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Simplification and defaults affect adoption and impact of technology, but decision makers do not realize it

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ABSTRACT

A field experiment (N = 6976) examines how enrollment defaults affect adoption and impact of an education technology that sends weekly automated alerts on students' academic progress to parents. We show that a standard (high-friction) opt-in process induces extremely low parent take-up (< 1%), while a simplified process yields higher enrollment (11%). Yet, with such low take-up, both fail to improve average student achievement. Meanwhile, automatically enrolling parents increases take-up to 95% and improves student achievement as measured by GPA and course passing. The GPA of students whose parents were automatically enrolled increased by an average of 0.06 points, and one in four students did 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 by standard opt-in processes.

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 and outcomes. 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 a powerful method through which decision makers can affect take-up of 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). One mechanism through which defaults can affect behavior is by influencing how end-users interpret the meaning of an enrollment choice (Davidai, Gilovich, & Ross, 2012). For example, consider a parent communications program like the one we study in this research. On the one hand, when parents are by default enrolled in a program to receive information on their student's academic performance, they subsequently face a decision over whether they would like to stop receiving the information that the school has decided they are going to receive. In this case, one can imagine that deciding to opt-out of such communications could be construed by a parent as diagnostic of what kind of a parent one is. On the other hand, when parents are by default not enrolled, they then face a decision over whether they would like to receive additional "bonus" information, above and beyond what the school intends to send them. A decision to opt-in might be construed as relatively undiagnostic of what kind of parent one is, and instead as more of a decision about convenience, intended engagement, or how useful one believes the information will be. Default options can also be interpreted as implicit recommendations (McKenzie, Liersch, & Finkelstein, 2006). When parents are by default enrolled they may interpret enrollment as a recommendation from school leaders that the information is useful. And conversely, when they are by default not enrolled, they may perceive it as a signal that school leaders do not think the program is particularly useful or effective. Between the effect defaults have on how a decision is construed and the implicit recommendation a default entails, 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.

In addition to affecting the meaning of people's decisions, defaults can overcome choice-deferral tendencies and spur inattentive people toward a specific option if they fail to make any decision at all. This inertia cannot account for the entirety of default effects, however, since defaults can be potent even when people are compelled to be attentive and when the impact of automatic enrollment is made transparent to

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users (Loewenstein, Bryce, Hagmann, & Rajpal, 2015; Steffel Williams, & Pogacar, 2016; Burns, Kantorowicz-Reznichenko, Klement, Jonsson, & Rahali, 2018).

This research is a powerful illustration of how defaults can meaningfully impact an important outcome (student academic success), and shows that defaults can also affect repeated post-enrollment behaviors of end users. In previous research, many of the long-term outcomes shown to be influenced by defaults arise mechanically as a result of enrollment (e.g., a portion of one's income is mechanically routed to one's 401k; one's organs are automatically harvested after one's death). Yet, many important behaviors instead require repeated on-going exertions of effort and attention (e.g., exercise; medication adherence; studying). One could imagine that defaults may only be effective at enrolling people into programs that require little or no effort or attention post-enrollment. Under this scenario, programs that prompt and require later effort and attention might cause people to un-enroll, thereby undoing the benefits of default enrollment. In contrast, we find that enrollment induced by changing the default option can lead to ongoing effortful behavior change that improves socially important outcomes (see, e.g., Fowlie, Wolfram, Spurlock, Todd, Baylis, & Cappers, 2017).

Another lever decision makers can use to affect program take-up is to simplify the enrollment process (Sunstein, 2013). Difficult or high-friction enrollment processes can reduce enrollment because they may confuse people, especially those who are inattentive, and can cause present-biased people to procrastinate the work needed to enroll given the exertion of effort required. Simplifying enrollment processes can help overcome these barriers. 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). One might expect that if parents want information about their students' academic performance small frictions would be inconsequential. Yet, we find that reducing seemingly trivial frictions makes a significant difference.

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 in Washington, DC public schools (DCPS)—a relatively underperforming and economically disadvantaged population. Seventy-seven percent of the nearly 50,000 students enrolled in DCPS are economically disadvantaged, and the district has one of the lowest high school graduation rates in the country (DCPS, 2018b; NCES, 2018). 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 and on average the impact of behavioral interventions (DellaVigna & Pope, 2018), little is known about whether key decision makers can. Given how noisy outcomes are in the world and the many taxes on leader attention, it is plausible that decision makers might hold miscalibrated beliefs of the adoption implications of different implementation strategies. Calibration of their beliefs matters because how leaders implement new technologies affects their overall impact.

Emerging research finds that technology-driven information interventions can increase student success (Escueta, Quan, Nicknow, & Oreopoulos, 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, 2018; Dizon-Ross, 2019; 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, 2018; Mayer, Kalil, Oreopoulos, & Gallegos, 2018). 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 may be 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 2–3%) increase in their GPA, and course failures were reduced by 0.2 courses per student, or about 9%. This is the equivalent of nearly one in four students not failing a class she would have otherwise. The lack of average impact for students whose parents were assigned to the Standard or Simplified condition is unsurprising given the extremely 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 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 experimental design and data. Section II presents the results on adoption and academic outcomes. Section III describes our survey results, and Section IV concludes.

1. Overview of experiment

1.1. Design

The experiment took place in Washington, D.C. The District of Columbia 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 6900, 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% (DCPS, 2018a). 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—an online learning management system for K-12 schools. Sample sizes within each school ranged from 261 to 1465 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). All enrolled students at these 12 schools 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 child's school 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 an affirmative 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 via text message.

The pre-intervention status quo allowed parents to log into the portal and enroll by their own volition, as in our "Control" condition, but schools did not actively or regularly communicate any enrollment information to parents. However, four schools began sending separate absence alerts to parents before our experiment began. These alerts did not overlap with the ones sent through our study, and only 430 students received alerts from both our study and from the school-wide alerts (see SOM). Of the 430 students who received extra alerts, 413 were part of the automatic enrollment condition. We do not exclude these schools or students from the primary analysis because the messages sent differed in content, timing, and frequency from those sent through our study. All additional messages sent by the schools were class absence alerts. Although we do not examine attendance as a primary outcome, it is possible that these extra alerts influenced student behavior, thereby also affecting our primary outcomes. As a robustness check we also run our primary specifications with an added control indicator for whether students attended one of the four schools that sent out absence alerts in parallel to our intervention (see SOM).

All 6976 students in the 12 participating schools were enrolled in the study. After randomization, 1598 were assigned to the opt-in conditions—773 to the Standard condition, and 825 to the Simplified condition; 2705 were assigned to the Automatic Enrollment condition; and 2673 to the Control condition. Based on similar experiments conducted previously (see, e.g., Bergman & Chan, 2019), our ex-ante prediction was that fewer than 15% of parents assigned to the two opt-in conditions would enroll in the alert service. With such dramatically incomplete take-up, we would require a prohibitively large sample to detect a plausible effect on academic performance. Assuming 20% take-up, a sample of nearly 80,000 students would be required to detect a 0.1 point effect on GPA with 80% power. If we had maximized our power to detect effects in all three conditions by making all treatment groups equal size, our minimum detectable effect (MDE) for GPA in each opt-in group—after factoring in incomplete take-up—would have been around 0.96 GPA points, which is an unrealistically large effect. Consequently, since our power to detect meaningful effects for either of the opt-in groups would be severely limited in any scenario, 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.

While this design limits our ability to make inferences about the causal effect of receiving alerts for students assigned to the Standard and Simplified conditions, our primary focus is not on isolating the effect of alerts themselves, which has been well documented in other similar studies. Rather, our goal is to assess how implementation strategy affects take-up and average student performance. Bergman and Chan (2019) show that a similar technology is effective for parents who receive the alerts. Yet, this requires that parents enroll in the service—either actively or passively. We show that the method by which parents are offered the service has a dramatic and significant impact on its adoption and on the average impact policy-makers can except to see from use of this technology.

Random assignment took place at the student-level, and was stratified by strata comprised of gender, grade level, a binary indicator for pre-intervention low GPA (below 1.9 on a 4.0 scale), 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. Because randomization was conducted at the student-level, some siblings were assigned different conditions. Without data on student addresses we cannot precisely determine which students are siblings. However, our sample universe included 1532 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 1532 students came from 736 households. In 536 of these households, these presumed siblings were randomized to different conditions. We run all analyses excluding presumed siblings who were randomized to different conditions as a robustness check (see SOM).

1.2. 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, a website 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 31% 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. As we discuss below, less than 1% of parents in the Control condition received any alerts. Parents in the Standard condition received a text message that informed them that they could log in online to the parent portal to enroll in the service, and that provided information on 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 Fig. 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. The first alerts were sent on January 30, 2015, at the beginning of the second semester. 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. Fig. 1 shows the full text of each message.

1.3. 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 phone numbers. 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 1). 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 SOM). 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.

1.4. 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). We focus on these two academic outcomes as they are of primary importance to students and decision makers, and to reduce the number of outcomes examined given the broader focus of our study on the effect of each treatment on take-up. Bergman and Chan (2019) present a comprehensive pre-registered analysis of the impact of text alerts on more specific academic outcomes.

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 (see SOM). Assessing adoption among the full randomized sample instead requires making an assumption about the behavior of users who did not have valid cell phones and did not receive the initial enrollment message. Assuming all users without valid cell phones remained enrolled in the Automatic Enrollment group and remained un-enrolled in the Simplified and Standard groups yields over- and underestimates of enrollment, respectively. We show these calculations in the SOM, but here we limit our analytic sample to only those users who we presume had cell phones in order to provide the most accurate

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.

Fig. 1. Text message content.

Table 1
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	0.063 (0.013, 0.142)	0.064 (0.016, 0.152)	0.066 (0.015, 0.141)	0.061 (0.017, 0.150)	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 class 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
Participation in prior study	348 (13.0%)	361 (13.3%)	119 (14.4%)	96 (12.4%)	0.66
Cell phone	1777 (66.5%)	1790 (66.2%)	558 (67.6%)	526 (68.0%)	0.72

Notes: 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. Cell phone reflects students who were part of presumed cell universe (see SOM).

representation of actual enrollment.

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. Again, this provides the most conservative estimate of the effect of implementation strategy on student outcomes by including all students who were randomized, regardless of whether they received the initial enrollment message. We show results of all primary analyses with the presumed cell phone universe in the SOM. All models are run with robust standard errors and control for randomization strata. We also include pre-intervention student-level covariates 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.

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. Average pre-intervention GPA in our sample was 1.9 on a 4.0 scale.

We also test the effect of implementation strategy, as reflected by condition assignment, 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. Students take, on average, five courses per term. The average pre-intervention course failure rate was approximately 1.2 courses per term, or 2.4 courses per semester.

1.5. Sample and attrition

As shown in Table 1, about 81% of our sample was Black or African-American, and 16% was Hispanic. On average, students' baseline GPA was 1.9, 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 classes—approximately one class absence every three days—and the median percent of missing assignments was 6.3%. Column (5) shows that we cannot reject the null hypotheses of no difference between the four condition groups for 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; 668 students (9.6%) 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 (see SOM). Eight percent of the students in our sample also transferred schools within the district during the course of the study. The primary analysis includes all students for whom we received outcome data, regardless of whether they transferred schools.

2. Results

2.1. User adoption

As shown in Fig. 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. As shown in the SOM, adoption rates among the full universe—including parents who do not have valid cell phones—exhibit the same trend.

Table 2 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 those with higher performing students would be the most likely to enroll in the text message parent alert system.

Of the 2874 parents who we believe had valid cell phones and were assigned to one of the three treatment conditions, we sent alerts to 1403 or about 49%. In total, we sent 27,207 alerts. Ninety-six percent of the alerts went to parents in the Automatic Enrollment condition (see Table 3). In addition, we sent alerts to 6 parents with cell phones who were assigned to the Control condition. All parents had access to the parent portal website and could enroll in the alert system of their own volition. First stage estimates show that being assigned to the Automatic Enrollment condition significantly increased the likelihood of take-up and of receiving alerts relative to all other conditions (see SOM). Assignment to the Standard condition did not meaningfully affect take-up or the probability of receiving an alert.

By condition, 75% 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 4). In terms of frequency, about 40% of parents in the presumed cell universe and assigned to the Automatic Enrollment condition and about 5% of those in the Simplified condition received alerts each week (see Fig. 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—about 4 alerts per month, on average. Parents in the Standard condition who enrolled and received at least one alert received an average of 9 alerts throughout the study, approximately 2 alerts per month. Of the 1403 treatment condition parents who received one or more subsequent alerts, 1355 (97%) received at least one absence alert, 1105 parents (79%) received at least one missing assignment alert, and 1117 (80%) received at least one low grade alert.

2.2. GPA and course failures

Table 5 reports the results of regression analyses examining the effect of condition assignment on second semester GPA and course failures among the full randomized universe. Assignment to the Automatic Enrollment condition increased average GPA by about 0.06 points, or about 3%, over the control group mean of 1.89 (Column 1). Adding a set of baseline controls to the model reduces the treatment effect slightly to 0.05 points, but improves the precision of the estimates (Column 2).

Table 5 shows that students in the Control condition failed an average of 2.4 courses during terms 3 and 4 (Column 3). Students take an average of five courses per term, meaning the average class failure rate in our sample is approximately 25%. Assignment to the Automatic Enrollment condition reduced the number of courses failed by 0.23 courses, or about 10%, from the Control-condition mean. This equates to an average of 1 in 4 students in the Automatic Enrollment condition passing a course they otherwise would have failed. Again, adding a set of baseline covariates reduces the observed treatment effect for those assigned to the Automatic Enrollment condition slightly to 0.21 courses, but increases the precision of the estimates (Column 4). Our findings are robust to excluding siblings who were randomized to different conditions, to controlling for enrollment in one of the four schools that turned on school-wide alerts prior to our study, and to limiting the analytic sample to only those in the presumed cell phone universe (see SOM).

The results in Table 5 show that the intervention effectively improved academic performance, as measured by average semester grade

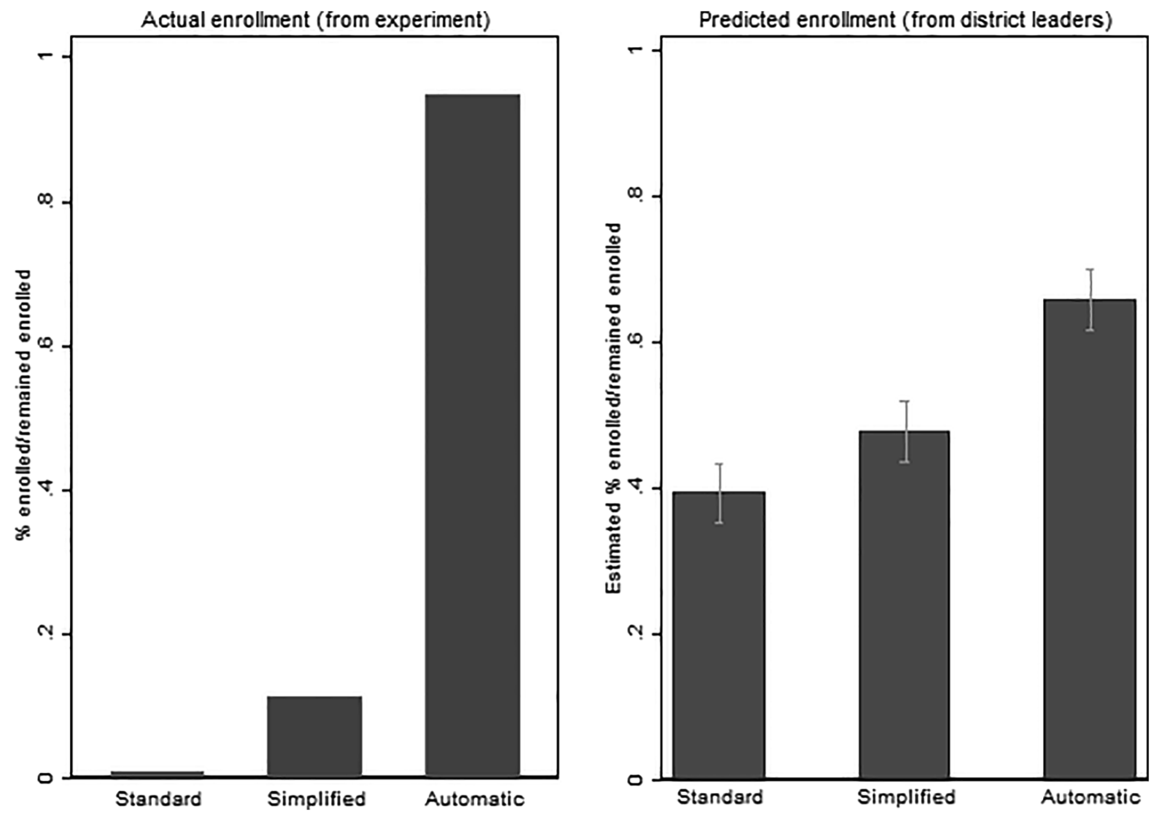

Fig. 2. Actual enrollment from experiment vs. predicted enrollment from survey, by implementation strategy. Error bars on predicted enrollment graph (right) represent 95% confidence intervals.

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 smaller in magnitude and not statistically significant in most models. As shown in the SOM, the effect of assignment to the Simplified group is positive and significant in some models, but these results are not robust nor precise across different specifications. However, by design, we are underpowered to detect meaningful effects in both the Standard and Simplified conditions. Thus, we do not interpret the lack of effect in these groups as a sign that the alerts were ineffective for these students. In fact, treatment-on-treated (TOT) estimates show that receiving alerts has a large impact on GPA and the total number of courses failed for all students whose parents received these messages (see SOM). The TOT estimates present a more accurate treatment effect for those who actually enroll and receive alerts, and our estimates align closely with those from similar studies (see, e.g., Bergman & Chan, 2019). Here, though, our focus is on the impact of implementation strategy. As such, we concentrate on the ITT estimates, which provide a more realistic sense of the total effect that a school or district leader can expect from implementing a similar technology.

While the TOT estimates demonstrate that alerts themselves are effective for all students regardless of condition assignment, they are only effective if parents adopt the technology. Our ITT estimates demonstrate that with opt-in enrollment, not enough parents enroll in the

Table 2
Number of parents enrolled in text message alert system technology.

	Automatic enrollment	Simplified	Standard	p-value
# assigned to treatment	2705	825	773	
# who received initial enrollment message	1790	558	526	
# remained enrolled/# actively enrolled	1697	63	4	
% remained enrolled/% actively enrolled	94.8%	11.3%	0.8%	< 0.001
Pre-intervention GPA for those who remained enrolled/actively enrolled	1.89	2.14	2.41	0.13
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%	0.006

Notes: All *p*-values calculated using Fisher's Exact Tests except for pre-intervention GPA, which uses an ANOVA. The control group is excluded from this table as we only have data on the parents in the control group who received alerts; we do not have data on the number of parents in the control group who enrolled (i.e., some parents may have enrolled and not received subsequent alerts).

Table 3
Number of alerts sent.[1]

	Control	Automatic enrollment	Simplified	Standard
Presumed cell phone universe	1776	1790	588	526
Total number of alerts sent	25	26,020	1129	33
Average number of alerts sent per student	0.03	14.5	2.0	0.07
Number of missing assignment alerts	7 (28.0%)	6675 (25.7%)	409 (36.2%)	5 (15.2%)
Number of absence alerts	16 (64.0%)	9910 (38.1%)	288 (25.5%)	25 (75.8%)
Number of low grade alerts	2 (8.0%)	9435 (36.3%)	432 (38.3%)	3 (9.1%)

Notes: Number of alerts sent to parents who were part of presumed cell phone universe.

[1] Summary statistics on alerts do not include those sent by schools that turned on the text message parent alert system school-wide. These messages only included absence alerts, and differed in frequency, timing, and content from the alerts sent through our study. Only 430 students who participated in our study also received one or more alerts through the school program (see SOM).

alert service to generate meaningful average improvement in student performance. With only 11% take-up, a school that implements this technology using a Simplified enrollment strategy would likely need a population of over 30,000 students to see any effect on student GPA or course pass rates. The average U.S. public high school has less than 1000 students (NCES, 2001). As such, schools that choose to implement this technology with an opt-in enrollment strategy may inaccurately conclude that the technology is ineffective when in reality, it is actually the enrollment strategy that is ineffective.

2.3. Heterogeneity

We evaluated the effect of treatment assignment on our two primary academic outcomes for two subgroups—high school students and students with below-average baseline GPA. Previous work by Bergman (2015) and Bergman and Chan (2019) showed that parents have upwardly-biased beliefs about their child's performance and effort. These biased beliefs are positively correlated with older and lower-performing students. As such, to the extent that providing parents with additional information on their child's performance can correct these misbeliefs and facilitate parental action and engagement, we hypothesize that it should be most effective for these subgroups. In addition, high school students are relatively lower-performing than middle school students: the average baseline GPA for high school students in our sample was 1.8 versus 2.0 for middle school students. Because this intervention alerts parents about lower performance, we thus might expect the effects to be larger among high school students.

As shown in Table A1, among high school students in the Automatic Enrollment condition, second semester GPA increased by 0.08 points, or about 4%, from the regression-adjusted Control condition mean of 1.8 points (Table A1). The number of courses failed among high school students in the Automatic Enrollment condition decreased by 0.21 courses compared to the Control condition, or about 8%. While there was no significant effect of treatment assignment on GPA for middle school students assigned to the Automatic Enrollment condition, we do find reduction in courses failed of about 8%. This result is not robust to the exclusion of controls, however.

Table A2 presents estimates of our primary outcomes by whether a student's baseline GPA was above or below the median for her grade. We find a consistent and significant 0.07–0.08-point effect on GPA and a 0.34-point reduction in course failures for those whose baseline performance was below the grade-level median and who were assigned to the Automatic Enrollment condition. Additionally, assignment to the Simplified enrollment condition had a significant impact on course failures for those below the baseline median, although we do not find a corresponding effect on GPA. Overall, this aligns with results described by Bergman and Chan (2019), who found similarly large effects of this intervention for high school students compared to middle school students and for students whose baseline GPA was below their grade-level median.

2.4. 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. Because this inquiry was sent via text message, 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. To assess this potential source of response bias, 264 parents in the presumed cell phone universe who were assigned to

Table 4
Number of parents receiving one or more alerts during the study.

	Control	Automatic enrollment	Simplified	Standard	p-value
N	2673	2705	825	773	
Presumed cell phone universe	1776	1790	558	526	
Number of parents who received 1 + alerts	6	1343	56	4	
Percentage of parents who received 1 + alerts	0.3%	75.0%	10.0%	0.8%	< 0.001
Average number of alerts received for those who received at least 1 alert	8.8	19.4	20.2	8.8	0.06

Notes: Number of parents in the presumed cell universe who received one or more alerts during the study. *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.

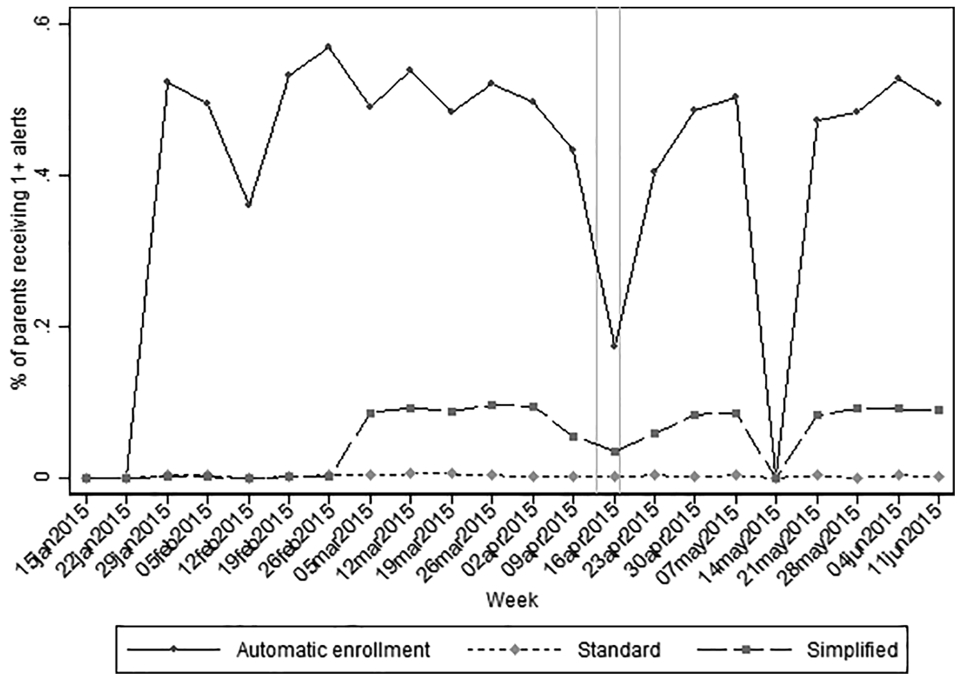

Fig. 3. *Proportion of parents in each condition in presumed cell phone universe who received alerts each week.* Notes: Vertical lines mark spring break, during which all DCPS schools were closed (April 13–17, 2015). Messages the week of May 14, 2015 failed to send.

Table 5
Primary academic outcomes.

Variables	(1) GPA	(2) GPA	(3) # classes failed	(4) # 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
Mean for Control	1.887	1.894	2.435	2.424

Notes: OLS estimates of the effect of condition assignment on average second semester GPA (columns 1–2), and total number of courses failed in the second semester (columns 3–4). All models control for randomization strata that are comprised of gender, grade level, and binary variables for pre-intervention low GPA (below 1.9), pre-intervention low attendance (missed 1 or more classes), 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 class absences. Reference group for race is non-Black. Full models (2 and 4) also include indicators for missing race, baseline GPA, and prior absences. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Table 6
Response rates to placebo text message.

	Automatic enrollment	Simplified	Effective control (standard)
# assigned to treatment	2705	825	773
# who received initial enrollment message (presumed cell phone universe)	1790	558	526
# sent placebo message	122	76	66
% received placebo message	6.8%	13.6%	12.6%
# responded to placebo message	25	16	17
% responded to placebo message	20.5%	21.1%	25.8%

Notes: This table shows the number and percentage of parents in each of the treatment groups who are part of the presumed cell phone universe and received or responded to the placebo text message.

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” (see SOM for details). Response rates to the placebo message were compared to evaluate non-responsiveness across treatment groups.

Subsequently, we sent 2319 parents in the presumed cell universe 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

Table 7
Interest elicitation text messages sent and response rates.

	Automatic enrollment	Simplified	Effective control (standard)
N	1790	558	526
Number sent at least 1 interest elicitation text message	1396	471	452
Number delivered exactly 1 interest elicitation text message and 1 “discontinue” message	1125	353	335
Percent delivered exactly 1 interest elicitation text message and 1 “discontinue” message	62.9%	63.3%	63.7%
Number who received 2 messages & responded “yes” to interest elicitation text message	174	58	38
Percent responded “yes” to interest elicitation text message	15.5%	16.4%	11.3%

Notes: This table shows the number and percentage of parents in each of the treatment groups who are part of the presumed cell phone universe and received or responded affirmatively to the interest elicitation text message.

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 19% 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 (78%) 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. There was no significant difference in the likelihood of receiving both messages as intended across treatment groups, nor across any baseline covariates (see SOM). Given how few parents in the Standard condition 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 20% and 21% of parents in the Automatic Enrollment and Simplified conditions respectively answered the placebo text message, while 26% of the Standard condition responded (see Table 6). The SOM shows results from a linear probability model estimating the effect of treatment on responding to the placebo message. 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 parents in the Automatic and Simplified Enrollment conditions 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 7. About 16% of parents in the Automatic Enrollment condition and the Simplified condition responded affirmatively to the interest elicitation text message. Only 11% of the Standard condition responded affirmatively.

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 may exist among those who have received previous messages (see SOM). 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. Nevertheless, parents assigned to the Automatic Enrollment and Simplified conditions were about 4 percentage points more likely to respond positively to the interest elicitation text message than those in the Standard condition (see SOM). Similarly, parents who received an alert—regardless of condition assignment—were 5 percentage points more likely to respond positively, and this was driven by increased demand among participants in the Automatic Enrollment and Simplified conditions. Enrollment method is strongly predictive of receiving alerts; 75% of parents in the Automatic Enrollment condition received at least one alert, compared to just 10% of parents in the Simplified condition. This suggests that the mechanism by which enrollment method influences demand is through subsequent use of the technology: enrollment strategy significantly affects take-up, which in turn impacts frequency and probability of use, and ultimately increases future demand for the technology.

3. 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 14% 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. 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. Fig. 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 0.06 GPA points and 8 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).

Among the participants whose districts already have such a technology, 79% indicated they enroll parents via an opt-in process. As we show, the average effect of this technology is negligible when implemented with opt-in enrollment processes. As a result, this may contribute to leaders' perception that this technology is ineffective, and their corresponding unwillingness to invest resources in its implementation.

4. 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 average student performance, which is not surprising since very few parents enrolled (< 1%). For similar reasons, the simplified implementation strategy did not cause meaningful improvements in overall average 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%). In line with recent research on behaviorally informed approaches to engage parents, these improvements are large. The reduction in course failures among students in the automatic enrollment condition is half the impact found with high-intensity tutoring (see, e.g., Cook et al., 2015), but at less than 1% of the cost.

These results have important implications. First, the way in which an organization implements a new technology can lead its leaders to draw radically different conclusions about whether the new technology is valuable and effective. The parent alert system technology has a positive effect on academic performance for those who enroll. However, the implementation process dramatically affects enrollment. 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 ineffective. 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. As such, to the extent that policy-makers value targeting certain populations of students, a default enrollment process may be better suited for achieving this goal.

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.[1] 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. That said, notice that the absolute rate of parents reporting interest in enrolling in such a program is low (15%). In our study, they were asked about opting in to the program. A different way of framing this question—and the way school leaders might prefer in light of this research—would be as a default option. Although we expect the absolute rate of interest in enrolling would increase across conditions if the question were framed as an opt-out, the relative rates across conditions would likely show similar patterns as we observed.

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.

[1] A number of studies have shown that short-run subsidies for new technologies can affect subsequent adoption either positively or negatively due to learning and screening effects (Ashraf, Berry, & Shapiro, 2010; Billeter, Kalra, & Loewenstein, 2011; Dupas, 2014).

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Appendix A. Heterogeneous treatment effects

Table A1
ITT subgroup analysis: middle vs. high school.

	GPA				Number of courses failed			
	High school		Middle school		High school		Middle school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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	3083	3083	3206	3206	3083	3083	3206	3206
R-squared	0.345	0.521	0.345	0.544	0.251	0.417	0.228	0.333
Mean for control	1.776	1.802	1.993	1.980	2.611	2.546	2.264	2.295

Notes: OLS estimates of the effect of condition assignment on average second semester GPA (columns 1-4), and total number of courses failed in the second semester (columns 5-8), by middle school vs. high school grade level. Subgroups are analyzed by limiting the sample to the group in question. Covariates detailed in Table 5 notes. All models also control for randomization strata. Reference group for race is non-Black. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Table A2
ITT subgroup analysis: baseline GPA.

	GPA				Number of courses failed			
	Below median		Above median		Below median		Above median	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Automatic enrollment	0.075**	0.072**	0.053	0.051	−0.344**	−0.338***	−0.128*	−0.090
	(0.034)	(0.030)	(0.034)	(0.032)	(0.134)	(0.123)	(0.068)	(0.062)
Standard	0.081	0.069	−0.055	−0.037	−0.099	−0.066	0.046	0.013
	(0.050)	(0.045)	(0.051)	(0.049)	(0.200)	(0.178)	(0.107)	(0.098)
Simplified	0.060	0.059	−0.040	−0.021	−0.395**	−0.399**	0.056	0.040
	(0.049)	(0.045)	(0.050)	(0.048)	(0.198)	(0.188)	(0.113)	(0.101)
Baseline GPA		0.474***				−1.173***		−0.809***
		(0.031)				(0.120)		(0.052)
# portal log-ins		0.001***		0.002***		−0.004**		−0.001***
		(0.001)		(0.000)		(0.002)		(0.000)
Absences		−0.005***		−0.014***		0.021***		0.030***
		(0.000)		(0.001)		(0.002)		(0.003)
Black		−0.117***		−0.272***		1.005***		0.515***
		(0.042)		(0.033)		(0.152)		(0.054)
Observations	2941	2941	3338	3338	2941	2941	3338	3338
R-squared	0.122	0.301	0.064	0.157	0.096	0.238	0.044	0.217
Mean for control	1.246	1.249	2.454	2.450	3.886	3.880	1.152	1.143

Notes: OLS estimates of the effect of condition assignment on average second semester GPA (columns 1-4), and total number of courses failed in the second semester (columns 5-8), by whether students' baseline GPA was above or below the median for their grade. Thirteen students who are missing baseline GPA are excluded from these analyses. Subgroups are analyzed by limiting the sample to the group in question. Covariates detailed in Table 5 notes. All models also control for randomization strata. Reference group for race is non-Black. Robust standard errors in parentheses. *** implies statistical significance at 1% level, ** at 5% level, * at 10% level.

Appendix B. Survey results

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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	0.01
Simplified enrollment	0.06	0.00
Automatic enrollment	0.07	0.07
% decrease in course failures		
Standard enrollment	16.6%	1%
Simplified enrollment	18.8%	7%
Automatic enrollment	22.5%	10%

Note: The actual effect is taken from the ITT analysis presented in Table 5.

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. Supplementary material

Supplementary data to this article can be found online at https://doi.org/10.1016/j.obhdp.2019.04.001.

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