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Behavioral Nudges Prevent Student Dropouts in the Pandemic

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ABSTRACT:

Background: Student dropouts are a major concern in developing countries; even before the pandemic, one out of three Brazilian students dropped out before graduating high school. School closures in the context of COVID-19 have been shown to magnify that problem, with at least seven million additional dropouts worldwide in 2020. Despite efforts from governments around the world to mitigate learning gaps by the time in-person classes return, interventions to motivate students to remain in school until then have been overlooked. In particular, behavioral nudges sent to parents' cell phones through text messages had shown promise in preventing student dropouts in developing countries before the pandemic. Having said that, such nudges typically work by leading parents to show up in school to a greater extent and monitor teachers more closely – a mechanism that might not be meaningful in the absence of in-person classes.

Methods: We conducted a cluster-randomized control trial with 18,256 high-school students in the State of Goiás, Brazil, randomizing 2/3 of them to receive behavioral nudges through text messages, between June and December 2020. The control group did not receive any messages. Within the treatment group, we additionally randomized students to variations in nudges' content, to study whether behavioral insights linked to framing and social pressure would lead to higher impacts. We estimate the impacts of nudges on dropout risk over the course of the school year, using administrative data from the State Secretariat of Education on whether students took math and Portuguese exams each school quarter. We also estimate heterogeneous impacts of nudges by risk levels (higher for boys, sophomore and junior students, and those below-median first-quarter Portuguese GPA), by whether messages were sent to parents' phones or directly to students, and by whether schools already offered online academic activities prior to the pandemic.

Findings: Nudges decreased dropout risk by around 26% over the course of the school year. Effects increased with exposure, and were concentrated in students at the highest risk of dropouts. Nudges only worked when sent directly to students' phones, and in schools that already offered online academic activities prior to the pandemic. Framing content in terms of the upside of graduating high school led to higher impacts than framing it in terms of the downside of dropping out. Alluding to peer motivation to return to in-person classes to leverage social pressure had no additional effects on dropout risk.

Interpretation: Results show that behavioral nudges can partially mitigate the dramatic increase in student dropouts during school closures by keeping adolescents motivated to stay in school. The patterns of heterogeneous treatment effects are consistent with complementarities between motivation and academic instruction. All in all, our results showcase that insights from the science of adolescent psychology can be leveraged to shift developmental trajectories at a critical juncture, but also raise caution against indiscriminately applying behavioral insights derived from evaluations of similar interventions in contexts of in-person classes or static decision-making.

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Student dropouts are a major concern in developing countries: as of 2010, one out of three Brazilian students dropped out before graduating high school; in Ivory Coast, that figure was two out of three.[1,2] Not finishing high school is associated with a lower probability of being employed, lower expected wages, and a higher probability of adverse outcomes such as teenage pregnancies, violence and incarceration.[3,4,5,6,7,8] Such concerns loom even larger in the context of the COVID-19 pandemic, which forced 1.6 billion children and adolescents across 160 countries to stay at home while schools were shut down on sanitary grounds.[9,10] Research on the impacts of similar historical events, such as the Spanish flu, Ebola and H1N1, documents that pandemics not only cost lives and employment, but also substantially deteriorate learning outcomes of school-age children,[11,12,13] with persistent impacts on their future labor market outcomes.[11] Multiple forces combine to push children out of school in such settings: lower returns to education in face of the economic crunch, demand for child labor among poor families, violence against children in a context of stress and with children at home, and loss of motivation to go to school in the absence of face-to-face interactions with teachers and peers.[14,15,16,17,18,19,20] As such, a surge in student dropouts is expected to take place unless drastic actions are put in place.[10,21] In fact, recent evidence showcases that dropout risk has increased during school closures by a factor of 2 or more;[10,22] as a result, seven million additional dropouts are expected by the time in-person classes return.[9]

While significant attention has been devoted to interventions that can support remote learning, particularly to mitigate learning deficits by the time children come back to in-person classes,[23,24,25,26] a large fraction of children and adolescents might never actually return.[9,10] Nevertheless, interventions to motivate students to remain in school until then have been largely overlooked. Recent evidence shows that, in developing countries, behavioral nudges – typically sent through text messages to parents' cell phones – have the potential to not only significantly improve learning outcomes,[27,28,29,30,31,32] but also to drastically decrease dropouts before the pandemic.[33] Having said that, it would be surprising if those effects replicated during school closures, because such nudges typically work by inducing parents to show up in school to a greater extent and monitor teachers more closely[33] – a mechanism that might not be meaningful in the absence of in-person classes.

Here we show that behavioral nudges to motivate high-school students to stay engaged with school activities during the pandemic substantially decreased dropout risk during school closures in Brazil.

Research Design and Intervention

To study this question, we conducted a cluster-randomized control trial in the State of Goiás, Brazil (pre-registered as trial 5986 at the AEA RCT Registry), in partnership with Instituto Sonho Grande and the Goiás State Secretariat of Education in the context of their full-time high school program (*Ensino Médio em Tempo Integral*). In Goiás, in-person classes were suspended in March 2020, and are not expected to resume until May 2021 (and even then, only if new COVID-19 cases in the State are kept under control). During school closures, classes switched to online, delivered through a video conferencing and team collaboration platform. Students were assigned daily exercises that they had to hand in through the platform. For those without internet access, schools handed out assignments in plastic bags hung at the school gate, and students had to hand them back in the same way.

The intervention, powered by Movva, consisted of sending behavioral nudges twice a week over text messages (SMS) to high-school students or their primary caregivers. Nudges consisted of encouragement messages meant to have students engage in remote learning activities (online and offline) and to keep them motivated about staying enrolled in school by the time in-person classes return; examples are provided in the supplementary materials. The intervention spanned the universe of State schools offering the full-time high-school program. The Education Secretariat had access to valid phone numbers for 18,256 students, roughly 40% of the total. While we do not have data on students outside our sample, studies in other Brazilian States provide insight into the nature of selection: students whose phone numbers are known by the school tend to be from wealthier households and display higher grades.[27,34] We discuss the implications of selection to the generalizability of our findings in the Discussion section.

In total, 12,056 high-school students across 57 public schools received nudges, while other 6,200 high-school students across 30 public schools received no nudges or other text messages from their schools over that period. Randomization was undertaken at the school level. 42% of treated students received nudges directly on their phones, whereas 58% had nudges targeted at their primary caregivers' phones; this happened only when the Secretariat did not have access to students' phone numbers directly. The intervention started on June 9th, during the second school quarter, and continued through the end of the 2020 school year. No messages were sent during the winter break in July.

More students were assigned to treatment than control because we split treated students into additional treatment arms, varying nudges' content across them. We cross-randomized treated students to (1) a framing experiment, and (2) a social pressure experiment. In the framing experiment, half of treated students were assigned to messages framing the motivation to stay in school in terms of *gains* (the upside of high-school completion), and the other half, to messages framing the motivation to stay in school in terms of *losses* (the downside of school dropouts). In the social pressure experiment, half of treated students were assigned to messages stating that 80% of their fellow students wanted to return to in-person classes after school reopening (based on a representative SMS survey; see below), and half, to messages that just stated the importance of returning to in-person classes *without* reference to or data on peers' motivation to do so. These additional experiments took place only in the first month of the intervention, during the second school quarter (Q2). All treated students received exactly the same content throughout the third (Q3) and fourth school quarters (Q4). Randomization to these additional treatment arms was undertaken at the individual level. Although these additional experiments were not pre-registered (because the implementing partner only decided the content of the additional treatment arms after the rollout of the experiment), we present their results in a separate section because they provide useful insights for the design of behavioral nudges to prevent student dropouts during the pandemic.

Definition of Outcomes and Estimation

We evaluate the impacts of the intervention by monitoring dropout risk during school closures, based on administrative records shared by the Education Secretariat. While dropouts are typically defined by enrollment status, using this definition during the pandemic would be misleading: most Education Secretariats in Brazil have re-enrolled students automatically in 2021. As a leading example, São Paulo State recorded 0% dropouts this year despite an average dropout rate of roughly 10% among middle- and high-school students in a typical year.[22] Instead, we define high dropout risk at each school quarter as students with *no math or Portuguese grades* in that quarter. A student who has not even taken math and Portuguese tests, the two subjects used to assess general proficiency across school systems, can be confidently assigned a high dropout risk; in fact, Instituto Sonho Grande uses this criterion as its main predictor of student dropouts, even before the pandemic. There is a vast literature that uses related measures – absenteeism and assignment completion – to predict dropouts.[35,36,37,38] In supplementary materials, we validate this proxy using data for actual dropouts in 2019 from a different Brazilian State. As such, we define high dropout risk equal to 1 if a student had no math grade and no Portuguese grade assigned to them in the administrative data for that quarter, and 0 otherwise. The distribution of this outcome over time and across groups suggests it is in fact a sensible proxy for student dropouts: it is nearly 0% at the first quarter, while in-person classes were still in place, and it gradually increases over time, reaching 2.5% by the end of the school year – 2.5-fold the average dropout rate among full-time high-school students in a typical year, and consistent with estimates from national surveys in 2020 and with other studies in similar contexts.[22] Dropout risk is concentrated on boys, and at the 1st and 2nd years of high school, again consistent with the distribution of dropouts in a typical year.

We estimate average treatment effects on this outcome with Ordinary Least Squares regressions. Since we cannot verify whether students effectively received messages as intended, we only estimate intention-to-treat (ITT) effects based on treatment assignment. Taking advantage of the fact that the program was not introduced until the second school quarter, we also estimate a differences-in-differences model, contrasting differences between treated and control students, before and after the onset of nudges. This allows us to estimate dose-response treatment effects. We cluster standard errors at the classroom level because our definition of high dropout risk is based on grades, which are assigned by teachers. We also estimate heterogeneous treatment effects by students' Q1 Portuguese GPA, gender and grade, by whether students' or caregivers' phones were targeted by nudges, and by whether schools already offered internet-based academic activities before the pandemic (according to the 2019 Brazilian School Census). We do not estimate treatment effects on quarterly attendance or grades because data on these outcomes suffer from quality issues. In particular, the Secretariat shared no data on Q3 attendance or grades, and we estimate significant bunching on passing grades in Q4 – likely reflecting strategic behavior by teachers to prevent massive grade repetition in 2020.

The fact that teachers were aware of treatment assignment might have induced selective reporting across treated and control schools. To rule out that treatment effects are merely driven by reporting biases, we complement these analyses with administrative data on students' behavior and survey data on students' motivation to return to school once in-person classes return (see supplementary materials).

Since data on Portuguese and math grades had not been made available by the Secretariat at the time of randomization, we could not stratify treatment assignment by those variables. Ex-post, we detect some small but systematic differences across the treatment and control groups with respect to average dropout risk at baseline. Another issue is that we were unable to recover information for 236 students who were no longer listed by the Education Secretariat after randomization was conducted. Since it is impossible to determine whether those students were originally included by mistake, transferred to private schools right after, or abandoned school, we drop those observations from our dataset. Incidentally, that also leads to small but systematic baseline differences in the share of girls across the treatment and control groups. Our differences-in-differences strategy does not entirely overcome biases from such imbalances: different initial conditions are likely associated with different counterfactual trajectories over the course of the school year. Having said that, such differences are driven by outliers – schools for which the share of girls and/or Q1 dropout risk were 1.5 times the inter-quantile range below p25 or above p75 of the baseline distributions of those variables. Dropping all 22 outlier schools (corresponding to 4,105 students) leads to a balanced sample (13,915 students; 7,802 in the treatment group, and 6,113 in the control group), which we use throughout the paper. Supplementary materials document that, when treatment effects are estimated in the full sample, differences-in-differences estimates are very similar, albeit somewhat less precisely estimated.

Effects on Dropout Risk During School Closures

Table 1 displays average treatment effects on the indicator of high dropout risk. Column (1) contrasts the treatment and control groups at Q4, while columns (2-4) estimate a differences-in-differences model, contrasting dropout risk in the treatment and control groups, before and after the onset of the nudges. Column (2) considers only Q1 and Q4; column (3) considers all data made available by the Education Secretariat, and column (4) estimates dose-response treatment effects.

	High dropout risk			
	Q4	Q1 vs. Q4	Q1 vs. Q2-Q4	
	(1)	(2)	(3)	(4)
Nudges	-0.0061*			
	(0.0034)			
Nudges x After		-0.0063*	-0.0040	-0.0017
		(0.0035)	(0.0025)	(0.0022)
Nudges x Dose				-0.0023
				(0.0016)
After		0.0242***	0.0172***	0.0101***
		(0.0028)	(0.0020)	(0.0018)
Dose				0.0070***
				(0.0013)
Classroom fixed-effects	No	Yes	Yes	Yes
Control mean (After=1)	0.0242	0.0242	0.0172	0.0172
Observations	13,915	27,830	41,745	41,745
R-squared	0.0042	0.0453	0.0369	0.0392

Table 1: Treatment effects of behavioral nudges on dropout risk

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with high dropout risk = 1 if the student had no math and Portuguese grades in that quarter, and 0 otherwise. Nudges = 1 in schools where students were nudged, and 0 otherwise. After = 1 for Q2-Q4, and 0 otherwise. In column (4), Dose = 0 for Q1 and Q2, and = 2 for Q4. Column (1) only considers observations at Q4; column (2), only at Q1 and Q4; and columns (3-4), at all available quarters. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. All columns control for students' gender and grade; and whether s/he owns her/his own phone. Standard errors in parentheses clustered at the classroom level. In supplementary materials, Table D.1 shows that student characteristics are balanced across the treatment and control groups after dropping outliers; Figure D.1 displays outlier schools dropped; and Tables D.2-D.3

document that results are similar when estimated with the full sample or with standard errors clustered at the school level, respectively (although slightly less precisely estimated). * p<0.1, ** p<0.05, *** p<0.01.

Columns 1 and 2 document that nudges decreased dropout risk by roughly 26% relative to the control group, an estimate significant at the 10% level. Contrasting columns (1-2) and column (3) shows that nudges' effect size increased substantially over time. Column 4 confirms that pattern: while nudges decreased dropout risk by less than 10% of its Q2-Q4 average immediately after their onset, their effect size increased over 3-fold throughout the school year (although the dose-response coefficient is imprecisely estimated).

Figure 1 displays how dropout risk evolved quarterly, separately for treatment and control students. Both groups started off with the same dropout risk, but such risk increases much more slowly among treated students relative to the control group, especially after the winter break. Although there is already a small but not statistically significant difference at Q2, nudges decrease dropout risk by almost 26% through Q4 (p-value=0.069).

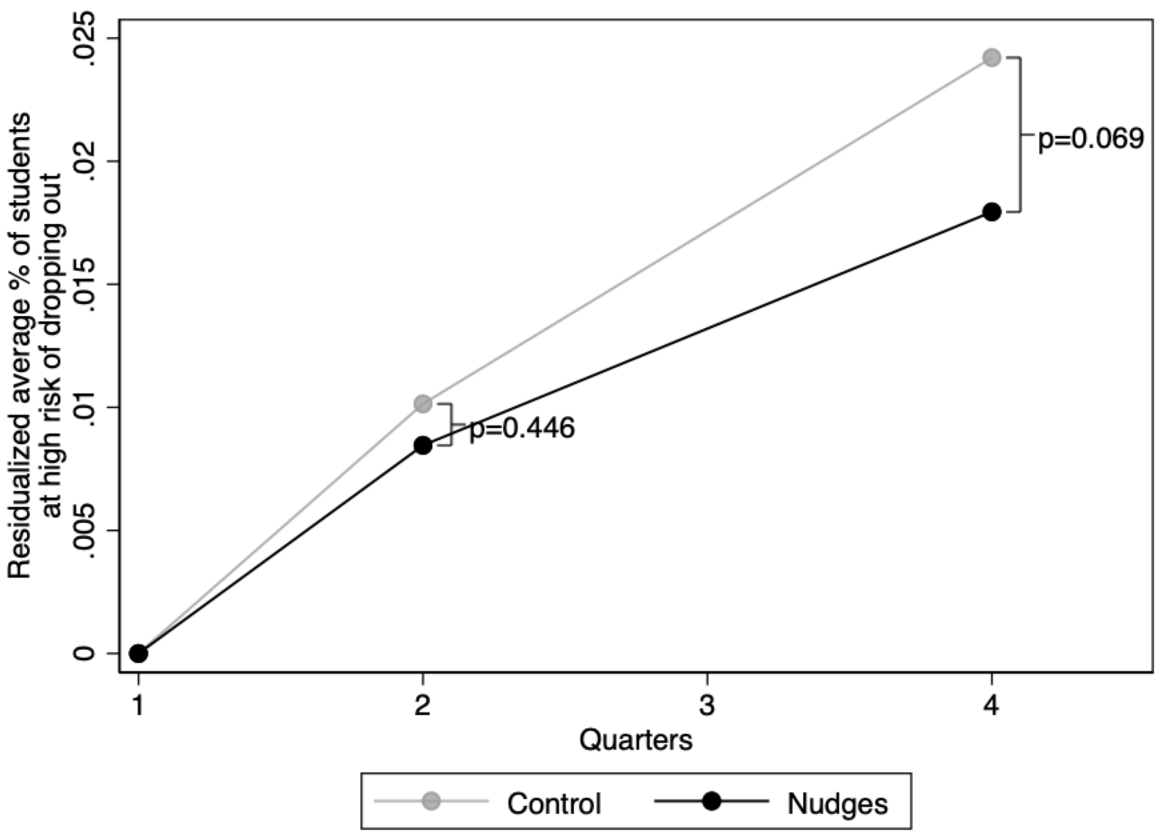

Figure 1: Quarterly incidence of high dropout risk across treatment and control students

Notes: Quarterly sample averages of dropout risk (i.e. % of students without math and Portuguese grades) for the treatment group (in black) and the control group (in light grey). Nudges = 1 in schools where students were nudged, and 0 otherwise. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. P-values computed with standard errors clustered at the classroom level. In supplementary materials, Table D.1 shows that student characteristics are balanced across the treatment and control groups after dropping outliers; Figure D.1 displays outlier schools dropped; and Figure E.1 documents the results using the full sample.

Targeting Insights: Heterogeneous Treatment Effects

We shed light on the nature of treatment effects by estimating heterogeneous impacts of the nudges. Tables C.1 to C.5 in supplementary materials estimate the differences-in-differences model separately for different sub-samples defined by students' and school characteristics, along with statistical tests of whether effect sizes of nudges vary systematically across them.

Table C.1 estimates treatment effects by students' Q1 Portuguese GPA. As expected, in the control group, dropout risk is concentrated on those below-median baseline achievement, about 2.3-fold its prevalence among those above the median. Consistently, treatment effects are also much larger among the former: the effect of nudges is concentrated on worst-performing students, for whom the interventions decreases dropout risk by about 1/3 (p-value of the difference = 0.06).

Next, Table C.2 estimates treatment effects by students' gender. In the control group, dropout risk is concentrated on boys, about 2.4-fold its prevalence among girls. Consistently, treatment effects are also much larger among the former (p-value of the difference = 0.12), for whom the intervention decreases dropout risk by 1.1 p.p. (about 30% of its prevalence in the control group, significant at the 10% level; column 2).

Table C.3 estimates treatment effects by grade. In the control group, dropout risk is concentrated on junior (1^{st} graders) and sophomore students (2^{nd} graders); for seniors (3^{rd} graders), it is extremely low, roughly 1/3 of its prevalence in the other grades. Also consistently, treatment effects are concentrated on the first two grades (p-value of the difference = 0.05). The largest effect size is on 1^{st} graders, for whom nudges decrease dropout risk by 1.3 p.p. (42.4% of its prevalence in that grade, significant at the 5% level; column 1).

Table C.4 then estimates treatment effects by whether nudges were sent to caregivers' cell phones or to students themselves. Even though targeting was not randomly assigned, we compare students for whom the Secretariat had access to their phone numbers (or only to their caregivers' phone numbers) across the treatment and control groups; importantly, students' phone ownership is balanced across treatment and control. In the control group, dropout risk is similar across the two sub-samples (2.10 p.p. and 2.89 p.p., respectively). We find that treatment effects are much larger and precisely estimated when nudges are sent directly to students (p-value of the difference = 0.03). For the latter, nudges decrease dropout risk by 1.3 p.p. (45.3% of its prevalence, significant at the 1% level; column 2). Incidentally, this result rules out that nudges work merely because teachers in treated schools know that students are being nudged. Within each treated school, there are always students who receive nudges directly and others who receive them via their caregivers, and teachers or school principals do not know what the individual assignment ultimately was.

Last, Table C.5 estimates treatment effects by whether the school offered students online academic activities before the pandemic or not, based on 2019 Brazilian School Census data. In the control group, dropout risk is similar across the two sub-samples (2.7 p.p. and 2.3 p.p., respectively). For schools without online activities prior to the pandemic, if anything, treatment effects are actually positive (and imprecisely estimated). In contrast, they are negative and precisely estimated for schools that already featured online instruction even before (p-value of the difference = 0.08); for the latter, nudges decrease dropout risk by 0.8 p.p. (36% of its prevalence, significant at the 5% level; column 2). While neither internet access nor educational strategies before the pandemic are randomly assigned, this result suggests that nudges worked by effectively engaging students in remote learning activities, which are more likely to have taken place effectively in schools with prior experience when it comes to online instruction.

Content Insights: Additional Experiments

Next, we present the results of the additional experiments that varied SMS content across treated students in the first month of the intervention. Figure 2 displays how dropout risk evolved quarterly, separately for treatment arms within each experiment, and for control students. Panel A focuses on the social pressure experiment. It showcases that students targeted by statistics on their peers' motivation to return to in-person classes did *not* experience a larger reduction in dropout risk relative to those who did not.

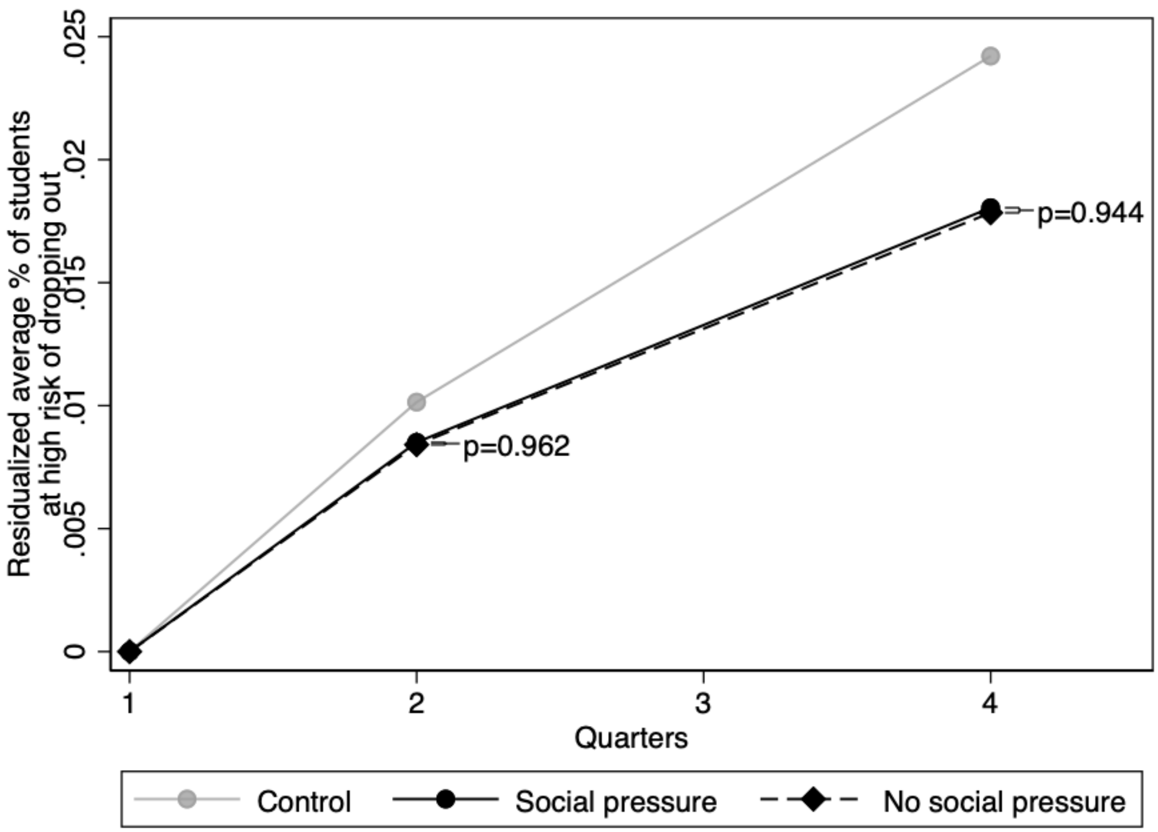

Figure 2 – Panel A: Quarterly incidence of high dropout risk across treatment arms in the social pressure' experiment and control students

Notes: Quarterly sample averages for high risk of dropout (i.e. % of students without math and Portuguese grades) for students assigned to messages emphasizing social pressure (in black; solid) or not (in black; dashed), and for the control group (in light grey). Social pressure = 1 if the student was assigned to content disclosing peers' motivation to return to in-person classes, and 0 otherwise. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. P-values computed with standard errors clustered at the classroom level. In supplementary materials, Table D.1 shows that student characteristics are balanced across the treatment and control groups after dropping outliers; Figure D.1 displays outlier schools dropped; and Table C.6 formally estimates average effects by treatment arm of the social pressure experiment, controlling for students' gender and grade, and whether s/he owns her/his own phone.

Panel B in Figure 2 turns to the framing experiment. It showcases that students targeted by messages emphasizing the upside of remaining in school experience a higher reduction in dropout risk relative to those who were targeted by messages emphasizing the downside of school dropouts (p-value of the difference = 0.26). Interestingly, even though content only varied across groups in Q2, differences compound over time and become much larger and precisely estimated in Q4.

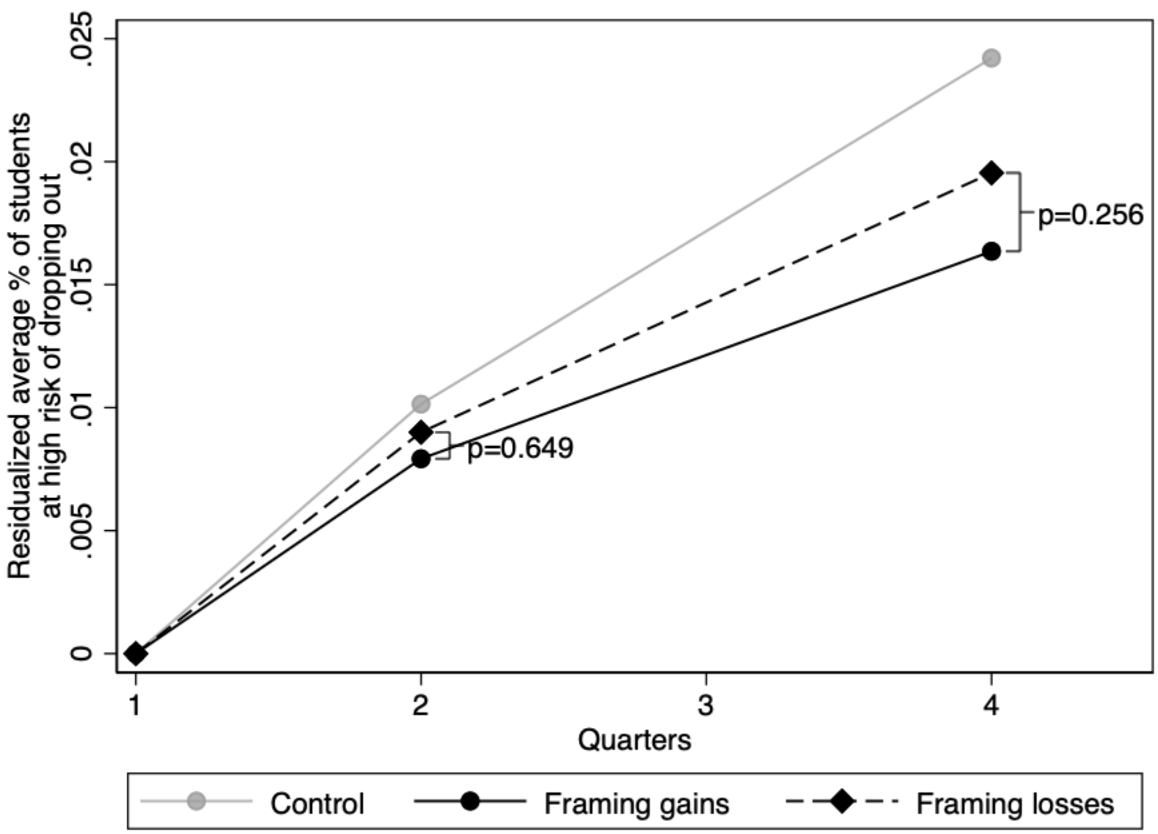

Figure 2 – Panel B: Quarterly incidence of high dropout risk across treatment arms in the framing experiment and control students

Notes: Quarterly sample averages for high risk of dropout (i.e. % of students without math and Portuguese grades) for students assigned to messages emphasizing social pressure (in black; solid) or not (in black; dashed), and for the control group (in light grey). Framing gains = 1 if the student was assigned to content disclosing peers’ motivation to return to in-person classes, and 0 otherwise. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. P-values computed with standard errors clustered at the classroom level. In supplementary materials, Figure D.1 displays outlier schools dropped; Table D.1 shows that student characteristics are slightly unbalanced across the treatment and control groups within the framing experiment, even after dropping outliers; and Table C.7 formally estimates average effects by treatment arm of the framing experiment, showing that results are very robust to controlling for student gender and grade, whether s/he owns her/his own phone, as well as interactions between these variables and an indicator variable = 1 after the onset of the nudges (for Q2-Q4), and 0 otherwise.

Incidentally, differences in treatment effects within the additional experiments confirm that the impacts of nudges are not merely driven by differential teacher behavior across treatment and control school, since content variations were assigned at the individual level and teachers were not aware of individual assignments within school.

Discussion

Taken together, this work suggests that motivating adolescents to stay in school, particularly by making the upside of graduating high-school more salient, might be critical to mitigate some of the most dramatic effects of school closures due to Covid-19 in developing countries.

Results are all the more striking not only because behavioral nudges sent through SMS are cheap and easily scalable, but also because these interventions have been largely overlooked by policy makers amidst efforts to keep curricular activities running in the absence of in-person classes. Cell phone penetration is very high worldwide,[39] and text messages do not require smartphones or internet access. Even in face of illiteracy challenges, there is recent evidence that nudges over text messages can work just as well as audio messages.[33] Our findings provide important lessons to address the global education crisis in the context of the pandemic: above and beyond focusing on curricular knowledge to address learning deficits, public school systems should reach out to families to provide support and encouragement during challenging times.

At the same time, our finding that treatment effects are concentrated on schools that already featured online academic activities prior to the pandemic showcases that merely motivating students is unlikely to mitigate dropout risk. Effective remote learning activities might be necessary – even if clearly not sufficient – to keep students engaged and motivated to return to in-person classes when the conditions allow.

Our experimental findings are based on the approximately 40% of students for whom the Education Secretariat had access to valid phone numbers, which leads to two important concerns. First, what keeps schools from establishing direct contact with the remaining 60% of public-school students in the State? For most of the students whom the Secretariat was unable to reach, the problem was not that they did not have phone numbers on record, but rather that it is common among the poor to frequently change phone numbers – among other reasons, to avoid insistent calls from debt collection companies in a country where 2 out of 3 adults have a negative credit score. Distributing SIM cards with earmarked connectivity, a policy adopted by less than 20% of Brazilian States in the 2020 school year,[40] might have the added benefit of establishing a reliable communication line between schools and low-income parents. Second, to what extent are our findings expected to generalize in case that communication barrier could be overcome? Our estimates of heterogeneous treatment effects suggest that, if anything, the impacts of the intervention should be *even larger* in that case; after all, dropout risk is particularly high among students from disadvantaged backgrounds, and the impacts of nudges are largely driven by students at the highest risk of dropping out.

Seminal work notes that neuro-biological changes during puberty redirect adolescents' attention and motivational salience,[41] with status-seeking behaviors, romantic interests and peer pressure often getting in the way of attending classes.[41] As a result, adolescents are the ones most likely to drop out,[15] and presumably even more so in the context of school closures. If insights from adolescent psychology suggest that this population is the one most at risk of dropping out amidst the pandemic, they also lay out opportunities to intervene. In fact, motivational interventions have been shown to successfully improve adolescents' choices, from healthy eating[42] to school effort,[43] with the potential to shift developmental trajectories. Our results showcase first-hand that, as with adult populations, and as in other decision domains[44,45,46,47] behavioral nudges to keep adolescents in school can be effective, by rendering school activities *top-of-mind* and changing behavior accordingly. This comes across in our findings most clearly in two ways. First, the impacts of the intervention are concentrated on students who receive messages directly on their phones, rather than indirectly through their primary caregivers. Second, treatment effects are sensitive to how messages are framed, even when their informational content is exactly the same.

Incidentally, the pattern that we document within the framing experiment goes in the opposite direction of loss-aversion – the tendency to react more to avoiding losses than to acquiring identical gains. Partly, this might be due to complex attention dynamics: the fact that differences between treatment arms keep increasing during the third and fourth school quarters even though we only varied content across them in the second school quarter suggests that framing effects influence future attention allocation. While loss aversion has been documented as a systematic phenomenon in the context of static decision-making,[48] in a dynamic setting, previous framings might influence how individuals consider all other future decisions, potentially reversing the intuition for expected effect sizes. Similarly, the small effect we document within the social pressure experiment is at odds with typical effect sizes in the literature,[49,50,51,52,53,54] what is perhaps unsurprising as peer pressure and social norms might be much less salient in the absence in-person classes. Last, while nudging primary caregivers has been shown to improve educational outcomes before the pandemic – even when it comes to adolescents[27] –, our results show that, at least in the context of school closures, it might be crucial to reach out to students directly.

All in all, our findings suggest that behavioral sciences should inform the design and targeting of interventions to mitigate the educational impacts of school closures in the pandemic,[32] but also raise caution against indiscriminately applying insights derived from evaluations of similar interventions in the context of in-person classes or from experimental settings that miss essential features of the decisions students actually face.

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METHODS

Ethics Approval. Approval for this study was obtained from the Institutional Review Board of the Department of Economics at the University of Zurich (2020-033). This experiment was conducted in the context of the full-time high school program "Ensino Médio em Tempo Integral" of the Goiás State Secretariat of Education. When it comes to informed consent, since participants are minors, broad consent was obtained from their legal guardians directly by the Education Secretariat (at the time of school enrollment), allowing researchers to use secondary information from administrative records without eliciting further consent. Our implementing partner Movva further obtained students' assent directly via text messages (SMS): participants were informed and reminded of the fact that they could opt-out from the intervention and SMS surveys at any point (by simply replying 'STOP' or 'CANCEL', free of charge), without consequence.

Participants. Participants consist of public school students enrolled in grades 10-12; typical age is 15-18 years old. All contacts were provided to Movva by the Goiás State Secretariat of Education. The total number of contacts in the database correspond to 18,256 students, 12,056 of which randomly assigned, across 57 schools, to receive SMS nudges between June 9th and December 31st, and 6,200 across 30 schools assigned not to receive nudges or any other SMS communication from their schools. Power calculations before the onset of the intervention pointed out this sample size was large enough to detect relevant minimum effects on the outcomes of interest.

Data collection. Before the start of the intervention, the contacts' database was shared with the authors to complete the randomization at the school level, stratified by gender, grade and phone ownership. Schools were randomly assigned to either a treatment or a control group, following the group sizes above and using the statistical software Stata. The database including a treatment assignment indicator was then returned to Movva such that SMS nudges could be sent accordingly to the treatment group, but not to the control group.

Data on online access to the platform and participation in offline school activities was shared by the Secretariat of Education with Movva, while data on motivation to return to regular classes once they resume was collected by Movva directly over SMS surveys, from rotating sub-samples of approximately 280 students in the treatment and control groups every week – from the week after the intervention started until 3 weeks after it ended. Weekly sub-samples were also randomly drawn from the subject pool. Data on quarterly grades and attendance was shared by the Secretariat of Education in March 2021. Balance tests using Wald tests of simple and composite linear hypotheses were conducted before and after dropping outliers to ensure that each treatment group is comparable with respect to students' and school characteristics. These tests and results are detailed in the supplementary material to this paper in Table D.1.

Outcome data was shared with the authors, and analyzed following a pre-analysis plan pre-registered as trial 5986 at the AEA RCT Registry (included as supplementary material to this paper). We did not pre-register that we would analyze treatment effects on the high dropout risk proxy rather than official enrolment status because we did not anticipate at the time that the State would automatically enroll all students in 2021. We also did not pre-register the additional experiments, varying content within treated students, that we report in a separate section. All analyses were conducted by the authors using the statistical software Stata.

Finally, collecting information on human participants over time is subject to attrition. Participants were free to leave the study at any time, which creates a risk of biasing the results if such attrition is correlated with treatment assignment. In this context, we tested whether the probability of students responding to the SMS surveys was affected by the treatment. The results, which are reported in the supplementary material to this paper in Tables D.2 and D.3, indicate that the probability of responding to SMS surveys is not systematically affected by the treatment.

Intervention. Movva, the start-up that powered the intervention evaluated in this study, specializes in promoting behavior change by sending frequent reminders and encouragement messages directly to users' cell phones. The concept of nudges – interventions that modify the choice architecture by changing the way decisions are framed to mitigate or amplify behavioral biases, inducing certain decisions while preserving subjects' freedom of choice – lies at the heart of the contributions of Nobel Memorial Prize in Economic Sciences winner Richard Thaler and co-authors. Nudges have been shown to effectively change behaviors across various contexts, from education to preventive health care to savings.[44] Eduq+, the intervention evaluated in this study, has been shown to improve educational outcomes in an environment of regular classes across different settings.[27,33] In the context of this study, two nudges per week were sent over text messages (SMS) to high-school students or their primary caregivers, depending on phone ownership, in the treatment group. Nudges were organized in 2-week sequences of 4 messages, as follows (translated from Portuguese):

Example 1: Common sequence and framing experiment during the second school quarter

	Week 1		Week 2	
Treatment	Fact	Activity	Interactivity	Growth
Common sequence	It is normal to be afraid in times of uncertainty. Use this scenario to your advantage: take the opportunity to develop the ability to focus on your plans for the future.	How about summarizing your life project? Highlight which dreams you would regret NOT realizing. Plan step by step how to get there.	Tell us! From 0 to 10, what is your level of confidence that completing high school will help with your plans for the future? SMS free of charge.	One step at a time! That's how we build our story. Be the protagonist of yours and focus on your studies to finish the school year.
Framing losses	Connect! 80% of your colleagues believe in high school to help them do well in the future. To get there, you need to organize your study time!	Time to study and watch movies! Make a schedule of the day, setting time to wake up, study, do activities and, of course, catch up on the latest episodes.	A day used to study gets you closer to your diploma. How has your time management been? 1. Good 2. Regular 3. Bad. SMS free of charge.	Social media can make the time fly. Set intervals during the day to look at your phone. Be careful not to compromise your studies with distractions.
Framing gains	Connect! 80% of your colleagues believe in high school to help them do well in the future. To get there, use the holidays to define a new routine.	Time to rest and organize! Set a routine for your vacations, defining a time to wake up and to do what you like. How about learning something new?	Having a routine is good for your mind! And to learn something new too ;) What are you doing on your vacations? SMS free of charge.	Enjoy the holidays and, between your leisure activities, invest in skills that will help you CONQUER the dreams you want for the future!

Example 2: Common sequence and social pressure experiment during the second school quarter

	Week 1		Week 2	
Treatment	Fact	Activity	Interactivity	Growth
Common sequence	What is the root of your feelings? To laugh or cry comes from within. To recognize what we feel helps to face life in difficult times.	Let's understand what makes you happy? Take a paper and write down EVERYTHING that makes you feel good: people, places, dreams ... This will be your happiness map!	What makes you happy, makes you stronger! Do you recognize what helps you to deal with the distance from the school? Yes or No? SMS free of charge.	That's it! Include in your routine what makes you happy! This will help you be resilient in this period. Be strong!; (Message sent to the participant's who did NOT answer the Interactivity message: Don't give up! Pick something from your map of happiness and put it in your routine. This will help ou to be resilient in this period.)
Social pressure	Did you know that you learn more when you explain what you have studied? This study technique is more efficient than to read the same content several times.	Google it! Search for study techniques, choose two and apply them this week! After, talk to your friends about your experience! ;)	Who has an open mind always re-LEARN TO LEARN. From 1 to 5, how is your motivation to apply new study techniques in your routine? SMS free of charge.	80% of your peers are learning new study techniques. Did you learn a new technique?
No social pressure	Did you know that you learn more when you explain what you have studied? This study technique is more efficient than to read the same content several times.	Google it! Search for study techniques, choose two and apply them this week! After, talk to your friends about your experience! ;)	Who has an open mind always re-LEARN TO LEARN. From 1 to 5, how is your motivation to apply new study techniques in your routine? SMS free of charge.	Be one step ahead using STUDY TECHNIQUES in your routine and develop learning skills that the university and the market value.

Example 3: Common sequence during the fourth school quarter

	Week 1		Week 2	
Treatment	Fact	Activity	Interactivity	Growth
Common sequence	The only one who doesn't err is the one who doesn't try! Failing when trying to learn is part of the process and prepares the brain to new learnings. Face your mistakes with curiosity!	When was the last time you did something for the first time? Hands on and learn something new: an art, a game, a recipe! #BeCurious :)	Learning means getting out of the comfort zone! Tell us about something you achieved after a lot of perseverance. It can be any activity! SMS free of charge.	Be aware! Who is open to make mistakes, is open to learn more and to stand out. Challenge yourself, make mistakes, make mistakes again... and learn ;)

Measures. *Student's prolonged absenteeism right before the winter break*. The Secretariat of Education shared administrative records with Movva, which then shared it with the authors. This dataset contained information on access to the online platform and participation in offline school activities at the student level, an indicator variable equal to 0 if the student logged in to the platform or participated in offline school activities on a given day and 1 in case s/he did not. Based on this information, we created a measure of prolonged absenteeism which was equal to 1 if a student had no attendance on record for two weeks in a row right before the winter break (between June 15th and June 26th), and 0 otherwise. To ensure that the results presented in the paper are robust to alternative definitions of dropouts, we constructed analogous measures of prolonged absenteeism as no attendance on record for the last week before holidays or for the last three weeks before holidays. Results, which are very robust to using alternative definitions of dropouts, are reported as supplementary material to this paper, in Table E.1.

Student's motivation to return to school once they reopen. Each week, students assigned to be surveyed by text message reported their motivation to return to school once regular classes resume by answering the following question: "Do you plan on returning to school once regular classes resume?". Movva coded lack of motivation to return as a binary indicator based on SMS replies, equal to 1 if the reply was “No” or similar, and zero otherwise, and shared that information with the authors.

Student dropout risk. We define high dropout risk equal to 1 if a student had no math and no Portuguese grades on record in that school quarter, and 0 otherwise.

Analysis method. All results presented in the paper use intention-to-treat analyses by linking student identification numbers to the treatment condition they were assigned to before the start of the intervention. The reason for restricting our analyses to intention-to-treat analyses is that we had no means of verifying whether students effectively received messages as intended. Throughout the paper, we report intention-to-treat effects obtained from Ordinary Least Squares (OLS) regressions, by regressing each outcome on a binary indicator equal to 1 if the student was assigned to treatment and 0 otherwise. In Panel B of Figure B.2, effect sizes are standardized by dividing the treatment effect coefficients by the average in the control group for this outcome, providing effect sizes in % terms. Last, since for one of the weeks of our SMS surveys, student characteristics were not balanced across respondents in the treatment and control groups even after dropping outlierss, we control for gender, grade in which the student is enrolled and phone ownership in this figure. We also control for those characteristics in analyses of student's dropout risk. All p-values are obtained from two-tailed tests of equality of coefficients between the treatment and control groups, with standard errors clustered at the classroom level in each case.

Data Availability

The data that support the findings of this study are available at https://osf.io/3sqfr/ or upon reasonable request to the authors.

Code Availability

Syntax can be found at https://osf.io/3sqfr/ or upon reasonable request to the authors.

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Author contributions

G.L. conceived the study and led the design, analysis and writing; J.C. was involved in every phase of the study, particularly the conception of the study, the study design, the interpretation of analyses and the writing of the manuscript.

Author information

G.L. is a co-founder and chairman at Movva, the implementing partner of the intervention evaluated in this study.

Supplementary Information is available for this paper.

Correspondence and requests for materials should be addressed to G.L. or J.C.

Supplementary Materials

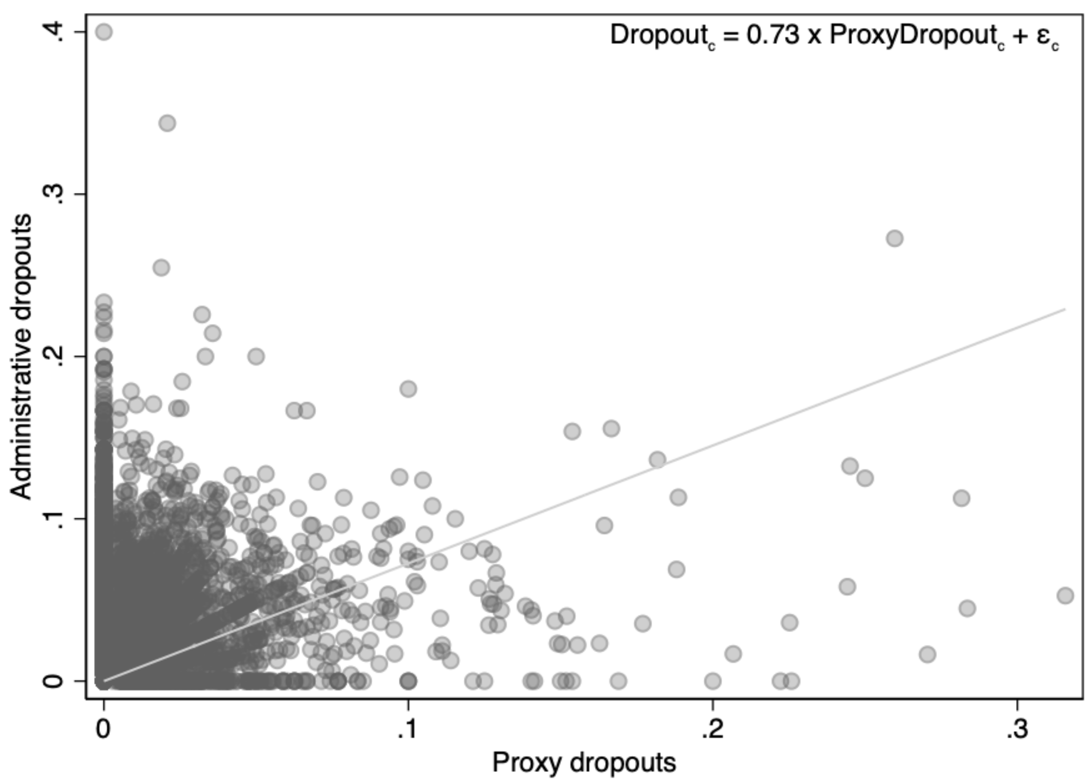
A. Validation of our proxy of student dropouts

This Appendix compiles evidence to validate our proxy for student dropouts (*high dropout risk*, equal to 1 if a student had *no math or Portuguese grades* assigned to them in the administrative data for that quarter, and 0 otherwise).

To do so, we use administrative data from the State of São Paulo Education Secretariat, which includes information on both math and Portuguese grades and actual dropouts for public high-school students in 2019. Concretely, administrative dropouts equal to 1 if a student was enrolled in a State school in 2019 but not in 2020, and 0 otherwise. We restrict attention to junior and sophomore students, as we cannot compute administrative dropouts for seniors.

Figure A.1 plots the prevalence of administrative and proxy dropouts at the classroom level, for the universe of 1st and 2nd high-school grades of São Paulo State. Even though administrative dropouts are measured with error – as students might not re-enroll for alternative reasons, from moving to a different State to switching over to a private school –, the figure showcases that the classroom-level actual and proxy dropouts are highly correlated, with a coefficient of 0.73. While 3.3% of junior and sophomore students dropped out of SP public schools in 2019, that figure was over 6-fold among those with missing math and Portuguese grades by the end of the school year.

Figure A.1: Scatter plot of proxy dropouts and administrative dropouts



Notes: The data used in this figure is from the school year 2019 in the State of São Paulo, Brazil. Administrative dropouts equal to 1 if a student was enrolled in school in 2019 but not in 2020, and 0 otherwise. Proxy dropouts equal to 1 if a student had *no math or Portuguese grades* assigned to them in the administrative data for that quarter, and 0 otherwise.

B. Short-Run Effects on Students' Behavior and Motivation

We complement the analyses by estimating treatment effects on students' behavior and motivation shortly after the onset of the intervention. These additional analyses help us rule out that the impacts of nudges could be merely driven by reporting biases, e.g. if teachers in treated schools, being aware of the intervention, inputted grades for students who do not take math and Portuguese tests to a greater extent than in control schools, out of social image concerns.

We rely on administrative data on students' daily attendance in the two weeks before the winter break – shortly after nudges were introduced –, and on survey data on students' motivation to return to school once they reopen, based on self-reports. We elicited the latter weekly over SMS, from rotating sub-samples of students in the treatment and control groups, from the week after the intervention started until 3 weeks into the winter break. These surveys targeted random sub-samples of around 15% of our sample each week, with an average response rate of 13% across weeks. Characteristics of respondents are balanced across the treatment and control groups, and we account for selective non-response in any particular week by appropriately bounding our estimates of treatment effects.

Figure B.1 estimates average treatment effects of nudges on an indicator variable equal to 1 if the student attended *no classes* in the two weeks before the winter break, and 0 otherwise, based on administrative data. Despite quality issues for quarterly attendance data, we were able to obtain daily administrative data on attendance for each student in our sample during June 2020. Such data was in fact made available for the vast majority of students (see the Supplementary Materials). Prolonged absenteeism is a well-known predictor of student dropouts. The figure showcases that while 7.21% of students in the control group had not followed remote learning activities right before the winter break, that figure was only 0.33% in the treatment group – an over 95% reduction (p-value = 0.00).

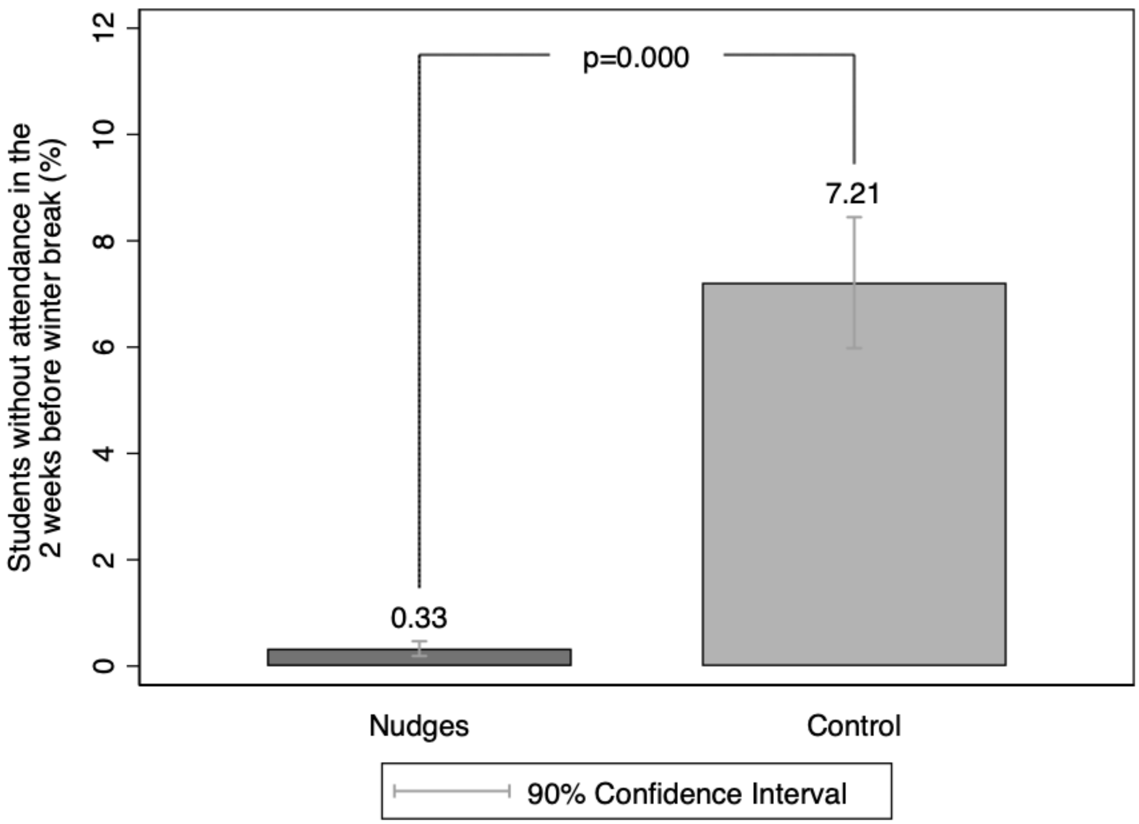


Figure B.1: Treatment effects of SMS nudges on prolonged absenteeism right before winter break

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with dependent variable = 1 if a student had no attendance on record over the last two weeks before the winter break, and 0 otherwise. Nudges = 1 in schools where students were nudged, and 0 otherwise. We dropped 22 schools that are outliers with respect to the baseline distributions of dropout risk and gender. 90% confidence intervals in light grey brackets; p-value in dark grey brackets from an OLS regression with standard errors clustered at the classroom level. In supplementary materials, Figure D.1 displays outlier schools dropped; Table D.1 shows that student characteristics are balanced across the treatment and control groups; and Table E.1 documents that results are robust to defining prolonged absenteeism as no attendance on record over the last week, or over the last three weeks before the winter break.

Figure B.2 estimates average treatment effects of nudges on an indicator variable equal to 1 if the student states that s/he does not want to go back to school once in-person classes return, and 0 otherwise, using self-reported data. Panel A displays weekly averages for the treatment and control groups, and Panel B estimates week-by-week treatment effects. Panel A documents a striking pattern for lack of motivation to return to in-person classes in the control group, which increased more than 2-fold in little over a month (starting from 15% by the 2nd week of June and reaching 39% by the 3rd week of July). Panel A also shows that lack of motivation to return to in-person classes not only started from a lower level in the treatment group already by week 2, but also increased at slower rates while the intervention lasted. Panel B confirms those patterns: nudges decreased lack of motivation to return to in-person classes by over 30% already by week 2, and effect sizes persisted even after nudges last (significant at the 5% level by week 4, and significant at the 10% level in week 5).

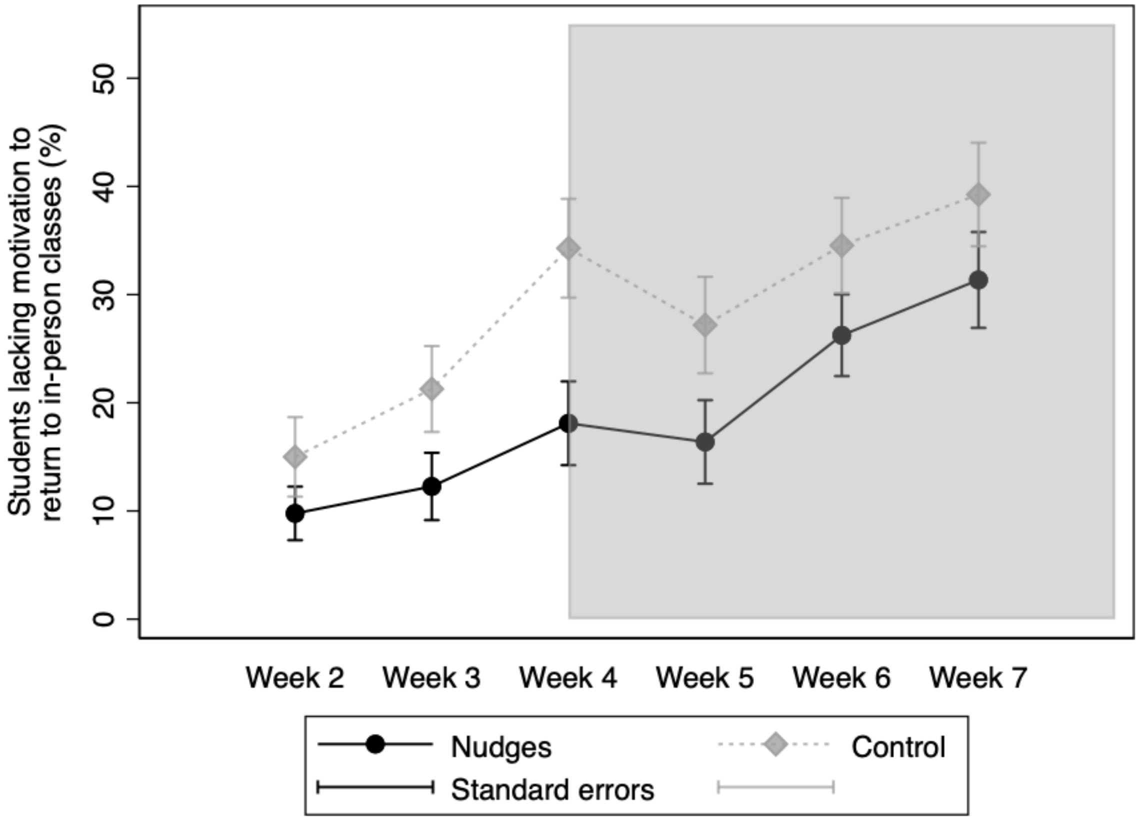


Figure B.2 – Panel A: Students' lack of motivation to return to school once they reopen (based on self-reported data) for the treatment and control groups, week by week

Notes: Weekly sample averages for lack of motivation to return to in-person classes (= 1 if the student states that s/he does not think s/he will be back in school when in-person classes resume, and 0 otherwise) for the treatment group (in black) and for the control group (in light grey). We dropped 22 schools that are considered outliers according to dropouts and gender in Q1. Self-reports based on weekly SMS surveys from rotating sub-samples of students in the treatment and control groups, from the week after the intervention started until 3 weeks after it ended. Standard errors clustered at the classroom level. The shaded area corresponds to the weeks during the winter break, when no nudges were sent.

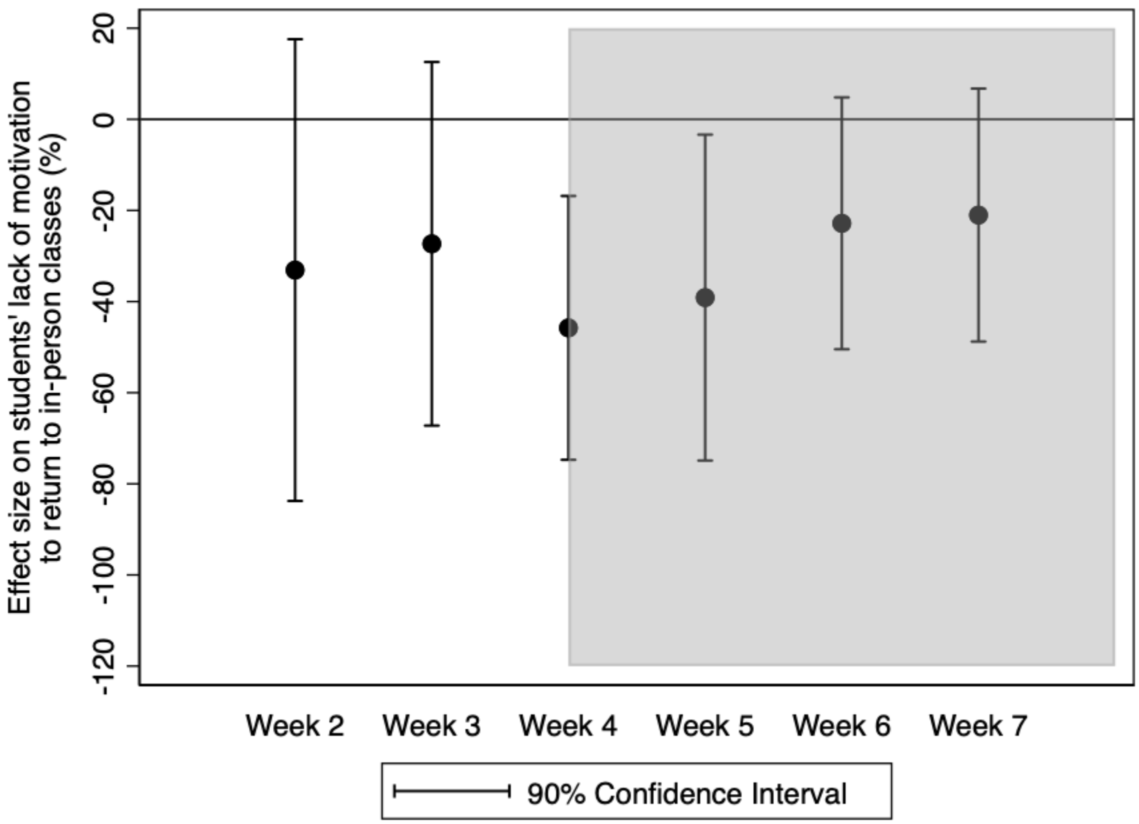


Figure B.2 – Panel B: Treatment effects of SMS nudges on students' lack of motivation to return to school once they reopen (based on self-reported data), week by week

Notes: ITT estimates from Ordinary Least Squares (OLS) regressions week by week with lack of motivation to return to in-person classes = 1 if the student states that s/he does not think s/he will be back in school when in-person classes resume, and 0 otherwise. We dropped 22 schools that are considered outliers according to dropouts and gender in Q1. Self-reports based on weekly SMS surveys from rotating sub-samples of students in the treatment and control groups, from the week after the intervention started until 3 weeks after it ended. Standard errors clustered at the classroom level. The shaded area corresponds to the weeks during the winter break, when no nudges were sent. In supplementary materials, Table D.1 shows that student characteristics are balanced across the treatment and control groups on average, and for individual weeks except week 3; for this reason, we control for student characteristics in all regressions week by week; Table E.2 documents that results are very similar when estimated with the full sample; and Table D.3 shows that the probability of responding to SMS surveys is not systematically affected by the treatment at any week.

After nudges temporarily stopped, during the winter break, treatment effects gradually faded out. Panel A in Figure B.2 shows that lack of motivation to return to in-person classes increased nearly by the same rate across the treatment and control groups during the winter break.

Panel B documents that effect sizes gradually declined, from 40% to 20% only two weeks later, and no longer statistically different from zero.

All in all, these patterns confirm that students react to nudges, both when it comes to their motivation to return to in-person classes and when it comes to attendance in remote learning activities right before the winter break, ruling out that treatment effects are merely driven by differential reporting by teachers in treated schools.

C. Heterogeneous Treatment Effects

Table C.1: Heterogeneous treatment effects of SMS nudges on high dropout risk, by Q1 Portuguese GPA

	High dropout risk Q1 vs. Q4	
	(1)	(2)
	Below-median	Above-median
Nudges x After	-0.0113**	-0.0001
	(0.0054)	(0.0036)
After	0.0328***	0.0140***
	(0.0044)	(0.0027)
Classroom fixed-effects	Yes	Yes
Control mean (After=1)	0.0328	0.0140
p-value([Below-median] = [Above-median])	0.0576	
Observations	14,166	13,648
R-squared	0.0830	0.0416

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with high dropout risk = 1 if the student had no math and Portuguese grades in that quarter, and 0 otherwise. Nudges = 1 in schools where students were nudged, and 0 otherwise. After = 1 for Q4, and 0 otherwise. Columns (1-2) consider observations at Q1 and Q4. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. All columns control for students' gender and grade; and whether s/he owns her/his own phone. Standard errors in parentheses clustered at the classroom level. In supplementary materials, Table D.1 shows that student characteristics are balanced across the treatment and control groups after dropping outliers; and Figure D.1 displays outlier schools dropped. * p<0.1, ** p<0.05, *** p<0.01.

Table C.2: Heterogeneous treatment effects of SMS nudges on high dropout risk, by gender

	High dropout risk Q1 vs. Q4	
	(1)	(2)
	Girls (54%)	Boys (46%)
Nudges x After	-0.0020	-0.0105*
	(0.0031)	(0.0056)
After	0.0147***	0.0347***
	(0.0021)	(0.0044)
Classroom fixed-effects	Yes	Yes
Control mean (After=1)	0.0147	0.0347
p-value([Boys] = [Girls])	0.1163	
Observations	14,902	12,928
R-squared	0.0463	0.0760

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with high dropout risk = 1 if the student had no math and Portuguese grades in that quarter, and 0 otherwise. Nudges = 1 in schools where students were nudged, and 0 otherwise. After = 1 for Q4, and 0 otherwise. Columns (1-2) consider observations at Q1 and Q4. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. All columns control for the grade of the student; and whether s/he owns her/his own phone. Standard errors in parentheses clustered at the classroom level. In supplementary materials, Table D.1 shows that student characteristics are balanced across the treatment and control groups after dropping outliers; and Figure D.1 displays outlier schools dropped. * p<0.1, ** p<0.05, *** p<0.01.

Table C.3: Heterogeneous treatment effects of SMS nudges on high dropout risk, by grade

	High dropout risk Q1 vs. Q4		
	(1)	(2)	(3)
	1st graders (42%)	2nd graders (32%)	3rd graders (26%)
Nudges x After	-0.0133**	-0.0061	0.0049
	(0.0060)	(0.0061)	(0.0048)
After	0.0314***	0.0265***	0.0096***
	(0.0049)	(0.0050)	(0.0033)
Classroom fixed-effects	Yes	Yes	Yes
Control mean (After=1)	0.0314	0.0265	0.0096
p-value ([1st graders] = [2nd graders] = [3rd graders])		0.0477	
Observations	11,652	9,070	7,108
R-squared	0.0496	0.0431	0.0394

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with high dropout risk = 1 if the student had no math and Portuguese grades in that quarter, and 0 otherwise. Nudges = 1 in schools where students were nudged, and 0 otherwise. After = 1 for Q4, and 0 otherwise. Columns (1-3) consider observations at Q1 and Q4. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. All columns control for the gender of the student; and whether s/he owns her/his own phone. Standard errors in parentheses clustered at the classroom level. In supplementary materials, Table D.1 shows that student characteristics are balanced across the treatment and control groups after dropping outliers; and Figure D.1 displays outlier schools dropped. * p<0.1, ** p<0.05, *** p<0.01.

Table C.4: Heterogeneous treatment effects of SMS nudges on high dropout risk, by targeting

	High dropout risk Q1 vs. Q4	
	(1)	(2)
	Caregiver's phone (58%)	Student's phone (42%)
Nudges x After	-0.0015	-0.0131***
	(0.0041)	(0.0048)
After	0.0210***	0.0289***
	(0.0030)	(0.0041)
Classroom fixed-effects	Yes	Yes
Control mean (After=1)	0.0210	0.0289
p-value([Caregiver]=[Student])	0.0281	
Observations	16,276	11,554
R-squared	0.0580	0.0669

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with high dropout risk = 1 if the student had no math and Portuguese grades in that quarter, and 0 otherwise. Nudges = 1 in schools where students were nudged, and 0 otherwise. After = 1 for Q4, and 0 otherwise. Columns (1-2) consider observations at Q1 and Q4. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. All columns control for students' gender and grade. Standard errors in parentheses clustered at the classroom level. In supplementary materials, Table D.1 shows that student characteristics are balanced across the treatment and control groups after dropping outliers; and Figure D.1 displays outlier schools dropped. * p<0.1, ** p<0.05, *** p<0.01.

Table C.5: Heterogeneous treatment effects of SMS nudges on high dropout risk, by offering of online activities before the pandemic

	High dropout risk Q1 vs. Q4 (1) No prior online activities (25%)	 (2) Prior online activities (75%)
Nudges x After	0.0081	-0.0080**
	(0.0086)	(0.0037)
After	0.0274***	0.0225***
	(0.0056)	(0.0031)
Classroom fixed-effects	Yes	Yes
Control mean (After=1)	0.0274	0.0225
p-value([Without]=[Internet])	0.0814	
Observations	6,894	20,936
R-squared	0.0512	0.0422

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with high dropout risk = 1 if the student had no math and Portuguese grades in that quarter, and 0 otherwise. Nudges = 1 in schools where students were nudged, and 0 otherwise. After = 1 for Q4, and 0 otherwise. Columns (1-2) consider observations at Q1 and Q4. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. All columns control for students' gender and grade; and whether s/he owns her/his own phone. Standard errors in parentheses clustered at the classroom level. In supplementary materials, Table D.1 shows that student characteristics are balanced across the treatment and control groups after dropping outliers; and Figure D.1 displays outlier schools dropped. * p<0.1, ** p<0.05, *** p<0.01.

Table C.6: Heterogeneous treatment effects of SMS nudges on student dropout risk across treatment arms in the social pressure' experiment and control students

	High dropout risk			
	Q4	Q1 vs. Q4	Q1 vs. Q2-Q4	
	(1)	(2)	(3)	(4)
Social pressure	-0.0060	-0.0004	-0.0004	-0.0004
	(0.0037)	(0.0003)	(0.0003)	(0.0003)
No social pressure	-0.0061*			
	(0.0037)			
Social pressure x After		-0.0062	-0.0039	-0.0016
		(0.0038)	(0.0027)	(0.0024)
Social pressure x Dose				-0.0023
				(0.0017)
No social pressure x After		-0.0064*	-0.0040	-0.0017
		(0.0038)	(0.0026)	(0.0024)
No social pressure x Dose				-0.0023
				(0.0018)
After		0.0242***	0.0172***	0.0101***
		(0.0028)	(0.0020)	(0.0018)
Dose				0.0070***
				(0.0013)
Classroom fixed-effects	No	Yes	Yes	Yes
Control mean (After=1)	0.0242	0.0242	0.0172	0.0172
p-value([Social pressure]=[No social pressure])	0.9748			
p-value([Social pressure x After]=[No social pressure x After])		0.9448	0.9404	0.9623
p-value([Social pressure x Dose]=[No social pressure x Dose])				0.9720
Observations	13,915	27,830	41,745	41,745
R-squared	0.0042	0.0453	0.0369	0.0392

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with high risk of dropouts = 1 if the student had missing values in math and Portuguese grades, and 0 otherwise. Social pressure = 1 for students who were assigned to messages stating that the majority of their fellow students wanted to return to in-person classes after school reopening, and 0 otherwise. No social pressure = 1 for students who were assigned to messages only stating the importance of returning to in-person classes without reference to or data on peers' motivation to do so, and 0 otherwise. After = 1 for Q2-Q4, and 0 otherwise. Dose = 0 for Q1 and Q2, and = 2 for Q4. In column (1), we drop Q1 and Q2 observations from the sample. In column (3), we drop Q2 observations from the sample. In column (4), we add an interaction between social pressure and dose, and between no social pressure and dose. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. We control for individual characteristics in all columns. Those controls are the gender and grade of the student; and whether s/he owns her/his own phone. Standard errors in parentheses clustered at the classroom level. * p<0.1, ** p<0.05, *** p<0.01.

Table C.7: Heterogeneous treatment effects of SMS nudges on student dropout risk across treatment arms in the framing experiment and control students

	High dropout risk			
	Q4	Q1 vs. Q4	Q1 vs. Q2-Q4	
	(1)	(2)	(3)	(4)
Framing gains	-0.0077**	-0.0001	-0.0002	-0.0002
	(0.0036)	(0.0004)	(0.0003)	(0.0003)
Framing losses	-0.0043			
	(0.0037)			
Framing gains x After		-0.0077**	-0.0050*	-0.0022
		(0.0036)	(0.0026)	(0.0025)
Framing gains x Dose				-0.0028
				(0.0017)
Framing losses x After		-0.0043	-0.0027	-0.0009
		(0.0038)	(0.0027)	(0.0025)
Framing losses x Dose				-0.0018
				(0.0017)
After		0.0258***	0.0190***	0.0120***
		(0.0047)	(0.0033)	(0.0033)
Dose				0.0070***
				(0.0013)
Classroom fixed-effects	No	Yes	Yes	Yes
Control mean (After=1)	0.0242	0.0242	0.0172	0.0172
p-value([Framing gains]=[Framing losses])	0.2268			
p-value([Framing gains x After]=[Framing losses x After])		0.2307	0.2938	0.6032
p-value([Framing gains x Dose]=[Framing losses x Dose])				0.4629
Observations	13,915	27,830	41,745	41,745
R-squared	0.0043	0.0472	0.0379	0.0402

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with high risk of dropouts = 1 if the student had missing values in math and Portuguese grades, and 0 otherwise. Framing gains = 1 for students who were assigned to messages framing the motivation to stay in school in terms of gains (the returns of high school completion), and 0 otherwise. Framing losses = 1 for students who were assigned to messages framing the motivation to stay in school in terms of losses (the costs of school dropouts), and 0 otherwise. After = 1 for Q2-Q4, and 0 otherwise. Dose = 0 for Q1 and Q2, and = 2 for Q4. In column (1), we drop Q1 and Q2 observations from the sample. In column (3), we drop Q2 observations from the sample. In column (4), we add an interaction between framing gains and dose, and between framing losses and dose. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. We control for individual characteristics in all columns, as well as for interactions between these individual characteristics and After. Those controls are the gender and grade of the student; and whether s/he owns her/his own phone. Standard errors in parentheses clustered at the classroom level. * p<0.1, ** p<0.05, *** p<0.01.

D. Balance and Selective Attrition Tests

Table D.1 – Balance tests

Panel A: Balance tests for individual characteristics (before and after dropping outliers)

	Sub-sample means in Q1		ANOVA test	# of obs.
	Control	Treatment	(p-value)	
Panel A.1: Before dropping outliers				
High risk of dropouts	0.000	0.006	**0.000**	18,020
Girl	0.524	0.540	**0.059**	18,020
Grade	1.841	1.827	0.823	18,020
Student owns phone	0.408	0.422	0.356	18,020
F-test			**0.000**	18,020
Panel A.2: After dropping outliers				
High risk of dropouts	0.000	0.000	1.000	13,915
Girl	0.524	0.544	**0.027**	13,915
Grade	1.844	1.831	0.855	13,915
Student owns phone	0.407	0.421	0.386	13,915
F-test			0.131	13,915

Notes: Ordinary Least Squares (OLS) regressions of student characteristics on treatment assignment at the time of randomization (Panel A.1), and after dropping outliers (Panel A.2). Results indicate that individual characteristics were unbalanced across the treatment and control groups before dropping outliers. We therefore drop 22 schools (corresponding to 4,105 students) to correct these imbalances. Those schools are considered outliers according to dropouts and gender in Q1. P-values from a test of equality of means of the treatment and control groups for each covariate controlling for individual characteristics (jointly for the F-test), with standard errors clustered at the classroom level. Those controls are the gender and the grade of the student; and whether s/he owns her/his own phone.

Panel B: Balance tests for individual characteristics across treatment arms in the social pressure' experiment and control students (before and after dropping outliers)

	Sub-sample means in Q1			ANOVA test	# of obs.
	Control	Social pressure	No social pressure	(p-value)	
Panel B.1: Before dropping outliers					
High risk of dropouts	0.000	0.007	0.005	**0.000**	18,020
Girl	0.524	0.539	0.540	0.166	18,020
Grade	1.841	1.830	1.824	0.870	18,020
Student owns phone	0.408	0.425	0.419	0.491	18,020
F-test				**0.000**	18,020
Panel B.2: After dropping outliers					
High risk of dropouts	0.000	0.000	0.000	1.000	13,915
Girl	0.524	0.541	0.548	**0.072**	13,915
Grade	1.844	1.831	1.831	0.984	13,915
Student owns phone	0.407	0.425	0.418	0.542	13,915
F-test				0.136	13,915

Notes: Ordinary Least Squares (OLS) regressions of student characteristics on treatment assignment at the time of randomization (Panel B.1), and after sample adjustment (Panel B.2). Results indicate that individual characteristics were unbalanced across the treatment and control groups before dropping outliers. We therefore drop 22 schools (corresponding to 4,105 students) to correct these imbalances. Those schools are considered outliers according to dropouts and gender in Q1. P-values from a test of equality of means of the treatment and control groups for each covariate controlling for individual characteristics (jointly for the F-test), with standard errors clustered at the classroom level. Those controls are the gender and the grade of the student; and whether s/he owns her/his own phone.

Panel C: Balance tests for individual characteristics across treatment arms in the framing experiment and control students (before and after dropping outliers)

	Sub-sample means in Q1			ANOVA test	# of obs.
	Control	Framing gains	Framing losses	(p-value)	
Panel C.1: Before dropping outliers					
High risk of dropouts	0.000	0.006	0.005	**0.000**	18,020
Girl	0.524	0.536	0.543	0.128	18,020
Grade	1.841	1.820	1.834	0.632	18,020
Student owns phone	0.408	0.417	0.427	0.362	18,020
F-test				**0.000**	18,020
Panel C.2: After dropping outliers					
High risk of dropouts	0.000	0.000	0.000	1.000	13,915
Girl	0.524	0.540	0.549	**0.068**	13,915
Grade	1.844	1.822	1.841	0.526	13,915
Student owns phone	0.407	0.418	0.425	0.565	13,915
F-test				**0.094**	13,915

Notes: Ordinary Least Squares (OLS) regressions of student characteristics on treatment assignment at the time of randomization (Panel C.1), and after sample adjustment (Panel C.2). Results indicate that individual characteristics are unbalanced across the treatment and control groups, even after dropping outliers. For this reason, we control for the gender and grade of the student; and whether s/he owns her/his own phone, as well as interactions between these variables and a dummy variable = 1 for Q2-Q4, and 0 otherwise in Table C.7. We dropped 22 schools (corresponding to 4,105 students). Those schools are considered outliers according to dropouts and gender in Q1. P-values from a test of equality of means of the treatment and control groups for each covariate controlling for individual characteristics (jointly for the F-test), with standard errors clustered at the classroom level. Those controls are the gender and the grade of the student; and whether s/he owns her/his own phone.

Panel D: Balance tests for students with valid outcome data (used in regressions; after dropping outliers)

	Sub-sample means		Diff.=0 [p-value]	Number of observations
	Treatment	Control		
Absenteeism				
Girl	0.46	0.48	**0.031**	13,980
Grade	1.84	1.84	0.931	13,980
Student owns phone	0.42	0.41	0.282	13,980
F-test			0.126	13,980
Self-reported motivation (weeks 2 to 4)				
Girl	0.44	0.40	0.264	654
Grade	1.79	1.95	0.536	654
Student owns phone	0.20	0.23	0.383	654
F-test			0.449	654
Self-reported motivation – week 2				
Girl	0.44	0.40	0.524	233
Grade	1.74	1.95	0.124	233
Student owns phone	0.17	0.16	0.921	233
F-test			0.490	233
Self-reported motivation – week 3				
Girl	0.43	0.37	0.352	200
Grade	1.85	1.79	0.614	200
Student owns phone	0.10	0.30	**0.001**	200
F-test			**0.002**	200
Self-reported motivation – week 4				
Girl	0.44	0.42	0.750	221
Grade	1.78	1.80	0.910	221
Student owns phone	0.34	0.25	0.164	221
F-test			0.565	221
Self-reported motivation – week 5				
Girl	0.38	0.47	0.191	219
Grade	1.82	1.67	0.249	219
Student owns phone	0.24	0.21	0.630	219
F-test			0.309	219
Self-reported motivation – week 6				
Girl	0.40	0.49	0.167	251
Grade	1.72	1.80	0.555	251
Student owns phone	0.27	0.23	0.460	251
F-test			0.425	251
Self-reported motivation – week 7				
Girl	0.42	0.37	0.489	225
Grade	1.74	1.90	0.220	225
Student owns phone	0.26	0.29	0.676	225
F-test			0.565	225

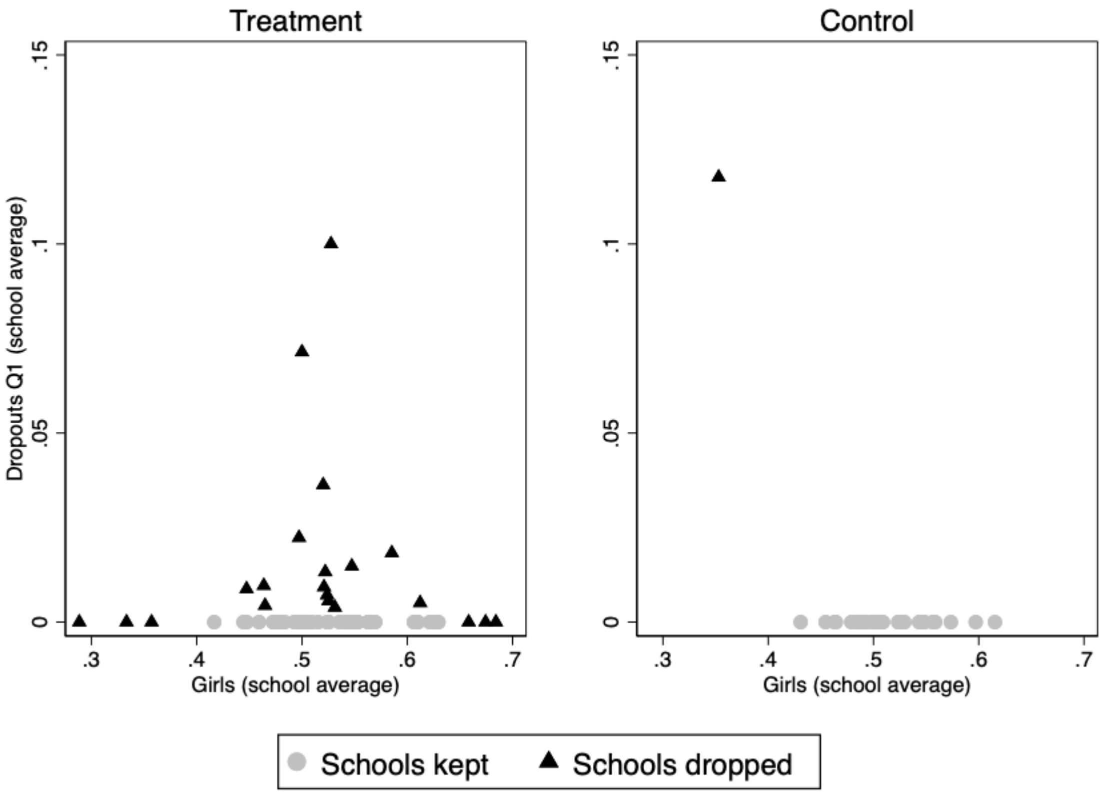
Notes: Ordinary Least Squares (OLS) regressions of student characteristics on treatment assignment for students with valid outcome data. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. P-values from a test of equality of means of the treatment and control groups for each covariate (jointly for the F-test), with standard errors clustered at the classroom level. Results indicate that student characteristics are balanced across the treatment and control groups when data are pooled together and for all weeks except week 3. For this reason, we control for student characteristics in all week-by-week regressions in Figure B.2 – Panel B.

Panel E: Balance tests for school characteristics (after dropping outliers)

	Sub-sample means in Q1		ANOVA test (p-value)	# of obs.
	Control	Treatment		
Science laboratory	0.621	0.500	0.338	65
Informatic laboratory	0.862	0.833	0.754	65
Sports facilities	0.931	0.917	0.832	65
Reading room	0.241	0.278	0.745	65
Room for Specialized Educational Assistance	0.345	0.444	0.423	65
# of classrooms used	13.000	12.333	0.456	65
# of classrooms used with climatization	6.069	7.083	0.511	65
Satellite dish	0.310	0.278	0.778	65
Multifunction printer	0.793	0.694	0.376	65
Scanner	0.517	0.361	0.213	65
Desktop computers for students	0.621	0.722	0.392	65
# desktop computers for students	7.379	6.528	0.698	65
Laptops for students	0.414	0.556	0.263	65
# laptops for students	2.414	1.611	0.536	65
Tablets for students	0.034	0.139	0.153	65
# tablets for students	0.069	0.889	0.171	65
Internet access	0.931	0.972	0.439	65
Internet access for student use	0.517	0.611	0.455	65
Internet access for use in teaching and learning processes	0.724	0.861	0.175	65
Internet access for community use	0.034	0.056	0.693	65
Broadband internet	0.926	0.886	0.603	62
# of teaching professionals	4.931	4.583	0.147	65
Website for institutional communication	0.759	0.917	**0.081**	65
School council	0.724	0.639	0.473	65
F-test			0.165	62

Notes: Ordinary Least Squares (OLS) regressions of school characteristics on treatment assignment after dropping outliers. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. P-values from a test of equality of means of the treatment and control groups for each covariate (jointly for the F-test). Results indicate that school characteristics are balanced across the treatment and control groups. Variables from the Brazilian School Census. Data collected in 2019.

Figure D.1: Outlier schools with respect to the baseline distributions of dropout risk and gender



Notes: We drop 22 outliers at the school level in Q1 (corresponding to 4,105 students). Those schools are considered outliers according to dropouts and gender in Q1.

Table D.2: Selective non-response tests for self-reported data (weeks 2 to 4; after dropping outliers)

	Weeks 2 to 4 (fig.2)
	(1)
Nudges	-0.0033
	(0.0042)
Control mean	0.0507
Observations	13,444
R-squared	0.0094

Notes: Ordinary Least Squares (OLS) regression with outcome variable = 1 if the student responded to the SMS surveys during the intervention (i.e. between weeks 2 and 4), and 0 otherwise. P-value from a Wald test of equality of coefficients between the treatment and control groups, with standard errors clustered at the classroom level. Results show that the probability of responding to SMS surveys is not systematically affected by the treatment. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. We control for student's gender, grade and whether s/he owns her/his own phone.

Table D.3: Selective non-response tests for self-reported data (week by week; after dropping outliers)

	Week 2	Week 3	Week 4	Week 5	Week 6	Week 7
	(1)	(2)	(3)	(4)	(5)	(6)
Nudges	0.0009	-0.0019	-0.0024	-0.0020	0.0001	-0.0025
	(0.0025)	(0.0024)	(0.0024)	(0.0023)	(0.0026)	(0.0026)
Control mean	0.0176	0.0165	0.0184	0.0181	0.0193	0.0188
Observations	13,012	12,974	12,996	12,994	13,022	12,998
R-squared	0.0054	0.0039	0.0016	0.0033	0.0028	0.0022

Notes: Ordinary Least Squares (OLS) regressions with outcome variable = 1 if the student responded to the SMS survey in each week, and 0 otherwise. P-value from a Wald test of equality of coefficients between the treatment and control groups, with standard errors clustered at the classroom level. Results show that the probability of responding to SMS surveys is not systematically affected by the treatment at any week. We dropped 22 schools that are considered outliers according to dropouts and gender in Q1. We control for student's gender, grade and whether s/he owns her/his own phone.

E. Robustness Checks

Table E.1: Treatment effects on students without attendance before winter break, during school closures (based on administrative data), for alternative definitions of the outcome variable (after dropping outliers)

	No attendance over the last X weeks	
	1 week	3 weeks
	(1)	(2)
Nudges	-0.0970***	-0.0675***
	(0.0103)	(0.0074)
Control mean	0.1186	0.0690
Observations	709,035	709,035
R-squared	0.0071	0.0057

Notes: ITT estimates from Ordinary Least Squares (OLS) regressions with outcome variable = 1 if a student had no attendance on record over the last week (column 1) or over the last three weeks (column 2) before the winter break, and 0 otherwise. P-value from a Wald test of equality of coefficients between the treatment and control groups, with standard errors clustered at the classroom level. We dropped 22 schools that are considered outliers according to dropouts and gender in Q1. Results show that patterns showcased by Figure B.1 are robust to alternative definitions of prolonged absenteeism during school closures.

Table E.2: Treatment effects of SMS nudges (before dropping outliers)

Panel A: Student dropout risk

	High dropout risk			
	Q4	Q1 vs. Q4	Q1 vs. Q2-Q4	
	(1)	(2)	(3)	(4)
Nudges	0.0004			
	(0.0035)			
Nudges x After		-0.0051	-0.0040	-0.0028
		(0.0035)	(0.0025)	(0.0023)
Nudges x Dose				-0.0012
				(0.0016)
After		0.0243***	0.0173***	0.0103***
		(0.0028)	(0.0020)	(0.0018)
Dose				0.0070***
				(0.0013)
Classroom fixed-effects	No	Yes	Yes	Yes
Control mean (After=1)	0.0246	0.0246	0.0176	0.0176
Observations	18,020	36,040	54,060	54,060
R-squared	0.0043	0.0540	0.0470	0.0489

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with high risk of dropouts = 1 if the student had missing values in math and Portuguese grades, and 0 otherwise. Treatment = 1 in schools where students were nudged, and 0 otherwise. After = 1 for Q2-Q4, and 0 otherwise. Dose = 0 for Q1 and Q2, and = 2 for Q4. In column (1), we drop Q1 and Q2 observations from the sample. In column (3), we drop Q2 observations from the sample. In column (4), we add an interaction between treatment and dose. We control for individual characteristics in all columns. Those controls are the gender and grade of the student; and whether s/he owns her/his own phone. Standard errors in parentheses clustered at the classroom level. * p<0.1, ** p<0.05, *** p<0.01.

Panel B: Prolonged absenteeism

	No attendance over the last X weeks		
	1 week	2 weeks	3 weeks
	(1)	(2)	(3)
Nudges	-0.0704***	-0.0466***	-0.0455***
	(0.0086)	(0.0060)	(0.0059)
Control mean	0.0961	0.0603	0.0577
p-value([Nudges]=0)	0.0000	0.0000	0.0000
Observations	963,985	963,985	963,985
R-squared	0.0059	0.0040	0.0040

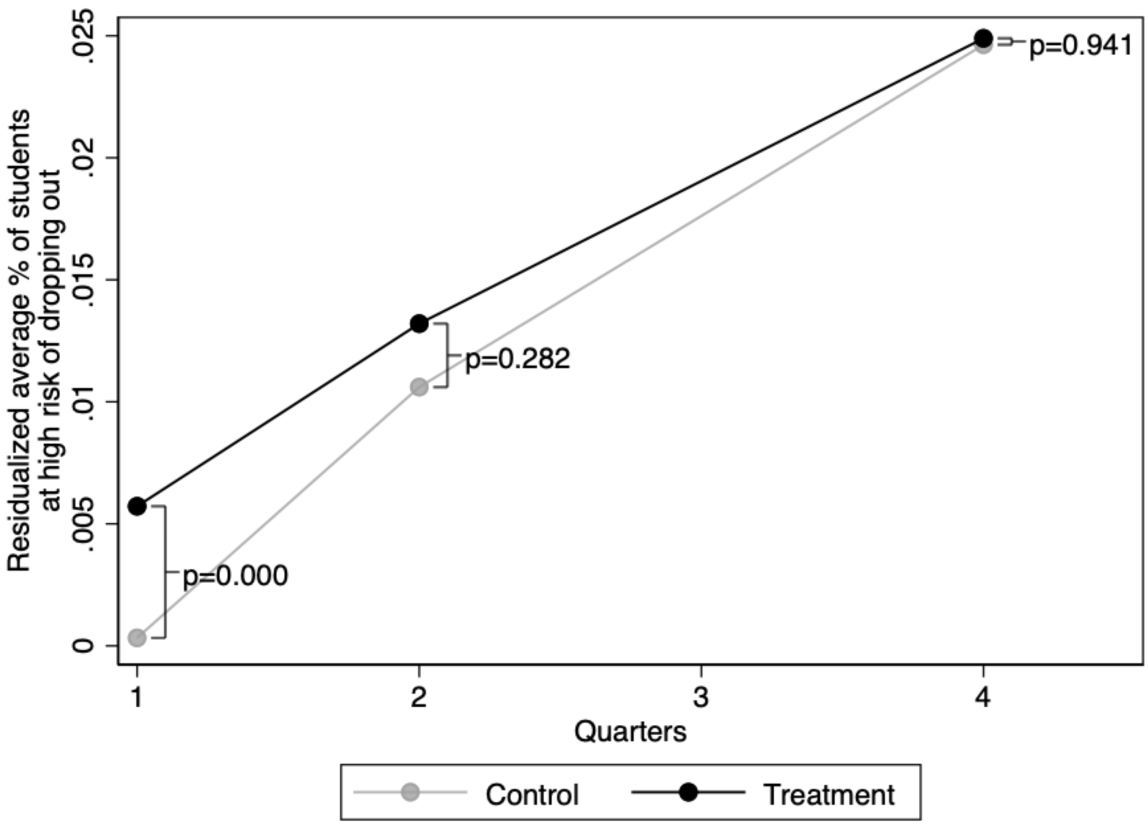
Notes: ITT estimates from Ordinary Least Squares (OLS) regressions with outcome variable = 1 if a student had no attendance on record over the last week (column 1), over the last two weeks (column 2), or over the last three weeks (column 3) before the winter break, and 0 otherwise. P-value from a Wald test of equality of coefficients between the treatment and control groups, with standard errors clustered at the classroom level.

Panel C: Students' lack of motivation to return to school once they reopen (based on self-reported data), week by week

	Weeks 2 to 4	Week 2	Week 3	Week 4	Week 5	Week 6	Week 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Nudges	-0.1011***	-0.0490	-0.0508	-0.1698***	-0.1001**	-0.0774	-0.0813
	(0.0292)	(0.0435)	(0.0489)	(0.0549)	(0.0526)	(0.0530)	(0.0613)
Control mean	0.2894	0.1500	0.2105	0.3491	0.2692	0.3455	0.3925
p-value([Nudges]=0)	0.0006	0.2622	0.3006	0.0022	0.0584	0.1460	0.1863
Observations	828	284	249	295	289	319	273
R-squared	0.0247	0.0207	0.0491	0.0448	0.0151	0.0134	0.0149

Notes: ITT estimates from Ordinary Least Squares (OLS) regressions week by week with outcome variable = 1 if the student states that s/he does not think s/he will be back in school when regular classes resume, and 0 otherwise. P-value from a Wald test of equality of coefficients between the treatment and control groups, with standard errors clustered at the classroom level. We control for student's gender, grade and whether s/he owns her/his own phone.

Figure E.1: High risk of dropouts



Notes: Treatment = 1 in schools where students were nudged, and 0 otherwise. Quarterly sample averages for students at high risk of dropout (i.e. % of students without math and Portuguese grades) for the treatment group (in black) and the control group (in light grey). P-values computed with standard errors clustered at the classroom level.

Table E.3: Treatment effects of SMS nudges on student dropout risk
(after dropping outliers; with standard errors clustered at the school level)

	High dropout risk			
	Q4	Q1 vs. Q4	Q1 vs. Q2-Q4	
	(1)	(2)	(3)	(4)
Nudges	-0.0061			
	(0.0064)			
Nudges x After		-0.0063	-0.0040	-0.0017
		(0.0066)	(0.0046)	(0.0039)
Nudges x Dose				-0.0023
				(0.0028)
After		0.0242***	0.0172***	0.0101***
		(0.0055)	(0.0039)	(0.0033)
Dose				0.0070***
				(0.0022)
Classroom fixed-effects	No	Yes	Yes	Yes
Control mean (After=1)	0.0242	0.0242	0.0172	0.0172
Observations	13,915	27,830	41,745	41,745
R-squared	0.0042	0.0453	0.0369	0.0392

Notes: ITT estimate from an Ordinary Least Squares (OLS) regression with high risk of dropouts = 1 if the student had missing values in math and Portuguese grades, and 0 otherwise. Treatment = 1 in schools where students were nudged, and 0 otherwise. After = 1 for Q2-Q4, and 0 otherwise. Dose = 0 for Q1 and Q2, and = 2 for Q4. In column (1), we drop Q1 and Q2 observations from the sample. In column (3), we drop Q2 observations from the sample. In column (4), we add an interaction between treatment and dose. We dropped 22 schools (corresponding to 4,105 students) – outliers with respect to the baseline distributions of dropout risk and gender. We control for individual characteristics in all columns. Those controls are the gender and grade of the student; and whether s/he owns her/his own phone. Standard errors in parentheses clustered at the school level. * p<0.1, ** p<0.05, *** p<0.01.

F. Pre-Analysis Plan

This randomized controlled trial was pre-registered as trial 5986 at the American Economic Association's registry for randomized controlled trial (AEA RCT Registry). The uploaded plan is detailed below.

Does Nudging Students Decrease Learning Deficits and Dropouts During and After a Pandemic? Experimental Evidence from Covid-19 Responses in Brazil

Pre-analysis Plan

The covid-19 pandemic has forced 1.5 billion schoolchildren in 160 countries to stay at home while schools were shut down on sanitary grounds. While several remote learning tools have been put in place in developing countries, a variety of factors raise critical concerns about learning deficits and school dropouts when schools are back, particularly amongst the most vulnerable students. This paper investigates whether sending reminders and encouragement messages to high-school students in Brazil during the pandemic increases attendance and assignment completion when it comes to remote learning, and decreases grade repetition and dropout rates in the aftermath.

I. Introduction

The covid-19 pandemic has forced 1.5 billion schoolchildren in 160 countries to stay at home while schools were shut down on sanitary grounds. Brazil is no exception. The nationwide decision to shut down schools for almost the entirety of the 2020 school year in order to limit the spread of the covid-19 pandemic has forced all schools to switch to remote learning. Such rapid transition, combined with a mismatch between delivery channels and access conditions – as several State Secretariats of Education switched to online, while nearly 70 million households have no or only precarious access to internet –, are expected to severely impact learning, and potentially lead to a spike in school dropouts (Brookings, 2020; World Bank, 2020).

Schools have been trying to keep contact with their students by sending personal letters via post or by creating an online platform with tools that students can use. However, the attendance of the students, whether on the platform with the online tools or at school to pick up printed class material, is reported to be remarkably low. São Paulo State has reported that only 50% of its 3.5 million students are accessing the online learning platform daily as expected.[*]

[*] The State Secretariat also broadcasts content on television. It is much harder to gather data on the share of students following classes on this format daily.

With the goal of increasing engagement in remote learning – and, particularly, online attendance and assignment completion – during the pandemic, as well as limiting its effects on learning gaps and school dropouts once schools are back, the Goiás State Secretariat of Education is testing various strategies in partnership with Instituto Sonho Grande.[†] As part of those strategies, they are interested in evaluating nudges (reminders and encouragement messages) sent twice a week to high school students, directly on their mobile phones via text messages (SMS). Towards that goal, they have hired Eduq+, an educational nudgebot that has been shown to improve educational outcomes (during normal times) in Brazil and Ivory Coast.

Eduq+ nudges users twice a week with motivating facts and suggested activities to engage them in the daily school life. It also allows schools to broadcast messages to all users weekly. The intervention has been evaluated in the context of regular schooling, targeted at parents of primary school children. The nudgebot has been shown to promote large impacts on school attendance, test scores and grade promotion rates (Bettinger et al., 2020), and to decrease school dropouts by 50% across multiple primary grades (Lichand and Wolf, 2020).

The version of Eduq+ to be evaluated in this study is, however, different from that in those studies, since nudges will be sent directly to students themselves.[‡] Moreover, the context of remote learning is also much more challenging. Whether the intervention is still able to improve educational outcomes under those conditions is an empirical question.

This pre-analysis plan summarizes the design of a field experiment to test the following primary hypotheses:

1. Does nudging students increase usage of online learning tools by high school students?
    - Hypothesis: SMS nudges increase the share of students who access the online platform daily, and the share of students who hand in assignments (online or not).
2. Does nudging students mitigate the negative effects of school closures on learning outcomes?
    - Hypothesis: SMS nudges improve attendance and grades, and decrease grade repetition and dropouts once in-person classes resume.

II. Intervention and experimental design

The intervention has been designed by Instituto Sonho Grande and the Goiás State Secretariat of Education, with the help of Movva (the implementing partner that powers Eduq+).[§] It will take place during the months

[†] Goiás a relatively poor state located in the Center-West region of Brazil. Instituo Sonho Grande is a non-profit organization committed to improving high-school educational outcomes in Brazilian public school.
[‡] In case they do not have their own phone, messages will be sent to the mobile phone of their primary caregivers.
[§] One of the authors (Guilherme) is a co-founder and chairman at Movva (http://movva.tech).

of June and July/2020, when public high schools will be randomly assigned to have their students receive two messages per week from Eduq+. 57 schools have been assigned to the treatment group, and 30 to the control group (which receives no intervention). Randomization is stratified by gender, grade and phone ownership. In case the student does not own a phone, messages will be sent to the mobile phone of his/her primary caregiver. The intervention is scheduled to be rolled out on June 9th.

Table 1: Randomization strategy - Treatment vs. Control

Treatment	**Control**
57 schools 12,056 students	30 schools 6,200 students

Table 1 above summarizes the randomization strategy for the first phase of intervention. Within the sample of 12,056 students assigned to receive nudges, less than half (5,188) own their own mobile phone and will receive messages directly. It is also important to note that not all students in the sample have access to the internet and that those who do not can pick up the printed class material once every week and hand in assignments the following week. For the purpose of this study however, we will be able to measure their outcomes in different ways.

At the end of July, we will be able estimate treatment effects on access to the online platform, and assignment completion, from administrative data provided by the Secretariat. Concretely, we have requested weekly student-level data on log in activity – or face-to-face pick-up of class materials – as well as assignment completion (again, online or offline). For those with online access, we hope to get access to daily data, which would allow us to also estimate high-frequency treatment effects through event studies. Last, after in-person classes resume, we will have access to administrative records on student-level attendance, grades, grade repetition and enrollment status.

The interpretation of these long-term effects will vary depending on the choice made by the Education Secretariat to continue or not the intervention after short-term results are made available. Depending on the short-term impacts of the nudges, the Education Secretariat might decide to keep testing Eduq+ for a longer period, to scale it up or to scale it down. As such, three scenarios can emerge after the first phase of the intervention: (1) the intervention continues for a longer period, keeping the treatment assignment fixed; (2) the control group starts receiving the nudges; or (3) the treatment group stops receiving the nudges. In case (1), long-term effects will reflect a combination of nudges sent during and after school closures; in case (2), long-term effects will only reflect differences in the intensity of the treatment; and in case (3), long-term effects will capture persistence of treatment effects (if any).

With the number of schools and the number of students presented in Table 1, and assuming an intra-cluster correlation of 0.16 (SARESP, 2014a, 2014b) as well as conservative variance estimation for binary outcomes (assuming that 50% of students access the online platform and hand in assignments, in the control group)-, we could detect treatment effects of at least 0.8 percentage points on those outcomes[**]. Since the typical treatment effect of nudges on binary decisions is 1.7 percentage points (Dellavigna and Linos, 2020), we conclude that the design is well powered to detect relevant short-term effect sizes.

III. Outcomes

We will document the effects of the treatments on the following categories of outcomes for students enrolled in high school (age 15 to 18):

A. Short-term outcomes: probability of logging into the online platform or picking up the material in school, probability of handed in of assignments, as measured by administrative records;

B. Long-term outcomes: attendance, grades, probability of grade repetition and probability of dropout, as measured by administrative records.

Since some students will receive messages on their own mobile phones, while for others it is their caregivers who will be nudged by Eduq+, we will estimate treatment effects within those two subgroups. Power calculations indicate that we could detect treatment effects of at least 1 and 0.9 percentage point for these two subsamples, respectively.

Since there are siblings in the data, we will remove from the main analysis cases when not all siblings are assigned to the same treatment conditions. Depending on how many siblings there are, we also plan to estimate within-family's externalities of the nudges, taking advantage of that sub-sample.

IV. Empirical analysis

Since the intervention is randomly assigned, comparing treatment and control groups yields treatment effects of the SMS nudges on the outcomes of interest (Section III). Using ordinary least squares regressions, we will estimate:

$$Y_{smi}^{j} = \beta_0 + \beta_1 T_{sm} + \theta_s + u_{smi}$$

Where:

- Y_{smi}^{j}: Outcome variable j for student i at school m and stratum s;

[**] These power calculations have been computed by clustering at the school level.

- T_m: Indicator variable equal to 1 if students I in school m and stratum s is assigned to receive SMS nudges, 0 otherwise;
- θ_s: stratum fixed effects.

We cluster standard errors at the school level, since that is the level at which the intervention is randomly assigned. We are interested in testing $\beta_1 = 0$.

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