Parent-Child Information Frictions and Human Capital Investment: Evidence from a Field Experiment*

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This paper studies information frictions between parents and their children, and how these affect human capital investments. I provide detailed, biweekly information to a random sample of parents about their child's missed assignments and grades and find parents have upwardly-biased beliefs about their child's effort. Providing additional information attenuates this bias and improves student achievement. Using data from the experiment, I then estimate a persuasion game between parents and their children that shows the treatment effect is due to a combination of more accurate beliefs and reduced monitoring costs. The experimental results and policy simulations from the model demonstrate that improving the quality of school reporting or providing frequent information to parents about their child's effort in school can produce gains in achievement at a low cost.

JEL Codes: I20, I21, I24.

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I Introduction

Most models of human capital development assume that parents have full control over investments in their children's skills. While this assumption is plausible at early stages of child development, it is likely less so as children get older and develop greater agency (Cunha and Heckman, 2007; Heckman and Mosso, 2014). If parent and child preferences over schooling diverge, this agency problem complicates the ability of parents to foster their child's skills. Specifically, parents need to motivate and track their child's progress in school but they cannot rely on their children to communicate the relevant information, as a child may have an incentive to manipulate it.

This paper uses a field experiment in combination with a structural modeling approach to understand potential information frictions between parents and their children and the extent to which such frictions can be resolved by providing information to parents about their children's academic progress. To measure the effects of providing additional information, I conducted an experiment at a school in a low-income area of Los Angeles. Parents or guardians were randomly selected to receive additional information about their child's academic progress. This information consisted of emails, text messages and phone calls listing students' missing assignments and grades several times a month over a six-month period. The information provided was detailed in nature: messages contained the class, assignment names, problems and page numbers of the missing work whenever possible. Course grades were sent to families every five to eight weeks. To quantify the effects of the treatment on student effort, achievement and parental behaviors, I gathered administrative data on assignment completion, work habits, cooperation, attendance and test scores. Parent and student surveys were also conducted immediately after the school year ended to provide additional data about each family's response.

The reduced-form results uncover several important information problems. First, I show that parents have upwardly-biased beliefs about their child's effort in school: when asked to estimate how many assignments their child has missed in math class, parents vastly understate. The size of this bias is negatively and significantly associated with student achievement and the information treatment attenuates this bias. Second, more information increases the intensity of parental monitoring and incentives, and increases inputs such as student effort. Parents in the treatment group contacted the school 83% more often than the control group and parent-teacher conference attendance increased by 53%. Third, schoolwork is consistently at the top of parents' mind: parents in both the treatment and control groups ask their child whether they have completed their schoolwork nearly five times per week, on average. However, parents rarely attend meetings with teachers to discuss their child's academics (15% attended parent-teacher conferences in the last semester) and neither parents nor schools reach out to each other often (median parent contact is 1.5 times per semester).

In terms of achievement, reducing these information problems can potentially produce gains on a par with education reforms such as the introduction of high-quality charter schools. GPA increased by .19 standard deviations. There is evidence that test scores for math increased by .21 standard deviations, though there was no gain for English scores (.04 standard deviations). These effects are driven by several changes in students' inputs: assignment completion increased by 25% and the likelihood of unsatisfactory work habits and cooperation decreased by 24% and 25%, respectively. Classes missed by students decreased by 28%. For comparison, the Harlem Children's Zone increased math scores and English scores by .23 and .05 standard deviations and KIPP Lynn charter schools increased these scores .35 and .12 standard deviations (Dobbie and Fryer, 2010; Angrist et al., 2010).

Based on these reduced-form results, I estimate a model of parent-child interactions as a game of strategic-information disclosure, or a persuasion game (Dye, 1985; Shin, 1994). This is a signaling game with incentives and parental monitoring combined with potentially biased parent beliefs about their child's effort. Children choose to exert effort in school or not and may offer parents verifiable reports regarding their effort, for instance via graded papers or report cards, or choose to hide this information. Parents have beliefs over their child's cost of effort and the probability of a verifiable report existing. The latter breaks the unraveling result (Grossman, 1981; Milgrom, 1981) in which parents simply assume the worst in the absence of information disclosure; this is particularly pertinent to low-achieving schools, where parents report school communication is often poor (Bridgeland et al., 2008). Parents may monitor effort, for example by going to the school to speak with teachers to obtain a verifiable report, and take away privileges if children are exerting inadequate effort with respect to parental preferences. I present identifying conditions that map observed actions into unique equilibria and I estimate the model using maximum likelihood.

The model serves two purposes. First, while the experimental variation alone cannot disentangle the channels through which the treatment influenced information problems and student effort, by estimating the model I can decompose the treatment effect into changes due to monitoring costs versus changes due to revised parental beliefs. Understanding these mechanisms has implications for when and how additional information will be effective. I find that a substantial portion of the treatment effect can be attributed to changes in parents' beliefs (42%) and reductions in monitoring costs (54%).

Second, I use the model to consider alternative policies that could be used to reduce information frictions. Rather than reducing monitoring costs, I consider a policy that improves school reporting. Simulating the effect of improving school reporting shows this policy can improve student effort as well, though by half the magnitude of providing additional information. Nonetheless, encouraging teachers to grade papers and enter them into gradebooks may be more scalable and less controversial than alternative policies to improve student achievement.

These costs are important because alternative policies aimed at improving the achievement of adolescents can be expensive. They often rely on financial incentives, either for teachers (Springer et al., 2010; Fryer, 2011), for students (Angrist and Lavy, 2002; Bettinger, 2008; Fryer, 2011) or for parents (Miller, Riccio and Smith, 2010).[1] Providing financial incentives

[1]Examples of other *information*-based interventions in education include providing families information describing student

for high school students cost $538 per .10 standard-deviation increase, excluding administrative costs (Fryer, 2011). If teachers were to provide additional information to parents as in this study, the cost per student per .10 standard-deviation increase in GPA or math scores would be $156 per child per year. Automation could reduce this to less than $10 per student.

An important question related to costs is how much parents might be willing to pay to reduce these information problems. My study does not address this question, but Bursztyn and Coffman (2012) use a lab experiment with low-income families in Brazil to show parents are willing to pay substantial amounts of money for information on their child's attendance.

While this paper shows that an intensive information-to-parents service can potentially produce gains to student effort and achievement, its policy relevance depends on how well it scales. Large school districts such as Los Angeles, Chicago, and Baltimore have purchased systems that make it easier for teachers to improve communication with parents by posting grades online, sending automated emails regarding grades, or text messaging parents regarding schoolwork. The availability of these services prompts questions about their usage, whether teachers update their grade books often enough to provide information, and parental demand for this information. This paper discusses but does not address these questions empirically.[2]

The rest of the paper proceeds as follows. Sections II and III describe the experimental design and the estimation strategy. Sections IV and V show reduced-form impacts on achievement and parent and child behaviors. Section VI presents the model, estimation procedure and results. Section VII concludes with a discussion of external validity and cost-effectiveness.

achievement at surrounding schools (Hastings and Weinstein, 2008; Andrabi, Das and Khwaja 2009), parent outreach programs (Avvisati et al., 2013), providing principals information on teacher effectiveness (Rockoff et al., 2010) and helping parents fill out financial aid forms (Bettinger et al., 2009).

[2]See Bergman (2016) on the low-parental usage of online information about student academic progress.

II Background and Experimental Design

A Background

I conducted the experiment at a K-12 school during the 2010-2011 school year. This school is part of Los Angeles Unified School District (LAUSD), which is the second largest district in the United States. The district has graduation rates similar to other large urban areas and is low performing according to its own proficiency standards: 55% of LAUSD students graduate high school within four years, 25% of students graduate with the minimum requirements to attend California's public colleges, 37% of students are proficient in English-Language Arts and 17% are proficient in math.[3]

The school is in a low-income area with a high percentage of minority students: 90% of students receive free or reduced-price lunch, 74% are Hispanic and 21% are Asian. Compared to the average district scores above, the school performs less well on math and English state exams; 8% and 27% scored proficient or better in math and English respectively. 68% of teachers at the school are highly qualified, defined as being fully accredited and demonstrating subject-area competence.[4] In LAUSD, the average high school is 73% Hispanic, 4% Asian and 89% of teachers are highly qualified.[5]

The school context has several features that are distinct from a typical LAUSD school. The school is located in a large building complex designed to house six schools and to serve 4,000 students living within a nine block radius. These schools are all new, and grades K-5 opened in 2009. The following year, grades six through eleven opened. Thus in the 2010-2011 school year, the sixth graders had attended the school in the previous year while students in grades seven and above spent their previous year at different schools. Families living within the nine-block radius were designated to attend one of the six new schools but

[3] This information and school-level report cards can be found online at http://getreportcard.lausd.net/reportcards/reports.jsp.

[4] Several papers have shown that observable teacher characteristics are uncorrelated with a teacher's effect on test scores (Aaronson et al., 2008; Jacob and Lefgren, 2008; Rivken et al., 2005). Buddin (2010) shows this result applies to LAUSD as well.

[5] This information is drawn from the district-level report card mentioned in the footnote above.

were allowed to rank their preferences for each. These schools are all pilot schools, which implies they have greater autonomy over their budget allocation, staffing, and curriculum than the typical district school.[6]

B Experimental Design

The sample frame consisted of all students in grades six through eleven enrolled at the school in December of 2010. The sample was stratified along indicators for being in high school, having had a least one D or F on their mid-semester grades, having a teacher think the service would helpful for that student, and having a valid phone number.[7] Students were not informed of their family's treatment status nor were they told that the treatment was being introduced. Teachers knew about the experiment but were not told which families received the additional information. Interviews with students suggest that several students discussed the messages with each other. Due to contamination in the middle school sample, I study the stratified sample of 306 students in high school.[8]

The focus of the information treatment was missing assignments, which included homework, classwork, projects, essays and missing exams. Each message contained the assignment name or exam date and the class it was for whenever possible. For some classes, this name included page and problem numbers; for other classes it was the title of a project, worksheet or science lab. Overwhelmingly, the information provided to parents was negative—nearly all about work students did not do. The treatment rule was such that a single missing assignment in one class was sufficient to warrant a message home. All but one teacher accepted late work for at least partial credit. Parents also received current-grades information three times and a notification about upcoming final exams.

[6] The smaller pilot school system in Los Angeles is similar to the system in Boston. Abdulkadiroglu et al. (2011) find that the effects of pilot schools on standardized test scores in Boston are generally small and not significantly different from traditional Boston public schools. For more information on LAUSD pilot schools, see http://publicschoolchoice.lausd.net/sites/default/files/Los%20Angeles%20Pilot%20Schools%20Agreement%20%28Signed%29.pdf.

[7] The validity of the phone number was determined by the school's automated-caller records.

[8] Middle school teachers had a school employee replicate the treatment for all students, treatment and control. This employee called parents regarding missing assignments and set up parent-teacher conferences in addition to the school-wide conferences. This contamination began four or five weeks after the treatment started and makes interpreting the results for the middle school sample difficult. See the appendix for all results from the middle school sample.

The information provided to parents came from teacher grade books gathered weekly from teachers. 14 teachers were asked to participate by sharing their grade books so that this information could be messaged to parents. The goal was to provide additional information to parents twice a month if students missed work. The primary constraint on provision was the frequency at which grade books were updated. Updated information about assignments could be gathered every two-to-four weeks from nine of the fourteen teachers. Therefore these nine teachers' courses were the source of information for the messages and the remaining teachers' courses could not be included in the treatment. These nine teachers were sufficient to have grade-book level information on every student.

The control group received the default amount of information the school provided. This included grade-related information from the school and from teachers. Following LAUSD policy, the school mailed home four report cards per semester. One of these reports was optional—teachers did not have to submit grades for the first report card of the semester. The report cards contained grades, a teacher's comment for each class, and each teacher's marks for cooperation and work habits. All school documents were translated into Spanish and Korean, and the school employed several Korean and Spanish translators. Parent-teacher conferences were held once per semester. Attendance for these conferences was very low for the high school (roughly 15% participation). Teachers could also provide information to parents directly. At baseline, most teachers had not contacted any parents. No teacher had posted grades on the Internet though two teachers had posted assignments.

Figure 1 shows the timeline of the experiment and data collection. Baseline data was collected in December of 2010. That same month, contact numbers were culled from emergency cards, administrative data and the phone records of the school's automated-calling system. In January 2011, parents in the treatment group were called to inform them that the school was piloting an information service provided by a volunteer from the school for half the parents at the school. Parents were asked if they would like to participate, and all parents consented, which implies no initial selection into treatment. These conversations included questions about language preference, preferred method of contact—phone call, text message or email—and parents' understanding of the A-F grading system. Most parents requested text messages (79%), followed by emails (13%) and phone calls (8%).[9]

The four mandatory grading periods after the treatment began are also shown, which includes first-semester grades. Before the last progress report in May, students took the California Standards Test (CST), which is a state-mandated test that all students are supposed to take.[10] Surveys of parents and students were conducted over the summer in July and August. Lastly, course grades from February 2012, more than one year after the experiment began and seven months after its conclusion, were also collected the following school year.

Notifications began in early January of 2011 and were sent to parents of middle school students and high school students on alternating weeks. This continued until the end of June, 2011. A bar graph above the timeline charts the frequency of contact with families over six months. The first gap in messages in mid February reflects the start of the new semester and another gap occurs in early April during spring vacation. This graph shows there was a high frequency of contact with families.

III Data and Empirical Strategy

A Baseline Data

Baseline data include administrative records on student grades, courses, attendance, race, free-lunch status, English-language skills, language spoken at home, parents' education levels and contact information. There are two measures of GPA at baseline. For 82% of students their cumulative GPA prior to entering the school is also available. The second measure of GPA is calculated from their mid-semester report card, which was two months before the treatment began. At the time of randomization only mid-semester GPA was available. Report cards contain class-level grades and teacher-reported marks on students' work habits

[9]A voicemail message containing the assignment-related information was left if no one picked up the phone.

[10]Students with special needs can be exempted from this exam.

and cooperation. As stated above, there is an optional second-semester report card, however the data in this paper uses mandatory report cards to avoid issues of selective reporting of grades by teachers.

Teachers were surveyed about their contact with parents and which students they thought the information treatment would be helpful for. The latter is coded into an indicator for at least one teacher saying the treatment would be helpful for that student.

B Achievement-Related Outcomes

Achievement-related outcomes are students' grades, standardized test scores and final exam or project scores from courses. Course grades and GPA are drawn from administrative data on report cards. There are four mandatory report cards available after the treatment began, but only end-of-semester GPA and grades remain on a student's transcript. There are two sets of end-of-semester grades: transcript grades obtained during the school year in which the experiment was conducted and transcript grades obtained seven months after the experiment concluded. Final exam and project grades come from teacher grade books and are standardized by class.

The standardized test scores are scores from the California Standards Tests. These tests are high-stakes exams for schools but are low stakes for students. The math exam is subdivided by topic: geometry, algebra I, algebra II and a separate comprehensive exam for students who have completed these courses. The English test is different for each grade. Test scores are standardized to be mean zero and standard deviation one for each different test within the sample.

C Effort-Related Measures

Measures of student effort are student work habits, cooperation, attendance and assignment completion. Work habits and cooperation have three ordered outcomes: excellent, satisfactory and unsatisfactory. There is a mark for cooperation and work habits for each class and each grading period, and students typically take seven to eight classes per semester. Assignment completion is coded from the teacher grade books. Missing assignments are coded into indicators for missing or not.

There are three attendance outcomes. Full-day attendance rate is how often a child attended the majority of the school day. Days absent is a class-level measure showing how many days a child missed a particular class. The class attendance rate measure divides this number by the total days enrolled in a class.

D Parental Investments and Family Responses to Information

Telephone surveys were conducted to examine parent and student responses to the intervention not captured by administrative data. For parents, the survey asked about their communication with the school, how they motivated their child to get good grades, and their perceptions of information problems with their child about schoolwork. Parent-teacher conference attendance was obtained from the school's parent sign-in sheets. The student survey asked about their time use after school, their communication with their parents and their valuations of schooling.[11]

The parent and student surveys were conducted after the experiment ended by telephone. 52% of middle-school students' families and 61% of high-school students' families responded to the telephone survey.[12] These response rates are analyzed in further detail below.

To reduce potential social-desirability bias—respondents' desire to answer questions as they believe surveyors would prefer—the person who sent messages regarding missing assignments and grades did not conduct any surveys. No explicit mention about the information service was made until the very end of the survey.

All variables, their observation counts, and their source is summarized in appendix Table A.1.

[11] Students were also asked to gauge how important graduating college and high school is to their parents, but there was very little variation in the responses across students so these questions are omitted from the analysis.

[12] The school issued a paper-based survey to parents at the start of the year and the response rate was under 15%. An employee of LAUSD stated that the response rates for their paper-based surveys is 30%.

E Attrition, Non Response, Missing CST Scores

Of the 306 students in the sample, 279 remained throughout the school year. 8% of attriters were in the treatment group and 6% were in the control group.[13] The most frequent cause of attrition is transferring to a different school or moving away. Students who left the school are lower performing than the average student. The former have significantly lower baseline GPA and attendance as well as poorer work habits and cooperation. Table A.2 shows these correlates in further detail. Attrition is more substantial for the longer-run followup, seven months after the conclusion of the intervention: 14% of students leave the sample between the end of the school year (June, 2011) to the end of the semester followup (February, 2012). This is not correlated with treatment status, however (Table A.3).

Just over one third of parents did not respond to the survey.[14] Table A.4 shows nonresponse correlates. Nonresponse is uncorrelated with treatment status for both children and parents. However, if those who did not respond differ from the typical family, then results based on the surveys may not be representative of the school population. This is true, as a regression of an indicator for non response on baseline characteristics shows the latter are jointly significant (results not shown). Nonetheless, the majority of families responded and provide insight into how they responded to the additional information.

Lastly, many students did not take the California Standards Test. 8% of scores are missing for math and 7% of scores are missing for English. These tests were taken on different days. Table A.5 in the appendix shows the correlates of missing scores. Baseline controls are added for each of the first three columns with an indicator for missing math scores as the dependent variable. The remaining three columns perform the same exercise for missing English scores. The treatment is negatively and significantly associated with missing scores. The potential bias caused by these missing scores is discussed further in the results section on test scores.

[13] This degree of attrition is not unusual in this area. Another study at a Los Angeles area school documented 7% attrition from the start to the end of the school year, and substantially more attrition from spring to fall (Dudovitz et al., 2013).

[14] For comparison, LAUSD has said their non-response rate for parent surveys is roughly double this number.

F Descriptive Statistics

In practice, the median treatment-group family was contacted 10 times over six months. Mostly mothers were contacted (62%), followed by fathers (24%) and other guardians or family members (14%). 60% of parents asked to be contacted in Spanish, 32% said English was acceptable, and 8% wanted Korean translation.

Table 2 presents baseline-summary statistics across the treatment and the control groups. Panel A contains these statistics for the original sample while Panel B excludes attriters to show the balance of the sample used for estimations. Measures of works habits and cooperation are coded into indicators for unsatisfactory or not and excellent or not. Of the 13 measures, one difference—the fraction of female students—is significantly different (p-value of .078) between the treatment and control group in Panel A. All results are robust to adding gender as a control. Work habits and students' cumulative GPA from their prior grades are better (but not significantly) for the control group than the treatment group. Panel B shows that baseline GPA is .06 points higher for the control group than the treatment group in the sample used for analysis, and as shown below, results are sensitive to this control. One concern with this baseline difference is mean reversion, however students' prior GPA, which is a cumulative measure of their GPA over several years, also shows the treatment group is lower achieving than the control group. In addition, GPA for the control group is highly persistent from the end of the first semester to the end of the second semester. A regression of the latter on the former yields a coefficient near one.[15]

G Empirical Strategy

The reduced-form analyses estimate intent-to-treat effects. Families in the treatment group may have received fewer or no notifications because their child has special needs (13 families); the guidance counselor requested them removed from the list due to family instability

[15]Mean reversion does occur between students' prior GPA and their baseline GPA, however this reversion does not differ by treatment status (results available on request).

(two families); or the family speaks a language other than Spanish, English or Korean (two families). All of these families are included in the treatment group.

To measure the effect of additional information on various outcomes, I estimate the following

$$y_i = \alpha + \beta * Treatment_i + X_i' \gamma + \varepsilon_i \tag{1}$$

Control variables in X include baseline GPA and cumulative GPA from each student's prior school, grade indicators and strata indicators. The results are robust to various specifications so long as a baseline measure of GPA is controlled for, which most likely makes a difference due to the .06 point difference at baseline.

I estimate equation 1 with GPA as a dependent variable. To discern whether there were any differential effects by subject or for "targeted" classes—those classes for which a teacher shared a grade book in a timely fashion—I also use class grades as a dependent variable.[16] This regression uses the same controls as 1 above but the standard errors are clustered at the student level.[17] End-of-semester grades are coded on a four-point scale to match GPA calculations.[18]

Similar to class grades, there is a work habit mark and a cooperation mark for each student's class as well. I estimate the effect of additional information on these marks using an ordered-Probit model that pools together observations across grading periods and clusters standard errors at the student level. The controls are the same as above with additional grading-period fixed effects. I report marginal effects at the means, but the average of the marginal effects yields similar results.

Effects on full-day attendance and attendance at the classroom level use the same specification and controls as the specifications for GPA and class grades, respectively.

[16] Recall that only nine of the 14 teachers updated their grade books often enough so that assignment-related information could be provided to parents. The class-grades regression estimates whether treated students in those nine teachers' classes showed greater gains than the classes of teachers who did not update grades often enough to participate.

[17] Clustering at the teacher level or two-way clustering by teacher and student yield marginally *smaller* standard errors.

[18] A is coded as 4, B as 3, C as 2, D as 1 and F as 0.

The number of observations changes depending on the particular outcome being studied. For instance, students take more than one course each term, so when examining heterogeneity across course subject there is more than one observation per child. This is also true for class exams, in which the same student may take exams in multiple classes, and is also true for class attendance and class-level behaviors. When applicable, tables will show both the number of observations and the number of students used in the analysis.

IV Results

The Effect of the Treatment on School-to-Parent Contact

Table 3 assesses the effect of the treatment on survey measures of school-to-parent contact. Parents were asked how often the school contacted them during the last month of school regarding their child's grades or schoolwork. During this time all parents had been sent a progress report about their child's grades. The first column shows how much more often the treatment group was contacted by the school than the control group, controlling for baseline GPA and cumulative GPA from students' prior schools.[19] The treatment increased contact from the school regarding their child's grades and schoolwork by 187% relative to the control group. The dependent variable in the second column measures the fraction of people that were contacted by the school more than once. This fraction increases by 158% relative to the control group. The treatment had large effects on both the extensive margin of contact and the intensive margin of contact from the school regarding student grades.

Effects on Achievement Measures

A GPA

Figure 3 tracks average GPA in the treatment and control groups over time. The red vertical line indicates when the treatment began, which is about one month before the first semester

[19] The results without controls are extremely similar.

ended in mid February. There is a steady decrease in GPA for the control group after the first semester ends in February followed by a spike upward during the final grading period. The treatment group does not experience this decline and still improves in the final grading period. Teachers reported that students work less in the beginning and middle of the semester and "cram" during the last grading period to bring up their GPA, which may negatively affect learning (Donovan, Figlio and Rush, 2006).

The regressions in Table 4 reinforce the conclusions drawn from the graphs described above. Column (1) shows the effect on GPA with no controls. The increase is .15 and is not significant, however the treatment group had a .06 point lower GPA at baseline. Adding a control for baseline GPA raises the effect to .20 points and is significant at the 5% level (column (2)). The standard errors decrease by 35%. The third column adds controls for GPA from students' prior schools and grade level indicators. The treatment effect increases slightly to .23 points. The latter converts to a .19 standard deviation increase in GPA over the control group.

The results in Table 5 are estimates of the treatment effect on class grades. Column (1) shows this effect is nearly identical to the effect on final GPA.[20] Column (2) shows the effect on targeted classes—those classes for which a teacher was asked to participate and that teacher provided a grade book so that messages could be sent home regarding missing work. This analysis is underpowered, but the interaction term is positive and not significant (p-value equals .16). Columns (3) and (4) show that math classes had greater gains than English classes (p-values equal .11 and .85, respectively). This effect disparity coincides with the difference in effects shown later for standardized tests scores.

Even though grading standards are school specific, the impact on GPA is important. In the short run, course grades in required classes determine high school graduation and higher education eligibility. In the longer run, several studies find that high school GPA is the best predictor of college performance and attainment (for instance Geiser and Santelices,

[20]The similarity in effects between this unweighted regression on individual grades and the regression on GPA is because there is small variation in the number of classes students take.

2007). GPA is also significantly correlated with earnings even after controlling for test scores (Rosenbaum, 1998; French et al., 2010). [21]

B Final Exams, Projects and Standardized Test scores

Additional information causes exam and final project scores to improve by .16 standard deviations (significant at the 5% level, Table 6). However, teachers enter missing finals as zeros into the grade book. On average, 18% percent of final exams and projects were not submitted by the control group. The effect on the fraction of students turning in their final exam or project is large and significant. Additional information reduces this fraction missing by 42%, or 7.5 percentage points.

Ideally, state-mandated tests are administered to all students, which would help separate out the treatment effect on participation from the effect on their score. Unfortunately, many students did not take these tests, and as shown above, missing a score is correlated with treatment status and treatment-control imbalance.[22] To account for this potential bias, the effects on math and English test scores are shown with a varying number of controls. The first and fourth columns in Table 7 control only for baseline GPA. The effect on math and English scores are .08 and -.04 standard deviations respectively. Columns (2) and (5) add controls for prior test scores, demographic characteristics and test subject. The treatment effect on math scores is .21 standard deviations, but remains near zero for English scores. Finally, if the treatment induces lower performing students to take the test, then those with higher baseline GPA might be less affected by this selection. This means we might see a positive coefficient on the interaction term between baseline GPA and the treatment. Columns (3) and (6) add this interaction term. While the interaction term is small for English scores, for math scores it implies that someone with the average GPA of 2.01 has a .20 standard

[21] One caveat, however, is that the mechanisms that generate these correlations may differ from the mechanisms underlying the impact of additional information on GPA.

[22] See the Appendix section on sample selection and test scores for further analysis of missing test takers; Table A.9 suggests that this attrition downwardly biases results.

deviations higher math score due to the additional information provided to their parents.[23]

This disparity between math and English gains is not uncommon. Bettinger (2010) finds financial incentives increase math scores but not English scores and discusses several previous studies (Reardon, Cheadle, and Robinson, 2008; Rouse, 1998) on educational interventions that exhibit this difference as well. There are three apparent reasons the information intervention may have had a stronger effect on math than English. First, the math teachers in this sample provided more frequent information on assignments that allowed more messages to be sent to parents. Potentially, this frequency might mean students fall less behind.[24] Second, 30% of students are classified as "limited-English proficient," which means they are English-language learners and need to pass a proficiency test three years in a row to be reclassified. Looking at class grades, these students tend to actually perform *better* in English classes, though interacting the treatment with indicators for language proficiency and English classes yields a large and negative coefficient (results not shown). In contrast, this coefficient is negative but 75% smaller when the interaction term includes an indicator for math classes rather than English classes. This means that the treatment effect for students with limited English skills is associated with smaller gains for English than math, which may in part drive the disparity in effects. Lastly, math assignments might provide better preparation for the standardized tests compared to English assignments if they more closely approximate the problems on the test.

V Parent and Child Behaviors

Impacts on Parental Beliefs

To study the impact of the additional information on parents' beliefs about their child's effort, I discretize parents' estimate of their child's missing assignments into the four categories in

[23]This marginal effect at the mean is significant at the 5% level.

[24]This theory is difficult to test since there is no within-class variation in grade-book upkeep or message frequency conditional on missing an assignment.

Table 8 and I estimate a multinomial-Logit model. Panel A show that the treatment causes parents to revise their beliefs. There is a large, statistically significant reduction in the probability of responding “I don’t know,” and a large, statistically significant increase in the probability responding their child has missed 3-5 assignments. There are no changes in the other categories. Second, the treatment impacts the size of the bias. Panel B shows results from a linear model for effects on the continuous “Difference from Truth” variable. The treatment significantly reduces the magnitude of the bias. This revision is particularly stark for parents of students with low baseline GPA, as indicated in the second column of Table 8.

This result is corroborated by results in Panel B in the third column, which shows that parents lacked awareness of the information problems with their child regarding school work. This column reports the answers to the question: does your child not tell you enough about his or her school work or grades? Parents in the treatment group are almost twice as likely to say yes as parents in the control group.

A How Parents Used the Additional Information

A change in parental beliefs and a reduction in monitoring costs may cause to parents to increase the use of monitoring and incentives. Panel A of Table 9 reflects impacts on measures of parental monitoring. Over the last semester, parents in the treatment group were 85% more likely to contact the school regarding their child’s schoolwork or grades, and this is corroborated by the school’s data on parent-teacher conference attendance, which increased by 53%. Parents regularly consider their child’s schoolwork; parents in the control group ask their children about completing their work nearly five times per week. There is little impact on this measure: the last column shows the coefficient on the treatment is negative and insignificant.

Panel B of Table 9 shows how parents interacted with their children. In terms of incentives, parents were asked how many privileges they took away from their child in the last month of school, which increased by nearly 100% for the treatment group (Column (1)). The most common privilege revoked by parents involved electronic devices—cell phones, television, Internet use and video games—followed by seeing friends.[25] There is some evidence parents also spoke about college more often to their child, though this is only significant at the 10% level.

Parents provide little direct assistance to their children overall. Children were asked how often they received help with their homework from their parents on a three point scale ("never," "sometimes," or "always," coded from zero to two). Overwhelmingly parents never help their children directly and the treatment has no significant effect on this behavior. Finally, the last column reports answers to whether parents agree they can help their child do their best at school. Parents in the treatment group are 16 percentage points more likely to say yes.

B Student Behaviors

The effects on work habits and cooperation are consistent with the effects on GPA. Table 10 provides the ordered-Probit estimates for work habits and cooperation (Panel A). Additional information reduces the probability of unsatisfactory work habits by 24%, or a six-percentage point reduction from the overall probability at the mean. This result mirrors the effect on excellent work habits, which increases by seven-percentage points at the mean. The probability of unsatisfactory cooperation is reduced by 25% and the probability of excellent cooperation improves by 13%.[26]

Panel B shows OLS estimates of the effects on attendance. The effect on full-day attendance is positive though not significant, however full-day attendance rates are already above 90% and students are more likely to skip a class than a full day. Analysis at the class level shows positive and significant effects. The treatment reduces classes missed by 28%. The

[25] An open-ended question also asked students how their parents respond when they receive good grades. 41% said their parents take them out or buy them something, 50% said their parents are happy, proud or congratulate them, and 9% said their parents do not do anything.

[26] Appendix Figures A.1, A.2, A.4 and A.3 show that work habits improve steadily for the treatment group over time.

final column of Panel B contains the estimated probability of missing an assignment. The average student does not turn in 20% of assignments. Assignments include all work, classwork and homework, and the grade books do not provide enough detail to discern one from the other.[27] At the mean, the treatment decreases the probability of missing an assignment by 25%.

These behavioral effects show that increased student productivity during school hours is an important mechanism underlying the effects of additional information. Assignments may be started in class but might have to be completed at home if they are not finished during class (e.g. a lab report for biology or chemistry, or worksheets and writing assignments in history and English classes). If students do not complete this work in class due to poor attendance or a slow work pace, they may not do it at home. The information treatment discourages poor attendance and low in-class productivity, which in turn may increase learning. The following section discusses what parent behaviors could have caused this result.

C Longer-Run Effects, Multiple Testing Adjustments and Alternative Explanations

The fact that the randomization was at the student level, within classrooms, raises two potential concerns. The first concern is teachers could have artificially raised treatment-group student grades to reduce any hassle from parental contact. If this were the case, the grade improvements observed in the treatment group would not be due to the effect of additional information on student effort and would not correspond to any actual improvement in student performance. A second concern is that teachers paid more attention to treatment-group students at the expense of attention for control-group students. This reallocation of attention to treatment-group students could reduce achievement for control students and bias effects away from zero. Lastly, I test many outcomes, which raises the issue that some results are spurious. While these are potential issues, several results undermine support for

[27] Several teachers said that classwork is much more likely to be completed than work assigned to be done at home.

these interpretations.

The most definitive results contradicting these interpretations are the significant effects on measures of student effort and parental investments that are less likely to be manipulated by teachers. The treatment group has higher attendance rates, higher levels of student-reported effort such as tutoring attendance and timely work completion, and a greater fraction of completed assignments. These results suggest that students indeed exerted more effort as a result of the information treatment. Consistent with this interpretation, the survey evidence from parents suggests that treatment-group parents took steps to motivate their children in response to the additional information beyond those of control-group parents, such as more intensive use of incentives.

It is also plausible that teachers who did not participate in the information intervention were less aware of who is in the treatment group versus the control group and therefore less likely to change their behavior as a result of the experiment. Consistent with the results above, the class-level analysis shows that students' grades also improved in the classes of these non-participating teachers (Table 5, column two).

Third, an analysis of treatment effects on control group students provides some evidence on whether the control group's grades increased or decreased as a result of the treatment. Though the experiment was not designed to examine peer effects, random assignment at the student level generates classroom-level variation in the fraction of students treated. Table A.7 shows the results from a regression of control students' class grades on the fraction of students treated in each respective class. While not statistically significant (p-value equals .27), the point estimate implies a positive impact of the fraction of students treated in a given classroom on the control students (a 25 percentage point increase in the fraction of students treated in a given classroom causes control group students' grades to increase by .14 points). This suggests that the gains observed among treatment students did not come at the expense of control students. To the contrary, there may have been positive spillovers onto control-group students' achievement that bias the effects of additional information toward zero.

There is also a question of whether impacts on grades persist after the conclusion of the treatment. A reminder or recency effect is less likely to be present at this stage, and a change in parental beliefs could lead to longer-run effects on outcomes. Despite balanced attrition from the school over summer months and the following semester (see Table A.3 for an analysis), Table 11 shows effects do persist. Column (1) shows the GPA effect is .15 and effect on grades is .17, which are 66% and 75% as large as the original treatment effects, respectively. However, while these results are longer-run, a limitation of the study is that it remains unclear whether the impacts would persist further or how they would develop over time if the intervention continued over several years.

Lastly, there are a large number of outcomes studied in this paper, which raises the potential for spurious findings do to multiple testing issues (cf. Romano et al., 2010). I use a step-down method developed by Romano and Wolf (2005) and further described in Heckman et al. (2010), which is less conservative than other techniques (e.g. Bonferroni (Dunn, 1961) or Holm (1979) procedures) by using a bootstrap-procedure to account for dependence across outcomes while maintaining strong control of the Family-Wise Error rate. Table A.8 shows that while some effects do lose significance at conventional levels, all the main results hold on academic outcomes and work habits hold.

VI Mechanisms: Structural Model

The reduced form results indicate that it is difficult for parents to monitor their children because school communication to parents is poor, and that families overestimate their child's effort in school. While the experiment identifies these causal effects, it cannot separately identify how each relates to changes in student effort. I incorporate these potential mechanisms into a model of how parents, children and schools interact. I model this as game of strategic-information disclosure, or a persuasion game (Dye, 1985; Shin, 1994). Children can signal their effort to their parents via verifiable reports, however this signal is imperfect because a report is not always available. Parents can monitor and incentivize their child's effort. The model relates to models of the parent-child dynamic as a principal-agent problem (Akabayashi, 2006; Bursztyn and Coffman, 2012; Cosconati, 2009; Hao et al., 2008; and Weinberg, 2001), and the estimation procedure presented below relates to the two-player signaling game of Kim (2015).

Set up and Notation

The actions and parameters are as follows. Children choose to exert effort ($E = 1$) in school or shirk ($E = 0$). If available, children may also signal their effort by disclosing ($D = 1$) verifiable reports to parents regarding their effort—for instance graded papers or report cards—or choose to hide this information ($D = 0$). Disclosure is costless. Effort in school is costly to the child but is less costly to high (H) types than low (L) types ($c_H < c_L$). Parents value their child's effort as V and assign zero value to shirking. Parents have beliefs their child is a high type with probability π and whether or not a verifiable report exists with probability R. The latter breaks the "unraveling result" (Grossman, 1981; Milgrom, 1981), in which a parent simply assumes the worst about their child's effort in the absence of information disclosure, and is particularly pertinent to low-achieving schools in which parents often report school communication is poor (Bridgeland et al., 2008). Parents may monitor ($M = 1$) effort at cost m, for example by going to the school to speak with teachers, and parents take away privileges w if children exert inadequate effort with respect to parental preferences. Parents may only take away privileges if the child discloses an unsatisfactory report or the parent obtains one via monitoring.

The timing is as follows. (1) Nature draws the child's type, high or low. (2) The child observes their own type and then chooses to exert effort in school or to shirk. (3) The school gives out a report or not. Reports are verifiable, and if drawn, document whether the child chose effort or shirked. (4) The child observes if a report exists and chooses whether to disclose this report to his or her parents or not. (5) Finally, the parent observes whether the child discloses a report and then decides whether to monitor (and obtain a verifiable report) or not to monitor.

The payoff for type t children is

$$U_{it} = w \cdot \mathbf{1}(E = 1 \vee M = 0) - c_t \cdot \mathbf{1}(E = 1) + \varepsilon_i(E)$$

and the parent's payoff is

$$U_p = V \cdot \mathbf{1}(E = 1) - w \cdot \mathbf{1}(E = 1 \vee M = 0) - m \cdot \mathbf{1}(M = 1) + \varepsilon_p(M)$$

where $\varepsilon_i, \varepsilon_p$ are i.i.d. standard normal variables that are unobservable to the econometrician but observable to both the parent and the child. The game tree with expected payoffs is shown in Figure 4.

To illustrate how strategic information disclosure and imperfect school-parent communication affects this game, consider parents' posterior beliefs if their child plays the strategy work and disclose if there exists a report conditional on being high type and shirk and do not disclose regardless of whether a report exists conditional on being low type. The following is parents' posterior belief their child is a high type conditional on the child not disclosing any information.

$$Pr(H|D = 0) = \frac{(1 - R)\pi}{1 - R\pi}$$

It is straightforward to see that as parents become more certain that the school has provided a report (the probability of R increases), parents become more certain their child is of low type if they did not disclose this information. If school reporting is infrequent or parents are unaware of school reports, parents can no longer discern a low type from a lack of school reporting. Moreover, inaccurate priors and high monitoring costs can impede parents' ability to motivate their child as well, as discussed below.

Identification

Many issues arise in estimation due to the fact that, for a given set of ε, there are multiple possible equilibria. Much of the literature on the estimation of static games deals with the issue of multiple equilibria (Bresnahan and Reiss 1990; Bresnahan and Reiss, 1991; Berry, 1992; Bajari, Hong, and Ryan, 2010). Kim (2015) uses equilibrium refinement of Cho and Kreps (1987) to eliminate multiple equilibria in the context of a two-player signaling game. I enumerate conditions below to refine the set of equilibria.

The game is characterized by seven structural objects and I impose the following identifying restrictions:

1. $c_L > 0$: cost of effort for L-types.
2. $c_H < c_L$: cost of effort for H-types.
3. $m > 0$: the monitoring cost for the parent.
4. $0 < R < 1$: the probability of a report being generated.[28]
5. $0 < \pi < 1$: the distribution of types.
6. $V > 0$: the utility to parents from their child's effort.
7. $w > 0$: the reward from parents to their child for their effort.

Under these conditions there is a mapping of preferences and parameters to unique, Perfect Bayesian Equilibria. Figure 5 summarizes this mapping. All Perfect Bayesian Equilibria—separating, pooling and hybrid—are derived in the appendix.

Estimation

With the uniqueness of the equilibria under the above conditions, I estimate the model using maximum likelihood. I construct the likelihood function as follows. First, I define the

[28]This restriction implies all beliefs are on the equilibrium path.

probability for each set of actions, $a = (M, E, D)$ and equilibrium. To derive these probabilities, I multiply the probability of an action given the parameters θ and the equilibrium that occurs, $P(a|Eq, \theta)$, by the probability that the equilibrium occurs, $P(Eq)$. For example, the probability of $a = (1, 1, 1)$ given the first separating equilibrium in the appendix, $S1$, is the probability that a high type is drawn and a report is generated. The probability that equilibrium $S1$ is drawn is $(\Phi(w - c_H) - \Phi(w - c_L))\Phi(-m)$ where $\Phi(\cdot)$ is the cumulative normal distribution function. Constructing the joint probabilities with hybrid equilibria is slightly more involved and evaluated for a given set of parameters using standard Monte Carlo integration. All derivations are detailed in the appendix.

The probability of observing each event a is the sum of the joint probabilities of a and equilibria eq.

$$P(a|\theta) = \sum_{eq \in Eq} P(a, eq|\theta)$$

To set up the likelihood function, let j be the observation of parent-child pairs with parent actions indexed by p and child actions index by i. The observed actions are: m_{pj}, e_{ij}, d_{ij} for monitoring, effort and disclosure, respectively, for observation j. The likelihood function is

$$\begin{aligned}
\log L((m_p, e_i, d_i)|\theta) = \frac{1}{n}\sum_{j=1}^{n}(&m_{pj}e_{ij}d_{ij}\log P((1,1,1)|\theta) + m_{pj}e_{ij}(1 - d_{ij})\log P((1,1,0)|\theta)\\
&+ m_{pj}(1 - e_{ij})(1 - d_{ij})\log P((1,0,0)|\theta)\\
&+ (1 - m_{pj})e_{ij}d_{ij}\log P((0,1,1)|\theta)\\
&+ (1 - m_{pj})e_{ij}(1 - d_{ij})\log P((0,1,0)|\theta)\\
&+ (1 - m_{pj})(1 - e_{ij})(1 - d_{ij})P((0,0,0)|\theta)
\end{aligned}$$

Given the payoff structure, the difference between V and w is not identified. I normalize the former to one. In addition to the parameter estimates, I also present the estimated distribution of equilibria in the results below. I estimate the model separately for the treatment and control groups.

Measures, Estimates, and Simulations

I use survey measures of punishment and disclosure described previously to generate indicators for whether the child was punished due to their schoolwork or grades and whether the child informs his or her parents about their academic progress, respectively. An indicator for effort is constructed in line with the report-card measure of effort studied previously; any unsatisfactory effort is assigned a zero and is a one otherwise. Despite summarizing these measures into unidimensional, binary variables, they follow the equilibrium predictions of the model well. For instance, all but one child who exerts effort also discloses to parents, and children who exert effort are rarely punished by parents (6 students).[29]

Note that not all families arrive at the same equilibrium. Heterogeneity stems from differences in parental beliefs between the treatment and control groups and the idiosyncratic component of families' utility functions. Table A.10 shows this point by presenting the distribution of equilibria for the treatment and control group, respectively. The proportions of certain separating equilibria, in particular, change across the two groups; the treatment shifts families toward separating and pooling equilibria with greater monitoring.

Table 12 presents the key parameter estimates, which show the intervention operates through a combination of effects. Compared to the treatment group, parents in the control group experience higher monitoring costs (higher M), are less likely to believe they can track their child's effort (lower R), and are significantly more likely to believe their child is a high type (higher π). As expected, these results are in line with the reduced-form findings, which shows revisions of parental beliefs and higher rates of contact with the school to discuss their child's academic progress. To understand whether the magnitudes of these estimates are reasonable, and how well the model fits the data, in the online appendix I show how well the simulated moments match their observed observed. The model matches observed

[29]These observations are dropped from the estimation.

measures of effort and disclosure well in both groups, though it slightly underestimates monitoring rates in the treatment group (Table A.11).

With these parameter estimates, I can also decompose the treatment effects on children's effort. The empirical treatment effect on the measure of effort is 4.2 percentage points, which is closely replicated by the model at 4.37 percentage points. The latter replication is not entirely surprising given that the model is estimated separately for the treatment and control group. However, I can vary certain parameters while holding others constant. Monitoring costs also explain a substantial portion of the effects, 54%. Equating the control group's beliefs to the treatment group's beliefs accounts for 42% of the treatment effect. The change in reporting accounts for only 3%. The substantial contribution to changes in beliefs could partly explain the persistence of the impacts into the following semester.

Moreover, these results suggest another simple policy tool schools can use to improve outcomes: improving school reporting. School districts vary in how often teachers must grade materials and enter them into a gradebook. While some parents face low monitoring costs and can find time to speak directly with teachers, low-income families are more likely single-parent households facing changing work schedules and transportation problems that make monitoring more difficult (Lott, 2001). Improved school reporting allows parents to more effectively target their monitoring. I simulate a policy that increases the likelihood of a report to 85% and, implicitly, makes this change salient to families. Increasing reporting in this way would cause an increase in effort by 2.15 percentage points for the control group, or roughly half the effect of providing additional information to families. While smaller in magnitude, this policy may be less controversial and easier to implement than output-focused policies, such as incentivizing teachers' value added.

VII Conclusion and Cost effectiveness

This paper uses an experiment to answer how information asymmetries between parents and their children affect human capital investment and achievement. The results show these problems can be significant and their effect on achievement large. Additional information to parents about their child's missing assignments and grades helps parents motivate their children more effectively and changes parents' beliefs about their child's effort in school. Parents also become more aware that their child does not tell them enough about their academic progress. These mechanisms drive an almost .20 standard deviation improvement in math standardized test scores and GPA. There is no estimated effect on middle-school family outcomes, however there was severe contamination in the middle school sample. One positive aspect of this contamination is that it reflects teachers' valuation of the intervention, which has helped scale this work further.

However it is important to consider how well these results extrapolate to other contexts. Identifying the mechanisms driving the treatment effects provides insight into the external validity of the findings. For instance, there is evidence that the information problems encountered in the context of this paper are more widespread. First, several papers document that parents in low-performing schools have upwardly biased beliefs about their child's academic performance both in the United States context (Bonilla et al., 2005; Kinsler and Pavan, 2016) and outside the United States (Dizon-Ross, 2016). Second, there is also evidence from the United States that the quality of school reporting is poor: in schools where the majority of students do not go on to college, only 43% of parents are satisfied with the communication they receive from their child's school about their schoolwork and grades (Bridgeland et al., 2008).

A limitation of this study is that the treatment lasted six months. The negative information about academic performance could create tension at home that might impact outcomes differently over the long run, which makes direct comparison to other, longer-run interventions difficult. However parents expressed a desire to continue the information service and treatment effects persisted into the following academic year, after the intervention concluded.

Importantly, this paper demonstrates several potentially cost-effective ways to bolster student achievement. For instance, contacting parents via text message, phone call or email took approximately three minutes per student. Gathering and maintaining contact numbers adds five minutes of time per child, on average. The time to generate a missing-work report can be almost instantaneous or take several minutes depending on the grade book used and the coordination across teachers.[30] For this experiment it was roughly five minutes. Teacher overtime pay varies across districts and teacher characteristics, but a reasonable estimate prices their time at $40 per hour. If teachers were paid to coordinate and provide information, the total cost per child per .10 standard-deviation increase in GPA or math scores would be $156.[31]

Automating aspects of this process could reduce this costs further, especially because many districts pay fixed costs for data systems. In response to this experiment, the school and a learning management company collaborated to develop a feature that automatically text messages parents about their child's missing assignments directly from teachers' grade books, which has been scaled to a large district. Relative to other effective interventions targeting adolescent achievement, the additional marginal cost is quite low.

[30] Some grade book programs can produce a missing assignment report for all of a student's classes.

[31] This cost-effectiveness analysis excludes the potentially significant time costs to parents and children.

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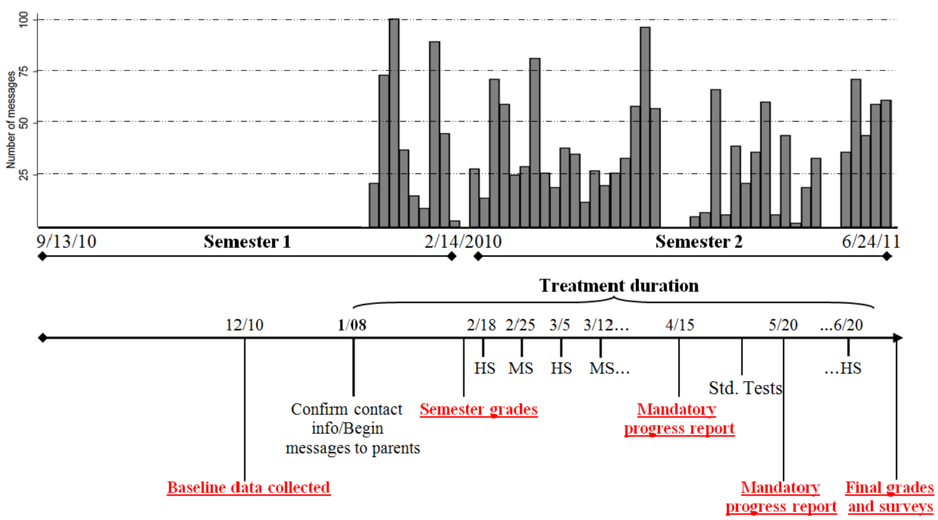
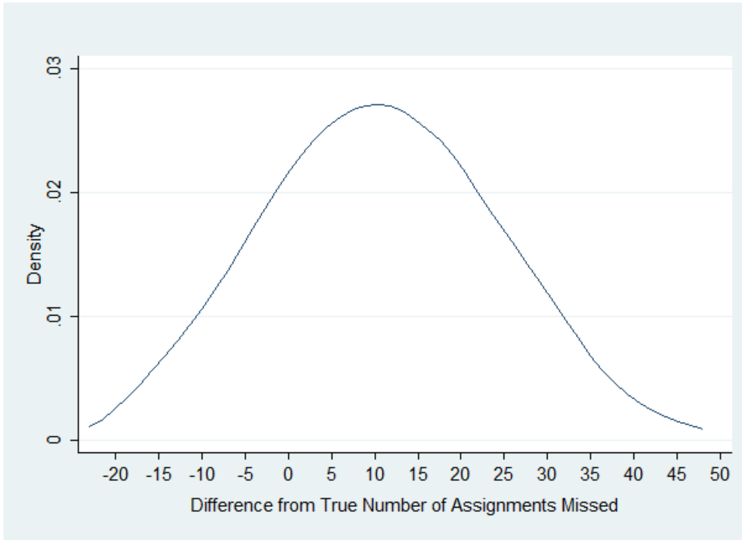
Figure 1: Timeline



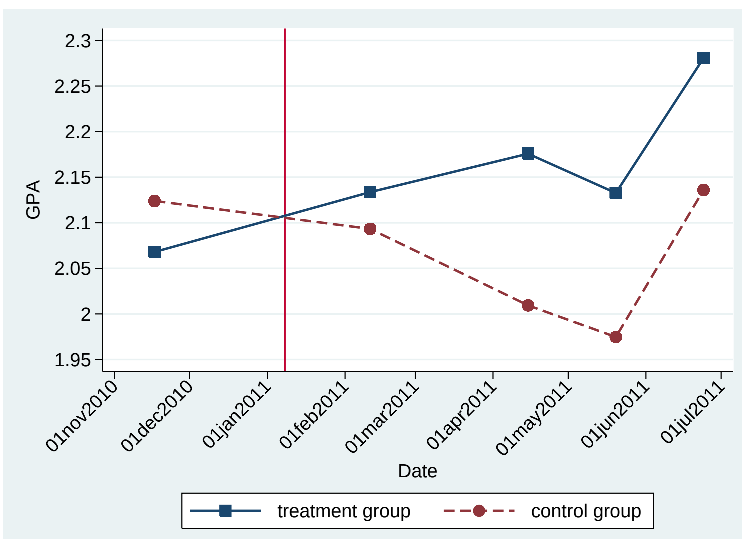
This figure shows the timeline of the experiment. Above the timeline is a chart of the frequency of messages sent to parents. Each bar signifies the number of messages sent over a three-day period and corresponds to the timeline dates below. The abbreviations HS and MS indicate that messages were sent to families of high school (HS) students families on alternate weeks with respect to middle school (MS) students families. "Std. tests" shows when the state-mandated standardized tests took place.

Figure 2: Parental Beliefs Relative to Truth



This figure shows a kernel-density plot of the true number of math assignments missed by a student minus the number of missed assignments estimated by parents via survey. Data come from surveys of parents and teacher grade books.

Figure 3: GPA over time for high school students



This graph plots the GPA of high school students in the treatment and control group over time. Each point represents the average GPA in a group calculated from progress report grades. The vertical red line indicates when the treatment began. To hold the composition of the sample constant over time, this plot excludes students who left the school prior to the end of the second semester.

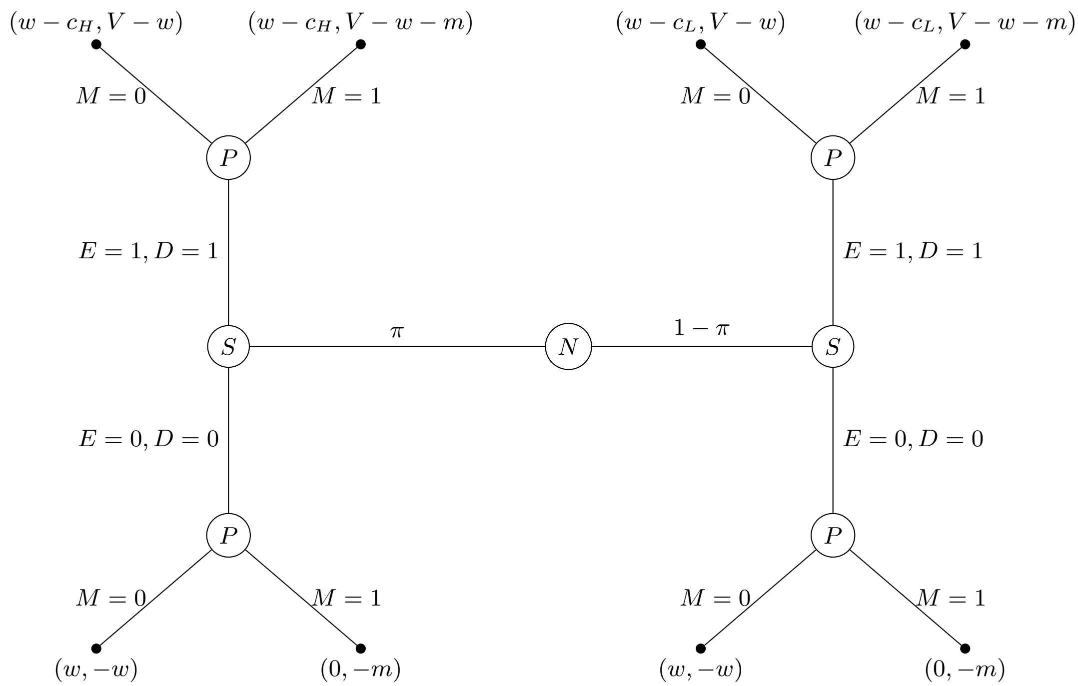


Figure 4: figure
Game Tree

This figure presents the persuasion game between parents and their children. Nature (N) selects the child's type with probability π. The student (S) then decides to exert effort (E) and disclose their progress (D) or to not exert effort and not disclose. Parents (P) then decide to monitor (M) their child or not and reward them (w) accordingly. The cost of effort is c_H and c_L for a high type and low type of student, respectively. Parents value their child's effort at V and pay a cost of monitoring of m.

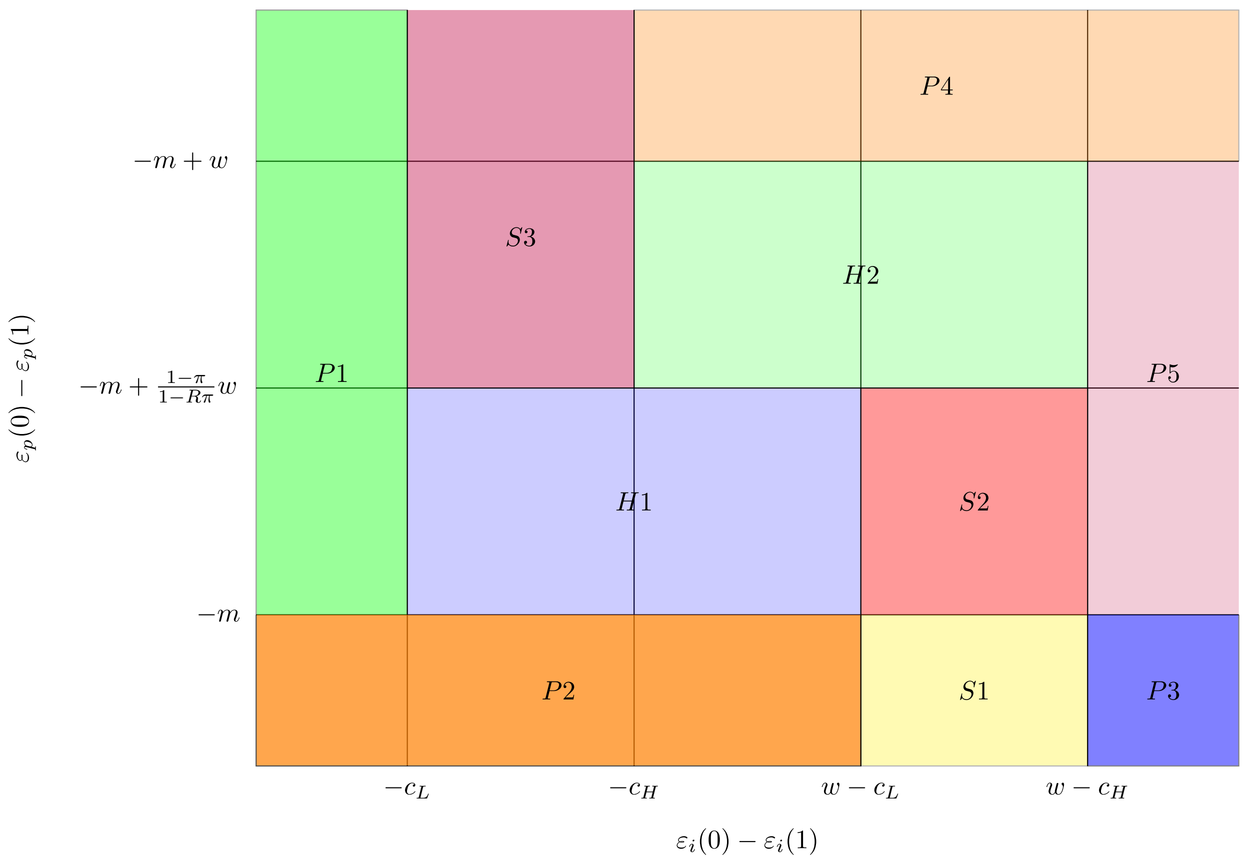


Figure 5: Equilibria of Game

This figure presents the mapping from the parameters of the model to the specific equilibria. S1-S3 index separating equlibria, P1-P5 index pooling equilibria, and H1 and H2 index hybrid equilibria. Further information about each equilibrium and its derivation is in the appendix.

Table 1: Correlations with Parental Beliefs of Student Effort

Dependent variable	(1) GPA	(2) GPA	(3) GPA	(4) GPA
Difference from Truth	-0.040***			
	(0.007)			
Contacted School		-0.031*		
		(0.016)		
Privileges			-0.091***	
			(0.019)	
Kid does not Disclose				-0.697***
				(0.143)
Observations	149	255	256	250
R-squared	0.366	0.013	0.122	0.086

This table shows associations between parent survey items and student performance. Column (1) shows the correlation of the true number of math assignments missed by a student minus the number of missed assignments estimated by parents with GPA. Sample size varies by response rate. Robust standard errors are in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2: Summary Statistics and Treatment-Control Group Balance

Panel A.	Sample balance including attriters					
	Control Mean	Treatment Mean	Difference	P-value	Students	Obs.
Female	0.363	0.463	0.099	0.078	306	306
Attendance	0.928	0.942	0.014	0.278	306	306
Baseline GPA	2.019	1.995	-0.024	0.848	298	298
Prior GPA	2.173	2.043	-0.130	0.282	252	252
Asian	0.24	0.219	-0.021	0.664	306	306
Black	0.021	0.031	0.011	0.559	306	206
Hispanic	0.699	0.725	0.026	0.612	306	306
Parent graduated HS	0.205	0.231	0.026	0.588	306	306
Free/Reduced Lunch	0.89	0.869	-0.022	0.563	306	306
Work habits unsatisfactory	0.326	0.308	-0.019	0.585	297	1953
Cooperation unsatisfactory	0.111	0.105	0.006	0.751	297	1952
Work habits excellent	0.354	0.310	-0.044	0.207	297	1953
Cooperation excellent	0.487	0.458	-0.029	0.385	297	1952

Panel B.	Sample balance excluding attriters					
	Control Mean	Treatment Mean	Difference	P-value	Students	Obs.
Female	0.382	0.462	0.079	0.182	279	279
Attendance	0.949	0.952	0.003	0.797	279	279
Baseline GPA	2.124	2.068	-0.056	0.658	272	279
Prior GPA	2.267	2.137	-0.130	0.289	228	228
Asian	0.243	0.238	-0.005	0.924	279	279
Black	0.022	0.028	0.006	0.753	279	279
Hispanic	0.691	0.706	0.015	0.784	279	279
Parent graduated HS	0.213	0.238	0.025	0.626	279	279
Free/Reduced lunch	0.904	0.888	-0.016	0.657	279	279
Work habits unsatisfactory	0.298	0.291	-0.007	0.846	279	1804
Work habits excellent	0.374	0.326	-0.048	0.188	279	1804
Cooperation unsatisfactory	0.089	0.093	0.003	0.855	279	1803
Cooperation excellent	0.511	0.482	-0.029	0.388	279	1803

Note: p-values are for tests of equality of means across the treatment and control group. Differences across work habits and cooperation are estimated by a regression of the behavior on treatment status with standard errors clustered by student. Baseline data are missing for students who enrolled in the school after the school year began. The "Students" column reports the number of student-level observations used in the analysis. The "Obs." column shows the total number of observations. The number of observations differs from the number of students for work habits and cooperation because a student receives these marks for each class he or she takes.

Table 3: Contact from the School Regarding Grades

Dependent variable	(1) School contact to parent	(2) Contacted more than once
Treatment	2.125***	0.453***
	(0.370)	(0.068)
Baseline GPA	0.100	-0.105**
	(0.411)	(0.052)
Prior GPA	-0.125	0.100*
	(0.341)	(0.051)
Control mean	1.134	0.286
Observations	183	183
R-squared	0.173	0.248

The dependent variable is drawn from surveys of parents. Parents were asked how many times they were contacted by the school regarding their child's grades or schoolwork during the last month of school. Column (1) uses the number of times contacted by the school as the dependent variable while column (2) uses an indicator for whether a parent was contacted more than one time. Baseline GPA is calculated from students' mid-semester progress reports from two months before the experiment began. Prior GPA is students' cumulative GPA from middle school and beyond. Strata and grade-level indicators are also included in each regression. Robust standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 4: GPA effect on High School Students

	(1)	(2)	(3)
Dependent variable	GPA	GPA	GPA
Treatment	0.145	0.203**	0.229**
	(0.143)	(0.093)	(0.090)
Baseline GPA		0.931***	0.760***
		(0.060)	(0.071)
Prior GPA			0.334***
			(0.072)
Grade 10			-0.248**
			(0.119)
Grade 11			-0.164
			(0.117)
Observations	279	279	279
R-squared	0.004	0.601	0.645

The dependent variable is students' end-of-semester GPA. Data used in these regressions are from administrative records. Baseline GPA is calculated from students' mid-semester progress reports from two months before the treatment began. Prior GPA is students' cumulative GPA from middle school and beyond. Strata indicators are also included in each regression. High school in this sample includes only grades nine through eleven because the school had just opened. Robust standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Effects on Grades

	(1)	(2)	(3)	(4)
Dependent variable	Class Grade	Class Grade	Class Grade	Class Grade
Treatment	0.231***	0.188**	0.208**	0.232**
	(0.088)	(0.095)	(0.089)	(0.090)
Treatment*Target		0.120		
		(0.086)		
Treatment*Math class			0.212	
			(0.132)	
Treatment*English class				0.022
				(0.119)
Students	279	279	279	279
Observations	2,224	2,224	2,224	2,224
R-squared	0.399	0.438	0.417	0.405

The dependent variable in these regressions is each students' class grade, which is coded into a four-point scale from their letter grades. Data used in these regressions are from administrative records. Each student typically takes eight classes. Grades marked incomplete are coded as missing. Additional controls in each regression are students' baseline GPA, prior GPA, grade-level indicators and strata indicators. Baseline GPA is calculated from students' mid-semester progress reports from two months before the treatment began. Prior GPA is students' cumulative GPA from middle school and beyond. Standard errors clustered by student are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Effects on Final Exams and Projects

	(1)	(2)	(3)	(4)
Dependent variable:	All Scores	Math Scores	English Scores	No Score
Treatment	0.160**	0.180*	0.329**	-0.075***
	(0.081)	(0.110)	(0.106)	(0.034)
Students	279	239	100	279
Observations	639	239	100	676
R-squared	0.347	0.430	0.465	0.184

All exam and final project scores are standardized by class to have a mean equal to zero and a standard deviation equal to one. Data in these regressions are from teacher grade books. Additional controls not shown are baseline GPA, prior GPA, grade-level indicators and strata indicators. Baseline GPA is calculated from students' mid-semester progress reports from two months before the experiment began. Prior GPA is students' cumulative GPA from middle school and beyond. The final column shows the effect of the treatment on not having score, excluding excused absences. If a student does not have a score it means they did not turn in any test or project. 18% of tests or final projects were not turned in by the control group. Standard errors clustered by student are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Effects on Standardized Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Math Score	Math Score	Math Score	English Score	English Score	English Score
Treatment	0.077	0.212**	-0.184	-0.039	0.008	0.036
	(0.107)	(0.102)	(0.236)	(0.226)	(0.091)	(0.209)
Baseline GPA	0.311***	0.245**	0.119	0.539***	0.339***	0.346***
	(0.076)	(0.091)	(0.104)	(0.083)	(0.078)	(0.101)
Prior GPA	0.292***	0.259***	0.198***	0.140*	0.075	0.018
	(0.084)	(0.077)	(0.081)	(0.078)	(0.070)	(0.071)
Treatment*baseline GPA			0.192**			-0.013
			(0.101)			(0.090)
Additional controls	No	Yes	Yes	No	Yes	Yes
Observations	256	256	256	257	257	257
R-squared	0.306	0.457	0.468	0.337	0.605	0.605

This table reports the effect of the treatment on the state-mandated test, the California Standards Test, for high school students. Scores are standardized by test subject to have a mean of zero and a standard deviation equal to one. The additional controls not shown above are prior test scores, race, sex, test subject, language spoken at home and free or reduced-price lunch status. Baseline GPA is GPA calculated from students' mid-semester progress reports two months before the treatment began. Prior GPA is students' cumulative GPA from middle school and beyond. Robust standard errors are in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8: Impact of Information on Parental Beliefs

Panel A.	Effect on Parental Beliefs			
Dependent variable	Pr(Don't Know)	Pr(missed 0-2)	Pr(missed 3-5)	Pr(missed 5+)
Treatment	-0.130**	-0.028	0.160**	-0.002
	(0.062)	(0.070)	(0.066)	(0.045)
Observations	172	172	172	172

Panel B.	Effect on Accuracy of Beliefs		
Dependent variable	Difference from Truth	Difference from Truth	Kid Doesn't Disclose
Treatment	-5.564*	-15.541**	0.195***
	(3.022)	(7.283)	(0.070)
Treatment*Baseline GPA		4.606*	
		(2.421)	
Control Mean	14.337		0.210
Observations	91	91	176
R-squared	0.485	0.512	0.160

This table shows the impact of the treatment on parental beliefs about the number of missed assignments their child has missed in math class. Results in Panel A show average marginal effects. Panel A includes parents who respond "I don't know" while Panel B excludes these respondents. Data comes from surveys of parents of high school students. Difference from truth outcome has fewer observations because this measure requires observed assignment information and a parent-survey response. Robust standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Parents' Responses to Additional Information

Panel A.	**Parental Monitoring**			
Dependent variable	Contacted School	Attended Conference	Ask HW	
Treatment	1.783***	0.079*	-2.533	
	(0.668)	(0.046)	(1.688)	
Control Mean	2.102	0.150	18.773	
Data source	Parent	School	Parent	
Observations	179	181	184	
R-squared	0.147	0.105	0.048	

Panel B.	**Parental Incentives and Assistance**			
Dependent variable	Privileges Taken	Talk College	Help Child	Can Help
Treatment	1.660**	2.611*	0.088	0.161**
	(0.718)	(1.415)	(0.069)	(0.072)
Control Mean	1.729	7.637	0.210	0.600
Data source	Parent	Parent	Child	Parent
Observations	180	183	183	181
R-squared	0.168	0.163	0.091	0.101

All columns show the effects of the information treatment on parents. Treatment effects are estimated using regressions that control for baseline GPA, prior GPA, strata indicators and grade-level indicators. Baseline GPA is GPA calculated from students' mid-semester progress reports two months before the experiment began. Prior GPA is students' cumulative GPA from middle school and beyond. Data source indicates whether the dependent variable came from a parent's survey response, a child's survey response, or school records. Robust standard errors are in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10: Effects on Behaviors

	In-Class Work Habits		In-Class Cooperation	
Dependent variable	Pr(Unsatisfactory)	Pr(Excellent)	Pr(Unsatisfactory)	Pr(Excellent)
Treatment	-0.061***	0.069***	-0.024**	0.056**
	(0.021)	(0.022)	(0.011)	(0.024)
Predicted probability	0.256	0.339	0.096	0.432
Students	279	279	279	279
Observations	8,795	8,795	8,795	8,795
		Attendance		**Assignment Completion**
Dependent Variable:	Full-day Rate	By-Class Rate	Classes Missed	Pr(Missed Asst.)
Treatment	1.675	2.879*	-1.401**	-0.049***
	(1.146)	(1.540)	(0.633)	(0.018)
Control mean	92.81	88.505	5.350	0.197
Students	278	278	278	279
Observations	278	2,252	2,252	27,297

The upper panel reports the effects of the treatment on the probability of unsatisfactory and excellent work habits or cooperation. These behaviors are measured as excellent, satisfactory and unsatisfactory. The coefficients reported are marginal effects at the means from ordered Probit models. Controls not shown are baseline GPA, cumulative GPA from prior schools, grading-period indicators, grade-level indicators and strata indicators. Baseline GPA is GPA calculated from students' mid-semester progress reports two months before the treatment began. Prior GPA is students' cumulative GPA from middle school and beyond. The number of observations differs from the number of students because each student receives a behavior mark for each class and for each of the four grading periods. In the lower panel, full-day attendance measures whether a student attended the majority of the school day, by-class measures attendance for each class, and classes missed measures how many classes a student did not attend over the semester, by course. Lastly, the probability of missing an assignment is reported as the marginal effect at the means from a Probit model. Standard errors clustered at the student level are in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 11: Effects on GPA Seven Months After Treatment

	(1)	(2)
Dependent variable	GPA	Grades
Treatment	0.151*	0.174**
	(0.089)	(0.087)
Observations	241	1,863
R-squared	0.542	0.243

This shows the correlation of the true number of math assignments missed by a student minus the number of missed assignments estimated by parents with GPA. Parents who respond "I don't know" are excluded from the sample, though the latter correlates negatively and significantly with GPA as well. Data comes from surveys of parents of high school students. Robust standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 12: Model Estimates

	(1)	(2)	(3)
Parameter	π	R	m
Treatment Group	0.242	0.444	0.260
	(0.012)	(0.025)	(0.056)
Control Group	0.413	0.355	0.471
	(0.017)	(0.030)	0.070)

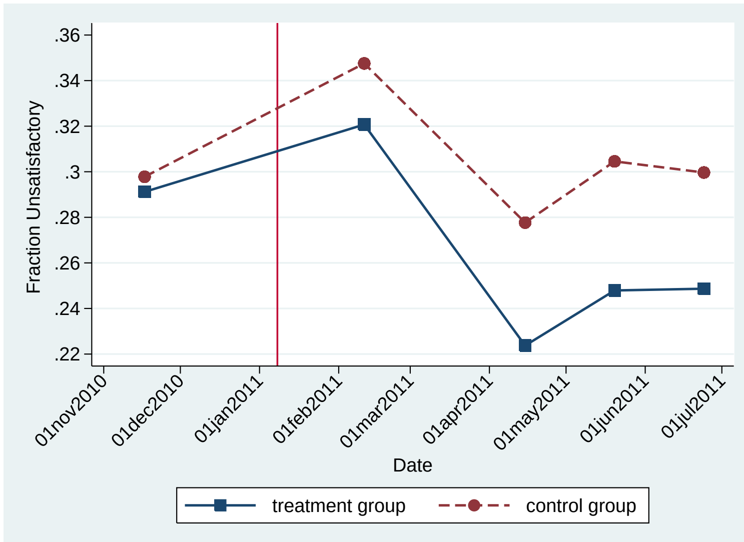
This table shows parameter estimates for parents' beliefs their child is a high type, π, the probability a report exists, R, and parents' monitoring costs, m. Boot-strapped standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Appendix

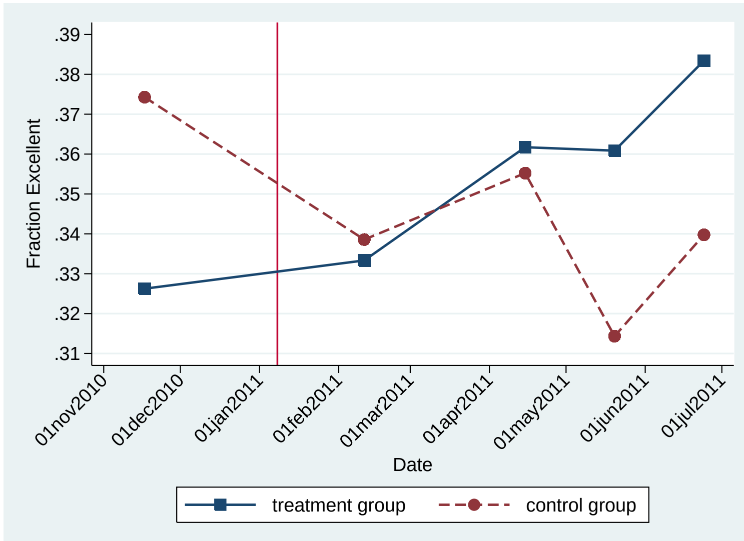
Figures

Figure A.1: Fraction of work habits marked unsatisfactory over time



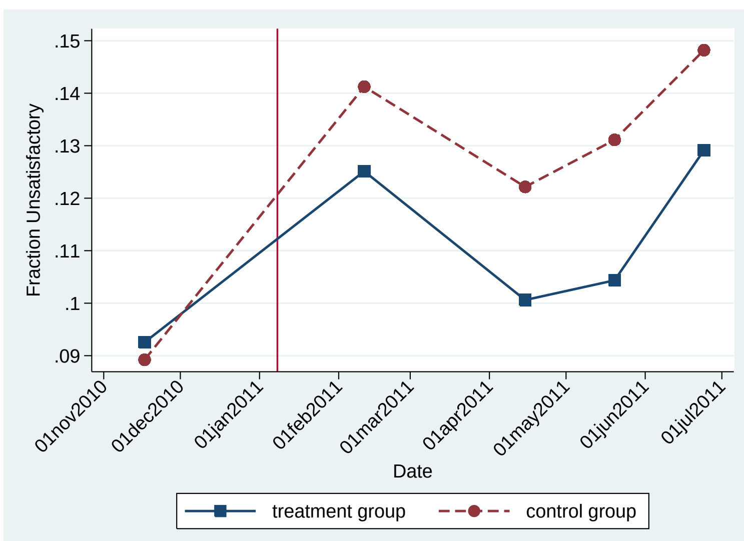
This graph plots the fraction of unsatisfactory work habit marks for the high school treatment and control groups over time. Work habits are graded as either excellent, satisfactory or unsatisfactory. Each point is calculated using progress report marks from each class. The vertical red line indicates when the treatment began. To hold the composition of the sample constant over time, this plot excludes students who left the school prior to the end of the second semester.

Figure A.2: Fraction of work habits marked excellent over time



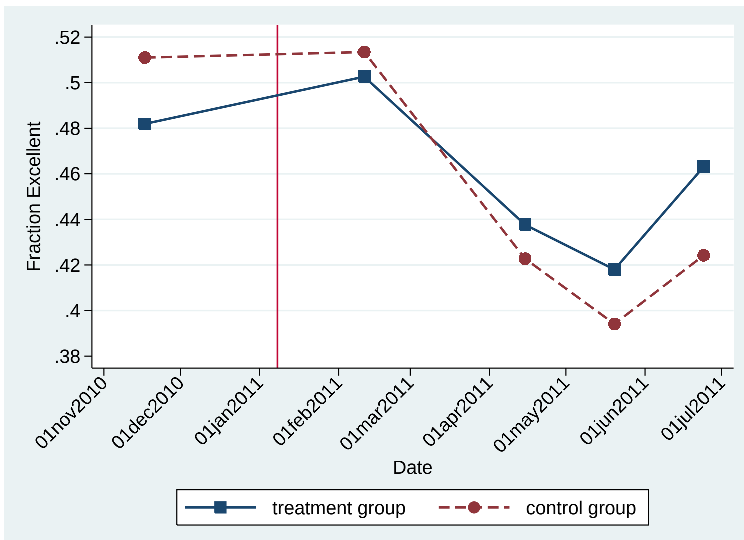
This graph plots the fraction of excellent work habit marks for the high school treatment and control groups over time. Work habits are graded as either excellent, satisfactory or unsatisfactory. Each point is calculated using progress report marks from each class. The vertical red line indicates when the treatment began. To hold the composition of the sample constant over time, this plot excludes students who left the school prior to the end of the second semester.

Figure A.3: Fraction of cooperation marks rated unsatisfactory over time



This graph plots the fraction of unsatisfactory cooperation marks for the high school treatment and control groups. Cooperation is graded as either excellent, satisfactory or unsatisfactory. Each point is calculated using progress report marks from each class. The vertical red line indicates when the treatment began. To hold the composition of the sample constant over time, this plot excludes students who left the school prior to the end of the second semester.

Figure A.4: Fraction of cooperation marks rated excellent over time



This graph plots the fraction of unsatisfactory cooperation marks for the high school treatment and control groups over time. Cooperation is graded as either excellent, unsatisfactory or excellent. Each point is calculated from progress report marks from each class. The vertical red line indicates when the treatment began. To hold the composition of the sample constant over time, this plot excludes students who left the school prior to the end of the second semester.

A Data Appendix

Table A.1: Outcomes, their sources, and observation numbers

Outcome	Source	Observation level	N
GPA	Transcripts	Student level	279
Class grades	Transcripts	Student-by-class level	2,224
Attendance rate	Administrative Data	Student level	278
Class attendance rate	Administrative Data	Student-by-class level	2,252
Works habits	Transcript data	Student-by-class-by-term level	8,795
Cooperation	Transcript data	Student-by-class-by-term level	8,795
School contact to parent	Parent Survey	Student level	183
Average final exam scores	Grade books	Student level	639
Math and English test scores	Administrative data	Student level	256
Missed assignments estimate	Parent survey	Student level	172
Child discloses information	Parent survey	Student level	176
Parent contacted school	Parent survey	Student level	179
Attended parent-teacher conference	Conference sign-in sheets	Student level	181
Ask about HW	Parent survey	Student level	184
Privileges taken	Parent survey	Student level	180
Talk about college	Parent survey	Student level	183
Helps child with HW	Parent survey	Student level	183
Can help child with HW	Parent survey	Student level	181
Missed assignments	Grade books	Student-by-assignment level	27,297
After-school tutoring frequency	Child survey	Student level	154
Last minute HW	Child survey	Student level	152
Study hours	Child survey	Student level	153
Grades important	Child survey	Student level	155
College important	Child survey	Student level	154

B Supplementary Empirical Analyses

Attrition

Table A.2: Attrition

	(1)	(2)	(3)
Dependent variable	Left the Sample		
Treatment	0.026	0.020	0.025
	(0.031)	(0.031)	(0.027)
Baseline GPA			-0.001
			(0.022)
Prior GPA			-0.047*
			(0.027)
Full-day attendance			-0.838***
			(0.255)
Female			-0.003
			(0.029)
Black			0.160
			(0.121)
Hispanic			0.067
			(0.044)
Asian			0.099**
			(0.045)
Free/Reduced Lunch			-0.026
			(0.040)
10th grade		0.089**	0.042
		(0.041)	(0.034)
11th grade		0.039	0.036
		(0.036)	(0.038)
Control mean	0.068		
Observations	306	306	306
R-squared	0.037	0.050	0.243

The dependent variable in these regressions is an indicator for having left the school. Columns (1)-(3) show the correlates of leaving for the high school. Baseline GPA is from mid-semester report cards two months before the treatment began and prior GPA is students' cumulative GPA from previous grades. Robust standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table A.3: Longer-Run Followup Attrition

	(1)	(2)	(3)
Dependent variable	Left the Sample		
Treatment	-0.022	-0.022	-0.027
	(0.042)	(0.042)	(0.040)
Baseline GPA			-0.049
			(0.039)
10th Grade		0.146***	0.110**
		(0.054)	(0.050)
11th Grade		0.066	0.075
		(0.051)	(0.047)
Full-day Attendance			-1.168***
			(0.283)
Female			0.024
			(0.043)
Black			-0.098
			(0.195)
Hispanic			-0.111
			(0.139)
Asian			-0.164
			(0.138)
Free/Reduced Lunch			-0.235**
			(0.094)
Control mean	0.155		
Observations	279	279	279
R-squared	0.015	0.044	0.217

The dependent variable in these regressions is an indicator for having left the school. Columns (1)-(3) show the correlates of leaving for the high school school after the conclusion of the treatment. Baseline GPA is from mid-semester report cards two months before the treatment began and prior GPA is students' cumulative GPA from previous grades. Robust standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table A.4: Survey Response Correlates

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable			Responded to Survey			
Treatment	0.043	0.033	0.036	0.026	0.013	0.015
	(0.056)	(0.055)	(0.050)	(0.057)	(0.056)	(0.053)
Baseline GPA		-0.067**	0.042		-0.062**	0.031
		(0.029)	(0.030)		(0.030)	(0.032)
9th grade		0.219***	0.224***		0.122*	0.129*
		(0.069)	(0.062)		(0.071)	(0.066)
10th grade		0.100	0.089		0.056	0.046
		(0.074)	(0.067)		(0.076)	(0.070)
Full-day attendance		0.765***	0.651***		0.914***	0.809***
		(0.265)	(0.241)		(0.274)	(0.255)
Female			-0.045			0.000
			(0.052)			(0.055)
Hispanic			0.305***			0.300***
			(0.108)			(0.114)
Asian			-0.225*			-0.175
			(0.115)			(0.121)
Free/Reduced lunch			0.075			0.108*
			(0.059)			(0.062)
Control Mean	0.582			0.493		
Sample	Parents	Parents	Parents	Children	Children	Children
Observations	306	306	306	306	306	306
R-squared	0.002	0.073	0.252	0.001	0.053	0.200

The dependent variable in these OLS regressions is an indicator for responding to the survey. Columns (1)-(3) show the correlates of response for the parent survey. Columns (4)-(6) show these correlates for the child survey. The control mean shows the percentage of control-group members who responded to the survey. These results are for families of high school students only. Robust standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table A.5: Missing CST Scores

Dependent variable	(1) Missing math	(2) Missing math	(3) Missing math	(4) Missing English	(5) Missing English	(6) Missing English
Treatment	-0.054	-0.060*	-0.065*	-0.047	-0.051	-0.056
	(0.033)	(0.033)	(0.035)	(0.032)	(0.032)	(0.035)
Baseline GPA		-0.050*	-0.041		-0.053*	-0.050
		(0.028)	(0.028)		(0.021)	(0.031)
Control mean	0.110			0.103		
Additional controls	No	No	Yes	No	No	Yes
Observations	279	279	279	279	279	279
R-squared	0 .010	0.010	0.122	0.008	0.090	0.173

The dependent variable in these OLS regressions is an indicator for having no test score. Additional controls include prior GPA, prior scores, test-subject indicators and demographic characteristics. Robust standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Student Response Outside of School

The first three columns of Table 10 (Panel A) show how students' work habits changed outside of school. Tutoring attendance over the semester increased 42%. The coefficient is marginally insignificant at standard levels (p-value equals .11). Tutoring was offered by teachers after school for free. The positive effect on tutoring is at least partially due to several teachers' requirement that missing work be made up during their after school tutoring to prevent cheating. The second column shows the effect on whether students did their homework at the last minute, which was coded from zero to two for "never," "sometimes" or "always." Students in the treatment group were significantly less likely to do their homework at the last minute. Nonetheless, student study hours at home did not significantly increase, which implies that most of the gains in achievement are due to improved work habits at school.

The remaining two columns of Panel A show students' valuations of schooling on a four-point scale. Students in the treatment group are more likely to say grades are important, but no more likely to say that college is important. While a generous interpretation may be that students now intrinsically value schooling more, another interpretation of these results is that grades are important because students will be punished if they do not do well, but their intrinsic valuation of schooling has not changed. I cannot distinguish between these interpretations however, but learning may develop regardless. Children's myopia or discount rates can change over time (Green et al., 1994), and perhaps having parents intercede until a change may occur is nonetheless valuable. This interpretation would have implications for longer-run results and the importance of extending the treatment over the longer time periods, which is a limitation of this study.

Table A.6: Student Responses to Additional Information

	How Students Responded Outside of School				
Dependent variable	Tutoring	Homework last minute	Study hours	Grades important	College important
Treatment	5.978	-0.227*	0.146	0.234**	0.040
	(3.763)	(0.116)	(0.263)	(0.102)	(0.074)
Control Mean	14.250	1.202	0.380	3.681	3.639
Data source	Child	Child	Child	Child	Child
Observations	154	152	153	155	154
R-squared	0.086	0.087	0.160	0.181	0.133

All columns show the effects of the information treatment on parents. Treatment effects are estimated using regressions that control for baseline GPA, prior GPA, strata indicators and grade-level indicators. Baseline GPA is GPA calculated from students' mid-semester progress reports two months before the experiment began. Prior GPA is students' cumulative GPA from middle school and beyond. Data source indicates whether the dependent variable came from a parent's survey response, a child's survey response, or school records. Robust standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table A.7: Peer Effects

	(1)
Dependent variable	Class Grade
Fraction treated	0.578
	(0.500)
Observations	1042
R-squared	0.417

This table shows how the fraction of the class treated affects class grades for the control group. This effect is calculated using an OLS regression that restricts the sample to the control group and controls for baseline GPA, GPA from a students prior school, grade-level indicators and strata indicators. Results are shown for high school students only. All data are from administrative records. Standard errors are clustered at the teacher level accounting for the 19 clusters using a Wild bootstrap t (Cameron, Gelbach, Miller, 2008) with 1000 repetitions.

*** p<0.01, ** p<0.05, * p<0.1

Table A.8: Multiple Testing Adjustment

Variable	Significance
School contact to parent	**
Contacted more than once	**
School contact to parent	**
Contacted more than once	**
GPA	**
Math Scores	*
English Scores	-
Pr(Don't know)	*
Pr(Missed 0-2)	-
Pr(Missed 3-5)	*
Pr(Missed 5+)	-
Difference from truth	-
Kid doesn't disclose	**
Contacted school	**
Attended conference	-
Ask HW	-
Privileges	*
Talk college	-
Help kid	-
Can help	**
Pr(in-class work habits unsatisfactory)	***
Pr(in-class work habits excellent)	***
Pr(in-class cooperation unsatisfactory)	-
Pr(in-class cooperation excellent)	-
Full-day rate attendance	-
By-class rate attendance	-
Classes missed	-
Pr(Missed assignment)	**
Tutoring	-
HW last minute	*
Study hours	-
Grades important	*
College important	-
Follow up GPA	*
Follow up Grades	*

This table shows significance tests for treatment effects on each of the outcomes listed. Each outcome is grouped with a family of like outcomes and test statistics are adjusted to strongly control for the Family-Wise Error Rate using the step down method of Romano and Wolf (2005). Hypothesis tests are two-sided against the null of zero effect.

*** p<0.01, ** p<0.05, * p<0.1, - p>0.1

Sample Selection and Test Scores

Ideally, state-mandated tests are administered to all students, which would help separate out the treatment effect on participation from the effect on their score. Unfortunately, many students did not take these tests, and as shown previously, missing a score is correlated with treatment status and treatment-control imbalance—prior test scores of treatment-group students are .26 lower and baseline GPA .13 points lower (results not shown). To see whether those who did not take the test responded to the treatment differently than those who did take the test, I compare the GPA results of those who took the standardized tests with those who did not. Specifically, the indicator for treatment is interacted with an indicator for having a math test score or English test score as follows.

$$GPA_i = \beta_0 + \beta_1 * Treatment_i + \beta_2 * Treatment_i * 1(HasScore_i) + X_i'\gamma + \varepsilon_i$$

Where the variable $HasScore_i$ is an indicator for either having an English test score or having a math test score. The coefficient on the interaction term, β_2, indicates whether those who have a test score experienced different effects on GPA than those who do not have a test score. This achievement effect might correlate with the achievement effect on test scores. If β_2 is large, it suggests how the test-score results might be biased—upwards if β_2 is positive and downwards is β_2 is negative.

Table A.9 below shows the results of this analysis. The coefficients on the interaction term for having a score is insignificant (p-value equals .14) but is large and negative. Thus there is some evidence that the treatment effect is smaller for those with test scores compared to those without, which may bias the estimates on test scores downward.

Table A.9

	(1)
Dependent variable	GPA
Treatment	0.700**
	(0.356)
Treatment*(has score)	-0.546
	(0.369)
Observations	279
R-squared	0.653

This table shows the treatment effect on GPA, and interacts the treatment variable with an indicator for whether or not a student has a math standardized test score an English standardized test score. These effects are estimated with an OLS regression that controls for baseline GPA, GPA from a students prior school, grade-level indicators and strata indicators. Results are shown for high school students only. All data are from administrative records. Robust standard errors in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

C The Model

Derivations of PBE

I denote the student's strategy as (type-H strategy; type-L strategy). I denote the parent's strategy as (after $D = 1$, after $D = 0$).

Separating Equilibria

Suppose the student's strategy is $(E = 1, D = 1; E = 0, D = 0)$.

1a. After $D = 1$, the parent knows $t = H$, and will monitor only $U(M = 1) \geq U(M = 0)$.

$$V - w - m + \varepsilon_p(1) \geq V - w + \varepsilon_p(0) \implies -m \geq \varepsilon_p(0) - \varepsilon_p(1).$$

1b. After $D = 0$, the parent does not know the type. However, he updates his beliefs about type $P(H|D = 0) = \frac{(1-R)\pi}{1-R\pi}$. The parent will monitor if $\mathbb{E}(U(M = 1)|D = 0) \geq \mathbb{E}(U(M = 0)|D = 0)$.

$$\begin{aligned} \frac{(1-R)\pi}{1-R\pi} \cdot (V - w - m) - \frac{1-\pi}{1-R\pi} \cdot m + \varepsilon_p(1) &\geq \frac{(1-R)\pi}{1-R\pi} \cdot (V - w) - \frac{1-\pi}{1-R\pi} \cdot w + \varepsilon_p(0) \\ \implies -m + \frac{1-\pi}{1-R\pi} \cdot w &\geq \varepsilon_p(0) - \varepsilon_p(1) \end{aligned}$$

Note that $-m + \frac{1-\pi}{1-R\pi} \cdot w \geq -m$ because $w \geq 0$.

2. **Case 1:** $-m + \frac{1-\pi}{1-R\pi} \cdot w \geq -m \geq \varepsilon_p(0) - \varepsilon_p(1)$ or $(M = 1, M = 1)$.

   - Then a type-H student will choose $E = 1, D = 1$ if $w - c_H + \varepsilon_i(1) \geq \varepsilon_i(0) \implies w - c_H \geq \varepsilon_i(0) - \varepsilon_i(1)$.
   - Then a type-L student will choose $E = 0, D = 0$ if $\varepsilon_i(0) \geq w - c_L + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_L$.

   S1. There is an equilibrium at $(M = 1, M = 1)$ and $(E = 1, D = 1; E = 0, D = 0)$ if $w - c_H \geq \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_L$, and $-m \geq \varepsilon_p(0) - \varepsilon_p(1)$.

Case 2: $-m + \frac{1-\pi}{1-R\pi} \cdot w \geq \varepsilon_p(0) - \varepsilon_p(1) \geq -m$ or $(M = 0, M = 1)$.

- Then a type-H student will choose $E = 1, D = 1$ if $w - c_H + \varepsilon_i(1) \geq \varepsilon_i(0) \implies w - c_H \geq \varepsilon_i(0) - \varepsilon_i(1)$
- Then a type-L student will choose $E = 0, D = 0$ if $\varepsilon_i(0) \geq w - c_L + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_L$

S2. There is an equilibrium at $(M = 0, M = 1)$ and $(E = 1, D = 1; E = 0, D = 0)$ if $w - c_H \geq \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_L$, and $-m + \frac{1-\pi}{1-R\pi} \cdot w \geq \varepsilon_p(0) - \varepsilon_p(1) \geq -m$.

Case 3: $\varepsilon_p(0) - \varepsilon_p(1) \geq -m + \frac{1-\pi}{1-R\pi} \cdot w \geq -m$ or $(M = 0, M = 0)$.

- Then a type-H student will choose $E = 1, D = 1$ if $w - c_H + \varepsilon_i(1) \geq w + \varepsilon_i(0) \implies -c_H \geq \varepsilon_i(0) - \varepsilon_i(1)$.
- Then a type-L student will choose $E = 0, D = 0$ if $w + \varepsilon_i(0) \geq w - c_L + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq -c_L$.

S3. Note that $c_H < c_L$. There is an equilibrium at $(M = 0, M = 0)$ and $(E = 1, D = 1; E = 0, D = 0)$ if $\varepsilon_p(0) - \varepsilon_p(1) \geq -m + \frac{1-\pi}{1-R\pi} \cdot w$, and $-c_H > \varepsilon_i(0) - \varepsilon_i(1) > -c_L$.

Suppose the student's strategy is $(E = 0, D = 0; E = 1, D = 1)$.

1a. After $D = 1$, the parent knows that $t = L$ and will choose $M = 1$ if $U(M = 1) \geq U(M = 0)$ or

$$V - w - m + \varepsilon_p(1) \geq V - w + \varepsilon_p(0) \implies -m \geq \varepsilon_p(0) - \varepsilon_p(1).$$

1b. After $D = 0$, the parent updates his beliefs on the about the student's type.

$$P(H|D = 0) = \frac{P(D = 0|H)P(H)}{P(D = 0|H)P(H) + P(D = 0|L)P(L)} = \frac{\pi}{1 - R + R\pi}$$

Define $P(H|D=0) = p$. Then the parent will not monitor if $\mathbb{E}(U(M=0)|D=0) \geq \mathbb{E}(U(M=1)|D=0)$.

$$\begin{aligned} p(-w) + (1-p)(V-w) + \varepsilon_p(0) &\geq p(-m) + (1-p)(V-w-m) + \varepsilon_p(1) \\ -pw + \varepsilon_p(0) &\geq -m + \varepsilon_p(1) \\ \varepsilon_p(0) - \varepsilon_p(1) &\geq -m + \frac{\pi}{1-R+R\pi} w \end{aligned}$$

Note that $-m + \frac{\pi}{1-R+R\pi} w \geq -m$ because $w \geq 0$.

2. **Case 1:** $-m + \frac{\pi}{1-R+R\pi} w \geq -m \geq \varepsilon_p(0) - \varepsilon_p(1)$ or $(M=1, M=1)$

   - Then type-H will not deviate if $\varepsilon_i(0) \geq w - c_H + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_H$.
   - Then type-L will not deviate if $w - c_L + \varepsilon_i(1) \geq \varepsilon_i(0) \implies \varepsilon_i(0) - \varepsilon_i(1) \leq w - c_L$.
   - Hence, this is not an equilibrium because $c_H < c_L$, so it cannot be the case that $w - c_L \geq \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_H$.

   Case 2: $-m + \frac{\pi}{1-R+R\pi} w \geq \varepsilon_p(0) - \varepsilon_p(1) \geq -m$ or $(M=0, M=1)$

   - Then type-H will not deviate if $\varepsilon_i(0) \geq w - c_H + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_H$.
   - Then type-L will not deviate if $w - c_L + \varepsilon_i(1) \geq \varepsilon_i(0) \implies \varepsilon_i(0) - \varepsilon_i(1) \leq w - c_L$.
   - Hence, this is not an equilibrium because $c_H < c_L$, so it cannot be the case that $w - c_L \geq \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_H$.

   Case 3: $\varepsilon_p(0) - \varepsilon_p(1) \geq -m + \frac{\pi}{1-R+R\pi} w \geq -m$ or $(M=0, M=0)$

   - Then type-H will not deviate if $w + \varepsilon_i(0) \geq w - c_H + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq -c_H$.
   - Then type-L not deviate if $w - c_L + \varepsilon_i(1) \geq w + \varepsilon_i(0) \implies -c_L \geq \varepsilon_i(0) - \varepsilon_i(1)$.
   - Hence, this is not an equilibrium because $c_H < c_L \implies -c_H > -c_L$, so it cannot be the case that $-c_L \geq \varepsilon_i(0) - \varepsilon_i(1) \geq -c_H$.

Pooling Equilibria

Suppose the student's strategy is $(E = 1, D = 1; E = 1, D = 1)$.

1a. After $D = 1$, knows that the student is exerting effort regardless of type. Nevertheless, they pick $M = 1$ if $U(M = 1) \geq U(M = 0)$. That is, $V - w - m + \varepsilon_p(1) \geq V - w + \varepsilon_p(0) \implies -m \geq \varepsilon_p(0) - \varepsilon_p(1)$.

1b. After $D = 0$, then either a report has not been generated with probability $1 - R$ or the students are playing an off equilibrium strategy. The parent will update their beliefs as follows:

$$P(H|D = 0) = \frac{P(H)P(D = 0|H)}{P(H)P(D = 0|H) + P(L)P(D = 0|L)} = \frac{\pi(1 - R)}{\pi(1 - R) + (1 - \pi)(1 - R)} = \pi$$

The parent cannot update their beliefs. Therefore, they will monitor if $U(M = 1) \geq U(M = 0)$. That is, $V - w - m + \varepsilon_p(1) \geq V - w + \varepsilon_p(0) \implies -m \geq \varepsilon_p(0) - \varepsilon_p(1)$.

2. **Case 1:** $-m \geq \varepsilon_p(0) - \varepsilon_p(1)$ or $(M = 1, M = 1)$.

   - Then type-H will not deviate if $w - c_H + \varepsilon_i(1) \geq \varepsilon_i(0) \implies w - c_H \geq \varepsilon_i(0) - \varepsilon_i(1)$.
   - Then type-L will not deviate if $w - c_L + \varepsilon_i(1) \geq \varepsilon_i(0) \implies w - c_L \geq \varepsilon_i(0) - \varepsilon_i(1)$.

   P1. Then there is a pooling equilibrium at $-m \geq \varepsilon_p(0) - \varepsilon_p(1)$ and $w - c_L \geq \varepsilon_i(0) - \varepsilon_i(1)$

   Case 2: $\varepsilon_p(0) - \varepsilon_p(1) \geq -m$ or $(M = 0, M = 0)$.

   - Then type-H will not deviate if $w - c_H + \varepsilon_i(1) \geq w + \varepsilon_i(0) \implies -c_H \geq \varepsilon_i(0) - \varepsilon_i(1)$.
   - Then type-L will not deviate if $w - c_L + \varepsilon_i(1) \geq w + \varepsilon_i(0) \implies -c_L \geq \varepsilon_i(0) - \varepsilon_i(1)$.

   P2. Then there is a pooling equilibrium at $\varepsilon_p(0) - \varepsilon_p(1) \geq -m$ and $-c_L \geq \varepsilon_i(0) - \varepsilon_i(1)$

Suppose the student's strategy is $(E = 0, D = 0; E = 0, D = 0)$.

1a. After $D = 1$, the parent sees that the student is playing off-equilibrium. He will monitor if $U(M = 1) \geq U(M = 0) \implies V - w - m + \varepsilon_p(1) \geq V - w + \varepsilon_p(0) \implies -m \geq \varepsilon_p(0) - \varepsilon_p(1)$.

1b. After $D = 0$, the parent cannot update his beliefs. He will monitor if $U(M = 1) \geq U(M = 0) \implies -m + \varepsilon_p(1) \geq -w + \varepsilon_p(0) \implies -m + w \geq \varepsilon_p(0) - \varepsilon_p(1)$. Note that $w > 0 \implies -m + w > -m$.

2. **Case 1:** $-m + w \geq -m \geq \varepsilon_p(0) - \varepsilon_p(1)$ or $(M = 1, M = 1)$.

   – An H-type student will not deviate if $\varepsilon_i(0) \geq w - c_H + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_H$

   – An L-type student will not deviate if $\varepsilon_i(0) \geq w - c_L + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_L$

   P3. Note that $w - c_H \geq w - c_L$, so there is an equilibrium at $\varepsilon_i(0) - \varepsilon_i(1) \geq w - c_H$ and $-m \geq \varepsilon_p(0) - \varepsilon_p(1)$.

   Case 2: $-m + w \geq \varepsilon_p(0) - \varepsilon_p(1) \geq -m$ or $(M = 0, M = 1)$.

   – An H-type student will not deviate if $\varepsilon_i(0) \geq w - c_H + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_H$

   – An L-type student will not deviate if $\varepsilon_i(0) \geq w - c_L + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_L$

   P4. Note that $w - c_H \geq w - c_L$, so there is an equilibrium at $\varepsilon_i(0) - \varepsilon_i(1) \geq w - c_H$ and $-m + w \geq \varepsilon_p(0) - \varepsilon_p(1) \geq -m$

   Case 3: $\varepsilon_p(0) - \varepsilon_p(1) \geq m + w \geq -m$ or $(M = 0, M = 0)$.

   – An H-type student will not deviate if $w - \varepsilon_i(0) \geq w - c_H + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq -c_H$

– An L-type student will not deviate if $w-\varepsilon_i(0) \geq w-c_L+\varepsilon_i(1) \implies \varepsilon_i(0)-\varepsilon_i(1) \geq -c_L$

P5. Hence, there is an equilibrium at $\varepsilon_i(0)-\varepsilon_i(1) \geq -c_H$ and $\varepsilon_p(0)-\varepsilon_p(1) \geq m+w$.

Hybrid Equilibria

H1. Suppose the student's strategy is $(E=1, D=1; \text{mix with } P(E=1, D=1)=q_L)$.

1a. After seeing $D=1$, the parent updates his beliefs as follows:

$$\begin{aligned} P(H|D=1) &= \frac{P(D=1|H)P(H)}{P(D=1|H)P(H)+P(D=1|L)P(L)} \\ &= \frac{R\pi}{R\pi+Rq_L(1-\pi)} \\ &= \frac{\pi}{\pi+(1-\pi)q_L} \end{aligned}$$

– The parent will be indifferent if $\mathbb{E}(U(M=1)|D=1)=\mathbb{E}(U(M=0)|D=1) \implies V-w-m+\varepsilon_p(1)=V-w+\varepsilon_p(0) \implies -m=\varepsilon_p(0)-\varepsilon_p(1)$.

1b. After seeing $D=0$, the parent knows that either a report has not been generated, or that the low type has not exerted effort.

$$\begin{aligned} P(H|D=0) &= \frac{P(D=0|H)P(H)}{P(D=0|H)P(H)+P(D=0|L)P(L)} \\ &= \frac{(1-R)\pi}{(1-R)\pi+((1-R)q_L+(1-q_L))(1-\pi)} \\ &= \mu \end{aligned}$$

– The parent will be indifferent after $D=0$ if $\mathbb{E}(U(M=1)|D=0)=\mathbb{E}(U(M=0)|D=0) \implies \mu(V-w-m)+(1-\mu)(q_L(V-w-m)+(1-q_L)(-m))+\varepsilon_p(1)=\mu(V-w)+(1-\mu)(q_LV-w)+\varepsilon_p(0) \implies -m+(1-\mu)(1-q_L)w=\varepsilon_p(0)-\varepsilon_p(1)$

Case 1: $-m = \varepsilon_p(0) - \varepsilon_p(1) \implies -m + (1 - (\mu_1 + (1 - \mu_1)q_L))\, w > \varepsilon_p(0) - \varepsilon_p(1)$. The parent's strategy is (mix with $\alpha_{01} = P(M = 0|D = 1)$ after $D = 1$, $M = 1$ after $D = 0$)

- The type-L student will be indifferent if $\mathbb{E}U(E = 1, D = 1) = \mathbb{E}U(E = 0, D = 0) \implies w - c_L + \varepsilon_i(1) = \varepsilon_i(0) \implies w - c_L = \varepsilon_i(0) - \varepsilon_i(1)$.

- The type-H student will not deviate if $\mathbb{E}U(E = 1, D = 1) \geq \mathbb{E}U(E = 0, D = 0) \implies w - c_H + \varepsilon_i(1) \geq \varepsilon_i(0) \implies w - c_H \geq \varepsilon_i(0) - \varepsilon_i(1)$.

– Then there is an equilibrium if $-m + (1 - (\mu_1 + (1 - \mu_1)q_L))\, w > \varepsilon_p(0) - \varepsilon_p(1) = -m, w - c_H \geq \varepsilon_i(0) - \varepsilon_i(1) = w - c_L$. However, as the probability of an event is not defined at a point, this equilibrium occurs with probability 0.

Case 2: $-m + (1 - \mu)(1 - q_L)w = \varepsilon_p(0) - \varepsilon_p(1) \implies \varepsilon_i(0) - \varepsilon_i(1) > -m$. The parent's strategy is ($M = 0$ after $D = 1$,mix after $D = 0$).

- The L-type student will be indifferent if $\mathbb{E}U(E = 1, D = 1) = \mathbb{E}U(E = 0, D = 0) \implies w - c_H + \varepsilon_i(1) = \alpha_{00}(w) + \varepsilon_i(0) \implies (1 - \alpha_{00})w - c_L = \varepsilon_i(0) - \varepsilon_i(1)$.

- The H-type will not deviate if $\mathbb{E}U(E = 1, D = 1) \geq \mathbb{E}U(E = 0, D = 0) \implies w - c_H + \varepsilon_i(1) \geq \alpha_{00}w + \varepsilon_i(0) \implies (1 - \alpha_{00})w - c_H \geq \varepsilon_i(0) - \varepsilon_i(1)$.

H1. Then there is an equilibrium at $-m + (1 - \mu)(1 - q_L)w = \varepsilon_p(0) - \varepsilon_p(1)) > -m$ and $(1 - \alpha_{00})w - c_H \geq \varepsilon_i(0) - \varepsilon_i(1) = (1 - \alpha_{00})w - c_L$. Further, as α and q_L both vary between 0 and 1, $-m + \frac{1-\pi}{1-R\pi}w \geq \varepsilon_p(0) - \varepsilon_p(1)) \geq -m$, and $w - c_L \geq \varepsilon_i(0) - \varepsilon_i(1) \geq -c_L$.

Suppose the student's strategy is ($E = 0, D = 0$; mix with $P(E = 1, D = 1) = q_L$).

1a. After $D = 1$, the parent knows that the child must be a low type. Therefore, the parent is indifferent if $U(M = 1) = U(M = 0) \implies V - w - m + \varepsilon_p(1) = V - w + \varepsilon_p(0) \implies -m = \varepsilon_p(0) - \varepsilon_p(0)$.

1b. After $D = 0$, the parent knows that either a report has not been generated for a hard working L-type, or both types are not working.

$$\begin{aligned} P(H|D=0) &= \frac{P(D=0|H)P(H)}{P(D=0|H)P(H)+P(D=0|L)P(L)} \\ &= \frac{\pi}{\pi+((1-R)q_L+(1-q_L))(1-\pi)} \\ &= \mu_2 \end{aligned}$$

Then the parent is indifferent between monitoring and not monitoring if $\mathbb{E}U(M=1) = \mathbb{E}U(M=0) \implies \mu_2(-m)+(1-\mu_2)(q_L(V-w-m)+(1-q_L)(-m))+\varepsilon_p(1) = \mu_2(-w)+(1-\mu_2)(q_L(V-w)+(1-q_L)(-w)) \implies -m+\mu q_L w = \varepsilon_p(0)-\varepsilon_p(1)$

Case 1: $-m+\mu q_L w \geq \varepsilon_p(0)-\varepsilon_p(0) = -m$. The parent's strategy is (mix α_{01} after $D=1$,$M=1$ after $D=0$).

- The L-type student will mix if $\mathbb{E}U(E=1,D=1) = \mathbb{E}U(E=0,D=0) \implies w-c_L+\varepsilon_i(1) = \varepsilon_i(0) \implies w-c_L = \varepsilon_i(0)-\varepsilon_i(1)$.

- The H-type student will not deviate if $\mathbb{E}U(E=0,D=0) \geq \mathbb{E}U(E=1,D=1) \implies \varepsilon_i(0) \geq w-c_H+\varepsilon_i(1) \implies \varepsilon_i(0)-\varepsilon_i(1) \geq w-c_H$.

- However, $w-c_H > w-c_L$. Therefore, no equilibrium exists.

Case 2: $-m+\mu q_L w = \varepsilon_p(0)-\varepsilon_p(1) \geq -m$. The parent's strategy is ($M=0$ after $D=1$, mix after $D=0$).

- The L-type will mix if $\mathbb{E}U(E=1,D=1) = \mathbb{E}U(E=0,D=0) \implies w-c_L+\varepsilon_i(1) = \alpha_{00}w+\varepsilon_i(0) \implies (1-\alpha_{00})w-c_L = \varepsilon_i(0)-\varepsilon_i(1)$.

- The H-type student will not deviate if $\mathbb{E}U(E=0,D=0) \geq \mathbb{E}U(E=1,D=1) \implies \alpha_{00}w+\varepsilon_i(0) \geq w-c_H+\varepsilon_i(1) \implies \varepsilon_i(0)-\varepsilon_i(1) \geq (1-\alpha_{00})w-c_H$.

- However, $(1-\alpha_{00})w - c_H > (1-\alpha_{00})w - c_L$. Therefore, no equilibrium exists.

Suppose the student's strategy is (mix with $P(E=1, D=1) = q_H; E=0, D=0$).

1a. After seeing $D=1$, the parent knows that it must be the H-type student. Therefore, he will mix if $U(M=1) = U(M=0) \implies V - w - m + \varepsilon_p(1) = V - w + \varepsilon_p(0) \implies -m = \varepsilon_p(0) - \varepsilon_p(0)$.

1b. After seeing $D=0$, the parent knows that either a report has not been generated for a H-type student exerting effort with q_H, or that neither type is exerting effort.

$$\begin{aligned} P(H|D=0) &= \frac{P(D=0|H)P(H)}{P(D=0|H)P(H) + P(D=0|L)P(L)} \\ &= \frac{((1-R)q_H + (1-q_H))\pi}{((1-R)q_H + (1-q_H))\pi + (1-\pi)} \\ &= \nu \end{aligned}$$

The parent will mix if $\mathbb{E}U(M=1) = \mathbb{E}U(M=0) \implies \nu_1(q_H(V-w-m) + (1-q_H)(-m)) + (1-\nu)(-m) + \varepsilon_p(1) = \nu_1(q_H(V-w) + (1-q_H)(-w)) + (1-\nu)(-w) + \varepsilon_p(0) \implies -m + (1-\nu q_H)w = \varepsilon_p(0) - \varepsilon_p(1)$.

Case 1: $-m + (1-\nu_1 q_H)w > \varepsilon_p(0) - \varepsilon_p(1) = -m$. The parent's strategy is (mix after $D=1$, $M=1$ after $D=0$)

- L-types will not deviate if $\mathbb{E}(U(E=0)) \geq \mathbb{E}(U(E=1)) \implies \varepsilon_i(0) \geq w - c_L + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_L$
- H-types will mix if $\mathbb{E}(U(E=1)) = \mathbb{E}(U(E=0)) \implies w - c_H + \varepsilon_i(1) = \varepsilon_i(0) \implies w - c_H = \varepsilon_i(0) - \varepsilon_i(1)$
- Then there is an equilibrium at $-m + (1-\nu q_H)w > \varepsilon_p(0) - \varepsilon_p(1) = -m$, $w - c_H = \varepsilon_i(0) - \varepsilon_i(1) \geq w - c_L$. However, as explained above, this equilibrium occurs with probability 0.

Case 2: $-m + (1-\nu q_H)w = \varepsilon_p(0) - \varepsilon_p(1) > -m$. The parent's strategy is ($M = 1$ after $D = 1$, mix after $D = 0$ with $\alpha_{00} = P(M = 0|D = 0)$)

- The L-type will not deviate if $\mathbb{E}(U(E = 0)) \geq \mathbb{E}(U(E = 1)) \implies \alpha_{00}w + \varepsilon_i(0) \geq w - c_L + \varepsilon_i(1) \implies \varepsilon_i(0) - \varepsilon_i(1) \geq (1-\alpha_{00})w - c_L$.
- The H-type will mix if $\mathbb{E}(U(E = 0)) = \mathbb{E}(U(E = 1)) \implies \alpha_{00}w + \varepsilon_i(0) = w - c_H + \varepsilon_i(1) \implies (1-\alpha_{00})w - c_H = \varepsilon_i(0) - \varepsilon_i(1)$.

H2. Then there is an equilibrium at $-m + (1-\nu q_H)w = \varepsilon_p(0) - \varepsilon_p(1) > -m$ and $(1-\alpha_{00})w - c_H = \varepsilon_i(0) - \varepsilon_i(1) \geq (1-\alpha_{00})w - c_L$. Further, as q_H and α_{00} vary from 0 to 1, $-m+w \geq \varepsilon_p(0) - \varepsilon_p(1)) \geq -m + \frac{1-\pi}{1-R\pi}w$, and $w - c_H \geq \varepsilon_i(0) - \varepsilon_i(1) \geq -c_H$.

H4. Suppose the student's strategy is (mix with $P(E = 1, D = 1) = q_H; E = 1, D = 1$).

1a. After seeing $D = 1$, the parent updates his beliefs.

$$\begin{aligned}
P(H|D=1) &= \frac{P(D=1|H)P(H)}{P(D=1|H)P(H) + P(D=1|L)P(L)} \\
&= \frac{q_H\pi}{q_H\pi + (1-\pi)} \\
&= \gamma_1
\end{aligned}$$

Then the parent will mix if $V - w - m + \varepsilon_p(1) = V - w + \varepsilon_p(0) \implies -m = \varepsilon_p(0) - \varepsilon_p(1)$.

1b. After seeing $D = 0$, the parent know that either a report was not generated or that the H-type is playing $E = 0, D = 0$.

$$\begin{aligned}
P(H|D=1) &= \frac{P(D=1|H)P(H)}{P(D=1|H)P(H) + P(D=1|L)P(L)} \\
&= \frac{(1-R)q_H\pi}{((1-q_H) + (1-R)q_H)\pi + (1-\pi)} \\
&= \gamma_2
\end{aligned}$$

Then the parent will mix if $\mathbb{E}U(M=1) = \mathbb{E}U(M=0) \implies \gamma_2(q_H(V-w-m)+(1-q_H)(-m))+(1-\gamma_2)(V-w-m)+\varepsilon_p(1) = \gamma_2(q_H(V-w)+(1-q_H)(-w))+(1-\gamma_2)(V-w)+\varepsilon_p(0) \implies -m+\gamma_2(1-q_H)w = \varepsilon_p(0)-\varepsilon_p(1)$.

Case 1: $-m+\gamma_2(1-q_H)w = \varepsilon_p(0)-\varepsilon_p(1) > -m$ The parent's strategy is ($M=0$ after $D=1$,mix after $D=0$ with $\alpha_{00} = P(M=0|D=0)$)

- The L-type will not deviate if $\mathbb{E}U(E=1) \geq \mathbb{E}U(E=0) \implies w-c_L+\varepsilon_i(1) \geq \alpha_{00}w+\varepsilon_i(0) \implies (1-\alpha_{00})w-c_L \geq \varepsilon_i(0)-\varepsilon_i(1)$.
- The H-type will mix if $\mathbb{E}U(E=1) \geq \mathbb{E}U(E=0) \implies w-c_H+\varepsilon_i(1) = \alpha_{00}w+\varepsilon_i(0) \implies (1-\alpha_{00})w-c_H = \varepsilon_i(0)-\varepsilon_i(1)$.
- Then there is no equilibrium because $w-c_H \geq w-c_L$.

Case 2:$-m+\gamma_2(1-q_H)w > \varepsilon_p(0)-\varepsilon_p(1) = -m$ The parent's strategy is (mix after $D=1$, $M=1$ after $D=0$)

- The L-type will not deviate if $\mathbb{E}U(E=1) \geq \mathbb{E}U(E=0) \implies w-c_L+\varepsilon_i(1) \geq \varepsilon_i(0) \implies w-c_L \geq \varepsilon_i(0)-\varepsilon_i(1)$.
- The H-type will mix if $\mathbb{E}U(E=1) = \mathbb{E}U(E=0) \implies w-c_H+\varepsilon_i(1) = \varepsilon_i(0) \implies w-c_H = \varepsilon_i(0)-\varepsilon_i(1)$.
- Then there is no equilibrium because $w-c_H \geq w-c_L$.

Constructing Probabilities

First, I define the probability for each set of actions, $a = (M,E,D)$, and equilibrium. To derive these probabilities, I multiply the probability of an action given the parameters θ and the equilibrium that occurs, $P(a|Eq,\theta)$, by the probability that the equilibrium occurs, $P(Eq)$. For example, the probability of $a=(1,1,1)$ given the first separating equilibrium in the appendix, $S1$, is the probability that a high type is drawn and a report is generated.

The probability that equilibrium $S1$ is drawn is $(\Phi(w-c_H)-\Phi(w-c_L))\Phi(-m)$ where $\Phi(\cdot)$ is the cumulative normal distribution function.

The joint probabilities for these actions and separating equilibria $S1-S3$ and pooling equilibria $P1-P5$ are defined as follows, letting $\gamma = \frac{1-\pi}{1-R\pi}$ to condense notation:

$$
\begin{aligned}
P(1,1,1,S1|\theta) &= \pi R(\Phi(w-c_H)-\Phi(w-c_L))\Phi(-m) \\
P(1,1,0,S1|\theta) &= \pi(1-R)(\Phi(w-c_H)-\Phi(w-c_L))\Phi(-m) \\
P(1,0,0,S1|\theta) &= (1-\pi)(\Phi(w-c_H)-\Phi(w-c_L))\Phi(-m) \\
P(0,1,1,S2|\theta) &= \pi R(\Phi(w-c_H)-\Phi(w-c_L))(\Phi(-m+\gamma w)-\Phi(-m)) \\
P(1,1,0,S2|\theta) &= \pi(1-R)(\Phi(w-c_H)-\Phi(w-c_L))(\Phi(-m+\gamma w)-\Phi(-m)) \\
P(1,0,0,S2|\theta) &= (1-\pi)(\Phi(w-c_H)-\Phi(w-c_L))(\Phi(-m+\gamma w)-\Phi(-m)) \\
P(0,1,1,S3|\theta) &= \pi R(\Phi(-c_H)-\Phi(-c_L))(1-\Phi(-m+\gamma w)) \\
P(0,1,0,S3|\theta) &= \pi(1-R)(\Phi(-c_H)-\Phi(-c_L))(1-\Phi(-m+\gamma w)) \\
P(0,0,0,S3|\theta) &= (1-\pi)(\Phi(-c_H)-\Phi(-c_L))(1-\Phi(-m+\gamma w)) \\
P(1,1,1,P1|\theta) &= R(\Phi(w-c_L))\Phi(-m) \\
P(1,1,0,P1|\theta) &= (1-R)\Phi(w-c_L)\Phi(-m) \\
P(0,1,1,P2|\theta) &= R\Phi(-c_L)(1-\Phi(-m)) \\
P(0,1,0,P2|\theta) &= (1-R)\Phi(-c_L)(1-\Phi(-m)) \\
P(1,0,0,P3|\theta) &= (1-\Phi(w-c_H))\Phi(-m) \\
P(1,0,0,P4|\theta) &= (1-\Phi(w-c_H))(\Phi(-m+w)-\Phi(-m)) \\
P(0,0,0,P5|\theta) &= (1-\Phi(-c_H))(1-\Phi(-m+w))
\end{aligned}
$$

For the hybrid equilibria, $H1$, the probability of the outcome $a=(0,0,0)$, given $\varepsilon_i(0)-\varepsilon_i(1)$ and $\varepsilon_p(0)-\varepsilon_p(1)$, is the probability of drawing a low type, 1-π, multiplied by the probability of the low type playing $E = 0$, $1 - q_L$, multiplied the probability of the parent playing $M = 0$, α. Note that q_L and α are functions of $\varepsilon_p(0) - \varepsilon_p(1)$ and $\varepsilon_i(0) - \varepsilon_i(1)$, respectively.

$$(1 - \pi)(1 - q_L(\varepsilon_p(0) - \varepsilon_p(1)))\alpha(\varepsilon_i(0) - \varepsilon_i(1))$$

I define $\Delta\varepsilon_j = \varepsilon_j(0) - \varepsilon_j(1)$.I need to integrate out the $\Delta\varepsilon$. Therefore, I integrate the over the intervals $(-c_L, w - c_L)$ and $(-m, -m + \gamma w)$.

$$P(0, 0, 0|H1, \theta) = \int_{-m}^{-m+\gamma w} \int_{-c_L}^{w-c_L} (1 - \pi)(1 - q_L(\Delta\varepsilon_p))\alpha(\Delta\varepsilon_i)\phi(\Delta\varepsilon_p)\phi(\Delta\varepsilon_i)d(\Delta\varepsilon_i)d(\Delta\varepsilon_p)$$

Note that $\Delta\varepsilon_i = -m + (1 - \mu)(1 - q_L)w$ and $\Delta\varepsilon_p = (1 - \alpha_{00})w - c_L$. Therefore, I can perform a change of variables.

$$\begin{aligned} P(0, 0, 0|H1, \theta) = \\ (1 - \pi) \int_0^1 (1 - q_L)\phi(-m + (1 - \mu)(1 - q_L)w)\frac{d(1 - \mu)(1 - q_L)w}{dq_L} dq_L \times \\ \int_0^1 \alpha_{00}\phi((1 - \alpha_{00})w - c_L)(-w)d\alpha_{00} \end{aligned}$$

These integrals are evaluated for a given set of parameters using standard Monte Carlo integration. All other probabilities for hybrid equilibria are evaluated similarly.

$$\begin{aligned} P(1, 0, 0|H1, \theta) = \\ (1 - \pi) \int_0^1 (1 - q_L)\phi(-m + (1 - \mu)(1 - q_L)w)\frac{d(1 - \mu)(1 - q_L)w}{dq_L} dq_L \times \\ \int_0^1 (1 - \alpha_{00})\phi((1 - \alpha_{00})w - c_L)(-w)d\alpha_{00} \end{aligned}$$

$$P(0, 1, 1|H1, \theta) =$$

$$\int_0^1 (R(\pi + (1-\pi)q_L)\phi(-m + (1-\mu)(1-q_L)w)\frac{d(1-\mu)(1-q_L)w}{dq_L}dq_L \times \int_0^1 \phi((1-\alpha_{00})w - c_L)(-w)d\alpha_{00}$$

$$P(0,1,0|H1,\theta) = \int_0^1 ((1-R)\pi + (1-R)(1-\pi)q_L)\phi(-m + (1-\mu)(1-q_L)w)\frac{d(1-\mu)(1-q_L)w}{dq_L}dq_L \times \int_0^1 \alpha_{00}\phi((1-\alpha_{00})w - c_L)(-w)d\alpha_{00}$$

$$P(1,1,0|H1,\theta) = \int_0^1 ((1-R)\pi + (1-R)(1-\pi)q_L)\phi(-m + (1-\mu)(1-q_L)w)\frac{d(1-\mu)(1-q_L)w}{dq_L}dq_L \times \int_0^1 (1-\alpha_{00})\phi((1-\alpha_{00})w - c_L)(-w)d\alpha_{00}$$

$$P(0,0,0|H2,\theta) = \int_0^1 ((1-\pi) + \pi(1-q_H))\phi(-m + (1-\nu q_H)w)\frac{d(1-\nu)q_H w}{dq_H}dq_H \times \int_0^1 \alpha_{00}\phi((1-\alpha_{00})w - c_H)(-w)d\alpha_{00}$$

$$P(1,0,0|H2,\theta) = \int_0^1 ((1-\pi) + \pi(1-q_H))\phi(-m + (1-\nu q_H)w)\frac{d(1-\nu)q_H w}{dq_H}dq_H \times \int_0^1 (1-\alpha_{00})\phi((1-\alpha_{00})w - c_H)(-w)d\alpha_{00}$$

$$P(1,1,1|H2,\theta) = \int_0^1 (\pi R q_H)\phi(-m + (1-\nu q_H)w)\frac{d(1-\nu)q_H w}{dq_H}dq_H \times \int_0^1 \phi((1-\alpha_{00})w - c_H)(-w)d\alpha_{00}$$

$$P(1,1,0|H2,\theta) = \int_0^1 (\pi(1-R)q_H)\phi(-m + (1-\nu q_H)w)\frac{d(1-\nu)q_H w}{dq_H}dq_H \times \int_0^1 (1-\alpha_{00})\phi((1-\alpha_{00})w - c_H)(-w)d\alpha_{00}$$

$$P(0,1,0|H2,\theta) = \int_0^1 (\pi(1-R)q_H)\phi(-m + (1-\nu q_H)w)\frac{d(1-\nu)q_H w}{dq_H}dq_H \times \int_0^1 \alpha_{00}\phi((1-\alpha_{00})w - c_H)(-w)d\alpha_{00}$$

The probability of each event a is just the sum of the equilibrium conditional probabilities of the event.

$$P(a|\theta) = \sum_{e \in EQ} P(a|e,\theta,\pi)$$

Estimation

Uniqueness of equilibria means that there is a well-defined likelihood function. Let j be the observation. The likelihood function is

$$\begin{aligned}\log L((m_p, e_i, d_i)|\theta) = \frac{1}{n}\sum_{j=1}^{n}&(m_{pj}e_{ij}d_{ij}\log P((1,1,1)|\theta) + m_{pj}e_{ij}(1-d_{ij})\log P((1,1,0)|\theta)\\ &+ m_{pj}(1-e_{ij})(1-d_{ij})\log P((1,0,0)|\theta)\\ &+ (1-m_{pj})e_{ij}d_{ij}\log P((0,1,1)|\theta)\\ &+ (1-m_{pj})e_{ij}(1-d_{ij})\log P((0,1,0)|\theta)\\ &+ (1-m_{pj})(1-e_{ij})(1-d_{ij})P((0,0,0)|\theta)))\end{aligned}$$

Clearly, V is not identified. Furthermore, there are a number of issues in identifying w, c_L, and c_H. To illustrate, suppose that $w = 0$. (Notice that this implies that there are no hybrid equilibria.) Then the probabilities of particular events reduce to

$$\begin{aligned}P(1,1,1|\theta) &= R(\pi\Phi(-c_H) + (1-\pi)\Phi(-c_L))\Phi(-m)\\ P(1,1,0|\theta) &= (1-R)(\pi\Phi(-c_H) + (1-\pi)\Phi(-c_L))\Phi(-m)\\ P(1,0,0|\theta) &= -(\pi\Phi(-c_H) + (1-\pi)\Phi(-c_L))\Phi(-m) + \Phi(-m)\\ P(0,1,1|\theta) &= R(\pi\Phi(-c_H) + (1-\pi)\Phi(-c_L))(1-\Phi(-m))\\ P(0,1,0|\theta) &= (1-R)(\pi\Phi(-c_H) + (1-\pi)\Phi(-c_L))(1-\Phi(-m))\\ P(0,0,0|\theta) &= -(\pi\Phi(-c_H) + (1-\pi)\Phi(-c_L))(1-\Phi(-m)) + (1-\Phi(-m))\end{aligned}$$

Let $\pi\Phi(-c_H) + (1-\pi)\Phi(-c_L) = \eta$. Then this system of equations reduces to

$$\begin{aligned}P(1,1,1|\theta) &= R\eta\Phi(-m)\\ P(1,1,0|\theta) &= (1-R)\eta\Phi(-m)\end{aligned}$$

$$\begin{aligned}
P(1,0,0|\theta) &= -\eta\Phi(-m)+\Phi(-m) \\
P(0,1,1|\theta) &= R\eta(1-\Phi(-m)) \\
P(0,1,0|\theta) &= (1-R)\eta(1-\Phi(-m)) \\
P(0,0,0|\theta) &= -\eta(1-\Phi(-m))+(1-\Phi(-m))
\end{aligned}$$

I can pin down $\Phi(-m)$, η, and R using the above equations. However, I cannot pin down π, c_H, and c_L individually.

This problem carries over to the case that $w > 0$. If the probability of a hybrid equilibria is small, then $P(1,1,1|\theta)$ is close to a linear combination of $P(0,1,1|\theta)$. If γ is close to one, then $P(1,1,0|\theta)$ is close to a linear combination of $P(1,0,0|\theta)$. $P(0,0,0|\theta)$ is close to a linear combination of $P(0,1,0|\theta)$ if w is small and the probability of hybrid equilibria is small. Under these conditions it is difficult to identify w, c_L, c_H.

Distribution of Equilibria and Model Fit

Table A.10: Distribution of Equilibria for Treatment and Control Groups

Equilibrium	Control Distribution	Treatment Distribution
S1	0.15972	0.19879
S2	0.31243	0.29546
S3	0.04625	0.00923
P1	0.15955	0.19854
P2	0.00094	0.00083
P3	0.00002	0.00002
P4	0.00002	0.00002
P5	0.00092	0.00051
H1	0.31231	0.29537
H2	0.00784	0.00124

Note: This table shows the distribution of equilibria estimated for the treatment and control groups, respectively. Each equilibrium (S1, S2, etc.) corresponds to an equilibrium labeled as such in the derivations below.

Table A.11: Simulated Moments Compared to Observed Moments

Variable	Model	Observed
Treatment Group		
Monitor	0.766	0.743
Effort	0.544	0.548
Disclosure	0.363	0.387
Control Group		
Monitor	0.731	0.717
Effort	0.501	0.512
Disclosure	0.320	0.356

Note: This table shows the distribution of equilibria estimated for the treatment and control groups, respectively. Each equilibrium (S1, S2, etc.) corresponds to an equilibrium labeled as such in the derivations below.

D Middle School Results

Table A.12 summarizes the effects on achievement and effort-related outcomes for middle school students, which are mostly small and not significantly different from zero. These results are consistent with the effects on how parents used the additional information, how children responded, and parents' awareness and demand for information (Table A.13). Based on these results and the contamination, it is difficult to discern what effect the treatment would have had on younger students.

There are several reasons the additional information may have had less effect on younger children. First, the middle school students have less margin to improve: Their GPA is almost a full standard deviation higher than high school students' GPA, middle school students miss 7.5% of their assignments compared to high school students who miss 20% of their assignments, and attendance and behavior are also better for middle school students. However if this were the only cause for small effects, there could still be an impact on students who were lower-performing at baseline. Unfortunately the study is underpowered to examine subgroups in the middle school, but point estimates show students with higher GPA respond more positively to additional information (results not shown).

A second reason there might be smaller effects for middle school students is that parents might be able to control younger children better than older children. It might be less costly for parents to motivate their children or information problems arise less frequently. There is some support for this hypothesis since teacher-measured behavior at baseline is better for middle school students than high school students, which might correlate with parents' ability to control their child. Third, the repeated messages to middle school parents through the information treatment and the contamination by the school employee may have annoyed them. If they had already resolved an issue such as a missing assignment, receiving a second message regarding that work might have been confusing and frustrating. Parents could have viewed the information treatment as less reliable given the lack of coordination about school-to-parent contact and started ignoring it, which might explain the small negative coefficients on several middle school outcomes.

Lastly, parents of middle-school students might already obtain information about their child's education more actively, which is reflected in the higher parent-teacher conference attendance. In addition, a comparison of the control groups in the high school and middle school shows that parents of middle school students are more likely to take away privileges from their children, be aware of information problems, contact the school about their child's grades, and feel that they can help their child try their best than parents of high school students in the control group.[32] It is possible that the contamination caused this higher level of involvement—meaning messages home did affect middle school parents—or it could be that these parents were already more involved than high school parents. If the latter, it leaves open the question of why this involvement wanes as children get older; perhaps parents perceive they have less control of their child's effort or that they no longer know how to help them. In short, the effect of additional information on younger children is inconclusive.

[32] Results available upon request.

Table A.12: Middle School Student Outcomes

Dependent Variable	Treatment	Standard Error	Students	Observations
GPA	-0.108	(0.102)	149	149
Final Exams	-0.054	(0.199)	87	87
Math CST	0.034	(0.119)	139	139
English CST	-0.017	(0.13)	145	145
Pr(missed assignment)	0.005	(0.01)	87	7,692
Work habits unsatisfactory	0.019	(0.019)	149	2,635
Work habits excellent	-0.041	(0.039)	149	2,635
Cooperation unsatisfactory	0.004	(0.009)	149	2,635
Cooperation excellent	-0.021	(0.038)	149	2,635
Full-day attendance	-1.101	(0.684)	149	148
By-class attendance	-1.918	(1.24)	149	1,933
Classes missed	0.782	(0.49)	149	1,933

This table summarizes the results of the treatment effects on middle-school student outcomes, where the experiment was contaminated. The results shown are the coefficients on the treatment indicator in a regression that controls for baseline GPA, prior GPA, grade-level indicators and strata indicators. The treatment effect on missing an assignment is the marginal effect at the means from a Probit model. Work habits and cooperation treatment effects are the marginal effects at the means from an ordered Probit model. All remaining results are estimated by OLS. Where the number of observations differs from the number of students, this is because each student receives a behavior mark for each class as well as each of the four grading periods. By-class attendance is an end-of-semester measure given for each class a student takes. The data for these regressions are drawn from administrative records. Final exam scores could not be obtained for the sixth grade. Standard errors clustered by student are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table A.13: Middle School Family Survey Outcomes

Dependent Variable	Treatment	Standard Error	N
	Contact from the Schools		
School Contact to parent	1.445***	(0.350)	80
Contacted more than once	0.308***	(0.111)	80
	How Parents Used the Information		
Privileges taken last month	0.260	(0.320)	79
Talk about college	-0.548	(1.325)	82
Ask about homework	-4.532	(2.745)	81
Help with homework	-0.186*	(0.111)	65
	How Students responded		
Tutoring	0.845	(1.500)	65
HW last minute	-0.001	(0.120)	64
Study hours	-0.267	(0.322)	60
Grades important	-0.167	(0.190)	65
College important	-0.152	(0.135)	65
	Information Problems and Information Demand		
Information problem?	0.100	(0.089)	80
Contacted School	-0.460	(0.601)	81
Can help	0.060	(0.077)	82

The dependent variables in these OLS regressions are from parent and student surveys. Additional controls in these regressions are baseline GPA, grade-level indicators and strata indicators. These results are for families of middle school students only, where the experiment was contaminated. Robust standard errors are in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$