Simplification and Defaults Affect Adoption and Impact of Technology, But Decision Makers Do Not Realize This

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ABSTRACT

A field experiment examines how enrollment defaults affect the take-up and impact of an education technology (N=6,976). It shows that a standard (high-friction) opt-in process induces extremely low parent take-up (<1%), while a simplified process yields higher take-up (11%), but both fail to reliably improve student achievement. Automatically enrolling parents increases adoption (95%) and improves student achievement—e.g., one in four students does not fail a class they would have otherwise failed. Surveys show automatic enrollment is uncommon, and its impact is underestimated: District leaders *overestimate* take-up under standard opt-in processes by about 40 percentage points and *underestimate* take-up under automatic enrollment by 29 percentage points. After learning the actual take-up rates, district leaders report being willing to pay substantially more for the technology when implemented under automatic enrollment than standard opt-in.

Keywords: Defaults; Education; Human behavior

Acknowledgements: We thank Hunt Allcott, David Deming, Brigitte Madrian, and Andrei Schleifer for feedback on the manuscript. We thank Vincent Baxter and Natalie Foglia at the District of Columbia Public Schools, as well as Spencer Kier and Alex Farivar at Engrade for their collaboration. We thank Josefa Aguirre for data assistance, and the Student Social Support R&D Lab at Harvard Kennedy School for general support, and the Silicon Valley Community Foundation and Laura and John Arnold Foundation for financial support.

Many potent technologies suffer from low end-user adoption. The experiment reported in this paper studies how simplifying the enrollment process and changing the default enrollment for a new technology affects end-user take-up and subsequent behavior. To understand why decision makers often fail to offer promising technologies in ways that maximize adoption, we explore their underlying beliefs about how enrollment processes affect end-user take-up.

Automatic enrollment is one lever decision makers can use to affect enrollment in programs. Making enrollment automatic—the default option—can impact involvement in programs ranging from retirement saving (Madrian & Shea, 2001) to organ donation (Johnson & Goldstein, 2003). Defaults can affect behavior by influencing how end-users interpret their options. Default options can be interpreted as implicit recommendations (McKenzie, Liersch, & Finkelstein, 2006), and actively choosing to opt-out of a default option can mean something radically different than making the choice to opt-in to that same option (Davidai, Gilovich, & Ross, 2012). Moreover, making the impact of automatic enrollment transparent to users does not appear to mitigate its effect (Loewenstein, Bryce, Hagmann, & Rajpal, 2015; Steffel Williams, & Pogacar, 2016; Burns, Kantorowicz-Reznichenko, Klement, Jonsson, & Rahali, 2016). There is little evidence, however, of the impact of automatic enrollment on the take-up and impact of programs and technologies that require end-users to continually modify their behavior after enrolling (see Fowlie et al., 2017).

Another lever decision makers can use to affect program take-up is to reduce the barriers to enrollment (Sunstein, 2013). For example, using data collected from tax forms completed on behalf of low-income families to simplify the Free Application for Federal Student Aid (FAFSA) application by auto-completing its form contents 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 listing the most cost effective plans for end-users increased plan switching and reduced end-user costs compared to sending a mailer that simply listed the website where this information was available (Kling, Mullainathan, Shafir, Vermeulen, & Wrobel, 2012).

In this paper, we examine how enrollment defaults and enrollment simplification affect the take-up and impact of a novel technology that aims to help parents improve student achievement. 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). We also document key education decision makers' beliefs about how enrollment defaults and simplification affect take-up and efficacy of this technology, as well as their subsequent willingness to pay for it.

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 they implement new technologies affects their overall impact.

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 behavior, 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, 2014; Mayer et al., 2015). However, as described above, the ability to successfully scale these interventions in schools depends on 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 (SIS). Phone numbers are retrieved from the SIS as well, and subsequently the academic information is automatically texted to parents who are enrolled and who have valid cell phone numbers. Each alert is sent on a different day of the week.

To understand how enrollment defaults and simplification affect parental take-up of this technology and its subsequent impact on student achievement, we randomly varied how the parents of students in 12 Washington, D.C. middle and high schools could enroll in the program. Those in the Standard Enrollment condition were told by text message that they could adopt the technology by enrolling via the parent portal, which is standard practice. Those in the Simplified Enrollment condition were told by text message that they could adopt the technology by replying "start" in response to a text message. Those in the Automatic Enrollment condition were told by text message that they were enrolled by default, and could thus adopt the technology passively by not opting out; to opt out, parents could respond "stop" to any text message alert.

We demonstrate several key findings. First, reducing the frictions involved in enrolling in the technology increased take-up of the alert system. Less than 1% of parents in the Standard condition adopted the new technology, while roughly 11% of parents in the Simplified condition adopted the new technology. Second, automatic enrollment has a large effect on parent adoption of the technology, despite parents being offered many opportunities to opt out. Automatically enrolling parents resulted in 95% adoption; only 5% of parents in this condition withdrew from the technology at any point during the school year. Relative to parents in the Automatic Enrollment condition who adopted the technology, 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. This implies that default enrollment not only affected take-up rates, but also influenced the characteristics of the families who ultimately enrolled. Many school districts aim to engage families with lower-performing students; opt-in enrollment is less likely to engage these families.

Third, we find that default enrollment 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 meaningful academic gains while those whose parents were assigned to the Simplified and Standard conditions showed no reliable academic gains relative to those in the Control condition. Students in the Automatic Enrollment condition saw a 0.05-0.06 point (about 3%) increase in their GPA, and course failures were reduced by 0.2 courses per student, or about 10%. This is the equivalent of nearly one in four students not failing a class she would have otherwise. The lack of impact for students whose parents were assigned to the Standard or Simplified condition is unsurprising given the low adoption rates among parents in those conditions.

Fourth, default enrollment and 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 a similar (but ostensibly different) 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 beliefs about the efficacy of the technologies and their subsequent willingness to pay for them. 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 under standard enrollment by around 40 percentage points, and they *underestimate* the take-up rate under automatic enrollment by 29 percentage points. After learning the actual take-up rates under each enrollment condition, there is a corresponding 144% increase in the willingness to pay for the technology when shifting implementation from standard opt-in enrollment to default 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. Overview of experiment

Design and sample

The experiment took place in Washington, D.C. The District is divided into eight administrative wards, all served by the District of Columbia Public School (DCPS) system. DCPS had 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, are spread across six of the eight wards, and are relatively under-performing compared to other DCPS middle and high schools. 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.

The 12 middle and high schools included in this study were selected by DCPS to pilot the text message parent alert system, which was part of the Engrade platform. 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 system online, but they were not sent any communication informing them that the service was available.

2. **Standard Enrollment:** 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 Enrollment:** 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 system 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 or students 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. If we had made all treatment groups equal size, with incomplete take-up, our minimum detectable effect (MDE) for GPA would have been around 0.6 GPA points, which is an unrealistic expected effect. 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 in the Automatic Enrollment condition.

We randomized at the student-level, which means that some siblings were assigned different conditions. Without data on student addresses we cannot precisely determine which students are siblings. However, our sample universe included 1,532 students who shared a parent phone number with at least one other student. Using parent phone number as a proxy for household, we estimate that these 1,532 students came from 736 households. In 536 of these cases, these presumed siblings were randomized to different conditions (see SOM). We run all analyses excluding presumed siblings who were randomized to different conditions as a robustness check (see SOM).

Procedure

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% 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 system 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 system, 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%, respectively. Figure 1 shows the full text of each message.

Cell phones

The automatic text message parent alert system 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. We used the three-digit prefixes to determine whether the phone numbers in the district's student information system (SIS) were valid cell phones. However, some parents who we believed had valid cell phone numbers did not receive the initial enrollment message as intended and, conversely, others received the message despite having what we believed to be a landline number in the SIS. Thus, we use a combination of the initial cell phone indicator and the list of parents who received an enrollment message to develop a proxy indicator for who had a valid cell phone (see SOM for details).

Based on this proxy indicator, we estimate that approximately 67% of our sample had valid cell phone numbers, balanced evenly across treatment conditions (see Table I). Students in this presumed cell phone universe had a significantly lower average baseline GPA and had significantly more prior absences compared to students whose parents did not have valid cell phone numbers (see Table II). This is consistent with anecdotal evidence that districts reach out to parents of lower performing students more often than to parents of higher performing students, and thus may make more of an effort to obtain and maintain up-to-date parent contact information for these students.

Outcome measures and data

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.

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 system. Second, we examine how implementation strategy (as reflected in condition assignment) affects student academic performance. For the latter, we use two measures of academic performance: the number of courses a student fails, and average semester grade point average (GPA).

As noted above, we estimate that about 33% of our sample universe did not have valid cell phone numbers at the time of randomization, and were thus unable to receive the initial enrollment message or enroll in the alert system. As a result, we structure our analysis to present the most conservative estimates. When evaluating how implementation strategy affects adoption, we limit our analysis to only those students who we presume to be part of the cell phone universe based on our proxy indicator.

Subsequently, we estimate the causal effect of condition assignment on student academic performance with an intent-to-treat (ITT) OLS model that utilizes the full randomized universe and regresses the outcome variable on a vector of indicators for assignment to one of the three treatment groups or the control condition. All models are run with robust standard errors, and include strata indicators as controls.[1] We also include pre-intervention student-level covariates, as detailed in Table VII notes.

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 her third and fourth term GPAs. The full conversion scale for numeric and letter grades can be found in Appendix C.

We also 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.

Sample

As shown in Table III, about 80% of our sample was Black or African-American, and 18% was Hispanic. On average, students' baseline GPA was 1.86, and 30% 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 6.6%. Column (5) shows that we cannot reject the null hypotheses of no difference between the four condition groups for all observable characteristics.

We received outcome data for 90.2% of our sample. Seventeen students (0.2%) could not be found in the Engrade system at the end of the study period; 667 students were present in the system, but did not have any third or fourth term grade information. In both cases, we assume that these students dropped out or transferred out of DCPS. Attrition was balanced evenly across treatment conditions, as shown in Table I. Eight percent of the students in our sample also transferred schools within the district during the course of the study. The primary analysis

[1] The strata used for randomization 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), a binary indicator for pre-intervention low attendance, and a binary indicator for participation in a prior study that involved providing information to parents about their parent-portal account.

includes all students for whom we received outcome data, regardless of whether they transferred schools.

II. RESULTS

User adoption

As shown in Figure 2, about 11% of parents who were assigned to the Simplified condition and are part of the presumed cell phone universe ultimately enrolled to use the technology, whereas 95% of parents who we believe had valid cell phones and were assigned to the Automatic Enrollment condition remained enrolled throughout the course of the study. Table IV shows that students of parents assigned to the Standard and Simplified conditions who enrolled had a 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 higher among those who actively enrolled in the Standard and Simplified conditions 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.

Of the 2,874 parents who we believe had valid cell phones and were assigned to one of the three treatment conditions, we sent alerts to 1,403 or about 49%. In total, we sent 27,182 alerts.[2] Ninety-six percent of the alerts went to parents in the Automatic Enrollment condition (see Table V). The distribution of alert types was similar for those who enrolled in the Automatic Enrollment and Simplified conditions.

By condition, 76% of parents with cell phones in the Automatic Enrollment condition received at least one alert, 10% in the Simplified condition received at least one alert, and less than 1% of the parents in the Standard condition received at least one alert (see Table VI). In terms of frequency, about 40% of parents in the Automatic Enrollment condition and about 5% 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 20 alerts over the course of the semester. Parents in the Standard condition who enrolled and received at least one alert received an average of 9 alerts throughout the study.

[2] In addition, we sent alerts to 13 parents in the Control condition, as we could not prevent parents from enrolling in the alert system. Thus, some parents found out about the alert system from other sources and enrolled on their own via the parent portal, although very few did so.

Of the 1,463 treatment condition parents who received one or more subsequent alerts, 1,415 (97%) received at least one absence alert, 1,150 parents (79%) received at least one missing assignment alert, and 1,165 (80%) received at least one low grade alert.

GPA and Course Failures

Table VII reports the results of regression analyses examining the effect of treatment assignment on second semester GPA and course failures among the full randomized universe. Assignment to the Automatic Enrollment condition increased average GPA by about 0.07 points, or about 3%, over the control group mean of 1.89 (Column 1). Adding 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, reduces the treatment effect slightly to 0.05 points, but improves the precision of the estimates (Column 2).

Assignment to the Automatic Enrollment condition reduced the number of courses failed by .23 courses, or about 10%, from the Control-condition mean of 2.4 courses failed (Column 3). 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 covariates reduces the treatment effect observed for those assigned to the Automatic Enrollment condition slightly to 0.21 courses, but increases the precision of the estimates (Column 4).

The results in Table 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. The effects for both conditions that required parents to actively enroll (Standard and Simplified) are small and not statistically significant in every model. Our findings are robust to excluding siblings who were randomized to different conditions (see SOM).

Given that 33% of parents in each condition did not receive the initial enrollment message and consequently did not have an opportunity to enroll in the alert system, these ITT estimates are conservative. As expected, we find slightly larger effects when we limit the analytic universe to only those who we presume had valid cell phone numbers. In the presumed cell-only universe, we find that assignment to the Automatic Enrollment condition resulted in a 0.09 point (5%) increase in GPA and a 0.28 point decrease (11%) in classes failed. When controlling for a set of baseline covariates, these estimates decrease slightly to 0.07 points and 0.26 points, respectively, but remain highly significant (see SOM).

Heterogeneity

We evaluated the effect of treatment assignment on our two primary academic outcomes for two subgroups—middle school and 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 regression-adjusted Control condition mean of 1.78 points. 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%. Including baseline controls reduces point estimates slightly, but all effects remain significant at the 5% level. There was no effect of treatment on GPA for middle school students assigned to the Automatic Enrollment condition, but we do find reduction in courses failed of about 8%. This result is not robust to the exclusion of controls, however. Overall, this aligns with results described in a recent study by Bergman and Chan (2017), which found similarly large effects of this intervention for high school students compared to middle school students. Again, we find slightly larger effects when we look only at students who are part of the cell phone universe (see SOM).

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 technology by asking parents if they would be interested in signing up for a similar 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 bias, 262 parents in the presumed cell phone universe who were assigned to one of 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." These 262 parents were selected at random from the three treatment conditions. There was no significant difference in observables for those who received the placebo message (see Table VIII), although parents in the Automatic Enrollment condition were significantly less likely to receive a message. As shown in Table IX, the majority of parents in the Automatic Enrollment group who received the placebo message had remained enrolled in the alert system throughout the study (94%), while the majority of parents in the Simplified and Standard groups who received the placebo message had not actively enrolled in the alert system (88% and 100%, respectively). Response rates to the placebo message were compared to evaluate non-responsiveness across treatment groups.

Subsequently, we sent 2,319 parents who were assigned to one of 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 parents 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% of the messages intended for parents in the treatment conditions. At the same time, some parents were inadvertently sent up to four copies of the same message. Nonetheless, the majority of parents (63%) who received at least one message received the correct number of messages—one interest elicitation message, and one discontinue message—as intended. We exclude the control group (most of whom 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 X show that there was no significant difference in the likelihood of receiving both messages as intended across treatment groups, nor across any 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 over 11% and 21% of parents in the Automatic Enrollment and Simplified conditions respectively answered the placebo text message, while 27% 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, as shown in Table XI. We find that those in the Automatic Enrollment condition were about 9 percentage points less likely to respond to the placebo message compared to those in the Standard condition, and those in the Simplified condition were 10 percentage points less likely to respond to the placebo text. Although these effects are not statistically significant, 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 and are part of the presumed cell phone universe, 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 XII. About 16% of parents in the Automatic Enrollment condition and the Simplified condition responded "yes" to the interest elicitation text message. Only 11% of the Standard condition responded "yes."

Linear probability estimates presented in Table XIII 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, 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 5% 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 4.6 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 (see SOM).

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 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 take-up under opt-in enrollment—even when simplified—is dramatically lower than under default enrollment, 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 a 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% from suburban. On average, respondents had about 15 years of experience in education. Although all populations show similar results, the response rate was highest among 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, if so, how they enroll 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 take-up under opt-in and default enrollment strategies. 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 Automatic Enrollment condition by 29 percentage points.

Respondents also overstate the efficacy of the technology 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% decrease in course failures, while students in the simplified opt-in group would see a 0.06-point increase in GPA and a 19% 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 144% 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 (see SOM). In talking with Engrade, as well as DCPS, lower-level decision makers are 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 the achievement gap 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 enrollment process increases adoption, and automatically enrolling end-users dramatically increases adoption. The standard enrollment strategy did not improve student performance, which is not surprising since very few parents enrolled (<1%). For similar reasons, the simplified implementation strategy did not cause statistically significant improvements in student performance either (although it did increase adoption to 11%). However, automatically enrolling parents in the alert system generated statistically significant improvements in student achievement (and increased adoption to 95%).

These results have important implications. First, the way in which an organization implements a new technology can lead it to draw 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 greater friction in the enrollment process leads parents of children with higher baseline achievement to be relatively more likely to enroll 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.[3] 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 there is less demand than expected for many promising technologies. For example, in the largest district in the US, the 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 affected its adoption. Moreover, our findings suggest that domain experience and expertise may not result in accurate knowledge about constituent adoption and behavior change decisions. Consequently, it may be of value to incorporate behavioral science tools into leadership training.

[3] 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

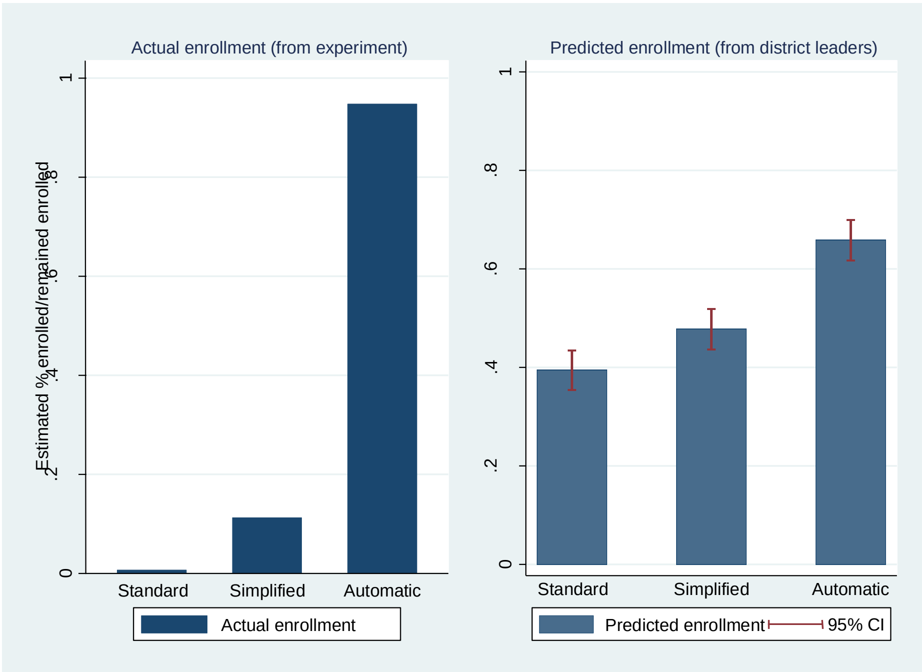
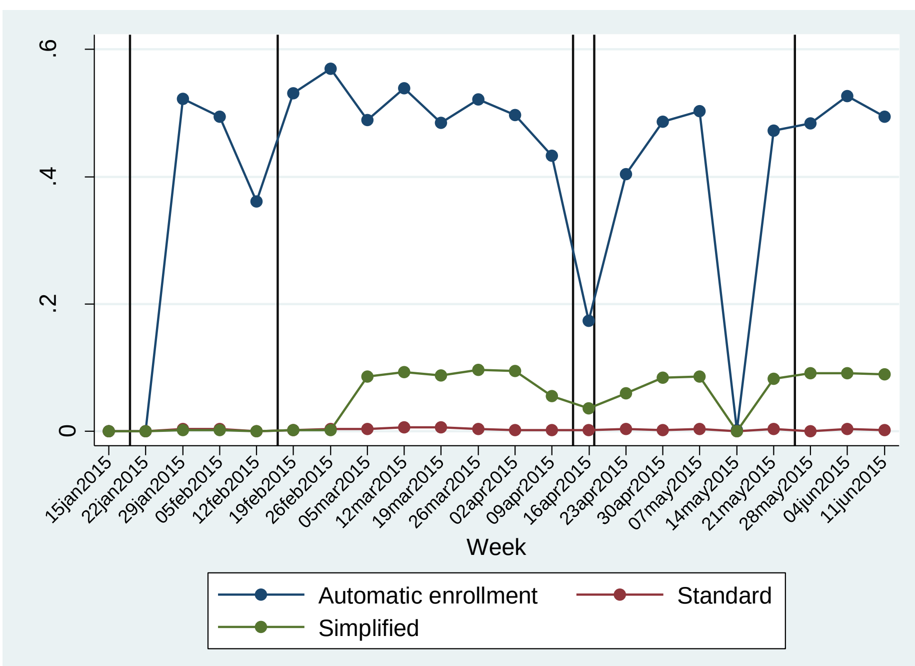


Figure 3. Percentage of parents in each condition who received alerts each week



Notes: Vertical black lines indicate school holidays, including spring break (April 13-17, 2015). Messages the week of May 14th, 2015 failed to send.

Table I. Sample size

Factor	Control	Automatic Enrollment	Simplified	Standard	p-value
N	2673	2705	825	773	
Cell phone	1777 (66.5%)	1790 (66.2%)	558 (67.6%)	526 (68.0%)	0.72
Attrition	258 (9.7%)	286 (10.6%)	77 (9.3%)	64 (8.3%)	0.26

Notes: *p*-values calculated using Fisher's Exact Tests.

Table II. Summary statistics: cell phone universe vs. non-cell phone universe

Factor	No cell phone	Cell phone	p-value
N	2325	4651	
Female	1132 (48.7%)	2233 (48.0%)	0.61
Black	1901 (81.9%)	3727 (80.2%)	0.087
White	71 (3.1%)	47 (1.0%)	<0.001
Asian	41 (1.8%)	38 (0.8%)	<0.001
Hispanic	295 (12.7%)	813 (17.5%)	<0.001
Fraction of missing assignments, median	.057 (.014, .137)	.067 (.015, .153)	0.003
Ever logged into parent portal	805 (34.6%)	1378 (29.6%)	<0.001
Grade 6	404 (17.4%)	775 (16.7%)	0.46
Grade 7	428 (18.4%)	763 (16.4%)	0.036
Grade 8	437 (18.8%)	755 (16.2%)	0.008
Grade 9	653 (28.1%)	1510 (32.5%)	<0.001
Grade 10	202 (8.7%)	474 (10.2%)	0.048
Grade 11	160 (6.9%)	300 (6.5%)	0.51
Grade 12	38 (1.6%)	72 (1.5%)	0.84
Pre-intervention absences, median (IQR)	14 (6, 31)	16 (7, 36)	<0.001
Pre-intervention GPA, mean (SD)	2.02 (1.11)	1.86 (1.11)	<0.001

Notes: All *p*-values calculated using Fisher's Exact Tests except for fraction of missing assignments and pre-intervention absences, both of which use a Wilcoxon rank-sum test, and pre-intervention GPA, which is calculated using a two-sample t-test.

Table III. 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)	.061 (.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 (1.07)	0.94

Note: All *p*-values calculated using Fisher's Exact Tests except for fraction of missing assignments and pre-intervention absences, both of which use Kruskal-Wallis tests, and pre-intervention GPA, which is calculated using an ANOVA.

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

	Automatic Enrollment	Simplified	Standard	p-value
# assigned to treatment	2,705	825	773	
# who received initial enrollment message	1,790	558	526	
# remained enrolled/# actively enrolled	1,697	63	4	
% remained enrolled/% actively enrolled	94.8%	11.3%	0.8%	<.001
Pre-intervention GPA for those who remained enrolled/actively enrolled	1.89	2.14	2.41	.13
Percent of parents who had ever logged into parent portal prior to intervention for those who remained enrolled/actively enrolled	31.3%	39.7%	100.0%	.006

Note: All *p*-values calculated using Fisher's Exact Tests except for pre-intervention GPA, which uses an ANOVA.

Table V. Number of alerts sent[4]

	Automatic Enrollment	Simplified	Standard
# who received initial enrollment message	1,790	558	526
Total number of alerts sent	26,020	1,129	33
Average number of alerts sent per student (who rec'd initial msg)	14.5	2.0	0.1
Number of missing assignment alerts	6,675 (25.7%)	409 (36.2%)	5 (15.2%)
Number of absence alerts	9,910 (38.1%)	288 (25.5%)	25 (75.8%)
Number of low grade alerts	9,435 (36.3%)	432 (38.3%)	3 (9.1%)

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

	All treatment conditions	Automatic Enrollment	Simplified	Standard	p-value
# assigned to treatment	4,303	2,705	825	773	
# who received initial enrollment message	2,874	1,790	558	526	
Number of parents who received 1+ alerts	1,403	1,343	56	4	
Percentage of parents who received 1+ alerts	48.8%	75.0%	10.0%	0.8%	<.001
Average number of alerts received for those who received at least 1 alert	19.3	19.4	20.2	8.8	.20
Number/percent of parents who received 1+ alerts, by type					
Missing assignment	1,107 (78.9%)	1,051 (78.3%)	51 (91.1%)	3 (75%)	
Absence	1,356 (96.7%)	1,303 (97.0%)	49 (87.5%)	3 (75%)	
Low course grade	1,119 (79.8%)	1,060 (78.9%)	54 (96.4%)	3 (75%)	

Note: *p*-value for percentage of parents who received 1+ alerts calculated using Pearson's Chi-square Test; for average number of alerts received, *p*-value comes from an ANOVA.

[4] Summary statistics on alerts do not include those sent by schools that turned on the text message parent alert system school-wide.

Table VII. Primary academic outcomes

	(1)	(2)	(3)	(4)
VARIABLES	GPA	GPA	# classes failed	# classes 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	1.887	2.435	2.435

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 VIII. Placebo message balance

	(1)	(2)
VARIABLES	Sent placebo msg	Sent placebo msg
Automatic Enrollment	-0.057***	-0.057***
	(0.016)	(0.016)
Simplified	0.010	0.010
	(0.021)	(0.021)
Baseline GPA		-0.010
		(0.010)
# portal log-ins		-0.000***
		(0.000)
Absences		-0.000
		(0.000)
Black		-0.005
		(0.014)
Observations	2,874	2,874
R-squared	0.043	0.045
Controls	Yes	Yes
Mean for Control	0.123	0.123

Notes: OLS estimates. Dependent variable is binary indicator where 1 indicates successful delivery of the placebo message. Reference group is Standard condition. Controls and covariates detailed in Table VIII 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)
# assigned to treatment	2,705	825	773
# who received initial enrollment message (presumed cell phone universe)	1,790	558	526
# sent placebo message	122	76	64
% rec'd placebo message	6.8%	13.6%	12.2%
# rec'd placebo message who had stayed enrolled/actively enrolled	115	9	0
% rec'd placebo message who had stayed enrolled/actively enrolled	94.3%	11.8%	0.0%
# responded to placebo message	14	16	17
% responded to placebo message	11.4%	21.1%	26.6%

Table X. 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	-0.006	-0.005	-0.005	-0.003
	(0.024)	(0.024)	(0.026)	(0.026)
Simplified	-0.000	0.003	0.007	0.010
	(0.030)	(0.030)	(0.032)	(0.032)
Sent placebo	0.004	0.004		
	(0.032)	(0.032)		
Baseline GPA		-0.011		-0.005
		(0.016)		(0.017)
# portal log-ins		-0.000		-0.000
		(0.000)		(0.000)
Absences		-0.000		-0.000
		(0.000)		(0.000)
Black		-0.033		-0.033
		(0.024)		(0.025)
Observations	2,874	2,874	2,616	2,616
R-squared	0.033	0.039	0.035	0.040
Controls	Yes	Yes	Yes	Yes
Mean for Control	0.634	0.634	0.633	0.633

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 VIII notes. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Table XI. Linear probability estimates of placebo message response

VARIABLES	(1) Placebo response	(2) Placebo response
Automatic Enrollment	-0.087	-0.085
	(0.077)	(0.078)
Simplified	-0.107	-0.104
	(0.074)	(0.077)
Baseline GPA		0.055
		(0.051)
# portal log-ins		-0.003**
		(0.001)
Absences		0.000
		(0.001)
Black		0.002
		(0.083)
Observations	258	258
R-squared	0.221	0.239
Controls	Yes	Yes
Mean for Effective Control	0.296	0.296

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 XII. Interest elicitation text messages sent and response rates

	Automatic Enrollment	Simplified	Effective control (Standard)
N	1,790	558	526
Number sent at least 1 interest elicitation text message	1,396	471	452
Number delivered exactly 1 interest elicitation text message and 1 “discontinue” message	1,125	353	335
Percent delivered exactly 1 interest elicitation text message and 1 “discontinue” message	62.9%	63.7%	63.3%
Number responded “yes” to interest elicitation text message	191	67	47
Percent responded “yes” to interest elicitation text message	15.5%	16.4%	11.3%

Table XIII. 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	0.042**	0.039*	0.042*	0.038*
	(0.021)	(0.021)	(0.022)	(0.022)
Simplified	0.060**	0.054**	0.067**	0.059**
	(0.027)	(0.027)	(0.030)	(0.029)
Sent placebo	-0.053*	-0.056**		
	(0.027)	(0.027)		
Baseline GPA		0.006		0.007
		(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		0.076***		0.084***
		(0.020)		(0.021)
Observations	1,813	1,813	1,650	1,650
R-squared	0.051	0.069	0.055	0.074
Controls	Yes	Yes	Yes	Yes
Mean for Effective Control	0.116	0.116	0.114	0.114

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 VIII notes. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Appendix A. Heterogeneity

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	1.776	1.993	1.993	2.611	2.611	2.264	2.264

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 VIII 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**