

# Leveraging Parents through Low-Cost Technology: The Impact of High-Frequency Information on Student Achievement\*

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**Abstract:** We partnered a low-cost communication technology with school information systems to automate the gathering and provision of information on students' academic progress to parents of middle and high school students. We sent weekly, automated alerts to parents about their child's missed assignments, grades, and class absences. The alerts reduced course failures by 28%, increased class attendance by 12%, and increased student retention, though there was no impact on state test scores. There were larger effects for below-median GPA students and high school students. We sent over 32,000 messages at a variable cost of \$63.

JEL Codes: I20, I21, I24, I28.

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# I Introduction

Families are both one of the greatest sources of inequality and a powerful determinant of academic achievement (cf. [Coleman et al., 1966](#); [Heckman, 2006](#); [Cunha and Heckman, 2007](#); [Todd and Wolpin, 2007](#)). While leveraging families has the potential to improve child outcomes, many programs that do so focus on skills-based interventions, can be difficult to scale due to high costs, and often focus on families with young children ([Belfield et al., 2006](#); [Olds, 2006](#); [Nores and Barnett, 2010](#); [Heckman et al., 2010](#); [Avvisati et al., 2013](#); [Duncan and Magnuson, 2013](#); [Gertler et al., 2014](#); [Mayer et al., 2015](#); [Doss et al., forthcoming](#); [York et al., forthcoming](#)).

However, for older children, recent research suggests it may be possible to leverage parents by resolving certain intra-household information frictions around their child’s effort in school.<sup>1</sup> For instance, [Bursztyn and Coffman \(2012\)](#) conducted a lab experiment that shows families prefer cash transfers that are *conditional* on their child’s school attendance over unconditional cash transfers because this helps them monitor their child. When parents are offered a chance to receive information about their child’s attendance via text message, they no longer are willing to pay for conditionality. [Bergman \(2014\)](#) conducted an experiment that sent information to parents about their child’s missing assignments to estimate a persuasion game ([Dye, 1985](#); [Shin, 1994](#)) in which children strategically disclose information about their effort to parents and parents incur costs to monitor this information. He found that, for high school students, the additional information reduces parents’ upwardly-biased beliefs about their child’s effort and makes it easier for parents to monitor their child.<sup>2</sup> There is evidence that students’ effort and achievement improve as a result.

The idea that providing information to parents about their child’s effort can improve outcomes is tantalizing. There is a lack of low-cost interventions that can successfully improve

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<sup>1</sup>Parents also exhibit information problems about their child’s ability, attendance, and the education production function ([Bonilla et al., 2005](#); [Bursztyn and Coffman, 2012](#); [Cunha et al., 2013](#); [Bergman, 2014](#); [Rogers and Feller, 2016](#); [Dizon-Ross, forthcoming](#); [Kinsler and Pavan, 2016](#); [Andrabi et al., 2017](#)). [Fryer \(2011\)](#) also finds that students may not accurately assess the education production function as well.

<sup>2</sup>The experiment became contaminated for middle school students when the school asked an employee to call all of these students’ parents about their missed assignments.

education outcomes for children during middle and high school (Cullen et al., 2013).<sup>3</sup> While there is promise that providing high-frequency information about students’ progress could impact outcomes, the current evidence comes from small, labor-intensive interventions, some of which do not look directly at measures of student achievement and may be difficult to scale. For instance, Kraft and Rogers (2014) show that personalized messages, written individually by teachers and sent by research staff to a sample of 435 parents, helped retain students in a summer credit-recovery program. Bergman (2014) sent bi-monthly text messages—typed by hand—and phone calls about students’ missed assignments and grades to a sample of 279 parents. In theory, placing student information online could help resolve these information problems, but Bergman (2016) finds that parent adoption and usage of this technology is low, especially in schools serving lower-income and lower-achieving students, which could exacerbate socio-economic gaps in student achievement.

We develop and test a low-cost technology that synchronizes with Student Information Systems and teacher gradebooks to push high-frequency information to parents about their child’s absences, missed assignments, and low grades via automated text messages.<sup>4</sup> The intervention automates sending out three types of alerts. First, an absence alert was sent weekly detailing the number of classes a child missed for each course in the last week. This by-class alert contrasts with how districts typically report absences to parents, which are usually reported in terms of full-day absences.<sup>5</sup> Similarly, if a student missed at least one assignment over the course of a week, a weekly alert was sent stating the number of assignments missed in each class during the past week. Lastly, a low-grade alert was sent once per month if a child had a class grade average below 70% at the end of the month. Messages were randomly assigned to be delivered to either the mother or the father.

We conducted a field experiment to evaluate the academic and behavioral effects of this information technology in 22 middle and high schools. In the first year of the intervention,

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<sup>3</sup>There are also a number of low-cost interventions that focus on the high-school-to-college transition, which have improved college enrollment outcomes, such as Bettinger et al. (2012); Castleman and Page (2015); Hoxby and Turner (2013); Carrell and Sacerdote (2017).

<sup>4</sup>Text messages have been used in several education interventions (Kraft and Rogers, 2014; Bergman, 2014; Castleman and Page, 2015, 2016; Page et al., 2016; Oreopoulos and Petronijevic, 2018; Castleman and Page, 2017).

<sup>5</sup>The definition of a full day can vary from district to district. In this district, it constitutes 70% of the school day.

we sent 32,472 messages to treatment group families, or an average of 52 messages per treated family. This increased the likelihood parents were contacted by schools at least once per month by 19 percentage points. This high-frequency contact contrasts with the existing amount of contact between schools and parents, which varies widely. Our surveys indicated that nearly 50% of parents were contacted less than one time in three months by the school about their child’s academic progress, but roughly one quarter of parents reported hearing from their school more than once per month. We also find that parents tend to overestimate their child’s grades, on average, and to underestimate their child’s missed assignments. Parents are more accurate about the former than the latter.

We find that, as a result of this additional contact, there was a substantial decrease in the number of courses students failed. In the first year, students failed one course, on average, and the text-message intervention reduced this by nearly 30%. High school students’ GPA improved by 0.17. Treatment group students attended 12% more classes and district retention increased by 1.5 percentage points. We do not find any improvements in state math and reading test scores. However, statewide exams had no stakes for students, and students spent roughly 100 minutes less time than the test provider expected for students to complete the exams. The district subsequently discontinued using these standardized tests.<sup>6</sup> In contrast, we do find significant, 0.10 standard-deviation increases for in-class exam scores.<sup>7</sup>

Most of the positive impacts were driven by two subgroups we pre-specified in our analysis plan: students with below-median GPAs and high school students. These groups experienced larger impacts on grades, GPA, missed assignments, course failures and retention while middle-school students show no significant, positive (or negative) effects. We find that the positive effects for high school students and students with below-median GPAs persisted into the second year of the intervention. We do not find a differential effect of alerting mothers versus fathers.

Our paper makes several contributions to the literature. First, we show that pushing high-

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<sup>6</sup>The state superintendent’s commission expressed concerns that the exams are not “an accurate gauge of student achievement” and “doesn’t give much reason for students to take it seriously.” See the [Charleston Gazette-Mail](#).

<sup>7</sup>Several recent studies have documented substantial increases in student performance in response to the stakes of the exam ([List et al., 2017](#)) and student effort ([Hitt, 2016](#); [Zamarro et al., 2016](#)).

frequency information to parents can significantly improve student performance in middle and high schools. One novel aspect of this information, which had not been previously tested, is that we tracked and sent information to parents about every class their child had missed (as opposed to full-day absences). We show that this information is particularly important because students are 50% more likely to miss an individual class than a full day of school. We are among the first papers to show that class absences occur at much higher rates than full-day absences (see [Whitney and Liu, 2017](#)).

Second, we test a unique, automated technology. The promise of automation is that, relative to other interventions, communicating with parents via automated alerts is extremely low cost, even at scale. Previous work has asked teachers to write tailored content about students or used research assistants to gather and provide information to families. Our intervention allows schools to leverage their data system “as is.” Moreover, sending information via text message is cheap; despite sending more than 32,000 text messages, the total cost was approximately \$63. If schools have no existing gradebook, the platform and personnel training cost an additional \$7 dollars per student. This low marginal cost of the collection and provision of information implies that automated messaging has a high potential to impact student achievement at scale. [Rogers and Feller \(2016\)](#) also demonstrated a successful, scalable attendance intervention by mailer, but they find (as they expected) that the attendance gains, equal to an additional day of school, were not large enough to improve student performance. We complement their research by demonstrating the value of targeting students’ *by-class* absences.

Lastly, previous research in areas such as cash transfers suggests that intra-household divisions of labor and bargaining power could lead to heterogeneous effects of education interventions if information does not flow perfectly between parents ([Doepke and Tertilt, 2011](#); [Duflo, 2012](#); [Yoong et al., 2012](#); [Akresh et al., 2016](#)). To assess this possibility, we randomized whether a student’s mother or father received the intervention.

Related to our paper is ongoing work by [Berlinski et al. \(2016\)](#), who recently conducted a texting intervention in eight elementary schools in Chile. They send information to parents

about their child’s math test scores, math grades, and attendance. These data are gathered from schools and entered by their research team into a digital platform, which is then used to send text messages to parents. In contrast, we automated this process by scraping data that is frequently entered into district student information systems, such as grades, attendance and missed assignments.

Providing high-frequency information to parents about their child’s academic progress is a potentially important tool in the set of effective interventions for older children. In contrast to the intervention tested here, this set of tools includes high-touch interventions such as intensive tutoring (Bloom, 1984; Cook et al., 2015; Fryer, 2017) or one-on-one advising (Oreopoulos and Petronijevic, 2018), but their scale may be limited by high costs.<sup>8</sup> However, despite the low-cost and scalability of our intervention, it is not a panacea. Automation alone also does not address other aspects of scalability, such as adoption by parents and usage of the system by teachers. In Washington D.C., Bergman and Rogers (2017) randomized whether approximately 7,000 parents can enroll in this technology via an opt-in process or an opt-out process. Take up for the opt-out process is above 95%, with similar effects on outcomes as in this study, and this group is more likely to opt into the technology in the future. Another potential constraint is whether teachers supply information to the gradebook system in a timely fashion. In this study teachers were blinded to the intervention assignment and we discerned no impacts of the intervention on teacher logins into the system. Some school districts contractually obligate teachers to update grades regularly (for instance, weekly). Our research suggests this type of policy could be an important—and potentially monitored—input for improving education outcomes.

The rest of the paper proceeds as follows. Section II describes the background and the experimental design. Section III describes the data collection process and outcome variables. Section IV presents the experimental design and the empirical specifications. Section V shows our results and Section VI concludes.

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<sup>8</sup>Cook et al. (2015) designed a novel, cheaper high-intensity tutoring intervention for male ninth and tenth grade students, which reduced course failures by 50% and increased math test scores by 0.30 standard deviations. The intervention, however, is costly at several thousand dollars per student. Alternatively, Oreopoulos and Petronijevic (2018) compares a one-on-one student coaching program to both a one-time online exercise and a text message campaign and find that only the high-touch program impacts achievement.

## II Background and Experimental Design

The experiment took place in 22 middle and high schools during the 2015-2016 school year in Kanawha County Schools (KCS), West Virginia. We were subsequently able to extend the intervention through the 2016-2017 school year. As a state, West Virginia ranks last in bachelor degree attainment and 49th in median household income among US states and the District of Columbia. It is the only state where less than 20% of adults over 25 years of age have a bachelors degree and households have an overall median income of \$42,019.<sup>9</sup> The state population is 93% white, 4% African-Americans, 1% Asians, and 2% Hispanic or Latino as of 2015. Data from the 2015 NAEP showed that 73% of West Virginia students were eligible for free or reduced-priced lunch. Students also scored significantly below the national average on all National Assessment of Educational Progress (NAEP) subjects tested, with the exception of 4th grade science, which was in line with national averages.<sup>10</sup>

KCS is the largest school district in West Virginia with over 28,000 enrolled students as of 2016. The district's four-year graduation rate is 71% and standardized test scores are similar to statewide proficiency rates in 2016. In the school year previous to the study, 2014-2015, 44% of students received proficient-or-better scores in reading and 29% received proficient-or-better scores in math. At the state level, 45% of students were proficient or better in reading and 27% were proficient in math. 83% of district students are identified as white and 12% are identified as Black. 79% of students receive free or reduced-priced lunch compared to 71% statewide.<sup>11</sup>

Like much of the state, the district has a gradebook system for teachers. Teachers record by-class attendance and mark missed assignments and grades using this web-based platform. Teachers are obligated to mark attendance, but the only obligation to update the gradebook is every six to nine weeks, which correlate to the dissemination of report cards. We worked with the Learning Management System (LMS) provider of this gradebook to design a tool that automatically drew students' missed assignments for each class, their percent grade by

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<sup>9</sup>American Community Survey one-year estimates and rankings by state can be found [here](#).

<sup>10</sup>NAEP Results by state can be found [here](#).

<sup>11</sup>These summary statistics come from the state education website, which can be found [here](#).

class and their class-level absences from the gradebook. The tool synchronized with the Student Information System (SIS) to pull in parents' contact information. This allowed us to automatically pair contact information with information on academic progress from the gradebook and then push it out to families using a text-messaging API developed by *Twilio*. These text messages form our parent-alert system. Each of the text messages was designed to be a consistent weekly or monthly update to the parents of students who had at least one absence or missing assignment during the week or who had a low course average over the course of a month.

The gradebook application also has a “parent portal,” which is a website that parents can log into to view their child’s grades and missed assignments. All parents in the study could access the parent portal and any parent could turn on our alerts by logging into the portal and turning on the alert feature. As we discuss further below, only 2% of parents in the control group received any alert. [Bergman \(2016\)](#) finds that, in general, very few parents ever use the parent portal and we find this is true in KCS as well; roughly a third of parents had ever logged in to view their child’s grades. Moreover, usage of the parent portal tends to be higher for higher-income families and families with higher-performing students.

We tested three types of parent alerts: Missed assignment alerts, by-class attendance alerts, and low-grade alerts. The text of the alerts are as follows, with automatically inserted data in brackets:

- Missed Assignment Alert: *“Parent Alert: [Student Name] has [X] missing assignment(s) in [Class Name]. For more information, log in to [domain]”*
- By-Class Attendance Alert: *“Parent Alert: [Student Name] has [X] absence(s) in [Class Name]. For more information, log in to [domain]”*
- Low Class Average Alert: *“Parent Alert: [Student Name] has a [X]% average in [Class Name]. For more information, log in to [domain]”*

If a child missed at least one assignment during a week for any course, the parent received a text-message alert reporting the number of assignments their child was missing for each course during the past week. The missing assignment message was scheduled for each Monday. These assignments included homework, classwork, projects, essays, missing exams,

tests, and quizzes. On Wednesdays, parents received an alert for any class their child had missed the previous week. Lastly, and normally on the last Friday of each month, parents received an alert if their child had a cumulative average below 70% in any course during the current marking period. Each alert was sent at 4:00 P.M. local time. The text messages also included a link to the website domain of the parent portal, where the parent could obtain specific information on class assignments and absences if necessary.

These alerts targeted lower-performing students. We hypothesized that impacts would be greatest for students with lower GPAs. We believed these students would have less incentive to tell their parents about their academic performance (Bergman, 2014). To the extent that additional information makes it easier for parents to track their child’s performance, the intervention might increase the accuracy of their beliefs about their child’s performance and facilitate parents’ ability to induce more effort from their child to do well in school. We explore these mechanisms in our endline survey results and stratified the randomization by baseline GPA to explore heterogeneous effects.

The rest of this section and Section III describes recruitment, the field experiment, and data collection. Figure 1 shows the timeline from the consent process through first year of the experiment. Baseline data were collected from June to July 2015. We obtained demographic and enrollment data for the 2014-2015 school year from KCS along with contact and address information. Consent letters were sent out beginning August 2015 during the beginning of the school year. Calls requesting verbal consent were completed in September. Randomization into treatment and control was completed in early October 2015. For parents who were selected into treatment, introductory text messages were sent late that same month. Included in the texts was the option to stop at any point by replying “stop” or any equivalent variation.<sup>12</sup> Over the course of the study, nine parents or guardians requested the messages stop.<sup>13</sup> The intervention ran between the end of October 2015 through the end of May when the school year was expected to conclude. Officially, the academic school year ended in early June, but varied slightly based on weather-induced make-up days at each school. After

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<sup>12</sup>We manually tracked replies to ensure the service was shut off when requested.

<sup>13</sup>These parents were included as “treated” families in all analyses.

the end of the school year, we collected endline survey data both by phone and by mail as described below.

At the end of the 2015-2016 year, we asked KCS whether we could continue the intervention for a second year. KCS agreed to continue the intervention, but limited our data collection to outcomes originating from the gradebook.

## Recruitment and Sample Selection

The initial sample began with approximately 14,000 total students who were enrolled in grades five through eleven during the 2014-2015 school year. Recruitment was at the household level. A number of these students lived in the same households, so the final sample frame was 10,400 households.

During the summer of 2015, one consent letter was sent to each household in the sample frame. This letter was specifically addressed to one randomly selected parent or guardian when contact information was available for more than one parent in the data provided by the district. The letter contained the name of a randomly selected student living in the household, and this student would be the subject of the intervention.<sup>14</sup>

Trained interviewers followed up the letter with a phone call to each selected parent to confirm their participation and contact information. This was required by the district and our IRB Institutional Review Board. We then asked their language preference and preferred modes of contact: text message or phone calls. As a result, the parent or guardian of 1,137 students consented to the study and provided their contact information for inclusion as a participant.<sup>15</sup> Though it deviated from our original design, to simplify our intervention and to save on costs we chose to implement a text-message-only intervention. Those who could only be contacted by a landline phone or did not wish to be contacted by text did not receive

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<sup>14</sup>Students were in grades 5-11 the previous year and were expected to be in grades 6-12 during the school year of the study.

<sup>15</sup>Of the 14,000 students in middle and high schools in the district, many were from the same households. This decreased our sample to 10,400 households at the time. These parents were called a maximum of three attempts. Of the 10,400 phone numbers, approximately 60% of households never picked up the phone. About 11% were bad numbers that either did not work or led to the incorrect household. Another 5% declined to speak with the consultant. As a result, 24% of households both answered the phone and were willing to continue speaking with the consultant. Out of the parents willing to speak to the consultant, about 43% proceeded to give verbal consent to participate. A small number of consenting parents preferred phone calls over text messages, so this resulted in a final sample of about 10% of the 10,400 sample who were willing to accept a text message-based alerts. Of these participants, 96% of the treatment and control groups preferred to receive text messages.

the intervention even if they were randomized into treatment.<sup>16</sup> Only four percent of parents could only be contacted by a landline or did not wish to be contacted by text.

We examine the possibility of self-selection into the study. Consent documents sent to parents state that the “new program uses mobile devices to communicate specific academic information with parents” either “via telephone and text messages.” The information provided to parents at the time of consent may appeal to parents who are more engaged in their children’s education and/or parents who are less able to keep track of their children’s academic progress. It may also appeal to those who are more technologically inclined. Table 1, Panel B, presents baseline summary statistics and a statistical comparison between nonparticipants and participants of the study. Consent into the study is not significantly correlated with most predictors of student achievement or parental involvement: baseline GPA, baseline days missed, English Language Learner Status, IEP status, gender, an indicator for being suspended, baseline number parent logins to the parent portal, or baseline number of student logins. However, consent is significantly and positively correlated with an indicator for the student being black, baseline parent ever log in, baseline student ever login, student’s classes missed, and number of parents in the household. There is the possibility that there is self-selection into the study, such as black parents and parents who have less adult caretakers in the household. However, there is mixed evidence regarding selection based on parent engagement levels and the types of students who end up participating in the study. In a separate study, which did not require parental consent, [Bergman and Rogers \(2017\)](#) examine how take up of this intervention is largely determined by opt-in versus opt-out offers to receive it. The latter results in over 95% take up and effects on achievement similar to those found in this paper.

## Random Assignment

Random assignment was at the school-by-grade level to minimize the potential for spillovers into the control group. The data were initially collapsed at the grade-by-school level and

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<sup>16</sup>No families are dropped from the analysis, however, and all families assigned to treatment remained so even if they never received a text message.

randomization was subsequently stratified by indicators for below-median grade point average (GPA) and middle versus high school grades. All school employees were blinded to the randomization process.

We also randomized which parent or guardian received the text-message alerts if we had contact information available for both the mother and father of a child, or if we had multiple listed guardians. The selected parent was the same as the parent to whom the consent letter was addressed and the parent who trained personnel obtained consent from on the phone.

Parents in the control group received the default level of information that the schools and the teachers provided. This included report cards that are sent home after each marking period every six to nine weeks along with parent-teacher conferences and any phone calls home from teachers. As discussed above, all parents had access to the online gradebook.

### **III Data Collection**

We gathered data from multiple sources: administrative data, gradebook data, survey data, and texting data. We collected initial baseline data from administrative records on student grades, courses, attendance, race and ethnicity, English language status, and contact information. We also obtained data from the gradebook application, which includes student's grades, assignments and assignments scores, class-level attendance and parent logins into the parent portal. These baseline data were available for all students in our sample frame. During the intervention we obtained monitoring records on the text messages. We used these data to track messaging stop rates, whether text messages were received by phone numbers, and the total number of text messages that went out weekly.

After the 2015-2016 school year concluded we surveyed parents. The surveys took place during between June and August 2016. Initially, households were sent a letter stating that they would be called for a survey. This letter included a \$5 unconditional award as an appreciation for their participation in the study. Households were then called by trained interviewers to conduct the survey. Around this time, West Virginian residents were afflicted

by severe flooding during several torrential storms in June 2016. Sadly, more than 1,000 people were left homeless in Kanawha County alone. During the summer, KCS had multiple schools declared “total losses” by the Federal Emergency Management Agency because of the flooding. As a result, we decided to mail surveys home instead of proceeding with subsequent rounds of calling. We provided a reward of \$30 for paper surveys returned postmarked by August 8th, 2016. Our total response rate was 40%. A copy of our survey can be found in Appendix C.1. The goal of the endline surveys was to examine parent responses to the intervention not captured by administrative data. Parents were primarily asked about their communication habits with the school in recent months, their perception of the child’s effort and achievement, and their communication and motivational habits with their child.

In the summer we obtained administrative data from the district and the gradebook application once again. These included standardized exam scores and suspension data, students’ final grades and assignment scores, daily class-level attendance, alerts received by treatment and control group, and parent and student logins into the parent portal.

## **Outcome Measures**

Prior to obtaining outcome data, we registered our analysis plan, which specified subgroups of interest, regression specifications and primary outcomes. We specified primary outcomes to be number of classes failed, total classes attended, student retention, and math and reading standardized test scores. Primary outcomes are derived from both the gradebook application and the KCS administrative data. Included in the gradebook data are outcomes related to the number of class and daily attendance and marking-period grades by course. Administrative data contained state standardized test scores in math and reading.

The standardized test scores are from the Smarter Balanced assessment, which is aligned to the Common Core. We received scaled standardized test scores for Math and ELA for 2015 and 2016 examinations. These were the first two years in which the assessment was given after the state switched from the previous standardized test in West Virginia, the Westest. At the time of the field experiment, students in grades 3-11 were required to take

the Smarter Balanced assessment.

We also obtained behavior-related outcomes from the gradebook application and KCS. These provided data on suspension rates, measured as the quantity of occurrences and the number of days suspended as well as attendance measures at the class level. Following our analysis plan, we convert the latter into “number of classes present” (number of classes marked either present or tardy) because effects on retention potentially cause an increase in absences while increasing the number of days enrolled. We code suspensions into an indicator for ever being suspended.

Lastly, we use the assignment-level data to examine the effects on missed assignments, assignment scores, and class test scores. We identify tests and exams by the assignment titles containing the words “test” or “exam.” Assignment scores and test scores are standardized according to the classroom means and standard deviations for each assignment or test. We restrict the analyses to those scores three standard deviations or less away from the mean to remove outliers.<sup>17</sup>

The survey of parents was designed to examine parent and student responses to the intervention not captured by administrative and gradebook data. Parents were asked about their communication with and from the school, their perceptions about how their child was performing academically, and household behavior such as talking with their child about their academic progress or taking privileges away as a result of their performance in school. We use a number of these survey measures, along with other gradebook and administrative measures, as secondary outcomes in this paper.<sup>18</sup>

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<sup>17</sup>Analyses are robust to various other restrictions to handle outliers, such as excluding observations 4 or 5 standard deviations away from the mean, or removing all scores from a particular assignment or exam if even one score is an outlier.

<sup>18</sup>Appendix B.4 and B.5 summarize all the secondary outcomes variables used in our analysis, their sources, and their construction. Appendix B.6 summarizes the hypothesized effect on each outcome; we did not pre-specify hypotheses for subgroups however.

## IV Empirical Strategy & Experimental Validity

We conduct analyses using intent-to-treat (ITT) estimates.<sup>19</sup> We specify regressions for each parent-child pair,  $i$ , during time period  $t$ , as follows:

$$\text{Outcome}_{it} = \alpha_0 + \alpha_1 \text{Treatment}_i + \alpha_2 X_{it-1} + \eta_{it} \quad (1)$$

$X_{it-1}$  is a set of pre-specified, child-level covariates, which are fraction of days absent in the previous year, baseline GPA, an indicator for a student identified as Black, an indicator for English-Language Learner status, an indicator for having ever been suspended in the previous year, an indicator for gender, and an indicator for having special needs. When the outcome of interest is standardized test scores, we include the baseline standardized test score as well. We impute missing covariates with the mean baseline value; we include indicators for missing to the regression as well. All regressions also include strata indicators as controls. There are 76 clusters, and we cluster standard errors at the level of treatment assignment, which is the grade level in a given school.

When looking at assignments and class test scores there are multiple observations per student—more than 70,000 assignments and 7,000 tests across the entire sample and all courses—post treatment. The baseline control variables remain the same as above when we analyze these outcomes.

We analyze subgroups by restricting the sample to each subgroup and studying outcomes in the same way as described above. We pre-specified several subgroups of interest: students with below-median GPA, students with male versus female parents or guardians, and students in middle school versus high school.

Finally, in our pre-analysis plan we hypothesized the intervention will have positive treat-

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<sup>19</sup>Our pre-analysis plan specified that we would run regressions to estimate treatment-on-the-treated (TOT) effects using an instrument for whether or not a participant ever received an alert. Given that control group parents could manually opt in to the treatment and some treatment group parents would never receive a text message, we believed at the time that it would be the best approach. However, during the peer review process, the feedback we received was that intent-to-treat effects are more realistic and relevant to policymakers given that noncompliers are expected in a realistic context based on the design of the alerts. We report ITT estimates in this paper, but if interested, TOT estimates are available in the online appendix.

ment effects for our primary outcomes<sup>20</sup>. For each of these outcome measures we initially ran all tests as one-sided tests for improvements in outcomes. However, the positive, significant results we find on primary outcomes would also pass two-sided tests at conventional levels as well. As a result, for the sake of simplicity, we report all estimates using two-sided tests in this paper. Regarding secondary outcomes, we often decided to use two-sided tests initially because we did not always have a strong hypothesis about the direction of any potential effect for our secondary outcomes and subgroups.<sup>21</sup> Similar to primary outcomes, we report all estimates using two-sided tests.

### Baseline Treatment-Control Balance

Table 1, Panel A, presents baseline summary statistics for the control group, the difference in means from the treatment group and the p-value showing the statistical significance of these differences. Demographically, the sample is 49% female, 16% black, and the majority of students live in two-parent households. On average, students' baseline GPA is 2.8, they missed 6% of school days, and 20% were suspended during the baseline year. The contrast between by-class and full-day absences is stark: students missed 10% of all classes compared to 6% of full days. As in [Bergman \(2016\)](#), many more students have logged into the online gradebook portal than parents, (96% to 36%, respectively).

Randomization appears to have created a treatment and control group that are similar in terms of observable variables; no treatment-controls differences are statistically significant at the 10% level. We also regress baseline covariates on our treatment indicator and conduct an F-test for whether these baseline covariates are jointly equal to zero. The test cannot reject that the coefficients on these covariates are jointly equal to zero (p-value equals 0.61).

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<sup>20</sup>In writing our pre-registered analysis plan, we followed [Olken \(2015\)](#) and [Christensen and Miguel \(2018\)](#) articles, which discuss pre-registered analysis plans as means of transparency. Both articles state that an advantage of these analysis plans—if pre-registered—is that we can specify an effect direction and one-sided hypothesis to take full advantage of statistical power. We did so in instances when we had prior evidence to suggest an effect direction. In other instances, such as many of our secondary outcomes, we were generally uncertain as to the potential direction of the effect. Regardless, our main results are statistically significant at conventional levels regardless of whether we specified one- or two-sided hypotheses.

<sup>21</sup>Appendix B.6 lists our secondary outcomes and hypothesized effects as stated in our pre-analysis plan.

## Attrition and Non Response

There are several sources of attrition and non response in this study: missing academic outcomes, missing behavior outcomes, and survey non response. A particular concern is whether there is differential attrition by treatment status, which would invalidate our ability to make causal inferences from the data.

Table 2 shows the effect of treatment status on several measures of attrition as well as other correlates of attrition. The first column shows there is no treatment effect on the likelihood a parent responded to the survey: the point estimate is both small and statistically insignificant. Academic and demographic characteristics are generally poor predictors of survey response as well, with the exception of “percent of days missed” during the baseline academic year, which is significant at the 5% level. This is encouraging because it provides some suggestive evidence that our survey sample may be representative of many families in the study.

This pattern generally is also true across the remaining indicators of missing data: school suspensions, math scores and reading scores. There are no treatment effects on any of these indicators. Only the percent of days missed the previous year is a strong predictor of missing math and reading score, which is not surprising given that attendance the previous year predicts having measures in the current year. There are no significant predictors of missing suspension data. Overall, there is no evidence of differential attrition or non-response by treatment status. Additionally, attrition from course taking will be an outcome of retention analyzed below. We define retention in the district as a student taking at least one course the first semester after the intervention.

## V Results

### Descriptive Results

We begin by describing the amount of communication between parents, children and schools as well as parents’ beliefs about their children’s performance and their correlates. Our survey

asks parents about the frequency at which they are contacted by their child's school regarding their child's academics. Results of the survey show that nearly 50% of parents heard from the school less than once every three months, about 15% heard once every two or three months, about 10% heard from the school once a month, and 25% of parents heard from their child's school at least twice per month. The contrast in aggregate responses shows the variation in families who are contacted frequently and those who are not. Appendix [A.1](#) examines the correlates of infrequent contact in column one. Surprisingly little predicts this infrequency. Neither GPA nor behaviors nor demographics significantly correlate with an indicator for hearing from the school less than once every three months. This question does not, however, assess whether parents find this communication useful.

Figure [3A](#) presents survey results describing the frequency parents spoke with their child about their progress in school. 55% of parents reported talking with their child every day about their schoolwork. Roughly 75% of parents talked with their child 2-3 times per week or more. At face value, it appears their children's schoolwork is at the top of parents' mind. One caveat is that this communication is self-reported, which may be subject to social-desirability bias. Parents' conversations about schoolwork is also demonstrated in Figure [3B](#), which shows how often parents talk to another adult in the household about their child's school work. For this behavior, no parent reported doing so every day, but 40% of respondents said they talk with another adult 2-3 times per week about their child's schoolwork or grades. Appendix [A.1](#) shows that, unsurprisingly, two-parent households were much more likely to have intra-household communication about their child. Little else seems to correlate with this behavior however.

Figure [4A](#) and Figure [4B](#) present control group parents' beliefs about their child's academic performance in terms of assignment completion and math grades, respectively. Figure [4A](#) shows the number of assignments parents believed their child missed in the past semester. More than 50% of parents believed their child had not missed any assignments. In comparison, according to administrative data, only 20% of respondents' children had missed no

assignments during that time frame.<sup>22</sup> In contrast, parents' perceptions about their children's math grades were more accurate: 60% correctly stated their child's grade in math. Around 25% overstated their child's grade in math. Many fewer underestimated it.

Estimates shown in Appendix A.1, fourth column, shows a measure of the quality of communication between parents and their children: an indicator for whether parents believed it was difficult to be involved in their child's education because their child does not tell them enough about their academic progress. 48% of parents believed their child does not disclose enough information about their academic progress to be easily involved in their education. This indicator negatively correlates with student's GPA and whether or not they are in high school. Parents with older or lower performing children were more likely to perceive that their child is not telling them enough about their schoolwork. In results not shown, parents who reported that their children do not disclose enough also report receiving significantly fewer report cards from their child's school as well.

Overall, these descriptives highlight how the information flow between parents, schools and their children may be particularly impeded when the child is performing poorly in school. While many parents frequently spoke with their child and another adult in the household about their academic progress, nearly one-half of parents believed it would be easier to be involved in their child's education if their child told them more about their schoolwork. The latter correlates strongly with students' grades and the receipt of report cards. In terms of parents' beliefs, parents tended to have more accurate beliefs about a student output—their grades—which is in line with what is provided on report cards. However, parents had much less accurate beliefs regarding assignment completion, which is a primary input to their child's grades. A key question is whether the automated-texting intervention studied here can increase parents' access to timely, actionable information and improve academic outcomes. The next section examines the effect of the treatment on school-to-parent communication and then looks at the effect on academic outcomes.

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<sup>22</sup>Appendix A.1 shows that inaccurate beliefs strongly and negatively correlate with their child's GPA.

## School-to-Parent Contact

We first assess whether the intervention functioned as intended. We do so by examining administrative data on alerts sent and received by parents and parent self-reported data on communication received from their child’s school.<sup>23</sup> According to administrative data on alerts received, parents in the treatment group were 71 percentage points more likely to receive an alert than the control group. The distribution of alerts for nonzero alert recipients in the treatment group is also shown in Figure 2. Approximately 29% of treatment parents never received a single alert for missing assignments, 28% never received an alert for class absence, and 35% never received an alert for low grade.

Not every family had a cell phone to receive text messages, so compliance is imperfect. Additionally, as discussed above, all parents in the study could access the platform and any parent could turn on our alerts by logging into the platform and turning on the alert feature. However, only two percent of the control group received any alert during the intervention period. On average, treatment group families received nearly 50 text-message alerts. Most messages were absence and assignment alerts because these were sent out weekly; families received an average of 21 of each of these alerts. Low-grade alerts went out monthly and so families received about six low-grade alerts, on average.

Further, we use survey data to examine whether parents also reported receiving more contact from the school about their child’s academic progress. Note that this measure includes any form of contact such as phone call, letter, email or a text message from the intervention. Parents could respond: “about twice a month,” “about once a month,” “once every two or three months,” and “less than once every three months.”<sup>24</sup>

In analyzing the effects of treatment assignment on these parent-reported measures of contact from the school, we find that an average of 45% of parents heard from their school

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<sup>23</sup>Appendix A.2 shows the effect of treatment status on alert receipt using administrative data. The first column shows an increase in the share of parents who received at least one alert as a result of the treatment. The second column shows the additional number of alerts that the treatment group received over the course of the school year relative to the control group. The remaining columns break the alerts down by the number of each type parents received.

<sup>24</sup>We specified in our pre-analysis plan that we would code this into an indicator for being contacted once per month or more, but in this paper we show mutually exclusive indicators for every possible response for completeness.

less than three times per month about their child’s progress.<sup>25</sup> Thirty-eight percent of the control group was contacted at least once per month, and treatment increased this by 19 percentage points. There is also a 12 percentage point reduction in the likelihood that a parent reported being contacted less than once every three months.

## Primary Academic Outcomes

In our pre-analysis plan, we specified five primary outcomes guided by the nature of the treatment, which targeted attendance, low grades and missed assignments. These outcomes are the number of classes students failed, the number of classes attended, retention in the district, and math and reading standardized test scores.

Table 3 presents the effects on these outcomes based on ITT estimates.<sup>26</sup> Column one shows that control students failed a little under one course on average. Receiving text message alerts reduced this by 28% or 0.27 points. Column two shows the effects on class attendance, which is again large and significant: students attended roughly 34 more classes than the control group, which is a 12% increase over the control group mean. Column three examines retention. About 3% of students from the control group did not take at least one course in the district in the semester post intervention, compared to 1.5% of students whose parents received alerts.

Columns four and five shows that the effects on test scores are small and statistically insignificant. There are several possible reasons for this given the results discussed above. A key concern is that the exams had zero stakes for students because they have no implications for their grades or their likelihood of graduating. This issue was evident to district officials, who expressed concern that students were spending less time on the exam than expected. Smarter Balanced, the test provider, estimated that 9th, 10th, and 11th-grade students need approximately 210 minutes to complete the exams at each grade level. However 9th graders spent 80 minutes to complete the exam, 10th graders spent 67 minutes, and 11th

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<sup>25</sup>Refer to Appendix A.3 for estimates of parent-reported measures of contact from their children’s schools.

<sup>26</sup>Treatment on the Treated (TOT) estimates are shown in Appendix A.4 As mentioned, participants assigned to treatment may not always receive the intervention, and control group users may manually opt into alerts. As a result, TOT estimates may be useful by scaling the effects of the intervention based on participants who actually received at least one alert.

graders spent 78 minutes to complete the exam, on average. The State Superintendent said in response, “our students are competing with Advanced Placement exams, SAT, ACT, final exams, and then we throw in Smarter Balanced. They have told me they’re not taking it seriously.” Of particular concern was what he saw as a “lack of effort by high schools on the test.”<sup>27</sup> The state decided to discontinue using the test in future years.

We examine these claims empirically. We look at the correlations between students’ GPA and the standardized test scores by regressing math and reading state test scores on students’ baseline GPAs (see Appendix A.5).<sup>28</sup> We also differentiate these correlations between middle and high school students to assess the Superintendent’s concern. The disparity in the correlations for middle school students and high school students is large: columns two and four of A.5 show that the correlation between GPA and test scores is roughly half as large for high school students as it is for middle school students. For middle school students, the correlation between GPA and math scores and reading scores is 0.75 and 0.86, respectively. For high school students, these correlations are 0.43 for math and 0.44 for reading. This difference between middle and high schools is statistically significant (p-value<0.001).

Second, the intervention may result in additional student effort for educational inputs that improve course grades but not learning outcomes reflected in the state test scores. The outcomes discussed above show improvements in students’ coursework and attendance. However, the curricular material covered during this additional course time may not have reflected the material covered in the exams, especially as the exams were only recently implemented in 2015. The superintendent stated they are “working on standards-based teaching making sure all the standards are covered.”<sup>29</sup> Moreover, because the exams had only recently been introduced, no school-based accountability measures associated with the exams had been released. These reasons may attenuate the potential to impact state test scores.

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<sup>27</sup>This made the local newspaper: [Charleston Gazette-Mail](#).

<sup>28</sup>Alternatively, we also tested for whether standardized test scores are orthogonal to other traditional demographic measures usually correlated with test scores such as race, ELL status, IEP status, gender, and number of parents in household. We find that there is very little correlation between baseline test scores and such baseline measures. In analysis not presented, a principal components analysis also formally corroborates that our test score measures are indeed orthogonal to these measures.

<sup>29</sup>This quote is from the [Charleston Gazette-Mail](#).

## Secondary Academic Outcomes and Behaviors and Robustness

Table 4, Panel A, presents the effects on students' marking period course grades in more detail. Column one shows the effects on the number of failed courses, as before, but columns two through five show the effects on the number of D's, C's, B's and A's students received as well. The intervention shifted students failing grades to C grades. Column one shows the large and significant negative effect in the number of F's students receive, presented previously. Column three shows a large, positive effect—a 0.3 point increase—in the number of C's a student receives. The coefficients on the number of B's and A's are negative and positive, respectively, but neither estimate is statistically significant. Overall, the evidence suggests that the treatment caused students to receive fewer F's and more C's. A primary facet of the intervention was to alert parents when their child's grade dips below a C, so parents know when their child is receiving a low grade. This is also consistent with the positive impacts on below-median GPA students, which we discuss when we present results on heterogeneous effects.

In Table 4, Panel B, we look closer at assignment scores, missed assignments, and class test scores. Column one shows that assignment scores improved by 0.06 standard deviations over the control group. On average, the control group did not submit 12% of their assignments, which included classwork, exams and homework. There is a 1.1% reduction in the number of missed assignments. As we show later, the effects are driven entirely by high school students, who experienced significant—both statistically and in magnitude—reductions in missed assignments.<sup>30</sup>

In contrast to the states' standardized test scores, scores on in-class exams increased by 0.10 standard deviations. One important difference between class tests and the standardized tests, among several, is that these scores count for students' grades and so contributed to the likelihood of a students' parent being alerted or not. The latter may have provided

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<sup>30</sup>In results not shown, all students (both treatment and control) were much less likely to miss class tests—68% less likely—than any other type of assignment.

added incentive for students to do well on these tests as a result of the alerts.<sup>31</sup> Comparing column one to column three, the treatment effects are suggestively larger for exams (which are worth more points than other assignments) than assignments overall, but this difference is not statistically significant.<sup>32</sup>

Table 4, Panel C, presents treatment effects on GPA, suspensions, and student logins. We find a positive, though insignificant, coefficient on GPA of 0.068 points. The impacts on suspensions and student logins are also insignificant. As discussed later, the effect on GPA is strong for students in high school and students with below-median GPAs at baseline. Overall, the improved assignment scores and net positive coefficient on GPA overall is encouraging. It is possible that students had held their effort constant and reallocated it toward their failing courses. The latter would not necessarily be negative given that it would result in increased credit completion, but the effects on attendance, assignment scores, and GPA provide evidence of overall net increase in student effort.

## Mechanisms and Heterogeneity in Effects

We hypothesized that the intervention could improve the accuracy of parents' beliefs about their child's performance. To study parental beliefs, we asked parents whether they thought their child missed no assignments, 1-5 assignments, 6-10 assignments, or more than 10 assignments during the last semester. We show the effects on parents' beliefs about the number of assignments their child had missed in Table 5. Fifty-three percent of parents in the control group believed their child missed zero assignments in the last semester. The treatment reduced this belief by 11 percentage points. We can see from the remaining columns that the treatment shifted responses away from no missed assignments across the remaining categories. There is a statistically significant, six percentage point increase, in the likelihood parents respond that their child has missed 6-10 assignments. Only six percent of the control group believed their child missed 6-10 assignments.

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<sup>31</sup>Recent research has shown how higher stakes causes increased test scores for U.S. students (Hitt, 2016; Zamarro et al., 2016; List et al., 2017), which can occur by reducing irrelevant answers to questions, such as writing in nonsensical words for questions with numeric answers or as using "careless" and "inconsistent" answering patterns as Hitt (2016) and Zamarro et al. (2016) find evidence of on the PISA exam.

<sup>32</sup>Appendix A.6 shows that all of these results are robust to other approaches to outlier observations.

Figure 5A compares these beliefs about missed assignments to the number of actual missed assignments documented in the administrative data. This figure, which depicts the absolute categorical differences in parental beliefs of missed assignments minus actual missed assignments, makes it apparent that there is no treatment effect on the accuracy of parents' beliefs about their assignment completion despite the large reduction in the share of parents who believed their child missed no assignments. Figure 5B shows a similar representation of parents' beliefs about their child's math grades relative to the grade in the administrative data. Here, there is a more visible improvement in parents' accuracy: the share of parents who accurately reported their child's grade increased by 9 percentage points and the magnitude of their errors tends to be smaller as well. We show this difference in a regression, discussed below, but a test of these distributions finds they are significantly different at the 5% level as well.<sup>33</sup>

We also examine several additional mechanisms. First, Table 6 shows several behavioral responses to the treatment by parents as measured by survey responses. Column one in Panel A shows that parents were much more likely to contact the school as a result of the intervention. The share of families who contacted the school at least once over the course of the semester increased by 12 percentage points. This result is in line with Bergman (2014), who found that parents contacted the school more often in response to additional information.

The treatment effect on parent logins to the parent portal to view their child's grades is not statistically significant, as was parents speaking to their child about schoolwork daily. Parents were also asked whether they took away any privileges during the last month of school for not doing their schoolwork, which is coded as an indicator for yes or no. The effect is positive and not statistically significant, though it is close to conventional levels of marginal significance (two-sided test;  $p\text{-value} < 0.14$ ). The question is worded slightly differently, but Bergman (2014) found parents were significantly more likely to take away privileges from their children.

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<sup>33</sup>We use a permutation test to compare the distributions ( $p\text{-value}$  is 0.048).

As reported above, column two of Panel B shows parents became significantly more accurate about their child’s grade in math class. The third column of Panel B shows where parents would like to continue the text message intervention. A high share—94%—of the control group would like to receive the intervention. The latter is not surprising, but what is encouraging is that the treatment caused a significant increase in parents’ demand for the text messages of four percentage points. This suggests that any new information parents learned from receiving the alerts increased their subsequent demand for the intervention.

Finally, we also examine heterogeneity of effects on various subgroups. Given that the intervention targeted those with low grades and attendance, we are particularly interested in the subgroup of students who began the study with below-median GPAs. These heterogeneous effects also highlight some mechanisms. For instance, we see that biased beliefs about students’ grades and poor parent-child communication positively correlate with students in high school and students with low GPAs (see Appendix A.1), and we examine how the treatment effects differ according to these subgroups that exhibit larger biases. We pre-specified the following groups: students with below-median grades (by grade level), students whose father received the messages versus those whose mother received the messages, and students in middle school compared to students in high school. Tables 7 and 8 present analyses for these groups.

Table 7, Panel A, shows that students with below-median GPA failed 0.6 fewer classes, attended 42 more classes, and saw retention rates improve by three percentage points. All of these effects are significant at the 5% level. As before, there were no impacts on state test scores, but students’ GPAs increased by 0.167 points, which is also significant at the 5% level. Appendix A.7 presents estimates showing that students with below-median GPAs missed 20% (3 percentage points) fewer assignments. As suggested above, these heterogeneous results are consistent with our survey findings showing a negative correlation between parents who believe that their child does not disclose their academic progress and their child’s GPA.

Furthermore, for this subgroup of students, the treatment effect of message receipt on parents’ desire to continue the intervention is 11 percentage points and significant at the 1%

level (results not shown). This effect on the desire to continue is significantly different from the effect on parents of children with above-average GPA, who expressed no greater desire to continue the intervention than the control group (however, the mean for the latter is already above 90%). This is further evidence of larger benefits for families whose children had lower baseline GPAs in the sample.

Table 7, Panels B and C, shows that high school students were also more positively impacted than the average student. These students failed 0.5 fewer classes, attended 31 more classes, and were three percentage points more likely to remain in the district. Moreover, these effects are substantially different from the effects on middle school students, shown in Panel C. The effects for the latter group are nearly all smaller and statistically insignificant, with the exception attendance. Additionally, we find that high school students also missed many fewer assignments (13% or two percentage points) than middle school students (where the coefficient is close to zero).<sup>34</sup>

We examine several possible explanations for the lack of effects in middle school. First, we examine the first stage of the treatment on the alerts for the middle and high school subgroups (see Appendix A.8). The share of parents alerted in both groups is nearly identical, but columns two and four show how many times high school parents were alerted and middle school parents were alerted, respectively. Parents of high school students received roughly 50% more alerts, which is statistically different at the 1% level. This difference is largely because middle school students performed much better in their grades than high school students. For instance, middle school students' GPAs were one half-standard deviation higher than high school students' GPAs at baseline (in addition to receiving fewer alerts, this also implies they had less room to improve). Second, we find that parents of high school students had more inaccurate beliefs. For instance, parents of middle school students were 8 percentage points more likely to accurately recall their child's last math grades.<sup>35</sup> As discussed above, parents' beliefs about their child's math grades became significantly more accurate as a result of the intervention. In results not shown, this effect is driven

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<sup>34</sup>See Appendix A.7

<sup>35</sup>Much of this difference is explained by high school students' lower GPAs.

almost entirely by high school students' parents and is statistically different than the effect on middle school parents' beliefs at the 10% level.

Table 8 shows the effects for targeting information to mothers and the fathers. While there are slight differences in effects by gender of the treated parent, the coefficients are similar in sign and show no clear pattern. Targeted fathers saw their children experience slightly better results in terms of classes failed and classes attended. In this context, we find no clear evidence that targeting one parent versus another yields different results.

Three additional groups were determined ex post to be of interest: Students who were consistently absent, failed a course at baseline, and parents who never logged in to the parent portal at baseline. The first two subgroups identify lower-performing students according to outcomes we provided alerts about, and the third identifies families who may have poor access to information and inaccurate beliefs.<sup>36</sup>

We find that students with an above-median number of absences during baseline, students had effects that are stronger than average (see Appendix A.9). These students decreased their failed courses by 0.46 courses (from a control mean of 1.44), increased class attendance by 10%, and increased retention by two percentage points. As before, there are still no effects on standardized test scores. For students who failed at least one course at baseline, we find evidence of larger decreases in course failures and larger increases on retention (see Appendix A.10). However, there are insignificant, though positive, effects on attendance, math scores, and reading scores.

Given the lack of effect on standardized test scores, a key question is whether there were effects on these test scores for any subgroup. We conducted exploratory analyses to answer this question. Parents with less than a college education saw positive effects on their child's test scores, but this is a small subgroup as the measure of parents' education is based on surveys. To corroborate this finding and expand the sample beyond the survey respondents, we linked families to census-tract level data on college attainment and household income levels. Families living in tracts with below-median income or below-median college attainment

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<sup>36</sup>Parents who have never logged in to view their child's grades predicts inaccurate beliefs.

relative to the rest of the sample saw larger effects, which were similar to the effects found for high school students, but not in terms of test scores.<sup>37</sup> We do find evidence that the intervention may be particularly effective for parents who may have the least information about their children’s academic performance, however. For parents who never logged in to the parent portal during baseline, math scores increased by 0.08 standard deviations, though there was no significant effect on reading scores. These students’ courses failed dropped by 0.3, GPA improved by 0.09 points, and class attended improved by 14% (see Appendix A.11). These results should be viewed with caution as they are exploratory. We note them here in case they prove useful in defining subgroups worthy of study in future research.

## Second-Year Impacts and Alternative Explanations

The district permitted us to continue the intervention into a second year, however we were only able to collect data from the teacher gradebooks. By that year, KCS had discontinued their standardized tests in all but the 11th grade. In the summer prior to the second year of the intervention, flooding throughout the county significantly disrupted student learning. For instance, students attending one middle school and one high school only attended half days of school because flooded school sites were closed. Middle school students attended classes in the morning and high schools students in the afternoon. Nonetheless, our intervention tracked students even if they moved schools within the county, though the second-year results must be interpreted within this context. The sample is reduced to 1,031 students because 12th-grade students from the first year graduated.

We report results for the overall sample and the subgroups that appeared to drive the initial-year results: the high school sample, the below-median GPA sample, and the middle-school sample. The latter saw little benefit in the first year of the intervention.

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<sup>37</sup>Results of income and parent educational attainment are shown in the Appendix. Appendix A.12 shows that students residing in census tracts where households have below-median income in Kanawha County were more positively impacted than those from census tracts with above median income households. This is especially true for the amount of classes failed and classes attended, where students from below median income households showed substantially larger effects. In terms of education levels, the differential effects were more modest, as seen in Appendix A.13, which shows the differences between students living in households from census tracts with below and above-median bachelor’s degree attainment in Kanawha county. The magnitude and significance of effects in classes failed and classes attended were both substantially larger for students from below median education households. Overall, the intervention seems to affect families from low socioeconomic backgrounds more so than better-off families. However, there are still no substantial effects on test scores.

Estimates for outcomes found in the course transcripts can be found in [Appendix A.14](#). The outcomes include courses failed, classes attended, retention, and overall GPA during the 2016-2017 school year. The magnitude of reduction in courses failed, improvements in attendance, and retention are all similar to first year effects and still statistically significant. The effect on overall GPA is nearly identical in magnitude and insignificant. Overall, the subgroups whose outcomes improved in year one continued to see beneficial effects in year two while the middle school effects remained insignificant. Particularly encouraging is that the below-median GPA subgroup continued to see positive effects in failed courses, attendance, and retention.

Evidence regarding the ability for informational interventions to persist is mixed (cf. [Allcott and Rogers, 2014](#)). Parents could have become habituated and unresponsive to a continual stream of information over a longer period of time. Though a second year of implementation does not provide conclusive evidence on its long-run effects, that results persist into a second year is nonetheless encouraging. Our results cannot inform, however, whether impacts would persist after the intervention ends.

One concern with the outcomes studied above is that teachers and students, though blinded to the random assignment, may have changed their behaviors as well. Teachers may have shifted attention or increased the frequency with which they grade assignments, and, though the intervention was randomly assigned at the school-by-grade level so that control students were always in a different grade than treated students in order to mitigate spillovers, spillovers may have occurred nonetheless.

We assess these hypotheses in several ways. First, we observe teacher logins into the gradebook, which would allow us to discern if teachers increased the frequency with which they record grades in their classrooms. Though this would be interesting if teachers had responded in this fashion, we find no impacts of the intervention on teacher logins into the gradebook for teachers who taught in treated grades compared to those teachers who did not. Second, we can observe gradebook outcomes for students who were not randomized into the intervention or the control group—the remainder of the initial recruitment sample

frame. Among these students, we can compare those students who were in treated school grades (but were not treated because they were not part of the randomization process) and compare their outcomes to students who were not in treated grades (and were also not part of the randomization process). If there were evidence of spillovers, we might find that, among students who were not randomized, those in treated grades have different outcomes than students in untreated grades.<sup>38</sup> We regress these gradebook outcomes on an indicator for being in a treated grade to look at these spillovers for the sample of students not randomized. We find that, across GPA, course failures, retention and class attendance, there is no evidence of spillovers—either positive or negative (see Appendix A.15).

Teachers also may have been wary of parents’ complaints as a result of the increased school-to-parent communication and then inflated treated students’ grades to avoid this reaction. We look at this possibility by examining the assignment data in greater detail. One easy way teachers could inflate grades is through “participation” grades, which may be viewed as more subjective grades relative to other assignments. Participation grades were typically marked as full credit or not—84% of participation grades are 100% scores. We therefore create an indicator for receiving a full participation grade (100%) or not. We find that there is no effect of the intervention on receiving a full participation score (see Appendix A.16). Another way teachers could have either inflated grades or reduced alerts is by marking certain assignments as “excused.” We examine whether there is a change in the share of assignments marked as excused. Again, there is no significant effect and the point estimate is essentially zero.

## VI Conclusion, Scalability and External Validity

We helped design and implement an automated-text messaging intervention to test whether this technology can resolve parent-child information problems and improve student achievement at scale. Our intervention sends automated weekly and monthly text message alerts to parents when their child misses a class or an assignment, or if they have a low course av-

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<sup>38</sup>Among students who were not part of the randomization process, we only have outcomes from the gradebook.

erage. A unique feature of the intervention is that it targets class-level absences as opposed to full-day absences.

We find receiving alerts significantly reduces class-level absences, along with course failures and district attrition, though not state test scores. These effects are larger for lower-performing students and students in high school, who also miss fewer assignments and have higher in-class exam scores as a result. Notably, the effects are negligible for middle school students. Overall, students saw a shift in grades from F's to C's. This is particularly important given the predictive power of grades on the likelihood of dropping out (McKee and Caldarella, 2016) and college success (Belfield and Crosta, 2012; Vulperhorst et al., 2018; Olani, 2009). Parents' beliefs about their child's grades become more accurate, but the evidence is more mixed regarding the accuracy of their child's missed assignments. We also find increases in parents' contact with schools to discuss their child's academic progress, and this is concentrated among middle school parents.

This intervention is low cost relative to other education interventions aimed at student achievement. The marginal cost of each text message is less than a fraction of one cent. Many schools already have district-wide gradebooks. Even if a school were to adopt the entire system in this study and receive training for how to use it, the cost would be \$7 per student.

Given this low cost and policy relevance, an important question is whether this intervention would work in other contexts and whether this intervention would be adopted by parents and teachers in practice. This paper does not specifically study the adoption of the intervention by parents. However, Bergman and Rogers (2017) worked with Washington D.C. Public Schools to examine how varying district opt-in policies can drastically affect the adoption and, in turn, the efficacy of this particular text-message intervention. They find that when parents are randomly assigned to opt in parents by default, fewer than 5% of parents choose to subsequently opt out at any point during the school year. When parents are randomly assigned to actively opt in, even when this opt in is as easy as replying to a text message offer, adoption rates are significantly lower. Bergman and Rogers also find sig-

nificant reductions in courses failed and increases in student GPA, especially for high school students. The latter study did not require active consent as it was a district-initiated intervention. In the current study, less than 2% subsequently opted out over the course of the treatment period. Another aspect to scale is that not all districts have a unified gradebook system. For instance, Chicago Public Schools, and District of Columbia Public Schools do have a unified system, however, New York City does not. Our findings suggest that such a unified grade-entry system can be a valuable tool if combined with “push” information to parents, as opposed to website-only, “pull” information.

There are other open questions. For instance, we do not provide evidence on varying the frequencies, timing, and content of the information to send to parents. In our study, we target messages to low-performing students and we do not know if tailored alerts could reach higher-performing students along other academic margins. Nor do we study how this intervention interacts with the amount of information parents already receive from various sources, and how the presence of additional information may interact with the mode in which this information is sent. Though possible, we speculate that the primary concern is not whether there will be many, individually-successful text-message interventions inundating parents simultaneously. Rather, an alternative scenario is that school districts begin to recognize that text messages garner parents’ attention and proceed to send even *pro forma* communications via text, and parents subsequently ignore all text messages. An important question for future research is how information interventions interact with the mode of communication and the existing amount of information flowing to recipients.

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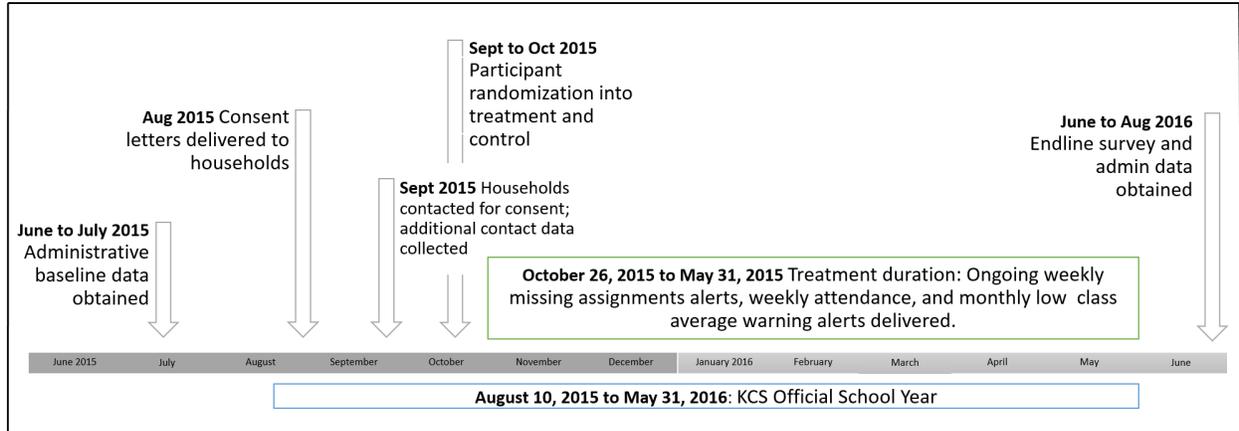
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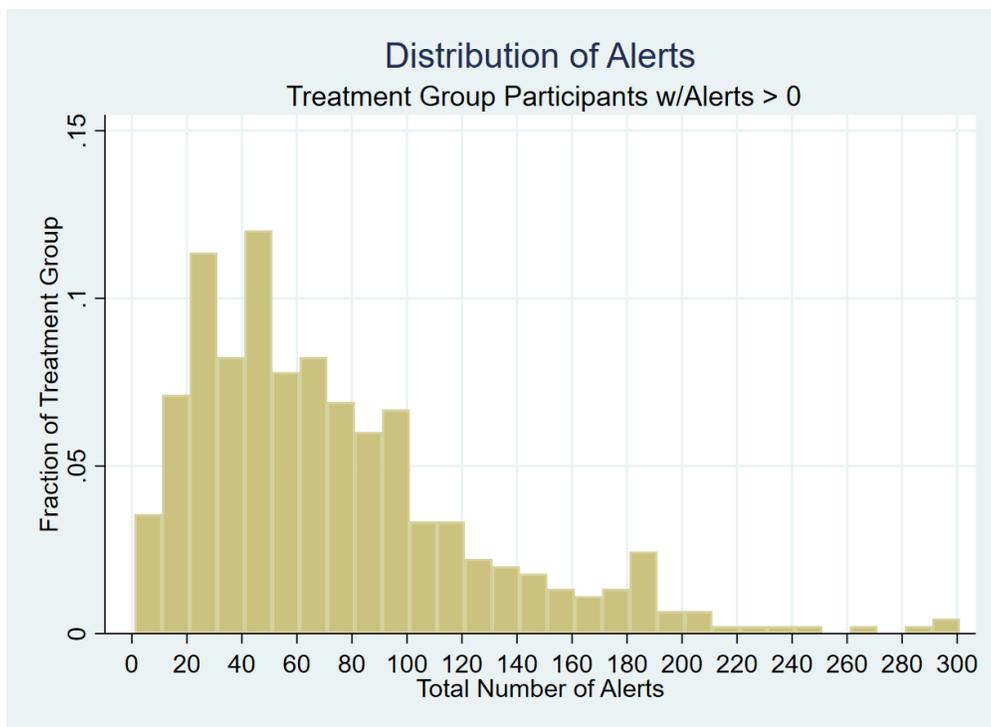
## VII Figures

Figure 1: Field Experiment Timeline



This figure shows the timeline of the project, which began during the summer of 2015 and lasted through the summer of 2016, when data collection ended formally. Later on, the district allowed continuation of the treatment for a second school year in 2016-2017, but only district administrative data was obtained.

Figure 2: Treatment parents distribution of alerts (Nonzero alerts receipt)



This figure shows the distribution of alerts for treatment parents who received at least one alert. 27.5% of treatment parents received zero alerts.

Figure 3: How often parent (A) talks to child and (B) another adult in the household about child’s schoolwork: Control group survey results

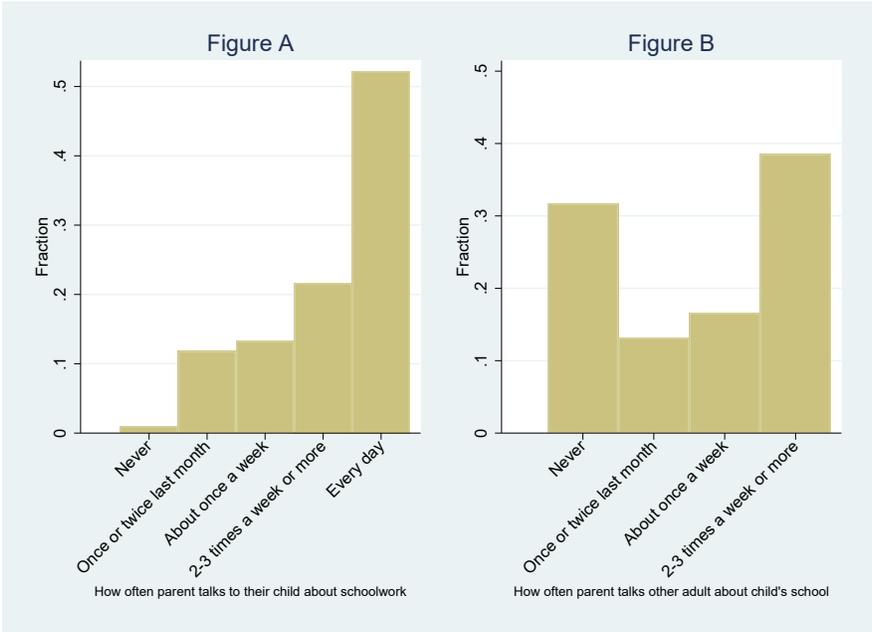


Figure A shows the frequency of parents talking to their child about schoolwork for the control group. Figure B shows the frequency of parents talking to another adult about schoolwork for the control group. Results are from endline parent survey.

Figure 4: Control group parents' beliefs about missed assignments

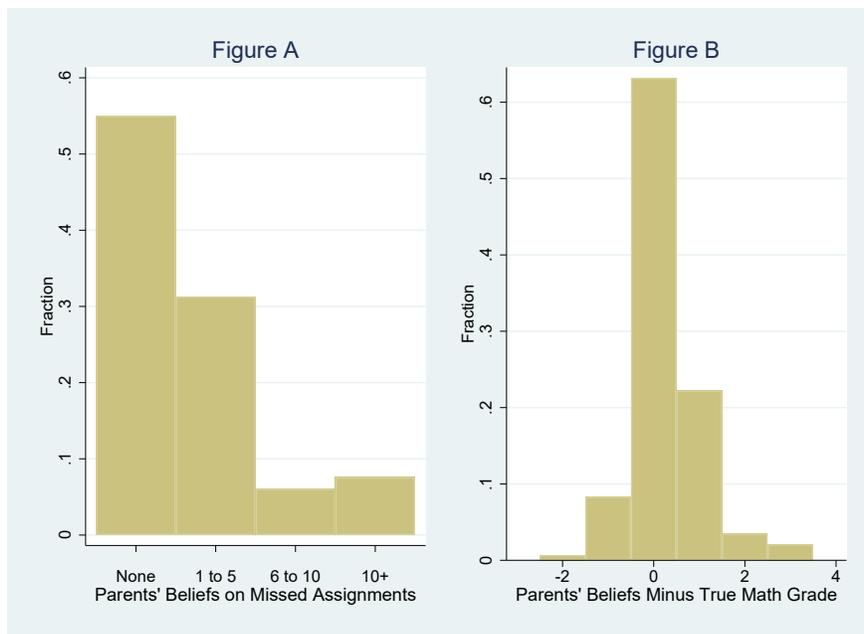


Figure A shows the fraction of parents in the control group who believe their child missed zero, between one to five, between six and ten, or more than ten assignments in the last semester. Figure B shows the inaccuracy of parental beliefs of math grade against actual grade. Calculations are made by subtracting actual math grade from parent's guess of student's math grade. Results to the right of zero shows the fraction of parents who overestimate a student's grade, and those to the left shows the fractions of parent who underestimate a student's grade. Results are calculated from endline parent survey and gradebook data calculations.

Figure 5: Treatment-Control Comparisons of Parents' Beliefs about (A) Missed Assignments and (B) Math Grades

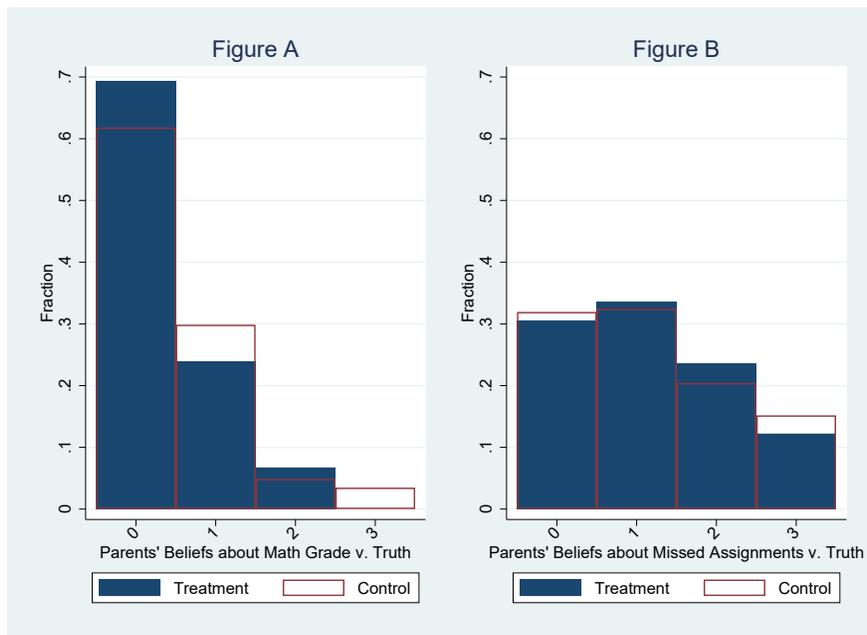


Figure A shows the treatment-control comparisons of parental belief of number of missed assignments versus actual number of missed assignments. The calculations are absolute values of the inaccuracy by categorical bins in which parents estimate their child's missed assignments - zero (0), one to five (1), six to ten (2), and more than ten (3). For example, if a parent estimated that their child missed six to ten assignments, but they actually missed more than ten, they would be off by a category of one. Figure B shows the treatment-control comparisons of parental belief of their child's math grade compared to their actual grade. The calculations are absolute values of the inaccuracy by math grade GPA, based on a 4.0 scale. For example, if a child received a B, but their parent believed they received an A, the parent would be off by an absolute value of one.

## VIII Tables

Table 1: Treatment-Control Group Balance and Non-Participant Descriptive Statistics

Variable	Panel A. Treatment (T) - Control (C) Balance				Panel B. Nonparticipants (NP) vs. Participants (P)			
	Control Mean	T-C Diff	P-Value	Obs	NP Mean	NP-P Diff	P-Value	Obs
Female	0.490	-0.011	0.685	1,137	0.494	0.013	0.400	13,911
Black	0.156	0.037	0.370	1,137	0.117	-0.061	0.000	13,911
ELL	0.019	-0.002	0.775	1,137	0.016	-0.004	0.328	13,911
IEP	0.131	0.006	0.743	1,137	0.146	0.014	0.202	13,911
Baseline Math	0.000	0.046	0.538	1,137	-	-	-	-
Baseline Reading	0.000	0.014	0.842	1,137	-	-	-	-
Suspended Last Year	0.201	0.006	0.660	1,137	0.198	-0.015	0.218	13,911
Baseline Parent Logins	15.261	-0.675	0.827	1,137	13.391	-1.960	0.183	12,943
Baseline Parent Ever Login	0.359	0.022	0.537	1,137	0.314	-0.049	0.001	12,943
Baseline Student Logins	93.544	-2.014	0.492	1,137	91.961	-3.909	0.119	12,943
Baseline Student Ever Login	0.961	-0.007	0.676	1,137	0.939	-0.027	0.000	12,943
Baseline GPA	2.819	0.010	0.784	1,137	2.844	-0.040	0.130	13,832
Baseline % of Days Missed	0.056	0.004	0.160	1,137	0.063	0.001	0.753	12,908
Baseline % of Classes Missed	0.101	-0.008	0.520	1,131	0.099	-0.008	0.043	12,859
Parents in the Household	1.770	-0.029	0.374	1,137	1.820	0.068	0.000	13,911

This table shows the balance on covariates between randomized treatment and control groups. P-values are for tests of equality of means across the treatment and control group via a regression of the baseline covariate on an indicator for treatment status. Standard errors clustered at the school-grade level. All regressions include strata indicators. The last four columns show the means for nonparticipants of the study, along with differences when means are compared to the experimental sample. For six of these measures in Panel B, (Baseline Parent Logins, Baseline Parent Ever Login, Baseline Student Logins, Baseline % of Days Missed, and Baseline % of Classes Missed) we show the differences conditional on students who attended at least one class at baseline. This is because students with high classes missed at baseline tend not to enroll during the following school year; No parent of a student who would not attend a class during the intervention year enrolled in the study. Baseline Math and Reading scores were not available for nonparticipants.

Table 2: Measures of Attrition

	Miss Survey	Miss Suspension	Miss Math	Miss Reading
Treatment	-0.016 (0.029)	-0.000 (0.010)	0.008 (0.015)	0.010 (0.015)
Percent of Days Missed	-0.560** (0.26)	0.294 (0.200)	0.770** (0.320)	0.830** (0.330)
Baseline GPA	0.016 (0.022)	-0.003 (0.014)	0.008 (0.016)	0.015 (0.016)
Black	0.024 (0.030)	-0.007 (0.015)	-0.022 (0.014)	-0.019 (0.015)
IEP	-0.064 (0.040)	-0.008 (0.022)	0.040 (0.017)	0.042* (0.023)
Female	-0.014 (0.029)	-0.010 (0.009)	-0.011 (0.014)	-0.008 (0.015)
Baseline Math Score	0.034 (0.025)	0.004 (0.004)	0.026** (0.011)	0.020* (0.011)
Baseline Reading Score	0.012 (0.028)	0.004 (0.006)	-0.024* (0.012)	-0.021* (0.011)
Observations	1,137	1,137	1,137	1,137

This table shows the correlates of several indicators of attrition and non response: survey non-response, missing endline GPA and missing endline test scores. Standard errors are clustered at the grade-school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table 3: Effects on Primary Academic Outcomes

	Classes Failed	Classes Attended	Retained	Math Score	Reading Score
Treatment	-0.269*** (0.100)	34.19** (16.406)	0.015** (0.008)	-0.004 (0.044)	-0.056 (0.040)
Control Mean	0.974	277.700	0.973	0.000	0.000
Observations	1,113	1,137	1,137	927	925

This table shows intent-to-treat (ITT) treatment effect estimates on primary academic outcomes specified in the pre-registered analysis plan. All regressions include strata indicators and a set of demographic covariates described in the text. Standard errors are clustered at the grade-school level. Outcome variables are from gradebook and administrative data. Classes failed are total failed courses after treatment started. Classes attended is the numerical total of classes marked as present after treatment started. Retention is defined as taking courses one marking period after the intervention began. Math and Reading scores are z scores from standardized test scores. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table 4: Effects on Other Student-Level Academic Outcomes

Panel A.		Student Grades			
	<u>F Grades</u>	<u>D Grades</u>	<u>C Grades</u>	<u>B Grades</u>	<u>A Grades</u>
Treatment	-0.269*** (0.100)	0.072 (0.070)	0.207** (0.084)	-0.121 (0.085)	0.180 (0.186)
Control Mean	0.974	0.841	1.319	1.790	3.329
Observations	1,113	1,113	1,113	1,113	1,113
Panel B.		Assignment Scores, Missed Assignments, and Class Exams			
	<u>Assignment Scores</u>	<u>Missed Assignments</u>	<u>Class Exams</u>		
Treatment	0.063*** (0.018)	-0.011 (0.009)	0.104*** (0.033)		
Control Mean	0.018	0.119	0.008		
Observations	70,076	91,954	7,342		
Panel C.		Other Student Outcomes			
	<u>GPA</u>	<u>Ever Suspended</u>	<u>Student Logins</u>		
Treatment	0.068 (0.045)	-0.009 (0.015)	-2.652 (11.696)		
Control Mean	2.615	0.225	209.800		
Observations	1,137	967	1,137		

This table shows the intent-to-treat treatment effect estimates on the number of each grade students received after the treatment began in Panel A, the effects on student assignment scores, the share of assignment missed and standardized in-class test scores in Panel B, and secondary student outcomes of interest in Panel C. For Panel B, assignment and exam scores are standardized according to the control group's score for each assignment or exam. Outliers more than 3 standard deviations away from the mean are excluded. Missed assignments is an indicator for a missing assignment and includes assignments and exams, including assignments marked "m" for missing in the gradebook. There are multiple observations per student because there are multiple assignments or exams per student after the intervention began. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$

Table 5: Parent Beliefs about Missed Assignments

	None	1-5	6-10	>10	Don't Know
Treatment	-0.107** (0.044)	0.048 (0.036)	0.061** (0.027)	0.012 (0.025)	-0.005 (0.012)
Control Mean	0.532	0.308	0.061	0.071	0.025
Observations	403	403	403	403	403

This table shows intent-to-treat estimates on parent beliefs about missed assignments. All regressions include strata indicators and a set of demographic covariates described in the text. Standard errors are clustered at the grade-school level. Outcome variables are constructed from survey results asking parents to give the number of missed assignments by students during the last semester. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table 6: Parents' Behavioral Responses

Panel A.	Contacted the School	Talked w/ Child	Parent Logins
Treatment	0.119** (0.047)	0.050 (0.041)	4.988 (4.629)
Control Mean	0.327	0.739	30.10
Observations	443	438	1,137
Panel B.	Took Privileges	Grade Inaccuracy	Continue Texts
Treatment	0.056 (0.040)	-0.133* (0.070)	0.031** (0.016)
Control Mean	0.317	0.500	0.945
Observations	401	307	433

This table shows intent-to-treat estimates on parents' behavioral responses. All regressions include strata indicators and a set of demographic covariates. Standard errors clustered at the grade-school level. Outcome variables here are based on survey results and gradebook data. Panel A shows results for an indicator for whether parents contacted the school, an indicator of whether parents talked to their child about school about schoolwork or grades, and total parent logins into the parent gradebook portal. Panel B shows the results for parents taking privileges away from student in the last month of school, the difference between students' actual math grade and parents' estimated math grade, and an indicator for parents' desire to start or continue a texting service to inform them about their child's academic progress. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table 7: Effects on Academic Subgroups of Interest

Panel A.		<b>Below Average GPA Subgroup</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.578*** (0.187)	0.167** (0.070)	42.406** (18.069)	-0.031** (0.013)	0.029 (0.063)	-0.003 (0.056)
Control Mean	1.699	2.015	261.400	0.958	-0.414	-0.493
Observations	550	566	566	566	445	444
Panel B.		<b>High School Subgroup</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.496*** (0.157)	0.171*** (0.063)	30.898* (21.519)	0.029** (0.013)	0.001 (0.060)	-0.055 (0.050)
Control Mean	1.421	2.301	436.000	0.956	0.00	0.00
Observations	581	597	597	597	419	417
Panel C.		<b>Middle School Subgroup</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.039 (0.110)	-0.053 (0.064)	35.613 (24.475)	0.004 (0.007)	-0.015 (0.063)	-0.059 (0.056)
Control Mean	0.561	2.915	126.6	0.989	0.000	0.000
Observations	532	540	540	540	508	508

This table shows the effects by subgroups of interest, in this case students with below-median GPA at baseline in Panel A, high school students in Panel B, and middle school students in Panel C. Treatment effects are intent-to-treat estimates. All regression include strata indicators and a set of demographic covariates described in the text. Standard errors are clustered at the grade-school level. All regressions include strata indicators. Outcome variables are from gradebook and administrative data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table 8: Effects by Subgroups of Mother and Father Alerted

Panel A.		<b>Mothers Alerted</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.262** (0.130)	0.070 (0.067)	22.947* (15.944)	0.018* (0.013)	-0.011 (0.057)	0.081 (0.061)
Control Mean	0.966	2.693	280.7	0.976	0.015	-0.052
Observations	423	431	431	431	345	346
Panel B.		<b>Fathers Alerted</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.356** (0.167)	0.06 (0.085)	28.294* (20.622)	0.010 (0.016)	0.072 (0.066)	-0.055 (0.072)
Control Mean	0.924	2.724	272.2	0.986	0.0230	0.0490
Observations	319	324	324	324	266	266

This table shows the results by subgroups of interest, in this case students with either their mothers or fathers receiving the text messages. The sample is restricted to those households with two parents. Treatment effects intent-to-treat estimates. All regressions include strata indicators and a set of demographic covariates as described in the text. Standard errors are clustered at the grade-school level. All regressions include strata indicators. Outcome variables are from gradebook and administrative data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

## Appendix A

Table A.1: Correlates of Communication

	Contact < 1x /3 mo.	Talk to Another Adult	Grade Inaccuracy	Child Discloses
Fraction Absent	-0.29 (0.33)	0.37 (0.30)	0.30 (0.98)	0.14 (0.36)
Ever Suspended	0.12 (0.08)	-0.08 (0.07)	-0.06 (0.13)	-0.04 (0.08)
GPA	0.03 (0.03)	0.01 (0.025)	-0.17*** (0.06)	-0.18*** (0.02)
Black	0.03 (0.07)	-0.05 (0.06)	-0.014 (0.09)	-0.07 (0.06)
IEP	-0.00 (0.08)	0.01 (0.07)	0.23 (0.15)	0.04 (0.08)
Female	0.04 (0.05)	-0.08* (0.05)	0.10 (0.08)	-0.05 (0.05)
Two Parents	-0.03 (0.06)	0.18*** (0.06)	0.04 (0.09)	-0.08 (0.05)
Parent Female	0.06 (0.05)	-0.01 (0.05)	-0.01 (0.09)	-0.00 (0.05)
High School	-0.05 (0.05)	-0.04 (0.04)	0.12 (0.09)	-0.14*** (0.05)
Control Mean	0.46	0.69	0.50	0.48
Observations	423	439	307	439

This table shows the correlates of several indicators of parental and student communication behavior. Standard errors clustered by student. All regressions include strata indicators. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.2: Administrative Data on Alerts

	Alerted	Alerts	Assignment	Absence	Low Grade
Treated	0.705*** (0.018)	48.915*** (3.165)	21.612*** (1.215)	20.810*** (2.080)	6.458*** (0.411)
Control Mean	0.021	0.375	0.058	0.006	0.056
Observations	1,137	1,137	1,137	1,137	1,137

This table shows the likelihood and amount of times parents are alerted due to being randomized into treatment. Alerts is the number of alerts received. Assignment, Absence, and Low Grade are the number of alerts received by parents by alert type. All regressions include strata indicators and a set of demographic covariates described in the text. Standard errors are clustered at the grade-school level. Outcome variables are from gradebook and administrative data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

Table A.3: School to Parent Contact about Child's Academic Progress

	$\geq 1x$ / month	2x / month.	1x / month	1x / 2-3 month	< 1x / 3 month
Treatment	0.133** (0.055)	0.044 (0.049)	0.089** (0.041)	-0.049* (0.027)	-0.084 (0.051)
Control Mean	0.381	0.253	0.129	0.165	0.454
Observations	424	424	424	424	424

This table shows the intent-to-treat estimates for how often schools contacted parents in any way about their child's academic progress in the last semester. All regressions include strata indicators and a set of demographic covariates described in the text. Standard errors are clustered at the grade-school level. The outcome variables are all from endline parent surveys. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.4: Primary Academic Outcomes: Treatment-on-the-Treated Estimates

	Classes Failed	Classes Attended	Retained	Math Score	Reading Score
Alerted	-0.379*** (0.139)	48.465** (23.083)	0.021** (0.011)	-0.005 (0.060)	-0.076* (0.054)
Control Mean	0.974	277.700	0.973	0.000	0.000
Observations	1,113	1,137	1,137	927	925

This table shows treatment-on-the-treated (TOT) effect estimates on primary academic outcomes specified in the pre-registered analysis plan. Treatment effects are estimated using 2SLS regressions with the instrumented alerted variable, an indicator for parents who received at least one text. All regressions include strata indicators and a set of demographic covariates described in the text. Standard errors are clustered at the grade-school level. Outcome variables are from gradebook and administrative data. Classes failed are total failed courses after treatment started. Classes attended is the numerical total of classes marked as present after treatment started. Retention is defined as taking courses one marking period after the intervention began. Math and Reading scores are z scores from standardized test scores. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.5: Correlations with State Test Scores

	Math Score	Math Score	Reading Score	Reading Score
Baseline GPA	0.55*** (0.05)	0.75*** (0.06)	0.60*** (0.05)	0.86*** (0.05)
Baseline GPA $\times$ High School		-0.32*** (0.09)		-0.42*** (0.09)
Observations	927	927	925	925

This table shows the correlations between students' baseline GPAs and state math and reading test scores. Columns two and four interact the baseline GPA variable with an indicator for whether or not a student is in high school. Robust Standard errors in parentheses. GPA is constructed from gradebook data. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$

Table A.6: Robustness: Assignment Scores, Missed Assignments, Class Exams

Panel A.	Assignment Scores $< 2\sigma$	Class Exams $< 2\sigma$
Treatment	0.063*** (0.014)	0.094*** (0.030)
Observations	67,032	7,043
Panel B.	Assignment Scores $< 4\sigma$	Class Exams $< 4\sigma$
Treatment	0.062*** (0.022)	0.86** (0.035)
Observations	71,063	7,407
Panel C.	Assignment Scores $< 5\sigma$	Class Exams $< 5\sigma$
Treatment	0.050** (0.024)	0.074** (0.036)
Observations	71,512	7,439

This table shows treatment effects on student assignment scores and assignment completed with varying exclusion criteria for outliers. Panel A excludes all observation that are plus or minus two standard deviations from the mean. Panel B excludes all observation that are plus or minus four standard deviations from the mean. Panel C excludes all observation that are plus or minus five standard deviations from the mean. The estimates in the main text shows excludes outliers plus or minus three standard deviations from the mean. Effects are intent-to-treat estimates. All regressions include strata indicators and a set of demographic covariates described in the text. Outcome variables are calculated from the gradebook data. Assignment and exam scores are standardized according to the control group's score for each assignment or exam. There are multiple observations per student because there are multiple assignments or exams per student after the intervention began. Standard errors are clustered at the grade-school level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$

Table A.7: Missed Assignments by subgroups

Missed Assignments			
Sample	Low GPA	High School	Middle School
Treatment	-0.033** (0.015)	-0.023* (0.012)	0.004 (0.011)
Control Mean	0.151	0.151	0.094
Observations	42,993	46,475	45,479

This table shows treatment effects on assignments completed for three different subgroups: column one shows results for students with below-median GPA, column two shows results for high school students, and column three shows results for middle school students. Effects are intent-to-treat estimates. Standard errors are clustered at the grade-school level. Outcome variables are calculated from the gradebook data. Missed assignments is an indicator for a missing assignment and include assignments and exams, including assignments marked “m” in the gradebook. There are multiple observations per student because there are multiple assignments or exams per student after the intervention began. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.8: Alerts by Middle v. High School

	Alerted HS	Alerts HS	Alerted MS	Alerts MS
Treatment	0.705*** (0.018)	48.915*** (3.165)	0.694*** (0.024)	31.174*** (2.287)
Control Mean	0.021	0.375	0.038	0.75
Observations	597	597	540	540

This table shows the likelihood and amount of times parents are alerted due to being randomized into treatment, where HS indicates students in high school and MS indicates students in middle school. Alerted is an indicator for ever alerted. Alerts is the number of alerts received. All regressions include strata indicators and a set of demographic covariates described in the text. Standard errors are clustered at the grade-school level. Outcome variables are from gradebook and administrative data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.9: Subgroup conditional on above-median absences at baseline

	Classes Failed	Classes Attended	Retained	Math Score	Reading Score
Treatment	-0.458** (0.209)	26.895* (17.742)	0.019* (0.015)	0.048 (0.063)	0.009 (0.067)
Control Mean	1.437	254.600	0.965	-0.279	-0.227
Observations	519	535	535	416	412

This table shows the results by subgroup of interest, in this case students who had above-median absences at baseline. Treatment effects are intent-to-treat estimates. All regression include strata indicators and a set of demographic covariates as described in the text. The p-values are for a test of whether the coefficient on the interaction term between the mother indicator and the treatment variable is equal to zero. Standard errors are clustered at the grade-school level. All regressions include strata indicators. Outcome variables are from gradebook and administrative data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.10: Subgroup conditional on students who failed at least one course during baseline

	<u>Classes Failed</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.719*** (0.244)	20.094 (20.764)	0.048*** (0.019)	0.031 (0.056)	0.052 (0.069)
Control Mean	2.6658	308.200	0.957	-0.549	-0.655
Observations	353	368	368	273	272

This table shows the results by subgroup of interest, in this case students who failed at least one course during baseline. Treatment effects are intent-to-treat estimates. All regression include strata indicators and a set of demographic covariates as described in the text. The p-values are for a test of whether the coefficient on the interaction term between the mother indicator and the treatment variable is equal to zero. Standard errors are clustered at the grade-school level. All regressions include strata indicators. Outcome variables are from gradebook and administrative data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.11: Effects by Baseline Parents Ever Logged in

Panel A.		<b>Parents EVER logged during baseline</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.119 (0.114)	0.044 (0.069)	26.294 (19.864)	0.004 (0.017)	-0.097 (0.063)	0.095 (0.073)
Control Mean	0.636	2.693	258.6	0.989	0.079	0.050
Observations	413	416	416	416	362	362
Panel B.		<b>Parents NEVER logged during baseline</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.339** (0.138)	0.088* (0.051)	39.459** (16.372)	0.020* (0.011)	0.084* (0.050)	-0.036 (0.050)
Control Mean	1.169	2.500	288.4	0.964	-0.048	-0.030
Observations	700	721	721	721	565	563

This table shows the results by subgroups of interest, in this case students whose parents either logged in or did not log in to the parent portal at least once at baseline. Treatment effects intent-to-treat estimates. All regressions include strata indicators and a set of demographic covariates as described in the text. Standard errors are clustered at the grade-school level. All regressions include strata indicators. Outcome variables are from gradebook and administrative data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.12: Subgroup of Below- and Above-Median Household Income

Panel A.		<b>Below-Median Household Income</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.385*** (0.134)	0.117** (0.059)	54.676*** (18.573)	-0.025* (0.015)	-0.057 (0.061)	0.002 (0.066)
Control Mean	1.190	2.488	253.0	0.966	-0.064	-0.117
Observations	549	563	563	563	455	453
Panel B.		<b>Above-Median Household Income</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.094 (0.138)	0.002 (0.071)	10.437 (17.388)	-0.012 (0.008)	0.039 (0.054)	-0.075 (0.050)
Control Mean	0.799	2.719	298.1	0.979	0.0520	0.0950
Observations	564	574	574	574	472	472

This table shows the results by subgroups of interest, in this case students whose household income at the census tract level is below or above the median for Kanawha County. Treatment effects are intent-to-treat estimates. All regression include strata indicators and a set of demographic covariates as described in the text. Standard errors are clustered at the grade-school level. All regressions include strata indicators. Outcome variables are from gradebook and administrative data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.13: Subgroup of Below- and Above-Median Proportion Households with minimum Bachelors Degree

Panel A.		<b>Below-Median Education</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.432*** (0.158)	0.126* (0.069)	44.527** (17.750)	-0.018 (0.016)	-0.010 (0.065)	0.022 (0.068)
Control Mean	1.227	2.424	253.1	0.967	-0.183	-0.206
Observations	543	558	558	558	446	444
Panel B.		<b>Above-Median Education</b>				
	<u>Classes Failed</u>	<u>GPA</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>Math Score</u>	<u>Reading Score</u>
Treatment	-0.186 (0.117)	0.046 (0.059)	26.118 (20.448)	-0.012 (0.011)	-0.038 (0.055)	-0.093 (0.060)
Control Mean	0.748	2.788	299.9	0.978	0.160	0.178
Observations	570	579	579	579	481	481

This table shows the results by subgroups of interest, in this case students whose household's highest education is bachelors or higher at the census tract level is below or above the median for Kanawha County. Treatment effects are intent-to-treat estimates. All regression include strata indicators and a set of demographic covariates as described in the text. Standard errors are clustered at the grade-school level. All regressions include strata indicators. Outcome variables are from gradebook and administrative data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.14: Academic Outcomes: Year 2

Panel A.		<b>Entire Year 2 Sample</b>			
	<u>Classes Failed</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>GPA</u>	
Treatment	-0.284** (0.111)	33.725* (17.622)	0.018** (0.009)	0.049 (0.050)	
Observations	1,009	1,032	1,032	1,032	
Panel B.		<b>High School</b>			
	<u>Classes Failed</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>GPA</u>	
Treatment	-0.585** (0.192)	29.500 (24.550)	0.039 (0.016)	0.154* (0.077)	
Observations	440	492	492	492	
Panel C.		<b>Middle School</b>			
	<u>Classes Failed</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>GPA</u>	
Treatment	-0.039 (0.110)	35.613 (24.475)	-0.004 (0.007)	-0.053 (0.064)	
Observations	532	540	540	540	
Panel D.		<b>Below-Median GPA</b>			
	<u>Classes Failed</u>	<u>Classes Attended</u>	<u>Retained</u>	<u>GPA</u>	
Treatment	-0.683*** (0.200)	44.163** (19.869)	0.035* (0.015)	0.170 (0.077)	
Observations	498	514	514	514	

This table shows intent-to-treat effect estimates on academic outcomes during the second year of the intervention. All regressions include strata indicators and a set of demographic covariates described in the text. Standard errors are clustered at the grade-school level. Outcome variables are from gradebook data. Classes failed are total failed courses during the 2016-2017 academic year. Classes attended is the number of total of classes marked as present during the 2016-2017 academic year. Retention is defined as taking courses during the 2016-2017 academic year. The sample size is smaller than the Year 1 sample because Year 1 12th-grade students graduated. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$

Table A.15: Spillover Effects

	Classes Failed	Classes Attended	Retained	GPA
Treated Grade	0.056 (0.077)	18.635 (21.310)	0.008 (0.010)	-0.026 (0.041)
Observations	9,226	9,709	9,709	9,709

This table shows treatment effects on academic outcomes for those students who were not part of the randomization process, and therefore were not treated. The Treated Grade variable is an indicator for students who were in the same grade and school as students who were treated. All regressions and outcomes are constructed as described in the text for those who were randomized, and therefore include strata indicators and a set of demographic covariates described in the text. Standard errors are clustered at the grade-school level. Outcome variables are from gradebook data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

Table A.16: Participation Grades and Excused Assignments

	Participation	Excused
Alerted	-0.041 (0.048)	0.002 (0.005)
Observations	4,932	80,643

This table shows treatment effects on participation grades and excused assignments. Column one shows the effects on participation grades, which is an indicator for receiving a full mark on participation grades in the gradebook. Column two shows the effects on an indicator for an assignment being marked by the teacher as “excused.” Effects are intent-to-treat estimates. All regressions include strata indicators and a set of demographic covariates described in the text. Outcome variables are calculated from the gradebook data. There are multiple observations per student because there are multiple scores per student after the intervention began. Standard errors are clustered at the grade-school level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

## Appendix B

Table B.1: Primary outcomes and their sources

Outcome	Source
Number of failed courses	Administrative Data
Number of classes present	Administrative Data
Retention	Administrative Data
Math scores	Administrative Data
English scores	Administrative Data

Table B.2: Primary outcomes and their construction

Outcome	Construction
Number of failed courses	Total number of 2nd semester courses with an F
Number of classes present	Total number of classes marked either present or tardy after the intervention began
Retention	Indicator for whether a student took 2nd semester courses
Math scores	Math scores standardized by control group mean and standard deviation by grade
English scores	English scores standardized by control group mean and standard deviation by grade

Table B.3: Primary outcomes and hypothesized effect

Outcome	Test
Number of failed courses	ATE > 0
Number of classes present	ATE > 0
Retention	ATE > 0
Math scores	ATE > 0
English scores	ATE > 0

Table B.4: Secondary outcomes and their sources

Outcome	Source
Number of alerts sent	Administrative Data
Number of parent logins	Administrative Data
Number of student logins	Administrative Data
Ever suspended	Administrative Data
GPA	Administrative Data
Total number of missed assignments	Administrative Data
School-to-parent contact	Survey Q3
Parent-to-school contact	Survey Q6
Accuracy of grade beliefs	Survey Q9 & Admin Data
Accuracy of missed assignment beliefs	Survey Q16 & Admin Data
Parent talks to child about schoolwork	Survey Q12
Parent takes privileges from child over schoolwork	Survey Q14
Desire to continue intervention	Survey Q19

Table B.5: Secondary outcomes and their construction

Outcome	Construction
Number of alerts sent	Total alerts sent post intervention start
Number of parent logins	Total parent logins post intervention start
Number of student logins	Total student logins post intervention start
Ever suspended	Indicator for a suspension of any length occurring post intervention start
GPA	Average of 2nd semester grades using a 4-point scale imputing zeros for missing.
Total number of missed assignments	Total number of assignments missed in the 2nd semester
School-to-parent contact	Indicator for once per month or greater
Parent-to-school contact	Indicator for above median contact
Accuracy of grade beliefs	Survey Q9 minus grade from last report card and indicator for “I don’t know”
Accuracy of missed assignment beliefs	Survey Q16 minus number from 2nd semester data and indicator for “I don’t know”
Parent talks to child about schoolwork	Indicator for 2-3 times per week and above.
Parent takes privileges from child over schoolwork	Indicator for true or not
Desire to continue intervention	Indicator for true or not

Table B.6: Secondary outcomes and hypothesized effect

Outcome	Test
Number of alerts sent	ATE > 0
Number of parent logins	ATE != 0
Number of student logins	ATE != 0
Ever suspended	ATE < 0
GPA	ATE > 0
Total number of missed assignments	ATE < 0
School-to-parent contact	ATE != 0
Parent-to-school contact	ATE != 0
Accuracy of grade beliefs	ATE > 0
Accuracy of missed assignment beliefs	ATE > 0
Parent talks to child about schoolwork	ATE != 0
Parent takes privileges from child over schoolwork	ATE != 0
Desire to continue intervention	ATE > 0

# Appendix C

Figure C.1: Endline Survey Letter

ID: <ID>

**School-to-Parent Communication Survey**

**INSTRUCTIONS**

(1) Please read each question closely and mark your response with an "X" in the box that is appropriate for your answer.

(2) Please use a pen if possible.

(3) When you are asked to write down numbers, please do so as clearly as possible.

(4) You may choose not to answer any particular question.

**For example:**

Did your child attend school last year?       Yes       No

**If you make a mistake** in your answer, please fill in the entire box to mark your response.

Did your child attend school last year?       Yes       No

1

This is page 1 of 4 of the endline survey letter sent to participant parents after the end of the treatment school year.

Figure C.2: Endline Survey Letter

ID: «ID»

<b>This survey pertains <i>only</i> to «child_name»</b>	
<p><b>1. What is your relationship with «child_name»?</b> <i>Please mark one</i></p>	<p><input type="checkbox"/> Mother      <input type="checkbox"/> Father      <input type="checkbox"/> Grandparent      <input type="checkbox"/> Aunt or uncle      <input type="checkbox"/> Brother or sister      <input type="checkbox"/> Other guardian</p>
<p><b>2. Would you say that you are primarily in charge of monitoring «child_name»'s academic progress?</b> <i>Please mark one</i></p>	<p><input type="checkbox"/> Yes      <input type="checkbox"/> No</p>
<p><b>3. How often does the school contact you by letter, phone, text message or email about «child_name» grades or absences?</b> <i>Please mark one</i></p>	<p><input type="checkbox"/> About twice a month      <input type="checkbox"/> About once a month      <input type="checkbox"/> Once every two or three months      <input type="checkbox"/> Less than once every three months</p>
<p><b>4. Please tell us if you agree with the following: "The school makes it easy for me to help «child_name» get good grades in school."</b> <i>Please mark one</i></p>	<p><input type="checkbox"/> Agree      <input type="checkbox"/> Disagree</p>
<p><b>5. Is it difficult to be involved in «child_name»'s education because he/she does not tell you enough about his/her work or grades?</b> <i>Please mark one</i></p>	<p><input type="checkbox"/> Yes      <input type="checkbox"/> No</p>
<p><b>6. Since January 1<sup>st</sup>, how many times did you call, email, or visit the school to talk about «child_name»'s schoolwork or grades?</b> <i>Please mark one</i></p>	<p><input type="checkbox"/> None      <input type="checkbox"/> Once      <input type="checkbox"/> Twice      <input type="checkbox"/> Three times      <input type="checkbox"/> Four times      <input type="checkbox"/> Five times      <input type="checkbox"/> More than five times</p>
<p><b>7. Since January 1<sup>st</sup>, how many report cards have you received?</b> <i>Please mark one</i></p>	<p><input type="checkbox"/> None      <input type="checkbox"/> One      <input type="checkbox"/> Two      <input type="checkbox"/> Three      <input type="checkbox"/> Four      <input type="checkbox"/> Five or more</p>

This is page 2 of 4 of the endline survey letter sent to participant parents after the end of the treatment school year.

Figure C.3: Endline Survey Letter

ID: «ID»

<p><b>8. Since January 1<sup>st</sup>, how many report cards have you received?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> None    <input type="checkbox"/> One    <input type="checkbox"/> Two    <input type="checkbox"/> Three    <input type="checkbox"/> Four    <input type="checkbox"/> Five or more</p>
<p><b>9. Since January 1<sup>st</sup>, how many times is your school <i>supposed</i> to send you a report card?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> None    <input type="checkbox"/> One    <input type="checkbox"/> Two    <input type="checkbox"/> Three    <input type="checkbox"/> Four    <input type="checkbox"/> Five or more</p>
<p><b>10. On «child_name»'s last report card, what was his/her grade in math class?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> A    <input type="checkbox"/> B    <input type="checkbox"/> C    <input type="checkbox"/> D    <input type="checkbox"/> F    <input type="checkbox"/> I don't know</p>
<p><b>11. Compared to other students in «child_name»'s grade, would you say his/her grade in math is above or below average?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> Above average    <input type="checkbox"/> About average    <input type="checkbox"/> Below average</p>
<p><b>12. During the last month of school, approximately how many <u>days</u> of school has «child_name» missed due to excused or unexcused absences?</b> <i>Provide a number that is your best guess.</i></p> <p>_____</p>
<p><b>13. During the last month of school, how many times did you talk to «child_name» about their schoolwork or grades?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> Once or twice last month    <input type="checkbox"/> About once a week    <input type="checkbox"/> 2-3 times a week    <input type="checkbox"/> Every day</p>
<p><b>14. During the last month of school, how many times did you talk to <i>another adult</i> in the household about «child_name»'s schoolwork or grades?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> Never    <input type="checkbox"/> Once or twice last month    <input type="checkbox"/> About once a week    <input type="checkbox"/> 2-3 times a week or more</p>

Figure C.4: Endline Survey Letter

ID: «ID»

<p>15. <b>During the last month of school, did you or another adult in the household take away privileges from «child_name» for not doing all of his/her schoolwork?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> Yes      <input type="checkbox"/> No</p>
<p>16. <b>Are you primarily in charge of «child_name»'s discipline?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> Yes      <input type="checkbox"/> No</p>
<p>17. <b>How many assignments would you guess «child_name» missed this semester?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> None      <input type="checkbox"/> Between one and five      <input type="checkbox"/> Between six and ten      <input type="checkbox"/> More than ten</p>
<p>18. <b>Have you heard of the Promise Scholarship in West Virginia?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> Yes      <input type="checkbox"/> No</p>
<p>19. <b>Do you know what GPA is required to be eligible for the Promise Scholarship? If not, provide your best guess.</b> <i>Please mark one and complete the required field</i></p> <p><input type="checkbox"/> Yes      <input type="checkbox"/> No Required GPA: _____      Best guess of required GPA: _____</p>
<p>20. <b>If your school started or continued a texting service to inform you about «child_name»'s academic progress, would you use it?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> Yes      <input type="checkbox"/> No</p>
<p>21. <b>If you answered yes to the previous question, how many times <u>per month</u> would you like to receive such information?</b> <i>Please provide a number</i></p> <p>_____</p>
<p>22. <b>What is the highest level of education you completed?</b> <i>Please mark one</i></p> <p><input type="checkbox"/> Did not complete high school      <input type="checkbox"/> Completed high school      <input type="checkbox"/> Some college      <input type="checkbox"/> Completed college      <input type="checkbox"/> More than college</p>