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The Demotivating Effect (and Unintended Message) of Awards

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Abstract

It is common for organizations to offer awards to motivate individual behavior, yet few empirical studies evaluate their effectiveness in the field. We report a randomized field experiment ($N = 15,329$) that tests the impact of two common types of symbolic awards: pre-announced awards (prospective) and surprise awards (retrospective). The context is U.S. schools, where we explore how awards motivate student attendance. Contrary to our pre-registered hypotheses and organizational leaders' expectations, the prospective awards did not on average improve behavior, and the retrospective awards *decreased* subsequent attendance. Moreover, we find a significant negative effect on attendance after prospective incentives were removed, which points to a crowding-out effect. Survey experiments probing the mechanisms suggest that awards may cause these unintended effects by inadvertently signaling that the target behavior (perfect attendance) is neither the social norm nor institutionally expected. In addition, receiving the retrospective award suggests to recipients that they have already outperformed the norm and what was expected of them, hence licensing them to miss school. Exploratory analyses shed further light on differential effects of awards by age and performance.

Keywords

Awards, Motivation, Attendance, Field experiment

1. Introduction

Researchers and practitioners alike share concerns about the deleterious effects of extrinsic incentives that undermine people's motivations (Deci, Koestner, & Ryan, 2001; Frey & Jegen, 2001; Gneezy, Meier, & Rey-Biel, 2011; Kohn, 1999). Monetary and contingent (“if-then”) rewards seem particularly prone to crowding out motivation (Frey, 1997; Pink, 2011). In contrast, many view non-monetary rewards as promising alternatives, in particular when used to recognize past behaviors. Unexpected, symbolic awards may provide positive reinforcement without being perceived as a bribe to engage in the rewarded behavior (Lepper, Greene, & Nisbett, 1973). In theory, such surprise awards can preserve recipients' sense of autonomy and potentially even reinforce intrinsic motivation (Bénabou & Tirole, 2003; Gallus & Frey, 2016). In practice, many organizations do indeed use awards to acknowledge a job well done after the task is completed (Nelson, 2005), and managers vary the particular situation, timing and form of recognition to maintain the element of surprise even as multiple awards are handed out over time (Bradler, Dur, Neckermann, & Non, 2016; Cranston & Keller, 2013; Walk, Zhang, & Littlepage, 2018).

Despite the theoretical and practical relevance of surprise awards, however, we lack empirical evidence on their impact and how they compare to announced awards in the same organizational context. This is partly because it is difficult to test recognition experimentally outside of laboratory settings (Gallus, 2017). In this paper, we conducted a large-scale field experiment ($N = 15,329$) in which we randomized the provision of recognition to study the effects of surprise, “retrospective” awards and pre-announced, “prospective” awards, compared to a control group that did not receive awards. To date, the existing field experimental literature on awards focuses on either announced, prospective awards (e.g., Kosfeld & Neckermann, 2011)

or on surprise, retrospective awards (e.g., Bradler et al., 2016; Gallus, 2017). To our knowledge, we are the first to provide a comparative evaluation of both types of awards in the field. Our context is U.S. schools, where school personnel frequently use awards to recognize students (Deci et al., 2001).

We focused on attendance awards, which are widely employed also beyond the field of education (e.g., Gubler, Larkin, & Pierce, 2016; Markham, Scott, & McKee, 2002). The intuitive appeal of attendance awards is that they do not entail competition, which can be particularly harmful in educational contexts (e.g., Kohn, 1999). Moreover, by recognizing effort, such awards are accessible to a broader share of people compared to awards based on performance outcomes, which often hinge on previously accumulated knowledge, ability or resources. In education, attendance is a particularly important input factor that affects both individual and organizational success. Because student absenteeism robustly predicts academic performance (Allensworth & Easton, 2007; Balfanz & Byrnes, 2012; Nichols, 2003) and educational failure, such as high school dropout rates (Balfanz & Byrnes, 2012), schools and local educational agencies have sought to make improving attendance a national priority (e.g., Every Student Succeeds Act, 2015). From an organizational perspective, many states rely on average daily attendance rates to distribute funding to schools, creating a financial stake for schools to encourage good attendance habits in their pupils (Ely & Fermanich, 2013). The state of California, which is where the present study was run, introduced a bill that explicitly encourages school administrators to “recognize pupils who achieve excellent attendance or demonstrate significant improvement in attendance” (“Assembly Bill No. 2815,” 2016). We study the effectiveness of different forms of providing such recognition.

Our analysis shows that giving surprise retrospective awards to honor and reinforce perfect attendance unexpectedly *demotivated* the target behavior: award recipients had significantly worse attendance than otherwise identical students in the control group. This negative effect was particularly pronounced among students with poor school performance. In contrast, offering announced prospective awards for perfect attendance on average did not change behavior. However, this main effect masks heterogeneity by age (albeit identified post-hoc). In line with previous findings by Levitt, List, Neckermann, and Sadoff (2016), the prospect of winning a symbolic award did motivate better attendance among younger students, but it was insufficient to motivate older students. Further exploratory analyses on the post-award period, when crowding-out effects should become visible, suggest that the prospective awards also led to a significant decrease in attendance when they were no longer offered (mirroring the findings of Visaria, Dehejia, Chao, and Mukhopadhyay; 2016). It appears that after the award period ends, students on average attended fewer days of school.

We conducted a follow-up experiment (Study 2) to explore the underlying mechanisms behind the unintended negative effects of awards. The findings suggest that the mere introduction of awards may have inadvertently signaled that perfect attendance was neither the norm nor expected. The retrospective award, in particular, signaled to recipients that they had already performed the behavior (attended school) more than their peers and more than was expected by the organization, thus licensing them to miss more school in the future.

2. Theoretical Background

2.1 Retrospective vs. Prospective Awards

Awards can be offered prospectively (i.e., the criteria for earning the award are stipulated in advance) or given retrospectively as recognition for past behavior (see notably Bénabou &

Tirole, 2003, who point out the importance of this distinction for incentives more generally).

Prospective awards are "pre-announced" or "ex ante" contingent rewards, also referred to as "if-then" motivators. Leaders define their expectations in advance and aspirants can work towards fulfilling them in order to attain the award. These awards are closely in line with the traditional economic view of explicit incentives,¹ with the exception that the promise of monetary pay is replaced by a non-monetary reward in the case of symbolic awards.

Retrospective awards are "now-that" or "ex post" rewards. They acknowledge a job well done after a task is completed and come as a surprise to recipients. They may be motivating because people are often unsure about their own ability and performance given the context they are in (e.g., the manager's or institution's norms and expectations). Receiving an award allows the recipients to make inferences about their performance and how it can be situated in a given environment. As explained by Bénabou and Tirole (2003): "the worker or child learns from the [ex post] reward that the task was considered difficult (and therefore that he is talented), or that the supervisor or parent is appreciative of, proud of, or cares about his performance – and therefore that it is worth repeating it" (p. 504).² The introduction of prospective awards can also send signals about the environment (e.g., norms and the giver's expectations), but they do not allow the person to draw inference about their own ability or performance in that context unless the individual has already competed for the incentive and either received the award or not.

In the emerging literature on awards, most of the attention has been paid to prospective, announced awards (Ashraf, Bandiera, & Lee, 2014; Kosfeld & Neckermann, 2011; Levitt et al.,

¹ There is an extensive literature in economics on explicit, ex ante incentives promised either for absolute performance (e.g., piece rate schemes as in Gneezy and Rustichini, 2000 and Lazear, 2000) or based on relative performance, as in tournament schemes.

² Similarly, Kamenica (2012) discusses how contextual inference may explain many of the inconsistent responses to incentives found in the empirical literature.

2016). More recently, however, scholars have started to explore the effects of surprise, retrospective awards. Unlike our study, which looks at non-competitive awards based on attendance, most of the experimental evidence revolves around awards for relative performance, in effect recognizing the best among a group according to some measurable outputs (Bradler et al., 2016; Gallus & Heikensten, 2019; Hoogveld & Zubanov, 2017; Neckermann & Yang, 2017). In most of these experiments it is not the recipients but rather the *non-recipients* (i.e., low performers who were made aware of but did not receive the retrospective award) who react to the intervention by increasing their subsequent performance. The studies cite a preference for conformity to social norms as a likely explanation.

A field experiment in a public goods context does find significant positive and long-lasting effects of retrospective awards when giving symbolic awards to voluntary Wikipedia editors (Gallus, 2017). In contrast to the other research, this study does not focus on awards that are reserved only for the top performers. The experiment instead randomizes who receives the symbolic awards out of a set of new editors who have all deserved a newcomer award by having passed some pre-determined but undisclosed performance threshold (similar to awards such as the Nobel Prizes where there is opaqueness as to who is being considered). The analysis suggests that the positive effects of the symbolic awards may be explained in part by an increased self-identification among these new editors with the community of Wikipedians. Moreover, some recipients seem to have been motivated by the status that awards confer within the community, even though Wikipedia editors operate under online pseudonyms and the awards have no offline reputational consequences.³

³ There are several factors that distinguish the retrospective awards studied in Gallus (2017) from our context, most notably that they recognize pro-social behavior (contributions to a public good) and that they are instituted in a fixed award scheme where each month a limited number of awards would be conferred to newcomers whose pseudonyms would henceforth be listed on a hall of fame-like award page.

We are only aware of one prior study that tests both prospective and retrospective awards within the same context. In a seminal experiment with 3-5-year-old nursery school children, Lepper et al. (1973) find that the promise of receiving a “Good Player” award for playing with magic markers shortened the amount of time the children subsequently opted to play with the markers. Importantly, no such effect was found when the award was given retrospectively. The authors attributed the negative effect of the prospective award to a change in children’s self-perception, which undermined their intrinsic interest in the task (see also Deci et al., 2001). The retrospective award, in contrast, did not corrupt children’s intrinsic motivation. However, there are reasons why even retrospective awards could backfire, and they involve the signals these awards emit.

2.2 Potential Negative Signals of Awards

The widespread use of awards is based on the simple and intuitive appeal that recognizing effort and performance will result in continued or even improved performance. Awards are often used without a full understanding of whether or how they produce the intended behavior. However, awards need not always induce desirable behaviors. While it seems clear that non-recipients may respond negatively (e.g., due to envy), even recipients’ behavior can be adversely impacted.

There are three main reasons why awards may have unintended negative effects on the recipients’ behavior. First, recipients may infer from the awards that their own performance does not conform to the social norm (e.g., Rogers, Goldstein, & Fox, 2018). Recipients of retrospective awards may assume they are recognized because they outperformed others, even when an award is based on their absolute, not relative, performance. This can lead them to reduce their effort, particularly if the behavior is inconsequential and not a reflection of the

recipients' abilities and achievements on an important performance dimension. Previous studies on awards have found such conformity preferences for non-recipients (Bradler et al., 2016; Hoogveld & Zubanov, 2017; Neckermann & Yang, 2017).

Second, awards may send inadvertent signals about the giver's intent or expectations, such that awards can cause the recipients to infer that they have exceeded the institutional expectations (Gallus & Frey, 2017). Research on licensing suggests that when people feel that they have fulfilled their obligations to behave in socially desirable ways, they may subsequently become less likely to perform the socially desirable behavior (Monin & Miller, 2001; Mullen & Monin, 2016). Thus, people who receive a retrospective award for their past performance may feel licensed to reduce their effort going forward (see Blanken, van de Ven, & Zeelenberg, 2015).

We would expect both of these signaling effects to be stronger for recipients who are more uncertain about their performance and for whom the award carries more "news" (i.e., is more unexpected). While otherwise high performing students will receive little new information from getting a retrospective award (on top of the feedback they already get from grades, for instance), a school-related award will be more unexpected for low performing students. Previous research suggests that this may lead those who do better than expected to subsequently lower their performance (Kuhnen & Tymula, 2012). Moreover, to the extent that the incentivized behavior (school attendance) is costlier for low performing students (Angrist & Lavy, 2009), they have an even stronger motivation to interpret the award as a license to reduce effort going forward.

People who are offered prospective awards may also interpret the awards as an indication that perfect attendance is neither the norm nor institutionally expected. However, the nature of

prospective awards implies that individuals cannot yet draw inference on how their own behavior compares to these contextual factors, thus failing to give them a license to reduce their efforts. Moreover, the promise of an award may incentivize some people to exert effort to win the award and thus mask possible negative signaling effects. Once the award is no longer offered, however, similar negative consequences of the introduction of awards may become visible. This would be in line with a crowding-out effect (Gneezy et al., 2011).

A third theoretical possibility is that awards single out individuals in a context where the social costs of being singled out outweigh the benefits of the distinction (Bradler et al., 2016; Bursztyn & Jensen, 2015; Jones & Linardi, 2014). Both prospective and retrospective awards could trigger negative consequences if people desire to avoid the peer social sanctions associated with being someone who tries too hard on a dimension such as attendance, which is not considered important (it may be different, for instance, for sports achievements). We reduce this concern about peer social sanctions by sending awards directly to the recipients' homes. Additionally, in line with the field experimental literature on retrospective awards, we minimize incentive effects (of possibly being singled out in the future) by clearly communicating that the award would remain a one-off event. Thus, social image concerns should not drive our results.

2.3 Awards and Attendance

We focused on attendance because it is a particularly important educational input factor that predicts almost all indicators of academic success (Balfanz & Byrnes, 2012). Furthermore, because many states use an incentivized funding formula to redistribute part of their funding based on a school's average daily attendance rate, attendance is an important consideration for local educational agencies. To date, there are only a handful of successful, experimentally-proven programs that reduce absenteeism and are scalable (e.g., Guryan et al., 2017; Robinson,

Lee, Dearing, & Rogers, 2018; Rogers & Feller, 2018). Despite little experimental research on how to effectively reduce absences—or perhaps because of it—many education organizations use awards to motivate good attendance. As aforementioned, the state of California encourages schools to award students for excellent or improved attendance ("Assembly Bill No. 2815," 2016).

These recommendations reflect the practices of educators. In a survey to U.S. educators, we found that the vast majority of schools do indeed offer recognition or awards for excellent student attendance. School leaders and teachers report using awards for attendance because they believe these awards are effective. Out of 307 educators, only a small fraction of the participants correctly predicted that retrospective awards would emit signals that disincentivize subsequent attendance. Only 2% of district leaders and 2% of teachers predicted that providing students with a retrospective award would result in students attending school less often (see the Supplementary Materials for more details on the survey).

But, the existing evidence on offering awards for attendance also shows conflicting results. One study on absenteeism in the workplace found that personal recognition for good attendance significantly decreased employee absenteeism: receiving recognition for attendance resulted in a 23-percentage point reduction in employee absences (52% to 29%) in a manufacturing plant (Markham et al., 2002). Another study found that an attendance award had short-term positive effects on low-attending employees, but the extrinsic reward from the program crowded out the internal motivation of those employees who had previously demonstrated excellent attendance and resulted in negative effects during the award period (Gubler et al., 2016). The researchers contend the award may have backfired because it failed to acknowledge those who had in the past espoused the desired behavior, prior to the introduction

of the award program. Finally, in the education literature, an experiment ($n = 302$) found that students in an out-of-school program setting who were offered a prospective symbolic award for their attendance attended 42.5% more tutoring hours than students assigned to the control group (Springer, Rosenquist, & Swain, 2015).

In sum, attendance is a behavior that can be shifted through informational interventions (e.g., Robinson et al., 2018; Rogers & Feller, 2018). We therefore focused our study on attendance awards. To combat the potential negative consequences of material rewards (e.g., Deci et al., 2001), we adopted an intervention that several studies found to be effective at improving future performance: symbolic awards (Bradler et al., 2016; Kosfeld & Neckermann, 2011; Gallus, 2017). Based on the studies cited in the section on retrospective and prospective awards, we anticipated that offering such symbolic awards would lead to improved attendance. We were interested in exploring which of the two award types would have stronger positive effects.

3. The Present Experiments

The present research examines the impact of offering symbolic awards for attendance through two studies. Study 1 reports a randomized field experiment ($N = 15,329$) that tests the impact of two types of symbolic awards on student attendance: prospective awards and retrospective awards. The intervention targeted students in grades 6-12 across 14 urban, suburban, and rural school districts on the West Coast of the United States. It involved delivering mail-based communications directly to homes of students, as this is the main channel for official communications from schools and school districts.

Contrary to our pre-registered hypotheses, students on average did not miss fewer days of school when offered the chance to earn a prospective attendance award, and they missed more

days of school after receiving a retrospective award for past attendance. We include findings from exploratory analyses on heterogeneous treatment effects and the post-award period (when the awards were no longer offered) to gain insight into how these awards impacted student behavior. We also conducted a follow-up experiment (Study 2) to further test the proposed mechanisms.

4. Study 1: Field Experiment

4.1.1 Participants

The sample of Study 1 consisted of 15,629 students across 14 school districts in a diverse county in California. Because secondary school students arguably can exert control over getting to and from school, it included all 6th through 12th grade students who did not have a sibling in grades K-12 (these households participated in a different, unrelated experiment and were therefore not eligible for the present study). The sample excluded students with inconsistent records of pre-randomization absences (where our two sources of absence data suggested different counts of days absent), students with unreliable addresses, students who opted out or whose opt-out consent forms were undeliverable, and students belonging to school-grade combinations of less than six students (for randomization purposes). To be able to randomly assign who would receive the award, the sample was restricted to participants who had achieved perfect attendance in at least one fall month (e.g., zero absences in September, October, or November) of that year, which included 88% of the otherwise eligible population. Therefore, all participants in the sample were eligible to receive an award for perfect attendance in a fall month.

We did not receive outcome data for 1.92% of the eligible students, so the final analytic sample consists of 15,329 students. Students for whom we did not have outcome data were

balanced equally across conditions ($p = .11$). Table 1 shows the baseline participant demographic information by condition. Participants in the final analytic sample were absent on average 0.42 to 0.46 days in each month from September through November, i.e., prior to the intervention. High school students (grades 9-12) comprised 76% of the sample. Thirty-five percent of students in our sample were identified as English Language Learners (ELL) and 20% of participants came from primarily Spanish-speaking households. ELL status was missing for 7% of the sample.

Table 1. Baseline equivalence among three conditions and overall demographics of the final analytic sample

Variables		Condition			Total	p-value
		Control	Prospective	Retrospective		
Language of letters	English	80.3%	80.2%	79.9%	80.1%	0.89
	Spanish	19.7%	19.8%	20.1%	19.9%	
ELL status	Non-ELL	58.0%	58.8%	57.9%	58.2%	0.85
	ELL	35.2%	34.5%	35.0%	34.9%	
	Missing	6.8%	6.7%	7.1%	6.9%	
Prior absences	September	0.39	0.42	0.44	0.42	0.12
		(1.03)	(1.23)	(1.15)	(1.14)	
	October	0.43	0.41	0.43	0.43	0.59
	November	(1.03)	(1.04)	(1.00)	(1.02)	0.33
		0.46	0.48	0.45	0.46	
		(0.99)	(1.00)	(0.94)	(0.98)	
Grade	6	8.6%	8.6%	8.5%	8.6%	1.00
	7	8.7%	8.6%	8.7%	8.7%	
	8	7.2%	7.2%	7.2%	7.2%	
	9	17.9%	17.8%	18.0%	17.9%	
	10	19.7%	19.7%	19.7%	19.7%	
	11	19.6%	19.6%	19.5%	19.5%	
	12	18.4%	18.4%	18.4%	18.4%	
N		5,109	5,099	5,121	15,329	

Standard deviations in parentheses.

p-values for English Language Learner (ELL) status, Language of letters, and Grade were computed using chi-squared tests.

p-values for Prior absences were computed using ANOVA.

4.1.2 Procedures & Measures

We tested the impact of sending students symbolic awards for attendance by randomly assigning grade 6-12 students who had perfect attendance in one fall month (i.e., zero absences in either September, October, or November) to one of three conditions: (1) Control (students received no additional communications) ($n = 5,216$), (2) *Prospective Award* ($n = 5,209$), or (3) *Retrospective Award* ($n = 5,204$).⁴ We performed a stratified randomization by school and grade.

Students in both award conditions received a mailing in the last week of January 2016. Students in the Prospective Award condition received a letter telling them that they would have the opportunity to earn an award if they had perfect attendance in February (i.e., the upcoming month). A picture of an award certificate that they would earn from the county office was printed on the letter. Students in the Retrospective Award condition received a letter telling them they had earned an award for perfect attendance in a fall month; a full-size, personalized award certificate was enclosed with the letter. In both award conditions it was noted that the award would not be offered again that year. For both award conditions, mailings were sent to students on the same date using identical-looking envelopes, with the only difference being the aforementioned content about the awards. Students with Spanish as their home language according to district records received letters in Spanish. All other letters were in English. See Figure 1 for an example of the intervention materials.

⁴ We also randomly assigned students to receive the awards privately or with a mention that their principal and superintendent would be informed, within both the Prospective Award and Retrospective Award conditions. Because assignment to these conditions did not significantly affect the results (i.e., there was no marginal impact of an award being public), we do not discuss the theoretical rationale for their inclusion for parsimony.

Figure 1. Study 1 Intervention Materials

A) Prospective Award Letter

Dear Sammy,

You can earn an award if you have perfect attendance in February!

If you have zero absences* in February, you will receive the Perfect Attendance Award (like the one below). The award will be sent to you in the mail, and it will not be offered again this school year.

Sincerely,
George Washington
American County Superintendent

Sample Perfect Attendance Award:



*For more information on how absences are determined, please visit www.americancounty.org/attendancematters. Please note that absences include excused and unexcused absences. This letter is part of the Attendance Matters Project, which aims to increase awareness about the importance of attendance in American County. Please visit www.americancounty.org/attendancematters if you have any questions. Please be sure to provide your unique four-digit code in all communications: A12B. If you received this card in error or have already opted out of receiving these cards, please disregard this one. We apologize for the inconvenience.

B) Retrospective Award Letter and Award Certificate

Dear Sammy,

You have earned an award for having **perfect attendance!** You attended school every day for one month this school year during the fall semester.

To recognize that **you had zero absences for one month, we present you with the Perfect Attendance Award**, enclosed with this letter. The award will not be offered again this school year.

Sincerely,
George Washington
American County Superintendent



*For more information on how absences are determined, please visit www.smcoe.org/attendancematters. Please note that absences include excused and unexcused absences. This letter is part of the Attendance Matters Project, which aims to increase awareness about the importance of attendance in San Mateo County. Please visit www.smcoe.org/attendancematters if you have any questions. Please be sure to provide your unique four-digit code in all communications: A12B. If you received this card in error or have already opted out of receiving these cards, please disregard this one. We apologize for the inconvenience.

In line with the guidelines of Gehlbach and Robinson (2018), we pre-registered an analysis plan (<https://osf.io/rabd3/>) before receiving outcome data from the school districts and pre-specified the following hypotheses:

- Hypothesis 1: Students in the treatment conditions (the Prospective Award and Retrospective Award conditions pooled together) will have improved attendance in the target month as compared to students in the control group.
- Hypothesis 2: Students in the Prospective Award condition will have improved attendance in the target month as compared to students in the Retrospective Award conditions.

The primary outcome variable was the student's number of absences in the month of February (the target month). We also examined whether students attained zero absences in February (i.e., the goal of the Prospective Award condition). In both cases, the total number of absences included both excused and unexcused absences because prior research suggests that the results are consistent whether examining excused and unexcused absences separately or together (Rogers & Feller, 2018), and missing school for any reason results in lost learning time.

We collected demographic variables from the school districts to use as covariates in the analysis, along with student absences in the pre-study months of September, October, and November. Demographic variables included the student's primary language spoken at home and ELL status. Home language was a binary covariate for whether letters were sent in English or Spanish. Because ELL status was not available for 7% of the final analytic sample, we imputed missing ELL status as non-ELL in a binary covariate and included an indicator for missing ELL status in all models using ELL status as a covariate.

Socioeconomically disadvantaged status was not available for 26% of the sample and was therefore not included in the analysis. School and grade level were accounted for as strata fixed effects. The districts also provided each student's number of absences in February, which is our outcome of interest, as well as absences in the other months of the school year.

At the end of the year, we received academic performance data (either standardized test scores or course grades) for only 42% of the sample ($n = 6,368$). To create a variable representing students' academic performance, we averaged Math and ELA subject letter grades when schools provided course grades (using a 0-4.33 GPA scale). For students who had standardized test scores in ELA and Math, we averaged scores from both tests. For both grades and test scores, we standardized measures within school and grade to a mean of 10 and a standard deviation of 1. We then created a composite academic performance variable combining the grade and test score averages to maximize the number of students for whom we had academic performance data. To explore the differences between high and low academically performing students we created a binary indicator for high/low academic performance (using a median split of the academic performance variable within school and grade). We also squared the students' academic performance variable to illuminate if effects have been driven by the extremely low or extremely high performing students.

4.1.3 Analytic Details

We checked for balance across conditions in the analytic sample using a multinomial logistic regression with condition assignment as the dependent variable and baseline variables as predictors.

In our main analyses, we used linear regression to estimate the Average Treatment Effect (ATE) on days absent and logistic regression to estimate the ATE on perfect attendance. For

each dependent variable, we estimated effects using three model specifications: a simple model with no strata fixed effects or controls, a model with strata fixed effects only, and a final model with strata fixed effects and student covariates as controls. Our final models controlled for student-level demographic indicators, student absences in the fall semester, and the student's school and grade level. For our continuous dependent variable, days absent, we calculated robust standard errors and conducted randomization inference tests for each model, reporting Fisher Randomization Test (FRT) p-values. The estimates remain meaningfully the same when using different model specifications (i.e., negative binomial regressions and linear probability models, difference-in-differences analysis, clustered standard errors; see Tables S2-S5 in the Supplementary Materials).

4.2 Results

4.2.1 Check for Baseline Equivalence

We checked to ensure the treatment and control groups were balanced across covariates (i.e., the primary language spoken at home, ELL status, pre-study absences, and randomization strata). The covariates in the model did not jointly predict treatment assignment, LR $\chi^2(308, n = 15,329) = 19.62, p > .99$.

4.2.2 Pre-Specified Hypotheses: Student Absences & Perfect Attendance

Contrary to our pre-registered hypothesis, our analysis shows that the Prospective Award and Retrospective Award conditions pooled together had no positive effect on attendance. See Table S1 in the Supplementary Materials for details.

Table 2 shows the results broken out by each treatment condition. We found that students assigned to the Prospective Award condition did not differ from students in the control condition in the number of days of school they were absent in February, $B = 0.006, SE = 0.024, FRT p =$

.823. The groups also did not differ in the fraction of students who had perfect attendance in February (62.42% compared to 62.55% in the control; $\beta = -0.006$, $SE = 0.044$, $p = .887$).

Table 2. Average Treatment Effect on Student Absences in February (Prospective vs. Control and Retrospective vs. Control)

	Absences			Perfect Attendance		
	(1)	(2)	(3)	(4)	(5)	(6)
Prospective	0.012 (0.026)	0.013 (0.025)	0.006 (0.024)	-0.012 (0.041)	-0.014 (0.043)	-0.006 (0.044)
Retrospective	0.064* (0.027)	0.065** (0.026)	0.060* (0.025)	-0.083* (0.041)	-0.090* (0.042)	-0.086* (0.043)
<i>N</i>	15,329	15,329	15,329	15,329	15,292	15,292
Control Mean	0.721	0.720	0.724	0.522	0.518	0.513
Strata	No	Yes	Yes	No	Yes	Yes
Covariates	No	No	Yes	No	No	Yes

Standard errors in parentheses.

Stratification variables were school and grade, controlled for as strata fixed effects.

Covariates include indicators for English Language Learner (ELL), missing ELL status and language of the letters, as well as the number of absences in the pre-study months of September, October, and November. Columns 1-3 coefficients are point estimates from OLS regression models. The associated p-values are from FRT.

Columns 4-6 coefficients (the estimated log-odds) and associated p-values are from logit regression models. Control means are in log-odds.

Columns 5 & 6 have fewer participants because a handful of small randomization strata perfectly predicted the outcome variable and were therefore dropped in the regression.

+ $p < .1$; * $p < .05$; ** $p < .01$; *** $p < .001$

However, students who were retrospectively offered awards as a surprise for their prior positive behavior had worse attendance in the following month. Compared to the control group, students assigned to the Retrospective Award condition were absent 0.06 more days ($SE = .025$, FRT $p = .025$), which corresponds to a regression-adjusted 8.3% increase in absences in the month of February, or an unadjusted effect size of 0.047. These students were also about two percentage points less likely to have perfect attendance in February (60.75% compared to 62.55% in the control), a 2.9% reduction ($\beta = -0.086$, $SE = 0.043$, $p = .047$).

Although our initial expectation that both awards would incentivize positive behavior was not met, we find evidence for our second pre-registered hypothesis: students who were

offered prospective awards did indeed have better attendance in February as compared to students who received a retrospective award. Students in the Prospective Award condition were absent 0.055 days less than students in the Retrospective Award condition ($SE = 0.025$, FRT $p = .029$). They were 1.7 percentage points more likely to have a perfect month of attendance ($p = .066$). But, as the above results suggest, this occurred because the retrospective award had adverse effects on student behavior.

4.2.3 Exploratory Analyses

To further understand how awards impacted student behavior, we conducted heterogeneity analyses and looked at behavior in the post-award period, when the prospective awards were no longer offered. This was motivated by the literature on crowding-out effects, which can be observed once the incentives are removed. The following results are exploratory and not confirmatory.

4.2.3.1 Student Academic Performance

We first explored whether the negative treatment effect in our field experiment was moderated by students' end-of-year average academic performance. Our motivation for this analysis was twofold. First, receiving a school-related retrospective award will carry more news for students who otherwise perform poorly academically. They receive a signal that they are doing better than they may have expected, which should lead them to lower their performance going forward (in line with Kuhnen & Tymula, 2012). High performing students will already have higher expectations and beliefs about their performance given that they receive more positive information (e.g., from grades). Second, and connected to the first rationale, compared to high performing students, students who struggle academically often find school difficult and are less engaged (Fredricks, Blumenfeld, & Paris, 2004). For them, attending school is costlier than for high performers (Angrist & Lavy, 2009). Therefore, low performing students who

receive an award signaling that they are meeting or exceeding the organization's expectations and the social norm are more likely to accept it as a license to miss a future day of school.

Table 3 shows how student performance interacted with the treatment. As Figure 2 illustrates, there was almost no difference in the subsequent number of school days missed among high performing students in the Retrospective Award and control conditions. However, we observe that low performing students receiving the retrospective award missed 0.13 more days of school than low performing students assigned to the control group, $SE = 0.055$, $p = .02$ (see also Figure S1 in the Supplementary Materials). The regression model shows that the interaction term is statistically significant, $SE = 0.068$, $p = .044$. The same pattern holds when exploring the effect of the interaction of student performance with the treatment on whether students had perfect attendance post-treatment. High performing students were equally likely to have a perfect month of attendance in February, no matter their condition assignment, while only 53% of low performing students in the Retrospective Award condition had a perfect month of attendance in February, compared to 60% in the control condition, $p = .002$. Again, the interaction term is statistically significant. Low performing students assigned to the Prospective Award condition did not respond to the treatment by increasing the number of school days they missed in February.

Table 3. Heterogeneous Treatment Effects on Student Absences in February (Prospective vs. Control and Retrospective vs. Control) by Academic Performance

	Absences		Perfect Attendance	
	(1)	(2)	(3)	(4)
Prospective	-0.028 (0.053)	-0.059 (0.215)	-0.029 (0.094)	-0.323 (0.378)
Retrospective	0.127* (0.055)	0.436+ (0.228)	-0.289** (0.094)	-1.067** (0.377)
High performance	-0.163** (0.048)		0.280** (0.097)	
Prospective*High performance	-0.022 (0.066)		0.142 (0.136)	
Retrospective*High performance	-0.137* (0.068)		0.290* (0.135)	
Performance squared		-0.007** (0.001)		0.010** (0.003)
Prospective*Performance squared		0.000 (0.002)		0.003 (0.004)
Retrospective*Performance squared		-0.004+ (0.002)		0.009* (0.004)
<i>N</i>	6,368	6,368	6,361	6,361
Strata Fixed Effects	Yes	Yes	Yes	Yes
Student Covariates	Yes	Yes	Yes	Yes

Standard errors in parentheses.

Stratification variables were school and grade, controlled for as strata fixed effects.

Covariates include indicators for English Language Learner (ELL), missing ELL status and language of the letters, as well as the number of absences in the pre-study months of September, October, and November.

For students for whom grade data was available (N=5,527), performance was measured as English Language Arts (ELA) and Math GPA standardized to a mean of 10 and standard deviation of 1. For students for whom only standardized test data was available (N=841), performance was measured as average ELA and Math test scores, standardized to a mean of 10 and standard deviation of 1.

Academic performance data was only available for 42% of students in the analytic sample.

Columns 1 & 3 interact treatment condition with a median split of this performance variable.

Columns 2 & 4 interact treatment condition with a continuous measure of performance squared.

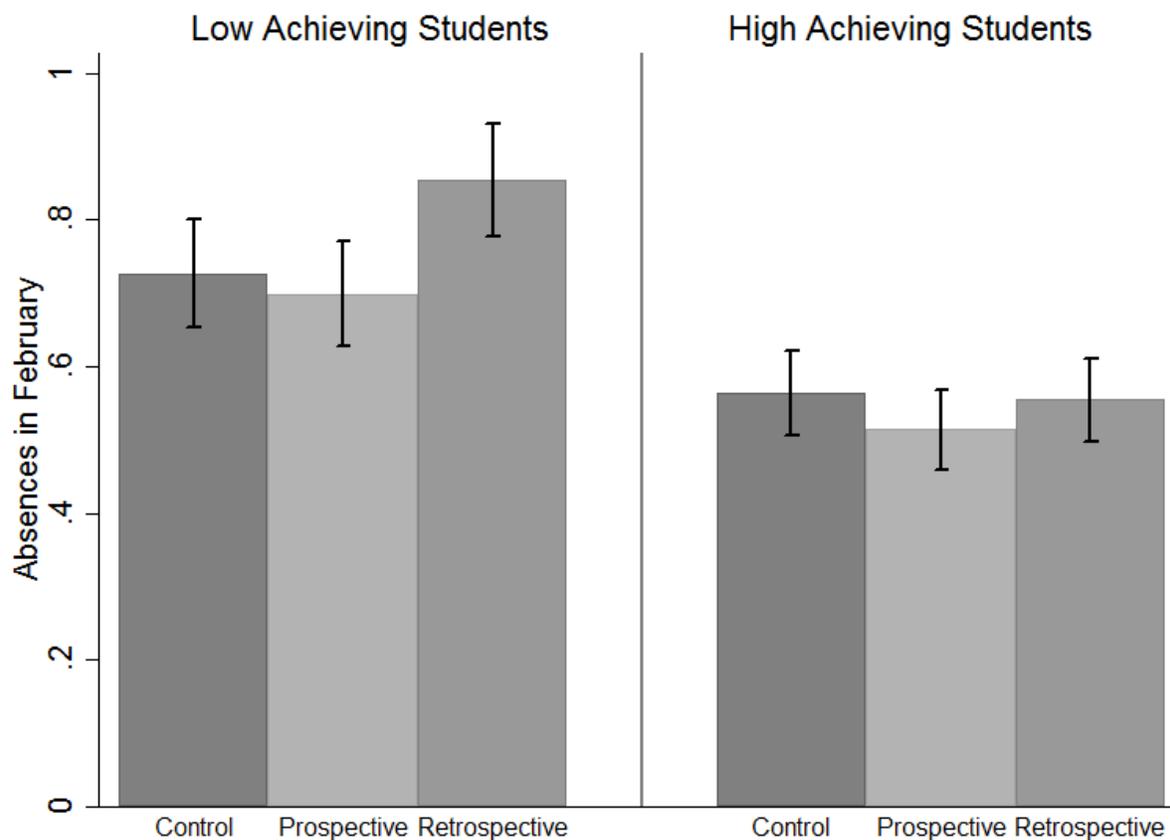
Columns 1 & 2 coefficients are point estimates from OLS regression models. Robust standard errors presented.

Columns 3 & 4 coefficients (the estimated log-odds) and associated p-values are from logit regression models.

Columns 3 & 4 have fewer participants because a handful of small randomization strata perfectly predicted the outcome variable and were therefore dropped in the regression.

+ $p < .1$; * $p < .05$; ** $p < .01$; *** $p < .001$

Figure 2. Heterogeneous Treatment Effects on Student Absences in February (Prospective vs. Control and Retrospective vs. Control) by Median Split of Academic Performance



Predictions from OLS regression with controls for strata and covariates (Table 3, Column 1).

4.2.3.2 Other Student Characteristics

Our exploratory analyses also examined whether there are differential responses to the offer of each type of award by three additional student characteristics: prior record of absences, gender, and grade level. First, we found no difference in the impact of each award condition by prior absences or student gender on post-treatment absences or the likelihood of having perfect attendance in February (see Tables S6 and S7 in the Supplementary Materials). Similarly, there was no evidence of a significant moderating effect for the retrospective award condition by

student grade level: regardless of grade level, students were more likely to be absent if they had earned the retrospective award by surprise (see Table 4).

However, we did observe a differential response by grade level to the offer of prospective awards for a perfect month of attendance. In line with previous results by Levitt et al. (2016), younger children were more motivated by the symbolic incentive than their older peers. As Figure 3 illustrates, middle school students assigned to the Prospective Award condition in our experiment had fewer absences in February than middle school students assigned to the control and Retrospective Award condition. The same is not true for high school students in the Prospective Award condition, who have directionally more absences than their peers in the control group. The regressions in Table 4 lend further support to this graphical result: sixth grade students (first year of middle school) in the Prospective Award condition were absent 0.12-days less in February than sixth graders in the control condition, $p = .011$, but each additional grade level was associated with a .035-increase in the number of days students were absent in February, $p = .006$.

Table 4. Heterogeneous Treatment Effects on Student Absences in February (Prospective vs. Control and Retrospective vs. Control) by Student Grade Level

	Absences			Perfect Attendance		
	(1)	(2)	(3)	(4)	(5)	(6)
Prospective	-0.310*	-0.313*	-0.331**	0.403+	0.431+	0.497*
	(0.126)	(0.122)	(0.119)	(0.225)	(0.230)	(0.235)
Retrospective	0.084	0.085	0.079	-0.378+	-0.393+	-0.390+
	(0.136)	(0.133)	(0.129)	(0.221)	(0.226)	(0.231)
Grade	0.055**	0.048+	0.031	-0.098**	-0.097*	-0.076+
	(0.010)	(0.025)	(0.024)	(0.016)	(0.040)	(0.041)
Prospective*Grade	0.033*	0.034**	0.035**	-0.043+	-0.046*	-0.052*
	(0.013)	(0.013)	(0.013)	(0.023)	(0.023)	(0.024)
Retrospective*Grade	-0.002	-0.002	-0.002	0.030	0.031	0.032
	(0.014)	(0.014)	(0.014)	(0.022)	(0.023)	(0.023)
<i>N</i>	15,329	15,329	15,329	15,329	15,292	15,292
Strata	No	Yes	Yes	No	Yes	Yes
Covariates	No	No	Yes	No	No	Yes

+ $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Standard errors in parentheses.

Stratification variables were school and grade, controlled for as strata fixed effects.

Covariates include indicators for English Language Learner (ELL), missing ELL status and language of the letters, as well as the number of absences in the pre-study months of September, October, and November.

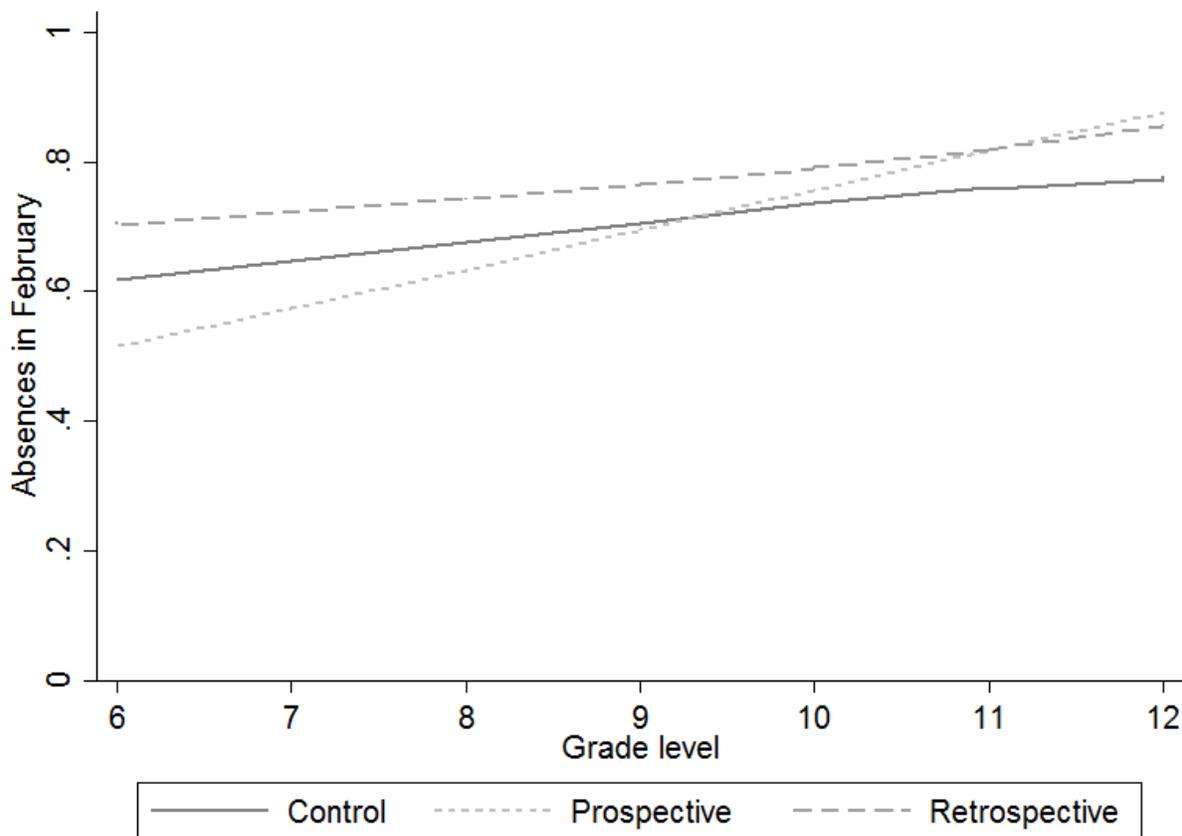
March attendance data was not available for 91 students (0.59%).

Columns 1-3 coefficients are point estimates from OLS regression models. Robust standard errors presented.

Columns 4-6 coefficients (the estimated log-odds) and associated p-values are from logit regression models.

Columns 5 & 6 have fewer participants because a handful of small randomization strata perfectly predicted the outcome variable and were therefore dropped in the regression.

Figure 3. Heterogeneous Treatment Effects on Student Absences in February by Award Condition and Student Grade Level



Locally weighted predicted days absent in February by experimental conditions, using a bandwidth of 1. Predictions from OLS regression with controls for strata and covariates (Table 4, Column 3).

4.2.3.3 Crowding-Out After Incentive Removal

Finally, we investigated whether the effects of the awards persisted and influenced student attendance beyond the month of February. Our main interest lay in examining the consequences of the removal of the prospective award. As argued in the literature on motivational crowding-out (e.g., Gneezy et al., 2011), once incentives are removed, any potential deleterious effects of extrinsic incentives on people's intrinsic motivations to perform a behavior should become visible. Table 5 shows the impact of the treatments on student absences in the month of March. We first observe that the negative effects of the retrospective award (which

students had received at the end of January) on the number of days of absence are no longer statistically significant in March, $B = 0.036$, $SE = 0.028$, $FRT\ p = .209$. However, students who had been offered prospective awards for perfect attendance in February had worse attendance in March, the month after the incentives were removed. Compared to the control group, students assigned to the Prospective Award condition were absent 0.067 more days ($SE = .028$, $FRT\ p = .013$), which corresponds to a regression-adjusted 8.92% increase in absences in the month of March, or an unadjusted effect size of 0.049—an impact comparable to that of the retrospective award on absences in February. Figure 4 provides details on month-by-month attendance for each condition.

Table 5. Average Treatment Effect (ATE) on Student Absences in March (Prospective vs. Control and Retrospective vs. Control)

	Absences			Perfect Attendance		
	(1)	(2)	(3)	(4)	(5)	(6)
Prospective	0.073*	0.074*	0.067*	-0.058	-0.061	-0.058
	(0.030)	(0.029)	(0.028)	(0.041)	(0.042)	(0.043)
Retrospective	0.040	0.040	0.036	-0.029	-0.031	-0.027
	(0.029)	(0.029)	(0.028)	(0.041)	(0.042)	(0.043)
<i>N</i>	15,238	15,238	15,238	15,238	15,188	15,188
Control Mean	0.743	0.742	0.746	0.521	0.515	0.511
Strata	No	Yes	Yes	No	Yes	Yes
Covariates	No	No	Yes	No	No	Yes

Standard errors in parentheses.

Stratification variables were school and grade, controlled for as strata fixed effects.

March attendance data was not available for 91 students (0.59%).

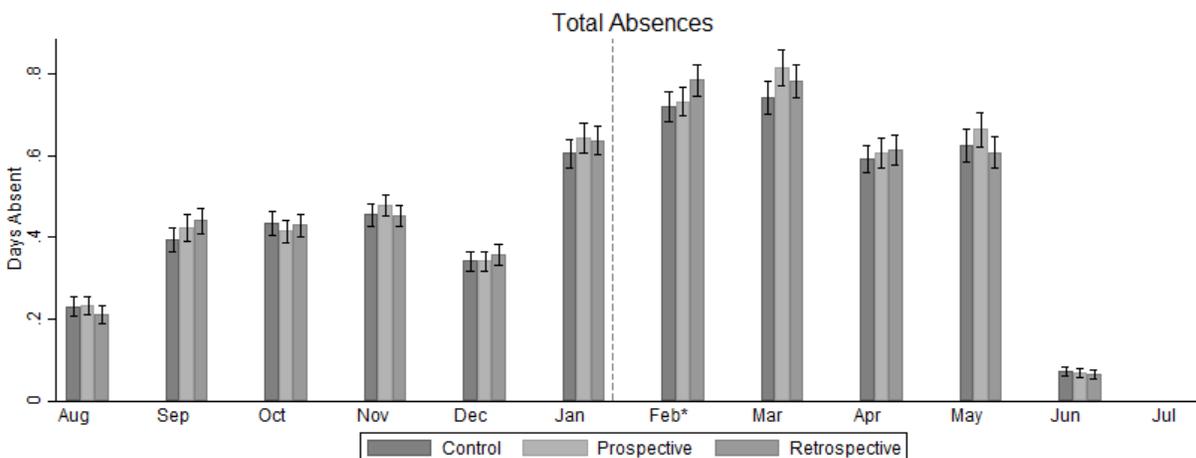
Covariates include indicators for English Language Learner (ELL), missing ELL status and language of the letters, as well as the number of absences in the pre-study months of September, October, and November.

Columns 1-3 coefficients are point estimates from OLS regression models. The associated p-values are from FRT.

Columns 4-6 coefficients (the estimated log-odds) and associated p-values are from logit regression models. Control means are in log-odds.

Columns 5 & 6 have fewer participants because a handful of small randomization strata perfectly predicted the outcome variable and were therefore dropped in the regression.

+ $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Figure 4. Monthly Total Absences by Experimental Condition

Average number of total absences by condition for each month. Dotted line represents the timing of the administration of treatments in January. *Total absences in February is the pre-specified dependent variable in this study. Error bars depict 95% confidence intervals.

4.3 Discussion

Counter to our expectations, we found that offering retrospective awards for prior attendance resulted in students attending *less* school in the following month. Our exploratory analysis suggests that this negative effect was particularly pronounced among academically low performing students, for whom the awards carried more unexpected news and who may have had a stronger motivation to interpret the award as a license to reduce costly effort by attending school less.

While offering prospective awards did not uniformly improve student attendance in the target month, an exploratory analysis suggests that younger students may have been motivated by the prospect of earning an award and improved their attendance, but the positive effect diminished as students grew older. Mirroring the effects of the Retrospective Award condition, once the incentive was removed, students in the Prospective Award condition became more likely to be absent in the following month. This suggests that the mere introduction of the awards

may have inadvertently signaled that the incentivized behavior, perfect attendance, was less common and less expectable than otherwise assumed.

Although the increase in students' absences was small, missing 8% more days of school in a month is cause for concern. For comparison, the most effective school attendance interventions to date only reduce absenteeism by 6-15% (e.g., Robinson et al., 2018; Rogers & Feller, 2018).

We conducted a follow-up experiment (Study 2) to complement the heterogeneity analyses and gain further insights about the underlying mechanisms that may explain the negative effects of awards. To establish these mechanisms after the field experiment had concluded, we needed to find an activity in a social context where people felt obligated to participate, but did not expect recognition for participating, and would have liked to participate as little as required.

5. Study 2: Exploring the Unintended Signals of Awards

We conducted an online experiment to examine signaling as a mechanism behind the unintended effects of awards found in our field experiment. While the introduction of both types of awards may have signaled that perfect attendance is not the norm and went beyond what is institutionally expected, the retrospective award should have sent a stronger signal to recipients about their own performance relative to these norms and expectations, thus giving them a license to exert less effort going forward.

5.1 Participants

We recruited 311 18- to 29-year-old participants on Amazon Mechanical Turk for a study that was described as asking questions about their opinions and attitudes. 42% of the participants identified as female and the average participant was 26 years old.

5.2 Procedures & Measures

After consenting to participate in the study, the Qualtrics platform randomly assigned participants to one of three conditions: the control group ($n = 104$), the *Prospective Award* condition ($n = 104$), or the *Retrospective Award* condition ($n = 103$). First, all participants read the following vignette:

Please imagine that you are a 10th grader living in a suburban town in California, near San Francisco. School started in late August. It is the end of January and you get home from school.

In the Prospective and Retrospective Award conditions, participants learned that they had a piece of mail waiting for them. In the Prospective Award condition, they were told about the opportunity to earn an award for their attendance in February. In the Retrospective Award condition, they received a retrospective award for their attendance in a prior month. The language of the letter mirrored that of the original field experiment and the award was designed to reflect the one actually received by students (see Figure 5).

After reading the vignette, all participants answered questions about how they thought their hypothetical absences compared to their classmates' absences, and about the school's expectations for their attendance. First, participants responded to the question, "How do you think your absences compare to those of your classmates?" Participants selected from three response options: *I had fewer absences than my classmates (I attended school more than my classmates)*, *I had about the same number of absences as my classmates (I attended school about as much as my classmates)*, and *I had more absences than my classmates (I attended school less than my classmates)*. We coded the response option *I had fewer absences than my classmates* as a 1 and the other two response options as 0.

Figure 5. Study 2 Intervention Materials Retrospective Award Condition:

Dear [NAME],

You have earned an award for having perfect attendance! You attended school every day for one month this school year during the fall semester.

To recognize that you had zero absences for one month, we present you with the Perfect Attendance Award, enclosed with this letter. The award will not be offered again this school year.



Prospective Award Condition:

Dear [NAME],

You can earn an award if you have perfect attendance in February!

If you have zero absences in February, you will receive the Perfect Attendance Award (like the one below). The award will be sent to you in the mail, and it will not be offered again this school year.

Sincerely,
County Superintendent

Sample Perfect Attendance Award:



Next, participants answered a question about their school's expectations for their attendance: "To what extent do you think your school expected you to attend school as much as you did in the Fall?" Participants responded on a 7-point Likert scale, ranging from 1 (*My school*

did not expect me to attend school as much as I did) to 7 (*My school very much expected me to attend school as much as I did*).

Before conducting the study, we pre-registered our design and hypotheses on *Open Science Framework* (<https://osf.io/rwcp3/>). Specifically, we predicted that participants who learned about receiving a retrospective award for attendance (i.e., the Retrospective Award condition) would be more likely to believe that they had fewer absences than their classmates and that the granting institution had lower expectations for their prior attendance, as compared to participants assigned to the Control group and the Prospective Award condition. We expected that there would be no significant difference between the Control group and Prospective Award condition, although the very existence of an award to incentivize attendance may signal that this behavior is not the norm and goes beyond what is expected.

5.3 Results

In line with our prediction, participants in the Retrospective Award condition were significantly more likely to assume that they had fewer absences than their classmates (93%), as compared to participants assigned to the Control group who did not learn about the award (38%) and the Prospective Award condition (65%). The differences between conditions were all statistically significant at the .001 level.

Participants in the Retrospective Award condition also perceived that the school had lower expectations for their attendance ($M = 4.60$, $SE = 0.18$) compared to participants in the control group ($M = 5.83$, $SE = 0.14$, $t(204) = 5.45$, $p < .001$, *Cohen's d* = 0.76) and the Prospective Award condition ($M = 5.30$, $SE = 0.17$, $t(202) = 2.84$, $p = .005$, *Cohen's d* = 0.40). The difference in perceived expectations between the Control group and Prospective Award

condition was smaller but also statistically significant ($t(204) = 2.37, p = .019, \text{Cohen's } d = 0.33$).

5.4 Discussion

Study 2 provides evidence that conformity to the perceived social norm of imperfect attendance and signals about low institutional expectations may be underlying mechanisms explaining why the awards decreased subsequent attendance compared to a control condition where no award was introduced.

As expected, the retrospective award appears to have sent a stronger signal about recipients' own performance relative to others' attendance and institutional expectations than the offer of a prospective award. When people feel that they have exceeded the expectations for a behavior that stands in conflict with more personally rewarding activities (e.g., leisure time), they may subsequently become less likely to perform that behavior (Blanken et al., 2015; Monin & Miller, 2001; Mullen & Monin, 2016). In our context, the retrospective award seems to have made recipients feel licensed to miss a future day of school because of what they perceived to be exceptionally low previous absences. The results for the Prospective Award condition moreover suggest that the mere introduction of an award for perfect attendance may have sent signals – albeit less strongly than the retrospective award – about the descriptive social norm and institutional expectations for attendance. This complements Study 1's exploratory analyses on the post-award period and may explain why the prospective award lead to increases in future absences once it was no longer offered.

6. General Discussion and Future Directions

In contrast to our prediction that both prospective and retrospective awards would improve attendance, we found that prospective awards did not on average motivate the target

behavior and retrospective awards even demotivated it. When students earned an unexpected retrospective award for positive prior attendance, they missed 8% more days of school in the following month. This negative effect was particularly pronounced for academically low performing students. Our survey experiment exploring the possible mechanisms behind these negative effects suggests that the retrospective awards may have inadvertently signaled that recipients were performing better than the descriptive social norm of their peers, and that they were exceeding the institutional expectations. In short, receiving the award gave students a license to miss more school.

Our exploratory analysis of the post-award period finds that negative effects also materialized in the Prospective Award condition after the award period ended. The mere introduction of awards seems to have signaled that perfect attendance was neither the norm nor expected, thus crowding out existing motivations to exert effort and attend school. This is an important finding, which should be studied in more detail in future field research. Our results have practical relevance given that most leaders and practitioners whom we surveyed in a separate study (see Supplemental Materials) reported using awards to motivate attendance, and almost none intuited that awards could demotivate the target behavior.

The present research differs from previous studies on several dimensions. First, as far as we know, our study is the first to examine the impact of both prospective and retrospective awards in the same field context. Second, most of the research to date has focused on relative performance awards based on outcomes (e.g., sales), while our study explores how awards for important but costly inputs (i.e., attendance) affect behavior. Third, our study examines mechanisms behind the effects of awards—in particular, inadvertent signaling and licensing.

While the study yields several novel findings, such that even retrospective awards can have unintended effects and that the mere introduction of non-financial awards can lead to motivation crowding-out, there are several limitations that we hope will be addressed in future research. First, the field experiment tests the impact of a single instance of offering students an award. In contexts where there is an ongoing interaction between the institution bestowing the award and the agent, the element of surprise may diminish after some time and the effect of the award may vary. However, as noted above, organizations frequently make efforts to vary the specific timing and form of awards, as well as the behavior being recognized (for a similar argument, see Bradler et al. 2016). Moreover, as online forms of collaboration and content creation such as Open Source Software production and User Generated Content platforms like Wikipedia become more prevalent, it will be particularly useful to understand the nuances of retrospective, unexpected social recognition. Contributors to these platforms frequently recognize one another's contributions in public and in retrospect – and previous work shows that such forms of recognition for behavior that enhances subjects' self- and social image can have positive and long-lasting effects (Gallus, 2017).

Second, we test a specific type of award that was mailed directly to students' homes, negating the public experience of receiving an award. It is possible that, in the same context, more visible awards could produce even stronger negative effects (in line with Bursztyn & Jensen, 2015). On the other hand, different types of awards (e.g., based on relative performance) or awards in domains considered important by recipients and their peers (e.g., academics, sports) might positively motivate recipients (e.g., Kosfeld & Neckermann, 2011). While we have focused on one dimension in the design of awards (announced versus surprise awards), we hope that future studies will test the real-world implications of modifying further dimensions, such as

varying the signal-worthiness of the underlying activity or the frequency of awards. In our context, some schools may have had other awards in place, which would most likely weaken the effects of the awards we tested.

Third, although the awards were sent directly to recipients' homes, we cannot entirely rule out spillover effects on non-recipients. Non-recipients who may have learned about the retrospective awards could have been more motivated to improve their attendance, which would change the implications of our findings. More generally, an important avenue for future research involves testing the effects of awards on non-recipients through field experiments.

Finally, while this study is among the first to provide empirical evidence on the effectiveness of prospective and retrospective awards in the same field context, the field needs more research evaluating the generalizability and boundary conditions of these findings. Other organizational settings may have different attendance norms and may be more or less heterogeneous in terms of people's preferences, motivations, and goals. In organizations where people share a mission to advance a common goal, even attendance awards, which do not signal their recipients' competence or skill, may have positive effects.

7. Conclusion

Our findings have implications for when and how different types of awards should be used to motivate desirable behaviors – and when they may backfire. Such boundary conditions have so far received only limited attention in the literature on organizational awards (Gallus & Frey, 2017). This study and its results provide an important cautionary note for the myriad organizations and leaders using awards. Awards are relatively cheap, easy to implement in institutions, and appear harmless. We find that awards can have more complicated consequences than might be intuitively expected. Contrary to pre-registered hypotheses, we observe the

counterproductive effects of awards: after the award period ends, students attend fewer days of school. We identify potential mechanisms, notably unintended signaling and licensing effects, which may mitigate and even undermine the potential benefits of awards.

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