Reducing Student Absences at Scale by Targeting Parents' Misbeliefs

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Student attendance is critical to educational success, and is increasingly the focus of educators, researchers, and policymakers. We report the results of a randomized experiment examining interventions targeting student absenteeism. Parents of 28,080 high-risk Kindergarten through 12th grade students received one of three personalized information treatments repeatedly throughout the school year or received no additional communication (control). The most effective versions reduced chronic absenteeism by 10% or more, partly by correcting parents' biased beliefs about their students' total accumulated absences. The intervention reduced student absences comparably across grade levels, and reduced absences among untreated cohabiting students in treated households. This intervention is easy to scale and is more than one order of magnitude more cost effective than current absence-reduction best practices. Educational interventions that inform and empower parents, like those reported here, can complement more intensive student-focused absenteeism interventions.

Student absenteeism in the United States is astonishingly high. Among US public school students, over 10% are chronically absent each year (defined as missing 18 or more days of school)[1,2]. The rates are even higher in low-income, urban districts[3]. The consequences of chronic absenteeism are significant. For students, absences robustly predict academic performance[2,4,5,6,7], high school graduation[8,9], drug and alcohol use[10], criminality[11,12], and risk of later life adverse outcomes[13]. For schools and districts, student absenteeism is often a key performance metric, and, in many states, absenteeism is tied directly to performance evaluations and funding[14]. Policymakers have recently redoubled their efforts to reduce absences, such as in the Every Student Succeeds Act[15] and in an Obama Administration initiative that aimed to reduce chronic absenteeism by 10% each year[16]. Meeting goals like these, however, is challenging. Existing best practices, such as assigning students mentors or social workers, have limited effect, can be difficult to scale, and can be expensive. A randomized experimental evaluation of a mentor program in a population similar to the one studied in this paper estimated that the program reduced absenteeism at a cost of around $500 per incremental day of attendance generated[17].

This manuscript reports results from a large-scale randomized experiment evaluating an intervention that reduces student absenteeism[18]. The intervention delivered personalized information through repeated rounds of mail-based messaging targeting key misbeliefs held by parents of at-risk students (N=28,080). The most effective treatment arm reduced total absences by 6% and the fraction of students who were chronically absent by over 10% relative to a control group. The approach is extremely cost-effective, costing around $6 per additional day of student attendance generated — more than one order of magnitude more cost-effective than one current best-practice intervention[18]. A key feature of the scalability of this intervention is that it is also particularly easy to implement with fidelity in other school districts[19]. We find that the intervention reduced student absences comparably across all grade levels, and reduced absences among untreated cohabiting students in treated households.

The intervention targets two biased beliefs held by parents of high-absence students: beliefs about total absences and beliefs about relative absences. First, parents severely underestimate their children's total absences (total absences bias). 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 two (9.6 estimated absences vs. 17.8 actual absences). Our experiment finds that providing total absences information as part of the treatment reduces parents' biased total absences belief, and nearly doubles the absence-reducing impact compared to similar treatment regimes that lack the total absences information. Second, parents are severely miscalibrated about how their children's absences compare to those of their classmates (relative absences bias). In the same pilot survey, only 28% of parents whose students have higher-than-average absences accurately reported that their students had missed more school than their classmates. Our experiment finds that providing relative absences information as part of the treatment reduces parents' biased relative absences belief. Providing this information in the way that we did, however, has no appreciable impact on student absences compared to similar messages without this information.

Parents' beliefs about their children's total absences may be inaccurate because bounded attention can make it challenging to sustain over time the attention needed to keep an accurate running tally of absences for an entire school year[20,21]. This may lead to parents being uncertain about their children's summative total absences. Amidst this uncertainty parents may believe that their students have missed far fewer total days of school than they actually have because they are motivated to hold favorable views about their students. Since children can be central to parents' own identities, biased total absences beliefs may benefit parents by allowing them to think more positively about themselves (i.e., "self-enhancement motive")[22]. Logically, correcting parents' biased beliefs about total absences 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. At the end of this paper, we report a simple survey experiment suggesting that parents do, in fact, believe that there are increasing marginal educational costs of incremental absences.

Parents' beliefs about their children's relative absences may be inaccurate because they have little direct exposure to when their children's peers are absent, and also because of parents' self-enhancement motives. Research has found that parents display a similar overconfidence as the residents in Garrison Keillor's fictional town of Lake Wobegon in believing that "...all the students are above average"[23]. Our pilot survey suggests that this Lake Wobegon effect applies also to parents' beliefs about how their children's attendance compares to that of their peers[23].

Informing parents about their children's total absences could be thought of as a form of personalized information intervention. Other interventions delivering personalized information have had sizable impacts on consequential behaviors. For example, providing senior citizens with price information about multiple prescription drug insurance options for which they are eligible can improve the efficiency of insurance plan selection[24]. Notifying inattentive cell phone subscribers with timely personalized information when they have exceeded their allotted usage changes subscribers' phone usage behavior[25]. Parents are particularly potent targets for interventions that communicate personalized information developed to change student behavior (like the total absences messaging) for several reasons: parents are active investors in their children's human capital[26], they can allocate rewards and punishments to students[27], and there is evidence that they have incomplete knowledge and bargaining power with respect to their children's human capital investments[28]. In line with this reasoning, several information interventions targeting parents have been found to change student behaviors. Mailing information about school quality to parents influences which schools students enroll in and how well they subsequently perform academically[29]. Recently, a range of personalized information interventions have found that providing parents with timely information about their students' behaviors and performance in school can increase student performance[30,31,32].

Informing parents about their students' relative absences has less clear normative implications for affecting their students' subsequent absences. However, research across a wide range of policy areas has found that the disclosure of social comparison information regarding consequential behaviors can result in conformity. This has been observed for charitable giving[33,34], water and resource conservation[35,36,37], energy conservation[38,39,40], job selection[41] and motivation to participate in elections[42,43]. At the end of this paper, we speculate as to why increasing parental accuracy regarding relative absences by adding this information to the treatments does not result in the conformity this previous research might predict.

We conducted our experiment in partnership with the School District of Philadelphia (SDP), the eighth-largest school district in the United States. 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 children's 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.

The following year, we conducted our main experiment with an eligible population of 161,922 students across the entire district. 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 Level, 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.

In the summer preceding the 2014-2015 School Year (SY), we sent opt-out consent forms to the parents of all eligible students. After relevant exclusions (see "Methods"), we were left with 40,326 eligible students in 32,437 households in our experiment universe. We randomly assigned households in equal numbers to a control group or to one of three treatment conditions, with randomization stratified by school, grade, and prior-year absences (see SOM). Random assignment was balanced across covariates (see SOM).

Households assigned to the control group 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 treatments associated with each condition adding an additional piece of information. See Figure 1. *Reminder* treatments reminded parents of the importance of absences and of their ability to influence them. *Total Absences* treatments added information about students' total absences. *Relative Absences* treatments further added information about the modal number of absences among focal students' classmates. Data reported in the first treatment, mailed in October 2014, reflected absences from the previous school year. Data reported in the remaining treatments, mailed January 2015 — May 2015, reflected current-year absences. The total cost of the treatment was around \$6.60 per household for production and labor (see SOM).

Not all parents assigned to the treatment conditions received all of the five treatment mailings, or the same regime of mailings over the course of the year. 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 condition 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 based our analysis on random assignment to treatment condition (i.e., Intent-to-Treat), rather than the number of treatment rounds received.

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

1. Does contacting guardians and encouraging them to improve their child's attendance reduce absences?

2. Does communicating to guardians the total number of days their child missed reduce absences?

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

We also address these exploratory research questions:

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

5. Do the treatment effects differ for students in early grade-levels (Kindergarten through 5th grades) compared to later grade-levels (6th through 12th grades)?

At the end of the school year, between June 20, 2015 and June 25, 2015, we surveyed parents to assess whether assignment to treatment condition also affected parent beliefs (survey N=1,268; American Association for Public Opinion Research Response Rate 2 of 23.0%). The survey had two primary purposes. First, we wanted to assess the internal validity of the study and include several manipulation checks. To this end, the survey included a set of questions to address whether the guardians received, read, and understood the mail. Second, we wanted to assess the impact of the treatments in parent beliefs. To this end, the survey included a set of questions about the importance of attendance and parental beliefs about their role in ensuring that their children got 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 experimental 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.

We find that 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* condition were absent 16.4 days on average (SE=0.1 days); students in the *Total Absences* condition were absent 16.0 days on average (SE=0.1 days); and students in the *Relative Absences* condition were absent 15.9 days on average (SE=0.1 days). Therefore, the ATE for the *Reminder* condition relative to the Control group was -0.6 days (FRT $p<0.001$). Adding absolute absences information nearly doubled this effect: the ATE for the *Total Absences* condition relative to the Control group was -1.1 days (FRT $p<0.001$), and relative to the *Reminder* condition it was -0.4 days (FRT $p<0.001$). However, adding relative absences information did not affect student absences: absences among those in the *Relative Absences* condition were nearly identical to those in the *Total Absences* condition (ATE=0.0 days compared to *Total Absences*, FRT $p=0.19$). See Figure 2. We found a similar pattern for chronic absenteeism: 36.0% of students in the Control group were chronically absent (SE=0.5pp), compared to 33.0% in the *Reminder* condition (SE=0.5pp), 32.4% in the *Total Absences* condition (SE=0.5pp), and 31.9% in the *Relative Absences* condition (SE=0.5pp). Thus, compared to students in the Control group, students in the *Reminder* condition were 8% (or 3pp) less likely to be chronically absent, students in the *Total Absences* condition were 10% (or 3.6pp) less likely to be chronically absent, and students in the *Relative Absences* condition were 11% (or 4.1pp) less likely to be chronically absent.

We used the fact that the focal student was randomly assigned to assess spillover in households with two or

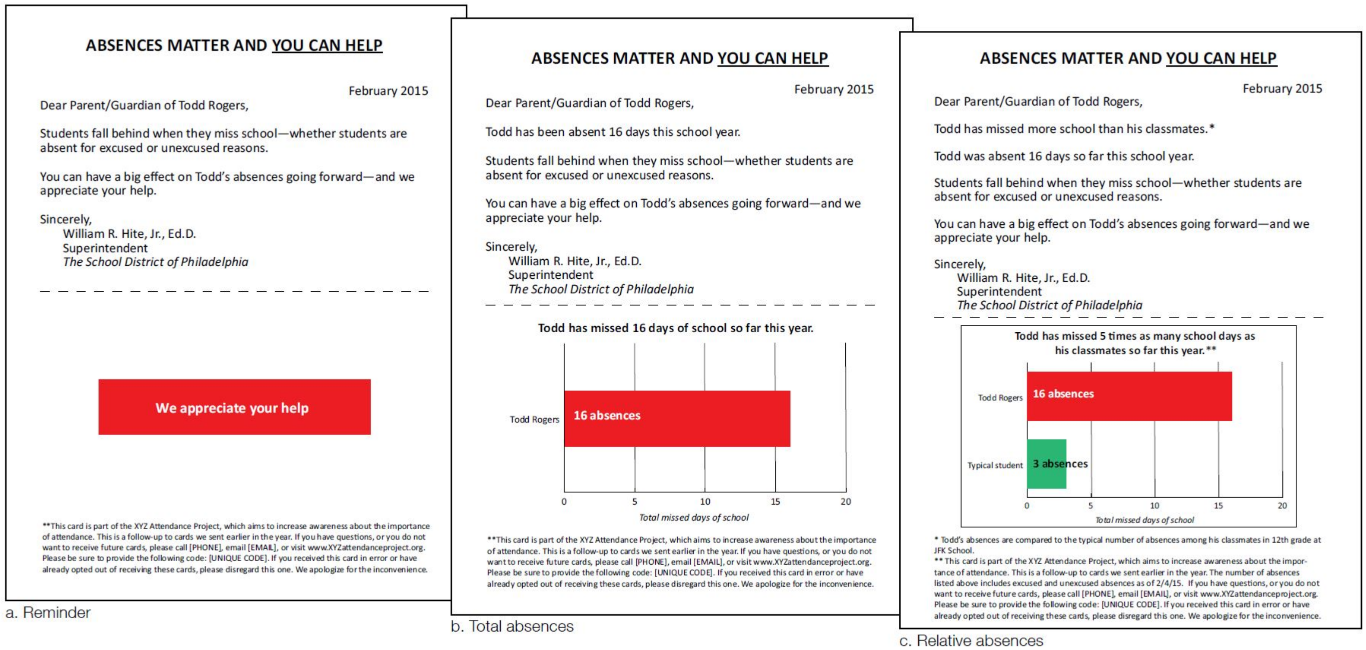

Figure 1. Sample mailings from each treatment condition. The *Reminder* treatment **(a)** reminds parents of the importance of absences and of their ability to influence them. The *Total Absences* treatment **(b)** builds on **(a)** and adds information about students' total absences. The *Relative Absences* treatment **(c)** builds on **(b)** and adds information about the modal number of absences among focal students' classmates. Focal students were randomly assigned to one of these three treatment conditions or a control group (N=6,994 Control; 7,041 Reminder; 7,037 Total Absences; 7,008 Relative Absences). See SOM for more information on the randomization strategy.

more qualifying students (N=5,028 households). Among non-focal students in households assigned to the *Reminder* condition, there was no evidence of spillover effects (ATE=0.0 days; SE=0.4 days). Among non-focal students in households assigned to the *Total Absences* and *Relative Absences* conditions, 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 of the treatments over time. Across all three treatment conditions, we find modest evidence that the impact was larger in the week immediately following delivery of each treatment round compared with the two subsequent weeks (*Reminder*: 0.18 v. 0.09 days/week, p=.05; *Total Absences*: 0.25 v. 0.16 days/week, p=.04; *Relative Absences*: 0.24 v. 0.18 days/week, p=.12; all comparisons versus Control). This action-and-backsliding pattern is similar to that observed in other repeated, personalized interventions[38]. Additionally, across the treatment conditions, the first round of treatment tends to have a non-significantly smaller effect than the average of the subsequent rounds. For example, the first treatment round in the Relative Absences condition reduced absenteeism by 0.22 days, and the average of the remaining treatments in the condition reduced absenteeism by 0.28 days per treatment round (see SOM). Though this is not statistically robust evidence of increasing impact per treatment, it is consistent with an interpretation that effects do not decrease with each successive round of treatment (e.g., that parents do not appear to be habituating to treatments). This suggests that increased frequency of communication to parents about absenteeism may reduce absenteeism even more. Additionally, one intriguing possible mechanism for the lack of habituation to repeated rounds of treatment is that the first treatment may have alerted parents to the possibility that they may be communicated with again during the school year specifically regarding their children's absenteeism. This could create a heightened sense in parents that their children's absenteeism was being carefully monitored and thus generate increased effort to reduce absenteeism[44].

We found no evidence of meaningful treatment effect variation by student grade-level, though statistical power was limited. If such variation was indeed minimal, this suggests that the treatment effect did not result from informing parents that their children had 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 total absences in the previous school year. However, as discussed in the SOM, we found meaningful variation in quantile treatment effects. This approach compared a given quantile for students assigned to the control group (e.g., the median) with the corresponding quantile of students assigned to treatment (in this case, we pooled the treatment conditions). In particular, we found a quantile treatment effect of around 1 day at the median of each group (around 3 weeks absent for students in Control) compared with a quantile treatment effect of around 0.5 days at the 10th percentile by absences of each group (around 1 week absent for students in Control). Estimates at much higher quantiles are imprecise. While inherently exploratory, these results suggest that there is indeed meaningful treatment effect heterogeneity not captured by pre-treatment covariates.

Finally, we explored the impact of the intervention on end-of-year standardized test scores for students in grades 4 through 8. Unfortunately, we were severely statistically under-powered to detect realistic impacts. As a benchmark, several recent studies have tried to quantify the impact of attending an additional school day on test scores. Using within-sibling comparisons with data from SDP, Gottfried (2011)[7] found that an additional day is associated with a roughly 0.01 SD increase in test scores. Using ECLS-K and administrative data from North Carolina, Gershenson et al. (2017)[4] found effects about half this size for comparable populations[6]. For the group for which we have end-of-year test scores, the pooled impact on attendance through the test date was 0.6 days (SE=0.1 days); thus, we would realistically expect test score impacts on the order of 0.001 to 0.006 SDs. The experiment was dramatically underpowered to study effects of this size, with a minimal detectable effect of around 0.03 SDs. Given the importance of standardized test scores, however, we nonetheless assessed the impact on this outcome, pre-registering our concerns about statistical power. Unsurprisingly, 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).

The end-of-year survey confirmed that parents actually received and remembered the treatments: 57% (SE=2pp) in the three treatment conditions recalled receiving the treatments compared to 26% (SE=3pp) in Control ($p<0.001$). The survey also showed that assignment to the *Reminder* condition 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[45], 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 next examined whether informing parents of their children's 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 children's total absences indeed corrects this bias: parents in Control and

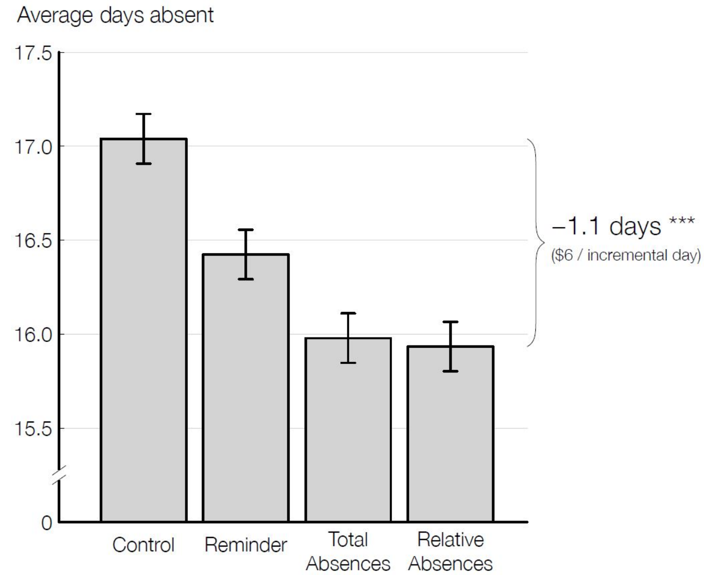

Figure 2. Absences by treatment condition. The average number of absences is compared for each treatment group. Bars show regression-adjusted means with error bars +/-1 SE. The joint FRT p-value for the null hypothesis of no impact is $p<0.001$. The ATE for the *Reminder* condition relative to the Control group is -0.6 days, while the ATE for the *Total Absences* condition relative to the Control group is -1.1 days. Absences among those in the *Relative Absences* condition were nearly identical to those in the *Total Absences* condition. At a cost of around $6 per incremental day of attendance, this makes the *Total Absences* treatment an order of magnitude more cost-effective than current absence-reduction best practices.

the *Reminder* condition under-reported their children's absences by 6.1 days (SE=0.6 days), roughly 50% more than parents in the *Total Absences* and *Relative Absences* conditions (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 children's relative absences. Parents' relative absences bias was calculated by asking parents whether their students were absent "more," "about the same," or "fewer" days than their children's typical classmates (this pattern holds across different measures). Among parents of students in Control, the *Reminder* condition, and the *Total Absences* condition, 9.2% (SE=1pp) responded correctly, compared to 16.2%

(SE=2pp) among parents of students in the *Relative Absences* condition [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 children's relative absences does not inhibit parents from reducing actual student absences.

This experiment makes three primary contributions. First, it develops and evaluates a cost-effective and scalable intervention that addresses a critical social problem. Second, it suggests that correcting parents' biased belief about how many total absences their children have accumulated causes parents to reduce student absences. Third, it suggests that using extreme social comparisons to correct parents' biased belief about how their children's absences compare to their children's classmates' absences does not cause an appreciable change in student absences.

Not attending school negatively affects student, school, school district, and national success. Yet, little research has validated interventions that can be implemented at scale with fidelity relatively inexpensively. The intervention reported here is both highly scalable and cost-effective at reducing at-risk students' absences, costing around \$6 per incremental school day generated. We focus on assessing the intervention's costs per additional student day rather than assessing costs versus benefits because we believe that student attendance is of intrinsic importance. As a practical matter, however, quantifying the impact of an additional student day is also quite challenging, due largely to data limitations. Fortunately, there is an active literature seeking to address this problem[4]; we hope that future research can shed additional light on this.

One common best practice for reducing absenteeism — absence-focused mentors — has been found to have substantially higher costs per student-day than the intervention reported in this manuscript. The only randomized controlled experiment evaluating this absence-reduction strategy involved a very similar at-risk population as the one studied in this manuscript (see SOM for discussion of population similarities). Students in Chicago Public Schools grades 1 through 8 (N=1,972) were randomly assigned a mentor[17]. Mentors reduced absenteeism among participating students in grades 5 through 7 by 3.4 days, and had no statistically significant effect on students in grades 1 through 4. The mentors did not affect students' GPA, but appeared to reduce failed courses among the subgroup of students in grades 5 through 7. The mentors cost around \$1,700 per year per treated student, leading the researchers to estimate that mentors cost around \$500 per additional day of attendance generated for students in grades 5 through 7. (See also Balfanz & Byrnes (2013)[46] for a quasi-experimental evaluation of absence-focused mentors targeting a population that also includes students with more extreme absenteeism.) The only other absenteeism intervention strategy that has been assessed with a randomized controlled experiment involved absence-focused SMS text messages; it produced no measurable impacts on absenteeism[47].

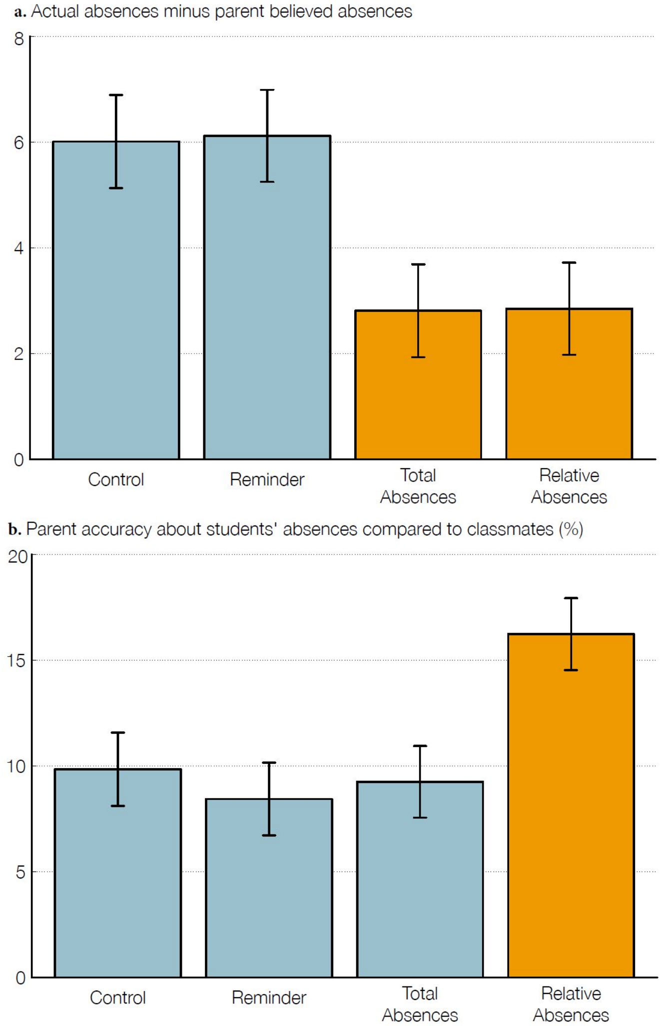

Figure 3. Treatments corrected parents' biased beliefs. Regression-adjusted means and standard errors based on end-of-experiment survey responses; error bars +/-1 SE; orange bars represent treatment conditions that included the relevant information. Informing parents of their students' total absences corrected their beliefs about their students' total number of absences **(a)**. When asked at the end of the year to report the number of absences, parents in the *Total Absences* and *Relative Absences* treatment conditions, both of which included information on students' total absences, were roughly 50% more accurate than parents in the *Control* and *Reminder* treatment conditions. Adding relative absences information to the treatments in the *Relative Absences* condition corrected parents' biased beliefs around relative absences **(b)**. About 16% of parents in the *Relative Absence* condition responded correctly when asked how their student compared to his/her typical classmates.

We note that despite its cost effectiveness, the mail-based intervention reported in this manuscript is not a substitute for more intensive approaches that address the deep personal and structural challenges facing students, families, and communities. After all, this intervention reduces chronic absenteeism by around 10% and cannot reach all at-risk students. 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 towards difficult absenteeism challenges that require more active and personal involvement.

One possible explanation for the additive impact of providing total absences information is 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 child had been absent six days as of about half way through the school year, and the other half were asked to imagine that their child 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 child had accumulated relatively many absences reported that being absent tomorrow would more negatively affect their child's success than did parents who imagined that their child had accumulated relatively few absences, $t_{253} = -4.33$, p=.002. (See SOM.) This is consistent with an interpretation that the *Total Absences* result arises because parents believe there are increasing marginal costs for each additional absence their child accumulates.

In our pre-registered analysis plan, we predicted that adding the personalized social comparison information to the treatment would reduce student absences compared to not including that information. This prediction was informed by the impact social comparisons have had in other domains. In fact, the *Relative Absences* treatment was modeled after the robust and widely studied OPOWER home energy report intervention, the central feature of which is personalized social comparison information[38,48]. The content of the home energy reports is informed by social psychology research showing that personalized social comparison information added to personalized energy use information resulted in energy use conformity[40]. While the evaluations of the specific OPOWER intervention have not isolated the motivational impact of the personalized social comparison information, other research has. For example, related research has shown that adding personalized social comparison information to personalized water use information resulted in water use conformity relative to just personalized water use information[35].

There are many possible reasons that correcting relative absences bias did not result in additional reduction in student absences. For example, perhaps the average gap between students' actual absences and their peers' absences was so large that it discouraged parents[49,50]. Across all rounds of treatment for those assigned to the *Relative Absences* condition, 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. Or, perhaps the treatment was simply too weak. Figure 3 shows that the *relative absences* information increased by 50% the fraction of parents who accurately reported that their students' had missed more school than their classmates — and yet the vast majority of parents still displayed relative absences bias. We hope future research will explore why reducing relative absences bias failed to result in additional parental motivation to reduce their children's absences.

The treatment effects were nearly 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[51]. 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[52]. Undoubtedly, the specific targeting and content of absence-reducing interventions will differ when directed at employee absences[53], though the research we report could provide useful insight for a program of work on this topic.

Additionally, this research examined a personalized information intervention aimed at changing focal individuals' behaviors by communicating to influential third parties — in this case, parents. For sensible reasons, the vast majority of research on information interventions targets the focal individuals directly. However, influential third parties are common in the world. They exist in workplaces (e.g., managers can influence employees), in healthcare settings (e.g., doctors can influence patients), in personal finance settings (e.g., financial advisors can influence investors), and within households (e.g., spouses can influence each other). The present research suggests that targeting influential individuals may be a particularly promising strategy for behavior change.

More research is also needed on the general intervention approach studied in this manuscript. Though rigorously examined and replicated with the pilot study reported in the SOM, this intervention targeted just two biased parent beliefs, when there are many possible beliefs to target. It targeted a specific sample of students in one large urban district, when there are many diverse student samples that merit study. We look forward to future research exploring other biased parent beliefs and broadening the sample frame.

In conclusion, parents of low-income and minority students are often seen as a contributing cause of student failure[54,55]. As we see it, this "deficit" view of parents hinders educational innovation, especially for students in Kindergarten through 12th grade. The intervention we report here shows that an "asset" view of parents can unlock new interventions that empower parents as partners in improving student outcomes[30,31,32,56].

METHODS

Sample selection. We used administrative records from the School District of Philadelphia in the summer preceding the 2014-2015 School Year (SY) to identify all students enrolled in a public school in the district. This yielded an initial study universe of 161,922 students in 103,408 households. On August 15, 2014, we sent opt-out forms to the parents of these students. In 146 (0.1%) households, the consent forms bounced back as being bad addresses, and 923 (0.9%) households opted out of the study. After excluding opt-outs and bounce backs, we determined our initial sample on the basis of the following criteria. We excluded students who enrolled in "non-regular status" schools, defined as any public schools in the district with a special designation, such as specialized schools (like schools for the blind), alternative education schools, charter schools, and online schools. We excluded students who were in 12th grade at the end of the 2013-2014 SY or whose administrative records indicated that the student had graduated, withdrawn, or was otherwise not enrolled as of June 2014. We excluded students who were currently enrolled in the schools used for the pilot study in Spring 2014, as well as any students who were enrolled in one of these schools at the time of the pilot study but who subsequently enrolled in a different school. We excluded students for whom administrative records indicated that (1) the student was not stably housed (i.e., homeless) or that (2) the student had an Individual Education Plan (i.e., learning disability). For students experiencing homelessness, we were concerned about the reliability of their addresses. Students with IEPs were excluded as these students were more likely to have many justifiable absences and we did not want to interfere with other conversations that these students and their parents were having with the district and schools. We excluded students whose indicated home language was not one of our consent form languages (English, Albanian, Arabic, Mandarin, French, Khmer, Russian, Spanish, or Vietnamese) or whose home language was missing in administrative records. This was an ethical concern related to human subjects research because it meant that the informed consent mailing may not have been understood, and thus parents may not have had the opportunity to opt-out of the study. Finally, we excluded all students who had a perfect attendance record in the previous school year because of high year-over-year within student correlation in absenteeism, which we found to be roughly r=0.6.

We further narrowed the sample universe as follows. We then selected all students whose 2013-2014 SY absences met the following criteria. First, we selected students who were absent 3 or more days more than their typical classmate (defined as the modal student in the participating student's school-grade — e.g., the modal student among all 11th graders at Washington High School). We anticipated that students with very good attendance might not show meaningful treatment effects due to ceiling effects. If there were multiple modes, we selected the minimum mode. More individual students were clustered around the minimum mode as compared to the maximum mode, so we concluded that the min-mode was a better representation of the "typical" student in a class. And second, we selected students whose absences did not exceed 2 standard deviations more than the mean student in their school-grade in 2013-2014 SY. We excluded students who had missed over 2 s.d. more days than the mean student in their school-grade for two reasons: (1) in consultation with the district, we determined that many of these students likely no longer lived in the district, but had failed to inform the district, and (2) we hypothesized that these students were more likely than others to be experiencing a grave challenge that might make them less treatment responsive (e.g., an undocumented health or family event). In households with multiple qualifying students (19%), we randomly selected one student from the household to be the focal student, with treatment messages focused on this student. Before post-randomization exclusions, this yielded 32,437 focal students from the 40,326 eligible students in our main study universe.

We also made the following exclusions *after* randomization. We excluded all students in households that were randomly assigned to a condition, but in which the focal student transferred out of the district before the first mail treatment was sent (N=1,242 students). We then excluded all students in a household if the focal student had missing or incomplete attendance data in the 2014-2015 SY (N=2,166 students); we also excluded all students in a household if a sibling of a focal student was missing outcome data in the 2014-2015 SY (N=358). Finally, we excluded all students in a household if the focal student transferred outside the district between the time we sent the first mailing and the end of the school year (N=1,896) (i.e., students who were lost to follow up); we also excluded all students in a household if a sibling of a focal student left the district (N=203). These post-randomization "attrition rates" did not differ across condition (χ^2 p=0.75). In the SOM, we show that impacts are nearly identical under a Missing at Random assumption (Table S7). The final student sample is 53% African American, 20% Hispanic, 52% female, 28% in high school, and 74% free or reduced-price lunch qualified. The final sample consists of 28,080 households (containing 34,461 eligible students) across 203 schools. The 28,080 focal students are the focus of the primary analyses presented below.

Table 1: Sample Size of Experiment Universe after Exclusions

Exclusion criterion	All students	Focal students
Initial eligible universe	161,922	
Pre-randomization		
Consented (did not opt-out)	160,413	
School type	104,088	
Likely enrolled in 2014-2015 SY	95,419	
Pilot study schools	90,430	
Student characteristics	76,167	
Home language for consent form	71,205	
Perfect attendance in 2013-2014 SY	69,387	
Low absenteeism	42,914	
Extremely high absenteeism	40,384	
Students in overly large households	40,326	
One focal student per household [randomly selected]		*32,437*
Post-randomization		
Transferred out of district before first treatment	39,084	
Missing or incomplete 2014-2015 SY attendance data	36,560	
Transferred out of district between beginning and end of experiment 2014-2015 SY	34,461	
Final number of focal students		*28,080*

Data. We obtained all of our student- and household-level data from school district administrative records. The primary outcome is the 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.

The distribution of baseline absences in our final experimental universe was similar to that of the consent universe (see SOM). Among the 110,299 students in the consent universe for whom we have baseline data, the average number of absences in 2013-2014 was 15.3 days (SD=17.7 days) with a minimum of zero and a maximum of 171 days absent. The average number of baseline absences in our final experimental universe was 16.3 days (SD=10.4 days), with a minimum of three and a maximum of 97. The tighter distribution of absences in our experimental universe is by design. First, we excluded high absence outliers, students who were absent more than 2 standard deviations above the mean student in their school-grade, which represented less than 1% of the consent universe. Second, we excluded students who had prior absences within three days (or fewer) of the modal number of absences for their specific school-grade, which represented about 15% of the consent universe.

Analysis. Prior to obtaining any information on outcomes, we pre-registered a detailed 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 (Rosenbaum, 2002). 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 condition, 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.

Ethical procedures. The consent procedures, study design and protocols, and all treatment materials for the pilot, main experiment, and phone surveys were approved by Harvard University's Committee on the Use of Human Subjects (IRB13-2911; IRB14-2747; IRB14-4651).

Code availability. All final analyses were conducted using StataSE and R. Code used to generate the results presented in the paper and Supplementary Information are available from the corresponding author upon reasonable request and with the permission of SDP.

Data availability. Restrictions apply to the availability of the data used to support the findings of this study, and so they are not publicly available. Data may be available from the authors upon reasonable request and with the permission of SDP after a formal review by the District's Research Review Committee.

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Author contributions. TR and AF designed the experiment, oversaw data analysis, and wrote this manuscript.

Competing interests. TR and AF had no competing financial interests while this project was being conducted. In light of the results of this and other projects TR and AF started an organization to help US schools implement this intervention to reduce student absenteeism. It is called *In Class Today* and worked with two school districts at the time of initial submission, including the school district in which the experiment reported in this manuscript was conducted, The School District of Philadelphia.

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Supplementary information is available for this paper.

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