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The Impact of Defaults on Technology Adoption, and its Underappreciation by Policymakers

Abstract

We conduct an experiment to understand how enrollment defaults affect the take up and impact of an education technology. We show that a standard and simplified opt-in process induce low take up. Automatically enrolling parents increases adoption significantly and improves student achievement. Our surveys show automatic enrollment is uncommon because its impact is underestimated: District leaders *overestimate* take-up under the standard condition by 38 percentage points and *underestimate* take-up under automatic enrollment by 31 percentage points. After learning the actual take-up rates, there is a 140% increase in willingness to pay for the technology when shifting implementation to automatic enrollment.

JEL-Codes: I210.

Keywords: adoption, defaults, technology.

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The impacts of many new interventions and technologies can depend critically on participant take up and subsequent behavior change. Default enrollment is one potential policy lever that can substantially affect the take up of various programs, such as retirement saving (Madrian & Shea, 2001) and organ donation (Johnson & Goldstein, 2003). However, there is less evidence on the impact of automatic enrollment on the take up of new technologies—especially those that require continual, post-enrollment behavioral change from the participant (see Fowlie et al., 2017). Moreover, if key decision makers underestimate the influence of defaults on take up, it could lead to the under-provision of high-impact technologies and programs due to a perceived lack of demand and efficacy.

Past research has focused on end-users and their perception of the default options implied by automatic enrollment. Users can interpret default options as implicit recommendations (McKenzie, Liersch, & Finkelstein, 2006), and they can interpret opting-out of a default option as meaning something radically different than opting-into it (Davidai, Gilovich, & Ross, 2012). Moreover, making the impact of automatic enrollment transparent to users does not appear to mitigate its impact (Loewenstein et al., 2015; Steffel et al., 2016; Bruns et al., 2016). While recent research shows that scholars can predict, to some extent, the impact of behavioral interventions (DellaVigna & Pope, 2016), little is known about whether key decision makers can. Their perceptions matter because how these decision makers implement new technologies can affect their overall impact.

In this paper, we show how enrollment defaults affect the take up and impact of a novel technology that aims to help parents improve student achievement. We also document key decision makers' beliefs about how enrollment defaults affect the take up and efficacy of this technology, as well as their subsequent willingness to pay for it. This context is important because parental and family engagement is among the strongest determinants of inequality and children's long-run outcomes (cf. Coleman et al., 1966; Heckman, 2006; Cunha & Heckman, 2007; Todd & Wolpin, 2007).

Emerging research finds that technology-driven information interventions can increase student success (Escueta et al., 2017). In particular, providing additional information to parents can produce significant gains in student achievement at low marginal cost by changing parents' beliefs about their child's effort and ability (Rogers & Feller, 2017; Dizon-Ross, 2017; Bergman, 2015; EEF, 2017) or their schooling options (Hastings & Weinstein, 2008), making it easier to monitor and incentivize their child throughout the school year (Kraft & Dougherty, 2013; Kraft & Rogers, 2015; Bergman, 2015; Bergman & Chan, 2017), and prompting parents to directly invest in their child's skills over time (York & Loeb, 2016; Mayer et al., 2015). However, as described above, the ability to successfully scale these interventions in schools depends upon decision makers' perceptions of parental demand for the technology and its efficacy.

The technology studied in this paper engages parents by providing high-frequency, actionable information about their child's academic progress. Three types of weekly, automated text-message alerts are sent 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 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 district's 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.

To understand how defaults affect parental take up of this technology and its subsequent impact on student achievement, we randomly varied how the parents of children in 12 Washington D.C. middle and high schools could adopt it. 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.

We demonstrate several key findings. First, automatic enrollment has a large effect on parent adoption of the technology, despite parents being offered many opportunities to opt out. Only 1% of parents in the Standard condition adopted the new technology. Roughly 8% of parents in the Simplified condition adopted the new technology. However, automatically enrolling parents resulted in 96% adoption; less than 4% of parents in this condition withdrew from the technology at any point during the school year. Parents who actively adopted the technology through either the Standard or Simplified method 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. These correlations imply that defaults not only affected take-up rates, but also the characteristics of the families who enroll. Many school districts aim to engage families with lower-performing students or who are less actively monitoring their child’s progress; opt-in enrollment is less likely to engage these families.

Second, we find that enrollment defaults affected student achievement, even though this implies sustained, active post-enrollment behavior change on the part of families. Students of parents assigned to the Automatic Enrollment condition showed academic gains 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%. This is the equivalent of each student increasing one course grade from a C+ to B-, and nearly one in four students not failing a class she would have otherwise. The lack of impact for the Standard and Simplified conditions is unsurprising given the low adoption rates among parents in those conditions.

Third, simplifying enrollment increased subsequent parent demand for the technology. 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. This illustrates how behaviorally-informed implementation strategies can lead to both higher initial adoption and persistent, increased demand.

Lastly, we provide evidence for a novel mechanism as to why productive technologies may be under-deployed: decision makers underappreciate the importance of implementation strategies, which impacts their willingness to pay for the new technology. We surveyed 130 education decision makers—superintendents, principals, and family engagement coordinators—drawn from a sample of 300 educators representing 55 districts serving more than 3.2 million students. These decision makers *overestimate* the take-up rate of the Standard condition by 38 percentage points and they *underestimate* the take-up rate of Automatic Enrollment by 31 percentage points. After learning the actual take up rates under each enrollment condition, there is a corresponding 140% increase in the willingness to pay for the technology when shifting implementation from opt-in enrollment to opt-out enrollment (from $1.12 per student offered the technology to $2.73 per student offered the technology). In addition, we also document that opt-in enrollment is commonplace: among the decision makers whose districts already have such a technology, 79% indicated they enroll parents via an opt-in process.

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 describes our survey results and Section IV 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 could 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 one or more missing assignments in the past week; one or more absence in the past week; and a course average below 70 percent, respectively. 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 67% 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 estimated using 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

[1] Strata also include a binary indicator for participation in a prior study that involved providing information to parents about their parent-portal account.

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 her 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

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

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

[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.

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, but this ethnic group comprises only 1% of the overall sample. When we regress our treatment indicators on all baseline covariates 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 more 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. For context, we used the three-digit prefixes to determine whether the phone numbers in the district's student information were cell phones. We found that roughly half of the phone numbers were cell phones. By condition, fifty-two percent of parents in the Automatic Enrollment condition received at least one alert, 7 percent in the Simplified condition received at least one alert, and less than 1 percent of the parents in the Standard condition received at least one alert (see Table V). In terms of frequency, 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

Though not all parents in the initial experiment universe enrolled to receive alerts, and not all of those who enrolled ever received alerts, we first show results from an intent-to-treat (ITT) analysis. Table VI reports OLS estimates of equation (1). Column (1) shows that the average

[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, though very few did so.

second semester GPA for the Control condition was 1.89. Assignment to the Automatic Enrollment condition increased average GPA by nearly 0.07 points, or about 3%. Assuming each student takes five courses, a 0.07 GPA point effect is equivalent to 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.

In Table VII we show two-stage-least-squares 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 (Table VII, column (4)). Both effects decrease slightly, but remain significant at the 5% level when including a set of baseline controls (Table VII, columns (3) and (5)).

The results in Tables VI-VII show 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. In results not shown, our findings are robust to excluding students who transferred schools within the district and including 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. 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 A, 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. The effects for middle school students are substantially smaller and not statistically significant. This aligns with results described in a more recent study by Bergman and Chan (2017), which found similarly large effects of this intervention for high school students compared to middle school students.

D. 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 by asking parents if they would be interested in signing up for the service 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 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. Nonetheless, the vast majority of parents (77%) who received at a message received the correct number of two messages—one interest elicitation message, and one discontinue message—as intended. Nor does receiving the incorrect number of messages correlate with any of the treatment conditions. We exclude the control group (who did not receive a message), and we regress an indicator for receiving exactly two messages on baseline covariates and our treatment condition. The results presented in Table VIII show that there was no significant difference in the likelihood of receiving both messages as intended across treatment groups, nor across most baseline covariates. Given the few numbers of parents in the Standard condition who enrolled in the alert system, this group effectively serves as an alternative reference group and we exclude the control group from the analyses below.

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, 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. 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 given the few parents who enrolled in the alert system, robust to the inclusion of a full set of baseline controls (Table XII, column (2)). Those in the Simplified condition were about 6 percentage points more likely to reply "YES" to the interest elicitation text message, which is 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, which suggests 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 speculate 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. SURVEY RESULTS

Given our findings above, which show how opt-in enrollment—even when simplified—dramatically affects take up, we sought to understand how decision makers implement this type of technology and why they may not leverage behavioral tools like strategic defaults. To do so, we conducted an online survey of superintendents, principals, administrators, and family engagement liaisons.

Respondents were drawn from two separate workshops held at Harvard University's Graduate School of Education and one Harvard executive education course, all of which were specifically for education professionals. About 300 people were enrolled across all three events, representing approximately 120 different schools and 55 different districts. These districts have a combined enrollment of over 3.2 million students. Out of these 300 attendees, 130 completed the survey. Seventy-eight percent of respondents came from urban school districts, and 13 percent from suburban. On average, respondents had about 15 years of experience in education. Although all populations show similar results, the response rate was highest in the workshop for principals, superintendents, and education leaders (e.g., chiefs of academic instruction): 60% responded. Enrollees in the second workshop and in the executive education course held positions ranging from family engagement coordinator to school nurse. As such, many participants in these sections are unlikely to be involved in purchasing and enrollment decisions, and response rates among these groups were expectedly lower—about 30%.

Participants were asked several questions analogous to the experimental design. We asked participants to estimate the percentage of parents who would enroll in an automated, text-message alert system under each enrollment condition: standard, simplified, and automatic. Participants were then asked to estimate the effect this program would have on student GPA and course failures under each of the three enrollment methods. After describing the results of the experiment—enrollment and efficacy under each condition—we asked participants to provide their willingness to pay for the technology under each enrollment condition. Lastly, we asked participants whether they had such a technology in their district already and how they enrollment families.

Questions were grouped into blocks that corresponded to one of the three enrollment conditions. The order in which the three blocks were shown was randomized, but questions appeared in the same order within each block. The willingness to pay questions were asked last, and the order of the three questions in this section was also randomized. Appendix Table B1 shows the exact language of each question.

We find that respondents have severe misperceptions about opt-in and opt-out enrollment rates. Figure 2 shows our results. While respondents correctly predicted that easier enrollment methods would result in increased participation, they overestimated enrollment for both opt-in conditions by roughly 40 percentage points. At the same time, participants underestimated enrollment for the opt-out condition by 31 percentage points.

Respondents also overstate the efficacy under opt-in enrollment. Table B2 shows that respondents believed the standard opt-in group would experience a 0.05-point increase in GPA and a 17 percent decrease in course failures, while students in the simplified opt-in group would see a 0.06-point increase in GPA and a 19 percent decrease in course failures. Although respondents accurately predicted that effects would be largest in the automatic enrollment group, the difference between participants' estimated effects for the automatic versus standard enrollment groups was only 0.02-points for GPA, and 6 percentage points for course failure. This is far less than the difference of 0.07 GPA points and 10 percentage points for course failures that we found in the experiment.

After participants viewed the take-up and efficacy results from the experiment, they were asked their willingness to pay for the technology. Table B3 shows this self-reported willingness to pay under each condition. Under automatic enrollment, respondents are willing to pay 140% more for the technology than under the standard opt-in condition.

Our results do not differ by the level of decision-maker; we find similar patterns among each survey group (results available on request). In talking with the text-message technology company we worked with to implement the experiment, as well as DCPS, the latter group of lower-level decision makers is unlikely to have much input on purchasing decisions or roll out, unlike principals and superintendents.

IV. CONCLUSION

We present a field experiment and a complementary survey examining three principal research questions. First, how does the strategy used by an organization to implement a new technology affect end-user adoption of the technology? Second, how does the strategy used by an organization to implement a new technology affect its overall impact? And third, do policymakers anticipate the impact of these implementation decisions? 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 on both 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 very few parents enrolled. For similar reasons, the simplified implementation strategy did not cause statistically significant improvements in student performance either. However, automatically enrolling parents generated statistically significant improvements in student achievement at low cost.

These results have important implications. First, the way in which 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 analysis regarding 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 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 increases, on average, as reflected in their desire to opt-in for the following year.

The fact that key school district leaders underestimate the impact of automatic enrollment may help explain why many promising technologies have less demand than expected. For example, in the largest district in the US, New York City Department of Education, a $95 million program to make student data more accessible and useful was abandoned because so few parents and teachers used it (Chapman, 2014). Our research suggests that how it was implemented and presented to users might have increased adoption. Moreover, our findings suggest that domain experience and expertise may not result in accurate knowledge about constituent behavior change and adoption decisions. Consequently, it may be of value to incorporate behavioral science tools into leadership training.

[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. Actual enrollment from experiment vs. predicted enrollment from survey, by method of enrollment

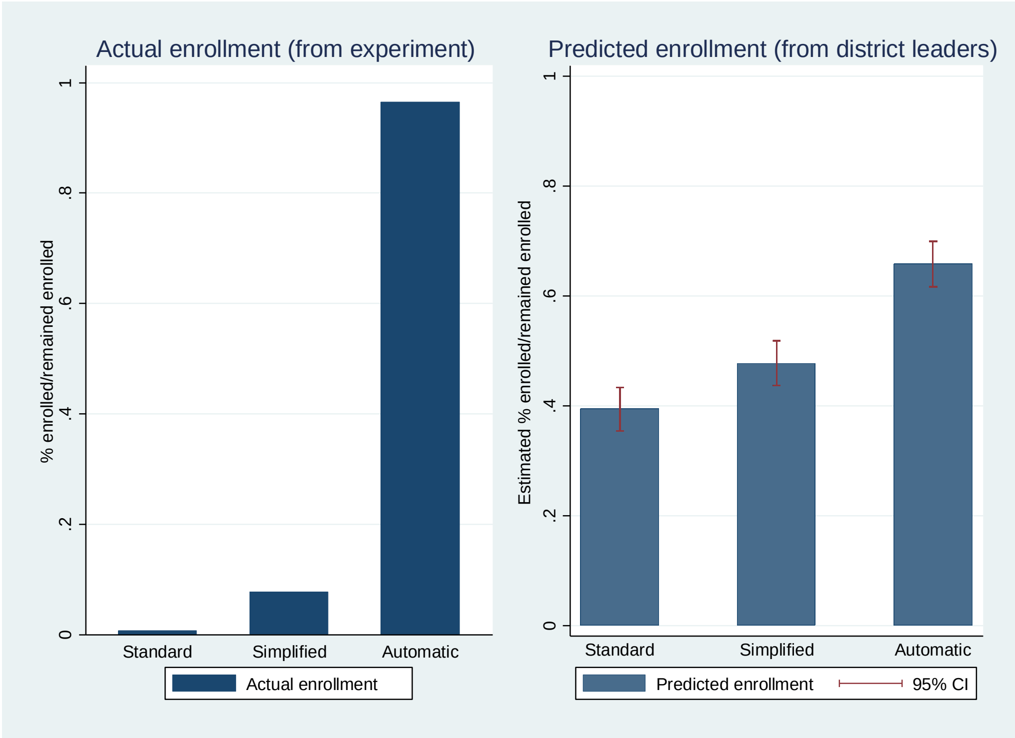
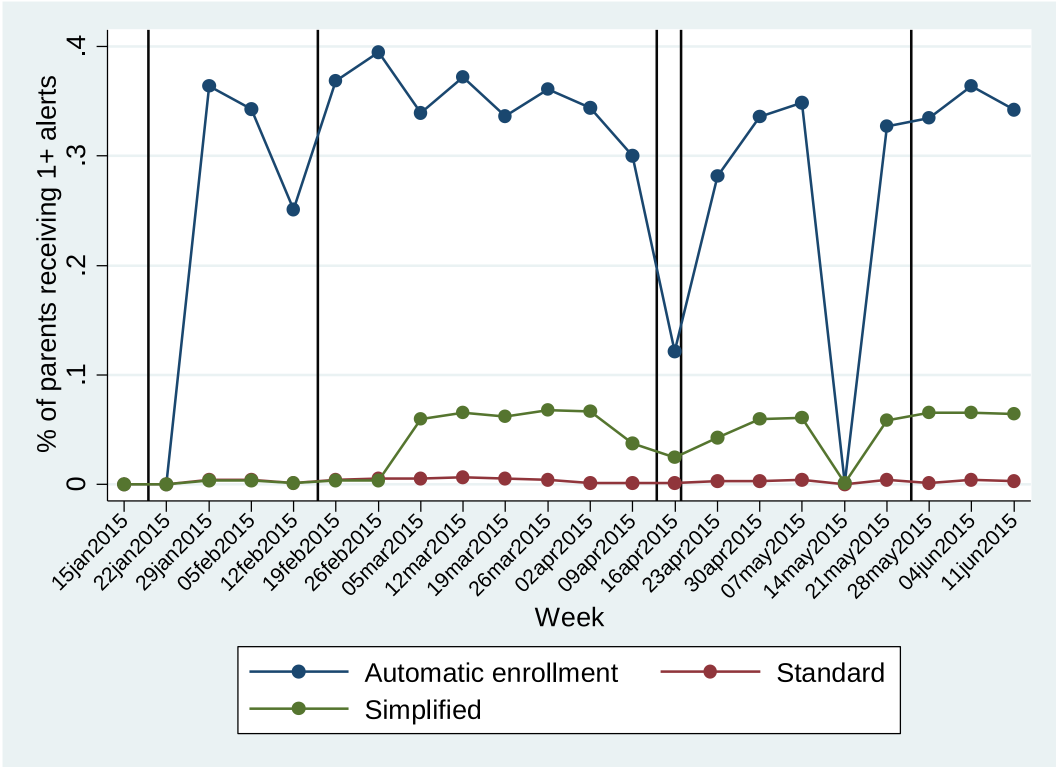


Figure 3. Percentage of parents in each condition who received alerts each week



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	215 (8.0%)	225 (8.3%)	64 (7.8%)	47 (6.1%)	0.23

Table II. Pre-intervention summary statistics

Factor	(1) Control	(2) Automatic Enrollment	(3) Simplified	(4) Standard	(5) 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	58
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%)	13 (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	10.0	.18

[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	1,137	354	339
Percent delivered exactly 1 interest elicitation text message and 1 "discontinue" message	42.0%	42.9%	43.8%
Number responded to interest elicitation text message	192	67	47
Percent responded to interest elicitation text message	13.4%	14.0%	10.3%

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. Heterogeneity: middle vs. high school

Table A1. 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.

Appendix B. Survey results

Table B1. Survey question text

Question number	Question text
Block 1: standard enrollment	
1	Imagine that you used phone numbers from the district's student information system to send parents a text message letting them know they could enroll in this program by signing up via an online parent portal. **What percent of parents would enroll?**
2	Given this enrollment process, by what percent would this program reduce **the total number of Fs** received by all students whose parents were offered the chance to receive these text messages (regardless of whether or not the parents ended up enrolling to receive them)?
3	Given this enrollment process, by how much would this program increase **GPA** for all students whose parents were offered the chance to receive these text messages (regardless of whether or not the parents ended up enrolling to receive them)?
Block 2: simplified enrollment	
1	Imagine that you used phone numbers from the district's student information system to send parents a text message letting them know they could enroll in this program by texting "START." **What percent of parents would enroll?**
2	Given this enrollment process, by what percent would this program reduce **the total number of Fs** received by all students whose parents were offered the

	chance to receive these text messages (regardless of whether or not the parents ended up enrolling to receive them)?
3	Given this enrollment process, by how much would this program increase **GPA** for all students whose parents were offered the chance to receive these text messages (regardless of whether or not the parents ended up enrolling to receive them)?
Block 3: automatic enrollment	
1	Imagine that you used phone numbers from the district's student information system to send parents a text message letting them know they would be automatically enrolled in this program unless they texted back "STOP" at any time. **What percent of parents would remain enrolled throughout the year?**
2	Given this enrollment process, by what percent would this program reduce **the total number of Fs** received by all students whose parents were offered the chance to receive these text messages (regardless of whether or not the parents ended up enrolling to receive them)?
3	Given this enrollment process, by how much would this program increase **GPA** for all students whose parents were offered the chance to receive these text messages (regardless of whether or not the parents ended up enrolling to receive them)?
Willingness to Pay (presented in random order)	
1	Imagine that allowing parents to enroll in this program by signing up via an online parent portal results in <1% of the parents in your school district enrolling to receive the text messages. **How much would you be willing to pay for this technology for an entire school (i.e., all families would have access to the technology)?** Note: Your estimate should count all students regardless of their enrollment in the program.
2	Imagine that allowing parents to enroll in this program by texting "START" results in 7% of the parents in your school district enrolling to receive the text messages. **How much would you be willing to pay for this technology for an entire school (i.e., all families would have access to the technology)?** Note: Your estimate should count all students regardless of their enrollment in the program.
3	Imagine that automatically enrolling parents in this program and allowing them to stop by texting "STOP" results in 96% of the parents in your school district remaining enrolled to receive the text messages. **How much would you be willing to pay for this technology for an entire school (i.e., all families would have access to the technology)?** Note: Your estimate should count all students regardless of their enrollment in the program.

Table B2. Predicted effects on GPA and course failures from survey vs. actual effect from study

	Predicted effect (from survey)	Actual effect (from experiment)
Point increase in GPA		
Standard enrollment	0.05	0.00
Simplified enrollment	0.06	0.00
Automatic enrollment	0.07	0.07
% decrease in course failures		
Standard enrollment	16.7%	0%
Simplified enrollment	18.8%	0%
Automatic enrollment	22.5%	10%

Table B3. Willingness to pay, by enrollment method (from survey)

Method	Amount
Standard enrollment	$1.12
Simplified enrollment	$1.60
Automatic enrollment	$2.73

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**