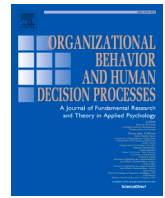




Contents lists available at ScienceDirect

Organizational Behavior and Human Decision Processes

journal homepage: www.elsevier.com/locate/obhdp

The limits of inconspicuous incentives

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ARTICLE INFO

Keywords:

Incentives
Inattentiveness
Salience
Field experiment
Management

ABSTRACT

Managers and policymakers regularly rely on incentives to encourage valued behaviors. While incentives are often successful, there are also notable and surprising examples of their ineffectiveness. Why? We propose a contributing factor may be that they are not sufficiently conspicuous. In a large-scale field experiment (Experiment 1) and three online experiments (Experiments 2–4), we show that even when incentives are transparently provided, failing to make them conspicuous vastly undermines their ability to shift behavior. Online experiments indicate that conspicuous incentives work by increasing people's extrinsic motivation to earn an incentive (Experiment 2) and do not merely serve as reminders to act (Experiment 3). We also assess whether people intuit that incentive conspicuousness matters (Experiment 4); nearly half of participants reject a costless opportunity to make their own incentives conspicuous, which leads them to earn less than they otherwise would. Yet, our results also hint at some degree of sophistication: those who benefit most from making incentives conspicuous are particularly likely to choose to make their incentives conspicuous.

1. Introduction

Managers and governments often rely on incentive programs to change people's behavior, deploying rewards to encourage valued actions such as charitable giving, home ownership, smoking cessation, vaccination, exercise, energy conservation, and savings. Although these programs are often highly successful (Bronchetti, Huffman, & Magenheimer, 2015; Charness & Gneezy, 2009; Imas, 2014; Royer, Stehr, & Sydnor, 2015; Sindelar, 2008; Volpp et al., 2008; Volpp et al., 2009), there are also notable and surprising examples of their ineffectiveness (e.g., Gneezy, Meier, & Rey-Biel, 2011). For instance, the Hope, Lifetime Learning, and American Opportunity tax credits all had a negligible effect on college enrollments, even though each program subsidized spending on higher education (Bulman & Hoxby, 2015; Long, 2004). One explanation could be that choosing to enroll in college is highly inelastic, but prior research has shown that college enrollment dramatically increases when tax professionals simply help families fill out financial aid forms (Bettinger, Long, Oreopoulos, & Sanbonmatsu, 2012).

Another puzzling failure of incentives occurred in a California program that offered electricity discounts for consumers who reduced their energy use by 20%. This program produced no measurable response (Ito, 2015), but mailings merely letting consumers know how their energy usage compared to that of their neighbors reliably and meaningfully reduced energy consumption (Allcott, 2011), as did messages associating pollution from energy usage with health and environmental problems (Asensio & Delmas, 2015). These examples and many others present a puzzle: why don't incentives to change highly elastic decisions always produce the desired response? After all, standard economic theory assumes that people will respond to positive incentives by increasing their engagement in the incentivized behavior (Mas-Colell, Whinston, & Green, 1995). In contrast, we show that information on available incentives may have little or even no effect *unless those incentives are made transparently conspicuous*. Unless made salient, incentives may fail to generate the extrinsic motivation they are intended to spark.

We begin by presenting a large-scale randomized field experiment

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Received 20 December 2018; Received in revised form 13 July 2022; Accepted 23 July 2022

Available online 13 September 2022

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(Experiment 1) to assess the effectiveness of conspicuous versus inconspicuous incentives on encouraging a highly consequential behavior: physical activity (425,000 Americans die prematurely each year due to physical inactivity and diet; Mokdad, Marks, Stroup, & Gerberding, 2004). A 40x increase in incentives was, in and of itself, insufficient to change behavior: only when the incentive increase was made conspicuous, via a low-touch email campaign, did it boost exercise (increasing daily step counts by 3–7%, depending on the estimation model, for every day of the two-week program and the two weeks that followed). Three online experiments replicate and extend this result. Specifically, we show that conspicuous incentives work by increasing people's extrinsic motivation to earn rewards (Experiment 2) and do not merely serve as reminders to act (Experiment 3). Finally, we assess whether people appreciate that incentive conspicuousness matters (Experiment 4); we find that many do not: almost half of people reject a costless opportunity to make their own incentives conspicuous, which leads to lost earnings.

Collectively, we show that when incentive programs are not made sufficiently conspicuous, even the best-intended, well-structured programs are likely to have far less impact than they could have. We contribute to the incentives literature by offering robust experimental evidence that incentive conspicuousness—over and above awareness that incentives exist—affects the capacity for incentives to shift behavior. We also highlight people's tendency to under-appreciate the importance of incentive conspicuousness.

1.1. Conceptual development

Prior work has explored how financial incentives can be deployed to support behavior change (for a review, see Kamenica, 2012).¹ This literature generally supports the conclusion that incentives “work” (i.e., they induce behavior change). However, incentives can backfire and dissuade people from engaging in a target behavior when they crowd-out intrinsic motives (Deci, 1971; Gneezy & Rustichini, 2000a), or when they are perceived as either inappropriately large (Baumeister, 1984; Ariely, Gneezy, Loewenstein, & Mazar, 2009) or inappropriately small (Gneezy & Rustichini, 2000b; Heyman & Ariely, 2004).

Despite their known limitations, incentives tend to produce meaningful behavior change in controlled experiments (Finkelstein, Linnan, Tate, & Birken, 2007; John et al., 2011; Volpp et al., 2008; Charness & Gneezy, 2009). But the incentives on offer in such studies are typically made extremely conspicuous. For example, in Volpp et al. (2008), a researcher informed participants of the incentives for behavior change via a one-on-one, face-to-face meeting that typically took thirty to sixty minutes. A take-home flyer reiterated this information. Moreover, participants also received daily text messages that reminded them of their earnings. This level of conspicuousness is common in field experiments intended to test the power of incentives as policy tools.

But what happens when incentives are not so conspicuously marketed? Arguably, the incentives we encounter in daily life—the fitness or savings subsidy offered by an employer, for example—tend to be buried in fine print on insurance plans or obscure websites. Given prior evidence of incentives' success in influencing behavior, one might expect rewards to be sufficiently motivating to overcome the attention problem. Yet, as recently developed economic models suggest, people often fail to pay attention to consequential financial information, even when that information is freely available. For example, Grubb (2015) proposes inattention as an explanation for the Federal Communications Commission's (FCC) recent attempts to reduce “bill shock,” or unexpected overage fees in the cell phone market. Even though cell phone usage data have long been freely available to customers at any time, consumers historically faced bill shock at high rates, which caused the FCC to compel U.S. cell phone carriers in 2011 to begin sending usage alerts

to customers when they exceed data limits (FCC, 2015). Similarly, Hanna, Mullainathan, and Schwartzstein (2014) show that people sometimes fail to learn not due to a lack of data, but due to a failure to notice the data in the first place.

Consistent with the idea that people may be inattentive to information about incentives, past research has provided initial evidence that information conspicuousness may influence behavior. For example, salience influences responses to taxation (Chetty et al., 2009; Finkelstein, 2009; Donnelly, Guge, Howell, & John, 2021; Taubinsky & Rees-Jones, 2017), purchasing decisions (Bordalo, Gennaioli, & Shleifer, 2013; Busse, Lacetera, Pope, Silva-Risso, & Sydnor, 2013), water usage (Tiefenbeck et al., 2018), and reactions to college rankings (Luca & Smith, 2013). Conversely, consumers' judgments and decisions can be affected by irrelevant but salient information (Dohmen, Falk, Huffman, & Sunde, 2006). Building on this prior work, we propose that the effectiveness of incentives depends not simply on incentives' presence or size, but sometimes more crucially on whether they are presented conspicuously. Specifically, we hypothesize that people will be less likely to engage in an incentivized behavior when incentives are inconspicuous than when they are conspicuous.

Why might increasing incentive conspicuousness boost a target behavior? Financial incentives have largely been thought to operate via external regulation, influencing people's extrinsic motives to earn the incentive (Ryan & Deci, 2000). Therefore, we explore whether making incentives conspicuous serves to augment such extrinsic motivation. Specifically, we test whether the effect of boosting incentive conspicuousness on target behavior uptake is mediated by an increase in extrinsic motivation to earn the incentive.

The present investigation, in which we manipulate incentive conspicuousness, is related to, yet distinct from, prior work showing that low-touch reminders can increase follow-through, (Briss et al., 2000; Ericson, 2017; Karlan, McConnell, Mullainathan, & Zinman, 2016; Karlan, Ratan, & Zinman, 2014; Rogers & Milkman, 2016; Shea, DuMouchel, & Bahamonde, 1996; Szilagyi et al., 2000). In contrast to reminders, which target forgetting, we propose that conspicuous incentives operate by drawing attention to desirable behaviors and thus increasing people's extrinsic motivation to engage in them.

Finally, we also explore whether or not people appreciate the value of conspicuous incentives—i.e., that boosting incentive conspicuousness can increase target behavior uptake. As people can be overconfident about their abilities to follow through on their intentions (Rogers & Milkman, 2016; Koehler, White, & John, 2011), we question whether people will choose to make their incentives conspicuous, even when doing so is costless. People may also fail to appreciate the impact of incentive conspicuousness on others' behavior. Indeed, recent research suggests that many observers do not appreciate the power of behavioral nudges in shaping behavior (Zlatev, Daniels, Kim, & Neale, 2017) and overestimate the degree to which others will respond to extrinsic incentives (Heath, 1999; Woolley & Fishbach, 2015). Such inaccurate beliefs may lead policymakers to design sub-optimal incentive schemes (Heath, 1999). Thus, we hypothesize that people may undervalue the power of conspicuous incentives to engage in a given behavior compared with when those incentives are inconspicuous.

2. Experiment 1: Field evidence

Our field experiment examines the impact of a conspicuous versus inconspicuous increase in incentives on physical activity. For two weeks, we offered users of a health rewards program a 40x increase in the incentives they were typically offered for their daily steps (tracked via pedometer), and we provided them with constant, real time access to information about the rewards they were accruing. For half of participants, we implemented a low-cost, eight-email campaign intended to increase the conspicuousness of the incentives. We predicted that daily step counts would be higher when the incentives were made conspicuous relative to when they were inconspicuous. Further, we predicted

¹ By “incentives,” we mean giving people cash for engaging in a target behavior (as opposed to say, offering taxes or subsidies for doing so).

step counts would be no higher among participants offered inconspicuous incentives than among participants in a holdout comparison group that received the standard (40x smaller) incentives.

2.1. Methods

2.1.1. Participants

Our study population was composed of users of Evidation,² an online reward platform for healthy activities run by Evidation Health. Evidation users could link their Fitbit pedometers with the Evidation platform and authorize their recorded step counts to be automatically transferred to their Evidation account. Every time Evidation users took 200 steps, they earned one point from the platform. Points were redeemable for cash rewards: after 200,000 steps, a user would earn \$1.00. Users received a check for every \$25 earned. Evidation sent all users (including all study participants throughout the course of the study) a weekly update email that contained information on a user's current number of unredeemed points. As described below, our study entailed boosting these incentives by a factor of 40, and manipulating the conspicuousness of this increase.

Of the Evidation users who had linked Fitbit devices, we selected 2,055 for our study based on three eligibility criteria. First, we excluded users who had participated in a previous study of incentives for exercise. Second, we excluded users whose historical usage data indicated that they opened fewer than one email per month from Evidation. This helped to ensure that users in the study actively checked their earnings balance emails from Evidation (in particular, ensuring that even those in the inconspicuous incentives condition would have easy access to information about the new incentives). Third, we excluded users who did not need to increase their daily step counts, which we measured through historical usage data that indicated they were above the 70th percentile for mean daily steps.

Based on our sample size and these exclusion criteria, we had 90% statistical power to detect a 15% difference between conditions in step counts over a two-week period using two-sided t-tests. Our experimental protocol was approved by the Institutional Review Board of a U.S. university. A waiver of informed consent was approved per Federal regulations (45 CFR 46.116(d)) given that the study was minimum risk, did not adversely affect the rights and welfare of participants, and could not be practicably carried out without the waiver.

2.1.2. Procedures

We randomly assigned Evidation users meeting eligibility requirements ($N = 2,055$) to one of two experimental conditions: a conspicuous incentives condition or an inconspicuous incentives condition. All study participants received the same incentive to walk: the number of Evidation points that they earned per step was multiplied by 40 for two weeks (that is, for every 200,000 steps, they earned \$40 instead of the usual \$1). Throughout the study, participants in the conspicuous incentives condition ($n = 1,027$) received a low-touch email campaign consisting of a kickoff email (described below) plus an email every other day detailing the duration and magnitude of these increased incentives. These emails were in addition to Evidation's standard weekly update emails containing the given user's point balance, which all participants received. Participants in the inconspicuous incentive condition ($n = 1,028$) were made aware of the increased incentives through their weekly points balance emails,³ in addition to having anytime access to

this information via the online portal. Thus, in both experimental conditions, participants had regular, on demand, easy access to an up-to-date report on the increased incentives they were earning. Critically however, participants in the inconspicuous incentives condition did not receive the low-touch campaign designed to increase the conspicuousness of these increased incentives.

Participants received increased incentives for walking for two weeks from January 27, 2015 through February 9, 2015. Participants in the conspicuous incentives condition received a kickoff email on January 26, 2015, the day before the start of the increased incentives. This email featured the subject line: "New Program to Encourage You to Walk (earn Bonus Points)." The contents of this email (depicted in Appendix, Fig. A1) explained to participants that they had been enrolled in a program to increase their walking. It showed a calendar with point multipliers highlighted on each day when they would earn increased incentives (every day for the next two weeks). Participants in the conspicuous incentive condition also received email notifications (depicted in Appendix, Fig. A2) about the program every other day for its duration (seven additional emails on days 1, 3, 5, 7, 9, 11, and 13 of the experiment), which contained all of the same information including the schedule of incentives depicted on a calendar.

No participants opted out of the incentive program. However, as we discuss further in the results section, some participants did not record daily steps on some days, either because they failed to wear Fitbits or because they did not sync their Fitbit data with Evidation. Other participants may have worn Fitbits only for a brief portion of a given day.

2.2. Statistical analyses

Our outcome variable of interest was daily steps taken. Participants' daily steps were tracked for three weeks before the intervention, two weeks during the intervention, and three weeks after the intervention. We tested for evidence of differential responses to the increased incentives by comparing mean daily steps between conditions during and after the intervention period.

Our statistical analysis strategy was a difference-in-differences approach. Like other standard difference-in-differences analyses (e.g., Pope & Pope, 2015), our analyses include covariates for experimental condition, temporal indicators (during-, and post-intervention), and the interaction of experimental condition and temporal indicators. We chose difference-in-differences analysis rather than a simple comparison of groups because it is effective at comparing the time changes in the means between groups, accounting for both group-specific and time-specific effects (Wooldridge, 2010). The coefficients of interest in regressions of this form are the interactions of experimental condition with an indicator for the experimental period. These coefficients measure the effect of the treatment at a given time period (Wooldridge, 2010). Importantly, our study uses random assignment to experimental condition, so experimental groups are the same in expectation (Athey & Imbens, 2006). Further, there were no significant differences in the observable pre-treatment characteristics of the two experimental groups (see Table 1).

We use ordinary least squares regressions for our analysis. To account for different walking levels on different days (due to weather, day of week, and seasonality), we include fixed effects for each date observed in the dataset. It would be natural to include user fixed effects in an analysis such as this one to account for different individual activity levels. However, in addition to varying in overall activity levels, individuals also vary substantially in their activity patterns throughout a given week. To account for such idiosyncrasies, we employ fixed effects that are more specific than user fixed effects. Specifically, we include fixed effects for an interaction of user with day of the week (Monday, Tuesday, etc.): seven fixed effects per individual. We also cluster standard errors at the user level to account for possible serial correlation within a user's daily steps over time. This analysis strategy increases our ability to identify experimental treatment effects.

² Evidation, my.evidation.com, formerly called "Achievemint" at the time of the field experiment.

³ Unfortunately, our field partner was unable to provide us with participants' email open rates during the intervention. However, we do have data on participants' pre-intervention propensity to open emails. This propensity did not differ between conditions, suggesting that participants in the inconspicuous condition were just as likely to open these weekly emails as those in the conspicuous condition. See Table 1.

Table 1

This table shows statistics from 3 weeks prior to the intervention up until the start of the intervention by experimental condition. Standard errors are in parentheses.

| | (1) Conspicuous | (2) Inconspicuous | (3) (1) vs. (2), <i>p</i> -value |
|---|---------------------|----------------------|-------------------------------------|
| Mean Daily Steps Pre-Intervention | 5,400.06 (89.40) | 5,577.47 (85.21) | 0.15 [<i>t</i> -test] |
| Fraction of People Who Recorded < 2,000 Steps on Any Day Pre-Intervention | 0.73 (0.01) | 0.69 (0.01) | 0.07 [proportions test] |
| Monthly Email Open Rate Pre-Intervention | 3.82 (2.51) | 3.93 (2.73) | 0.36 [<i>t</i> -test] |
| Kickoff Email Opened | 0.64 (0.02) | N/A | N/A |
| Opened At Least One Notification Email | 0.83 (0.01) | N/A | N/A |
| Observations | 1,027 | 1,028 | |

The following is the ordinary least squares (OLS) regression equation we used to estimate the coefficients shown in Table 2.

$$(1) \quad \text{daily_steps}_{it} = \beta_0 + \alpha_1 \text{user_x_day_of_week}_{it} + \alpha_2 \text{day}_t + \beta_1 \text{conspicuous_incentives}_i \times \text{during_treatment}_t + \beta_2 \text{conspicuous_incentives}_i \times \text{Oto2weeks_post_treatment}_t + \beta_3 \text{conspicuous_incentives}_i \times \text{more_than_2weeks_post_treatment}_t + \epsilon_{it}$$

In this equation, *conspicuous_incentives_i* is an indicator variable taking on a value of one if an individual, *i*, was in the conspicuous condition and zero otherwise; *day_t* is a fixed effect for each date included in the data; *user_x_day_of_week_{it}* is a fixed effect for user-day-of-week, and the other variables represent 0–1 indicators for whether an observation occurred during, within the first two weeks after, or during the third week after the intervention. The β_1 , β_2 , and β_3 coefficients therefore measure the differences between conspicuous incentives condition participants and inconspicuous incentive condition participants during, shortly after, and long after the experimental intervention.

In addition to our primary analyses, we conducted supplemental analyses to compare participants in our experimental groups with participants in a matched holdout group. For this group, we selected Evidation users who were excluded from our experiment only because they had been participants in a previous experiment, but who would not have been excluded otherwise (because they met all other inclusion criteria as experimental participants).⁴

The following is the OLS regression equation we used to estimate the effect of (a) an inconspicuous increase in incentives relative to receiving the standard, smaller incentives, and (b) conspicuously increased incentives relative to inconspicuously increased incentives. Coefficients from this regression analysis are shown in Table 3. This regression’s standard errors were also clustered by user and we again include user-day-of-week fixed effects as well as date fixed effects.

$$(2) \quad \text{daily_steps}_{it} = \beta_0 + \alpha_1 \text{user_x_day_of_week}_{it} + \alpha_2 \text{day}_t + \beta_1 \text{in_experiment}_i \times \text{during_treatment}_t + \beta_2 \text{in_experiment}_i \times \text{Oto2weeks_post_treatment}_t + \beta_3 \text{in_experiment}_i \times \text{more_than_2weeks_post_treatment}_t + \beta_4 \text{conspicuous_incentives}_i \times \text{during_treatment}_t + \beta_5 \text{conspicuous_incentives}_i \times \text{Oto2weeks_post_treatment}_t + \beta_6 \text{conspicuous_incentives}_i \times \text{more_than_2weeks_post_treatment}_t + \epsilon_{it}$$

⁴ Because difference-in-differences analyses can encounter problems when comparing groups with underlying differences, it is possible that our analyses using Equation 2 (found in Table 3) are biased. However, our matched holdout group was selected specifically to be similar to the experimental group in observable characteristics.

We also directly compare the effect of the conspicuous incentive condition to the holdout group using Wald tests of the coefficients from Equation 2 ($\beta_4 - \beta_1$, $\beta_5 - \beta_2$, and $\beta_6 - \beta_3$).

2.3. Results

2.3.1. Primary results

Fig. 1 shows the differences in mean daily steps taken by participants in the conspicuous and inconspicuous incentives conditions during and after our intervention. As predicted, Fig. 1 shows that users in the conspicuous incentives condition had a higher mean daily step-count both during and after the intervention relative to the inconspicuous incentives group.

To test the significance of these differences, we turn to regression analyses. Table 2 shows the results of our difference-in-differences OLS regressions predicting daily steps with fixed effects for each participant on each day of the week as well as date fixed effects (all calculated on an intent-to-treat basis). As predicted, participants in the conspicuous incentive condition took significantly more daily steps during the intervention than those in the inconspicuous incentive condition (an estimated 367 extra daily steps, representing a 7% increase, $p < .001$). Further, our regression estimates indicate that participants in the conspicuous incentives condition took more daily steps than those in the inconspicuous condition for the two weeks after our intervention (an estimated 332 extra daily steps, representing a 6% increase, $p < .01$), although the regression estimated effect dissipated three weeks after our intervention (an estimated 146 extra daily steps, *NS*).

2.3.2. Addressing alternative interpretations

We conducted a variety of additional analyses to address alternative explanations and to assess the robustness of our results (see Supporting Information Supplementary Analysis A). First, it is possible that conspicuous incentives simply prompted participants to wear their Fitbits more often, as opposed to actually changing their walking habits. A previous study of walking with an adult participant pool consisting of several thousand people with no attrition found that participants never took fewer than 2,000 steps per day (Hirvensalo et al., 2011). We therefore used a very conservative 2,000 steps as a cutoff for defining a participant an “attriter:” days on which a given user recorded fewer than 2,000 steps were regarded as a failure to properly wear and sync Fitbits (note that all reported results are stronger if we instead define attrition as 0 steps per day). Table 1 shows decent balance across experimental

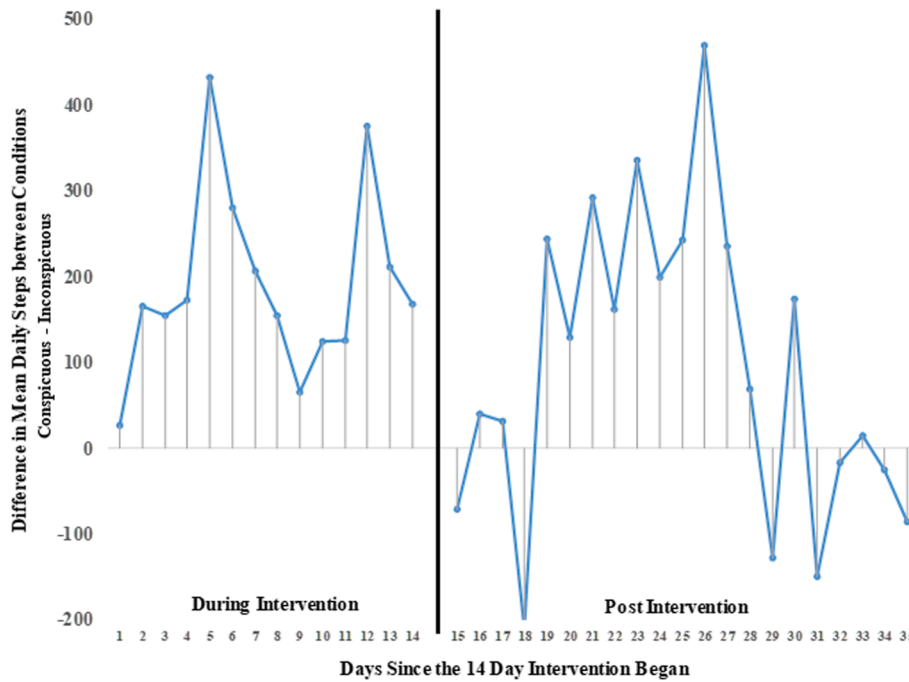


Fig. 1. Plot of the difference in mean daily steps taken by participants in the conspicuous incentives condition and the inconspicuous incentives condition, using raw data from during and after the intervention. (Experiment 1).

Table 2

This table reports coefficient estimates from an ordinary least squares regression model (see Equation 1) predicting daily steps taken by a given user. Robust standard errors are clustered by participant and reported in parentheses. The analyzed data include observations of participants' daily steps from the 21 days before the intervention, 14 days during the intervention, and 21 days after the intervention. This model is estimated using observations of all study participants (including participant-days with zero steps observed).

| | Daily Steps |
|---|-----------------------|
| (Conspicuous Incentive Condition) × (Treatment Period) | 367.06*** (105.71) |
| (Conspicuous Incentive Condition) × (0–2 Week Post-Treatment) | 331.92** (119.61) |
| (Conspicuous Incentive Condition) × (3 Week Post-Treatment) | 145.98 (141.36) |
| Fixed effects for day of the year | Yes |
| Fixed effects for (user) × (day of the week) | Yes |
| Observations | 115,080 |
| R-squared | 0.51 |
| Clusters | 2,055 |
| Subset of Observations: | All Observations |

Standard errors in parentheses.
 + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$.

conditions in the fraction of people who recorded < 2,000 steps on any day in each experimental condition during the 21 days pre-intervention. Supplementary Table S1 indicates that our analyses hold when we exclude attriters (i.e., people with very low daily step counts) or replace days with fewer than 2,000 daily steps with the average of the relevant individual's pre-intervention daily step counts stratified by day of week and excluding days with fewer than 2,000 steps.

Differential awareness of the incentives is another potential alternative interpretation. It is possible, for example, that the treatment not only increased the conspicuousness of the incentives, but also awareness of their existence. Indeed, although all participants could access information on increased incentives, only participants in the conspicuous condition received a kick-off email, which saliently introduced the increased incentives. Therefore, we assessed whether our results hold among a subset of participants likely to be aware of the incentive program. For those in the inconspicuous condition, the primary means of accessing the incentive information was via opening weekly emails. Our results hold when we restrict our analyses to a subset of participants who were particularly likely to have opened their weekly summary emails (which conveyed the 40x increase in incentives): those who had the highest email open rates prior to the experiment (see Supplementary Table S3).

2.3.3. Comparing the effect of increasing incentives to that of making this increase conspicuous

All results reported thus far have compared users between our two experimental conditions (conspicuous incentives and inconspicuous incentives). Both experimental groups, however, received the same increased incentives during our two-week intervention period. Thus, while we have measured the effect of making this increase conspicuous, we have not measured the effect of the increased incentives themselves.

Using a difference-in-difference estimation strategy, we compare the change in pre- versus during- and post-intervention behavior of participants in our inconspicuous incentives group with that of participants in a matched holdout group (described above). Table 3 shows the results of ordinary least squares regressions predicting daily steps with fixed effects for user-day-of-week and the calendar date. The key predictor variables are interactions between experimental status (in experiment, in conspicuous incentives condition) and different time periods (during- and post-intervention). Standard errors are clustered by participant and reported in parentheses.

The first three coefficients estimate the difference between inconspicuous incentive participants' and holdout users' mean daily steps at

Table 3

This table reports coefficient estimates from an ordinary least squares regression model (see Equation 2) predicting daily steps taken by a given user. Robust standard errors are clustered by participant and reported in parentheses. The analyzed data include observations of participants' daily steps from the 21 days before the intervention, 14 days during the intervention, and 21 days after the intervention, as well as the daily steps of a matched holdout group. This model is estimated using observations of all study participants (including participant-days with zero steps observed).

| | Daily Steps |
|---|-----------------------|
| (In Experiment) × (Treatment Period) | -110.04 (89.72) |
| (In Experiment) × (0–2 Weeks Post-Treatment Period) | -413.82** (129.58) |
| (In Experiment) × (3 Weeks Post-Treatment Period) | -130.16 (123.58) |
| (Conspicuous Incentive Condition) × (Treatment Period) | 406.42*** (106.05) |
| (Conspicuous Incentive Condition) × (0–2 Weeks Post-Treatment Period) | 337.04** (119.23) |
| (Conspicuous Incentive Condition) × (3 Weeks Post-Treatment Period) | 145.98 (141.04) |
| Wald Test ($\beta_4 - \beta_1$) Difference in Coefficients | 516.46** (174.17) |
| Wald Test ($\beta_5 - \beta_2$) Difference in Coefficients | 750.85*** (213.02) |
| Wald Test ($\beta_6 - \beta_3$) Difference in Coefficients | 276.14 (234.35) |
| Observations | 235,424 |
| R-squared | 0.33 |
| Clusters | 4,204 |

Standard errors in parentheses
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

each time period—they estimate the effects of the increased incentives after controlling for user-day-of-week and calendar date fixed effects. The second three coefficients estimate the difference between conspicuous incentive participants' and inconspicuous incentive participants' mean daily steps at each time period—they estimate the marginal effect of conspicuousness on responses to these increased incentives, again after controlling for user-day-of-week and calendar date fixed effects.

Table 3 indicates that inconspicuous incentive group participants' behavior did not differ from the behavior of holdout group participants during the intervention, though their behavior may have differed after the intervention in the opposite of the intended direction. Specifically, at 0–2 weeks post intervention, the model shows a statistically significant difference such that those in the inconspicuous incentive group exercised less than those in the holdout group. Supplementary Table S2 indicates that these results are similar when we exclude attriters (i.e., people with very low daily step counts) or replace days with fewer than 2,000 daily steps logged with the average of the relevant individual's pre-intervention daily step counts stratified by day of week and excluding days with fewer than 2,000 steps. Given that this result was not predicted a priori, and that it only surfaced in one of the models, we speculate that it is an anomaly; however, future research could ascertain whether it replicates (and if so, why).

We further assessed the effect of conspicuously increased incentives by directly comparing participants' activity in the conspicuous condition to the holdout group via Wald tests, also reported in Table 3. As predicted, participants in the conspicuous incentive condition took significantly more daily steps during the intervention than those in the holdout group, although this effect did not persist after the intervention.

2.4. Robustness tests

Although random assignment to conditions was successfully balanced on static traits, as reported in Table 1, we also conducted parallel trends analyses to test for pre-treatment equivalence in the trajectory of daily step counts between groups. The results are summarized in Supporting Information (SI) Table S4. The assumption of parallel pre-treatment trends is not well-supported in SI Table S4, suggesting that random assignment was unsuccessful on this observable dimension of our sample. Attrition (days when participants failed to wear their pedometers and record steps) is a large source of noise in our dataset and we suspected this noise could be driving the imbalanced parallel trends detected pre-treatment. In fact, a comparison of the proportion of pre-intervention days when participants took fewer than 2,000 steps revealed a significant difference between conditions ($M_{conspicuous} = 0.25$, $SD_{conspicuous} = 0.01$, $M_{inconspicuous} = 0.22$, $SD_{inconspicuous} = 0.01$; $t(2,053) = -2.74$, $p < .01$).

To test the possibility that participants who wore their pedometers unreliably introduced noise that led our data to fail the parallel trends test, we re-ran our parallel trends analysis excluding any participants whose steps were missing any day pre-treatment. When we do this, our data indeed supports the assumption of parallel pre-treatment trends. SI Table S5 shows that the parallel trends test is well-supported when we focus on this sub-population. SI Figure S1 depicts pre-treatment trends for all of our data and for the sub-sample including only users who wore pedometers every day pre-treatment. This figure depicts that parallel trends were present in the sub-population of participants who wore pedometers each day pre-treatment but not in the full population. To ensure imbalances in parallel trends could not be responsible for our primary findings, we repeated all of our analyses, only including the subset of participants who had no missing step count data pre-treatment (and whose parallel trends were balanced, as shown in SI Table S5). Our results are in SI Table S6 and SI Table S7, and are robust in this sub-population, suggesting that imbalanced pre-intervention step count trajectories cannot account for our findings.

We also conducted the following robustness checks (see Supporting Information): (1) we clustered data by user-day-of-week, using data only on experimental participants (SI Table S8); (2) we clustered data by user-day-of-week, using data on experimental participants together with the matched holdout group (SI Table S9); (3) we clustered data by user, winsorizing step counts at the 99% level, using data only on experimental participants (SI Table S10); (4) we clustered data by user-day-of-week, winsorizing step counts at the 99% level, using data only on experimental participants (SI Table S11); (5) we clustered data by user, winsorizing step counts at the 99% level, using data on experimental participants together with the matched holdout group (SI Table S12); and (6) we clustered data by user-day-of-week, winsorizing step counts at the 99% level, using data on experimental participants together with the matched holdout group (SI Table S13). In each of these models, users in the conspicuous incentives condition take more daily steps than users in the inconspicuous incentives condition both during and up to two weeks after the experimental intervention, with $p < .05$ for both of these coefficients in each model.

2.5. Discussion

Our field experiment supports our contention that incentives alone can be insufficient to change behavior: physical activity levels were no higher among participants who were inconspicuously offered a 40x increase in incentives than among those in a holdout comparison group who received the standard, much smaller, incentives. By contrast, a simple and inexpensive email campaign designed to make these increased incentives more conspicuous was sufficient to unlock their power and meaningfully change behavior for the better. However, it is possible that the effect of conspicuous incentives was driven by increased awareness (though the supplementary analyses presented in

supplementary information Table S3 indicated that the effect of conspicuous incentives holds when restricting the sample to those most likely to have been aware of the incentives). In addition, Experiment 1 could be said to be testing the effect of making an *increase* in incentives conspicuous, as opposed to the “pure effect” of incentive conspicuousness. Experiment 2 addresses these possibilities, by testing our basic hypothesis in a tightly controlled online experiment.

3. Experiment 2: Mechanism

Experiment 2 is a conceptual replication of Experiment 1 and assesses a process by which incentive conspicuousness increases uptake of a targeted behavior. Specifically, Experiment 2 explores whether conspicuous incentives work by heightening extrinsic motivation to earn incentives. This study was preregistered (https://aspredicted.org/47R_X19), and the data and materials for this experiment and the remaining experiments are posted at <https://osf.io/tnxvp/>.

Experiment 2 tests a more conservative manipulation of incentive conspicuousness and hence, provides a stronger test of our basic prediction that people are less likely to engage in an incentivized behavior when incentives are inconspicuous than when they are conspicuous. Specifically, in Experiment 2, all participants encountered a description of their incentives (whereas in Experiment 1, participants in the inconspicuous condition had to open a weekly email tabulating their earnings; or login to an app, to learn about their incentives). For half of participants, we made this information more conspicuous by simply presenting it on each page of the study.

3.1. Method

Six hundred and eight participants were recruited online from Amazon’s Mechanical Turk (mTurk) (51.0% male, $M_{age} = 40.9$ years, $SD = 11.9$) and told upfront that they would be given a \$0.40 payment for completing the study, which entailed completing a multiple choice opinion survey. They were also told that they would have the opportunity to earn an additional bonus payment of up to \$0.16. Participants were told the following, about the (somewhat peculiar) way they could earn the bonus:

During this survey, you will also have an additional task. 4 of the survey questions will include an option for you to fill in an open-ended text box. For each of these questions, please enter the phrase “seagulls fly over the sea” in the open-ended response. For each open-ended text box you fill in, you will earn an additional \$0.04.

Below this description of the incentivized behavior, participants were shown a screenshot example of the kind of open-ended text box they would need to fill in to earn the bonus payments (see Fig. A3). This information was presented on a single page with no other information, meaning that the information about how to earn the bonus incentive was clearly conveyed to all participants.

Next, participants responded to a 14-item multiple choice opinion survey about politics and news, with items sourced from the Pew Research Center, a nonpartisan research organization (<https://www.pewresearch.org>). We asked questions such as, “How much would you say you know about science?” with response options: “a lot,” “some,” “not much,” and “nothing at all.” Each item from the multiple-choice survey was presented on its own page. Four questions included an “other” multiple choice option with an open-ended text box next to it, where participants could write the bonus phrase. Critically, for half of participants (those in the *conspicuous condition*), we included the sentence “You can earn an additional four cent bonus for each question with an open-ended response by selecting the open-ended option and writing

in ‘seagulls fly over the sea.’” at the top of each page in the survey (see Fig. A3).⁵

After completing the opinion survey, participants responded to four items intended to measure what motivated them to engage in the incentivized behavior (i.e., writing in the bonus phrase). Specifically, and drawing on self-determination theory (Ryan & Deci, 2000; Ryan & Connell, 1989), we test whether the effect of conspicuous incentives is driven by increased enjoyment of performing the task (i.e., intrinsic motivation) or by an increased desire to satisfy a goal that coincides with completing the task (i.e., extrinsic motivation). We asked participants to indicate the extent to which they agreed with the statements (in randomized order): “In this study, I wanted to write in the bonus phrase because...”.

- 1) “I wanted to earn the money”
- 2) “I felt the researchers wanted me to”
- 3) “it was personally important to me”
- 4) “I enjoyed it”

Participants responded to these items on a scale of 1 = strongly disagree to 5 = strongly agree. Given that we expected conspicuous incentives to operate via extrinsic motivation, three of these four process measures focused on extrinsic motivation. Specifically, they assess three distinct possible extrinsic motives: 1) a desire to earn the external incentive, 2) a desire to please the researchers, and 3) a desire to accomplish a task deemed to be of personal importance. The fourth item assessed intrinsic motivation. Finally, all participants indicated their age and gender.

3.2. Results and discussion

Participants in the *conspicuous condition* were more likely to engage in the incentivized behavior (i.e., to write in the bonus phrase; $M = 3.46$ bonus phrase write-ins across four opportunities, $SD = 1.20$) than those in the *inconspicuous condition* ($M = 2.83$ bonus phrase write-ins, $SD = 1.61$; $t(606) = 5.45$, $p < .01$, $d = 0.44$), earning an average of \$0.025 (22%) more in bonus pay.⁶ Participants in the *conspicuous condition* also indicated significantly higher motivation to earn the money (item 1, i.e. “I wanted to earn the money”) ($M = 4.70$, $SD = 0.64$) than those in the *inconspicuous condition* ($M = 4.56$, $SD = 0.88$), $t(606) = 2.39$, $p = .02$, $d = 0.19$. In contrast, measures of other types of motivation did not differ between conditions (item 2, i.e., “I felt the researchers wanted me to”: $M_{conspicuous} = 4.00$, $SD_{conspicuous} = 1.13$, $M_{inconspicuous} = 3.94$, $SD_{inconspicuous} = 1.29$, $t(606) = 0.54$, $p = .59$, $d = 0.04$; item 3, i.e., “it was personally important to me”: $M_{conspicuous} = 2.54$, $SD_{conspicuous} = 1.32$, $M_{inconspicuous} = 2.64$, $SD_{inconspicuous} = 1.42$, $t(606) = 0.91$, $p = .36$, $d = 0.07$; and item 4, i.e., “I enjoyed it”: $M_{conspicuous} = 2.91$, $SD_{conspicuous} = 1.20$, $M_{inconspicuous} = 2.79$, $SD_{inconspicuous} = 1.31$, $t(606) = 1.14$, $p = .25$, $d = 0.09$).

Following our preregistration, we assessed each of the motivation items as mediators of the effect of incentive conspicuousness on uptake of the target behavior using the bias-corrected bootstrap method recommended by Preacher and Hayes (2004) with 10,000 bootstraps (Hayes, 2017, PROCESS Model 4). The 95% confidence interval for the indirect effect of incentive conspicuousness on uptake of the target behavior through the desire to earn the money (i.e., item 1 in the list above) did not contain zero [0.020, 0.209], providing evidence of significant mediation (Fig. 2).

The remaining three motivation items did not significantly mediate

⁵ The bonus phrase was displayed as a photo so that it could not be copy and pasted by participants.

⁶ Six participants wrote in variations of the requested bonus phrase (e.g. “seagulls fly over sea,” “the seagulls fly over the se”); these participants were considered as having engaged in the incentivized behavior (our results are similar when these entries are counted as incorrect).

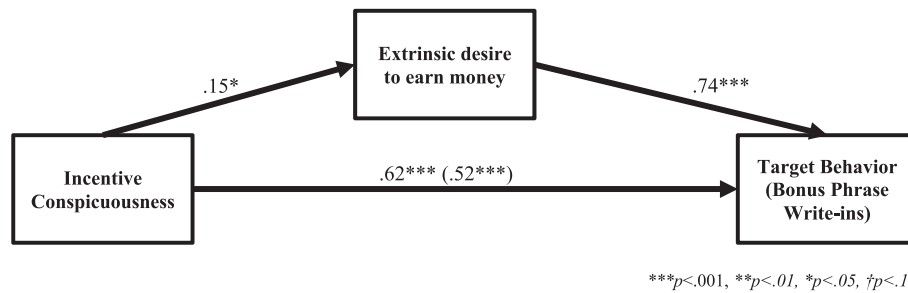


Fig. 2. Mediation of Incentive Conspicuousness on Uptake of the Target Behavior through Extrinsic, External Motivation (Desire to Earn the Money) (Experiment 2).

the effect (item 2: 95% CI: [-0.031, 0.062]; item 3: 95% CI: [-0.019, 0.010]; item 4: 95% CI: [-0.009, 0.027]). Participants' desire to earn money (item 1) also significantly mediated the effect of incentive conspicuousness on uptake of the target behavior both when (1) the other three motivation items were included in the model as simultaneous mediators (95% CI: [0.016, 0.195]), and (2) when the other three items were included in the model as control variables (95% CI: [0.013, 0.184]).

These results suggest that incentive conspicuousness can increase uptake of an incentivized behavior by increasing people's extrinsic desire to earn rewards. Notably, these results also suggest that our findings are unlikely to stem from a demand effect; if a demand effect were central to our results, we would have expected a significant difference in participants' endorsement of the statement "I felt the researchers wanted me to." However, participants did not report differences in this measure. That said, there is still the possibility that participants in the conspicuous condition nonetheless acted out of a desire to please the researchers, but that they were unaware of this desire (Nisbett & Wilson, 1977), or uncomfortable admitting to it (Edwards, 1957).

In the supplement, we report two additional studies, using the same experimental paradigm, that replicate the effects demonstrated in Experiment 2. Experiment S1 was a confirmatory version of Experiment 2, in which we explicitly predicted that the effect of conspicuous incentives would be mediated by heightening the desire to earn money (https://aspredicted.org/36K_QKN, see SI S1). First, the basic effect replicated: the conspicuous incentive facilitated the target behavior more so than the inconspicuous incentive ($M_{conspicuous} = 3.36$ bonus phrase write-ins, of four opportunities, $SD = 1.24$, $M_{inconspicuous} = 2.94$ bonus phrase write-ins, $SD = 1.59$), $t(1,008) = 4.72$, $p < .001$, $d = 0.30$). Second, while the effect of incentive conspicuousness on the desire to earn money was directionally the same as Experiment 2, here, it was only marginally significant ($M_{conspicuous} = 4.65$, $SD_{conspicuous} = 0.73$, $M_{inconspicuous} = 4.55$, $SD_{conspicuous} = 0.89$, $t(1,008) = 1.88$, $p = .061$, $d = 0.12$). Accordingly, the mediation analysis was significant at the alpha = 0.10 level (PROCESS Model 4, 90% CI: [0.008, 0.118]), but not at the alpha = 0.05 level (95% CI: [-0.002, 0.128]).

In a second additional study (<https://aspredicted.org/blind.php?x=ys8st5>, see SI S2), we required all participants to pass a quiz question about the incentives prior to proceeding with the task. This setup further ensures that all participants—even those in the inconspicuous incentives condition—were aware of the incentives. Even in this very conservative set-up, the basic effect of incentive conspicuousness replicated.

Finally, a third study (<https://aspredicted.org/blind.php?x=63zy8z>, see SI S3) replicated the effect of incentive conspicuousness with a different manipulation of conspicuousness: using a bold, highlighted

font to convey incentive information (as opposed to by repeating the incentive information on each page, as in Experiments 2, S1, and S2).

These studies provide converging evidence for our basic hypothesis, namely, that making incentives conspicuous increases target behavior uptake. Further, they provide evidence of a psychological process underlying the effect. Finally, these experiments also address potential concerns that the results of our field experiment are specific to the behavior in question (walking), to the experimental context, or the interaction between them.

4. Experiment 3: Specificity of the effect

So far, we have shown that incentives are more effective at shaping behavior when they are conspicuously presented. However, it could be argued that conspicuous incentives may also serve as salient reminders to engage in the target behavior. Thus, in Experiment 3, we assess whether a conspicuous incentive outperforms a conspicuous reminder to engage in the target behavior.

The experiment was a 2x2 design in which we manipulated both conspicuousness and the presence of a meaningful incentive. It spanned two days; on Day 1, participants were informed of the incentive they could earn if they returned on Day 2 to complete a short task. Half of participants could earn a meaningful incentive (\$0.30) for returning; the other half of participants could merely earn a trivial or "placebo" incentive (\$0.01) for returning. The latter condition is essentially a reminder or a "placebo" incentive because earning a penny is so inconsequential, yet it allowed us to hold all of the language throughout the study constant. This manipulation was crossed with a manipulation of conspicuousness, whereby half of participants received a message on the second day emphasizing the incentives for which they were eligible. In essence, this meant that those in the placebo incentive condition effectively received merely a reminder to return, whereas those in the meaningful incentive condition had that incentive made conspicuous via a reminder. We predicted an interaction such that making a meaningful incentive conspicuous would increase target behavior uptake relative to all other conditions. This prediction also helps to address the possibility of experimental demand; such an effect should manifest as a main effect of receiving a reminder rather than an interaction effect. This study was pre-registered (https://aspredicted.org/GQF_T9R).

4.1. Method

Seven hundred and ninety-nine participants were recruited online from Amazon's Mechanical Turk (mTurk) to complete a two-day study for \$0.40 (53.6% male, $M_{age} = 37.4$ years, $SD = 11.5$). On day 1, all participants were told:

Today, you will be asked to view and describe one photo. Then, you will be asked to return tomorrow ([date]) between 10am and 8 pm EST to a link to describe a different photo. The link will be provided on the final page of this study. Please copy and save it so that you will be able to complete the second photo description tomorrow.

Participants were told that they would either earn a meaningful incentive (\$0.30) for returning and completing the second photo description, or that they would earn a placebo incentive (\$0.01). Specifically, participants were told:

If you return to the study tomorrow and complete the second photo description, you will earn an additional \$0.30 (\$0.01). The \$0.30 (\$0.01) will be paid as a bonus to this HIT.

Then, to ensure participants understood the incentive structure, participants were required to correctly pass four comprehension questions to continue (see Appendix A4). Next, participants were shown a photo and described it in an open-ended text box. Then, participants indicated their age and gender. On the final page, participants were provided with a link to return to complete the second photo description task on the following day:

To complete the second photo description task, please return to the link below tomorrow, ([the date of the following day]), between 10am and 8 pm EST. Please copy and save this link so that you will be able to complete the second photo description tomorrow.

[link] If you return to the study tomorrow and complete the second photo description, you will earn an additional \$0.30 (\$0.01). The \$0.30 (\$0.01) will be paid as a bonus to this HIT.

On the following day, participants who had been randomized into the conspicuous conditions received the following message:

This HIT is to bring to your attention to the fact that you can earn an additional \$0.30 (\$0.01) today as the 10am-8pm window has opened to participate in the second part of the photo description study. As a reminder, you will not be penalized in any way if you do not return to the link to complete this task. The link is:

[link] If you complete and submit this photo description, you will receive \$0.30 (\$0.01) paid as a bonus to the HIT you completed yesterday.

Those in the inconspicuous condition did not receive this message. The primary outcome was whether or not each participant came back and described the photo within the allocated time window.

4.2. Results and discussion

As preregistered, we estimated a logistic regression predicting participants' uptake of the incentivized behavior with an indicator for assignment to the real incentive condition, an indicator for assignment

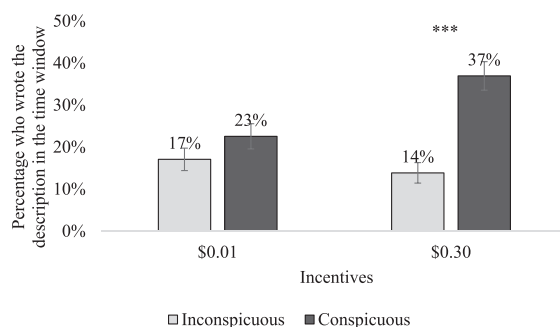


Fig. 3. Interaction between conspicuousness and incentives on uptake of incentivized behavior (Experiment 3) Notes: Error bars represent ± 1 s.e.; * $p < .05$, ** $p < .01$, *** $p < .001$.

to the conspicuous condition, and an interaction between the real incentive condition indicator and conspicuous condition indicator. The results revealed no main effects of either conspicuousness (logistic $\beta = 0.35$, $se = 0.26$, $p = .17$) or incentives (logistic $\beta = -0.25$, $se = 0.28$, $p = .37$). Critically however, there was an interaction (logistic $\beta = 0.95$, $se = 0.36$, $p < .01$). Specifically, conspicuousness only increased target behavior uptake when the incentive was meaningful (logistic $\beta_{conspicuousness} = 1.29$, $p < .001$); it had no effect in the placebo incentive condition (logistic $\beta_{conspicuousness} = 0.35$, $p = .17$), see Fig. 3.

In sum, Experiment 3 suggests a benefit of incentive conspicuousness in particular; target behavior uptake was higher in the meaningful incentive conspicuous relative to all other conditions.

5. Experiment 4: Demand for conspicuous incentives

In Experiment 4, we assessed people's demand for conspicuous incentives. Specifically, we assessed whether a nontrivial proportion of study participants would undervalue conspicuous incentives by choosing not to make incentives conspicuous—even when doing so was costless and would improve their outcomes. We used the same experimental paradigm as in Experiment 2: participants could earn bonus payments for writing a special phrase into response boxes. After reading about how to earn these bonus payments, participants were given the option of having this information displayed on every page of the survey (i.e., they were given the option to make the incentives conspicuous). Half of participants were randomized *not* to receive their chosen option (i.e., half of those who had opted out of making the incentives conspicuous in fact received conspicuous incentives, and vice versa). This design enabled us to test the wisdom or error of people's decisions about whether to make their incentives conspicuous. Experiment 4 also provides an additional test of our basic prediction that conspicuous incentives improve performance). This study was preregistered (<https://aspredicted.org/blind.php?x=wn3dp2>).

5.1. Method

Participants ($N = 1,013$; 46.5% male, $M_{age} = 36.9$ years, $SD = 13.0$) were recruited online from Prolific Academic. Participants were guaranteed a \$0.64 payment and told they could earn a bonus payment of up to \$0.16.

Similar to Experiment 2, participants were told that they would answer a short survey in which they would have four opportunities to earn an additional \$0.04 bonus payment by selecting the "other" option in response to a question and writing "seagulls fly over the sea" into the open-ended response area. Also consistent with Experiment 2, participants then viewed an example of an open-ended response question with the bonus phrase written in and were required to pass a quiz (the same quiz as in Experiment 2) ensuring they understood the bonus information. Next, they were asked:

If you want, we can repeat the bonus instructions at the top of each page of this survey. Do you want us to do this?

The response options were: "Yes, repeat the instructions at the top of each page" and "No need to repeat the instructions at the top of each page."

Next, all participants were told:

Please note - we have been experiencing some occasional glitches with this survey in that sometimes the choice you just made isn't properly implemented - i.e., sometimes the instructions are not repeated for those who have chosen to have them repeated; and sometimes the instructions are repeated for those who have chosen not to have them repeated. If this error occurs for you, we sincerely apologize. Note that you will still receive any bonus payments you're eligible for.

At this point, participants proceeded with the same, 14-item multiple choice opinion survey as in Experiment 2. Also as in Experiment 2,

participants were randomized to either a conspicuous incentives condition (in which the instructions “You can earn an additional \$0.04 bonus for each question with an open-ended response by selecting the open-ended option and writing in ‘seagulls fly over the sea’” appeared at the top of each page of the opinion survey); or an inconspicuous incentives condition, in which these instructions did *not* appear at the top of each page. In effect, this meant that approximately half of participants had their choice—of whether to make the incentives conspicuous—implemented; the others were randomized to receive the option they had forgone. After completing the opinion survey, all participants indicated their age and gender and were debriefed that the “glitch” was intentional.

5.2. Results and discussion

Although doing so was costless, only 53.6% of participants opted to view conspicuous incentives (i.e., requested to view their bonus instructions on each page of their survey). A binomial test indicated this percentage was significantly below 100%, $p < .001$. That is, a significant proportion of participants (46.4%)—almost half, in fact—chose *not* to make their incentives conspicuous, providing support for our prediction that many people undervalue incentive conspicuousness.

In exploratory (i.e., non pre-registered) analyses, we assessed the wisdom or error of this choice by conducting a 2x2 ANOVA of incentive conspicuousness and participant opt-in preference. First, a main effect of incentive conspicuousness indicated that, consistent with Experiments 1–3, conspicuousness matters: performance was better when incentives were conspicuous ($M = 3.21$ bonus phrase write-ins, $SD = 1.33$) relative to when they were not ($M = 2.83$ bonus phrase write-ins, $SD = 1.61$, $F(1, 1,012) = 16.15$, $p < .01$, $\eta^2 = 0.02$). There was also a significant main effect of opting in, such that those who opted into receiving conspicuous incentives performed worse ($M = 2.83$ bonus phrase write-ins, $SD = 1.59$) than those who did not opt in ($M = 3.24$ bonus phrase write-ins, $SD = 1.33$, $F(1, 1,012) = 19.59$, $p < .01$, $\eta^2 = 0.02$). Interestingly, there was a marginally significant interaction, $F(1, 1,012) = 2.90$, $p = .09$, $\eta^2 < 0.01$.

Turning back to our pre-registered analyses, making incentives conspicuous benefitted both those who opted to receive them ($M_{chose_and_received_conspicuous} = 3.09$ bonus phrase write-ins, of four opportunities, $SD_{chose_and_received_conspicuous} = 1.42$; $M_{chose_conspicuous_received_inconspicuous} = 2.57$ bonus phrase write-ins, $SD_{chose_conspicuous_received_inconspicuous} = 1.71$; $t(541) = 3.91$, one-tailed $p^7 < .01$, two-tailed $p < .01$, $d = 0.33$), as well as those who opted *not* to receive them ($M_{conspicuous} = 3.34$ bonus phrase write-ins, $SD_{conspicuous} = 1.21$; $M_{inconspicuous} = 3.13$ bonus phrase write-ins, $SD_{inconspicuous} = 1.43$; $t(468) = 1.75$, one-tailed $p = .04$, two-tailed $p = .08$, $d = 0.16$).⁸ However, as is apparent from Fig. 4, the marginally significant interaction in our exploratory analysis suggests that the benefit may be larger for those who opted into incentive conspicuousness relative to those who did not. Thus, in addition to supporting our key prediction—that the conspicuous incentives would benefit both those who choose, and those who choose not, to make incentives conspicuous—the marginally significant interaction points to a degree of sophistication. Namely, it suggests that those who stand to benefit from this intervention are, to some degree, aware of this fact.

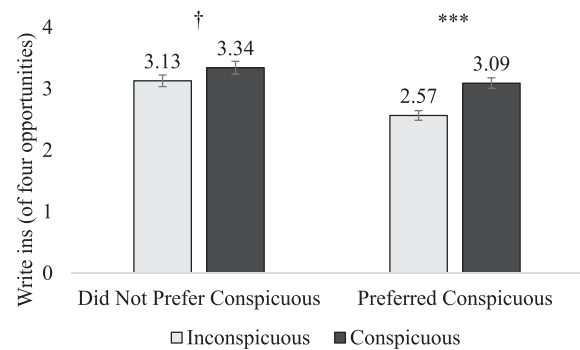


Fig. 4. Plot of the average number of incentivized actions taken across conditions (Experiment 4). Notes: Error bars represent ± 1 s.e.; Preregistered one-sided p -values are reported in the text, but two-sided p -values are shown in the figure; two-sided $^\dagger p < .10$, $*p < .05$, $**p < .01$, $***p < .001$.

6. General discussion

Our investigation advances research on incentives in three primary ways. First, we illuminate conspicuousness as a new and important boundary condition to incentive effectiveness. Although prior work has illustrated that information salience shapes consumer behavior (e.g. Finkelstein, 2009; Luca & Smith, 2013; Taubinsky & Rees-Jones, 2017), it would be reasonable to expect the opportunity to earn incentives might be immune to attention problems. Prior experimental research on incentives has largely tested their effects in settings where information about payments was made extremely salient (e.g. Charness & Gneezy, 2009; Volpp et al., 2008). This was for good reason. This past work sought to test the impact of incentives, not the means by which information about incentives was delivered. However, our findings suggest that when they are inconspicuous, incentives do not increase extrinsic motivation to the same extent, which vastly limits their influence on behavior. Second, we provide evidence of why conspicuous incentives work—they heighten people’s extrinsic desire to earn rewards. Finally, we provide insight into whether people intuit that incentive conspicuousness matters. Our findings suggest that many people do not appreciate that making incentives conspicuous dramatically increases their potency. This naiveté suggests many incentive programs may be underused in part because their designers fail to fully appreciate the importance of making incentives themselves more conspicuous (in fact, in a supplementary study we show this is the case; see Supporting Information Study S2).

One symptom of insufficient appreciation for incentive conspicuousness could be a general lack of awareness of incentive programs, even among their intended beneficiaries. To that end, we polled a nationally representative sample of 106 U.S. citizens (see Supporting Information Study S4, Table S14 and Table S15 for details) and assessed their knowledge of a wide range of existing U.S. federal incentive programs. Respondents were presented with a list of 35 real subsidy programs and tasked with identifying which were offered by the U.S. government (versus only in other countries). On average, participants did not perform better than chance; they correctly categorized only 63% of the incentive programs (20 were U.S. programs, 15 were not). More importantly, only 66% of those who reported being eligible for a given U.S. incentive program were actually aware of it before taking our survey, and only 55% reported actually taking advantage of those subsidies for which they were eligible. Given our research indicating that inconspicuous incentives are far less effective than conspicuous ones, these survey results suggest that governments may be influencing far fewer behaviors through policy than they could be if their incentive programs were more aggressively promoted.

⁷ Because we anticipated a directional effect (i.e. that incentive conspicuous would improve participant uptake of the target behavior), we preregistered one-tailed tests; however, we also report the two-tailed results.

⁸ Fifty-six participants wrote in variations of the requested bonus phrase (e.g. “seagulls fly over the ocean,” “the seagulls fly over the sea”); these participants were given credit for engaging in the incentivized behavior (our results are similar when these entries are counted as incorrect).

6.1. Opportunities for future research

We see several opportunities for future research. We have shown that conspicuous incentives work in part by augmenting people's extrinsic motivation to earn the incentive. Future research could also explore additional, complementary, mechanisms that may undergird the effect of making incentives conspicuous. For example, conspicuously-presented information may be more memorable, enabling people to more easily recall incentives when they become applicable (Rogers & Milkman, 2016). Another possibility is that being exposed to conspicuous incentives over time, as was the case in our field study, could trigger loss aversion, spurring people to action (Kahneman, 2011) when failure to engage in the behavior is coded as a loss (more so than it might when the incentives are inconspicuous). Exploring these possibilities would be valuable.

In addition to showing that conspicuousness matters, we also found that people tend to undervalue conspicuousness; future research could seek to uncover why. One possibility is that overconfidence and over-optimism are at play (Moore & Healy, 2008; Weinstein, 1980). There may also be a 'curse of knowledge' type phenomenon: when people are well aware of the existence of these programs either because they've just been described or perhaps because they designed the program, it may impede their ability to empathize with the state of would-be recipients, who may themselves be overconfident about their abilities to take advantage of available incentives (Moore & Healy, 2008; Rogers & Milkman, 2016). Our findings are consistent with past research suggesting that we are often unsophisticated about the degree to which others will exhibit bias (Zlatev et al., 2017; Heath, 1999). Future research exploring these ideas further would be valuable.

However, in addition to showing that almost half of people forgo a costless opportunity to make their incentives conspicuous, Experiment 4 also suggests that people may have *some* degree of sophistication—in the sense that those who chose to make their incentives conspicuous were also the ones who benefited the most from this intervention. This exploratory finding is intriguing, especially when contrasted against prior work pointing to people's lack of self-awareness about their (in)ability to exert self-control (O'Donoghue & Rabin, 1999). Additional research is needed to understand when and why people have—versus do not have—insight into their own shortcomings, including, as is our focus, their (in)attentiveness.

Finally, future research may also test the boundaries of the effects of making incentives conspicuous. For example, it could be useful to investigate whether the effects of incentive conspicuousness diminish over time. Similarly, future work could examine the relative effectiveness of incentive conspicuousness in driving one-shot behaviors (such as attending a vaccination appointment) versus repeated ones (such as walking a certain number of steps each day). Although our studies span both one-shot (Experiment 3) and repeated (Experiments 1, 2, 4) behaviors, future work could examine whether the effects of incentive conspicuousness are stronger in driving one or the other type of behavior.

Incentivized behaviors also differ in the extent to which they are personally important or meaningful to actors, which could influence the effect of incentive conspicuousness. Future research could consider whether incentive conspicuousness has a larger effect on relatively meaningful behaviors (by drawing attention to an action already

deemed meaningful), or a weaker effect on them (by drawing attention to an incentive, crowding out intrinsic motivation).

Finally, future work could also consider how the time of the delivery of conspicuous incentive information influences its effectiveness. Although our field experiment did not vary the time of day in which the conspicuous incentive information was relayed, such timing may influence the effect of conspicuousness on behavior. We suspect that presenting incentive information conspicuously at times in which the behavior is most likely to be undertaken (e.g. at dinner time for nutrition incentives) may amplify its effects.

7. Concluding comment

In recent years there has been increased interest from academics and policymakers in using incentives to drive behavior change in settings ranging from health (Volpp et al., 2008; Volpp et al., 2009) to education (Fryer, 2013; Fryer, Levitt, List & Sadoff, 2019). Our investigation implies that standard randomized controlled trials conducted to evaluate the efficacy of incentives with the goal of proving their policy value may overestimate their impact in natural environments because such trials invariably entail making the incentives on offer conspicuous, which does not always mirror natural contexts. We hope our work will inspire further research on inattention and how to best reduce it, ensuring that valuable policies achieve their well-intended goals of changing behavior for the better.

CRedit authorship contribution statement

Leslie K. John: Conceptualization, Methodology, Investigation, Resources, Writing – original draft, Writing – review & editing, Supervision. **Hayley Blunden:** Conceptualization, Methodology, Formal analysis, Investigation, Data curation, Writing – review & editing, Supervision. **Katherine Milkman:** Conceptualization, Methodology, Investigation, Resources, Writing – original draft, Writing – review & editing, Supervision. **Luca Foschini:** Software, Data curation. **Bradford Tuckfield:** Methodology, Formal analysis, Data curation, Writing – original draft.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgments

This research was supported in part by Humana. The authors gratefully acknowledge Kristine Mullen, Vice President of Wellness at Humana and Glenn Morell at Evidation Health for helping to deploy the field experiment; Francesca Gino for her early involvement in this research and valuable feedback on the manuscript; Andrew Marder, Predrag Pandiloski, Andrew Joung, David Mao, and Quoc Dang Hung Ho for analyzing data; Anne Marie Green, Holly Howe, Shannon Sciarappa, and Trevor Spelman for research assistance; and Alex Rees-Jones for helpful feedback.

Appendix A

Users in the conspicuous incentives condition received the following email on Jan. 26, 2015 (the day before incentives began) (see Fig. A1):

Subject Line: New Program to Encourage You to Walk (earn Bonus Points)
 Message:



Bonus Points Opportunity

New Program to Encourage You to Walk (earn Bonus Points)

Tomorrow is the first day of a two week walking program designed in partnership with experts at Harvard and the University of Pennsylvania to get you moving. Tomorrow and every day after that for the next two weeks, we'll encourage you to walk by multiplying the points you earn for walking by 40.

To push you to walk more, your bonuses from Achievemint over the next two weeks will follow this schedule:

JANUARY 2015

| Sunday | Monday | Tuesday | Wednesday | Thursday | Friday | Saturday |
|--------|--------|------------|------------|------------|------------|------------|
| | | | | 1 | 2 | 3 |
| 4 | 5 | 6 | 7 | 8 | 9 | 10 |
| 11 | 12 | 13 | 14 | 15 | 16 | 17 |
| 18 | 19 | 20 | 21 | 22 | 23 | 24 |
| 25 | 26 | 27 | 28 | 29 | 30 | 31 |
| | | 40x | 40x | 40x | 40x | 40x |

FEBRUARY 2015

| Sunday | Monday | Tuesday | Wednesday | Thursday | Friday | Saturday |
|------------|------------|------------|------------|------------|------------|------------|
| | | | | | | |
| 40x | 40x | 40x | 40x | 40x | 40x | 40x |
| 40x | 40x | | 11 | 12 | 13 | 14 |
| 15 | 16 | 17 | 18 | 19 | 20 | 21 |
| 22 | 23 | 24 | 25 | 26 | 27 | 28 |

We will be sending you reminders every two days about upcoming bonuses.

We hope that this program will help you improve your walking habits!
 For the next two weeks, we'll be emailing you every other day about these bonuses.
 If you don't want to receive these emails, please click [here](#).

You're receiving this email because you signed up for email reminders.
 Not interested anymore? You can easily change your subscription preferences by clicking [this link](#).
 Copyright 2015 Achievemint. All Rights Reserved.

Fig. A1. Kickoff email. Users in the conspicuous incentives condition received the following email on Jan. 26, 2015 (the day before incentives began):

Users in the conspicuous incentives condition received the following notification email every other day (day 1, day 3, ..., day 13) during the 14-day incentive program (see Fig. A2).

Subject Line: Program to Increase Walking (Earn Bonus Points Today and Tomorrow)
 Message:

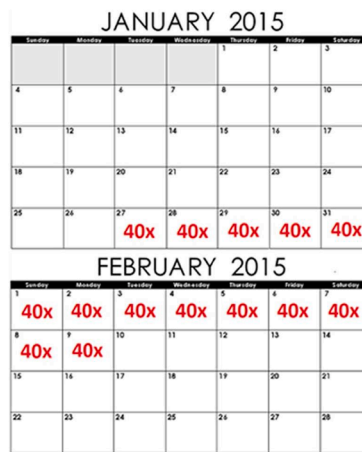


📌 Bonus Points Opportunity

Program to Increase Walking (Earn Bonus Points Today and Tomorrow)

You are in the middle of a two week walking program designed in partnership with experts at Harvard and the University of Pennsylvania to get you moving. Today and tomorrow, we'll encourage you to walk: all points you earn for walking will be multiplied by 40.

To push you to walk more, your bonuses from Achievemint over the next two weeks will follow this schedule:



For the duration of this two week period, we'll be emailing you every other day about these bonuses. If you don't want to receive these emails, please click [here](#).

You're receiving this email because you signed up for email reminders.
 Not interested anymore? You can easily change your subscription preferences by clicking [this link](#).
 Copyright 2015 Achievemint. All Rights Reserved.

Fig. A2. Notification emails.

Inconspicuous condition

In this study, you will have the opportunity to earn an additional \$0.16 in the following way:

During this survey, you will also have an additional task. 4 of the survey questions will include an option for you to fill in an open ended text box. For each of these questions, please enter the phrase “seagulls fly over the sea” in the open ended response. For each open ended text box you fill in, you will earn an additional \$0.04.

Although you have been assigned this task, you will still receive the study payment of \$0.40 even if you do not complete this additional task (you will receive the \$0.40 and your submission will not be rejected as long as you finish the survey).

An example is below (see Fig. A3).

Should health insurance...

be provided through a single national health insurance system run by the government

continue to be provided through a mix of private insurance companies and government programs

don't know

other

Fig. A3. Experiment 2 Conspicuousness Manipulation.

Conspicuous condition

In this study, you will have the opportunity to earn an additional \$0.16 in the following way:

During this survey, you will also have an additional task. 4 of the survey questions will include an option for you to fill in an open ended text box. For each of these questions, please enter the phrase “seagulls fly over the sea” in the open ended response. For each open ended text box you fill in, you will earn an additional \$0.04.

Although you have been assigned this task, you will still receive the study payment of \$0.40 even if you do not complete this additional task (you will receive the \$0.40 and your submission will not be rejected as long as you finish the survey).

An example is below.

Should health insurance...

be provided through a single national health insurance system run by the government

continue to be provided through a mix of private insurance companies and government programs

don't know

other

Repeated at the top of each survey page:

You can earn an additional \$0.04 bonus for each question with an open-ended response by selecting the open-ended option and writing in "seagulls fly over the sea"

Experiment 3 quiz questions

In Experiment 3, participants were required to correctly answer the four quiz questions below before continuing the study. Correct answers are italicized.

1. How can you earn an additional \$0.30 (\$0.01) in this study? (response options: *returning to the study tomorrow, [the date of the following day] between 10am and 8 pm EST to describe a photo*, writing my opinion of the photo instead of a description of it, answering 4 additional questions at the end of the study)
2. Is the statement below TRUE or FALSE?: I will be paid \$0.40 regardless of whether I come back tomorrow do to the second photo task. (response options: *TRUE, FALSE*)

3. Is the statement below TRUE or FALSE?: If I decide to come back tomorrow to do the second photo task, I will be paid an additional \$0.30 (\$0.01). (response options: TRUE, FALSE)
4. Is the statement below TRUE or FALSE?: If I decide NOT to come back tomorrow, I will NOT be penalized in any way. (response options: TRUE, FALSE)

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