Incentives, Evidence, and Reminders for Bureaucrats: Overcoming Barriers to Policy Scale Up*

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Abstract

Scaling up effective policies often requires the attention of frontline bureaucrats with many competing responsibilities. Even when policymakers adopt effective programs, implementation may not follow. In a nationwide experiment in the Dominican Republic, we test interventions to increase school principals' implementation of an educational program proven effective in a previous RCT. Only 37% of control schools verifiably implemented the intervention when ordered to by the Ministry of Education, compared with 83% in the original trial. Implementation was no higher among schools that previously implemented the program in the RCT, suggesting that fixed costs of adoption do not explain non-adoption. We find precise null effects of sharing research evidence, providing modest financial incentives, or offering implementation assistance to principals. In contrast, additional reminder calls increased implementation by 20 percentage points. A second experiment targeting a different mandated program yields the same pattern: reminders produce large effects, while monitoring messages have smaller effects. Our findings point to limited attention among bureaucrats as an important barrier to scaling policies.

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1 Introduction

Evidence-based policymaking requires at least three steps: generating evidence on what works, policymakers using such evidence to guide policy choice, and governments successfully implementing the chosen policy. Randomized controlled trials are increasingly used for the first step, while recent work has studied when and how evidence generation affects policy choice (Hjort et al., 2021; DellaVigna et al., 2024; Rao, 2024; García-Hombrados et al., 2024). Yet even when evidence exists and policymakers act on it, execution often depends on frontline bureaucrats with limited bandwidth and many competing demands. Policy selection at higher levels may thus not translate into change on the ground (Muralidharan and Niehaus, 2017; Vivalt, 2020; List, 2022). This paper studies the last step in an otherwise successful example of evidence-based policymaking: what happens *after* an evidence-based policy is chosen by policymakers.

We examine this problem in the context of a nationwide educational intervention in the Dominican Republic. The program, *Aprendiendo el Valor de la Educación* (AVE), provides students with information on the returns to schooling through classroom videos. Building on evidence that correcting misperceptions about education returns can increase schooling (Jensen, 2010), the initial RCT—conducted in two phases in 2015 and 2016 with 1,594 treatment schools—produced positive results: test scores rose by 0.05 standard deviations and dropout rates fell by 3 percentage points (Berry et al., 2018). Encouraged by these results, the Ministry of Education adopted AVE in 2018 and mandated its implementation across all public secondary schools.

The experimental and scaled interventions were meant to deliver the same content to students, but their implementation protocols differed. The initial trial relied on intensive in-person workshops to train and motivate school officials. The nationwide rollout, by contrast, used a more standard and less expensive approach to roll out the new educational programs. Specifically, the ministry informed school principals about the program by adding AVE to the official school calendar and by sending emails and a follow-up phone call to principals to instruct them to implement it. This shift provides a setting to examine whether implementation frictions that were minimized in the evaluation RCT become binding constraints when delivered as usual

at full scale. We partnered with the government during the rollout to implement experimental interventions designed to overcome these barriers.

Implementation during the national rollout is far from universal. Only 37 percent of control group schools completed the program and provided proof as ordered by the ministry, compared with much higher rate of 83% in the original trial.[1] This incomplete implementation could substantially attenuate the program's aggregate impact. It represents an instance of the voltage effect operating through the extensive margin of implementation rather than diminished treatment efficacy (List, 2022).

What explains this implementation gap? We examine several potential mechanisms. First, fixed costs of adoption do not appear to be the main barrier. Implementation is no higher among schools with prior experience—those randomly assigned to treatment in the original RCT. Second, principals do not appear to select into implementation based on predicted student benefits. Third, we find suggestive evidence that the principal's 'bandwidth constraints' matter: schools with a secretary to assist the principal show higher implementation rates. Together, these patterns are difficult to reconcile with a pure cost-benefit explanation and instead point toward organizational or attentional frictions in implementation.

To identify which barriers bind most strongly, we randomized $n = 2{,}724$ schools to different interventions designed to increase implementation. Control school principals received detailed instructions via email and one follow-up phone call, which is the standard government practice. Treatment schools received additional interventions targeting specific frictions: sharing evidence on program effectiveness, providing modest financial incentives, or offering assistance with implementation. We find precise null effects of each of these interventions, consistent with our observational evidence that neither learning costs nor perceived benefits are the primary constraint.

In contrast, simply reminding principals via two additional phone calls led to a 20 percentage point ($p < 0.001$) increase in implementation as measured by our prespecified primary outcome. The reminders provided no new information about the program and were more effective among principals whom we classify as having

[1] Our pre-specified primary outcome measure is whether principals submitted proof of implementation. We find qualitatively similar results using other measures of implementation, including student reports.

more demands on their attention. A second experiment reinforces this finding: when principals were asked to implement a different mandated program (an anti-bullying campaign), reminders again produced large effects (24 percentage points, $p < 0.01$), while messages emphasizing monitoring had statistically significant but smaller effects (4.5 percentage points, $p < 0.01$). The evidence is consistent with limited attention or memory as a central constraint on scaling effective policies.[2]

Our findings contribute to three strands of literature. First, they speak to work on the voltage effect—the phenomenon that interventions successful in trials often fail at scale (Banerjee et al., 2017; Al-Ubaydli et al., 2017; Bold et al., 2018; List, 2022). This literature documents several reasons why treatment effects diminish with scale and changes in context. We add evidence of a likely voltage drop in government through incomplete implementation by frontline bureaucrats, with little evidence of selection on expected benefits.

Second, our findings relate to a large literature on bureaucratic performance and public sector management in the developing world (see Finan et al. (2017); Pepinsky et al. (2017); Besley et al. (2022) for reviews). The primary lens in this literature is one of principal-agent problems. We instead point to a behavioral friction and show that simple reminders can substantially increase compliance with mandated policies, even when traditional—and more expensive—tools like financial incentives and technical assistance prove ineffective. In doing so, we build a bridge to the behavioral science literature on reminders, which typically target consumers and patients rather than bureaucrats or managers (Thaler and Sunstein, 2021; Szilagyi et al., 2000). Our findings resemble those of Gertler et al. (2025), who study behavioral frictions among managers of firms in Mexico, finding that reminders substantially increase the take-up of a profitable offer among small firms.

Third, our results inform research on policy adoption and the use of evidence by decision-makers (Hjort et al., 2021; García-Hombrados et al., 2024; DellaVigna et al., 2024). This work has not separated two key steps: selection of an effective policy and its actual implementation. We focus on a setting where high-level policymakers

[2]Reminders might also signal how important the program is to the Ministry. However, one might reasonably expect the evidence and incentive treatments to similarly signal the Ministry's interest, yet they have no effect on implementation. Similarly, the monitoring messaging in the second experiment has a much smaller effect than the reminder treatment.

had already chosen a policy proven effective in an RCT they commissioned, yet on-the-ground adoption remained far from complete. Our null effect of sharing research evidence contrasts with Hjort et al. (2021), who find that evidence increases policy adoption by mayors in Brazil. One interpretation is that different barriers bind at different levels of the bureaucratic hierarchy: mayors may be constrained by lack of information, while school principals face attentional overload rather than informational frictions. Similarly, while DellaVigna et al. (2024) find that organizational inertia predicts adoption of nudge treatments in US cities, we find no effect of prior implementation experience—plausibly because temporary pilot participation does not build the lasting institutional routines needed to overcome such inertia.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and AVE program. Section 3 presents the experimental design. Section 4 outlines the empirical strategy, and Section 5 presents results. Section 6 concludes.

2 Institutional Setting and the AVE Program

The education system in the Dominican Republic consists of six years each of primary and secondary education. Seventy-seven percent of secondary schools are public. Public secondary schools are run by principals appointed by the Ministry of Education. Principals have wide-ranging authority within the school but are required to follow the official school curriculum designed by the Ministry of Education. Schools are organized into 122 educational districts.

School quality is low, and dropout is widespread. The Dominican Republic ranked last in international student performance assessments in 2013 and 2015 (UNESCO, 2015; OECD, 2016), and approximately 38 percent of students who take the 8th-grade national exam do not complete high school.

Motivated by earlier research that shows that students underestimate the returns to schooling (Jensen, 2010), the Ministry of Education collaborated with academics to create a scalable version of an education information campaign. This partnership produced the "Aprendiendo el Valor de la Educación" (AVE) program, which comprises four 15-minute videos designed to provide secondary school students with information about the potential monetary and non-monetary benefits of education.

Earnings information was drawn from nationally representative surveys and presented in a comprehensible manner. For instance, wages are discussed by showing the distribution of wages for 100 representative adults for a given level of schooling. The videos also illustrated how various factors, such as luck and student performance, contribute to the heterogeneity of outcomes. Implementation involved students watching the videos during class and discussing them with their teachers.

The program was evaluated in a randomized field experiment conducted in 2015 and 2016, as outlined by Berry et al. (2018). In the initial round of the study in 2015, the Ministry of Education randomly assigned 398 public schools to the treatment group to implement the program for 7th and 8th-grade students and 200 public schools to the control group.[3] School officials from all treated institutions were required to attend a two-day workshop in Santo Domingo, during which they received physical copies of the videos and detailed instructions for implementation. Subsequently, schools received multiple follow-up phone calls to ensure program adherence.

In the second round of the study in 2016, the program expanded to include students across all secondary school grades (7th through 12th). The sample comprised 2,469 public schools, of which 1,594 were assigned to the treatment group. This round also retained the schools from the first round, which maintained their original treatment assignments. Due to the larger sample size, the program was implemented in a decentralized manner. The Ministry of Education initially provided the program material to the educational districts, which then organized in-person workshops with local schools to facilitate program implementation. Implementation rates—achieved through intensive in-person engagement—were 97% in the first round and 80% in the second, making for an average implementation rate of 83%. Intent-to-treat estimates from Berry et al. (2018) reveal that the AVE program increased test scores by 0.05 standard deviations and reduced dropout rates in the following year by 3 percentage points.

Based on these findings, the Ministry of Education decided to expand the AVE

[3]The initial experiment consisted of two treatment arms in which the program either emphasized quantitative statistics or qualitative information. Since both treatment arms had similar effects on test scores and dropout, we pool them together. The scale-up version of the program was based on the quantitative statistics treatment.

program in the 2018-19 academic year.[4] Under the new initiative, every student enrolled in 7th grade or above was required to watch the program's videos within the school setting. To facilitate this, the government designated a specific week, known as AVE week, and incorporated the intervention into the official school calendar, during the week of September 24-28 (see Online Figure B.1). This school calendar—and thus the AVE program—is in theory compulsory for all schools.

To scale the program, the government changed the implementation approach. Instead of conducting in-person workshops with principals, the scaled-up version of the program instructed principals to download the necessary materials, including the videos and implementation protocols, from an online platform. These instructions and protocols were directly communicated by the Ministry to principals via phone and email.

3 Experimental design

After the Ministry decided to scale up the program, we partnered with them to experimentally test which interventions could increase implementation by school principals. Implementation rates in the original trial were 83%, suggesting at least some room for improvement. Moreover, we anticipated that switching from in-person workshops to the usual channels for informing and motivating principals might further reduce implementation.

3.1 Sample selection

Our initial sampling frame was all 4,351 schools which offered secondary school grades. We excluded schools with fewer than five students enrolled in any secondary school grade (n=162). We further restricted the sample to the 3,881 schools that participated in either the 8th-grade National Exam in 2016 or the 12th-grade National Exam in 2018, effectively excluding informal schools from our analysis (n=301).

In cases where principals supervised multiple schools meeting our sample criteria,

[4]The government did not require any schools to implement the AVE program during the 2017–18 academic year, resulting in a one-year gap between the second round of the initial study and the nationwide scale-up.

one school was randomly selected, and the others were excluded (n=119). Among the remaining 3,762 schools, we dropped those whose principals were unreachable by phone during the baseline survey (n=626), those where the school principal did not have email access (n=343), a few schools that were used for piloting (n=44), and a small number of additional administrative exclusions (n=25). Ultimately, our final sample consisted of 2,724 principals. For a detailed breakdown of the sample selection process, see Table A.1 in Appendix A.[5]

3.2 Implementation protocol

The academic term began on August 20. Between August 20 and Sep 4, we attempted to call 3,881 principals to conduct baseline surveys over the phone, successfully reaching 83% of them.

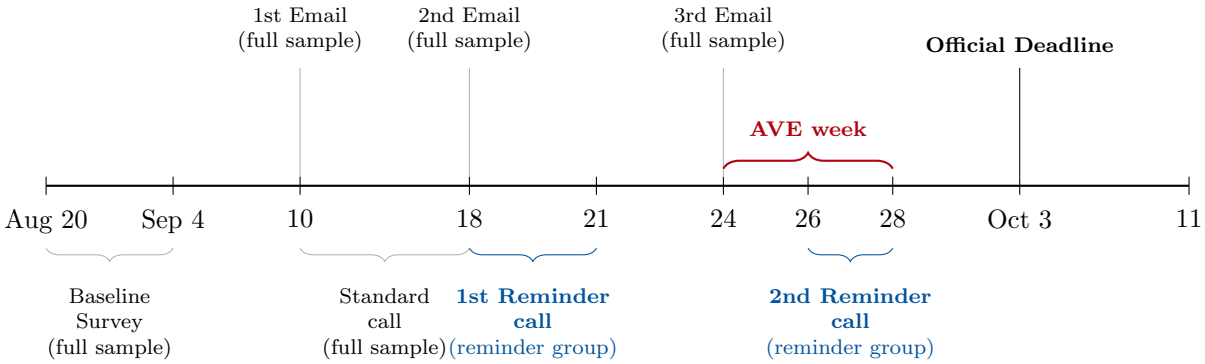
The academic calendar, posted by the Ministry before term began, indicated to all schools that AVE week was Sep 24 to 28. On September 10th—two weeks before AVE week—the Ministry emailed all school principals a link to the AVE platform, from which school staff could download the program's videos and implementation protocol, along with a brochure with instructions (see Figure B.2 for an example). During the following week, the Ministry called all principals to repeat this information and direct attention to the email. On Sep 18 and Sep 24—one week before and the start date of the AVE week—principals were sent a second and third email repeating these instructions. All communications were sent from official Ministry of Education email accounts and phones.

The Ministry instructed principals to submit a form and a photo of students watching the AVE videos as proof of implementation, with a deadline of October 3rd. Figure 1 shows the full timeline.[6]

[5] How might the exclusion criteria we used affect our subsequent experimental results? Excluding schools with very few students is unproblematic. Uncontactable principals and those supervising multiple schools might be less likely to implement the program on average. Whether they would have higher or lower treatment effects is ambiguous, but it seems plausible that they would react less to the reminder intervention, which involved two additional phone calls.

[6] As of October 11, only 41% of principals had submitted a photo. Reacting to this low compliance, the Ministry then sent two additional emails and made two follow-up calls to all principals who had not yet responded. We use October 11 as the cut-off date for our analysis and also report effects as on Dec 1 as a robustness check.

Figure 1: Implementation Timeline



Notes: This figure shows the timeline of communications sent to principals. Items labeled "(Full Sample)" were sent to all principals. Items labeled "(Reminders)" were sent only to the reminders treatment group.

3.3 Treatment arms

We randomized schools into several treatment arms, each targeting a different potential barrier to implementation: "Evidence" (sharing research findings), "Incentive" (financial motivation), "Reminders" (additional follow-up calls), and "Assistance" (implementation support). We describe each below.

Evidence Treatment. In the first treatment arm, principals received information about the results of the AVE pilot. Specifically, the emails and phone call script included the following paragraph:

> *It is important to note that a research study, carried out between 2014 and 2016 in 2,500 schools in the Dominican Republic, showed that implementing this campaign significantly improves scores on national tests and decreases dropout rates. The schools that showed the videos increased national test scores by 0.6 points in total. This study shows that if all the schools in the country showed these videos to their students, the number of dropouts in secondary schools would decrease by 6,500 students per year."*

This information was also conveyed through easy-to-read figures in the brochure sent to principals in the evidence group (see Figure B.3). The script for the phone calls

in each treatment are included in Appendix C.

Incentive Treatment. In a second treatment arm, principals received a financial incentive in the form of a lottery. The emails and the call scripts included this extra paragraph:

> *Please note that you have been randomly selected to be one of the directors who can participate in the draw for a Samsung tablet to help you run the school. You will enter this lottery only if you implement the AVE campaign at your school as outlined in the instructions below in this email. One out of 10 people who enter the drawing will win a tablet.*

The value of the tablets was equal to 16% of the average monthly salary of a school principal. This treatment was expected to serve as a monetary benchmark for the effectiveness of other treatment arms.

Reminders Treatment. In a third treatment arm, principals received two additional reminder calls from the Ministry. The first reminder call was between September 18th and 21st, after every school in the sample received the standard call, and about one week before AVE was to be implemented. The second reminder call was placed during the AVE week.

Assistance Treatment. A fourth treatment arm offered principals additional assistance through a helpline and a link to a website with further resources. This was meant to provide help if schools had any technical difficulties with downloading or screening the videos.

Randomization was conducted at the school level and stratified by school district. We cross-randomized the four treatments, with the exception that we excluded the group that contained both reminders and incentives to preserve budget and because we (incorrectly) anticipated high implementation rates and potentially ceiling effects. Figure A.1 illustrates the treatment cells.

3.4 Summary statistics and balance tests

Table A.3 shows summary statistics and balance across treatment arms, using both administrative data and the baseline survey. Panel A reports the characteristics

of schools and Panel B reports characteristics of principals. The average school in the control group has 224 students. Seventy-six percent of the schools in our sample were public and 73% were located in urban areas. Most schools had the necessary infrastructure to easily implement the AVE campaign, including electricity (99%), a projector (77%), and an internet connection (72%).[7] One-third of the sample was part of the treatment group in the previous pilot study of the AVE program. In the baseline survey, around half of the principals say that students undervalue education and 83% say that providing information on the benefits of completing schooling would significantly improve test scores and decrease dropout rates. While we treat these responses with caution since they might suffer from reporting bias, they suggest that most principals already see the program as being beneficial. The treatment and control groups are largely balanced across school and principal characteristics.

4 Outcome measures and empirical strategy

Our outcome of interest—implementation of the AVE program—is measured using different types of administrative and student data. To study heterogeneity of treatment effects, we also leverage the baseline survey and administrative data.

Emails were sent through a marketing platform that tracked whether each principal received, opened, and clicked on the email. The email contained links to an online platform where principals registered to download the AVE campaign material; we observe downloads linked to each principal. We also recorded all phone calls made to the schools. Overall, 98% of principals received the initial email and 75% opened it (Table A.4, columns 1 and 2).[8] Ninety-five percent also received the first call from the Ministry, which reiterated the information provided in the email (column 5). These rates are similar across treatment arms.

Principals were instructed to submit a photo via WhatsApp and a form with implementation details via email. Following our pre-analysis plan, we use an indicator variable for whether the principal (or their staff) uploaded a photo as our primary

[7] If a school did not have an internet connection, principals could still download the AVE material from a cybercafe or at home.

[8] Since the treatment information was also repeated in the calls, email opening rates are endogenous.

outcome. We also examine download rates and form submissions as secondary measures of implementation. We also aggregate information on downloads, photo and form submissions using a standardized index per Anderson (2008). We supplement these outcomes with information gathered through student questionnaires. Specifically, for students who took the 8th-grade or 12th-grade national exam in 2019, we included questions in the national exam asking whether their school had participated in the AVE campaign during the 2018-2019 academic year. This provides an entirely independent measure of implementation, with the caveat that it measured recall a year later for only a subset of students, and schools in all arms received additional reminders after our main study period ended (based on our results).

The primary treatment effects of interest are estimated by calculating the intent-to-treat (ITT) estimate of take-up via OLS:

$$Y_i = \sum_k \beta^k \cdot T_i^k + X_i'\gamma + \epsilon_i \tag{1}$$

where Y denotes the outcome for principal i, T^k is an indicator for assignment into the treatment arm k, and coefficients β^k are the parameters of interest. Our main analysis focuses on the average impact of each of the four treatments (without treatment interactions), but we also report the effects for each treatment cell. The vector X_i includes stratification dummies and baseline covariates selected via the double lasso approach by Belloni et al. (2014) from the covariates listed in Appendix Table A.3. Table A.6 reports q-values corrected for multiple hypotheses testing.

Preregistration and deviations from pre-analysis plan. Our experiment was pre-registered on the Social Science Registry. We follow the pre-analysis plan with minor deviations, detailed in Table A.2. In the text, we mention when an analysis was exploratory (i.e. not pre-registered).

5 Results

Descriptive results. We begin by examining program adoption in the control group. Overall implementation rates are modest: only 37% of control schools submitted photographic proof of implementation and 27% submitted the form by the deadline, compared with implementation rates—measured similarly—of 83% on

Table 1: Predictors of AVE Campaign Adoption

		Primary Outcome: Submitted Photo	
	Mean of Predictor in the Control Group	Only Control Group	Full Sample
	(1)	(2)	(3)
Limited Bandwidth Index	-0.000	-0.039	-0.036***
		(0.028)	(0.011)
Principal Has Multiple Positions	0.656	-0.098	-0.031
		(0.061)	(0.024)
Principal Has No Secretary	0.279	-0.075	-0.109***
		(0.070)	(0.028)
Benefits Index	-0.000	0.007	0.014
		(0.024)	(0.010)
Believes Extra Info Would Have an Effect on Students	0.826	0.049	0.007
		(0.064)	(0.027)
Believes Penalty after Non-Compliance is Very Likely	0.537	-0.024	0.032*
		(0.050)	(0.019)
Fixed Costs of Implementation			
School Was in the Treatment Group in Pilot	0.313	-0.042	0.030
		(0.053)	(0.020)
Outcome Mean		0.374	0.407
Observations		380	2,724

Notes: This table examines predictors of AVE campaign adoption. Column (1) shows the mean of the predictor in the control group. In Columns (2) and (3), we regress a dummy for whether the principal submitted a photo of the AVE campaign adoption on the corresponding predictor. Column (2) is restricted to the control group and Column (3) includes the full sample. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

average in the original trial. Indeed, only 69% of principals even downloaded the material to begin with.[9] This gap illustrates the "voltage effect" operating through the extensive margin (List, 2022): while the program to be delivered to students is itself unchanged, the lighter implementation protocol at scale sees many schools fail to deliver the program.

Table 1 examines predictors of program adoption. In an exploratory analysis,

[9] Among schools which had not been part of the treatment group in the previous trial two years ago, and thus had not had any access to the AVE materials whatsoever, download rates were still only 70%.

we group baseline characteristics into three classes of factors: (i) limited attentional bandwidth, (ii) perceived benefits of implementation, and (iii) fixed costs of implementation. The limited bandwidth index includes whether the principal has multiple positions (typically serving as a teacher in addition to being the principal) and whether the principal lacks a secretary. The benefits index uses principals' baseline survey responses regarding likely student benefits from AVE and likely penalties for non-implementation. Variation in fixed costs of implementation are captured by whether the school was in the treatment group in the AVE pilot. The idea is that, having previously implemented the program, any fixed costs of learning how to implement the program should already be sunk.[10]

Strikingly, principals who perceive the program to be beneficial to their students are no more likely to actually implement the program in the scale-up, either in the control group or in the full sample. Schools do not appear to select into program implementation based on (perceived) benefits, as one might hope in case of incomplete adoption. Nor does past experience with having implemented the program affect compliance.[11] In contrast, we do find suggestive evidence that principals with greater attentional bandwidth—those with a secretary and those who supervise only one school—are more likely to implement the program.

Experimental Results. Table 2 reports treatment effects on program adoption. Our prespecified primary outcome is whether the principal submitted a photo of students participating in the AVE program (column 1). Secondary outcomes include whether the principal downloaded the campaign material (column 2) and submitted the mandatory form (column 3). Column 4 reports a standardized adoption index combining these three measures.

Across all outcomes, we observe precise null effects for the evidence, incentive, and

[10]The program is a quite simple one—downloading and screening videos on a projector and facilitating a discussion with students after they watch the videos. Still, given limited technical capacity and a high workload, this may not have been a trivial cost for some schools.

[11]This result holds when restricting the sample to schools that were part of the pilot RCT. Thus, we find no causal effect of past implementation on implementation rates in the scale up (Table A.5). This contrasts with DellaVigna et al. (2024), who find that organizational inertia predicts adoption of nudge interventions in U.S. cities. One possible explanation for the difference is that temporary participation in a pilot may not be sufficient to establish persistent routines, or that the one-year gap between the initial experiment and the scale-up was enough to erode any institutional learning.

Table 2: Treatment Effects on AVE Campaign Adoption

	Index Components			
	Primary Outcome	Secondary Outcomes		
	Submitted Photo	Downloaded Material	Submitted Form	Adoption Index (in Std)
	(1)	(2)	(3)	(4)
Reminders	0.204***	0.151***	0.131***	0.411***
	(0.022)	(0.019)	(0.021)	(0.042)
Evidence	-0.013	-0.008	-0.030*	-0.043
	(0.019)	(0.018)	(0.018)	(0.039)
Incentive	0.014	-0.023	0.055**	0.031
	(0.024)	(0.024)	(0.023)	(0.051)
Assistance	-0.012	-0.034**	-0.020	-0.062*
	(0.018)	(0.017)	(0.017)	(0.036)
Mean of Control Group	0.374	0.687	0.271	0.000
Observations	2724	2724	2724	2724

Notes: This table shows the treatment effects on different adoption measures of the AVE campaign. We regress each outcome on indicator variables for assignments to the evidence treatment group, the incentive treatment group, the reminders treatment group and the assistance treatment group, stratification dummies, and baseline controls selected by LASSO. Table A.3 shows the list of potential lasso controls. The adoption index (column 4) is standardized to have mean zero and standard deviation one in the control group. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

assistance treatments. Sharing research findings on the program's effectiveness does not increase adoption, nor does offering a financial incentive through a lottery or providing implementation support. These null results are consistent with the descriptive patterns in Table 1. Principals who perceive the program as more beneficial are no more likely to implement it; nor does experimentally providing evidence on benefits to students increase implementation. Similarly, the null effect of offering technical assistance with implementation is consistent with previous experience having no effect on implementation in the nationwide scale up.

The only intervention that increased program adoption is the reminders treatment. Principals who received two additional follow-up calls showed a 20-percentage-point

increase in photo submission (column 1), a 54% increase relative to the control group mean. This effect is large and highly significant ($p < 0.001$), and holds across all outcome definitions. This finding remains highly statistically significant ($q = 0.001$) when controlling the False Discovery Rate using the method of Benjamini et al. (2006) (Table A.6). When breaking the results up by each treatment cell in the cross-randomized design, treatment effects for the primary outcome are similar and statistically significant across each cell which involves a reminder, while all other treatment cells remain non-significant (Table A.7).

Figure 2 plots implementation over time. The cumulative adoption curves for the evidence, incentive, assistance, and control groups are nearly indistinguishable throughout the entire sample period, reinforcing that the null effects are not a matter of delayed response. The reminders group, by contrast, shows persistently higher adoption, with the gap emerging early and widening through the deadline.[12]

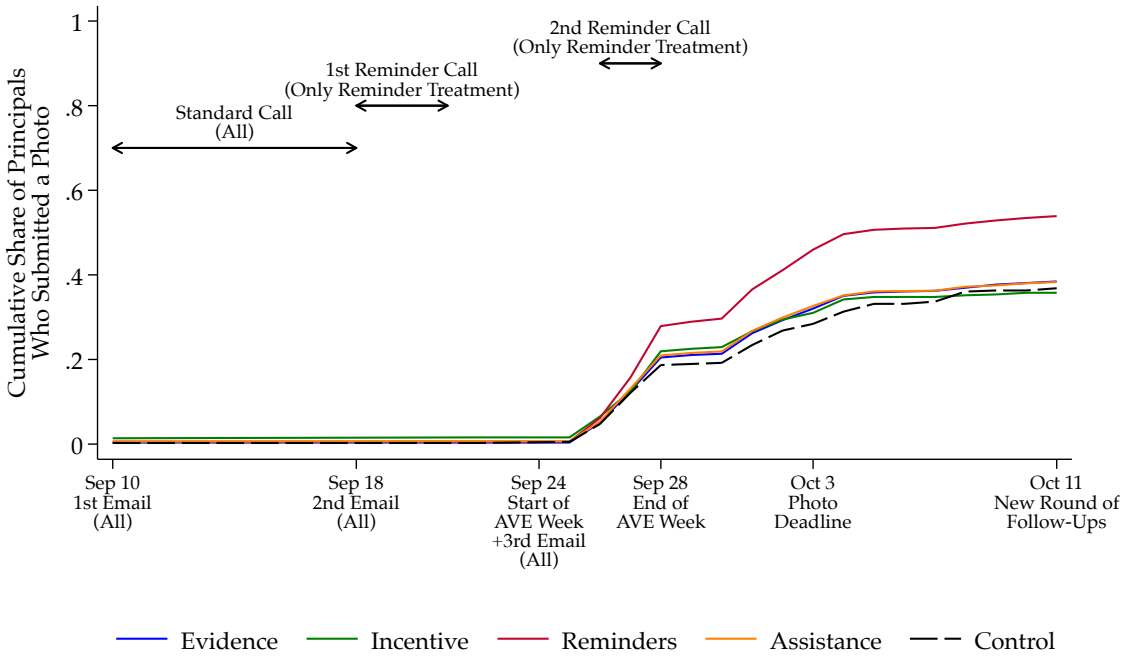
An important question at this stage is whether the reminder treatment truly affected implementation of AVE in schools or simply its *reporting* as required by the Ministry. Several pieces of evidence point to implementation actually being affected. First, the reminder treatment had similarly-sized effects on downloads of the AVE program videos, without which implementation is not possible. In addition, we collaborated with the Ministry of Education to include questions in the national exams—about a year after our experiment—asking students whether they or their peers participated in the AVE program. We find qualitatively similar results with statistically significant increases in recall of AVE videos among students from reminder-group schools (Table A.8). This provides further reassurance that reminders increased actual program implementation.[13]

In Table 3, we report results from a follow-up experiment in which the same principals were required to implement an anti-bullying campaign through a similar process. In this experiment, we varied whether principals received two reminder

[12]Table A.9 shows that the reminder effects persist when we extend the date to December.

[13]Note that the levels and treatment effects in the student reports are not comparable to our main implementation measures. Student reports were collected nearly a year later, and the Ministry made several additional follow-up emails and calls to principals who had not provided proof of implementation by the deadline upon learning our results. We would thus expect treatment effects to be attenuated a year later. In addition, some students might have forgotten the videos while others might have remembered the videos from the AVE pilot.

Figure 2: Take-Up Rate Over Time by Treatment



Notes: This figure shows the cumulative share of principals who submitted a photo of the implementation of the AVE campaign in their school over time. The blue line corresponds to the evidence treatment, the green line to the incentive treatment, the red line to the reminder treatment, the orange line to the assistance treatment, and the black dashed line to the control group.

calls, whether they were informed that their compliance would be monitored, and whether they received a motivational message (see Appendices D and E for additional details).[14] Reminders again produced large effects, increasing adoption by 24 percentage points ($p < 0.001$). The monitoring message increased take-up by 4.5 percentage points ($p < 0.01$). The motivational message emphasizing the importance of the anti-bullying campaign had no effect on adoption, mirroring the null effect of the evidence treatment in the AVE experiment. The pattern across both experiments is consistent: principals respond strongly to reminders of new tasks, while effects of interventions which pull more traditional levers—trying to increase motivation, provide assistance, or monitoring—have much smaller effects.

[14] In contrast to the AVE scale-up experiment, the control group in the second experiment only received one email and no calls.

Table 3: Treatment Effects on Anti-Bullying Campaign Adoption

	Index Components			
	Primary Outcome	Secondary Outcomes		
	Submitted Photo	Clicked on Declaration	Submitted Form	Adoption Index (in Std)
	(1)	(2)	(3)	(4)
Monitoring	0.045***	0.035**	0.062***	0.124***
	(0.017)	(0.017)	(0.017)	(0.036)
Motivation	-0.011	-0.018	-0.004	-0.029
	(0.017)	(0.017)	(0.017)	(0.036)
Reminders	0.241***	0.281***	0.178***	0.639***
	(0.017)	(0.018)	(0.018)	(0.038)
Mean of Control Group	0.182	0.379	0.215	0.000
Observations	2724	2724	2724	2724

Notes: This table shows the treatment effects on different adoption measures of the anti-bullying campaign. We regress each outcome on indicator variables for assignments to the motivation treatment group, the monitoring treatment group, and the reminders treatment group, stratification dummies, and baseline controls selected by LASSO. Table D.1 shows the list of potential lasso controls. The adoption index (column 4) is standardized to have mean zero and standard deviation one in the control group. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

Mechanisms. Recall that the reminder calls did not explicitly include any new information. They simply repeated information that principals had already received by email and via one previous phone call. Why then do the reminders increase implementation? We can think of two plausible mechanisms. First, reminders may simply bring to mind—at the right time—tasks which otherwise are forgotten, given the many responsibilities of schools principals. Second, reminders might signal to the principals how important the task is to the Ministry, thereby increasing their motivation to carry out the task.

While the 'signaling importance' mechanism may well play some role, we suspect that the simple attentional / limited memory mechanism is the primary one in our

setting. In the first experiment, the incentives and evidence treatments plausibly also signaled the ministry's enthusiasm for the program, and yet had no effect. In the second experiment, reminders had a much larger effect (24 pp vs. 4.5 pp, $p < 0.01$) than a monitoring treatment that urged principals to implement the anti-bullying program and explicitly told them they were being monitored by the Ministry.

Further, albeit suggestive, evidence is provided by the heterogeneity analysis reported in Table A.10. Consistent with the view that reminders operate by alleviating attention constraints, we find larger treatment effects of reminders among principals with more limited baseline bandwidth, using the variables defined in Table 1. We do not find differential effects for the other treatment arms.

6 Discussion and Conclusion

In some ways, our study context is an ideal scenario for evidence-based policy. A government ministry commissioned a large-scale RCT to evaluate an intervention which was itself based on previous research (Jensen, 2010). The intervention proved effective and the ministry adopted the policy nationwide (Berry et al., 2018). But scaling up an effective policy requires more than selecting the right intervention—it requires ensuring that frontline bureaucrats actually implement it. Our study shows that even after the Ministry of Education mandated a proven educational program, only 37% of control group schools verifiably implemented the program, a stark contrast to the 83% compliance achieved through intensive in-person engagement in the original trial. Neither sharing research evidence, offering financial incentives, nor providing implementation assistance increased adoption. In contrast, simple reminder calls produced large gains in implementation, a finding replicated in a second experiment with a different program. These results suggest that limited attention among frontline bureaucrats is a central barrier to scaling effective policies.

Our findings carry practical implications. The intensive in-person workshops used in the original trial achieved near-universal implementation but are costly and difficult to scale. A far cheaper alternative—additional phone calls that simply remind principals to act—can recover a meaningful share of the implementation gap. Reminders are not a substitute for well-designed programs or good policy selection, but

may be a low-cost tool to boost program implementation.

More broadly, our findings suggest that in settings where frontline bureaucrats manage many competing mandates, the traditional toolkit for improving performance—incentives, assistance, monitoring, and information provision—may be less effective than commonly assumed. Designing implementation protocols that account for the attentional demands facing these actors could be a useful complement to the growing emphasis on evidence-based policy selection.

Several limitations warrant discussion. First, further research in such settings is required to understand for which kinds of tasks reminders for frontline bureaucrats would be helpful, whether reminders have persistent effects, and when they should be phased out. It seems plausible that reminders are helpful for new tasks which require one-time action or delegation. For tasks that require sustained effort or repeated actions, in contrast, reminders might not be as effective. Second, if attention is limited, as we argue, then it is possible that the reminders we deployed diverted principals' attention from some other valuable task. Third, whether attentional barriers bind as strongly in other bureaucratic settings remains an open question.

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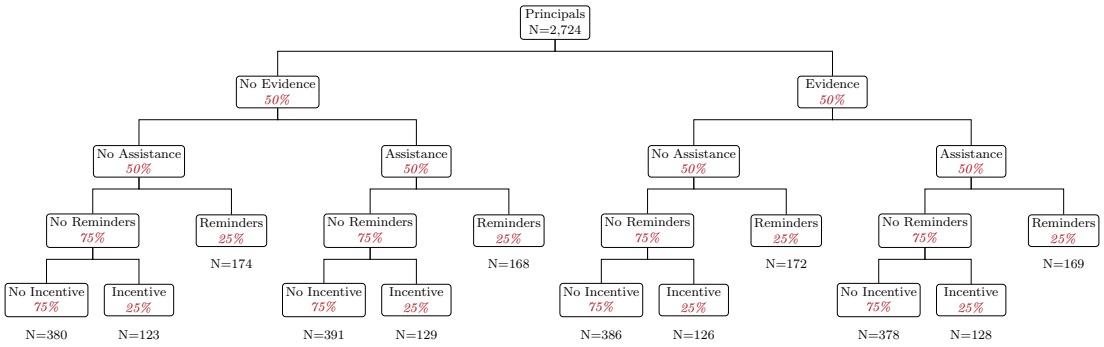
Online Appendix

A Implementation Details

Figure A.1: Experimental Design



Notes: This figure shows the experimental design. We cross-randomized schools into treatment cells based on interactions between the "Evidence", "Assistance", "Reminders", and "Incentive" treatments. The Incentive × Reminders interaction was excluded from the design due to budget constraints.

Table A.1: Sample Selection

Sample (1)	Description (2)
7,341	Has no students in grades 7-12 in 2018 ($n = 2,990$)
4,351	Has less than five students in grades 7-12 in 2017 ($n = 162$)
4,189	Did not participate in at least one standardized exam between 2016 and 2018 ($n = 301$)
3,888	No contact details ($n = 7$)
3,881	Randomly select one school to drop if principal supervises more than one ($n = 119$)
3,762	Not reached at baseline ($n = 626$)
3,136	Principal has no access to email ($n = 343$)
2,793	Randomly select one school to drop if principals share the same email address ($n = 16$)
2,777	Did not meet criteria 1-3 in the updated administrative data ($n = 9$)
2,768	Part of pilot survey ($n = 44$)
2,724	Final sample

Notes: This table shows the sample selection criteria. Column (1) shows the remaining number of schools and column (2) describes the selection criteria. Out of 7,341 schools in the Dominican Republic in total, 2,724 schools are included in our final sample.

Table A.2: Pre-Analysis Plan and Implemented Analysis

Table	Specified in PAP	Deviations
Table 2 - AVE Campaign Adoption		
(1) Submitted Photo	*"We will measure whether the principal uploads evidence of completion as required in the program instructions."*	None.
(2) Downloaded Material	*"The secondary outcomes - which are better thought of as intermediate steps rather than separate outcomes - are whether the principal clicked on the landing page in the email, and whether they downloaded the videos."*	We do not separately report results on whether the principals clicked on the landing page, as this closely mirrors the download rate.
(3) Submitted Form		Not explicitly mentioned in the first PAP, as we were still working out the reporting process at the time of submission
(3) Adoption Index		Not pre-specified.
Table 3 - Anti-Bullying Campaign Adoption		
(1) Submitted Photo	*'Principals were required to send pictures of students performing the task to a phone number of the Ministry."*	None.
(2) Clicked on Declaration	*"The secondary outcomes - which are better thought of as intermediate steps rather than separate outcomes - are whether the principal clicked on the email attachment containing the instructions of the task."*	None.
(3) Submitted Form	*"We will measure whether the principal uploads evidence of completion as required in the program instructions."*	The PAP lists it as a primary outcome but we use it as a secondary outcome to be consistent with Table 2.
(3) Adoption Index		Not pre-specified.
Additional Analysis		
Cutoff date	An extension to the first PAP reports the treatment effects as of October 11 and notes that we will report additional results separately after extending the deadline and conducting another round of outreach to principals.	None.
Student reports	The first PAP explicitly lists under primary outcomes that *'we will attempt to acquire independent validation of this through student surveys, although the feasibility of doing so is not clear at the time of this registration*".	While we list student reports as primary outcomes in the PAP, we report these results separately in Table A.8 because the outcomes were collected with a substantial delay and only after additional follow-ups with principals.
Multiple hypothesis test corrections		Not pre-specified.
Spillover effects	*"High evidence share vs. low evidence share."*	We do not examine spillovers since we find no direct effects of the evidence treatment.

Notes: The project was pre-registered under AEA registry ID AEARCTR-0003324 and AEARCTR-0003621.

Table A.3: Balance Check

	Control		Evidence		Incentive		Reminders		Assistance		
	Mean (1)	St. D. (2)	Coeff. (3)	St. E. (4)	Coeff. (5)	St. E. (6)	Coeff. (7)	St. E. (8)	Coeff. (9)	St. E. (10)	N (11)
Panel A: School Characteristics											
Has Internet Connection	0.720	[0.449]	-0.010	(0.017)	-0.014	(0.021)	-0.024	(0.019)	-0.018	(0.016)	2,717
Has Projector	0.769	[0.422]	-0.003	(0.017)	0.003	(0.021)	0.006	(0.019)	0.010	(0.016)	2,719
Has Computer Lab	0.562	[0.496]	-0.004	(0.019)	-0.007	(0.024)	0.020	(0.022)	-0.007	(0.018)	2,717
Has Electricity	0.987	[0.113]	0.000	(0.005)	0.000	(0.006)	0.004	(0.005)	0.000	(0.004)	2,719
Students in 7-12th Grade, 2018	223.887	[234.501]	-14.470	(8.948)	-0.047	(11.652)	-0.725	(9.660)	-7.388	(8.345)	2,724
Total PN Score 12th Grade, 2018	73.352	[7.269]	0.044	(0.424)	-0.599	(0.449)	-0.859*	(0.470)	0.090	(0.378)	1,324
Total PN Score 8th Grade, 2016	69.009	[7.363]	0.147	(0.331)	-0.491	(0.392)	-0.107	(0.369)	0.074	(0.301)	1,925
Shares Building with Another Sample School	0.100	[0.300]	-0.002	(0.012)	0.016	(0.016)	0.001	(0.013)	-0.018	(0.011)	2,724
Offers Extended School Day	0.494	[0.500]	-0.022	(0.018)	0.014	(0.022)	-0.004	(0.020)	-0.009	(0.017)	2,724
Located in Urban Area	0.725	[0.447]	-0.020	(0.017)	0.009	(0.022)	-0.026	(0.019)	0.013	(0.016)	2,358
School Is Public	0.763	[0.426]	-0.001	(0.016)	0.010	(0.020)	0.018	(0.018)	-0.023	(0.015)	2,724
Treatment Group in AVE Pilot	0.307	[0.462]	0.006	(0.019)	0.014	(0.024)	0.011	(0.021)	-0.013	(0.018)	2,724
Panel B: Principal Characteristics											
Experience of Principal	19.571	[7.589]	-0.951***	(0.356)	1.000**	(0.458)	0.438	(0.409)	-0.200	(0.336)	1,993
Principal Has Post-Graduate Degree	0.550	[0.498]	-0.010	(0.023)	-0.009	(0.030)	-0.045*	(0.026)	-0.008	(0.022)	1,955
Principal Supervises Multiple Schools	0.087	[0.282]	0.012	(0.012)	-0.011	(0.014)	0.009	(0.014)	-0.013	(0.011)	2,710
Principal Checks Emails Daily	0.593	[0.492]	0.006	(0.020)	-0.039	(0.025)	-0.049**	(0.023)	0.001	(0.019)	2,630
Believes Penalty after Non-Compliance is Very Likely	0.540	[0.499]	-0.006	(0.020)	0.034	(0.025)	0.005	(0.023)	0.014	(0.019)	2,723
Principal's Main Motivation is Professional Success	0.424	[0.495]	-0.003	(0.020)	-0.035	(0.025)	0.023	(0.023)	-0.001	(0.019)	2,723
Believes Extra Info Would Have An Effect	0.850	[0.358]	-0.015	(0.014)	-0.018	(0.019)	0.018	(0.015)	0.006	(0.013)	2,724
Believes Students Undervalue Education	0.499	[0.500]	-0.002	(0.021)	0.039	(0.026)	0.030	(0.024)	-0.008	(0.020)	2,630
Number of Valid Emails	1.349	[0.679]	0.029	(0.027)	-0.038	(0.033)	-0.013	(0.031)	-0.011	(0.025)	2,724
Joint Test p-value			0.56		0.38		0.39		0.93		

Notes: This table shows balance for baseline covariates. Panel A reports on school-level outcomes and Panel B on principal-level outcomes. Differences in sample sizes across variables reflect missing data. Column (1) reports the control group mean of the dependent variable. Columns (3), (5), and (7) report the difference in the dependent variable from OLS regressions of each outcome on indicator variables for assignments to the evidence treatment group, the incentive treatment group, the reminders treatment group and the assistance treatment group, and stratification dummies.

Table A.4: Intermediate Implementation Outcomes

	1st Email		2nd Email		3rd Email				
	Sent	Opened	Sent	Opened	Sent	Opened	Standard Call	1st Reminder Call	2nd Reminder Call
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Evidence	-0.000	0.030*	-0.008*	0.032*	-0.001	0.027	-0.017*	0.004	-0.011*
	(0.006)	(0.016)	(0.005)	(0.018)	(0.006)	(0.018)	(0.009)	(0.005)	(0.007)
Incentive	-0.003	-0.019	-0.004	0.005	-0.003	-0.041*	-0.008	0.001	0.002
	(0.007)	(0.021)	(0.006)	(0.023)	(0.007)	(0.023)	(0.012)	(0.001)	(0.002)
Reminders	-0.013*	0.007	-0.009	0.064***	-0.001	0.010	0.004	0.942***	0.877***
	(0.007)	(0.018)	(0.006)	(0.019)	(0.006)	(0.020)	(0.010)	(0.009)	(0.012)
Assistance	0.001	-0.023	0.004	-0.023	0.002	-0.044**	0.003	0.002	0.002
	(0.006)	(0.015)	(0.004)	(0.017)	(0.005)	(0.017)	(0.009)	(0.004)	(0.006)
Sample	Full	Full	Full	Full	Full	Full	Full	Full	Full
Mean of Control Group	0.982	0.750	0.995	0.687	0.982	0.692	0.955	0.000	0.000
Observations	2724	2724	2724	2724	2724	2724	2724	2724	2724

Notes: This table shows differences in implementation outcomes by treatment arm. We regress each outcome on indicator variables for assignments to the evidence treatment group, the incentive treatment group, the reminders treatment group and the assistance treatment group, stratification dummies, and baseline controls selected by LASSO. Table A.3 shows the list of potential lasso controls.

Table A.5: Effect of AVE Pilot Assignment on AVE Campaign Adoption

	Submitted Photo (1)	Downloaded Material (2)	Submitted Form (3)	Adoption Index (4)
Treatment Group in AVE Pilot	-0.027	0.010	-0.011	-0.016
	(0.028)	(0.027)	(0.026)	(0.056)
Outcome Mean	0.437	0.647	0.304	0.079
Observations	1351	1351	1351	1351

Notes: This table shows the effect of having been assigned to the treatment group in the AVE pilot on AVE campaign adoption during the scale-up. We regress each outcome on an indicator for assignment to the treatment group in the AVE pilot and stratification dummies. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

Table A.6: Multiple Hypothesis Correction

	Index Components			
	Primary Outcome	Secondary Outcomes		
	Submitted Photo	Downloaded Material	Submitted Form	Adoption Index (in Std)
	(1)	(2)	(3)	(4)
Reminders	0.001	0.001	0.001	0.001
Evidence	0.705	0.480	0.069	0.220
Incentive	0.705	0.299	0.024	0.378
Assistance	0.705	0.069	0.134	0.157

Notes: This table reports sharpened false discovery rate (FDR) q-values corresponding to the estimates in Table 2. Q-values are computed following Benjamini et al. (2006) and control the expected proportion of false discoveries among all rejected hypotheses. For each outcome column, the correction is applied across the family of four hypotheses corresponding to the four treatment arms.

Table A.7: Effects of All Treatment Cells on AVE Campaign Adoption

	Index Components			
	Primary Outcome	Secondary Outcomes		
	Submitted Photo	Downloaded Material	Submitted Form	Adoption Index (in Std)
	(1)	(2)	(3)	(4)
Evidence	-0.001	-0.014	-0.033	-0.033
	(0.034)	(0.033)	(0.032)	(0.070)
Incentive	-0.029	-0.036	0.051	-0.012
	(0.047)	(0.048)	(0.047)	(0.104)
Reminders	0.194***	0.162***	0.145***	0.445***
	(0.044)	(0.037)	(0.043)	(0.086)
Assistance	0.004	-0.010	0.001	0.002
	(0.034)	(0.032)	(0.031)	(0.070)
Evidence + Incentive	0.031	-0.040	0.076	0.059
	(0.048)	(0.048)	(0.047)	(0.105)
Evidence + Reminders	0.200***	0.145***	0.104**	0.376***
	(0.045)	(0.037)	(0.042)	(0.086)
Evidence + Assistance	-0.046	-0.072**	-0.047	-0.142**
	(0.034)	(0.034)	(0.031)	(0.071)
Incentive + Assistance	0.074	-0.090*	0.037	-0.007
	(0.050)	(0.049)	(0.046)	(0.108)
Reminders + Assistance	0.158***	0.065*	0.094**	0.263***
	(0.045)	(0.039)	(0.042)	(0.086)
Evidence + Incentive + Assistance	-0.061	-0.018	-0.022	-0.082
	(0.047)	(0.047)	(0.043)	(0.098)
Evidence + Reminders + Assistance	0.222***	0.138***	0.102**	0.391***
	(0.045)	(0.038)	(0.043)	(0.087)
Mean of Control Group	0.374	0.687	0.271	0.000
Observations	2724	2724	2724	2724

Notes: This table shows the treatment effects on different adoption measures of the AVE campaign. We regress each outcome on indicator variables for assignments to each treatment cell, stratification dummies, and baseline controls selected by LASSO. Table A.3 shows the list of potential lasso controls. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

Table A.8: Effects on Student Recall of AVE Videos a Year Later

	Student Share (1)	Student Share $\geq$ 50% (2)
Reminders	0.033**	0.057**
	(0.013)	(0.027)
Evidence	0.015	0.003
	(0.011)	(0.024)
Incentive	-0.034**	-0.077***
	(0.014)	(0.029)
Assistance	-0.017	-0.016
	(0.011)	(0.022)
Mean of Control Group	0.538	0.617
Observations	1588	1588

Notes: This table reports the effects of the AVE campaign adoption based on student reports. In Column (1), the outcome is the share of students who report either participating in the AVE campaign themselves or observing participation by other students in their school. In Column (2), the outcome is an indicator equal to one if this share is at least 50%. We regress each outcome on indicator variables for assignments to the evidence treatment group, the incentive treatment group, the reminders treatment group and the assistance treatment group, stratification dummies, and baseline controls selected by LASSO. Table A.3 shows the list of potential lasso controls. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

Table A.9: Treatment Effects on AVE Campaign Adoption by December 1

	Index Components			
	Primary Outcome	Secondary Outcomes		
	Submitted Photo	Downloaded Material	Submitted Form	Adoption Index (in Std)
	(1)	(2)	(3)	(4)
Reminders	0.165***	0.100***	0.120***	0.309***
	(0.021)	(0.017)	(0.022)	(0.041)
Evidence	0.009	-0.018	-0.012	-0.027
	(0.019)	(0.017)	(0.019)	(0.039)
Incentive	-0.033	-0.043*	-0.003	-0.071
	(0.024)	(0.023)	(0.024)	(0.051)
Assistance	-0.027	-0.032**	-0.026	-0.073**
	(0.018)	(0.016)	(0.018)	(0.036)
Mean of Control Group	0.553	0.763	0.450	-0.000
Observations	2724	2724	2724	2724

Notes: This table shows the treatment effects on different adoption measures of the AVE campaign. We regress each outcome on indicator variables for assignments to the evidence treatment group, the incentive treatment group, the reminders treatment group and the assistance treatment group, stratification dummies, and baseline controls selected by LASSO. Table A.3 shows the list of potential lasso controls. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

Table A.10: Heterogeneity Analysis

	Primary Outcome: Submitted Photo		
Heterogeneity Variable:	Limited Bandwidth Index	Benefits Index	Implementation Costs
	(1)	(2)	(3)
Evidence	-0.004	-0.012	-0.023
	[0.022]	[0.019]	[0.023]
Incentive	0.013	0.014	0.012
	[0.027]	[0.024]	[0.029]
Reminders	0.197***	0.203***	0.185***
	[0.025]	[0.022]	[0.027]
Assistance	-0.004	-0.013	-0.006
	[0.021]	[0.018]	[0.022]
Evidence × Heterogeneity Var.	-0.008	-0.000	0.034
	[0.022]	[0.020]	[0.040]
Incentive × Heterogeneity Var.	-0.012	0.002	0.007
	[0.029]	[0.026]	[0.052]
Reminders × Heterogeneity Var.	0.046*	0.001	0.061
	[0.028]	[0.025]	[0.048]
Assistance × Heterogeneity Var.	-0.000	0.005	-0.020
	[0.022]	[0.020]	[0.039]
Heterogeneity Var.	-0.047**	0.007	-0.050
	[0.020]	[0.018]	[0.039]
Mean of Control Group	0.390	0.374	0.374
Mean of Heterogeneity Var.	-0.025	0.090	0.323
Observations	2085	2724	2724

Notes: This table shows heterogeneous treatment effects on whether the principal submitted a photo of the implementation of the AVE campaign in their school. We regress the outcome on treatment indicators, the relevant index, their interactions, stratification dummies, and baseline controls selected by LASSO. Table A.3 shows the list of potential lasso controls. Appendix Table A.11 reports the results for each index component. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

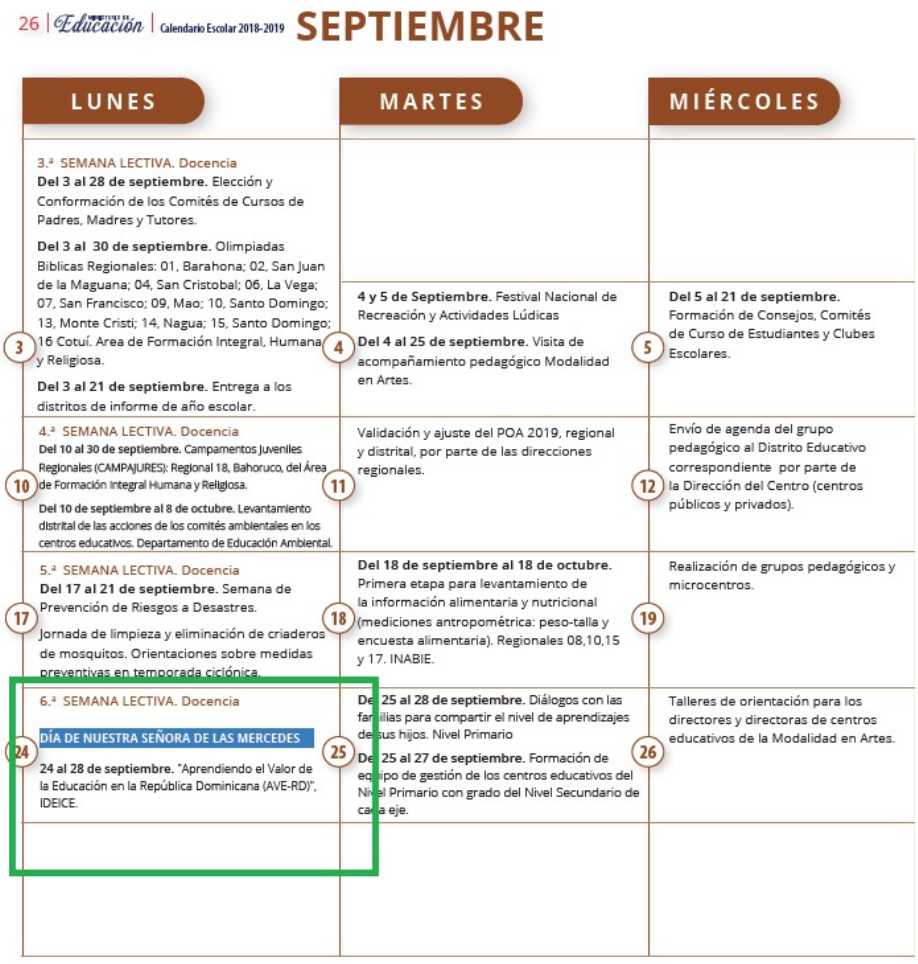
Table A.11: Heterogeneity Analysis by Index Components

	Primary Outcome: Submitted Photo			
	Limited Bandwidth Index Components		Benefits Index Components	
Heterogeneity Variable:	Principal Has Multiple Positions	Principal Has No Secretary	Believes Info Would Have Effect	Believes Penalty is Very Likely
	(1)	(2)	(3)	(4)
Evidence	0.012	-0.002	-0.003	-0.016
	[0.038]	[0.030]	[0.050]	[0.028]
Incentive	-0.005	0.061	0.008	0.012
	[0.050]	[0.038]	[0.061]	[0.036]
Reminders	0.108**	0.210***	0.177***	0.216***
	[0.046]	[0.034]	[0.063]	[0.033]
Assistance	0.010	-0.048*	-0.030	-0.013
	[0.037]	[0.028]	[0.049]	[0.027]
Evidence × Heterogeneity Var.	-0.015	-0.027	-0.010	0.006
	[0.046]	[0.057]	[0.053]	[0.037]
Incentive × Heterogeneity Var.	0.029	-0.062	0.006	0.001
	[0.061]	[0.077]	[0.067]	[0.049]
Reminders × Heterogeneity Var.	0.149***	-0.029	0.031	-0.022
	[0.056]	[0.069]	[0.068]	[0.045]
Assistance × Heterogeneity Var.	-0.032	0.080	0.021	0.001
	[0.046]	[0.056]	[0.053]	[0.037]
Heterogeneity Var.	-0.065	-0.129**	-0.009	0.026
	[0.044]	[0.052]	[0.049]	[0.036]
Mean of Control Group	0.391	0.372	0.374	0.374
Mean of Heterogeneity Var.	0.663	0.250	0.862	0.553
Observations	1994	1526	2724	2723

Notes: This table shows heterogeneous treatment effects on whether the principal uploaded a photo of the implementation of the AVE campaign in their school for each index component. We regress the outcome on indicator variables for assignments to the evidence treatment group, the incentive treatment group, the reminders treatment group, the assistance treatment group, the heterogeneity dummy, interactions between the treatment group variables and the heterogeneity dummy, stratification dummies, and baseline controls selected by LASSO. Table A.3 shows the list of potential lasso controls. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

B Additional Material for AVE Campaign

Figure B.1: Official Academic Calendar



Notes: This figure shows the official academic calendar for the 2018-2019 school year in the Dominican Republic. The calendar states that the AVE campaign should be implemented between September 24 and 28.

Figure B.2: Intervention Brochure for Control Group

Notes: This figure shows the brochure that was attached to the first email for the control group.

Figure B.3: Intervention Brochure for Evidence Treatment Group

Notes: This figure shows the brochure that was attached to the first email for the evidence group.

C Email and Call Scripts for the AVE Campaign

Email Script (Control Group)

Greetings.

As you are aware, the week of September 25 to 28 marks the "Learning the Value of Education in the Dominican Republic" (AVE-RD) week, as outlined in the 2018-2019 MINERD school calendar.[15] AVE-RD is an informational campaign being promoted by MINERD through the Directorate of Guidance and Psychology, in collaboration with IDEICE.

The campaign consists of four videos in a telenovela format, with a total running time of one hour; these videos must be screened for students in grades 1 through 6 of secondary education at all educational centers throughout the country. The videos focus on informing students about the economic, social, and personal benefits of education in an engaging and relatable manner.

On the webpage XXX, you may download the videos and other materials that will guide you—along with your management team and Guidance and Psychology staff—through the steps required to fulfill this mandate.

Additionally, this email includes a set of instructions for downloading the videos and manuals, as well as an informational brochure regarding this campaign. Please download both documents by clicking on the download buttons labeled "Descargar Folleto" (Download Brochure) and "Descargar Instructivo" (Download Instructions), and review them carefully.

We appreciate your collaboration and are confident that you will successfully carry out this campaign. Please reply to this email to confirm receipt of this message.

Director of Guidance and Psychology, MINERD
Executive Director, IDEICE

[15] While the official academic calendar lists September 24–28 as the AVE week, the emails and phone calls refer to September 25–28, since September 24 was a religious holiday.

1st Call (Full Sample)

Everyone:

Hello, my name is XXX and I am calling on behalf of the Ministry of Education to give you an important message. Do you have a couple of minutes?

Are you currently in charge of the school XXX with Center Code XXX?

We are calling to remind you about the delivery of an email regarding the informational campaign Learning the Value of Education in the Dominican Republic (AVE-RD) on behalf of MINERD and IDEICE.

I first need to verify some of your information and information about your school. Are you XXX?

This email was sent by the Ministry of Education and IDEICE on behalf of Minerva Perez, Director of the Directorate of Guidance and Psychology, on Sunday, September 9, and contains relevant information so that your school can implement the campaign.

I wanted to notify you that we sent the email to the following accounts: XXX

Have you been able to see this email?

As mentioned, this campaign consists of a series of four videos in telenovela format, which you must show to secondary school students in your school between September 25 and September 28, as established in the MINERD 2018-2019 school calendar.

These videos provide information about the economic, social, and personal benefits of finishing school.

Evidence Treatment Arm:

We would like to tell you that a study conducted in 2,500 schools in the Dominican Republic showed that there are proven benefits of implementing this campaign in your educational center.

Schools that showed these videos significantly improved their national exam scores by 0.6 points in total, and reduced their school dropout rates.

This study concludes that if all schools in the country showed these videos, the number of secondary school dropouts would decrease by 6,500 students per year.

Would you like me to repeat the information?

Incentive Treatment Arm:

If you implement the AVE-RD campaign in your school, you will have the opportunity to win a 7-inch Samsung tablet to help you manage your educational center. One out of every 10 people who participate in the raffle will win a tablet.

You will be able to participate in this raffle only if you implement the campaign in your school as established in the instructions in the email we sent you.

Would you like me to repeat the information?

Assistance Treatment Arm:

We would like to mention that a support center is available should you need assistance or technical support. You can contact them by email at ideice@minerd.gob.do or by phone at 809-688-9700, extensions 7961 and 7962, Monday through Friday, from 9:00 a.m. to 4:00 p.m.

Would you like me to repeat the email address or phone number?

Everyone:

In the same email, you must download a Brochure and an Instruction Guide, which appear as download buttons at the end of the email. It is very important that you download both files and review them carefully.

Note that both download buttons are at the end of the email.

We appreciate your cooperation and trust that the implementation of this program will be a success thanks to your efforts.

2nd Call (Only Reminders Group)

Everyone:

Hello, my name is XXX and I am calling on behalf of the Ministry of Education to give you an important message. Do you have a couple of minutes?

Are you currently in charge of the school XXX with Center Code XXX?

We are calling to remind you about the delivery of an email regarding the informational campaign Learning the Value of Education in the Dominican Republic (AVE-RD) on behalf of MINERD and IDEICE.

I first need to verify some of your information and information about your school. Are you XXX?

This email titled "School Calendar 2018-2019: AVE-RD Week" contains relevant information so that your school can implement the campaign.

This campaign consists of videos that provide information about the benefits of finishing school. These videos should be shown to secondary school students in your school between 25 and 28 September, as established in MINERD's 2018-2019 school calendar.

If you would like, we can resend this email to you. Would you like me to resend it?

Evidence Treatment Arm:

We would like to tell you that a study conducted in 2,500 schools in the Dominican Republic showed that there are proven benefits of implementing this campaign in your educational center.

Schools that showed these videos significantly improved their national exam scores by 0.6 points in total, and reduced their school dropout rates.

This study concludes that if all schools in the country showed these videos, the number of secondary school dropouts would decrease by 6,500 students per year.

Would you like me to repeat the information?

Incentive Treatment Arm:

If you implement the AVE-RD campaign in your school, you will have the opportunity to win a 7-inch Samsung tablet to help you manage your educational center. One out of every 10 people who participate in the raffle will win a tablet.

You will be able to participate in this raffle only if you implement the campaign in your school as established in the instructions in the email we sent you.

Would you like me to repeat the information?

Assistance Treatment Arm:

We would like to mention that a support center is available should you need assistance or technical support. You can contact them by email at ideice@minerd.gob.do or by phone at 809-688-9700, extensions 7961 and 7962, Monday through Friday, from 9:00 a.m. to 4:00 p.m.

Would you like me to repeat the email address or phone number?

Everyone:

In the same email, you must download a Brochure and an Instruction Guide, which appear as download buttons at the end of the email. It is very important that you download both files and review them carefully.

Note that both download buttons are at the end of the email.

We appreciate your cooperation and trust that the implementation of this program will be a success thanks to your efforts.

3rd Call (Only Reminders Group)

Hello, my name is XXX and I am calling on behalf of the Ministry of Education to give you an important message. Do you have a couple of minutes?

Are you currently in charge of the school XXX with Center Code XXX?

We are calling to remind you about the delivery of several emails regarding the

informational campaign Learning the Value of Education in the Dominican Republic (AVE-RD) on behalf of MINERD and IDEICE.

I first need to verify some of your information and information about your school. Are you XXX?

These emails contain relevant information so that your school can implement the campaign. This campaign consists of videos that provide information about the benefits of finishing school. These videos should be shown to secondary school students in your school starting on 25 September, as established in the MINERD 2018-2019 school calendar.

If you would like, we can resend this email to you. Would you like me to resend it? Please remember that you must send a WhatsApp message to the number XXX with photos of the video showings.

You must also complete the implementation form and send it to XXX. This form will be downloaded together with the videos.

We appreciate your cooperation and trust that the implementation of this program will be a success thanks to your efforts.

D Additional Results for Anti-Bullying Campaign

Here is the content of the messages we randomized in the follow-up experiment:

- Motivation
  - All principals: *The school climate is an important factor that promotes learning. The school climate is something that can be managed, and the "Declaration for Good Treatment" can contribute to this end. Be part of the solution and let's help together to reduce bullying and bad treatment at school.*
- Monitoring
  - For principals who implemented the AVE campaign: *According to our records, your school successfully implemented the Learning the Value of Education campaign (AVE-RD). We thank you for your participation, and we urge you to also participate in the "Declaration for Good Treatment". Again we will be monitoring compliance with this instruction, to ensure that this campaign is implemented properly.*
  - For principals who did not implement the AVE campaign: *According to our records, your school did not satisfactorily implement the previous campaign Learning the Value of Education (AVE-RD). We urge you to participate in the "Declaration for Good Treatment" on this occasion. Again we will be monitoring compliance with this instruction, to ensure that this campaign is implemented properly.*

A third intervention arm received additional reminders. While other principals were only contacted via email, the reminders treatment group received two additional calls from a call center. All three intervention arms were perfectly cross-randomized. Randomization was stratified based on whether the school was part of the evidence, incentive, reminders, or assistance treatment arm in the AVE intervention and based on whether the school had implemented the AVE campaign.

Table D.1: Balance Check for Anti-Bullying Campaign

	Control		Motivation		Monitoring		Reminders		
	Mean	St. D.	Coeff.	St. E.	Coeff.	St. E.	Coeff.	St. E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School Characteristics									
Has Internet Connection	0.738	[0.440]	-0.004	(0.017)	0.013	(0.017)	0.000	(0.017)	2,717
Has Projector	0.753	[0.432]	0.014	(0.016)	-0.001	(0.016)	-0.007	(0.016)	2,719
Has Computer Lab	0.558	[0.497]	0.008	(0.019)	-0.005	(0.019)	0.014	(0.019)	2,717
Has Electricity	0.994	[0.077]	-0.007*	(0.004)	0.006	(0.004)	-0.006	(0.004)	2,719
Students in 7-12th Grade, 2018	188.632	[186.571]	-8.408	(8.458)	8.693	(8.472)	-7.335	(8.466)	2,724
Total PN Score 12th Grade, 2018	72.931	[7.263]	0.722*	(0.396)	-0.502	(0.393)	0.340	(0.396)	1,324
Total PN Score 8th Grade, 2016	69.418	[7.422]	-0.105	(0.337)	-0.098	(0.336)	0.183	(0.335)	1,925
Shares Building with Another Sample School	0.079	[0.271]	0.012	(0.011)	0.005	(0.011)	0.002	(0.011)	2,724
Offers Extended School Day	0.409	[0.492]	0.012	(0.019)	0.026	(0.019)	0.015	(0.019)	2,724
Located in Urban Area	0.737	[0.441]	-0.018	(0.019)	-0.013	(0.019)	-0.008	(0.019)	2,358
School Is Public	0.732	[0.443]	0.012	(0.016)	-0.013	(0.016)	-0.003	(0.016)	2,724
Treatment Group in AVE Pilot	0.332	[0.472]	0.026	(0.018)	-0.022	(0.018)	-0.014	(0.018)	2,724
Panel B: Principal Characteristics									
Experience of Principal	19.183	[8.045]	0.295	(0.333)	0.063	(0.334)	-0.471	(0.334)	2,111
Principal Has Post-Graduate Degree	0.550	[0.498]	-0.019	(0.020)	-0.017	(0.020)	-0.010	(0.020)	2,568
Principal Supervises Multiple Schools	0.083	[0.276]	-0.007	(0.011)	0.007	(0.011)	0.011	(0.011)	2,710
Principal Checks Emails Daily	0.592	[0.492]	0.023	(0.019)	-0.016	(0.019)	0.005	(0.019)	2,630
Believes Penalty after Non-Compliance is Very Likely	0.560	[0.497]	-0.002	(0.019)	0.028	(0.019)	0.003	(0.019)	2,723
Principal's Main Motivation is Professional Success	0.447	[0.498]	-0.007	(0.019)	0.022	(0.019)	-0.011	(0.019)	2,723
Believes Extra Info Would Have An Effect	0.868	[0.339]	-0.012	(0.013)	-0.010	(0.013)	0.013	(0.013)	2,724
Believes Students Undervalue Education	0.531	[0.500]	-0.023	(0.019)	-0.023	(0.019)	0.019	(0.019)	2,630
Number of Valid Emails	1.347	[0.698]	-0.004	(0.026)	-0.025	(0.026)	0.039	(0.026)	2,724
Joint Test p-value			0.31		0.22		0.78		

Notes: This table shows balance for baseline covariates. Panel A reports on school-level outcomes and Panel B on principal-level outcomes. Differences in sample sizes across variables reflect missing data. Column (1) reports the control group mean of the dependent variable. Columns (3), (5), and (7) report the difference in the dependent variable from OLS regressions of each outcome on indicator variables for assignments to the motivation treatment group, the monitoring treatment group, and the reminders treatment group, and stratification dummies.

Table D.2: Multiple Hypothesis Correction for Anti-Bullying Campaign

	Index Components			
	Primary Outcome	Secondary Outcomes		
	Submitted Photo	Clicked on Declaration	Submitted Form	Adoption Index (in Std)
	(1)	(2)	(3)	(4)
Monitoring	0.009	0.040	0.001	0.001
Motivation	0.207	0.105	0.381	0.162
Reminders	0.001	0.001	0.001	0.001

Notes: This table reports sharpened false discovery rate (FDR) q-values corresponding to the estimates in Table 3. Q-values are computed following Benjamini et al. (2006) and control the expected proportion of false discoveries among all rejected hypotheses. For each outcome column, the correction is applied across the family of three hypotheses corresponding to the three treatment arms.

Table D.3: Effects on Anti-Bullying Campaign Adoption by Treatment Cell

	Index Components			
	Primary Outcome	Secondary Outcomes		
	Submitted Photo	Clicked on Declaration	Submitted Form	Adoption Index (in Std)
	(1)	(2)	(3)	(4)
Motivation	0.026	-0.030	-0.003	-0.021
	(0.029)	(0.035)	(0.030)	(0.071)
Monitoring	0.043	0.065*	0.054*	0.151**
	(0.030)	(0.036)	(0.032)	(0.075)
Reminders	0.318***	0.342***	0.190***	0.770***
	(0.033)	(0.033)	(0.033)	(0.069)
Motivation + Monitoring	0.100***	0.071**	0.063**	0.205***
	(0.031)	(0.035)	(0.031)	(0.074)
Motivation + Reminders	0.217***	0.290***	0.158***	0.623***
	(0.032)	(0.035)	(0.033)	(0.070)
Monitoring + Reminders	0.310***	0.302***	0.231***	0.755***
	(0.033)	(0.034)	(0.034)	(0.073)
Monitoring + Motivation + Reminders	0.286***	0.307***	0.244***	0.756***
	(0.033)	(0.034)	(0.034)	(0.073)
Mean of Control Group	0.182	0.379	0.215	0.000
Observations	2724	2724	2724	2724

Notes: This table shows the treatment effects on different adoption measures of the anti-bullying campaign. We regress each outcome on indicator variables each treatment cell, stratification dummies, and baseline controls selected by LASSO. Table D.1 shows the list of potential lasso controls. The adoption index (column 4) is standardized to have mean zero and standard deviation one in the control group. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

Table D.4: Effects on Student Reports for Anti-Bullying Experiment

	Student Share (1)	Student Share $\geq$ 50% (2)
Motivation	-0.013	-0.028
	(0.012)	(0.022)
Monitoring	0.015	0.019
	(0.012)	(0.022)
Reminders	0.084***	0.118***
	(0.012)	(0.023)
Mean of Control Group	0.526	0.545
Observations	1588	1588

Notes: This table reports effects on anti-bullying campaign adoption based on student reports. In Column (1), the outcome is the share of students who report either participating in the anti-bullying campaign themselves or observing participation by other students in their school. In Column (2), the outcome is an indicator equal to one if this share is at least 50%. We regress each outcome on indicator variables for assignments to the motivation treatment group, the monitoring treatment group, and the reminders treatment group, stratification dummies, and baseline controls selected by LASSO. Table D.1 shows the list of potential lasso controls. All regressions are estimated by OLS and robust standard errors are reported in parentheses.

E Call Scripts for the Anti-Bullying Campaign

1st and 2nd Reminders Call

Everyone:

Hello, my name is XXX and I am calling on behalf of the Ministry of Education to give you an important message. Do you have a couple of minutes? Are you currently in charge of the school XXX with Center Code XXX?

We are calling to notify you about the delivery of an email regarding the Culture of Peace and Declaration of Good Treatment campaign sent on November 8. I first need to verify some of your information and information about your school. Are you XXX?

This email was sent by the Ministry of Education and IDEICE on behalf of Minerva Perez, Director of the Directorate of Guidance and Psychology, on Thursday, November 8, and contains relevant information so that your school can implement the campaign.

I wanted to notify you that we sent the email to the following accounts: XXX

Have you been able to see this email?

The MINERD 2018-2019 school calendar establishes the month of November as the month of good treatment in schools. For this reason, MINERD, through the Directorate of Guidance and Psychology, is promoting the Culture of Peace and the Declaration of Good Treatment campaign. This campaign consists of all classrooms collaboratively creating a mural or bulletin board related to good treatment on November 23, and reading and signing the "Declaration for Good Treatment."

Monitoring - Implemented AVE Campaign:

According to our records, your educational center successfully implemented the Learning the Value of Education (AVE-RD) campaign.

We appreciate your participation, and we encourage you to also participate in the "Declaration for Good Treatment." MINERD and IDEICE will again monitor com-

pliance with this instruction to ensure that this campaign is properly implemented.

Monitoring - Did Not Implement AVE Campaign:

According to our records, your educational center did not successfully implement the previous campaign Learning the Value of Education (AVE-RD).

We encourage you to participate in the "Declaration for Good Treatment" this time. MINERD and IDEICE will again monitor compliance with this instruction to ensure that this campaign is properly implemented.

Motivation:

School climate is an important factor that promotes learning.

Moreover, school climate is something that can be managed, and the "Declaration for Good Treatment" can contribute to this goal.

We invite you to be part of the solution so that together we can help reduce bullying and improve treatment in schools.

Everyone:

To comply with this instruction, you must follow the steps below:

First: Download the "Declaration for Good Treatment" that you will find in the email and print one copy for each student. If you cannot print one for each student, print one per classroom.

Step 2: Download the Implementation Guide that you will find in the email and follow the instructions to carry out the campaign on November 23 in all secondary classrooms.

Step 3: Document compliance with the declaration by sending photos via WhatsApp on November 23 as indicated in the guide (one photo per classroom), and complete an implementation form that you must send to XXX before November 30.

On the webpage for the month of good treatment, which is included in the email we sent you, you will find summarized instructions, additional materials on the appli-

cation protocols for the Culture of Peace, and contact information should you have any questions about the activities related to the campaign.

Please review the email we sent and download the attached files using the download buttons as soon as you have a moment. It is very important that you download both files and review them carefully.

We appreciate your cooperation, and we trust that the implementation of this program will be a success thanks to your efforts.