Intervening through Influential Third Parties:

Reducing Student Absences at Scale via Parents

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Short Title: Reducing Student Absences at Scale

125-character summary: Correcting the biased beliefs of influential third parties can motivate them to reduce student absenteeism (RCT N=28,080)

For Supplemental Online Materials, see here:
https://www.dropbox.com/s/tbm8hug7tf4cn7k/SOM.all.blinded.docx?dl=0

ABSTRACT

Most behavioral interventions provide information directly to targeted individuals. We examine an information intervention that instead encourages influential third parties to affect targeted individuals' behavior. Influential third parties are common (e.g., managers, advisors, spouses). Our interventions encourage parents to reduce their students' absenteeism, and study which biased parent beliefs prevent them from exerting this influence. We report the first randomized experiment examining interventions targeting student absenteeism (N=28,080). Parents of high-risk, K-12 students received one of three yearlong regimes of personalized information. The most effective regime reduced chronic absenteeism by 10% across all grade-levels, partly by correcting parents' biased beliefs about their students' total absences. We observe that effects spill over to other students in target students' households. Correcting parents' biased beliefs about how their students' absences compare to their classmates' absences had no impact, a finding that is inconsistent with research on social conformity. This intervention is easy to scale and is dramatically more cost effective than current best practices.

Behavioral research has uncovered a wide array of scalable and cost-effective information interventions. The vast majority of these interventions provide information directly to the targeted individual (e.g., information about your own energy use). We report a large-scale field experiment testing an intervention that instead provides information to a third party who is influential to the target individual. Influential third parties are common. They exist in workplaces (e.g., managers can influence employees), in healthcare (e.g., doctors can influence patients), in personal finance (e.g., financial advisors can influence investors), and within households (e.g., spouses can influence each other).

We focus on the influential relationship parents[1] have with their children, and study the extent to which two specific biased parent beliefs prevent them from influencing their students' absenteeism. We find that correcting parents' biased beliefs about how many total absences their students have accumulated strongly impacts their students' subsequent absenteeism. However, we find that correcting parents' biased beliefs about how their students' total absences compare to those of their students' classmates does not appreciable impact their students' subsequent absenteeism. This result is inconsistent with a vast body of published research on people's motivation to conform to the typical behavior of others.

A diverse array of information interventions has been found to change behavior. Interventions that provide households with information comparing their energy or water usage to that of their neighbors can change the consumption of these resources (*1, 2, 3*). Interventions that provide senior citizens with price information about multiple prescription drug insurance options can improve the efficiency of insurance plan selection (*4*). Interventions that notify inattentive cell phone subscribers with timely information when they have exceeded their allotted usage changes subscribers' phone behavior (*5*). Interventions that provide parents with information about school quality can influence which schools parents choose to send their students to – and consequently how well their students perform on standardized tests (*6*). Interventions that provide parents with timely information about how their students are doing in their classes can lead to improved student performance (*7, 8*). Finally, one of the relatively few interventions focused on intervening through influential others examined bullying in schools. It found that training students in relatively central social networks position can reduce bullying behaviors among the targets and those with whom they are socially connected (*9*).

[1] We use the term "parent" to represent caregivers who are students' legal guardians, recognizing the diversity of family structures.

Most of these interventions do not disentangle the relative behavioral impacts of changing biased beliefs versus increasing the cognitive accessibility of a behavior. Here, we explicitly focus on changing biased beliefs. Of course, not all information interventions result in changed behavior. Sometimes biased beliefs cannot be corrected (*10*) and sometimes correcting biased beliefs does not necessarily affect the behaviors presumably linked to those beliefs (*11*). For example, information aimed at correcting parents' mistaken belief that vaccinations cause autism succeeded in correcting the belief, but did not increase motivation to vaccinate their students (*12*).

We report the first randomized controlled experiment examining an intervention aimed at reducing student absenteeism. Student absenteeism in the United States is astonishingly high. Among US public school students, over 10 percent—roughly 5 million students—are chronically absent each year (defined as missing 18 or more days of school) (*13*). The rate triples in low-income, urban districts. Chronic absenteeism matters. For students, absences robustly predict academic performance (*14, 15, 16*), high school graduation (*17*), drug and alcohol use (*18*), and criminality (*19, 20*). For schools and districts, student absenteeism is often a key performance metric, and, in many states, is tied directly to school funding (*21*). Policymakers have recently redoubled their efforts to reduce absences, such as in the newly enacted *Every Student Succeeds Act* (*22*) and in a recent Obama Administration initiative that aims to reduce chronic absenteeism by ten percent each year (*23*). Meeting goals like this, however, will be challenging. Existing best practices, such as assigning students school-based mentors or social workers, are difficult to scale (*24*).

Our intervention focuses on two biased beliefs held by parents of relatively high-absence students: beliefs about *total absences* and about *relative absences*. First, parents severely underestimate their students' *total absences*. A pilot survey of parents of high-absence students in our partner school district shows that parents underestimate their own students' absences by a factor of 2 (9.6 estimated absences vs. 17.8 actual absences). We find that providing *total absences* information corrects parents' biased beliefs, and nearly doubles the absence-reducing impact of reminders about the important of absences.

Parents' beliefs about their students' total absences may be inaccurate because bounded attention can make it challenging to sustain the attention needed to keep an accurate running tally for an entire school year (*25, 26*). This may create enough uncertainty that parents who are motivated to hold favorable views about their students may come to believe that their students have missed far fewer days of school than they actually have. Since students can be central to people's own identities, this self-benefiting bias would be consistent with people's tendency to think positively about themselves (i.e., "self-enhancement motive"; *27*).

Logically, correcting parents' *total absences* bias will not necessarily lead to increased parent motivation to reduce student absences. The motivational effect of correcting this bias will depend on parents' belief about whether the marginal cost to students of additional absences is increasing or decreasing. For example, consider a parent who incorrectly believed that her student had accumulated 8 absences and the intervention corrected her belief so that she now believes her student has accumulated 16 absences. If correcting this belief motivated the parent to reduce her students' absences, then it may suggest that the parent believes that the educational consequence of what would have been the student's 9th absence is less than the educational consequence of what would be the student's 17th absence. We report a simple survey experiment suggesting that parents do, in fact, believe that there are increasing marginal educational costs of incremental absences.

We also examine the impact of parents of relatively high-absence students' biased belief about their students' *relative absences* compared to those of their students' classmates. In the same pilot survey we refer to above, only 28% of parents with higher-than-average-absence students accurately reported that their students had missed more school than their classmates. Just as the residents in Garrison Keillor's fictional town of Lake Wobegon believed that "…all the students are above average," parents seem to hold this same overconfidence regarding how their students compare to their peers (*28*).

Information interventions conveying relative social comparisons have been used to induce conformity across a wide range of policy relevant domains. Such interventions have been shown to influence charitable giving (*29, 30*), water and resource conservation (*31, 32, 33*), energy conservation (*1, 34, 2*), job selection (*35*) and motivation to vote in elections (*36, 37*). We find that providing relative absences information corrects parents' biased beliefs, but has no appreciable impact on student absences. This result is inconsistent with the robust evidence across policy domains showing that relative information interventions result in increased conformity (e.g., *38*).

Practically, the intervention approach we report is extremely cost-effective. It costs around $6 per additional day of student attendance it generates—which is more than an order of magnitude more cost-effective than the current best-practice intervention of deploying absence-focused social workers and mentors (*24*). It is also unusually easy to scale with fidelity (*39*).

DATA AND EXPERIMENTAL DESIGN

Pilot study

We conducted a pilot study in the spring prior to the launch of the main experiment. In brief, the pilot study assessed two main questions. First, does sending mailings to parents regarding their students' total absences indeed decrease absenteeism? Second, does including the absences of the typical student (relative comparison) lead to a greater decrease in absenteeism? We tested these questions by randomly assigning 3,007 households in the School District of Philadelphia to one of three experimental conditions: *Total Absences*, *Relative Absences*, and *Control*. Those assigned to *Total Absences* and *Relative Absences* received two rounds of mail treatments in the spring 2014 semester. Both treatments reduced the number of absences by about 0.7 days (6% relative to control) over a 14-week period. While both treatment conditions were significantly different from control, we were not able to distinguish whether the effect on those in the *Total Absences* and *Relative Absences* conditions differed. See SOM for additional details.

Setting

We conducted our experiment in partnership with the School District of Philadelphia (SDP), the eighth-largest school district in the United States. At the time of the experiment, SDP had more than 130,000 students enrolled. The student population is racially diverse: 53% of enrolled students are Black/African American, 19% are Hispanic/Latino, and 14% are White/Caucasian (as of the 2013-2014 school year). Almost three-quarters of all SDP students qualify for Free or Reduced Price Lunch, and a third of all students in Philadelphia live in households below the Federal Poverty Line, making it the poorest major city in the United States. SDP has a budget of over $13,000 per student per year. Finally, 58% of all students scored "Below Basic" on the 2014-2015 Math Pennsylvanian System of School Assessment (PSSA) exams.

Data

Our main analysis sample consists of 28,080 households across 203 schools. Households were included in the experiment if their students were enrolled in non-charter, non-specialized schools, were not included in the pilot study of this experiment, were not flagged as homeless or with an Individual Education Plan, did not have a home language other than that of the mailed consent form, did not have perfect attendance in 2014-2015 school year, did not have inordinately high levels of absences (2 standard deviations above the mean), and did not have more than seven eligible students in the same household (see SOM, Table S1). In households with multiple qualifying students (19%), we randomly selected one student to be the target student. Finally, we excluded 1% of students who transferred outside the district during the experiment (i.e., a complete-case analysis), since attrition rates were very low and did not differ across conditions (χ^2 p=0.75). The final student sample is 53% African American, 20% Hispanic, 52% female, 28% in high school, and 74% free or reduced-price lunch qualified. See SOM.

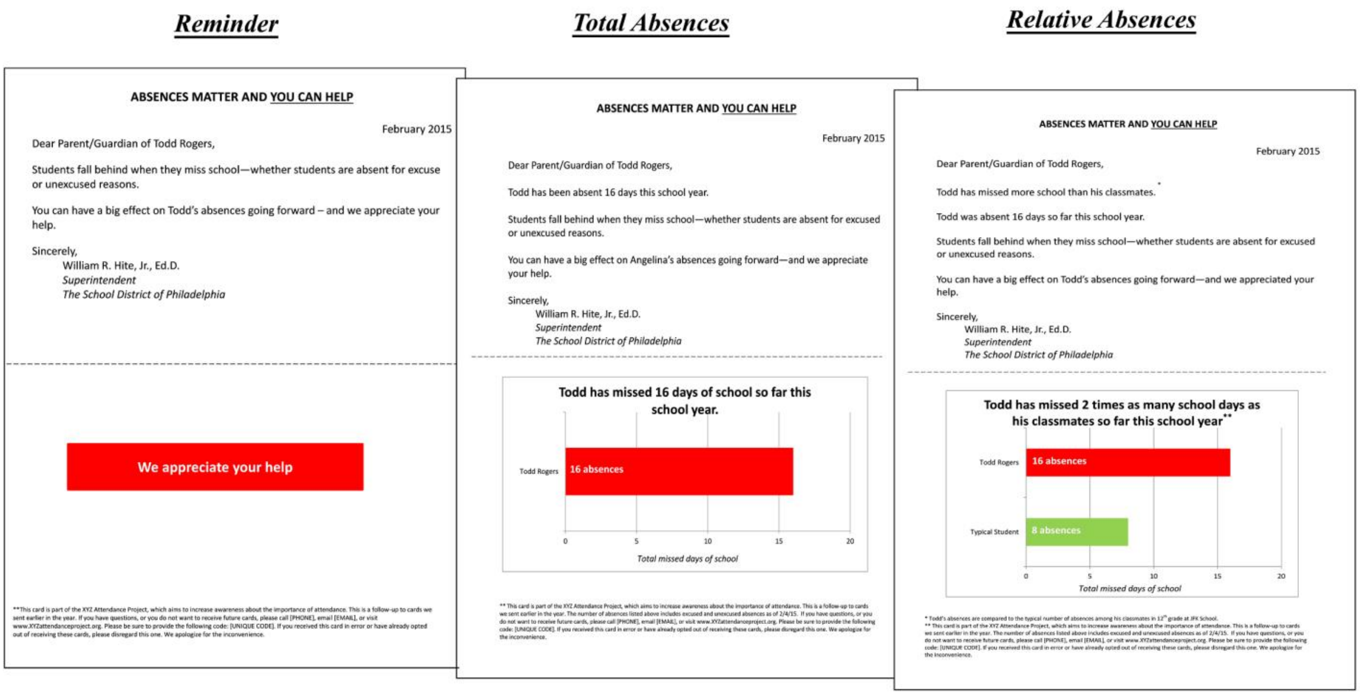
We obtained all of our student- and household-level data from school district administrative records. The primary outcome is total number of absences from the date of the first mailing through the end of the school year. This outcome includes both excused and unexcused absences; the results are consistent examining these outcomes separately. As discussed in the SOM, secondary outcomes include standardized test scores and number of tardies. We use the following demographic control variables: student gender, whether the student has Low English Proficiency (LEP), speaks English as the primary home language, is eligible for Free and Reduced Price Lunch, or is Black/African American. We also control for the number of days absent in the prior school year and in the current school year prior to randomization. Finally, we control for school and grade (i.e., fixed effects) unless otherwise stated. As a practical matter, the data quality is excellent overall, with minimal missingness. We address this and sample attrition in more detail in the SOM.

Experimental design

We randomly assigned households in equal numbers to a control group or to one of three treatment regimes, with randomization stratified by school, grade, and prior-year absences (see SOM). Random assignment was balanced across covariates (see SOM).

Households assigned to control received no additional contact beyond normal school communications (e.g., report cards, school announcements, parent-teacher conferences; see SOM). Households assigned to treatment received up to five rounds of treatment mail throughout the school year. All treatments within each round were sent on the same day and have the same overall appearance; the treatments differed only in their content, with each successive treatment adding an additional piece of information. See Figure 1. Treatments in the *Reminder* regime reminded parents of the importance of absences and of their ability to influence them. Treatments in the *Total Absences* regime added information about students' total absences. Treatments in the *Relative Absences* regime further added information about the modal number of absences among target students' classmates. Data reported in the first treatment, mailed 10/2014, reflected absences from the previous school year. Data reported in the remaining treatments, mailed 1/2015–5/2015, reflected current-year absences. The total cost of the treatment was around $6.60 per household for production and labor.

Figure 1.



Not all parents assigned to the treatment regimes received all of the five treatment mailings. First, we were unable to send treatments to parents who moved during the school year without leaving valid forwarding information. Second, when student absences were especially low – either overall or compared to their classmates – parents received the most informative treatment the district permitted for that round (see SOM). On average, we sent treatment regime households 4.2 mailings over the school year (*Reminder*=4.24; *Total Absences*=4.21; *Relative Absences*=4.18). As we discuss next, we therefore base our analysis on random assignment to treatment regime (i.e., Intent-to-Treat), rather than on treatment rounds received.

Experimental analysis protocol

Prior to obtaining any information on outcomes, we registered a detailed pre-analysis plan (#AEARCTR-0000829, www.socialscienceregistry.org). The SOM provides extensive details on the analysis methods. We assess the impact of random assignment on student attendance in two ways. First, we use Fisher Randomization Tests (FRT) to obtain exact *p*-values for the sharp null hypothesis of no impact (*40*). This is a non-parametric approach that is fully justified by the randomization itself. Second, we use linear regression to estimate the Average Treatment Effects (ATE) of random assignment to each treatment regime, with covariate adjustment for student-level demographics and prior absences as well as the student's school and grade. The SOM provides additional details on the procedure for multiple test correction.

Research Questions

This experiment evaluates the effectiveness of using parental engagement to improve student attendance. We address three main research questions:

RQ1: Does contacting guardians and encouraging them to improve their students' attendance reduce absences?

RQ2: Does communicating to guardians the total number of days their student missed reduce absences?

RQ3: Does communicating to guardians the total number of days their student missed *as compared to the absences of a typical student* reduce absences?

We also address these exploratory research questions:

RQ4: Do these interventions impact the attendance of other students in the household not explicitly mentioned in the mailings?

RQ5: Do the treatment effects differ for students in early grade-levels (K-5) compared to later grade-levels (6-12)?

Survey design and analysis plan

At the end of the school year, between 6/20/2015 – 6/25/2015, we surveyed parents to assess whether treatment regimes also affected parent beliefs (survey N=1,268; AAPOR Response Rate 2 of 23.0%). The survey had two primary purposes:

(1) *Internal Validity and Manipulation Checks* - A set of questions address whether the guardians received, read, and understood the mail.
(2) *Impact on Parental Beliefs* – How did the mail pieces impact parental beliefs about the importance of attendance and their role in ensuring their students get to school?

A secondary purpose of the survey was to assess the impact of the treatments on parental behavior. Because we surveyed a minority of the experiment universe, the responses are informative of the mechanisms underlying the experimental treatment effects, but are not conclusive evidence of the mechanisms. The full survey and the survey analysis plan are included in the SOM.

Results on student outcomes

Random assignment to treatment significantly reduced student absences relative to the Control group (joint FRT p<0.001). Students in the Control group were absent 17.0 days on average (all means regression-adjusted; SE=0.1 days); students in the *Reminder* regime were absent 16.4 days on average (SE=0.1 days); students in the *Total Absences* regime were absent 16.0 days on average (SE=0.1 days); and students in the *Relative Absences* regime were absent 15.9 days on average (SE=0.1 days). Therefore, the ATE for the *Reminder* regime relative to the Control group is -0.6 days (FRT p<0.001). Adding absolute absences information nearly doubled this effect: the ATE for the *Total Absences* regime relative to the Control group is -1.1 days (FRT p<0.001; ATE=-0.4 days relative to the *Reminder* regime, FRT p<0.001). However, adding relative absences information did not affect student absences: absences among those in the *Relative Absences* regime were nearly identical to those in the *Total Absences* regime (ATE=0.0 days compared to *Total Absences*, FRT p=0.19). See Figure 2. We find a similar pattern for chronic absenteeism: 36.0% of students in the Control group are chronically absent (SE=0.5pp), compared to 33.0% in the *Reminder* regime (SE=0.5pp; ATE=-8.4%), 32.4% in the *Total Absences* regime (SE=0.5pp; ATE=-10.0%), and 31.9% in the *Relative Absences* regime (SE=0.5pp; ATE=-11.5%).

We used the fact that the focal student was randomly assigned to assess spillover in households with two or more qualifying students (N=5,185). Among non-focal students in households in the *Reminder* regime, there was no evidence of spillover effects (ATE=0.0 days; SE=0.4 days). Among non-focal students in households in the *Total Absences* and *Relative Absences* regimes, spillover effects were nearly as large as the effects for focal students (*Total Absences:* ATE=-1.0 days, SE=0.4 days; *Relative Absences*: ATE=-1.0 days, SE=0.4 days).

Daily attendance data allowed us to examine the impact over time. Across all three treatment regimes, the impact was roughly twice as large in the week immediately following delivery of each treatment round compared to the two subsequent weeks (*Reminder:* 0.14 v. 0.07 days/week, p=0.006; *Total Absences:* 0.14 v. 0.05 days/week, p<0.001; *Relative Absences:* 0.17 v. 0.11 days/week, p=0.015;

all comparisons versus Control). This action-and-backsliding pattern is similar to that observed in other repeated, personalized interventions (*2*).

We found no evidence of meaningful treatment effect variation by student grade-level. This suggests that the treatment effect does not result from informing parents that their students have been cutting school. After all, 18 year-old seniors in high school are far more likely to covertly cut school than 7 year-old first graders, yet both age groups show comparable effect sizes. We found no evidence of meaningful treatment effect variation by gender, race, or by total absences in the previous school year. As discussed in the SOM, however, we find meaningful variation in quantile treatment effects. This approach compares a given quantile for students assigned to control (e.g., the median) with the corresponding quantile of students assigned to treatment (in this case, we pool treatment regimes). In particular, we find a quantile treatment effect of around 1 day at the median of each group (around three weeks absent for students in *Control*) compared to a quantile treatment effects of around 0.5 days at the 10th percentile by absences of each group (around one week absent for students in *Control*). Estimates at much higher quantiles are highly imprecise. These results suggest that there is indeed meaningful treatment effect heterogeneity not captured by pre-treatment covariates.

Finally, we found no significant effect on end-of-year standardized test scores for students in grades 4 through 8 (for pooled treatments, Math ATE=-0.001 SD, SE=0.012 SD; Reading ATE=-0.015 SD, SE=0.012 SD). For this group, the pooled impact on attendance through the test date was 0.6 days (SE=0.1 days). The pre-registered analysis plan anticipated this null effect. The minimum detectable effect on test scores would was roughly 0.03 standard deviations. To put this in context, the average annual gain in effect sizes for grades 4 to 8 on nationally normed tests is around 0.3 standard deviations (*41*). Thus, the minimal detectable effect corresponds to roughly three weeks of additional school---approximately 30 times larger than the attendance effect we observe prior to test day. As a result, the null effect is not surprising.

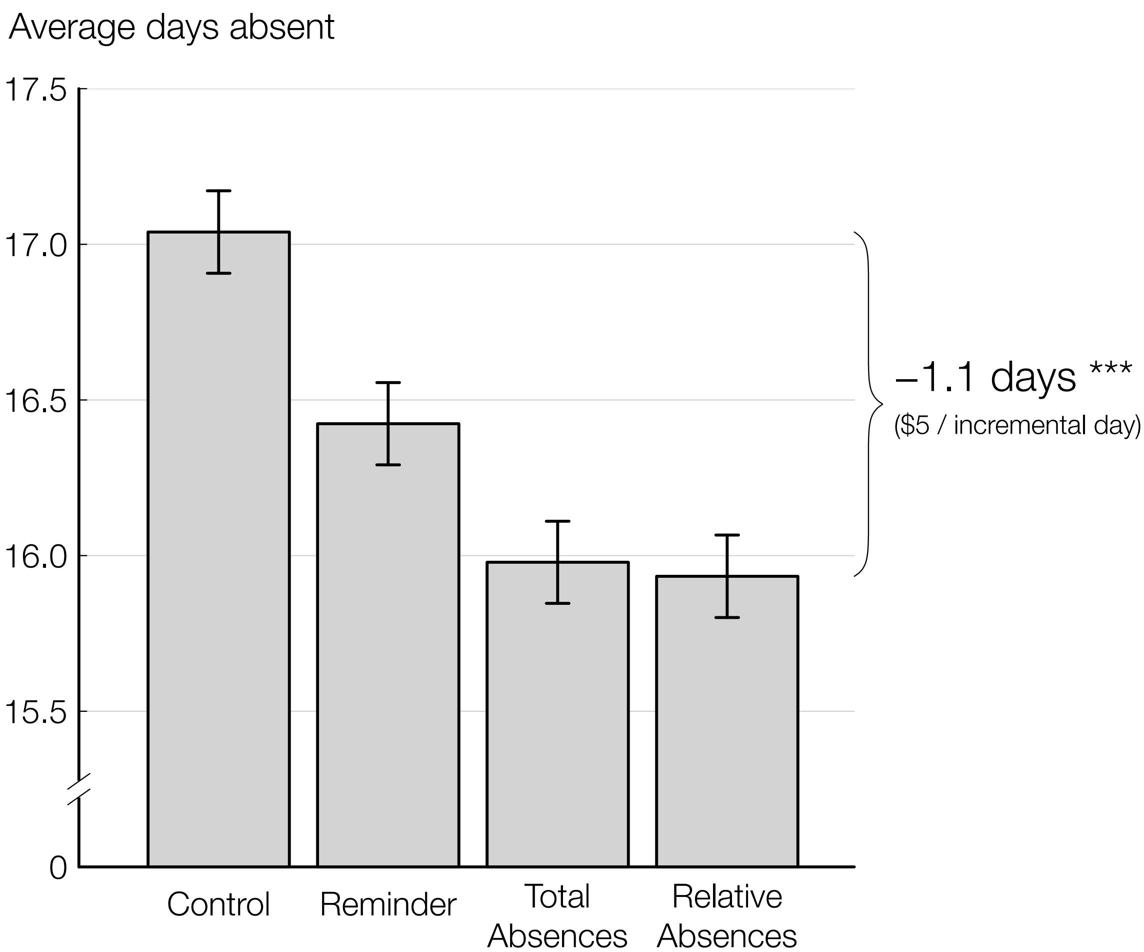


Figure 2. Absences by treatment regime. Regression-adjusted means and standard errors; error bars +/-1 SE; joint FRT p-value for the null hypothesis of no impact is p<0.001.

Results on correcting parents' biased beliefs

The survey confirmed that parents actually received and remembered the treatments: 57% (SE=2pp) in the three treatment regimes recalled receiving the treatments compared to 26% (SE=3pp) in Control (p<0.001). The survey also showed that the *Reminder* regime did not change parents' reports of the importance of absences or parents' role in reducing absences. This suggests that the *Reminder* treatments primarily focused parents' attention on absences (*42*), but did not affect their relevant beliefs; parents' attitudes about attendance across seven questions did not differ across conditions (F-test p=0.48).

We then examined whether informing parents of their students' total number of absences corrected parents' biased beliefs about these absences. Parents' total absences bias was calculated as the difference between parents' self-reported absences and their students' actual absences (this pattern holds across different measures as well). See Figure 3. Informing parents of their students' total absences indeed corrects this bias: parents in Control and the *Reminder* regime under-reported their students' absences by 6.1 days (SE=0.6 days), roughly 50% more than parents in the *Total Absences* and *Relative Absences* regimes (2.8 days; SE=0.6 days; ATE=-3.2; SE=0.9). Adding total absences information to the treatments reduced parents' biased beliefs and reduced absences, suggesting that parents' total absences bias inhibits them from reducing actual student absences. Adding total absences information may have also increased the amount of attention people devoted to the treatments, amplifying the cognitive accessibility and perceived importance of student absences. Though we cannot fully rule out that interpretation, we note that the change in parent beliefs is aligned with the proposed parent belief mechanism.

Finally, we assessed whether providing parents with information about typical absences corrected parents' biased beliefs about their students' relative absences. Relative absences bias was calculated by asking parents whether their students were absent "more," "about the same," or "fewer" days than their students' typical classmates (this pattern holds across different measures). Among parents of students in *Control*, the *Reminder* regime, and the *Total Absences* regime, 9.2% (SE=1pp) responded correctly, compared to 16.2% (SE=2pp) among parents of students in the *Relative Absences* regime [ATE=7.1pp, p=0.001]. See Figure 3. Adding relative absences information to the treatments corrected parents' relative absences bias, but did not affect actual student absences. This suggests that parents' biased beliefs about their students' relative absences does not inhibit parents from reducing actual student absences.

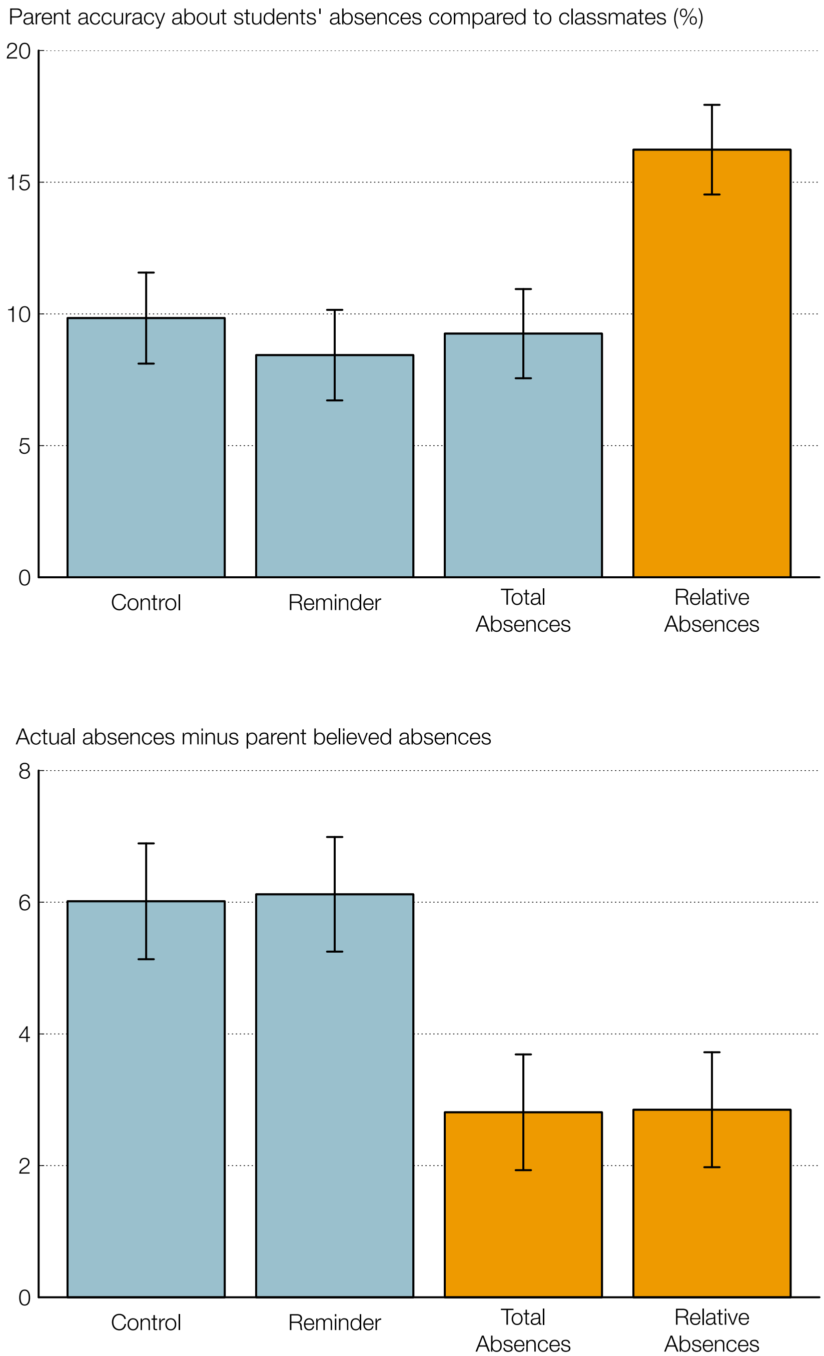


Figure 3. Treatments corrected parents' biased beliefs. Regression-adjusted means and standard errors based on end-of-experiment survey responses; error bars +/-1 SE; orange bars represent treatment regimes that included the relevant information

Discussion

This experiment (N=28,080 households) makes four primary contributions. First, it illustrates the power of information interventions that encourage influential others to change the behavior of targeted individuals. Second, it shows that correcting parents' biased belief about how many total absences their students have accumulated *causes* parents to reduce student absences. Third, it shows that correcting parents' biased belief about how their students' absences compare to their students' classmates' absences causes no appreciable change in student absences. Finally, it develops and evaluates an extremely cost-effective and scalable intervention that addresses a critical social problem.

The fact that correcting parents' total absences bias caused parents to reduce student absences suggests that parents believe that there are increasing repercussions for every additional day of school missed; in other words, parents appear to believe that the marginal educational cost of absences is increasing. We conducted an online survey experiment to examine this further. Parents of students in grades kindergarten through twelfth grade recruited on Amazon's Mechanical Turk (N=255) were randomly assigned to one of two conditions. Half were asked to imagine that their student had been absent six days as of about half way through the school year, and the other half were asked to imagine that their student had been absent twelve days as of halfway through the school year. They were all asked "*How much would being absent from school tomorrow affect your child's success in school this school year?"* Parents who imagined that their student had accumulated relatively many absences reported that being absent tomorrow would more negatively affect their student's success than did parents who imagined that their student had accumulated relatively few absences, $t(253) = -4.33$, $p=.002$. (See SOM). This provides additional support for the educational production function interpretation of the *Total Absences* result.

It is surprising that the social comparison information did not reduce student absences given the impact of social comparisons in other domains. The intervention itself was modeled after the robust and widely studied OPOWER home energy report intervention (*2*). There are many possible reasons that correcting relative absences bias did not result in reduced absences. For example, perhaps the average gap between students' actual absences and their peers' absences was so large that it discouraged parents (e.g., *43*). Across all rounds of treatment in the *Relative Absences* regime, the average ratio of own-student absences to comparison-student absences was 5 to 1. It is conceivable that this gap seemed insurmountable and so discouraged parents. Or, perhaps relative comparisons tend to be less motivating in domains that are especially identity-central (e.g., parental support of education) because they elicit especially strong counter-arguing and rationalization. We hope future research will help explain why correcting biased beliefs about relative absences did not motivate parents to reduce absences in this experiment.

The treatment effects were about as large on other students living in the targeted households as they were on the focal students. This suggests that analyses of household-level interventions that do not incorporate intra-household spillover effects dramatically underestimate intervention cost effectiveness (e.g., *44*). It could be that this spillover arose from students directly influencing other students within their households, or from the interventions motivating parents to influence the absenteeism of all students within their households. We cannot determine the mechanism from the current study, though follow up research could tease these apart.

This research examined absenteeism among students, though absenteeism is an important challenge for most organizations. Employee absenteeism in the US is estimated to cost organizations $202 billion each year (*45*). Undoubtedly, the specific targeting and content of absence-reducing interventions will differ when directed at employee absences (see *46*), though the research we report could provide useful insight for a program of work on that topic.

Missing school negatively affects student, school, and district success. The intervention reported here is both highly scalable and extremely cost-effective at reducing at-risk students' absences, costing ~$5 per incremental school day generated. Current best practices like absence-focused social workers and mentors can cost over $120 per incremental school day generated (see SOM). Nonetheless, this mail-based intervention is not a substitute for these more intensive approaches that address the deep personal and structural challenges facing students, families, and communities – after all, this intervention reduces chronic absenteeism around 10%. No single intervention is a panacea, rather system-level change will require many such interventions woven together. By harnessing the intervention we report, schools can better target educational resources and personnel toward difficult absenteeism challenges that require more active and personal involvement. Parents of low-income and minority students are often seen as a contributing cause of student failure (*47, 48*). As we see it, this "deficit" view of parents hinders educational innovation, especially for K-12 students. An "asset" view of parents instead unlocks new interventions that empower parents as partners in improving student outcomes (*49*, 7).

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