Reducing Student Absences at Scale
By Involving Families

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Supplemental Online Materials (75 pages):

https://www.dropbox.com/s/e1s2p57vpxpm847/SOM.all.docx?dl=0

ABSTRACT

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 year-long regimes of personalized information. The most effective versions reduced chronic absenteeism by 10% across all grade-levels, partly by correcting parents' misbeliefs about their students' total absences. We observe that effects spill over to other students in target students' households. Unexpectedly, correcting parents' biased social comparison beliefs had no impact. This intervention is easy to scale and is twenty times more cost effective than current best practices. Educational interventions that inform and empower parents, like those reported here, can complement more intensive student-focused absenteeism interventions.

Parents[1] of low-income and minority students are often viewed as a contributing cause of student failure (*1*,*2*). We argue that 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 (*3*–*5*). In this manuscript, we report a large-scale randomized experiment evaluating a behavioral policy innovation focused on empowering parents to improve a critical educational input: student absenteeism.

Student absenteeism in the United States is astonishingly high. Among U.S. public school students, 13 percent—over 6 million students—are chronically absent each year (defined as missing 18 or more days of school) (*6*). The rate triples in low-income, urban districts. Chronic absenteeism matters: for students, absences robustly predict academic performance (*7*–*9*), high school graduation (*10*), drug and alcohol use (*11*), and criminality (*12*, *13*). For schools and districts, student absenteeism is often a key performance metric, and, in many states, is tied directly to school funding (*14*). Policymakers have recently redoubled their efforts to reduce absences, such as in the newly enacted *Every Student Succeeds Act* (PL 114-95) and in a recent Obama Administration initiative that aims to reduce chronic absenteeism by ten percent each year (*15*). 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 (*16*).

Although absenteeism is a significant problem in the U.S., we report the first randomized controlled experiment examining an intervention aimed at reducing it. This intervention delivered targeted information to parents of at-risk students through several mail-based messages (N=28,080). The most effective version reduced total absences by 6% and chronic absenteeism by over 10% relative to a control group. The approach is extremely cost-effective, costing around $5 per additional day of student attendance—more than an order of magnitude more cost-effective than the current best-practice intervention (*16*). It is also particularly easy to scale with fidelity (*17*).

This intervention explores whether parents' misbeliefs about their students' absences contribute to absenteeism. Typically, correcting misbeliefs also changes behavior (*18*–*20*). Sometimes, however, it is not possible to correct misbeliefs (*21*); nor does correcting misbeliefs necessarily change behavior (*22*). For example, communications aimed at correcting parents' mistaken belief about the causal link between vaccinations and autism succeeded in correcting the belief, but did not increase parents' motivation to vaccinate their children (*23*). We focus on two mistaken beliefs held by parents of high-absence students (*24*, *25*). First, these parents severely underestimate their students' *total absences*. A pilot survey of parents of high-absence students shows that parents underestimated their own students' absences by a factor of 2 (9.6 estimated vs. 17.8 actual). Second, parents of high-absence students are largely unaware of their students' *relative absences* compared to other students in the same school and grade ("classmates"). In the same pilot survey, only 28% accurately reported that their students had missed more school than their classmates.

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

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 ($\chi 2$ 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 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 $5.50 per household for production and labor.

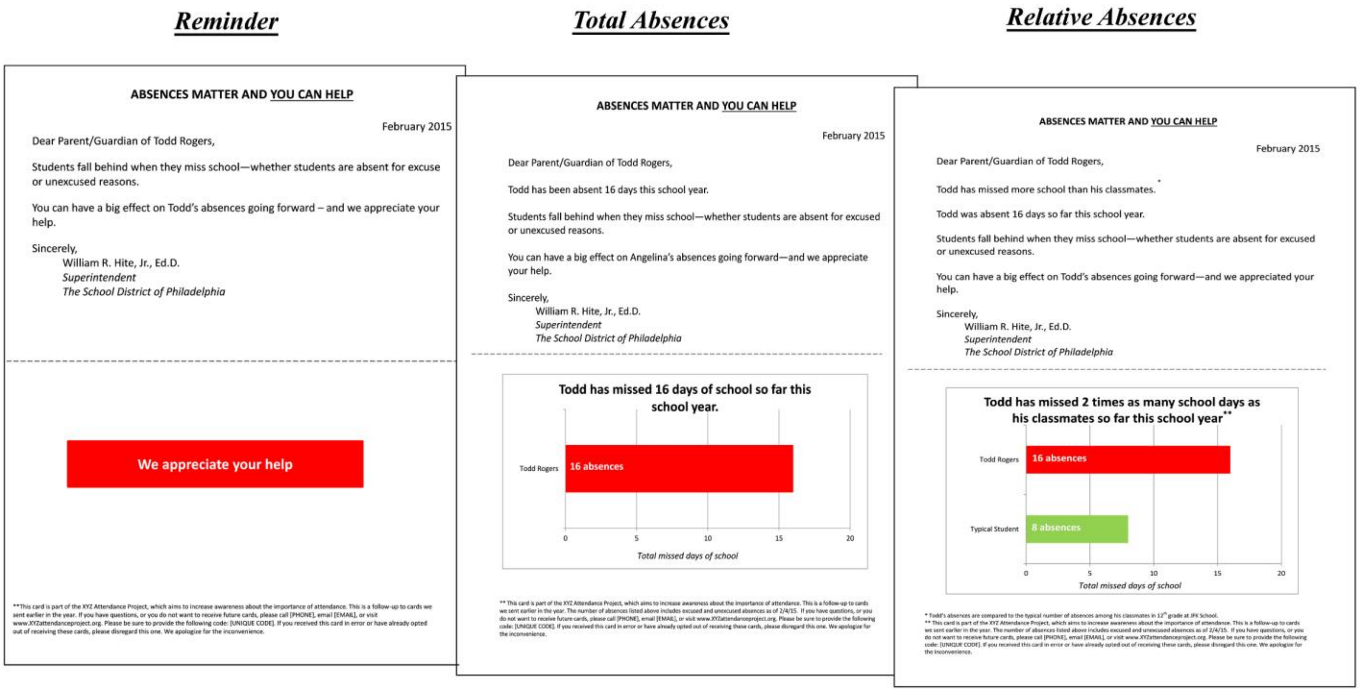

Figure 1.

Not all parents assigned to the treatment regimes received all 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). We therefore base our analysis on random assignment to treatment regime (i.e., Intent-to-Treat), rather than on treatment rounds received. See SOM, which provides extensive detail on analyses reported below, and registered pre-analysis plan (#AEARCTR-0000829, www.socialscienceregistry.org). The SOM also reports a complete pilot study that replicates the results reported in this manuscript.

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. 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 (*26*). 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.

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 (*27*).

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. Additionally, 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 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 (*28*). 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.

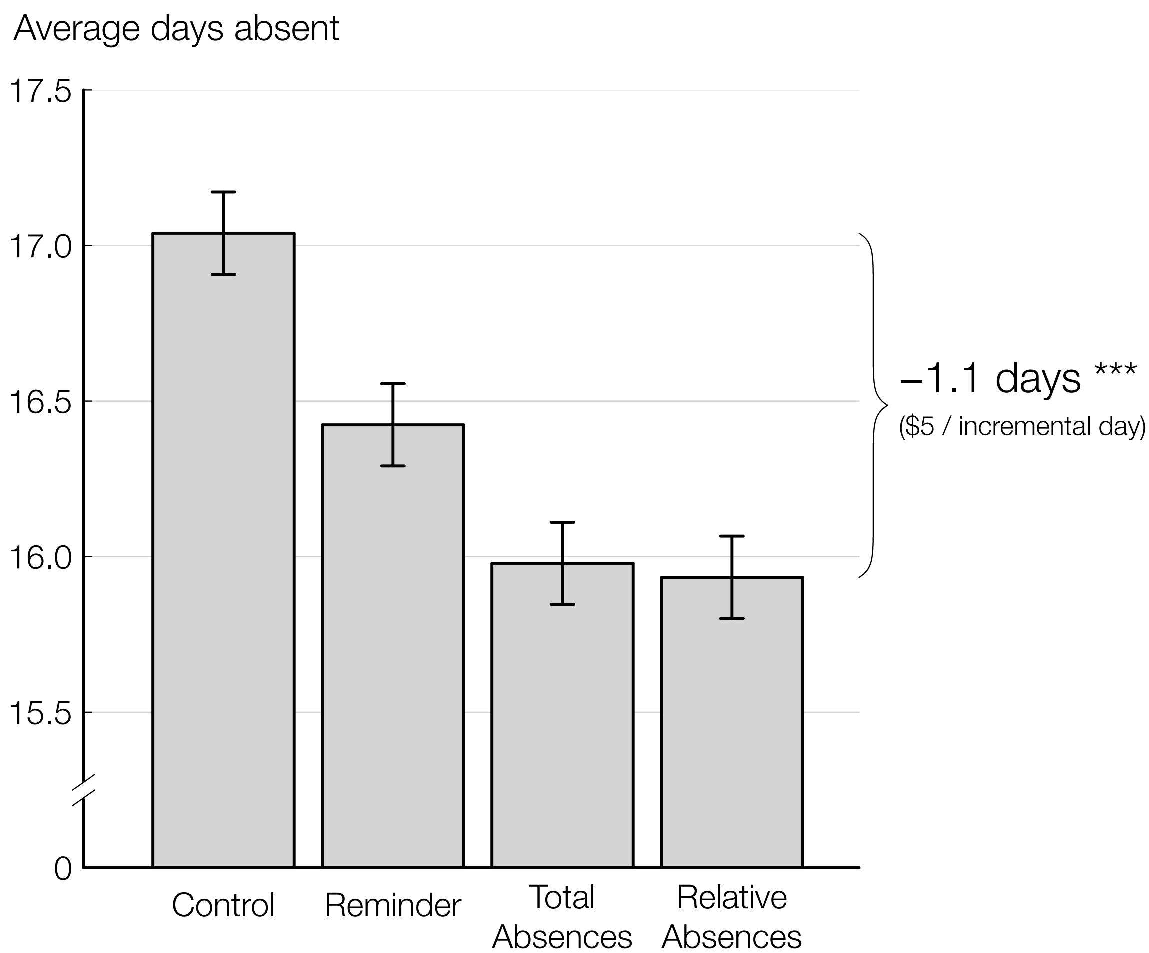

Figure 2. Absences by treatment regime. Regression-adjusted means and standard errors; error bars +/-1 SE; joint FRT p-value for the null hypothesis of no impact is p<0.001.

At the end of the school year, we surveyed a sample of parents to assess whether treatment regimes also affected parent misbeliefs (survey N=1,268; AAPOR Response Rate 2=23.0%). 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 survey showed that parents actually 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 (*30*), 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' misbeliefs about these absences. Parents' misbelief was calculated as the difference between parents' self-reported absences and their students' actual absences (this pattern holds across different measures of misbelief). See Figure 3. Informing parents of their students' total absences indeed corrects this misbelief: 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* regime (2.8 days; SE=0.6 days; ATE=-3.2; SE=0.9). Adding total absences information to the treatments corrected parents' misbeliefs and reduced absences, suggesting that parents' misbeliefs about their students' total absences inhibits parents from reducing actual student absences. Adding total absences information may have also increased the salience of the treatments, turning these treatments into amplified reminders. 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' misbeliefs about their students' relative absences. This misbelief was calculated by asking parents of high-absence students whether their students were absent "more," "about the same," or "fewer" days than their students' typical classmates (this pattern holds across different measures of misbelief). Among parents of high-absence 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 in the *Relative Absences* regime [ATE=7.1pp, p=0.001]. See Figure 3. Adding relative absences information to the treatments corrected parents' misbeliefs, but did not affect absences. This suggests that parents' misbeliefs about their students' relative absences does not inhibit parents from reducing actual student absences and is particularly surprising given the behavioral impact of relative comparisons in other domains (*27*, *30*–*32*). There are many possible reasons correcting beliefs about relative absences 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 (*33*). 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. 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 misbeliefs about relative absences does not motivate parents to reduce absences.

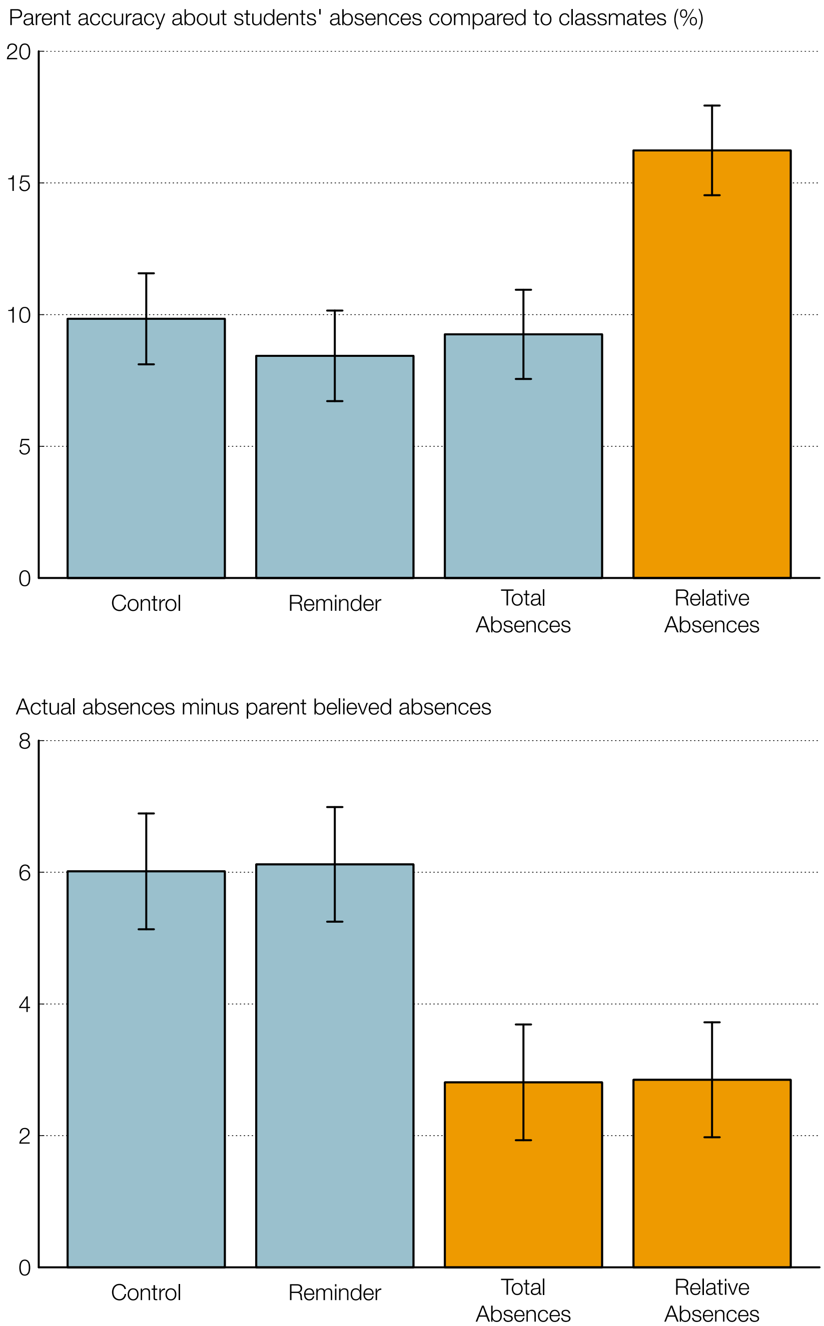

Figure 3. Treatments corrected parent misbeliefs. Regression-adjusted means and standard errors based on end-of-experiment survey responses; error bars +/-1 SE; orange bars represent treatment regimes that included the relevant information

Discussion

This experiment makes four primary contributions. First, it develops and evaluates an extremely cost-effective and scalable intervention that addresses a critical social problem. Second, it shows that some biased parent beliefs causally impact student behavior: parents' under-estimation of their students' total absences prevents them from reducing student absences. Third, it shows that some biased parent beliefs have no impact on student behavior. Parents' biased social comparison belief about how their students' absences compare to their students' classmates' absences has no appreciable impact on student absences. Finally, this experiment illustrates the power of information interventions that encourage influential others to change the behavior of targeted individuals.

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 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, $t(253) = -4.33$, $p=0.002$. (See SOM). This provides additional support for the educational production function interpretation of the Total Absences result.

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 about $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 (*1*, *2*). 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 (*3*, *4*).

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