Is this Technology Useless? How Seemingly Irrelevant Factors Affect Adoption and Efficacy

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ABSTRACT (119 words)

We conduct a field experiment (N=6,291) to understand how the strategies organizations use to implement new technologies affect their adoption and efficacy. Specifically, we show that the standard strategy schools use to introduce a text message alert system for parents—online signup—induces negligible adoption. Simplifying the enrollment process by allowing parents to enroll via text messages modestly increases adoption—especially among parents of higher-performing students. Automatically enrolling parents dramatically increases adoption since very few parents opt out. The standard and simplified implementations generate no detectable increases in student performance. However, automatically enrolling parents meaningfully increases GPA and reduces student course failures. Simple changes to the implementation of new technologies can lead to radically different conclusions about whether new technologies are valuable and their ability to close achievement gaps.

It can be challenging for organizations to decide whether a new technology is a worthy investment because the expected benefits are difficult to assess. To make this assessment, organizations must forecast user adoption as well as the impact the technology would have on the outcomes they value. If potential users within an organization have standard, rational preferences, their decision to adopt the technology may closely reflect how much they value it relative to the costs of using it. However, if users' preferences are not well-formed or stable, then even subtle changes to how the technology is implemented may impact their decisions to adopt as well. When the latter is true, organizational leaders tasked with making technology investment decisions may fail to disentangle the potential benefits of the technology from the effects of how the technology is implemented. This could lead to inaccurate assessments about the value of new technologies.

The value created by new technologies is particularly salient in the education sector, where U.S. K-12 schools spent $5 billion on information technologies in 2015 (McCarthy, 2016). Many of these technologies aim to close achievement gaps between advantaged and disadvantaged students, but the evidence of their effectiveness is mixed. A number of papers have shown varying impacts of education technologies such as computers (Machin, McNally, & Silva, 2007; Barrera-Osorio & Linden, 2009; Malamud & Pop Eleches, 2011; Fairlie & Robinson, 2013; Vigdor, Ladd, & Martinez, 2014; Beuermann, Cristia, Cueto, Malamud, & Cruz-Aguayo, 2015), access to the Internet (Goolsbee & Guryan, 2006; Belo, Ferreira, & Telang, 2013; Bulman & Fairlie, 2016; Dettling, Goodman, & Smith, 2015), computer-aided instruction (Angrist & Lavy, 2002; Rouse & Krueger, 2004; Barrow, Markman, & Rouse, 2009; Banerjee, Cole, & Duflo, 2007; He, Linden, & McLeod, 2008; Taylor, 2015; Marsh, Pane, & Hamilton, 2016), teacher dashboards (Tyler, 2013) and mobile devices (Fryer, 2013; Bergman, 2015; Kraft & Rogers, 2015; Castleman & Page, 2015; York & Loeb, 2014; Murphy & Beland, 2015; Castleman & Page, 2016; Berlinski, Busso, Dinkelman, & Martinez, 2016; Bergman & Chan, 2017).

One source of variation in these impacts stems from user adoption. Many technologies used in educational settings simply fail to be used (cf. Cuban, 2003; Hess & Saxberg, 2013, and it is unclear whether they would be effective if they were used. This underscores the importance of understanding how the adoption and efficacy of new technologies is affected by the implementation strategies of schools, in particular, and of organizations, more generally. However, there is little experimental research testing how implementation affects the usage of educational technologies. Much of the existing research on technology adoption examines how pricing strategies affect the usage of critical health products, especially in developing contexts. This includes research on the adoption of insecticide-treated bed nets (Cohen & Dupas, 2010; Tarozzi et al., 2014; Dupas, 2014), deworming pills (Kremer & Miguel, 2007), vaccines (Banerjee, Duflo, Glennerster, & Kothari, 2010), water treatment (Ashraf, Berry, & Shapiro, 2010; Kremer, Leino, Miguel, & Zwane, 2011), as well as agricultural inputs such as fertilizer and high-yield variety seeds (cf. Foster & Rosenzweig, 2010; Duflo, Kremer, & Robinson, 2011). In contrast, there is less research on how the various strategies used to implement new education technologies to end users may affect their rate of adoption, who adopts the technologies, and the effect of the new technologies on student outcomes.

We report a randomized field experiment testing how the strategies to implement a new text message parent alert system affect its adoption and efficacy. The technology sends three types of weekly, automated text-message alerts to parents. The first type of text message alerts parents to which classes their child has missed during the week. The second type of text message alerts parents about the number of assignments their child is missing in each class. The third and last type of text message alerts parents to the courses in which their child is receiving a grade below 70%. The technology draws this academic information from digital grade books used by teachers and the districts' Student Information System. Phone numbers are automatically retrieved from the Student Information System as well, and the academic information is subsequently texted to parents. Each alert is sent on a different day of the week.

Emerging research shows that communications to parents conveying actionable educational information can produce important achievement gains, often at low marginal cost. Kraft and Dougherty (2013) conducted an experiment in a Boston charter school that shows daily phone calls home to parents from their child's teachers improve student behaviors. Bergman (2015) randomized the provision of bimonthly text messages to parents detailing their child's missing assignments and grades, which increased student effort and achievement. Kraft and Rogers (2015) show that messages from teachers to parents significantly reduced dropout from a high school credit recovery program. Rogers and Feller (2017) sent letters to parents about their child's absences leveraging ideas from behavioral science and found they reduced absenteeism. Given the promise of interventions improving parents' access to timely, actionable information, this manuscript explores how to bring these interventions to scale by testing how implementation policies affect take up.

We vary how this new communication technology can be adopted by parents of children in 12 Washington D.C. middle and high schools. Specifically, we vary elements of implementation that normative models of human behavior might predict should have trivial effect on adoption and efficacy – what Thaler calls “seemingly irrelevant factors” (Thaler, 2015). Those in the Standard condition were told by text message that they could adopt the technology by enrolling on the district website, which is standard practice. Those in the Simplified condition were told by text message that they could adopt the technology by replying “start” in response to a text message. Those in the Automatically Enrolled condition were told by text message that they could adopt the technology passively by not opting out of being enrolled by default, which parents could do by responding “stop” to any text-message alert.

The Simplified condition aims to reduce barriers to adopting the technology. Previous research suggests that reducing frictions in the enrollment process can increase adoption (Sunstein, 2013). Using data collected from tax forms completed on behalf of low-income families to auto-complete the Free Application for Federal Student Aid (FAFSA) forms has been found to dramatically increase the percentage of students submitting the form, gaining financial aid, matriculating to college, and succeeding in college (Bettinger, Long, Oreopoulos, & Sanbonmatsu, 2012). Another study examined how reducing frictions affects plan switching and cost savings for Medicare Part D prescription drug insurance plans. Sending a mailer to individuals that listed the most cost-effective plans increased plan switching and reduced their costs compared to sending a mailer that just listed the website where this information was available (Kling, Mullainathan, Shafir, Vermeulen, & Wrobel, 2012). These studies show that multi-step enrollment processes can make it difficult to infer end-user valuations based on end-user adoption, and that simplification of the enrollment process can increase end-user adoption.

The Automatically Enrolled condition makes adopting the technology relatively effortless while imposing some effort to not adopt the new technology. This contrasts with the Standard and Simplified conditions, which make it effortless to not adopt the technology. Automatic enrollment has been widely observed to increase adoption of new programs. For example, automatically enrolling new employees in a 401(k) retirement plan dramatically increases initial and long-term enrollment relative to automatically not enrolling new employees (Madrian & Shea, 2001). Another widely cited example involves enrolling to be an organ donor. Automatically enrolling citizens to be organ donors appears to dramatically increase organ-donor enrollment relative to automatically not enrolling citizens (Johnson & Goldstein, 2003). Automatic enrollment can be especially powerful because end-users can interpret defaults as implicit recommendations (McKenzie, Liersch, & Finkelstein, 2006), and because opting-out may be interpreted as meaning something radically different than opting-in (Davidai, Gilovich, & Ross, 2012).

The relationship parents have with their children and their children's schools may amplify the potency of automatic enrollment in the context of an educational technology like the one we study.

We find that reducing seemingly-small frictions to using the technology drastically affects its adoption and efficacy. Only 1% of parents in the Standard condition adopted the new educational technology. Roughly 8% of parents in the Simplified condition adopted the new educational technology. Parents who actively adopted the technology through either Standard or Simplified tended to have higher-achieving children and tended to be more engaged in their children's educations before the study began than those who did not enroll. The greatest increase in adoption came from automatically enrolling parents, which resulted in 96% adoption. Less than four percent of parents in this condition withdrew from the technology at any point during the school year.

At the end of the school year the school district asked parents whether they would like to use the same technology during the following academic year. Parents in both the Simplified and Automatic Enrollment conditions were more likely to want to use the technology the following school year compared to those in the Standard condition. Inducing parental adoption by varying the implementation strategy increased subsequent demand for using the technology. This illustrates how behaviorally-informed implementation strategies can lead to both higher initial adoption and sustained, increased demand.

The implementation strategies affected student achievement as well. Students of parents assigned to the Automatic Enrollment condition showed academic gains at low cost while those whose parents were assigned to the Simplified and Standard conditions showed no academic gains relative to those in the Control condition. Students in the Automatic Enrollment condition saw a 0.05-0.06 point increase in their GPA, and course failures were reduced by 0.2 courses per student, or about 10%. Those in the Standard and Simplified conditions showed no significant student achievement impacts, which is unsurprising given the low adoption rates among parents in those conditions. The rest of this paper proceeds as follows. Section I describes the experiment design and data. Section II presents the results on usage and academic outcomes. Section III concludes.

I. EXPERIMENTAL DESIGN AND DATA

A. Background

The experiment took place in Washington, D.C. The school district is divided into eight administrative wards, all served by the District of Columbia Public School (DCPS) system. DCPS has 115 schools and a total enrollment of 47,548 students during the 2014-2015 academic year. The 12 schools included in this study had a total population of just over 6,900, and are spread across six of the eight wards. In these 12 schools in 2015, 81% of students were Black, 16% Hispanic, and just under 2% white. Across the entire school district, 67% of all enrolled students in 2015 were Black, 17% Hispanic, and 12% white. The 2015 graduation rate for DCPS as a whole was 64%, and the graduation rate for the four high schools in our sample was 68%. Overall, 25% of all DCPS students met ELA proficiency on the PARCC assessment, and 21% met math proficiency. In our 12 school sample, 9% of students met ELA proficiency, and 5% met math proficiency on the PARCC assessment in 2015.

B. Experimental Design

Twelve under-performing middle and high schools were selected by DCPS to pilot the text message parent alert system; all 12 schools participated in this study. Sample sizes within each school ranged from 260 to 1,460 students. Our sample included eight middle schools serving grades 6 to 8; three high schools serving grades 9 to 12; and one combined school with grades 6 to 12. About 49% of the overall sample were high school students (grades 9 – 12). Within each school, all enrolled students were randomized into one of four conditions:

1. **Control:** Parents could access their child's information via Engrade's online parent portal and could sign up for the text message alert service online, but they were not sent any communication informing them that the service was available.
2. **Standard:** Parents were sent a text message with information about the text message alert system, and were given instructions to enroll in the service online using a password they collect from their children's schools if they were interested.
3. **Simplified:** Parents were sent a text message with information about the text message alert system, and were given instructions to enroll in the service online if they were interested. Shortly thereafter they were also sent a follow-up text message allowing them to enroll in the alert service via a text message response.
4. **Automatic Enrollment:** Parents were automatically enrolled in the text message alert system, and were given the option to "opt-out" at any time.

Four schools had begun sending absence alerts to parents before the experiment began. However, these alerts did not overlap with the ones sent through our study, and only 428 students received alerts from both our study and from the school-wide alerts. We do not exclude these schools from the analysis because the messages sent differed in content and frequency from those sent through our study.

All 6,976 students in the 12 participating schools were enrolled in the study. After randomization, 1,598 were assigned to the opt-in conditions—773 to the Standard condition, and 825 to the Simplified condition; 2,705 were assigned to the Automatic Enrollment condition, and 2,673 to the Control condition. Our ex-ante prediction was that the treatment effect would be smaller for those assigned to the two opt-in conditions. As such, we limited the size of the Standard and Simplified conditions in order to increase our power to detect treatment effects on academic outcomes.

C. Intervention implementation

All 12 schools in the sample began using the text message parent alert system in 2014. As part of the system, all parents in participating schools were given access to an online parent portal, through which they could find information on their child's attendance, grades, homework completion, and academic progress. In order to access the parent portal, parents needed to contact the school to receive login information. Some schools also distributed this information at school-wide events such as parents' nights or school orientations. Accessing the information in the parent portal required the parent to actively log into the online platform. On average, only about 30 percent of parents had ever logged into the portal prior to the experiment beginning.

The online platform also allowed for student-specific information to be automatically sent to parents via text message. Parents in the Control condition had access to the parent portal, and could enroll in the text message parent alert service on their own, but were not offered any encouragement or instructions for doing so as part of the experiment. Parents in the Standard condition received a text message informing them that they could log in online to the parent portal to enroll in the service, and how they could obtain their account information, if they did not have it. Parents in the Simplified condition received a text message telling them that they could enroll in the service by simply replying "start." Parents in the Automatic Enrollment condition were sent a text message at the beginning of the study informing them that they had been automatically enrolled in the alert service, and that they could text back "stop" at any time to withdraw. See Figure 1 for full message text.

From January, 2015, to June, 2015, enrolled parents received automated text message alerts if their child had missing assignments, a class absence, or a low average course grade. One message was sent per type of alert on a specific day each week. Absence alerts were sent every Tuesday, missing assignment alerts on Thursdays, and low course average alerts on Saturdays. Thus, parents could receive up to three alerts per week if their student had a missing assignment, a class absence, and a low average course grade. All alerts were personalized with student-specific information. The thresholds for receiving these alerts were 1 or more missing assignments in the past week; 1 or more absence in the past week; and a course average below 70 percent. Figure 1 shows the full text of each message.

D. Data collection

The analyses used in this manuscript involve routinely collected administrative data including basic demographic information, attendance data, course grades, and individual assignment grades. Student-level, class-specific data are entered into the Engrade gradebook platform by teachers; administrative data such as parent phone numbers are entered by school administrative staff. Absence information is collected by teachers and entered into a district-wide system, which is then synced with the Engrade system each evening. All data used in this study were extracted from the gradebook platform.

The automatic text message parent alert service uses cell phone numbers provided by parents. FERPA regulations allow for student-specific academic information to be sent to parents using contact information they voluntarily provided to the school. About 66% of our sample had valid cell phone numbers, balanced evenly across treatment conditions (see Table I). The primary analysis presented in this paper utilizes the full randomized sample.

E. Outcomes

We are interested in two primary outcomes. First, we are interested in how implementation strategy (as reflected in condition assignment) affects adoption of the text message parent alert service. Second, we are interested in how implementation strategy (as reflected in condition assignment) affects student academic performance. For the latter, we use two measures of academic performance: number of courses a student fails, and average semester grade point average (GPA).

F. Empirical Strategy

We estimate the causal effect of condition assignment by the OLS regression

$$(1) \qquad y_i = \propto_0 + \beta_1 T_i + X_i + \varepsilon_i$$

where y_i is the outcome variable for student i; T_i is a vector of indicators for assignment to one of the three treatment groups or the Control condition; X_i is a vector of pre-intervention student-level covariates; and ε_i is an error term. The standard errors are derived from the Huber-White robust estimator for the variance-covariance matrix. Student-level covariates included in X_i are baseline GPA, a continuous measure of the number of times the parent had ever logged into the parent portal prior to the intervention, the number of student absences prior to the start of the intervention, and an indicator for students who are Black or African-American. All regressions also include strata indicators as controls. Strata are comprised of gender, grade level, a binary indicator for pre-intervention low GPA (below 1.67 for high school, or below 1.94 for middle school), and a binary indicator for pre-intervention low attendance.[1]

The first outcome variable we test is average second semester grade point average. Students receive grades four times per year: in October, January, March, and May. Each of the four terms has 44-46 school days, and final semester grades are given in January and in May. Students receive numeric grades on a 100-point scale in each course, as well as letter grades ranging from A+ to F-. Letter grades of a D- or below are considered failing. We calculated an average term GPA for each student from individual course grades received in language, math, science, history, and arts courses. We then calculated each student's second semester GPA by averaging their third and fourth term GPAs. The full conversion scale for numeric and letter grades can be found in Appendix C.

We also use equation (1) to test the effect of treatment on the number of courses failed in the second semester. To pass a course, students must have a final grade of 64 or above on a 100-point scale, which is equivalent to a "D" letter grade. The total number of courses a student failed was calculated based on letter grades, and summed across terms 3 and 4. The baseline control variables remain the same when we analyze this outcome.

To examine the effect of receiving treatment—the treatment-on-the-treated or "TOT" effect—we use a two-stage least squares model in which we instrument an indicator for receiving at least one alert with the treatment indicator.[2] In the first stage we evaluate

$$(2) \qquad alerted_i = \propto_0 + \beta_1 T_i + X_i + \varepsilon_i$$

[1] Strata also include a binary indicator for participation in a prior study that involved sending alerts to parents (n=2,183, 31% of total sample).

[2] The 2SLS model also includes any alerts schools may have sent by automatically enrolling parents as the purpose of the TOT analysis is to evaluate the impact of receiving *any* alert. This is the only analysis presented in this manuscript that includes any school-wide alerts as we focus on the ITT analysis in the main tables.

where $alerted_i$ is an indicator for whether a student received at least one alert during the intervention; T_i is a vector of indicators for assignment to one of the three treatment groups or the Control condition; X_i is a vector of the same pre-intervention student-level covariates described above; and ε_i is an error term. All specifications again include strata as controls.

G. Baseline balance

Table II presents pre-intervention summary statistics by condition, and *p*-values showing the statistical significance of the difference in means across the four conditions. About 80% of our sample was Black or African-American, and 16% was Hispanic. On average, students' baseline GPA was 1.91, and 31% of parents had logged into the parent portal at least once prior to the intervention. The median number of pre-intervention absences was 16 days, and the median percent of missing assignments was 6.3%. Column (5) shows that we cannot reject the null hypotheses of no difference between the four condition groups for nearly all observable characteristics. The only characteristic that differs significantly across condition groups is the number of Asian students, yet this ethnic group comprises only 1% of the overall sample. When we regress baseline covariates on our treatment indicators and conduct an F-test to determine if these covariates are jointly significant, there is no significant difference in any covariate across condition groups.

H. Attrition

We received outcome data for 99.8 percent of our sample. Seven percent of the students in our sample transferred schools within the district during the course of the study; 0.2 percent of students in our sample could not be found in the Engrade system at the end of the study. We assume that students who could not be found at the end of the study period dropped out or transferred out of DCPS. We consider attrition to comprise both those students who transferred schools within the district, as well as those whose outcome data we could not find. Attrition was balanced evenly across treatment conditions, averaging about 8 percent overall, as shown in Table I. The primary analysis includes all students for whom we received outcome data.

II. RESULTS

A. User Adoption

As shown in Figure 2, slightly less than 8 percent of parents assigned to the Simplified condition ultimately enrolled to use the technology, whereas 96 percent of parents assigned to the Automatic Enrollment condition remained enrolled throughout the course of the study. Table III shows that students of parents assigned to the Simplified condition who enrolled had a significantly higher baseline GPA than those who remained enrolled in the Automatic Enrollment condition. In addition, the percentage of parents who had logged into the Engrade parent portal at least once prior to the start of the intervention was significantly higher among those who actively enrolled in the Simplified condition than those who remained enrolled in the Automatic Enrollment condition. This supports our hypothesis that, given the chance, the most engaged parents and the higher performing students would be the most likely to enroll in the text message parent alert system.

As part of the study, we sent 28,533 alerts to 1,489 parents.[3] Ninety-five percent of the alerts went to parents in the Automatic Enrollment condition (see Table IV). The distribution of alert types was similar for those who enrolled in the Automatic Enrollment and Simplified conditions.

Of the 4,303 parents assigned to the three treatment conditions, we sent alerts to 1,475 or about 34 percent. 52 percent of parents in the Automatic Enrollment condition, 7 percent of those in the Simplified condition, and less than 1 percent of the parents in the Standard condition received at least one alert (see Table V). On average, about 30 percent of parents in the Automatic Enrollment condition, and about 4 percent of those in the Simplified condition, received alerts each week (see Figure 3). Parents in the Automatic Enrollment and Simplified conditions who enrolled and received at least one alert received an average of about 19 alerts over the course of the semester. Parents in the Standard condition who enrolled and received at least one alert received an average of 10 alerts throughout the study.

Of the 1,475 treatment condition parents who received one or more alerts, 1,423 (96%) received at least one absence alert, 1,157 parents (78%) received at least one missing assignment alert, and 1,169 (79%) received at least one low grade alert.

B. Primary Outcomes

[3] The total number of parents who received 1 or more alerts includes 14 parents in the Control condition, as we could not prevent parents from enrolling in the alert service. Thus, it is possible that some parents found out about the alert service from other sources and enrolled on their own via the parent portal.

Since not all parents in the initial experiment universe enrolled to receive alerts, and since not all of those who enrolled actually received alerts, we first conduct an intent-to-treat (ITT) analysis in which all parents assigned to a treatment condition are considered treated. Table VI reports OLS estimates of equation (1). Column (1) shows that the average second semester GPA for the Control condition was 1.89. Being assigned to the Automatic Enrollment condition increased average GPA by nearly 0.07 points, about 3%. Assuming each student takes five courses, a 0.07 GPA point effect implies a 0.35 point increase in GPA in one course in one semester, or approximately one-third of a letter grade. Column (2) adds a set of baseline controls to the model, including a continuous measure of baseline GPA, the number of pre-intervention log-ins to the parent portal, pre-intervention absences, and an indicator for Black or African-American students. This reduces the treatment effect slightly to 0.05 points, but improves the precision of the estimates.

Column (3) examines the effect of treatment on the number of courses failed. Students in the Control condition failed an average of 2.4 courses in the second semester. Being assigned to the Automatic Enrollment condition reduced the number of courses failed by .23 courses, or about 10%, from the Control condition mean. This implies that an average of 1 in 4 students in the Automatic Enrollment condition passed a course they otherwise would have failed. Again, adding a set of control variables reduces the treatment effect observed for those assigned to the Automatic Enrollment condition slightly to 0.21 courses, but increases precision of the estimates.

The results presented in Table VI underestimate the average treatment effect since only 52 percent of parents assigned to the Automatic Enrollment condition and 7 percent of parents assigned to the Simplified condition—for an average of 34 percent—actually received one or more alerts. Thus, to predict the effect of actually receiving alerts we use a two-stage least squares model based on equation (2) to estimate the TOT effect. Table VII presents 2SLS estimates using assignment to any treatment as an instrument for receiving alerts. Column (2) shows that receiving alerts increased students' average semester GPA by 0.16 points. For a student enrolled in five courses, this equates to a 0.8 GPA point increase in one course, which is equivalent to an increase of about two-thirds of a letter grade in one course (e.g., from a "C" to a "B-")—a larger effect than we observed in the ITT analysis. Receiving alerts reduced the number of courses failed by .5 courses (column (4)). Both effects decrease slightly, but remain significant when including a set of baseline controls (columns (3) and (5)).

The patterns in Tables VI-VII suggest that the intervention effectively improved academic performance, as measured by average semester grade and number of courses failed, for students in the Automatic

Enrollment condition compared to those in the Control condition. These results remain consistent when we exclude students who transferred schools within the district, as well as when the sample is limited to only those with valid cell phone numbers. The effects for both conditions that required parents to actively enroll (Standard and Simplified) are small and not statistically significant in every model.

C. Automatic Enrollment vs. Standard Enrollment

The results in Table A1 (Appendix A) present the same primary outcomes, but using the Standard condition as the comparison group. Columns (1) and (2) show a positive effect on average GPA among students in the Automatic Enrollment condition. Although the treatment effect for those in the Automatic Enrollment condition is not statistically significant compared to the Standard condition, the estimate is directional (p-value is 0.13). Columns (3) and (4) show a reduction of about 0.2 courses, or 8%, in the number of courses failed among students in the Automatic Enrollment condition compared those in the Standard condition, who failed an average of 2.4 courses in the second semester. This effect is significant at the 10% level. Recall from the previous section that those assigned to the Standard condition did not see their academic performance improve compared to the Control group, while those assigned to Automatic Enrollment did. The analyses in this section show that the effect sizes of being assigned to Automatic Enrollment condition are roughly the same when compared to being assigned to the Standard condition, although the sample size renders the comparisons not as statistically significant. Overall, this suggests not only that receiving alerts affects academic outcomes, but also that the method of enrollment in such a service is a meaningful factor in the effectiveness of treatment.

D. Heterogeneity

We evaluated these primary academic outcomes for two subgroups—middle school and high school students—by restricting the sample to each and evaluating the same equations as above. Results for each subgroup are presented in Appendix B, showing that the effect of being assigned to the Automatic Enrollment condition was largest for high school students. Among high school students in the Automatic Enrollment condition, second semester GPA increased by 0.14 points, or about 8%, from the Control condition mean of 1.78 points. Assuming each student takes five courses, a 0.14 GPA point effect implies an increase of two-thirds of a letter grade in just one course (e.g., from a “C” to a “B-”). The number of courses failed among high school students in the Automatic Enrollment condition decreased by 0.34 courses compared to the Control condition, or about 13%. These effects are all significant at the

1% level, or the 5% level when including baseline controls. However, the effects for middle school students are substantially smaller and all statistically insignificant from zero. This aligns with results described in a more recent study by Bergman and Chan (2017), which also found similarly large effects of a text message parent alert system for high school compared to middle school students.

Again using an instrumental variable approach to estimate the TOT effect for high school students yields higher estimates than the ITT analysis, which is not surprising since only about 20% of high school students received one or more alerts. Table B2 shows that receiving alerts increased students' average semester GPA by 0.4 points, and reduced the number of courses failed by 1 full course. Both effects decrease slightly but remain significant when we include a set of baseline controls.

E. Demand for the technology the following academic year

After the academic year ended we assessed whether being enrolled in the text message parent alert system increased parents' demand for the text message parent alert system by asking parents if they would be interested in signing up for a text message parent alert system if offered the following academic year. This inquiry was sent via text message, but we were concerned that parents who had been enrolled in the text message parent alert system would be less responsive to text messages after having received near-weekly message alerts over the previous six months. Thus, to assess this potential source of response rate bias, 381 parents in the three treatment groups were first sent a placebo text message asking, "Did you fill out your enrollment paperwork for next school year? Text YES if you did. If not, and you need help getting started, pls reach out to your school." Response rates to the placebo message were compared to evaluate non-responsiveness across treatment groups.

Subsequently, we sent 2,369 parents across the three treatment conditions a message asking, "DCPS may offer a service next yr that texts if your child has a low grade, missed assignment or absence. DCPS wants to keep you informed. Text YES if interested." Each parent also received a "discontinue" message, which read "You can opt out of texts at any time by replying STOP."

The original study design called for sending both the message that elicited interest in the service for next year, and corresponding "discontinue" message to all students in all four conditions, but the messages intended for those in the Control condition failed to send due to a vendor error, as did about 20 percent of the messages intended for students in the treatment groups. At the same time, some parents were inadvertently sent up to six copies of the same message. Overall, about 77% of parents who received at least one message received exactly two messages—one interest elicitation message, and one opt-out message—as intended. If we restrict our sample to only those who received the two intended messages, we find that approximately 43% of parents assigned to each treatment group received both messages. Restricting to this universe, we also regress baseline covariates and our treatment indicators on an indicator for receiving two messages. The results presented in Table VIII show that there was no significant difference in messages received across treatment groups, nor across most baseline covariates.

Despite the imperfect implementation, we find that about 15 percent of parents in both the Automatic Enrollment and Simplified conditions answered the placebo text message, while 21 percent of the Standard condition responded (see Table IX). Using a simple linear probability model, we estimate the effect of treatment on responding to the placebo message and find that those in the Automatic Enrollment condition were about 8.9 percentage points less likely to respond to the placebo message compared to those in the Standard condition, when including strata of controls, as shown in Table X. This effect is not significantly different from zero at conventional level, although it is has a *p*-value of 0.12. Those in the Simplified condition were 11 percentage points less likely to respond to the placebo text, which is statistically significant at the 10% level. This is consistent with our concern that continuous messaging for those in these treatment groups lowered their propensity to respond to additional messages.

Analyzing response rates to the subsequent interest elicitation text message, and limiting our sample to only those who received the intended two messages, we still see a higher response rate among those in the Automatic Enrollment and Simplified conditions than among those in the Standard condition, as shown in Table XI. About 13 percent of the Automatic Enrollment condition and about 14 percent of the Simplified condition responded “yes” to the interest elicitation text message. Only 10 percent of the Standard condition responded “yes.”

Linear probability estimates presented in Table XII show that receiving the placebo message decreased the probability of responding to the interest elicitation text message by about 5 percentage points, implying that a response rate bias exists among those who have received previous messages. Even when controlling for the decreased responsiveness arising from the placebo message, those in the Automatic Enrollment treatment group were still 4 percentage points more likely to reply “YES” to the interest elicitation text message than those in the Standard condition which serves as a proxy control condition, robust to the inclusion of a full set of baseline controls (column (2)). Meanwhile, those in the Simplified condition were about 6 percentage points more likely to reply “YES” to the interest elicitation text message, statistically significant at the 10% level and similarly robust to the inclusion of controls. If we exclude those who received the placebo message from the analysis, we see almost identical effects, as shown in columns (3) and (4). Together, the Automatic Enrollment and Simplified conditions are 4.7 percentage points more likely to respond positively to the interest elicitation text message than those in the Standard condition, suggesting that the method of enrollment is a significant factor affecting future demand for the service.

The fact that those who received the placebo message were 5 percentage points less likely to respond to the interest elicitation text message than those who did not receive the placebo message suggests that receiving prior messages decreases the probability of responding to subsequent messages. As such, families who were enrolled to use the text message parent alert system technology may have been less inclined to respond to the interest elicitation text message after five months of receiving alerts as part of the first phase of the study. Based on the results presented in Table XII, and assuming a conservative estimate of a negative 2 percentage point bias, we speculatively infer that those in the Automatic Enrollment condition may have actually been up to 6 percentage points more likely to demand the text message parent alert system absent this source of bias.

III. Conclusion

We present a field experiment examining two principal research questions. First, how does the strategy used by an organization to implement a new technology affect end-user adoption of the technology? And second, how does the strategy used by an organization to implement a new technology affect its overall impact? These questions are particularly relevant in school districts. Many new technologies aim to close achievement gaps between high- and low-performing students. However, the ability to realize this goal is contingent upon the capacity for these technologies to improve student achievement and which families use them.

We find that the standard, high-friction way schools implement a parent alert system generates negligible adoption, simplifying the implementation process increases adoption, and automatically enrolling end-users dramatically increases adoption. The standard implementation strategy did not improve student performance, which is not surprising since almost any parents enrolled. For similar reasons, the simplified implementation strategy did not cause statistically significant improvements in student performance as well. However, automatically enrolling parents generated sizable improvements in student achievement: GPA increased by the equivalent of one-third of a letter grade in one course, and reduced course failure rate by about one quarter of a course per student.

These results have important implications. First, how an organization implements a new technology could lead it to radically different conclusions about whether the new technology is valuable. Schools using opt-in strategies—even when simplified—may find the technology studied in this manuscript to have low adoption and, in turn, little impact on student achievement. Consequently, they may (mistakenly) determine that the technology is useless. Second, we find that when schools introduce frictions into the adoption process, parents of children with higher baseline achievement are more likely to adopt than parents of lower-performing students. This implies that typical, opt-in strategies to promote new technologies could exacerbate achievement gaps rather than close them.

The final analysis about parental demand for the text message parent alert system during the subsequent academic year suggests that end-users learn about the value of the technology by using it: demand for the technology appears to increase with usage.[4] This is important as it implies that the higher rate of adoption from automatic enrollment does not just stem from the increased cost of un-enrolling. Instead, families' valuations of the technology actually increase, on average, as reflected in their desire to opt-in for the following year.

Finally, this study is one of a growing number showing that tools that automatically empower parents with actionable information can improve student achievement. A prerequisite for implementing this new technology is that teachers regularly and uniformly use a shared learning management system. Interventions like this one can only be possible after data is collected in a single database. Therefore, a critical policy implication for broadening the use of this specific new technology is to encourage teachers to standardize their use of learning management systems.

[4] A number of studies have shown that short-run subsidies for new technologies could affect subsequent adoption either positively or negatively due to learning and screening effects (Ashraf, Berry, & Shapiro, 2010; Billeter, Kalra, & Loewenstein, 2010; Dupas, 2014).

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Tables and Figures

Figure 1. Text message content

Group	Frequency	Message
Automatic enrollment	Once at beginning of treatment	[School Name] is testing a service that texts you if your child has a low grade, missed assignment, or missed class. You may change this service by logging onto www.engradepro.com or replying STOP. Please call the school at 202-XXX-XXXX if you have any questions.
Standard	Once at beginning of treatment	[School Name] is testing a service that texts you if your child has a low grade, missed assignment, or a missed class. Turn on this service by logging onto www.engradepro.com. Please call the school at 202-XXX-XXXX for your account information.
Simplified	Once at beginning of treatment	[School Name] is testing a service that texts you if your child has a low grade, missed assignment, or a missed class. Turn on this service by replying “START” to this message or logging onto www.engradepro.com.
Missing assignment alert	Weekly, Thursdays	Engrade Parent Alert: [Student name] has X missing assignment(s) in [Course Name]. For more information, log in to www.engradepro.com.
Absence alert	Weekly, Tuesdays	Engrade Parent Alert: [Student Name] has X absence(s) in [Course Name]. For more information, log in to www.engradepro.com.
Low course average alert	Weekly, Saturdays	Engrade Parent Alert: [Student Name] has a X% average in [Course Name.] For more information, log in to www.engradepro.com.

Figure 2. Percentage of parents who remained enrolled in the Automatic Enrollment condition vs. the percentage of parents who actively enrolled in the Standard and Simplified conditions

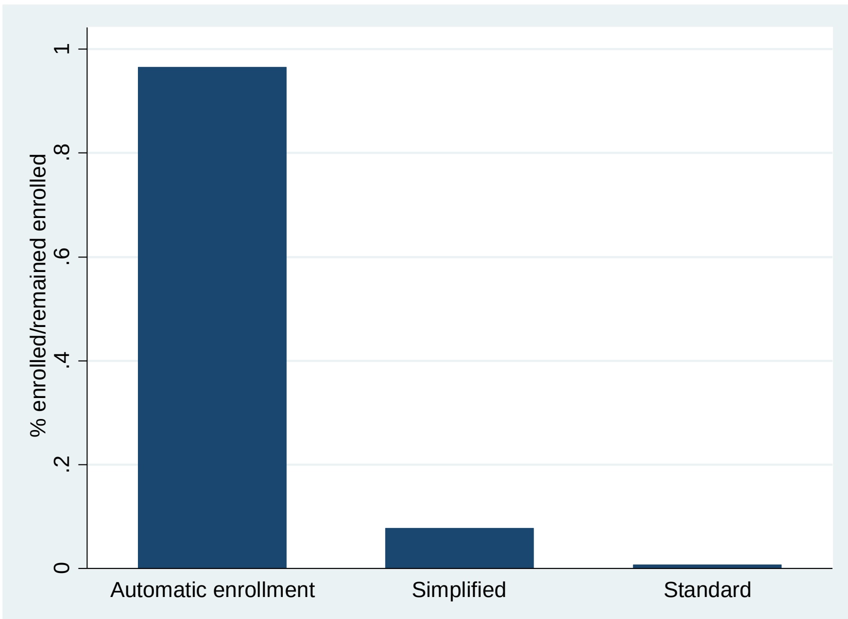
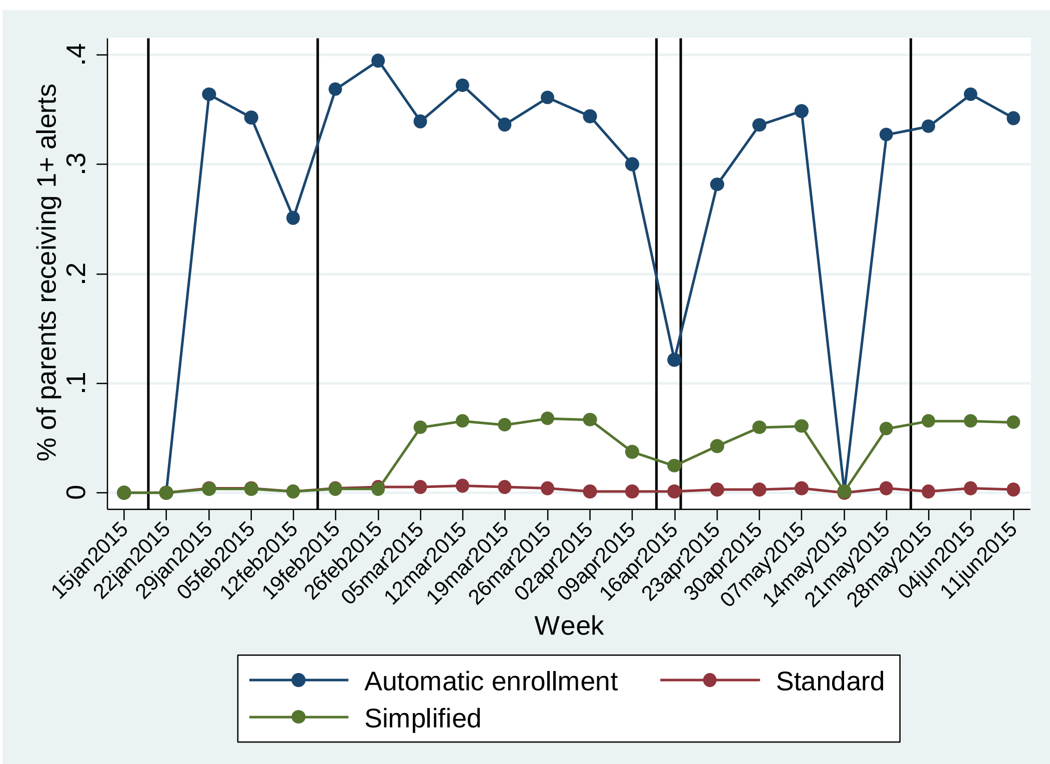


Figure 3. Percentage of parents in each condition who received alerts each week



Note: Vertical black lines indicate school holidays, including spring break (April 13-17, 2015).

Table I. Sample size

Factor	Control	Automatic Enrollment	Simplified	Standard	p-value
N	2673	2705	825	773	
Cell phone	1766 (66.1%)	1819 (67.2%)	567 (68.7%)	533 (69.0%)	0.32
Attrition	196 (7.3%)	209 (7.7%)	59 (7.2%)	43 (5.6%)	0.23

Table II. Pre-intervention summary statistics

	(1)	(2)	(3)	(4)	(5)
Factor	Control	Automatic Enrollment	Simplified	Standard	p-value
N	2673	2705	825	773	
Female	1289 (48.2%)	1305 (48.2%)	398 (48.2%)	373 (48.3%)	1.00
Black	2155 (80.7%)	2168 (80.4%)	680 (82.5%)	625 (81.0%)	0.58
White	41 (1.5%)	50 (1.9%)	15 (1.8%)	12 (1.6%)	0.80
Asian	44 (1.6%)	24 (0.9%)	5 (0.6%)	6 (0.8%)	0.020
Hispanic	417 (15.6%)	443 (16.4%)	120 (14.6%)	128 (16.6%)	0.56
Fraction of missing assignments, median	.063 (.013, .142)	.064 (.016, .152)	.066 (.015, .141)	.062 (.017, .150)	0.47
Ever logged into parent portal	825 (30.9%)	868 (32.1%)	260 (31.5%)	230 (29.8%)	0.60
Grade 6	454 (17.0%)	456 (16.9%)	136 (16.5%)	133 (17.2%)	0.98
Grade 7	455 (17.0%)	463 (17.1%)	142 (17.2%)	131 (16.9%)	1.00
Grade 8	457 (17.1%)	461 (17.0%)	141 (17.1%)	133 (17.2%)	1.00
Grade 9	830 (31.1%)	837 (30.9%)	254 (30.8%)	242 (31.3%)	1.00
Grade 10	259 (9.7%)	264 (9.8%)	79 (9.6%)	74 (9.6%)	1.00
Grade 11	175 (6.5%)	180 (6.7%)	56 (6.8%)	49 (6.3%)	0.98
Grade 12	42 (1.6%)	43 (1.6%)	14 (1.7%)	11 (1.4%)	0.98
Pre-intervention absences, median (IQR)	16 (6, 34)	15 (7, 34)	16 (6.5, 34)	16 (7, 36)	0.62
Pre-intervention GPA, mean (SD)	1.90 (1.11)	1.92 (1.12)	1.92 (1.11)	1.93 (1.08)	0.94

Table III. Number of parents enrolled in text message alert system technology

	Automatic Enrollment	Simplified	Standard	p-value
Assigned to treatment	2,705	825	773	
# remained enrolled/# actively enrolled	2,610	64	6	
% remained enrolled/% actively enrolled	96.5%	7.8%	0.8%	<.001
Pre-intervention GPA for those who remained enrolled/actively enrolled	1.92	2.17	2.09	.19
Percent of parents who had ever logged into parent portal prior to intervention for those who remained enrolled/actively enrolled	32.2%	40.6%	100.0%	<.001

Table IV. Number of alerts sent[5]

	Automatic Enrollment	Simplified	Standard
Total number of alerts sent	27,261	1,154	59
Average number of alerts sent per student[6]	10.1	1.4	0.8
Number of missing assignment alerts	6,985 (25.6%)	421 (36.5%)	14 (23.7%)
Number of absence alerts	10,389 (38.1%)	295 (25.6%)	37 (62.7%)
Number of low grade alerts	9,887 (36.2%)	438 (37.9%)	8 (13.6%)

Table V. Number of parents receiving one or more alerts during the study

	Automatic Enrollment	Simplified	Standard	p-value
N	2,705	825	773	
Number of parents who received 1+ alerts	1,409	60	6	
Percentage of parents who received 1+ alerts	52.1%	7.3%	0.8%	<.001
Average number of alerts received for those who received at least 1 alert	19.4	19.6	10.2	.19

[5] Summary statistics on alerts do not include those sent by schools that turned on the text message parent alert system school-wide.
[6] Calculated per student assigned to condition, not per student who received at least 1 alert.

Table VI. Primary academic outcomes

VARIABLES	(1) GPA	(2) GPA	(3) # courses failed	(4) # courses failed
Automatic Enrollment	0.065***	0.047**	-0.233***	-0.205***
	(0.024)	(0.021)	(0.073)	(0.066)
Standard	0.009	0.003	-0.036	-0.040
	(0.037)	(0.031)	(0.111)	(0.098)
Simplified	0.001	0.003	-0.166	-0.160
	(0.036)	(0.031)	(0.110)	(0.103)
Baseline GPA		0.639***		-0.956***
		(0.018)		(0.052)
# portal log-ins		0.001***		-0.002***
		(0.000)		(0.000)
Absences		-0.005***		0.024***
		(0.000)		(0.002)
Black		-0.200***		0.731***
		(0.023)		(0.063)
Observations	6,291	6,291	6,291	6,291
R-squared	0.348	0.532	0.244	0.376
Controls	Yes	Yes	Yes	Yes
Mean for Control	1.887	0.880	2.435	3.166

Notes: OLS estimates of equation (1). Dependent variables are average second semester GPA (columns (1) and (2)), and total number of courses failed in the second semester (columns (3) and (4)). Controls consist of strata of gender, grade level, and binary variables for pre-intervention low GPA (below 1.67 for high school; below 1.94 for middle school), pre-intervention low attendance (missed 1 or more days of school), and participation in a prior study that involved sending alerts to parents. Number of portal log-ins is a measure of the total number of times parents had logged into the Engrade portal prior to the start of this intervention. Baseline GPA is calculated as an average of term 1 grades for all language, math, science, history, and art courses. Term 1 runs from the start of the school year to the end of October. Absences is a continuous measure of pre-intervention student absences. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Table VII. Instrumental variable estimates of TOT effect

VARIABLES	(1) First stage	(2) GPA	(3) GPA	(4) # courses failed	(5) # courses failed
Automatic Enrollment – assigned	0.410***				
	(0.0129)				
Standard – assigned	0.00273				
	(0.0182)				
Simplified – assigned	0.0547***				
	(0.0186)				
Alert – received		0.157***	0.114**	-0.505***	-0.440***
		(0.055)	(0.046)	(0.161)	(0.147)
Baseline GPA			0.637***		-0.950***
			(0.018)		(0.052)
# portal log-ins			0.001***		-0.001**
			(0.000)		(0.001)
Absences			-0.005***		0.024***
			(0.000)		(0.002)
Black			-0.176***		0.637***
			(0.025)		(0.069)
Observations	6,291	6,291	6,291	6,291	6,291
R-squared		0.355	0.533	0.249	0.375
Mean for Control	2.918	2.918	1.048	0.823	3.368
Controls		Yes	Yes	Yes	Yes

Notes: 2SLS estimates of equation (2). Dependent variables are average second semester GPA (columns (2) and (3)), and total number of courses failed in the second semester (columns (4) and (5)). Controls and covariates detailed in Table VI notes. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Table VIII. Interest elicitation text message balance

	All recipients		Excluding placebo recipients	
	(1)	(2)	(3)	(4)
VARIABLES	Two messages delivered	Two messages delivered	Two messages delivered	Two messages delivered
Automatic Enrollment	-0.015	-0.015	-0.005	-0.005
	(0.020)	(0.020)	(0.021)	(0.021)
Simplified	-0.002	-0.001	0.004	0.004
	(0.025)	(0.025)	(0.026)	(0.026)
Sent placebo	0.004	0.004		
	(0.027)	(0.027)		
Baseline GPA		-0.017		-0.012
		(0.014)		(0.014)
# portal log-ins		-0.000**		-0.000*
		(0.000)		(0.000)
Absences		0.000		0.000
		(0.000)		(0.000)
Black		-0.028		-0.028
		(0.020)		(0.021)
Observations	4,303	4,303	4,026	4,026
R-squared	0.029	0.033	0.030	0.034
Controls	Yes	Yes	Yes	Yes
Mean for Control	0.435	0.488	0.417	0.457

Notes: OLS estimates. Dependent variable is binary indicator where 1 indicates successful delivery of exactly two messages: one "discontinue" message, and one interest elicitation text message. Reference group is Standard condition. Columns (3) and (4) exclude all placebo recipients. Controls and covariates detailed in Table VI notes. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Table IX. Response rates to placebo text message

	Automatic Enrollment	Simplified	Effective control (Standard)
Number sent placebo message	187	114	80
Number responded to placebo message	28	17	17
Percent responded to placebo message	14.97%	14.91%	21.25%

Table X. Linear probability estimates of placebo message response

VARIABLES	(1) Placebo response	(2) Placebo response
Automatic Enrollment	-0.089	-0.086
	(0.057)	(0.058)
Simplified	-0.111*	-0.112*
	(0.061)	(0.061)
Baseline GPA		0.016
		(0.037)
# portal log-ins		-0.000*
		(0.000)
Absences		0.000
		(0.001)
Black		0.017
		(0.058)
Observations	381	381
R-squared	0.175	0.183
Controls	Yes	Yes
Mean for Effective Control	0.240	0.195

Notes: OLS estimates. Dependent variable is binary indicator of response to placebo text message, where 1 indicates any reply. Reference group is Standard condition. Controls and covariates detailed in Table VI notes. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Table XI. Interest elicitation text messages sent and response rates

	Automatic Enrollment	Simplified	Effective control (Standard)
N	2,705	825	773
Number sent at least 1 interest elicitation text message	1,433	478	458
Number delivered exactly 1 interest elicitation text message and 1 “discontinue” message	1,138	354	341
Percent delivered exactly 1 interest elicitation text message and 1 “discontinue” message	42.1%	42.9%	44.1%
Number responded to interest elicitation text message	192	67	47
Percent responded to interest elicitation text message	13.40%	14.02%	10.26%

Table XII. Linear probability estimates of interest elicitation text message response, compared to Standard

	All who received two messages		Excluding placebo recipients	
	(1)	(2)	(3)	(4)
VARIABLES	Offer response	Offer response	Offer response	Offer response
Automatic Enrollment	0.043**	0.040*	0.043*	0.040*
	(0.021)	(0.021)	(0.022)	(0.022)
Simplified	0.061**	0.056**	0.068**	0.061**
	(0.027)	(0.027)	(0.029)	(0.029)
Sent placebo	-0.051*	-0.054**		
	(0.027)	(0.027)		
Baseline GPA		0.007		0.009
		(0.015)		(0.016)
# portal log-ins		-0.000		-0.000
		(0.000)		(0.000)
Absences		-0.001***		-0.001***
		(0.000)		(0.000)
Black		0.073***		0.080***
		(0.020)		(0.021)
Observations	1,830	1,830	1,667	1,667
R-squared	0.049	0.067	0.053	0.072
Controls	Yes	Yes	Yes	Yes
Mean for Effective Control	0.114	0.0818	0.112	0.0694

Notes: OLS estimates. Dependent variable is binary indicator where 1 indicates response of "yes" to the interest elicitation text message. All models limit sample to only those who received two messages (one "discontinue" and one interest elicitation text message). Columns (3) and (4) exclude all placebo recipients. Reference group is Standard condition. Controls and covariates detailed in Table VI notes. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Appendix A. Primary outcomes compared to Standard condition

Table A1. Primary academic outcomes, compared to Standard

	(1)	(2)	(3)	(4)
VARIABLES	GPA	GPA	# courses failed	# courses failed
Automatic Enrollment	0.056	0.044	-0.196*	-0.166*
	(0.037)	(0.032)	(0.109)	(0.097)
Simplified	-0.009	-0.000	-0.129	-0.120
	(0.045)	(0.039)	(0.137)	(0.125)
Control	-0.009	-0.003	0.036	0.040
	(0.037)	(0.031)	(0.111)	(0.098)
Baseline GPA		0.639***		-0.956***
		(0.018)		(0.052)
# portal log-ins		0.001***		-0.002***
		(0.000)		(0.000)
Absences		-0.005***		0.024***
		(0.000)		(0.002)
Black		-0.200***		0.731***
		(0.023)		(0.063)
Observations	6,291	6,291	6,291	6,291
R-squared	0.348	0.532	0.244	0.376
Controls	Yes	Yes	Yes	Yes
Mean for Control	1.896	0.883	2.399	3.126

Notes: OLS estimates of equation (1). Dependent variables are average second semester GPA (columns (1) and (2)), and total number of courses failed in the second semester (columns (3) and (4)). Controls and covariates detailed in Table VI notes. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Appendix B. Heterogeneity: middle vs. high school

Table B1. ITT subgroup analysis: middle vs. high school

	GPA				Number of courses failed			
	High school		Middle school		High school		Middle school	
Automatic Enrollment	0.135***	0.078**	-0.002	0.020	-0.342***	-0.206**	-0.129	-0.183**
	(0.036)	(0.031)	(0.033)	(0.027)	(0.113)	(0.099)	(0.093)	(0.087)
Standard	0.017	-0.007	0.001	0.024	0.041	0.076	-0.111	-0.153
	(0.054)	(0.045)	(0.050)	(0.043)	(0.174)	(0.150)	(0.137)	(0.126)
Simplified	0.011	-0.002	-0.009	0.011	-0.194	-0.118	-0.138	-0.183
	(0.052)	(0.046)	(0.050)	(0.042)	(0.159)	(0.149)	(0.153)	(0.142)
Baseline GPA		0.570***		0.702***		-0.862***		-1.032***
		(0.028)		(0.023)		(0.079)		(0.067)
# portal log-ins		0.001***		0.001***		-0.002**		-0.002***
		(0.000)		(0.000)		(0.001)		(0.001)
Absences		-0.007***		-0.003***		0.033***		0.016***
		(0.001)		(0.000)		(0.003)		(0.003)
Black		-0.122***		-0.282***		0.854***		0.499***
		(0.033)		(0.033)		(0.086)		(0.091)
Observations	3,083	3,083	3,206	3,206	3,083	3,083	3,206	3,206
R-squared	0.345	0.521	0.345	0.544	0.251	0.417	0.228	0.333
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean for Control	1.776	0.989	1.993	0.797	2.611	2.636	2.264	3.700

Notes: OLS estimates of equation (1). Dependent variables are average second semester GPA (columns (1) - (4)), and total number of courses failed in the second semester (columns (5) - (8)). Controls and covariates detailed in Table VI notes. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Table B2. Instrumental variable estimates of TOT effect, high school only

	(1)	(2)	(3)	(4)	(5)
VARIABLES	First stage	GPA	GPA	# courses failed	# courses failed
Automatic Enrollment – assigned	0.338***				
	(0.0192)				
Standard – assigned	0.0214				
	(0.0285)				
Simplified – assigned	0.0676**				
	(0.0286)				
Alert – received		0.400***	0.252***	-1.002***	-0.653**
		(0.099)	(0.088)	(0.310)	(0.282)
Baseline GPA			0.563***		-0.844***
			(0.028)		(0.079)
# portal log-ins			0.001*		-0.001
			(0.000)		(0.001)
Absences			-0.007***		0.033***
			(0.001)		(0.003)
Black			-0.036		
			(0.045)		
Observations	3,083	3,083	3,083	3,083	3,083
R-squared		0.348	0.517	0.259	0.413
Mean for Control	2.306	2.306	0.876	0.890	2.737
Controls		Yes	Yes	Yes	Yes

Notes: 2SLS estimates of equation (2). Dependent variables are average second semester GPA (columns (2) and (3)), and total number of courses failed in the second semester (columns (4) and (5)) for all high school students, which encompasses students in grades 9-12. Controls and covariates detailed in Table VI notes. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Appendix C. DCPS Grade scale and conversion

	Credit	GPA	On Grade	Honors*	AP* or IB*	
A (93%to 100%)	Yes	Yes	4.0	4.5	5.0	
A- (90% to 92%)	Yes	Yes	3.7	4.2	4.7	
B+ (87%to 89%)	Yes	Yes	3.3	3.8	4.3	
B (83% to 86%)	Yes	Yes	3.0	3.5	4.0	
B- (80% to 82%)	Yes	Yes	2.7	3.2	3.7	
C+ (77%to 79%)	Yes	Yes	2.3	2.8	3.3	
C (73% to 76%)	Yes	Yes	2.0	2.5	3.0	
C- (70% to 72%)	Yes	Yes	1.7	2.2	2.7	
D+ (67%to 69%)	Yes	Yes	1.3	1.8	2.3	
D (64% to 66%)	Yes	Yes	1.0	1.5	2.0	
F 63% & below	No	0				
W	No	Null				
L (late entry)	No	Null				**Converts to AUD (audit) at end of following advisory**