, or effectively enforcing the bans), and 4 institutions could not provide complete student-level data or secure the necessary administrative approvals. Consequently, the final sample comprised 10 HEIs where the experiment protocol and data collection were fully implemented. Prior to initiating any data collection, we informed all participating institutions that the data would be used solely for academic and research purposes and would remain undisclosed to non-academic entities or third parties.

A.2 Participants and Experimental Design

A.2.1 Target Population and Sampling

For each selected HEI, we focused on all undergraduate and postgraduate classrooms. The treatment period spanned the full Spring 2024 semester (February–May 2024), although start and end dates varied slightly by institution. For cross-verification, specific academic calendars were collected from each HEI.

A.2.2 Randomization and Group Assignments

We performed randomization at the level of academic department-semester for each HEI[7]. Within each institution, roughly half of the targeted department-semesters were randomly allocated to the treatment group (phone ban), and the remaining departments served as the control group (business-as-usual). This stratification was based on student enrollment data provided by each institution. In the treatment departments, teachers enforced a phone ban by requiring students to deposit their phones into a designated box at the start of each lecture. The phone ban applied throughout the semester in *all* the courses offered by those departments. There were no phone-ban restrictions on teachers. In control departments, teachers and students continued their regular classroom routines, retaining access to smartphones during lectures.

Notably, many classrooms were shared by treatment and control departments. As such, phone-boxes were installed in all classrooms for consistency. The distinguishing factor between treatment and control remained the explicit enforcement of depositing phones, rather than the mere presence of the boxes. Students with special circumstances (e.g., documented emergencies or caregiving obligations) could request teacher approval to retain their devices during class, provided they informed the instructor in advance.

[7] For instance, in college A, all psychology first year students are in the treatment arm, and all economics second year students are in the control arm.

A.3 Intervention Implementation

A.3.1 Phone Deposit Boxes

To standardize the phone-banning procedure, the project team commissioned the manufacturing of approximately one thousand wooden phone deposit boxes, fabricated from recycled materials. Each phone box was locally designed in consultation with the college coordinators, state coordinator, and principal investigators (PIs). Once delivered, these boxes were distributed among classrooms across all partner HEIs. For classrooms with larger seating capacities, multiple boxes were provided to accommodate potentially higher student enrollments. The boxes were either mounted on walls or placed on desks at the front of each classroom. See Figure A1.

Figure A1: An example of a phone box placed in the classroom

A.3.2 Field Team: Human Resources and Roles

State Coordinator. One state coordinator oversaw the project implementation across the selected HEIs, maintained regular contact with the PIs, and coordinated onsite visits.

State-Level Research Assistant (RA). A state-level RA was responsible for liaising with individual college coordinators and RAs, scheduling data collection, and ensuring consistent application of the experimental protocols (including timely enforcement of the phone ban). This RA also organized online and in-person meetings, handled logistical issues, and consolidated enrollment, attendance, and grade data from each institution.

College Coordinators. Each institution designated a teacher to coordinate the local aspects of the experiment. These coordinators managed tasks such as placing and maintaining phone deposit boxes, guiding participating teachers and students on the experiment protocols, and verifying compliance.

College RAs. Each college employed a research assistant (RA) who was not a student enrolled at that institution. College RAs assisted in daily operations, verified that the phone ban was being observed, and collected attendance, grading, and survey data. This on-site presence helped ensure fidelity to the experimental design throughout the semester.

A.4 Data Collection Procedures

A.4.1 Grades and Attendance

The first source of data included objective measures of student performance and attendance, gathered from both the pre-treatment (before February 2024) and treatment (February–May 2024) periods. Specifically, we obtained official final grades from institutional examination records and cross-checked them with departmental transcripts. Attendance logs were maintained either digitally or via manual rosters; we compiled these records through the admissions and examination sections of each participating college.

A.4.2 Surveys and data-collection deviations

Second, we administered an online student survey in two waves over the semester. Each institution's college coordinator disseminated the survey link at the start of the term, supplemented by periodic reminders via emails, notice boards, or short announcements in class. This survey captured self-reported measures of demographic background, digital device usage, academic motivation, well-being indicators, and experiences relating to classroom phone policies.

Notably, the first survey wave was intended as a baseline, but we were unable to administer it before the term began. Therefore, we deployed nearly identical surveys in subsequent waves. In addition, no usage-tracking app was installed on the students' smartphones.

A.4.3 Classroom Observations

Lastly, our RAs conducted random classroom observations to document student behaviors. For each visit, the RA observed a classroom briefly through a window or from the doorway, usually for under one minute, to minimize disruptions. A structured checklist was used to record observations such as whether students were using phones, interacting with peers, or asking the instructor questions. These short visits were carried out over the semester, spaced irregularly – more often later in the term – to discourage any predictable pattern.

A.5 Compliance Monitoring and Training

Before the experiment began, university and college administrators—including principals, vice chancellors, heads of departments, and teachers—received orientation sessions. These sessions, conducted both online and in person, clarified the experimental protocol, data privacy measures, and roles of all parties involved. Teachers in the treatment arm were strongly encouraged to ban phones consistently, while control-arm teachers were instructed to continue with standard classroom practices. Compliance checks were performed by the PIs, the state coordinator, and RAs, ensuring that treatment departments enforced the phone ban and that data were recorded accurately.

A.6 Ethical Considerations

All participating HEIs confirmed their consent in writing, and students were informed that any data collected would be anonymized and used strictly for academic research. Those with valid exceptions for phone use (e.g., medical emergencies) were permitted by prior arrangement with the instructor. As per our agreement with the institutions, no individual student-level data were shared with external, non-academic entities, and all sensitive information was de-identified.

B Supporting results grades and attendance

B.1 Student grades as outcomes

This subsection report the output from estimating regression models where students' grade and attendance are the outcomes. We estimate more models based on model (1) used in Figure 1 along with its course-level specification forms below.

$$y_{i,course,t} = \beta \text{PhoneBan}_{course,t} + GPA_{i,t-1} + \varepsilon_{i,course,t}, \tag{2}$$

We also estimate a number of alternative model specifications, see below.

$$y_{i,t} = \beta \text{PhoneBan}_{cohort(i),t} + \delta GPA_{i,t-1} + \gamma_{college(i)} + \varepsilon_{i,t}, \tag{3}$$

$$y_{i,t} = \beta \text{PhoneBan}_{cohort(i),t} + \varepsilon_{i,t}, \tag{4}$$

$$y_{i,t} = \beta \text{PhoneBan}_{cohort(i),t} + \gamma_{college(i)} + \varepsilon_{i,t}, \tag{5}$$

$$y_{i,t} = \alpha_i + \beta \text{PhoneBan}_{cohort(i),t} + \eta_{college(i),t} + \varepsilon_{i,t}, \tag{6}$$

$$y_{i,t} = \alpha_i + \beta \text{PhoneBan}_{cohort(i),t} + \nu_s + \eta_{college(i),t} + \varepsilon_{i,t}, \tag{7}$$

These models also have corresponding specifications using course-level outcomes, see below.

$$y_{i,course,t} = \beta \text{PhoneBan}_{course,t} + \delta GPA_{i,t-1} + \gamma_{college(i)} + \varepsilon_{i,course,t}, \tag{8}$$

$$y_{i,course,t} = \beta \text{PhoneBan}_{course,t} + \varepsilon_{i,course,t}, \tag{9}$$

$$y_{i,course,t} = \beta \text{PhoneBan}_{course,t} + \gamma_{college(i)} + \varepsilon_{i,course,t}, \tag{10}$$

$$y_{i,course,t} = \alpha_i + \beta \text{PhoneBan}_{course,t} + \eta_t + \varepsilon_{i,course,t}, \tag{11}$$

$$y_{i,course,t} = \alpha_i + \beta \text{PhoneBan}_{course,t} + \eta_{college(i),t} + \nu_s + \varepsilon_{i,course,t}, \tag{12}$$

B.1.1 Balancing tests

SI Table B.1 presents balance tests comparing baseline academic performance (measured by prior GPA) between students in the treatment and control groups. Across all model specifications—including simple mean comparisons and regressions with fixed effects for college, and field of study — we find no statistically significant differences in baseline GPA between groups. Point estimates are small in magnitude and consistently fall well within standard error bounds. This suggests that students in treated and control cohorts were academically comparable prior to the policy intervention, supporting the internal validity of our subsequent causal analyses.

Table B.1: Balancing check of phone ban policy

	Prior GPA is High			Prior GPA (standardized)		
	(1)	(2)	(3)	(4)	(5)	(6)
Phone Ban	0.031	0.019	0.009	0.058	0.018	0.006
	(0.034)	(0.028)	(0.017)	(0.062)	(0.046)	(0.030)
College fixed effects	–	x	x	–	x	x
Department fixed effects	–	–	x	–	–	x
Observations	17,050	17,050	17,050	17,050	17,050	17,050
R^2	0.001	0.122	0.198	0.001	0.120	0.183
R^2 Within	–	0.000	0.000	–	0.000	0.000

Regressions test for baseline balance in prior academic performance between treatment and control groups. The estimated model corresponds to (4) and (5). Columns (1)–(3) use a binary indicator for high GPA; columns (4)–(6) use standardized GPA. College fixed effects are included in columns (2)–(3) and (5)–(6). Standard errors clustered at the college level.

B.1.2 Level of aggregation and model specifications

This subsection reports on alternative specifications to our main model. The estimates of the average treatment effects of the ban are found SI Table B.4 for student level average grades (GPA) as the outcome and SI Table B.5 for course-level grades. We report only on the aggregate level outcomes as the results from the course-level models are qualitatively the same.

Estimates from the main model are inserted in column 1 as a reference. We see that adding college fixed effects lowers the estimate, but the conclusion remains unchanged. Estimates from using a simpler model, which includes only the smartphone ban as an explanatory variable, yields a slightly larger estimate than the main model ($\hat{\beta} = 0.112$, $\text{CI}_{95\%} = [0.016, 0.228]$, $t = 2.11$, $p_{\beta>0} = 0.018$). Again, adding college level fixed effects lowers the estimate but with the same conclusion.

A balanced panel model uses the previous semester to includes student and semester-time fixed effects to account for unobserved heterogeneity gives a lower, but qualitatively similar result ($\hat{\beta} = 0.044$, $\text{CI}_{95\%} = [-0.016, 0.104]$, $t = 1.47$, $p_{\beta>0} = 0.071$). Using an unbalanced panel model also yields similar but lower estimates with insufficient precision to reject the null.

We note that the models in columns (3) and (5) correspond exactly to our pre-specified models. Our pre-registered analysis plan is available at https://aspredicted.org/8dv5-qd78.pdf.

Table B.2: Ban impact on average grades

	GPA (standardized)			
	(1)	(2)	(3)	(4)
Phone Ban	0.086***			
	(0.036)			
Phone Ban × Prior GPA is High		0.001		
		(0.040)		
Phone Ban × Prior GPA is Low		0.161***		
		(0.062)		
Phone Ban × First-year student			0.142**	
			(0.062)	
Phone Ban × Second-year student			-0.023	
			(0.041)	
Phone Ban × Third-year student			0.124**	
			(0.063)	
Phone Ban × Field of study is STEM				0.059
				(0.056)
Phone Ban × Field of study is not STEM				0.097**
				(0.046)
Observations	16,955	16,955	16,955	16,955
R^2	0.185	0.187	0.198	0.186
Adj. R^2	0.185	0.187	0.198	0.186

This table presents regression coefficients of how the cohort-level smartphone ban affects average grades obtained by students, estimated from model (1). Each column shows a different model specification. Standardization of grades is performed using the mean and standard deviation of all observations. The sample consists of observations from Spring 2024, including colleges with implementation issues. Standard errors are reported in parentheses and clustered at the cohort-level. Statistical significance (one-sided tests of positive impact): * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.3: Ban impact on course-level grades

	Course grades (standardized)			
	(1)	(2)	(3)	(4)
Phone Ban in Course	0.061**			
	(0.028)			
Phone Ban in Course $\times$ Prior GPA is High		0.013		
		(0.027)		
Phone Ban in Course $\times$ Prior GPA is Low		0.103**		
		(0.048)		
Phone Ban in Course $\times$ First-year student			0.095**	
			(0.050)	
Phone Ban in Course $\times$ Second-year student			-0.004	
			(0.033)	
Phone Ban in Course $\times$ Third-year student			0.079*	
			(0.048)	
Phone Ban in Course $\times$ Field of study is STEM				0.052
				(0.043)
Phone Ban in Course $\times$ Field of study is not STEM				0.070**
				(0.037)
Observations	76,374	76,374	76,374	76,374
R^2	0.146	0.147	0.153	0.147
Adj. R^2	0.146	0.147	0.153	0.147

This table presents regression coefficients of how the course-level smartphone ban affects course-level grades from model (2). Each column reports a different model specification. Grades are standardized using the mean and standard deviation from all observations. The sample includes data from Spring 2024, including colleges with implementation issues. Standard errors (in parentheses) are clustered at the cohort level. Statistical significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.4: Ban impact on grades – alternative model specifications

	GPA (standardized)					
	(1)	(2)	(3)	(4)	(5)	(6)
Phone Ban	0.086***	0.062**	0.112**	0.069**	0.044*	0.034
	(0.036)	(0.032)	(0.053)	(0.037)	(0.030)	(0.027)
College fixed effects	-	x	-	x	-	-
Department fixed effects	-	x	-	x	-	-
Student fixed effects	-	-	-	-	x	x
College $\times$ Time fixed effects	-	-	-	-	x	x
Academic Term fixed effects	-	-	-	-	-	x
P-value, one-sided	0.009	0.025	0.018	0.031	0.071	0.107
t-stat	2.39	1.96	2.11	1.88	1.47	1.24
Model Equations	(1)	(3)	(4)	(5)	(6)	(7)
Observations	16,955	16,955	16,955	16,955	35,881	70,753
R^2	0.185	0.217	0.003	0.108	0.861	0.767
R^2 Within	-	0.123	-	0.001	0.001	0.000

This table presents regression coefficients estimating how the cohort-level smartphone ban affects students' mean GPA using alternative model specifications listed under "Model Equations." Each column shows results from a different specification. GPA is standardized using the full sample mean and standard deviation. Standard errors are reported in parentheses and clustered at the cohort level. One-sided significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.5: Ban impact on course-level grades – alternative model specifications

	Course grades (standardized)					
	(1)	(2)	(3)	(4)	(5)	(6)
Phone Ban in Course	0.061**	0.050**	0.073**	0.053**	0.038**	0.031*
	(0.028)	(0.026)	(0.043)	(0.030)	(0.023)	(0.021)
College fixed effects	-	x	-	x	-	-
Department fixed effects	-	x	-	x	-	-
Student fixed effects	-	-	-	-	x	x
College $\times$ Time fixed effects	-	-	-	-	x	x
Academic Term fixed effects	-	-	-	-	-	x
P-value, one-sided	0.015	0.026	0.044	0.039	0.047	0.070
t-stat	2.17	1.96	1.71	1.76	1.68	1.48
Model Equations	(2)	(8)	(9)	(10)	(11)	(12)
Observations	76,374	76,374	76,374	76,374	160,784	319,022
R^2	0.146	0.165	0.001	0.073	0.602	0.519
R^2 Within	-	0.099	-	0.001	0.000	0.000

This table presents regression coefficients estimating how the course-level smartphone ban affects course grades, based on model specifications listed under "Model Equations." Each column represents a different model. Grades are standardized using the mean and standard deviation from the full sample. Standard errors (in parentheses) are clustered at the cohort level. One-sided significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

B.1.3 Alternative grade standardization

Table B.6: Ban impact on grades – standardized by college

	GPA (standardized within college)			
	(1)	(2)	(3)	(4)
Phone Ban	0.083**			
	(0.049)			
Phone Ban × Prior GPA is High (within college)		0.018		
		(0.047)		
Phone Ban × Prior GPA is Low (within college)		0.148**		
		(0.081)		
Phone Ban × First-year student			0.054	
			(0.090)	
Phone Ban × Second-year student			0.073	
			(0.079)	
Phone Ban × Third-year student			0.106**	
			(0.062)	
Phone Ban × Field of study is STEM				0.065
				(0.054)
Phone Ban × Field of study is not STEM				0.086*
				(0.066)
Observations	16,955	16,955	16,955	16,955
R^2	0.147	0.148	0.159	0.150
Adj. R^2	0.147	0.148	0.158	0.150

This table presents regression coefficients estimating how the cohort-level smartphone ban affects average grades, based on model (1). Each column shows a different model specification. Grades are standardized within each college. The sample includes data from the Spring 2024 experiment period, including colleges excluded from implementation. Standard errors (in parentheses) are clustered at the cohort level. One-sided significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.7: Ban impact on grades – standardized by control group statistics

	GPA (standardized by control group statistics)			
	(1)	(2)	(3)	(4)
Phone Ban	0.072**			
	(0.036)			
Phone Ban × Prior GPA is High (relative to control group)		-0.009		
		(0.039)		
Phone Ban × Prior GPA is Low (relative to control group)		0.146***		
		(0.062)		
Phone Ban × First-year student			0.117**	
			(0.061)	
Phone Ban × Second-year student			-0.031	
			(0.040)	
Phone Ban × Third-year student			0.117**	
			(0.062)	
Phone Ban × Field of study is STEM				0.048
				(0.056)
Phone Ban × Field of study is not STEM				0.083**
				(0.045)
Observations	16,955	16,955	16,955	16,955
R^2	0.187	0.188	0.200	0.188
Adj. R^2	0.187	0.188	0.200	0.187

This table presents regression coefficients estimating the impact of the cohort-level smartphone ban on average student grades based on model (1). Each column reflects a different model specification. Grades are standardized using the control group's mean and standard deviation. The sample includes Spring 2024 data from the full experimental population, including colleges with implementation issues. Standard errors (in parentheses) are clustered at the cohort level. One-sided significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.8: Ban impact on grades – standardized by year of matriculation

	GPA (standardized within years of study)			
	(1)	(2)	(3)	(4)
Phone Ban	0.087***			
	(0.034)			
Phone Ban × Prior GPA is High within years of study		0.008		
		(0.037)		
Phone Ban × Prior GPA is Low within years of study		0.161***		
		(0.062)		
Phone Ban × First-year student			0.148***	
			(0.062)	
Phone Ban × Second-year student			-0.023	
			(0.045)	
Phone Ban × Third-year student			0.114**	
			(0.056)	
Phone Ban × Field of study is STEM				0.058
				(0.052)
Phone Ban × Field of study is not STEM				0.099**
				(0.043)
Observations	16,955	16,955	16,955	16,955
R^2	0.191	0.193	0.197	0.192
Adj. R^2	0.191	0.193	0.197	0.192

This table presents regression coefficients estimating the impact of the cohort-level smartphone ban on average student grades based on model (1). Each column reflects a different model specification. Grades are standardized by matriculation year. The sample includes Spring 2024 data from the full experimental population, including colleges with implementation issues. Standard errors (in parentheses) are clustered at the cohort level. One-sided significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.9: Ban impact on grades – standardized by pre-experiment statistics

	GPA (standardized by pre-period statistics)			
	(1)	(2)	(3)	(4)
Phone Ban	0.086***			
	(0.036)			
Phone Ban × Prior GPA is High		0.001		
		(0.040)		
Phone Ban × Prior GPA is Low		0.161***		
		(0.062)		
Phone Ban × First-year student			0.142**	
			(0.062)	
Phone Ban × Second-year student			-0.023	
			(0.041)	
Phone Ban × Third-year student			0.124**	
			(0.063)	
Phone Ban × Field of study is STEM				0.060
				(0.056)
Phone Ban × Field of study is not STEM				0.097**
				(0.046)
Observations	16,955	16,955	16,955	16,955
R^2	0.185	0.187	0.198	0.186
Adj. R^2	0.185	0.187	0.198	0.186

This table presents regression coefficients estimating how the cohort-level smartphone ban affects average grades, based on model (1). Each column shows a different specification. Grades are standardized using the mean and standard deviation from the pre-experiment period. The sample includes Spring 2024 data, including colleges with implementation issues. Standard errors (in parentheses) are clustered at the cohort level. One-sided significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

B.1.4 Changes to trade variation / inequality / concentration

Table B.10: Grade distribution metrics by policy group and period

Student-level metrics

A) Std-Dev Variation

Group	Timing vs. policy Pre	Post	Δ Pre/Post
Treat	90.1	95.26	5.16
Control	93.01	103.84	10.83
Δ Control/Treat	-2.91	-8.58	-5.67

B) HHI Concentration

Group	Timing vs. policy Pre	Post	Δ Pre/Post
Treat	3.63	0.61	-3.02
Control	0.84	23.47	22.64
Δ Control/Treat	2.79	-22.87	-25.66

Course-level metrics

C) Std-Dev Variation

Group	Timing vs. policy Pre	Post	Δ Pre/Post
Treat	73.22	93.33	20.11
Control	73.89	98.66	24.77
Δ Control/Treat	-0.67	-5.33	-4.66

D) HHI Concentration

Group	Timing vs. policy Pre	Post	Δ Pre/Post
Treat	0.46	0.22	-0.24
Control	0.22	2.6	2.38
Δ Control/Treat	0.24	-2.38	-2.62

Notes: This table report measures of variation of grades obtained by students. Each panel contrasts the treatment cohort (*ban*) with the control cohort (*no ban*) across pre- and post-policy periods. Panels A/C report the standard deviation of GPA; Panels B/D report the Herfindahl–Hirschman Index of GPA shares. "Δ control/treat" = Control – Treatment; "Δ pre/post" = Post-policy – Pre-policy within each cohort. Values are multiplied by 100 and rounded to two decimals.

B.1.5 Additional LOESS estimates

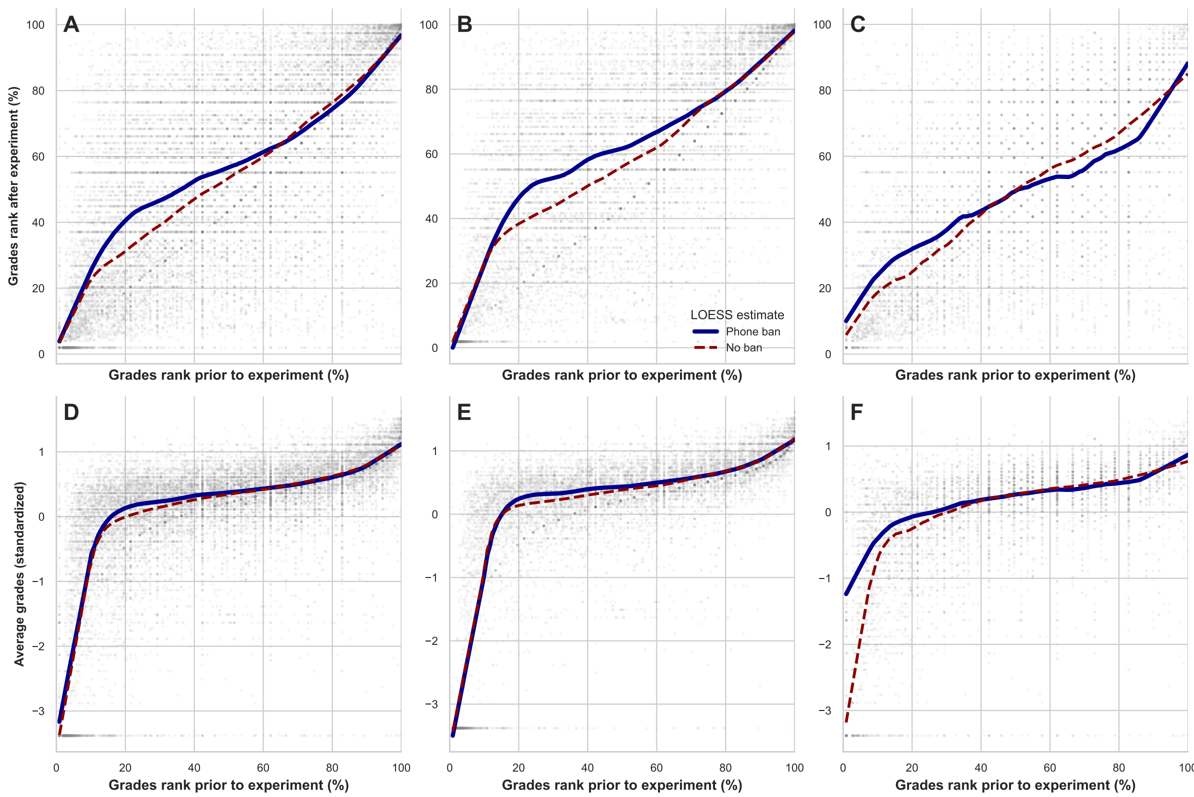

Figure A2: **Smartphone ban and academic performance across the grade distribution.** Locally weighted scatterplot smoothing (LOWESS) curves illustrate the relationship between students' baseline academic rank and post-intervention academic outcomes, separately for treatment (phone ban) and control group (no ban). The estimation bandwidth is set to the nearest 20% of student in terms of ranking in prior grades. The top panels (A-C) contain student rank as outcomes, while the bottom (D-F) show actual grades that are standardized. The left panels (A,D) contain all students while the middle panels (B,E) contain second and third year students, while the right panels (C,F) contain the first year students only.

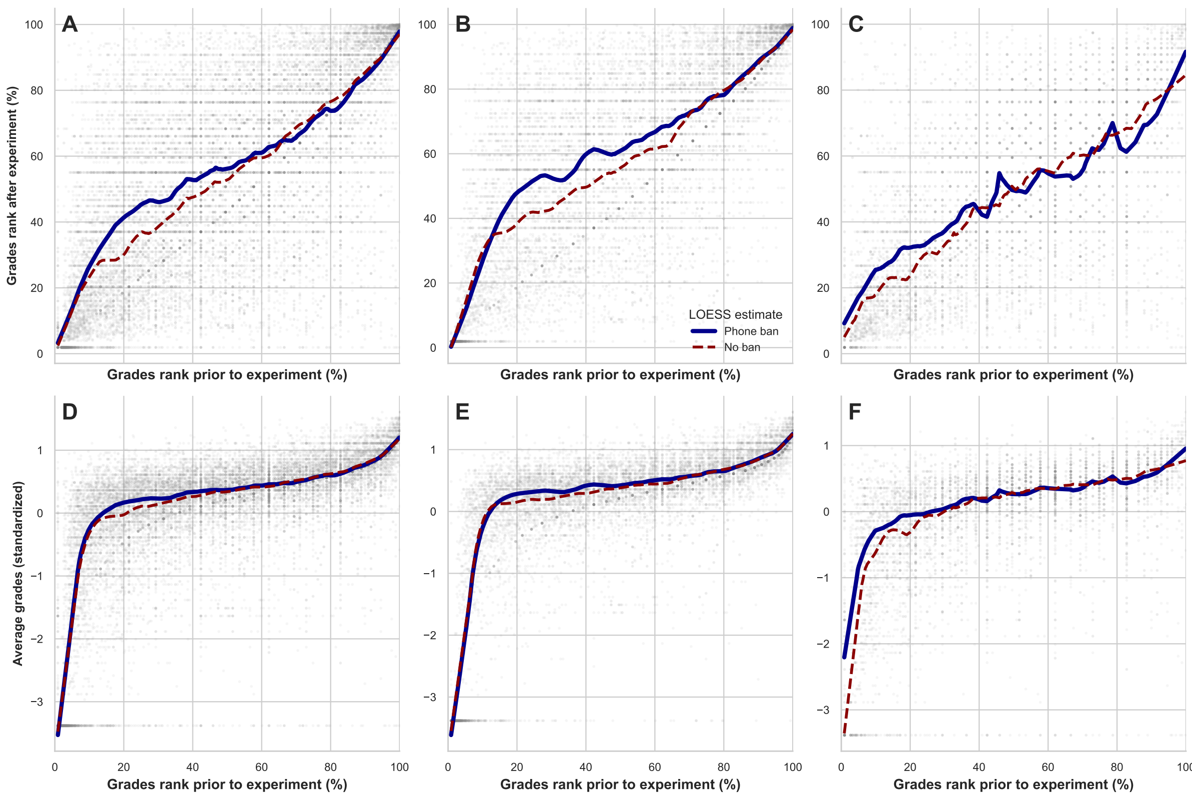

Figure A3: **Smartphone ban and academic performance across the grade distribution.** Locally weighted scatterplot smoothing (LOWESS) curves illustrate the relationship between students' baseline academic rank and post-intervention academic outcomes, separately for treatment (phone ban) and control group (no ban). The estimation bandwidth is set to the nearest 10% of student in terms of ranking in prior grades. The top panels (A-C) contain student rank as outcomes, while the bottom (D-F) show actual grades that are standardized. The left panels (A,D) contain all students while the middle panels (B,E) contain second and third year students, while the right panels (C,F) contain the first year students only.

B.2 Attendance as outcome

Table B.12: Ban impact on mean class attendance

	Mean attendance (proportion of total)			
	(1)	(2)	(3)	(4)
Phone Ban	-0.006			
	(0.013)			
Phone Ban $\times$ Prior GPA is High		-0.006		
		(0.017)		
Phone Ban $\times$ Prior GPA is Low		-0.005		
		(0.013)		
Phone Ban $\times$ First-year student			-0.007	
			(0.016)	
Phone Ban $\times$ Second-year student			0.003	
			(0.029)	
Phone Ban $\times$ Third-year student			-0.016	
			(0.020)	
Phone Ban $\times$ Field of study is STEM				0.007
				(0.021)
Phone Ban $\times$ Field of study is not STEM				-0.011
				(0.016)
Observations	16,932	16,932	16,932	16,932
R^2	0.004	0.004	0.007	0.005
Adj. R^2	0.003	0.003	0.006	0.005

This table presents regression estimates of how the cohort-level smartphone ban affects mean class attendance, using model (1) with MeanAttendance as the outcome. Each column shows a different model specification. The sample consists of observations from Spring 2024, including colleges with implementation issues. Standard errors are in parentheses and clustered at the cohort-level. Statistical significance (one-sided tests of positive impact): * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.13: Ban impact on course-level class attendance

	Course attendance (proportion of total)			
	(1)	(2)	(3)	(4)
Phone Ban in Course	-0.008			
	(0.014)			
Phone Ban in Course × Prior GPA is High		-0.013		
		(0.019)		
Phone Ban in Course × Prior GPA is Low		-0.005		
		(0.014)		
Phone Ban in Course × First-year student			-0.017	
			(0.018)	
Phone Ban in Course × Second-year student			0.003	
			(0.032)	
Phone Ban in Course × Third-year student			-0.016	
			(0.019)	
Phone Ban in Course × Field of study is STEM				-0.002
				(0.027)
Phone Ban in Course × Field of study is not STEM				-0.003
				(0.017)
Observations	75,719	75,719	75,719	75,719
R^2	0.002	0.002	0.004	0.002
Adj. R^2	0.002	0.002	0.004	0.002

This table presents regression estimates of how the course-level smartphone ban affects course-level class attendance, estimated from model (2) with MeanAttendance as the outcome. Each column reports a different model specification. The sample includes observations from Spring 2024, including colleges with implementation issues. Standard errors (in parentheses) are clustered at the cohort-level. Statistical significance is indicated by one-sided tests of positive impact: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

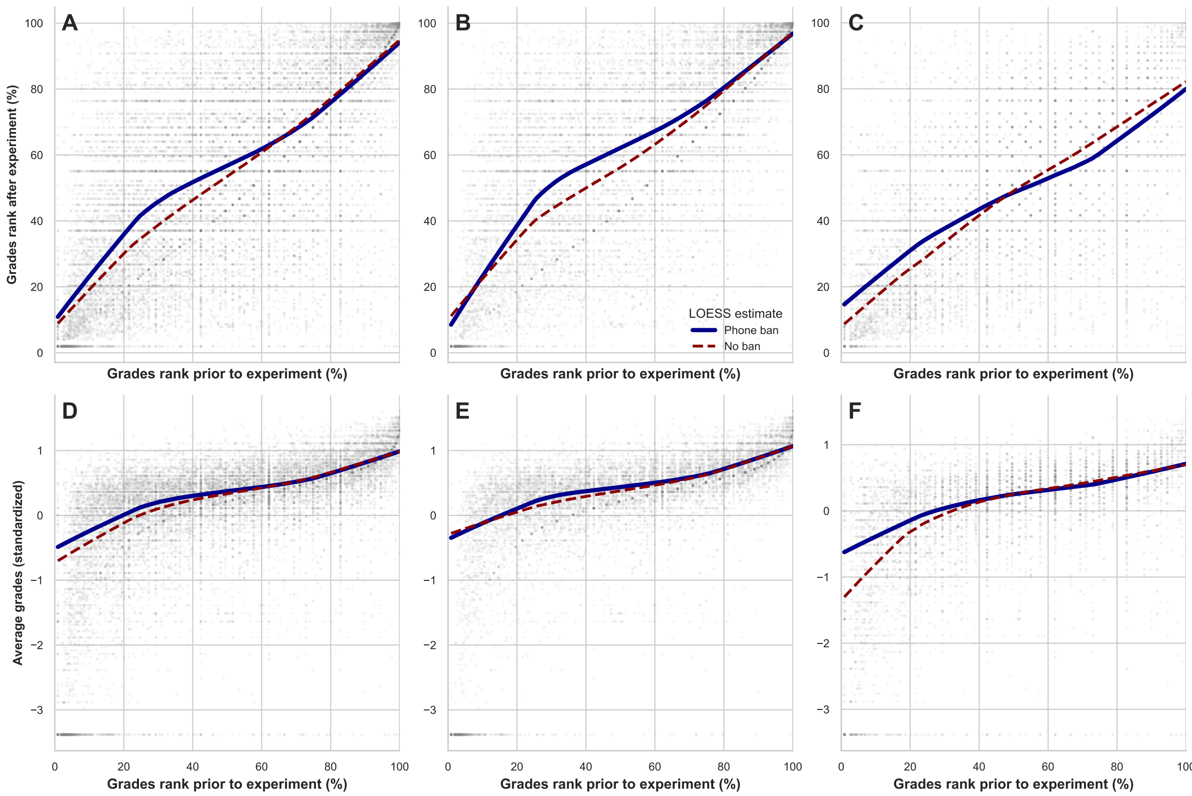

Figure A4: **Smartphone ban and academic performance across the grade distribution.** Locally weighted scatterplot smoothing (LOWESS) curves illustrate the relationship between students' baseline academic rank and post-intervention academic outcomes, separately for treatment (phone ban) and control group (no ban). The estimation bandwidth is set to the nearest 50% of student in terms of ranking in prior grades. The top panels (A-C) contain student rank as outcomes, while the bottom (D-F) show actual grades that are standardized. The left panels (A,D) contain all students while the middle panels (B,E) contain second and third year students, while the right panels (C,F) contain the first year students only.

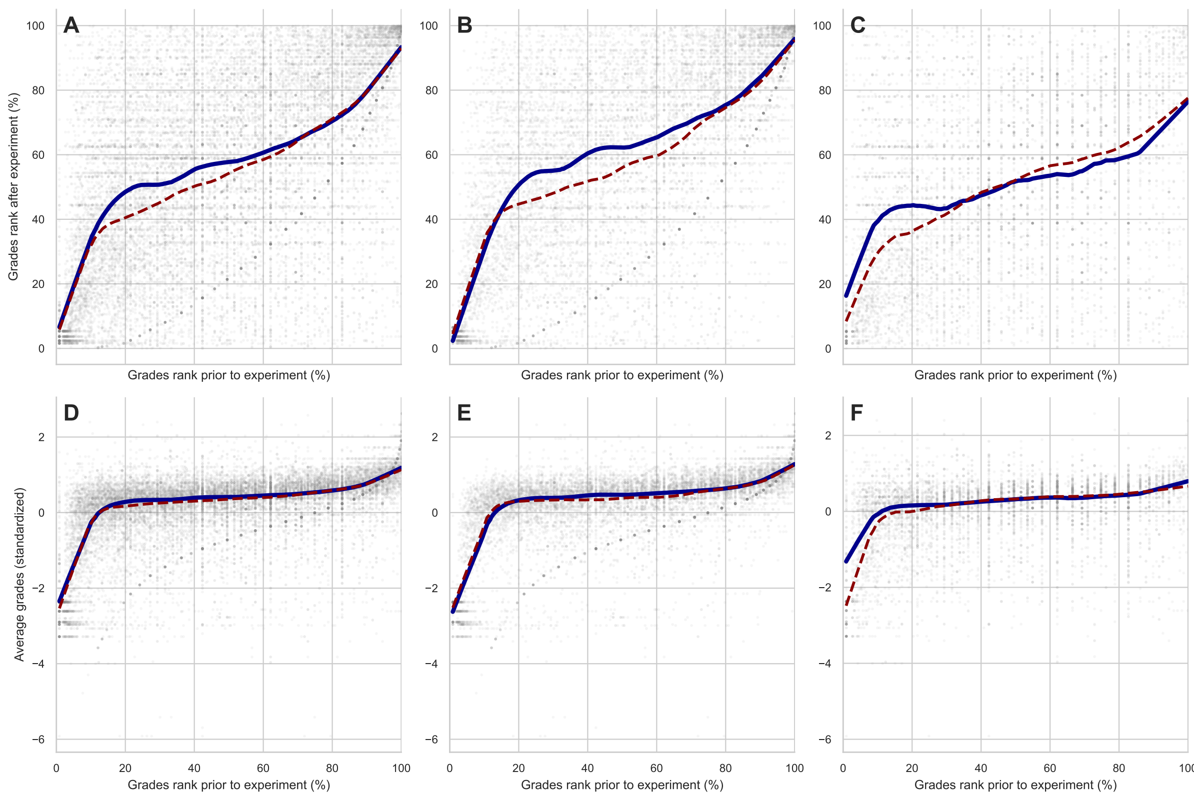

Figure A5: **Smartphone ban and within college academic performance across the grade distribution.** Locally weighted scatterplot smoothing (LOWESS) curves illustrate the relationship between students' baseline academic rank and post-intervention academic outcomes, separately for treatment (phone ban) and control group (no ban). The estimation bandwidth is set to the nearest 20% of student in terms of ranking in prior grades. The top panels (A-C) contain student rank within colleges as outcomes, while the bottom (D-F) show actual grades that are standardized within colleges. The left panels (A,D) contain all students while the middle panels (B,E) contain second and third year students, while the right panels (C,F) contain the first year students only.

C Supporting results for survey measures

We began with 14,349 survey entries, including 10,019 from the first wave and 4,330 from the second wave. We first removed any "idle" responses—those with no information entered[8]—followed by duplicate entries and entries with student IDs that did not match the official college administrative records. This initial cleaning yielded 10,366 surveys. Next, we focused on the 10 higher education institutions (HEIs) where our experiment and data collection took place, resulting in 5,109 responses. We then excluded surveys that were incomplete, failed the attention check, or were completed in less than three minutes or more than two hours. After these final steps, the dataset consisted of 2,557 valid surveys. Note that our results remain qualitatively similar if we use (i) the sample of 5,109 respondents without excluding for completeness and time criteria, or (ii) the broader dataset without restricting to the 10 participating HEIs.

[8]For instance, if a participant opened the survey link but did not complete any questions.

Table C.14: Regression results on Student Self-reported Distraction Factors Breakdown; the analysis follows the specification in Table 1. We estimate the following model: $Y_i = \alpha + \beta \times \text{PhoneBan}_i + \varepsilon_i$, where Y_i denotes the survey outcome variable and PhoneBan_i is the binary in-class phone-ban treatment status for student i. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables and report sharpened FDR q-values to correct for multiple-hypothesis testing, following (*20*). Significance at *p<0.1; **p<0.05; ***p<0.01

Outcome	N (1)	Phone Ban (2)	Sharpened Q-value (3)
Distraction Index	2378	0.1704* (0.0959)	0.1688
Distraction Avg	2378	0.1354* (0.0775)	0.1688
Phone usage	2517	0.1773 (0.1096)	0.1688
Using other electronic devices	2492	0.1901* (0.1110)	0.1688
Classmates phone usage	2498	0.2133** (0.1082)	0.1688
Phone notifications	2502	0.1697 (0.1160)	0.1758
Social media scrolling or messaging	2500	0.1848 (0.1134)	0.1688
Environmental Noise	2512	0.2344** (0.0970)	0.1688
Daydreaming	2517	0.1737* (0.0942)	0.1688
Overall Well-being	2510	0.1220 (0.1067)	0.2858
Influence of classmates (e.g., talking)	2505	0.1649 (0.1046)	0.1688
Sleep on previous night	2503	0.1563 (0.1068)	0.1758
Time of day	2522	0.1840* (0.1014)	0.1688
Course subject	2511	0.0935 (0.0949)	0.3408

Table C.15: Regression results on Student Self-reported FOMO Items Breakdown; the analysis follows the specification in Table 1. We estimate the following model: $Y_i = \alpha + \beta \times \text{PhoneBan}_i + \varepsilon_i$, where Y_i denotes the survey outcome variable and PhoneBan_i is the binary in-class phone-ban treatment status for student i. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables and report sharpened FDR q-values to correct for multiple-hypothesis testing, following (*20*). Significance at *p<0.1; **p<0.05; ***p<0.01

Outcome	N (1)	Phone Ban (2)	Sharpened Q-value (3)
FOMO Index	2557	0.1790** (0.0877)	0.1049
FOMO Avg	2557	0.1383** (0.0705)	0.1049
I fear others have more rewarding experiences than me	2557	0.1663** (0.0808)	0.1049
I fear my friends have more rewarding experiences than me	2557	0.1697* (0.0941)	0.1049
I get worried when I find out my friends are having fun without me	2557	0.1915* (0.1029)	0.1049
I get anxious when I do not know what my friends are up to	2557	0.1564 (0.1003)	0.1361
It is important that I understand my friends ”in jokes”	2557	0.2050** (0.0912)	0.1049
Sometimes, I wonder if I spend too much time keeping up with what is going on	2557	0.1835* (0.0956)	0.1049
It bothers me when I miss an opportunity to meet up with friends	2557	0.1742* (0.0978)	0.1049
When I have a good time it is important for me to share the details online (e.g. updating status)	2557	0.1734* (0.1005)	0.1066
When I miss out on a planned get-together it bothers me	2557	0.2206** (0.0949)	0.1049
When I go on vacation, I continue to keep tabs on what my friends are doing	2557	0.1319 (0.1025)	0.2079

Table C.16: Regression results on Student Self-reported Focus Factors Breakdown; the analysis follows the specification in Table 1.We estimate the following model: $Y_i = \alpha + \beta \times \text{PhoneBan}_i + \varepsilon_i$, where Y_i denotes the survey outcome variable and PhoneBan_i is the binary in-class phone-ban treatment status for student i. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables and report sharpened FDR q-values to correct for multiple-hypothesis testing, following (*20*). Significance at *p<0.1; **p<0.05; ***p<0.01

Outcome	N (1)	Phone Ban (2)	Sharpened Q-value (3)
Focus Index	2557	0.1482* (0.0853)	0.2155
Focus Avg	2557	0.1083 (0.0664)	0.2155
Focus Impact Phone	2557	0.1430 (0.1170)	0.2458
Focus Impact Classmate Messaging	2557	0.1404 (0.1180)	0.2458
Focus Impact Classmate Chatting	2557	0.1233 (0.1014)	0.2458
Focus Impact Thought/Daydreaming	2557	0.1750* (0.0951)	0.2155
Focus Impact Tablet/Laptop	2557	0.1463 (0.1168)	0.2458
Focus Impact Tiredness	2557	0.1818* (0.0958)	0.2155
Focus Impact Time to Prepare	2557	0.1091 (0.0801)	0.2458
Focus Impact Teaching Quality	2557	0.1156 (0.0705)	0.2155
Focus Impact Family Effect Performance	2557	0.1241 (0.0824)	0.2383
Focus Impact Distance	2557	0.1758* (0.0995)	0.2155

*p<0.1; **p<0.05; ***p<0.01

Table C.17: Robustness: Regression results for student-survey outcomes (Table 1) with school and semester fixed effects; the analysis follows the specification in Table 1.We estimate the following model: $Y_i = \alpha + \beta \times \text{PhoneBan}_i + \varepsilon_i$, where Y_i denotes the survey outcome variable and PhoneBan_i is the binary in-class phone-ban treatment status for student i. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables and report sharpened FDR q-values to correct for multiple-hypothesis testing, following (*20*). Significance at $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Outcome	Phone Ban (1)	Sharpened Q-value (2)
Panel A. Policy Preference and Attitudes Toward Phone Ban		
Supports Phone Ban	0.1797*** (0.0555)	0.0044
Preference Towards No Phone Ban	-0.1519*** (0.0501)	0.0059
Perceived Benefits of Phone Ban	0.1932*** (0.0535)	0.0023
Panel B. Impact on Perceived Educational Experience		
Student Subjective Well-being Index	0.0493 (0.0550)	0.3398
Perceived In-class Distraction Frequency	0.0290 (0.0456)	0.3861
Academic Thriving Index	0.0469 (0.0498)	0.3398
Perceived Learning	0.0648 (0.0474)	0.2104
Panel C. Effects on Digitally Pertinent Outcomes		
Fear of Missing Out Index	0.1307** (0.0528)	0.0245
Mean Total Phone Screen Time	0.0281 (0.0560)	0.4116
Bring Phone to School	-0.0710 (0.0490)	0.2104
Experienced Any Online Harassment	0.0363 (0.0495)	0.3785

Table C.18: Robustness: Regression results for student-survey outcomes (Table 1) include surveys with partial responses (i.e., unfinished) and those completed in more than three hours; the analysis follows the specification in Table 1. We estimate the following model: $Y_i = \alpha + \beta \times \text{PhoneBan}_i + \varepsilon_i$, where Y_i denotes the survey outcome variable and PhoneBan_i is the binary in-class phone-ban treatment status for student i. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables and report sharpened FDR q-values to correct for multiple-hypothesis testing, following (*20*). Significance at *p<0.1; **p<0.05; ***p<0.01

Outcome	Phone Ban (1)	Sharpened Q-value (2)
Panel A. Policy Preference and Attitudes Toward Phone Ban		
Supports Phone Ban	0.1197**	0.0418
	(0.0483)	
Preference Towards No Phone Ban	-0.2015***	0.0004
	(0.0491)	
Perceived Benefits of Phone Ban	0.1485***	0.0145
	(0.0502)	
Panel B. Impact on Perceived Educational Experience		
Student Subjective Well-being Index	0.0283	0.6241
	(0.0494)	
Perceived In-class Distraction Frequency	0.0532	0.3644
	(0.0427)	
Academic Thriving Index	-0.0067	0.7588
	(0.0457)	
Perceived Learning	0.0095	0.7588
	(0.0427)	
Panel C. Effects on Digitally Pertinent Outcomes		
Fear of Missing Out Index	0.1016	0.3386
	(0.0695)	
Mean Total Phone Screen Time	0.0355	0.6241
	(0.0667)	
Bring Phone to School	-0.0368	0.6241
	(0.0523)	
Experienced Any Online Harassment	0.0391	0.3644
	(0.0327)	

Table C.19: Balance Table on Survey Sample. The analysis follows the specification described in Table 1. We dropped two individuals who reported a non-binary gender. Note that Prior GPA is only available for students that are matched with the admin data and phone ownership question is only available from the first wave of the survey. We estimate the following model: $Y_i = \alpha + \beta \times \text{PhoneBan}_i + \varepsilon_i$, where Y_i denotes the survey outcome variable and PhoneBan_i is the binary in-class phone-ban treatment status for student i. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables and report sharpened FDR q-values to correct for multiple-hypothesis testing, following (*20*). Significance at *p<0.1; **p<0.05; ***p<0.01

Outcome	N (1)	Phone Ban (2)	Sharpened Q-value (3)
Age	2555	0.1310 (0.1453)	0.7334
Male	2555	0.0451 (0.0354)	0.7334
Classroom Seating (Front vs Mid-or-back Bench)	2555	0.0229 (0.0326)	0.7334
Household Income (INR)	2531	-1067.9827 (1326.2639)	0.7334
Years of Phone Ownership	1519	0.0670 (0.1194)	0.7334
Prior GPA	1816	-0.0226 (0.0505)	0.7334
Grade Level	2555	0.0946 (0.2441)	0.7334

*p<0.1; **p<0.05; ***p<0.01

Table C.20: Correlation with Prior GPA in the control arm. The analysis follows the specification described in Table 1. We restrict the analysis to the control arm to avoid any bias induced by the phone-ban treatment. We estimate the following model: $Y_i = \alpha + \beta \times \text{Grade}_i + \varepsilon_i$, where Y_i denotes the survey outcome variable and Grade_i is the GPA of student i prior to the experiment. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables and report sharpened FDR q-values to correct for multiple-hypothesis testing, following (*20*). Significance at *p<0.1; **p<0.05; ***p<0.01

	N (1)	Grade (2)	Sharpened Q-value (3)
Preference Towards No Phone Ban	874	0.0000 (0.0818)	0.6792
Perceived Benefits of Phone Ban	874	0.0132 (0.0760)	0.6640
Support For Phone Ban Policy	874	0.0553 (0.0777)	0.4586
Student Subjective Well-being Index	874	0.1023 (0.0986)	0.3457
Perceived In-class Distraction Frequency	874	-0.0317 (0.0649)	0.5556
Academic Thriving Index	874	0.2052*** (0.0602)	0.0037
Perceived Learning	874	0.1474** (0.0616)	0.0322
Fear of Missing Out Index	874	-0.1095 (0.0712)	0.1594
Last Month Avg Phone Screen Time	874	0.0679 (0.0778)	0.4020
Bring Phone to School Frequency	874	0.1662** (0.0647)	0.0235
Age	874	0.0001 (0.1046)	0.6792
Male	874	-0.0879** (0.0401)	0.0467
Classroom Seating (Front vs Mid-or-back Bench)	874	-0.0705* (0.0407)	0.1202
Household Income	863	4926.1852*** (1719.6790)	0.0121
Years of Phone Ownership	527	0.0272 (0.1338)	0.6640
Prior GPA	874	1.0000*** (0.0000)	0.0000
Grade Level	874	0.5117*** (0.1722)	0.0114

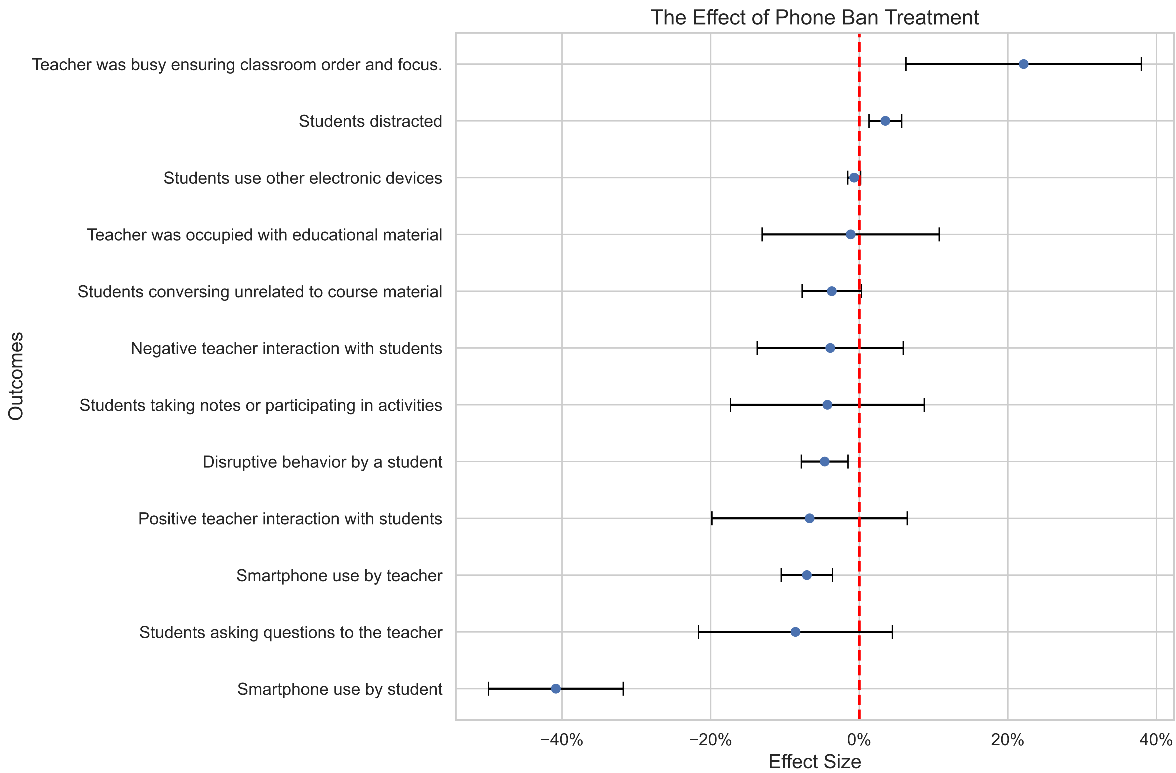

Figure A6: Robustness: Figure 2 without college-level re-weighting. Results are based on unannounced *in-class* spot checks by independent enumerators. During each visit, an enumerator randomly selected a classroom, observed the session unobtrusively for under a minute (e.g. through a window or doorway), and coded a binary indicator for every behaviour listed above. We estimate the linear-probability model $Y_i = \alpha + \beta \times \text{PhoneBan}_i + \varepsilon_i$, where Y_i is a binary outcome and $\text{PhoneBan}_i = 1$ if student i was subject to the in-class phone ban. Standard errors are clustered at the treatment-assignment level (department $\times$ gra1‘de within school). Bars show 95% confidence intervals. $N = 7{,}797$ enumerator spot checks. Gray shaded areas reflect teacher-involved behaviors.

Table C.21: Preferred phone ban policy. Estimates reflect a multinomial logit model. Omitted case is the preference towards no ban. Significance at *p<0.1; **p<0.05; ***p<0.01

Alternative	Constant	Phone Ban
Prefer All Day Ban	0.5395***	0.7579***
	(0.1360)	(0.1999)
Prefer Lecture Only Ban	1.2094***	0.6028***
	(0.1294)	(0.1823)
Prefer Phone Free Zone	-0.0308	0.5498***
	(0.1429)	(0.1959)
Prefer App Solution	-0.5210***	0.4349*
	(0.1686)	(0.2445)

Table C.22: Pairwise p-values for testing the equality of means for Phone Ban Estimates in Table C.21

Preferred Ban Policy:	All Day Ban	Lecture Only Ban	Phone Free Zone	App Solution
Prefer All Day Ban		0.5663	0.4572	0.3064
Prefer Lecture Only Ban			0.8432	0.5821
Prefer Phone Free Zone				0.7138

D Heterogeneity in Survey Outcomes

D.1 Location in Classroom (Front and Back "Benchers")

Table D.23: Heterogeneity: location in the classroom. We split the sample based on students self-reported outcome on where they typically sit in the classroom (front, middle or back seats) and re-run the model describe in Table 1. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables. Significance at *p<0.1; **p<0.05; ***p<0.01

	Front Row		Middle/Back Row		*p-value for t-test:*
	Phone Ban (SE)	N	Phone Ban (SE)	N	Front = Back
Preference Towards No Phone Ban	-0.1594** (0.0733)	1201	-0.2117*** (0.0644)	1356	0.5343
Perceived Benefits of Phone Ban	0.2797*** (0.0725)	1201	0.1233 (0.0759)	1356	0.0998
Support For Phone Ban Policy	0.2851*** (0.0687)	1201	0.0801 (0.0755)	1356	0.0223
Student Subjective Well-being Index	0.1233 (0.0793)	1201	0.0536 (0.0676)	1356	0.4340
Perceived In-class Distraction Frequency	0.0703 (0.0707)	1201	0.0579 (0.0674)	1356	0.8819
Academic Thriving Index	0.1184* (0.0693)	1201	0.0093 (0.0665)	1356	0.2005
Perceived Learning	0.1312** (0.0636)	1201	0.0348 (0.0640)	1356	0.2349
Fear of Missing Out Index	0.1445* (0.0861)	1201	0.2100* (0.1187)	1356	0.5752
Last Month Avg Phone Screen Time	0.0425 (0.0867)	1201	0.1127 (0.1072)	1356	0.5155
Bring Phone to School Frequency	-0.0618 (0.0729)	1201	-0.0294 (0.0650)	1356	0.6790
Experience Any Online Harassment	0.0046 (0.0724)	1201	0.1127 (0.0748)	1356	0.2311

D.2 Prior GPA

Table D.24: Heterogeneity: Prior GPA. We split the sample based on above (High) and below-median (Low) GPA students and re-run the model describe in Table 1. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables. Note that despite we have the grade data from all students from our 10 partner HEIs, we were able to (exactly) match 1,818 students GPA data with their corresponding survey. Significance at *p<0.1; **p<0.05; ***p<0.01

	Below Median Grade		Above Median Grade		*p-value for t-test:*
	Phone Ban (SE)	N	Phone Ban (SE)	N	Low GPA = High GPA
Preference Towards No Phone Ban	-0.2546*** (0.0886)	909	-0.2014** (0.0818)	909	0.6060
Perceived Benefits of Phone Ban	0.1173 (0.0876)	909	0.2970*** (0.0760)	909	0.0814
Support For Phone Ban Policy	0.1084 (0.0899)	909	0.2765*** (0.0790)	909	0.1297
Student Subjective Well-being Index	0.1105 (0.0897)	909	0.1310 (0.0828)	909	0.8491
Perceived In-class Distraction Frequency	0.0321 (0.0839)	909	0.0731 (0.0888)	909	0.7054
Academic Thriving Index	0.1293* (0.0782)	909	0.0812 (0.0774)	909	0.6259
Perceived Learning	0.1412* (0.0768)	909	0.0820 (0.0753)	909	0.5488
Fear of Missing Out Index	0.3116** (0.1374)	909	0.1652* (0.0984)	909	0.3179
Last Month Avg Phone Screen Time$_z$	0.1689 (0.1410)	909	0.0248 (0.0984)	909	0.3171
Bring Phone to School Frequency	-0.0607 (0.0910)	909	-0.0436 (0.0818)	909	0.8768
Personal Online Experience EM$_z$	0.1184 (0.0846)	909	0.0130 (0.0851)	909	0.3466

D.3 Gender

Table D.25: Heterogeneity: Gender. We split the sample into male and female students and re-run the model described in Table 1. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables. Significance at *p<0.1; **p<0.05; ***p<0.01.

	Female		Male		*p-value for t-test:*
	Phone Ban (SE)	N	Phone Ban (SE)	N	Female = Male
Preference Towards No Phone Ban	-0.1739*** (0.0600)	1676	-0.2235** (0.0996)	879	0.6534
Perceived Benefits of Phone Ban	0.2778*** (0.0599)	1676	0.0766 (0.0944)	879	0.0508
Support For Phone Ban Policy	0.2531*** (0.0607)	1676	0.0617 (0.0931)	879	0.0603
Student Subjective Well-being Index	0.1180* (0.0677)	1676	0.0255 (0.0934)	879	0.3777
Perceived In-class Distraction Frequency	-0.0171 (0.0631)	1676	0.2117*** (0.0767)	879	0.0115
Academic Thriving Index	0.0740 (0.0616)	1676	0.0343 (0.0838)	879	0.6792
Perceived Learning	0.1244** (0.0575)	1676	0.0054 (0.0821)	879	0.2091
Fear of Missing Out Index	0.0780 (0.0722)	1676	0.3484** (0.1451)	879	0.0388
Last Month Avg Phone Screen Time	-0.0304 (0.0695)	1676	0.2483** (0.1192)	879	0.0075
Bring Phone to School Frequency	-0.0938 (0.0643)	1676	0.0248 (0.0826)	879	0.2037
Experience Any Online Harassment	0.0708 (0.0658)	1676	0.0389 (0.0877)	879	0.7393

D.4 Field of Study: Natural Science *vs.* Social Science and Humanities

Table D.26: Heterogeneity: natural vs social sciences. We split the sample based on their field, and re-run the model described in Table 1. Standard errors are clustered at the same level as the treatment assignment: department-grade-institute level. We apply an inverse-covariance indexing method for composite (Index) variables. Significance at *p<0.1; **p<0.05; ***p<0.01.

	Natural Sciences		Social Sciences and Humanities		*p-value for t-test:*
	Phone Ban (SE)	N	Phone Ban (SE)	N	STEM = Non-STEM
Preference Towards No Phone Ban	-0.1575* (0.0813)	1106	-0.2113*** (0.0732)	1451	0.6230
Perceived Benefits of Phone Ban	0.1766** (0.0827)	1106	0.2088** (0.0814)	1451	0.7807
Support For Phone Ban Policy	0.0786 (0.0801)	1106	0.2456*** (0.0834)	1451	0.1487
Student Subjective Well-being Index	0.0906 (0.0850)	1106	0.0724 (0.0803)	1451	0.8763
Perceived In-class Distraction Frequency	0.1320* (0.0755)	1106	0.0258 (0.0794)	1451	0.3322
Academic Thriving Index	0.0329 (0.0752)	1106	0.0849 (0.0709)	1451	0.6143
Perceived Learning	0.0338 (0.0711)	1106	0.1086 (0.0697)	1451	0.4522
Fear of Missing Out Index	0.2451** (0.1245)	1106	0.1191 (0.1184)	1451	0.4631
Last Month Avg Phone Screen Time	0.1820 (0.1157)	1106	0.0280 (0.1214)	1451	0.3580
Bring Phone to School Frequency	-0.0109 (0.0853)	1106	-0.0519 (0.0750)	1451	0.7181
Experience Any Online Harassment	0.0911 (0.0968)	1106	0.0241 (0.0657)	1451	0.5669

E Survey Instrument

E.1 Preference Towards No Phone Ban

This variable was created by binarizing the question below: it takes the value 1 if ”No ban” was selected, and 0 otherwise.

Question: If one of the following options were available as an in-class mobile phone usage policy, which one would you prefer?

1. A phone ban during entire school hours (e.g., storing the phone in a secured location until the end of all lectures)
2. A phone ban during lecture hours (e.g., putting phones in a box during classes, phones are allowed during breaks)
3. Phone-free zones: phones can only be used in a dedicated area on the campus
4. A usage-restriction app: a smartphone app will limit your access to certain apps (e.g., social media) during school hours
5. No ban: full flexibility

E.2 Perceived Benefits of Phone Ban

Question: Do you think collecting phones in a box during class would be good for you?

1. Strongly disagree
2. Somewhat disagree
3. Neither agree nor disagree
4. Somewhat agree
5. Strongly agree

E.3 Support For Phone Ban Policy

Question: Would you support such policy? (previous question)

1. Strongly oppose
2. Somewhat oppose
3. Neutral / indifferent
4. Somewhat support
5. Strongly support

E.4 Student Subjective Well-being Index

Question: Here are some questions about your college experience. Read each sentence and choose the one response that best describes how you felt in the past month.

(1 = Strongly disagree, 2 = Disagree, 3 = Slightly disagree, 4 = Neutral, 5 = Slightly agree, 6 = Agree, 7 = Strongly agree)

Statement	1	2	3	4	5	6	7
I have had a great academic experience at this college.	( )	( )	( )	( )	( )	( )	( )
I am a hard worker in my classes.	( )	( )	( )	( )	( )	( )	( )
I feel like a real part of this college.	( )	( )	( )	( )	( )	( )	( )
I am so thankful that I'm getting a college education.	( )	( )	( )	( )	( )	( )	( )
I am happy with how I've done in my classes.	( )	( )	( )	( )	( )	( )	( )
I am a diligent student.	( )	( )	( )	( )	( )	( )	( )
People at this college are friendly to me.	( )	( )	( )	( )	( )	( )	( )
I am grateful to the professors and other students who have helped me in class.	( )	( )	( )	( )	( )	( )	( )
I am satisfied with my academic achievements since coming to college.	( )	( )	( )	( )	( )	( )	( )
I am an organized and effective student.	( )	( )	( )	( )	( )	( )	( )
I can really be myself at this college.	( )	( )	( )	( )	( )	( )	( )
I feel thankful for the opportunity to learn so many new things.	( )	( )	( )	( )	( )	( )	( )
I am pleased with how my college education is going so far.	( )	( )	( )	( )	( )	( )	( )
I study well for my classes.	( )	( )	( )	( )	( )	( )	( )
Other students here like me the way I am.	( )	( )	( )	( )	( )	( )	( )
I am grateful for those who have helped me succeed in college.	( )	( )	( )	( )	( )	( )	( )

E.5 Perceived In-class Distraction Frequency

Question: How often do you find yourself getting distracted in a typical class?

1. Never
2. Rarely
3. Sometimes
4. Often
5. Always

E.6 Academic Thriving Index

Note: These are slider questions.

Question: Compared with other things you do, how important is it to you to do well in your classes?

0 = Not at all important to do well *100 = Extremely important to do well*

Question: Compared to other things you do, how interesting are your classes?

0 = Not at all interesting *100 = Extremely interesting*

Question: Do you feel like you can succeed in your classes, if you try?

0 = I don't feel like I can succeed at all *100 = I feel like I totally can succeed*

Question: In your college, do you feel like you fit in?

0 = No, I don't feel like I fit in at all *100 = Yes, I feel like I totally fit in*

Question: How much do you think you learn in college?

0 = I don't feel like I learn at all *100 = I feel like I totally learn a lot*

E.7 Perceived Learning

Note: This is a slider question.

Question: How much do you think you learn in college?

0 = I don't feel like I learn at all *100 = I feel like I totally learn a lot*

E.8 Fear of Missing Out Index

Question: Below is a collection of statements about your everyday experience. Using the scale provided please indicate how true each statement is of your general experiences. Please answer according to what really reflects your experiences rather than what you think your experiences should be. Please treat each item separately from every other item.

Scale: 1 = Not at all true of me   2 = Slightly true of me   3 = Moderately true of me   4 = Very true of me   5 = Extremely true of me

Statement	**1**	**2**	**3**	**4**	**5**
I fear others have more rewarding experiences than me.	( )	( )	( )	( )	( )
I fear my friends have more rewarding experiences than me.	( )	( )	( )	( )	( )
I get worried when I find out my friends are having fun without me.	( )	( )	( )	( )	( )
I get anxious when I don't know what my friends are up to.	( )	( )	( )	( )	( )
It is important that I understand my friends ”in jokes”.	( )	( )	( )	( )	( )
Sometimes, I wonder if I spend too much time keeping up with what is going on.	( )	( )	( )	( )	( )
It bothers me when I miss an opportunity to meet up with friends.	( )	( )	( )	( )	( )
When I have a good time it is important for me to share the details online (e.g. updating status).	( )	( )	( )	( )	( )
When I miss out on a planned get-together it bothers me.	( )	( )	( )	( )	( )
When I go on vacation, I continue to keep tabs on what my friends are doing.	( )	( )	( )	( )	( )

E.9 Last Month Avg Phone Screen Time

Question: On an average day in the last month, how many hours have you spent looking at your phone?

Please choose a number from 0 to 10.

Scale: 0 1 2 3 4 5 6 7 8 9 10

E.10 Bring Phone to School Frequency

Question: How often do you bring your mobile phone to school?

1. Never
2. Less than once a week
3. Once or twice a week
4. On most days
5. Always

E.11 Personal Online Experience EM

This variable was generated by assigning a value of 1 if the student selected at least one of the items from the list below, and 0 if none were selected.

Question: Thinking about your experiences online or on your mobile phone, which of the following, if any, has ever happened to you personally?

- ❑ Been called offensive names (1)
- ❑ Been threatened with physical harm (2)

- ❑ Had someone spread false rumors about you (3)
- ❑ Had someone share explicit images of you without your consent (4)
- ❑ Had someone send you explicit images you did not ask for (5)
- ❑ Had someone, other than a parent, constantly ask where you were, who you were with or what you were doing (6)

E.12 Distraction Questions

Question: For each of the following sources, please indicate the extent to which it affects your level of distraction during a class.

1 = No impact at all 2 = Slight impact 3 = Moderate impact 4 = Significant impact 5 = It affects a lot

Source of Distraction	**1**	**2**	**3**	**4**	**5**
Overall using my mobile phone	( )	( )	( )	( )	( )
Using other electronic devices (e.g., laptop or tablet)	( )	( )	( )	( )	( )
My classmates using their mobile phones	( )	( )	( )	( )	( )
Phone Notifications	( )	( )	( )	( )	( )
Social media scrolling and messaging	( )	( )	( )	( )	( )
Environmental noise (e.g., traffic)	( )	( )	( )	( )	( )
Personal thoughts / daydreaming	( )	( )	( )	( )	( )
My overall well-being	( )	( )	( )	( )	( )
Influence of my classmates (e.g., talking during class)	( )	( )	( )	( )	( )
Sleep on the previous night	( )	( )	( )	( )	( )
Time of day	( )	( )	( )	( )	( )
Subject matter interest (e.g., how interesting or boring a course is)	( )	( )	( )	( )	( )

E.13 Focus Questions

Question: How concerned are you about the following factors impacting your focus during class?

1 = Not at all concerned   2 = Slightly concerned   3 = Moderately concerned   4 = Very concerned   5 = Extremely concerned

Factor	**1**	**2**	**3**	**4**	**5**
Looking at my phone during class	( )	( )	( )	( )	( )
Classmates messaging me on my phone during class	( )	( )	( )	( )	( )
Classmates in person (talking to me, signaling to me) during class	( )	( )	( )	( )	( )
My own thoughts or daydreaming during class	( )	( )	( )	( )	( )
My tablet or laptop (if allowed in class) during class	( )	( )	( )	( )	( )
Being tired in the class	( )	( )	( )	( )	( )
Finding enough time to prepare for classes and exams	( )	( )	( )	( )	( )
Quality of teaching	( )	( )	( )	( )	( )
My family's influence on my academic performance	( )	( )	( )	( )	( )
Impact of the distance to college on my academic performance	( )	( )	( )	( )	( )