Reducing Student Absences at Scale by Targeting Parents' Misbeliefs

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ABSTRACT (Words=124)

Student attendance is critical to educational success, and is increasingly the focus of educators, researchers, and policymakers. 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 personalized information treatments repeatedly throughout the school year. The most effective versions reduced chronic absenteeism by 10%, partly by correcting parents' biased beliefs about their students' total 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 an 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 percent are chronically absent each year (defined as missing 18 or more days of school) (Balfanz, & Byrnes, 2012; Gottfried, 2009). The rates are even higher in low-income, urban districts (Nauer, Mader, Robinson, & Jacobs, 2014). And chronic absenteeism matters. For students, absences robustly predict academic performance (Allensworth & Easton, 2007; Goodman, 2014; Gottfried 2010; Gottfried 2009), high school graduation (Byrnes & Reyna, 2012; Schoeneberger, 2012), drug and alcohol use (Henry & Thornberry, 2010), criminality (Baker, Sigmon, & Nugent, 2001; Jacob & Lefgren, 2003), and risk of later life adverse outcomes (Rohrman, 1993). 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 (Ely & Fermanich, 2013). Policymakers have recently redoubled their efforts to reduce absences, such as in the Every Student Succeeds Act ("Every Child Succeeds Act," 2015) and in an Obama Administration initiative that aimed to reduce chronic absenteeism by ten percent each year (Lynch, Burwell, Castro, & Duncan, 2015). 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.[1]

This manuscript reports the first large-scale randomized experiment evaluating an intervention that reduces student absenteeism (see Sutphen, Ford, & Flaherty, 2010). 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 chronic absenteeism 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 an order of magnitude more cost-effective than the current best-practice intervention (Balfanz & Byrnes, 2013; Guryan et al., 2017). A key feature of the scalability of this intervention is that it is also particularly easy to implement with fidelity in other school districts (O'Donnell, 2008). 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 students' total absences. A pilot survey of parents of high-absence students in our partner school district shows that

[1] A randomized experimental evaluation of a mentor program called Check & Connect estimated that the program reduced absenteeism at a cost of $500 per incremental day generated (Guryan et al., 2017). A quasi-randomized experiment evaluated a school-based mentor program called Success Mentors. We estimate that the program costs around $121 in personnel opportunity cost per incremental day generated (see SOM; Balfanz & Byrnes, 2013).

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 beliefs, and nearly doubles the absence-reducing impact compared to similar treatments that lack the total absences information. Second, parents are severely miscalibrated about how their students' absences compare to those of their classmates (relative absences). 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 beliefs. Providing this information, however, has no appreciable impact on student absences compared to similar messages without this information.

Theoretical review

In this section we briefly review the cognitive reasons underpinning why the two biased parental beliefs studied in this manuscript might arise. We then review past research examining interventions targeting similar biased beliefs in other contexts or behaviors related to the ones studied in this manuscript.

Parents' beliefs about their students' 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 (Chugh & Bazerman, 2007; Simons & Chabris, 1999). This may lead to parents being uncertain about their students' 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"; Sedikides, Gaertner, & Toguchi, 2003).[2]

Parents' beliefs about their students' relative absences may be inaccurate because of the combination of parents having little direct exposure to when their students' peers are absent and parents'

[2] 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. In the Discussion we report a simple survey experiment suggesting that parents do, in fact, believe that there are increasing marginal educational costs of incremental absences.

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" (Lee, 1991). Our pilot survey suggests that this Lake Wobegon effect applies also to parents' beliefs about how their students' attendance compares to that of their peers (Lee, 1991).

Informing parents about their students' total absences could be thought of as a form of personalized information intervention. Other interventions delivering personalized information have been found to have sizable impacts on consequential behaviors. [3] 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 (Kling, Mullainathan, Shafir, Vermeulen, & Wrobel, 2011). Notifying inattentive cell phone subscribers with timely personalized information when they have exceeded their allotted usage changes subscribers' phone usage behavior (Grubb & Osborne, 2015). 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 (Becker, 1974), they can allocate rewards and punishments to students (Heckman & Mosso, 2014), and under normal conditions parents have incomplete knowledge and bargaining power with respect to their children's human capital investments (Bursztyn & Coffman, 2011). In line with this reasoning, several information interventions targeting parents have been found to change student behaviors. Mailing information about school quality to parents has been found to influence which schools students enroll in and how well they subsequently perform academically (Hastings & Weinstein, 2007). 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 (Bergman, 2015; Bergman & Rogers, 2017; Kraft & Rogers, 2014).

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 (Frey & Meier, 2004; Shang & Croson, 2009), water and resource conservation (Ferraro, Miranda, & Price, 2011; Ferraro & Price, 2013; Goldstein & Cialdini, 2009), energy conservation (Allcott & Rogers, 2014; Nolan et al., 2008; Schultz, Nolan,

[3] Of course, not all information interventions result in changed behavior. Sometimes biased beliefs cannot be corrected (Nyhan & Reifler, 2010) and sometimes correcting biased beliefs does not affect the behaviors presumably linked to those beliefs (Lewandowsky, Ecker, Seifert, Schwarz, & Cook, 2012). For example, one experiment found that 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 (Nyhan, Reifler, Richey, & Freed, 2014).

Cialdini, Goldstein, & Griskevicius, 2007), job selection (Coffman, Featherstone, & Kessler, 2014) and motivation to participate in elections (Gerber & Rogers, 2009; Keane & Nickerson, 2009). In the Discussion we speculate as to why increasing parental accuracy regarding relative absences bias by adding the relative absences information to the treatments does not result in the conformity this past research might predict.

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. We discuss these choices in more detail in the SOM (see SOM, "Sample Selection"). In households with multiple qualifying students (19%), we randomly selected one student to be the target student. Finally, we excluded 5% of students who transferred outside the district between when we sent the first mailing and the end of the experiment. This represents 1% of the overall sample consented. Attrition rates did not differ across conditions (χ^2 p=0.75). In the SOM, we show that impacts are nearly identical under a Missing at Random assumption (Table S8). 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 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.

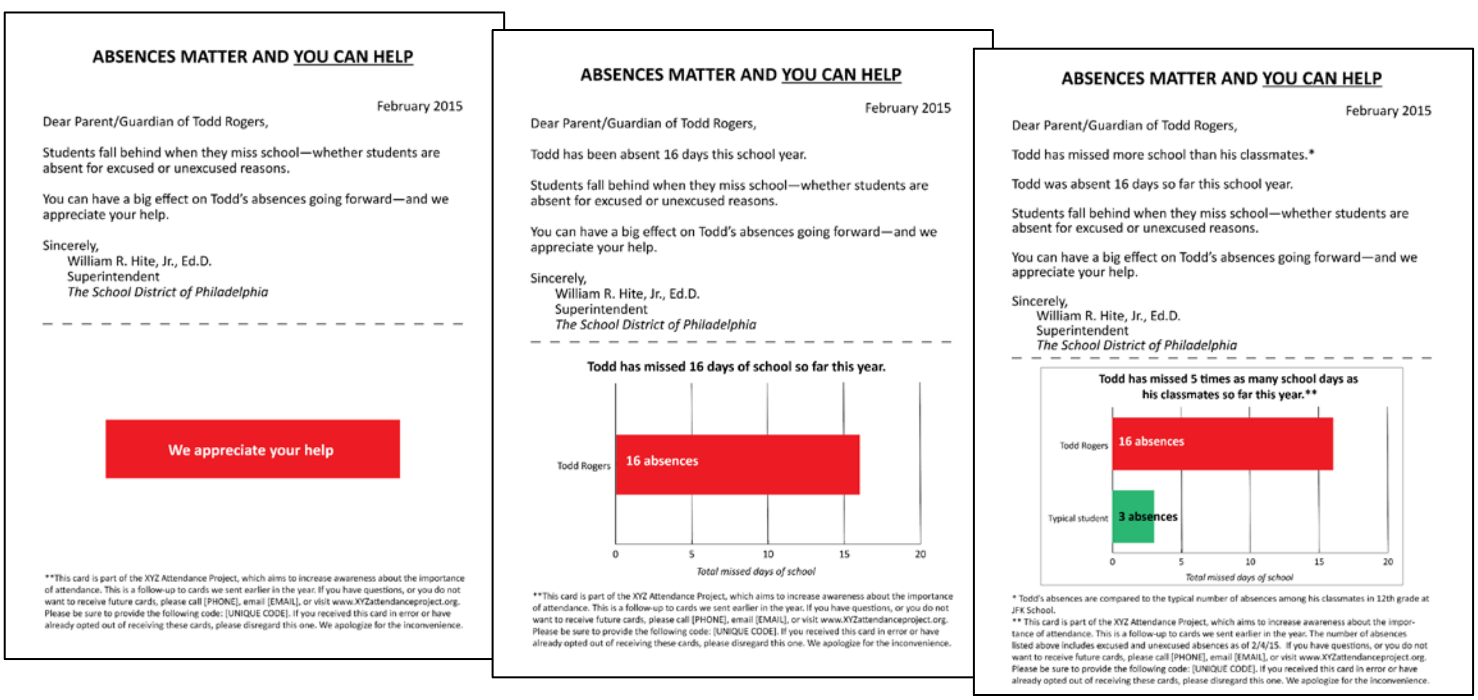
The distribution of baseline absences in our final experimental universe was similar to that of the consent universe (see SOM). Among the 110,229 students in the consent universe for whom we have baseline data, the average number of absences in 2013-2014 was 15.3 days (SD=17.7 days) with a minimum of 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.

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 (see SOM).

Figure 1. Sample mailings from each treatment condition.



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 (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 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), 32.4% in the *Total Absences* regime (SE=0.5pp), and 31.9% in the *Relative Absences* regime (SE=0.5pp). Thus, compared to students in the Control group, chronic absenteeism is 8% lower in the *Reminder* regime, 10% lower in the *Total Absences* regime, and 11% lower in the *Relative Absences* regime.

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, we find modest evidence that the impact was larger in the week immediately following delivery of each treatment round compared to 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 (Allcott & Rogers, 2014). Additionally, across each treatment regime, the first round of treatment has a smaller effect than the average of the subsequent rounds (see SOM for details). While there are many possible reasons for this, one intriguing mechanism is that the first treatment may have alerted parents to the possibility that they may be communicated with again during the school year specifically about their students' absenteeism. This could have created a heightened sense in parents that their students' absenteeism was being carefully monitored and thus generated increased effort to reduce absenteeism (e.g., Rogers, Yoeli, & Ternovski, 2017).

While statistical power was limited, we found no evidence of meaningful treatment effect variation by student grade-level. If such variation is indeed limited, 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. While inherently exploratory, these results suggest that there is indeed meaningful treatment effect heterogeneity not captured by pre-treatment covariates.

Finally, we explore the impact of the intervention on end-of-year standardized test scores for students in grades 4 through 8. For this group, the pooled impact on attendance through the test date was 0.6 days (SE=0.1 days). As a benchmark, the average increase in effect sizes for grades 4 to 8 on nationally normed tests over an entire school year is roughly 0.3 standard deviations (Hill, Bloom, Black, & Lipsey, 2008). Since 0.6 days is roughly 0.3% of the 180-day school year, a simple back-of-the-envelope calculation would suggest an increase in standardized test scores for this group on the order of 0.001 standard deviations. The experiment was dramatically underpowered to study effects of this size; indeed, the minimal detectable effect was an order-of-magnitude larger than this, or around 0.03 standard deviations. 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).

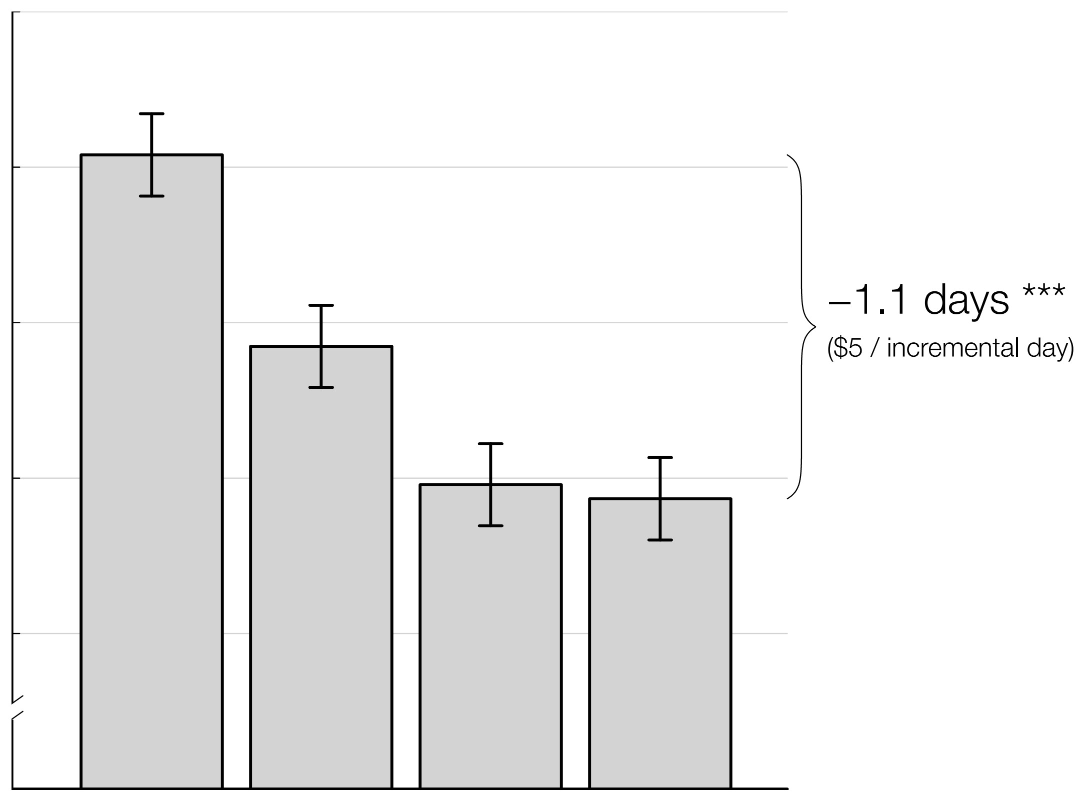


Figure 2. Absences by treatment regime. Regression-adjusted means and standard errors; error bars +/-1 SE; joint FRT p-value for the null hypothesis of no impact is p<0.001.

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 (Karlan, McConnell, Mullainathan, & Zinman, 2016), 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.

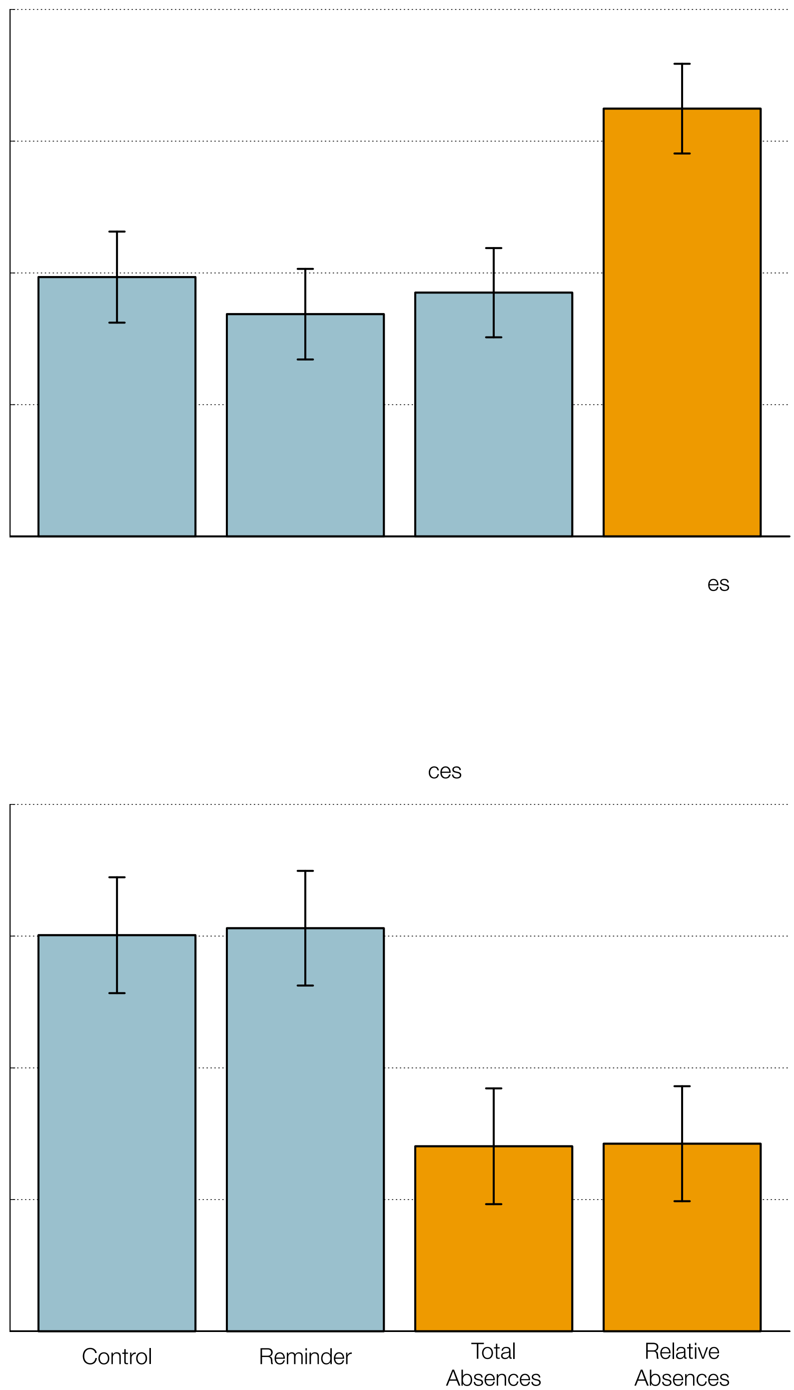


Figure 3. Treatments corrected parents' biased beliefs. Regression-adjusted means and standard errors based on end-of-experiment survey responses; error bars +/-1 SE; orange bars represent treatment regimes that included the relevant information.

Discussion

This experiment 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 students have accumulated causes parents to reduce student absences. Third, it suggests that correcting parents' biased belief about how their students' absences compare to their students' classmates' absences does not cause an appreciable change in student absences.

Missing school negatively affects student, school, school district, and national success. 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. Current best practices like absence-focused social workers and mentors can cost $121-$500 per incremental school day generated (see SOM; Balfanz & Byrnes, 2013; Guryan et al., 2017), while SMS-based absence-focused interventions have thus far produced no measurable impacts (Balu, Porter, & Gunton, 2016). Nonetheless, this mail-based intervention 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%. 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.

One possible explanation for the 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 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 is consistent with an interpretation that the total absences result arises because parents believe there are increasing marginal costs for each additional absence their student 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 intervention we studied was modeled after the robust and widely studied OPOWER home energy report intervention, the central feature of which is personalized social comparison information (Allcott & Rogers, 2014; Allcott, 2011). The OPOWER intervention was modeled after research in social psychology showing that personalized social comparison information added to personalized energy use information resulted in energy use conformity (Schultz et al., 2007). Related research has shown similar patterns for water consumption: adding personalized social comparison information to personalized water use information resulted in water use conformity relative to just personalized water use information (Ferraro, Miranda, & Price, 2011).

There are many possible reasons that correcting relative absences bias did not result in additional reduction in absences. For example, perhaps the average gap between students' actual absences and their peers' absences was so large that it discouraged parents (e.g., Rogers & Feller, 2016; Beshears et al., 2015). 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. 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 students' absences.

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., Nickerson, 2008). 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 (Goetzel, Hawkins, Ozminkowski, & Wang, 2003). Undoubtedly, the specific targeting and content of absence-reducing interventions will differ when directed at employee absences (see ten Brummelhuis, Johns, Lyons, & ter Hoeven, 2016), 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 intervention approach reported in this manuscript. Though rigorously studied, 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 being studied. 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 (Robinson & Harris, 2014*;* Valencia, 1997). As we see it, this "deficit" view of parents hinders educational innovation, especially for K-12 students. 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 (Henderson & Mapp, 2002; Bergman, 2015; Kraft & Rogers, 2015; Bergman & Rogers, 2017).

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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 currently assists two school districts, one of which is school district in which the study reported in this manuscript was conducted, the School District of Philadelphia.

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Author contributions TR and AF were involved in the experimental design, analysis, and writing of this manuscript.

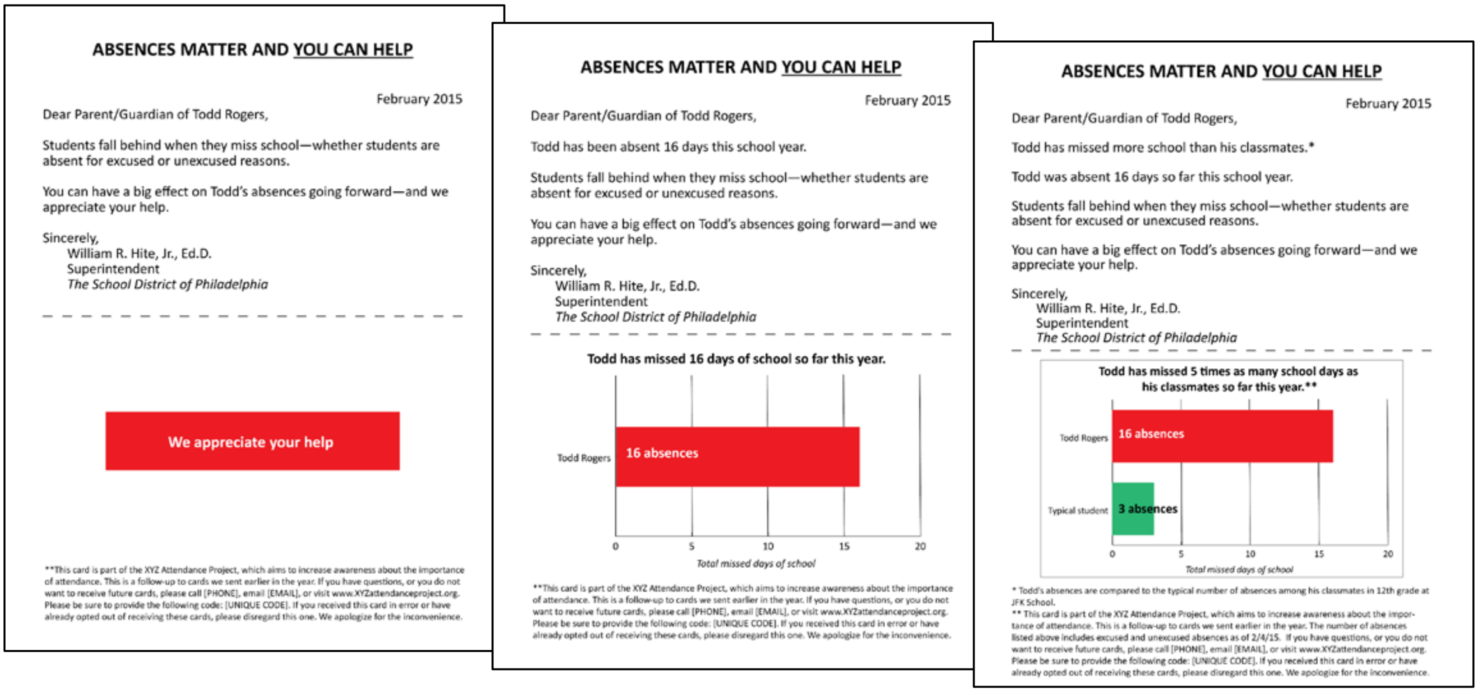

Figure 1. Sample mailings from each treatment condition.

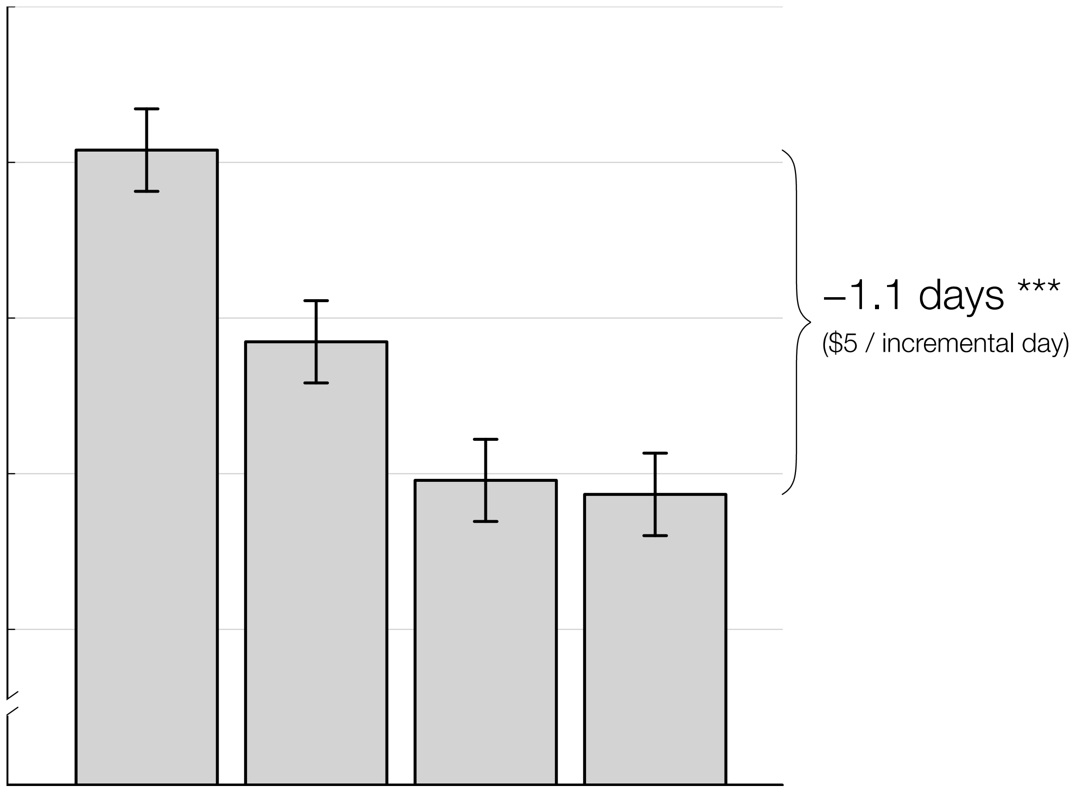

Figure 2. Absences by treatment regime. Regression-adjusted means and standard errors; error bars +/-1 SE; joint FRT p-value for the null hypothesis of no impact is p<0.001.

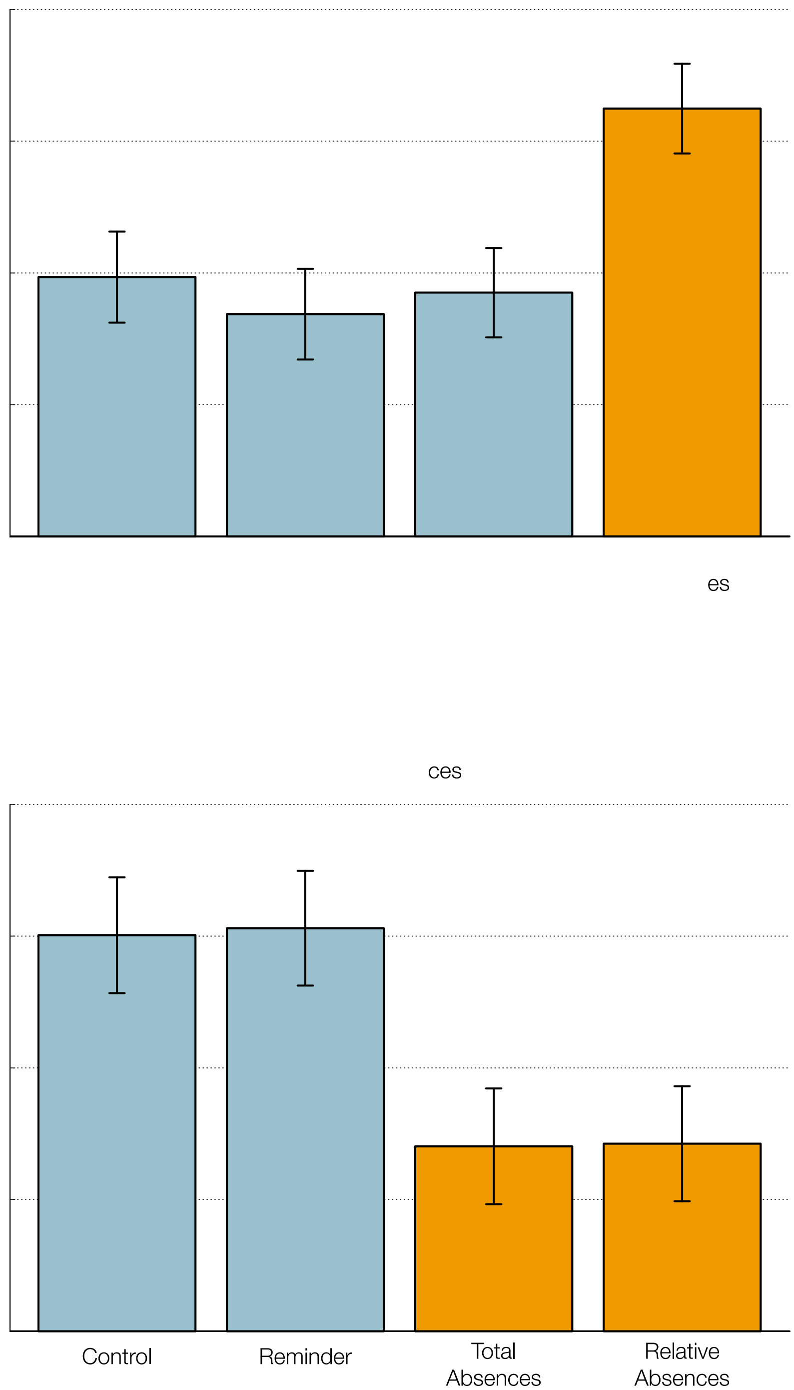

Figure 3. *Treatments corrected parents' biased beliefs. Regression-adjusted means and standard errors based on end-of-experiment survey responses; error bars +/-1 SE; orange bars represent treatment regimes that included the relevant information*