



The Ineffectiveness of Inconspicuous Incentives: A Field Experiment on Inattention

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Abstract:	<p>Abstract: Policymakers in government and organizations regularly rely on incentive programs to encourage valued behaviors such as smoking cessation, educational achievement, home ownership, energy conservation, and savings. While often highly effective, there are also notable and surprising examples of their impotence. Why don't incentives put in place by policy makers to change highly elastic decisions always produce the desired response? We demonstrate that even when incentives are transparently provided and easily trackable in real-time, a failure to make them conspicuous renders incentives ineffectual at shifting valuable behaviors. Specifically, we present evidence from a field experiment in which thousands of pedometer users were offered incentives for each step taken (\$2 for every 10,000 steps) that could easily be tracked through a rewards application on their smart phones and an online portal, but only those users who received alerts highlighting these rewards increased their physical activity. We next present results from a nationally-representative survey study revealing that numerous incentive programs designed by the U.S. government to effect policy change are inconspicuous (namely, their intended beneficiaries are unaware of them) and thus likely far less effectual than they should be. Our findings lend support to recently-developed economic models proposing that inattention to incentives may be a common phenomenon with important consequences for policymakers, organizations and individual employees and citizens.</p>



The Ineffectiveness of Inconspicuous Incentives: A Field Experiment on Inattention

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Abstract: Policymakers in government and organizations regularly rely on incentive programs to encourage valued behaviors such as smoking cessation, educational achievement, home ownership, energy conservation, and savings. While often highly effective, there are also notable and surprising examples of their impotence. Why don't incentives put in place by policy makers to change highly elastic decisions always produce the desired response? We demonstrate that even when incentives are transparently provided and easily trackable in real-time, a failure to make them conspicuous renders incentives ineffectual at shifting valuable behaviors. Specifically, we present evidence from a field experiment in which thousands of pedometer users were offered incentives for each step taken (\$2 for every 10,000 steps) that could easily be tracked through a rewards application on their smart phones and an online portal, but only those users who received alerts highlighting these rewards increased their physical activity. We next present results from a nationally-representative survey study revealing that numerous incentive programs designed by the U.S. government to effect policy change are inconspicuous (namely, their intended beneficiaries are unaware of them) and thus likely far less effectual than they should be. Our findings lend support to recently-developed economic models proposing that inattention to incentives may be a common phenomenon with important consequences for policymakers, organizations and individual employees and citizens.

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INTRODUCTION

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Policymakers in government and organizations regularly rely on incentive programs to encourage valued behaviors such as smoking cessation, educational achievement, home ownership, energy conservation, charitable donations, and savings. While often effective (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 impotence (see Gneezy, Meier, & Rey-Biel, 2011 for some examples). For instance, the Federal government offered the Hope, Lifetime Learning, and American Opportunity Tax Credits to subsidize spending on higher education, and these incentive programs had a negligible effect on college enrollments (Long, 2004; Bulman & Hoxby, 2015). One explanation could be that the college enrollment decision is highly inelastic, but this decision has been shown to respond dramatically when tax professionals simply help families fill out financial aid forms (Bettinger et al., 2012). Another puzzling failure of incentives can be seen in a California program that offered discounts on electricity if consumers cut back energy use by 20%. This program produced no measurable response among coastal residents (Ito, 2015), but mailings merely letting people know how their energy usage compares to that of their neighbors reliably and meaningfully reduce energy consumption (Allcott, 2011) as do messages associating pollution due to energy usage with health and environmental problems (Asensio & Delmas, 2015). These examples and many others present a puzzle: why don't incentives put in place by policy makers to change highly elastic decisions always produce the desired response? After all, standard economic theory assumes that if you change incentives, rational agents' utility maximization calculations will respond by increasing the expected utility of engaging in any positively incentivized behavior (Mas-Colell, Whinston, & Green, 2012). In this paper, we call this assumption into question, demonstrating through a field experiment that even when incentives are transparently provided and easily trackable in real-time, failing to make them conspicuous renders them completely and totally ineffectual.

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Our findings lend support to recently-developed economic models proposing that inattention to incentives may be a common phenomenon with important consequences (Grubb, 2014; Chetty, Looney, & Kroft, 2009). Grubb (2014) proposes inattention as an explanation for actions taken recently by the Federal Communications Commission (FCC) to reduce "bill shock," or unexpected overage fees, in the cell phone market. Even though cell phone usage data is freely available to customers at any time, concerns that consumers frequently faced bill shock caused the FCC to compel U.S. carriers in 2011 to begin sending usage alerts to customers when they had exceeded data limits and were thus at risk of experiencing bill shock (FCC, 2015). Conversely, people's judgments and decisions can be affected by

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3 irrelevant but salient information (Dohmen, Falk, Huffman, & Sunde, 2006). Inattention of the type we
4 document here may not only lead to bill shock but to dramatic reductions in the efficacy of many
5 incentive policies, meanwhile leading numerous individuals to leave significant surplus on the table (de
6 Clippel et al., 2014; Dellavigna, 2009; Stango & Zinman, 2009, 2014; Armstrong & Vickers, 2012).
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11 Following this past research, we test the hypothesis that the effectiveness of incentive programs
12 depends not simply on incentives' presence or size, but, sometimes more crucially on their
13 conspicuousness. Consistent with this notion, past research has shown that the salience of information
14 influences individuals' responses to taxation (Chetty et al., 2009; Finkelstein, 2009), car purchasing
15 decisions (Busse et al., 2013), water usage (Tiefenbeck et al., forthcoming), reactions to college rankings
16 (Luca & Smith, 2013). Moreover, accounting for individual differences in attentiveness can profoundly
17 change social welfare calculations, as has been shown in applications to energy nudges (Alcott &
18 Taubinsky, 2015) and sales tax inattention (Taubinsky & Rees-Jones, 2016). Past research on the efficacy
19 of reminders also points to the important challenge of inattention (Ericson, *forthcoming*; Karlan, Ratan, &
20 Zinman, 2014; Rogers & Milkman, *forthcoming*; Shea, DuMouchel, & Bahamonde, 1996). In this paper,
21 we first present evidence that offering easily trackable but inconspicuous incentives renders such
22 incentives completely ineffective (but costs nearly the same amount as offering conspicuous incentives).
23 Next, we show that the problem of deploying inconspicuous incentives is rampant in public incentive
24 programs offered in the United States, almost certainly dramatically limiting the impact of incentives as a
25 policy tool.
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44 Study 1 is a large-scale field experiment that examines the effect of incentive conspicuousness on
45 behavior change. For two weeks, all participants were offered monetary incentives intended to increase
46 their physical activity levels and provided with constant, real-time access to the rewards they were
47 accruing. For half of participants, we implemented a shoestring marketing campaign intended to increase
48 the conspicuousness of the incentives. We find that incentives alone are insufficient to change behavior.
49 Physical activity levels were no higher among participants who were offered inconspicuous incentives
50 than among participants in a holdout comparison group that received no incentives. By contrast, the low-
51 touch marketing campaign was sufficient to unlock the power of incentives to meaningfully boost
52 exercise (increasing daily step counts by 200 to 400 steps depending on the estimation model for every
53 day of the two week program and the two weeks that followed). Simply put, study 1 shows that the
54 conspicuousness of incentives is absolutely critical to their impact (far more critical than their presence, in
55 fact).
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3 Study 2 speaks to the implications of the problem identified by study 1: without making incentive
4 programs conspicuous, even the most well-intended and well-structured incentive programs can be for
5 naught. Study 2 polls a representative sample of United States citizens, probing them for their knowledge
6 —or more aptly, lack thereof—of a wide variety of federal subsidy programs. Specifically, respondents
7 were presented with a list of subsidy programs and tasked with identifying which were actual incentives
8 offered by the US government (versus distractor incentive programs, which were programs offered in
9 other countries but not in the U.S.). We find that, on average, participants correctly categorize only 63%
10 of the subsidies. If this were a test, our respondents would get a D. Moreover, among the subsidies for
11 which respondents believed themselves to be eligible, the uptake rate was only 55%. Given that
12 inconspicuous incentives are ineffective, as shown in study 1, study 2 suggests that the U.S. government
13 is likely influencing dramatically fewer behaviors through incentives than it could be if many of its
14 inconspicuous incentive programs were more aggressively marketed.
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29 The remainder of this paper is organized as follows. First, we present results from our field
30 experiment demonstrating that only conspicuous incentives promote behavior change (increased physical
31 activity) in a highly policy-relevant setting (425,000 Americans die prematurely each year due to physical
32 inactivity and diet; Mokdad et al., 2000). We next present results from our survey study highlighting that
33 numerous incentive programs designed by the U.S. government to effect policy change are inconspicuous
34 and thus likely far less effectual than they should be. Finally, we conclude with a discussion of our
35 findings, their implications for research and policy, and directions for future research.
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44 **STUDY 1: FIELD EXPERIMENT**

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46 Our field experiment examines the impact of conspicuous versus inconspicuous incentives on
47 physical activity.
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50 **Methods**

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52 We randomly assigned 2,055 pedometer users to one of two experimental conditions. All users
53 received incentives for walking and had access to information about incentives, which could easily be
54 tracked through a rewards application available on their smart phones and an online portal. All users also
55 received weekly emails updating them on their total earnings. Users in the *conspicuous incentives*
56 *condition* received eight extra emails containing information about offered incentives. Users in the
57 *inconspicuous incentives condition* did not receive these extra emails. As expected, we found no
58 significant differences in the observable pre-treatment characteristics of the two experimental groups
59 (Table 1).
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Participants

Our study population was composed of users of an online platform called Achievemint, a reward platform for healthy activities powered by Evidation Health. Achievemint users can link their Fitbit pedometers with the Achievemint platform by authorizing their recorded step counts to be automatically transferred to their Achievemint account.

Every time an Achievemint user takes 200 steps, he or she earns one point from the platform. Points are redeemable for cash rewards: after a user has taken 200,000 steps, he or she earns \$1.00. Users receive a check for every \$25 earned. Achievemint sends all users a weekly update email that contains information on a user's current number of unredeemed points.

Of the Achievemint users who had linked Fitbit devices, we selected 2,055 for our study based on three eligibility criteria. First, users who had been part of a previous study of incentives for exercise were excluded. Second, users whose historical usage data indicated that they opened fewer than one email per month from Achievemint were excluded. Third, users whose historical usage data indicated that they were above the 70th percentile for mean daily steps were excluded. These exclusions were based on calculations that indicated that a test with this sub-population would yield about 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 the University of Pennsylvania. A waiver of informed consent was approved per Federal regulations (45 CFR 46.116(d)) in light of the fact 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.

Procedures

Achievemint users meeting eligibility requirements were randomly assigned to one of two experimental conditions: a conspicuous incentives condition or an inconspicuous incentives condition. All study participants received the same incentive to walk in the form of a "point multiplier," such that the Achievemint points that they earned were multiplied by 40 for two weeks (that is, for every 200,000 steps, they earned \$40.00 instead of the usual \$1). Participants in the conspicuous incentives condition (n = 1,027) received emails (in addition to Achievemint's standard weekly update emails) detailing the duration and magnitude of these point multipliers. Participants in the inconspicuous incentive condition (n = 1,028) did receive point multipliers, but did not receive extra emails about the point multipliers. All participants were able to track their Achievemint points through the Achievemint app, website, and through standard weekly update emails.

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3 Participants received point multipliers for two weeks from Jan. 27, 2015 through Feb. 9, 2015.
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5 Participants in the conspicuous incentives condition received a kickoff email on Jan. 26, 2015: the day
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7 before the start of the point multiplier incentives. This email featured the subject line: “New Program to
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9 Encourage You to Walk (earn Bonus Points).” The contents of this email (depicted in Supporting
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11 Information, Figure S1) explained to participants that they had been enrolled in a program to increase
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13 their walking. It showed a calendar with point multipliers highlighted on each day when they would be
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15 rewarded (every day for the next two weeks). Conspicuous incentive condition participants also received
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17 email reminders about the program every other day for its duration (seven additional emails on days 1, 3,
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19 5, 7, 9, 11, and 13 of the experiment), which contained all of the same information including the schedule
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21 of incentives depicted on a calendar. A reminder email is depicted in Supporting Information, Figure S2.
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25 No participants opted out of the incentive program. However, as we discuss further in the results
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27 section, some participants did not record daily steps on some days, either because they failed to wear
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29 Fitbits, or because they did not sync their Fitbit data with Achievemint. Other participants may have worn
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31 Fitbits only for a brief portion of a given day.
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33 **Statistical Analyses**

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35 Our outcome variable of interest was *daily steps walked*. Participants’ daily steps were tracked for
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37 two weeks before the intervention and during the intervention, and for four weeks after the intervention.
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39 We tested for evidence of differential responses to incentives by condition by comparing mean daily steps
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41 in each group during and after the end of the intervention period.
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45 Our statistical analysis strategy was a difference-in-differences approach. Like other standard
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47 difference-in-differences analyses (e.g., Pope & Pope, 2015), our analyses include covariates for
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49 experimental condition, temporal indicators (during-, and post-intervention), and the interaction of
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51 experimental condition and temporal indicators. We chose difference-in-differences analysis rather than
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53 simple comparison of groups because it is effective at “comparing the time changes in the means for the
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55 treatment and control groups” so that “both group-specific and time-specific effects are allowed for”
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57 (Wooldridge, 2010, p. 148). It therefore enables comparisons between experimental groups (conspicuous
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59 and control) at different times (during and after the intervention). The coefficients of interest in
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regressions of this form are the interactions of experimental condition and temporal indicators. These
coefficients measure the effect of the treatment at a given time period (Wooldridge, 2010, p. 148).
Importantly, our study uses random assignment to experimental condition, so experimental groups are the
same in expectation. This enables our analysis to avoid many potential methodological issues that come

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3 from using difference-in-differences methods to compare groups that have significant underlying
4 differences (Athey & Imbens, 2002).
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7 We use ordinary least squares regressions with fixed effects for all of our analyses. To account
8 for different walking levels on different days (due to weather, day of week, and seasonality), we include
9 fixed effects for each day observed in the dataset. It would be natural to include user fixed effects in an
10 analysis such as this one to account for different individual activity levels. However, in addition to
11 varying in overall activity levels, individuals likely vary on their patterns of activity throughout a given
12 week. For example, some individuals may take a recreational Sunday walk, while others walk more
13 during the week as part of a commute. Individuals work on different days and have different exercise and
14 gym attendance habits. To account for these detailed individual differences, we employ fixed effects that
15 are more specific than user fixed effects. Specifically, we include fixed effects for an interaction of user
16 with day of the week (Monday, Tuesday, etc.): seven fixed effects per individual. We also cluster
17 standard errors at the user level, to account for possible serial correlation within a user's daily steps over
18 time. This analysis increases our ability to identify experimental treatment effects.
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33 The following is the ordinary least squares (OLS) regression equation used to estimate the
34 coefficients shown in Table 2 as part of our difference-in-differences strategy.
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37 **Equation 1.**

$$\begin{aligned}
 \text{daily steps}_{it} = & \beta_0 + \alpha_1 \text{userxdayofweek}_{it} + \alpha_2 \text{day}_t \\
 & + \beta_1 \text{conspicuous_incentives}_i \times \text{duringtreatment}_t \\
 & + \beta_2 \text{conspicuous_incentives}_i \times \text{lessthan15daysaftertreatment}_t \\
 & + \beta_3 \text{conspicuous_incentives}_i \times \text{between15and21daysaftertreatment}_t \\
 & + \varepsilon_{it}
 \end{aligned}$$

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51 In this equation, *conspicuous_incentives_i* is an indicator variable taking on a value of one if an
52 individual, *i*, was in the conspicuous condition and zero otherwise; *day_t* is a fixed effect for each day
53 included in the data; *userxdayofweek_{it}* is a fixed effect for user-day-of-week, and the other variables
54 represent 0-1 indicators for whether an observation occurred during, within the first two weeks after, or
55 during the third week after the intervention. The β_1 , β_2 , and β_3 coefficients therefore measure the
56 differences between conspicuous incentive condition participants and inconspicuous incentive condition
57 participants during, shortly after, and long after the experimental intervention.
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In addition to our primary analyses, which measure the difference between our experimental groups, we conducted supplemental analyses to compare incentivized participants with participants in a

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3 matched holdout group. We selected the matched holdout group from the same pool of app users from
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5 which the experimental participants were selected (Achievemint users with linked Fitbit accounts). To
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7 select a holdout group that closely matched the experimental group in observable, including demographic,
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9 characteristics, we selected Achievemint users who were excluded only because they had been
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11 participants in a previous experiment, but who would not have been excluded otherwise (because they
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13 met all other inclusion criteria as experimental participants).
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16 The following is the OLS regression equation used to estimate the effect of receiving incentives
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18 in the inconspicuous incentives group in our experiment versus receiving no incentives, and coefficients
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20 from this regression analysis are shown in Table 3 as part of our secondary difference-in-differences
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22 analysis. This regression's standard errors were also clustered by user.
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25 Equation 2.

$$\begin{aligned}
 & \text{daily steps}_{it} = \beta_0 + \alpha_1 \text{userxdayofweek}_{it} + \alpha_2 \text{day}_t \\
 & \quad + \beta_1 \text{inexperiment}_i \times \text{duringtreatment}_t \\
 & \quad + \beta_2 \text{inexperiment}_i \times \text{lessthan15daysaftertreatment}_t \\
 & \quad + \beta_3 \text{inexperiment}_i \times \text{between15and21daysaftertreatment}_t \\
 & \quad + \beta_4 \text{conspicuous_incentives}_i \times \text{duringtreatment}_t \\
 & \quad + \beta_5 \text{conspicuous_incentives}_i \times \text{lessthan15daysaftertreatment}_t \\
 & \quad + \beta_6 \text{conspicuous_incentives}_i \times \text{between15and21daysaftertreatment}_t \\
 & \quad \quad \quad + \varepsilon_{it}
 \end{aligned}$$

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45 In this equation, *inexperiment_i* is an indicator variable taking on a value of one if an individual, *i*, was in
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47 the experiment and zero otherwise, *conspicuous_incentives_i* is an indicator variable taking on a value of
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49 one if an individual, *i*, was in the conspicuous condition and zero otherwise, *day_t* is a fixed effect for each
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51 day included in the data, *userxdayofweek_{it}* is a fixed effect for user-day-of-week, and the other variables
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53 represent 0-1 indicators for whether an observation occurred during, within two weeks after, or in the
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55 third week after the intervention. Like Equation 1, Equation 2 represents a difference-in-differences
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57 analysis strategy. The matched holdout group was carefully selected so that it would not differ in
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59 expectation from the experimental group.¹
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RESULTS

¹ Since 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.

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3 Figure 1 shows the differences in mean daily steps taken by participants in the conspicuous and
4 inconspicuous incentives conditions before, during, and after our intervention. As predicted, Figure 1
5 shows that users in the conspicuous incentives condition had a higher mean daily step-count both during
6 and after the intervention relative to the inconspicuous incentives group. To test the significance of these
7 differences, we turn to regression analyses. We are able to identify the effects of our intervention with
8 considerable precision by controlling for individual differences in pre-intervention propensity to walk
9 while identifying differences between our experimental groups in during- and post-intervention behavior.
10 As described previously, our analysis uses a difference-in-differences regression strategy, which in
11 general enables comparisons between groups and over time. For our research, the difference-in-
12 differences strategy enables us to compare the mean daily steps of the participants in the conspicuous
13 incentives group with the mean daily steps of participants in the inconspicuous incentives group during,
14 shortly after, and long after our experimental intervention.

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29 Table 2 shows the results of OLS regressions predicting daily steps with fixed effects for each
30 participant on each day of the week as well as date fixed effects. The primary predictor variables of
31 interest are interactions between an indicator for our conspicuous incentives condition and indicators for
32 each of the time periods studied during and post-intervention (two weeks during the intervention, two-
33 weeks immediately post-intervention, and three weeks post-intervention). Standard errors are clustered by
34 participant and reported in parentheses. The primary coefficients of interest in these regressions estimate
35 the difference between conspicuous and inconspicuous incentives groups' mean daily steps during each
36 time period of interest during and post-intervention after absorbing: (a) average individual differences in
37 exercise as a function of day of week, (b) average pre-treatment differences in participants across
38 condition (since the same participant is always in the same experimental condition), and (c) average
39 differences in participant exercise by date.

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53 Table 2, Model 1 reports the results of this regression estimated on an intent-to-treat basis. As we
54 predicted, conspicuous incentive condition participants took significantly more daily steps during the
55 intervention than inconspicuous incentive participants (an estimated 367 extra daily steps, $p < 0.01$).
56 Further, our regression estimates indicate that participants in the conspicuous incentives condition took
57 more daily steps than those in the inconspicuous condition for the two weeks after our intervention (an
58 estimated 332 extra daily steps, $p < 0.01$), although the regression estimated effect dissipated three weeks
59 after our intervention (an estimated 146 extra daily steps, *NS*).
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3 One concern about the above results is that they could be driven by differential use of Fitbits
4 rather than differential exercise if conspicuous incentives prompted participants to wear their Fitbits more
5 often than participants in our inconspicuous incentives condition without actually changing their walking
6 habits. One way of addressing this concern is to examine differences in attrition by condition. One
7 measure of attrition in Fitbit usage could be indicated by a user recording zero steps for a given day.
8 Attrition could also be indicated by a user recording a small, nonzero number of steps, for example if he
9 or she only wore a Fitbit for a brief part of a day. A previous study of walking with an adult participant
10 pool consisting of several thousand people with no attrition found that participants never took fewer than
11 2,000 steps per day (Hivensalo et al., 2011). We therefore used a very conservative 2,000 steps as a cutoff
12 for defining a participant an “attriter”: days on which a given user recorded fewer than 2,000 steps were
13 regarded as a failure to properly wear and sync Fitbits (note that all reported results are stronger if we
14 instead define attrition as 0 steps per day). Table 1 shows the numbers of attriters in each experimental
15 condition, during each time period. We performed two-sample proportion tests to determine whether the
16 conspicuous and inconspicuous incentives groups had different attrition rates during, shortly after, and
17 long after our intervention. We see (in Table 1) no significant differences at the 0.05 significance level
18 between the attrition rates in the experimental conditions during, shortly after, or long after our
19 experimental intervention.

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22 However, to ensure the robustness of our regression results to attrition, we replace our intent-to-
23 treat analysis with an even more conservative test of our hypothesis by deleting all person-day
24 observations from both experimental conditions that show fewer than 2,000 daily steps logged. Table 2,
25 Model 2 shows our primary regression estimated only on the subset of data containing observations of
26 daily steps that are greater than 2,000.

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29 The coefficient estimates reported in Table 2, Model 2 provide more evidence that conspicuous
30 incentives boost step counts significantly and that the observed effects reported in Model 1 are not
31 artifacts of differential attrition. According to the estimates from Model 2, which again control for
32 participant-day-of-the-week fixed effects and date fixed effects, non-attriting participants in the
33 conspicuous incentives condition took more steps than participants in the inconspicuous incentives
34 condition during the intervention (an estimated 229 extra daily steps, $p < 0.05$) and up to two weeks after
35 the intervention (an estimated 211 extra daily steps, $p < 0.10$), but not 3 weeks after the intervention (an
36 estimated 96 extra daily steps, *NS*). Although the coefficient estimates in this model are slightly smaller
37 than those in Model 1, the effects of the intervention during the intervention period and for the two weeks

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3 post-intervention remain large, positive and significant, indicating that differences between experimental
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5 groups cannot be explained by differential attrition.
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8 All results reported thus far have compared users across our two experimental conditions
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10 (conspicuous incentives and inconspicuous incentives). Both experimental groups however, received the
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12 same inflated incentives during our two week intervention period. Thus, while we have measured the
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14 effect of incentive conspicuousness on behavior, we have not measured the effect of the incentives
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16 themselves. Of course, this was not the goal of our research, but it is useful to understand how magnifying
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18 the conspicuousness of incentives compares with providing incentives in the first place. We use a
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20 difference-in-differences regression estimation strategy to compare the change in pre- versus during- and
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22 post-intervention behavior of participants in our inconspicuous incentives group versus participants in a
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24 holdout group who were excluded from our experiment only because they had previously participated in
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26 another, unrelated experiment, but who otherwise would have been eligible for participation. Because this
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28 group met the study's eligibility criteria based on observable characteristics, it provides an ideal control
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30 group for comparisons with experimental participants in a difference-in-differences analysis.
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34 Table 3 shows results of ordinary least squares regressions predicting daily steps with fixed
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36 effects for user-day-of-week and the calendar date. The predictor variables are interactions of
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38 experimental status (in experiment, in conspicuous incentives condition) and different time periods
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40 during- and post-intervention. Standard errors are clustered by participant and reported in parentheses. As
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42 in Table 2, the results from two models are included: Model 1 includes all users, whereas Model 2
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44 accounts for possible differential attrition between groups. The first three coefficients in these regressions
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46 estimate the difference between inconspicuous incentive participants' and holdout users' mean daily steps
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48 at each time period—that is, they estimate the effects of the incentives themselves after controlling for
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50 user-day-of-week and calendar date fixed effects. The second three coefficients estimate the difference
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52 between conspicuous incentive participants' and inconspicuous incentive participants' mean daily steps at
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54 each time period—that is, they estimate the marginal effect of conspicuousness on responses to
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56 incentives, again after controlling for user-day-of-week and calendar date fixed effects.
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60 The results in Table 3 indicate that inconspicuous incentive group participants' behavior did not
differ from the behavior of holdout group participants during the intervention, though they may have
differed after the intervention in the opposite of the intended direction (at 0–2 weeks post intervention one
of the models oddly shows a statistically significant difference such that inconspicuous incentive group
members exercised less than members of the holdout group). The size and significance of the coefficients

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3 estimated in Table 3, in both models, indicates that although making incentives conspicuous was effective
4 in changing behavior as shown in Table 3, incentives alone without any emphasis did not meaningfully
5 alter exercise.
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9 10 **Robustness Tests**

11 In addition to the analyses reported here, we conducted a variety of robustness checks on our results
12 (see Supporting Information). Specifically, we performed robustness checks with the following
13 specification details:
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- 17 • We clustered data by user-day-of-week, using data only on experimental participants (Table S1)
- 18 • We clustered data by user-day-of-week, using data on experimental participants together with the
19 matched holdout group (Table S2)
- 20 • We clustered data by user, winsorizing step counts at the 99% level, using data only on
21 experimental participants (Table S3)
- 22 • We clustered data by user-day-of-week, winsorizing step counts at the 99% level, using data only
23 on experimental participants (Table S4)
- 24 • We clustered data by user, winsorizing step counts at the 99% level, using data on experimental
25 participants together with the matched holdout group (Table S5)
- 26 • We clustered data by user-day-of-week, winsorizing step counts at the 99% level, using data on
27 experimental participants together with the matched holdout group (Table S6)

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44 In each of these models, users in the conspicuous incentives condition take more daily steps than users in
45 the inconspicuous incentives condition both during and up to two weeks after the experimental
46 intervention, with $p < 0.05$ for both of these coefficients in each model.
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50 **STUDY 2: NATIONALLY REPRESENTATIVE SURVEY**

51 Study 1 demonstrates that incentives alone are insufficient to change behavior. Specifically,
52 physical activity levels were no higher among participants who were offered inconspicuous incentives
53 than among those in a holdout comparison group who received no incentives. By contrast, a simple and
54 inexpensive marketing campaign designed to make these incentives more conspicuous was sufficient to
55 unlock the power of incentives to meaningfully change behavior for the better. Study 2 extends this
56 finding by exploring its broader implication: without making incentive programs conspicuous, even the
57 most well-intended and well-structured of incentive programs can be for naught. In study 2, we polled a
58 representative sample of United States residents, assessing their awareness of a wide variety of federal
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3 subsidy programs to assess the extent to which inconspicuous incentives may be limiting the policy
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5 impact of U.S. rewards programs.
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7 8 **Methods**

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10 A nationally representative sample of United States citizens ($N = 106$, 48.1% male, $M(SD)_{age} =$
11 48.07(16.45)) completed this survey in exchange for \$0.75. Participants were presented with a
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13 randomized list of 35 subsidies, 20 of which were real incentives offered to U.S. citizens (e.g., childcare
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15 savings accounts, charitable donations deductions, and health savings accounts; Table 4); the other 15,
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17 distractor subsidies, were real incentives offered in other countries, and not available to U.S. citizens (e.g.,
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19 public transportation deduction, and working mothers' child relief; Table 5).
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23 To assess U.S. citizens' attentiveness to government subsidies for which they may be eligible, we
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25 asked participants to indicate, for each subsidy, whether it was "available to US citizens (i.e., a real IRS
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27 tax incentive)" versus "not available to US citizens (i.e., not a real IRS tax incentive)." U.S. tax subsidies
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29 were pulled from the first fifty posts on TurboTax's "Tax Deductions and Credits" website section in July
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31 2016. Posts were not included if they had not been updated for the 2015 tax season. The subsidies that
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33 were not US subsidies were in fact legitimate subsidies in other countries. To ensure that the subsidies
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35 we chose for this survey were neither particularly well-known nor obscure, we conducted a pilot test in
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37 which we asked participants ($N = 106$) to perform the same categorization task as that eventually used in
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39 the main study, but for a larger list of 44 subsidies. We used this pilot test to generate a more manageable
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41 list of 35 subsidies that were representative with respect to the extent to which they were known, for use
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43 in the main study.
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47 After having completed the categorization task, the rest of the survey focused exclusively on the
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49 20 real US subsidies. Specifically, we assessed uptake and perceived eligibility: for each of the 20
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51 subsidies, participants were asked: "Have you ever claimed this incentive?" (Yes / No / I don't know) and
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53 "Do you believe you are eligible for this incentive? (e.g., if you tried to claim it this year, would your
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55 claim be accepted?" (Yes / No). The survey concluded with demographic questions including age, gender,
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57 education, annual household income.
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59 60 **Results**

(In)attentiveness. On average, participants correctly categorized only 63% of the subsidies.
Although this rate is significantly better than chance ($z = 2.71$, $p = .0068$), it clearly leaves room for
improvement. In fact, if this were a test, the average grade would be a D. Performance was no different
for the U.S. versus non-U.S. subsidies ($z = 0.95$, $p = .34$).

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Perceived Eligibility. On average, participants believed they were eligible for 23% of the U.S. subsidies, with perceived eligibility for individual subsidies ranging from 10% of the sample (for the Alternative Fuel Vehicle Refueling Property Credit) to 52% (for the charitable donations subsidy).

Uptake. We assessed, among respondents believing they were eligible for a given subsidy, the percent that had actually redeemed it. For example, 19% of respondents believed they were eligible for the American Opportunity Tax Credit, which offers up to \$2500 for tuition expenses and course materials; of these respondents, only 40% said that they had claimed it. On average, participants perceived themselves to be eligible for 23% of the subsidies. Among the subsidies for which respondents believed themselves to be eligible, the uptake rate was only 55%.

In sum, study 2 shows that participants claim government subsidies in roughly half of the instances when they are eligible to do so. We suggest that inattentiveness—the fact that participants could correctly identify the US subsidies only 63% of the time—is likely a major culprit of these low redemption rates. Taken together, the results of study 1 and study 2 suggest that if inattentiveness could be addressed, uptake and efficacy of well-intended US incentive programs could be greatly enhanced, with even the most modest of marketing campaigns.

DISCUSSION

Both policy makers and scholars have long relied on incentives as a key lever for changing behavior. But even in contexts where preferences have been proven elastic, the benefits of incentivizing desirable behaviors have often proven surprisingly small or nonexistent, contrary to the predictions of standard economic theory (Mas-Colell et al., 2012). In this paper, we provide an explanation for the apparent inconsistency between theory and observed behavior by showing that even when incentives are transparent and can be easily tracked in real-time, they do not change behavior unless they are made conspicuous. Based on evidence from a randomized field experiment aimed at increasing healthy habits (walking more steps per day) and a survey we conducted on a representative sample of United States citizens, we show the critical role of attention to incentives in driving behavioral change. Our research contributes to an emerging body of evidence suggesting that neither incentives alone nor the mere awareness of a problem (Bronchetti, Huffman, & Magenheimer, 2015) is insufficient to change behavior (Tiefenbeck et al., forthcoming) and helps to explain why so many Americans fail to take advantage of incentive programs for which they are eligible.

In the last several years we have witnessed increased interest from academics and policy makers in the use of behavioral insights to drive behavioral change (Chapman, Colby, & Yoon, 2010;

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3 Loewenstein, Asch, & Volpp, 2013; Shafir, 2012). From the 2015 Obama directive to use such insights to
4 inform U.S. policy making to the creation of dozens of Behavioral Insights Units across the globe,
5 understanding human behavior appears to have become a must for policy makers seeking to devise
6 policies that can effectively address important societal problems ranging from eliminating cheating on
7 taxes to increasing school attendance, reducing smoking, fighting obesity, to increasing civic engagement.
8 Importantly, standard randomized, controlled trials conducted to evaluate the efficacy of incentives with
9 the goal of proving their policy efficacy certainly overestimate their impact in natural environments
10 because such trials focus attention on making their participants fully aware of all incentives on offer,
11 which does not mirror natural contexts. Our research contributes to a growing literature suggesting how
12 behavioral science can inform policy.
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16 In Study 1, we conducted a field experiment and showed that physical activity levels did not
17 differ among participants who were offered inconspicuous incentives and those in a comparison holdout
18 group that received no incentives. What made a difference in increasing physical activity in this context
19 was a simple marketing campaign that made incentives more conspicuous. And consistent with studies
20 that have provided (salient) incentives for people to exercise (Charness & Gneezy, 2009; Acland & Levy,
21 2010), our treatment effect persists; it does not extinguish until well after incentives are removed. In study
22 2, we build on these findings by exploring their broader implications: without making incentive programs
23 conspicuous, even the most well-structured incentive programs do not produce the intended and expected
24 changes in behavior. Specifically, using a representative sample of United States citizens, we find that a
25 large percentage of them are unaware of many federal subsidy programs from which they could benefit—
26 essentially free money.
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30 Our findings provide empirical evidence consistent with recently-developed economic models
31 suggesting that inattention to incentives is common and has important consequences (Grubb, 2014).
32 Inattention of the type we document here is likely the root cause of failures in the efficacy of many
33 incentive policies, preventing many people from accruing the benefits they deserve (and/or qualify to
34 receive). We hope our work will inspire further research on inattention and how to best reduce it, ensuring
35 that valuable policies achieve their well-intended goal of changing behavior for the better.
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Tables and Figures

Table 1: Pre-treatment characteristics of the study sample reveal no significant differences in t-tests conducted at the 0.05 level. This table shows statistics from 3 weeks prior to the intervention up until the start of the intervention. Standard errors are in parentheses. Attriters are defined as individuals who recorded fewer than 2,000 steps on at least one day in the 21 days immediately preceding the intervention.

	(1)	(2)	(3)
	Salient	Control	(1) vs. (2)
			p-value
Mean Daily Steps Walked	5400.06 (89.40)	5577.47 (85.21)	0.15
% Who Walked \leq 2,000 Steps on at Least One Day (Attriters)	72.74 (1.39)	69.16 (1.44)	0.07
Mean Daily Steps of Attriters	4475.5 (95.04)	4608.92 (93.53)	0.32
Number of Emails Opened Monthly	3.82 (0.08)	3.93 (0.09)	0.36
First Email Opened (Days Since 6/1/2014)	75.19 (2.34)	72.88 (2.36)	0.49
Observations	1,027	1,028	

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 study participants' daily steps from the 21 days before the intervention, 14 days during the intervention, and 21 days after the intervention. Model 1 is estimated using all observations. Model 2 is estimated using observations with daily steps greater 2,000.

	Model 1	Model 2
(Conspicuous Incentives Condition) x (Treatment Period)	367.06*** (105.71)	229.04** (83.63)
(Conspicuous Incentives Condition) x (0-2 Weeks Post-Treatment)	331.92** (119.61)	210.93* (92.60)
(Conspicuous Incentives Condition) x (3 Weeks Post-Treatment)	145.98 (141.36)	96.29 (117.78)
Fixed effects for day of the year	Yes	Yes
Fixed effects for (user) x (day of the week)	Yes	Yes
Observations	115,080	85,789
R-squared	0.51	0.48
Clusters	2,055	2,029
Observations Included	All	Steps>2000

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. Model 1 is estimated using all observations. Model 2 is estimated using observations with daily steps greater than 2,000.

	Model 1	Model 2
(In Experiment) x (Treatment Period)	-110.04 (89.72)	52.08 (72.12)
(In Experiment) x (0-2 Weeks Post-Treatment)	-413.82** (129.58)	-198.22 (140.87)
(In Experiment) x (3 Weeks Post-Treatment)	-130.16 (123.58)	141.74 (101.51)
(Conspicuous Incentives Condition) x (Treatment Period)	367.06*** (105.68)	228.33** (83.72)
(Conspicuous Incentives Condition) x (0-2 Weeks Post-Treatment)	331.92** (119.58)	212.08* (92.73)
(Conspicuous Incentives Condition) x (3 Weeks Post-Treatment)	145.98 (141.33)	96.00 (117.88)
Fixed effects for day of the year	Yes	Yes
Fixed effects for (user) x (day of the week)	Yes	Yes
Observations	235,424	172,117
R-squared	0.33	0.26
Clusters	4,204	4,144
Observations Included	All	Steps>2000

Table 4: US Subsidies: (In)attentiveness, Perceived eligibility, and Uptake rates by individual subsidy.

Subsidy	(In) attentiveness	Perceived Eligibility	Uptake
Adoption credit: A tax credit of up to \$13400 for adopting a child	58%	12%	23%
Alternative Fuel Vehicle Refueling Property Credit: Tax credit up to \$1000 for installing equipment to refuel an electric car	59%	10%	45%
American Opportunity Tax Credit: Tax credit up to \$2500 for tuition expenses and course materials	48%	19%	40%
Charitable Donations: Tax deductions for charitable donations as well as transportation costs to and from volunteering	82%	52%	82%
Child Care Reimbursement Account: A tax-advantaged savings account that parents can direct up to \$5000/year into. This account can be used for childcare expenses	71%	14%	60%
Child Tax Credit: A \$1000 tax credit for parents with dependents under 17	82%	28%	73%
Earned Income Tax Credits: A tax credit available to working citizens with household incomes below \$53267	82%	31%	82%
Expenses for Business Use of Your Home: Deductions for electricity, rent, water and other miscellaneous costs (e.g., landscaping) for individuals who use their home as an office	80%	16%	82%
Health Savings Accounts: A non-taxable savings account for funds used to pay healthcare expenses	69%	29%	55%
Homeowner Tax Deductions: An income tax deduction for property taxes and mortgage interest on a taxpayer's primary residence. Primary residences are not limited to houses and apartments and may also include boats, RVs, or house trailers if they meet qualifications	75%	44%	91%
Investment Costs: Tax deductible for costs incurred by investment or tax planning costs, so long as they exceed 2% of the taxpayer's annual income.	50%	13%	36%
Lifetime Learning Tax Credit: Up to \$2000 in tax credits to offset the cost of tuition	50%	18%	37%
Medical Expenses: An income tax deduction for medical expenses (including hormone therapy, doctor recommended physical activity, psychiatrist services and bariatric surgery) up to 7.5% of one's income	69%	32%	65%
Moving Expense Deductions: Tax credits for moving expenses incurred when taking a new job	63%	15%	63%
Nonbusiness Energy Property Tax Credit: Tax credits for "green" home improvements including insulation, doors, windows, roofing, and natural gas burning appliances	65%	25%	27%
Publication 535: Tax deductible for fees paid to professionals (e.g., attorneys, accountants) when they relate to an ongoing business.	53%	15%	69%
Residential Energy Efficiency Property Tax Credit: Tax credits for installing solar, geothermal, or wind power in one's home	78%	19%	35%
Sales and Other Dispositions of Capital Assets: A deductible whereby individuals and businesses may write-off bad debts (i.e., lending money to someone who does not pay them back).	54%	13%	21%
Savers Tax Credit: Tax credits up to \$1000 for contributions to a retirement savings plan (401k and certain other plans)	50%	31%	55%
Travel, Entertainment, Gift, and Car Expenses: Tax deductions for any mileage individuals accrue travelling between job sites, or travelling for health-related reasons. Individuals who drive for work can also claim vehicle repair and maintenance	63%	23%	54%

Notes. (In)attentiveness: Correct categorization rate; Perceived Eligibility: Percent believing they are eligible; Uptake: Among the perceived eligible, % claiming

Table 5: Non-US Subsidies: (In)attentiveness

Subsidy	(In)attentiveness: correct categorization rate
Artist Tax Exemption: The first \$50,000 per annum earned by artists, writers and performers for the sale of their work is exempt from income tax	19%
Children's Art Tax Credit: A tax credit up to \$500/child for eligible children's art program registration costs	18%
Children's Fitness Tax Credit: An income tax credit for the cost of the registration of the taxpayer's child(ren) in eligible sports and physical activity programs	18%
Commuting Costs: A tax deductible for costs incurred as a result of travel to and from work	48%
Financial Assistance to Adult Children: A deductible for the financial support taxpayers provide to adult (21+) children	27%
Foreign Maid Levy Relief: A credit that working mothers can claim for the income they paid to a foreign domestic childcare workers or home maintenance workers	21%
Funeral Expenses: Deductible for funeral expenses paid out-of-pocket for oneself or one's immediate family's funeral costs	29%
Home Refurbishment Deduction: A tax deduction of up to 15% of the costs associated with refurbishing a primary residence	24%
Maternity Grants Act: Compensation for up to \$150 of expenses related to caring for newborn children (crib, clothes, cloth diaper, etc.)	29%
Private Schooling Deductions: Deductible up to 30% of the tuition paid to private primary and secondary schools for dependents.	27%
Public Transportation Deduction: A tax deduction for monthly public transit passes	25%
Rent Benefit: A tax deduction for rental costs, available to low-to-moderate income individuals	27%
Revenue Job Assist: Individuals who have been unemployed for 12 months or more and begin a new job are eligible for a reduced tax rate	33%
Tax Deductions for Veterans and Service Members: A tax deductible up to \$5000 available to veterans and active service members.	57%
Working Mother's Child Relief: A deductible of 15-25% of total income for working mothers.	26%

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Figure 1: Mean daily steps taken, difference between conspicuous incentives condition and the inconspicuous control condition, using raw data for days before, during, and after the intervention.

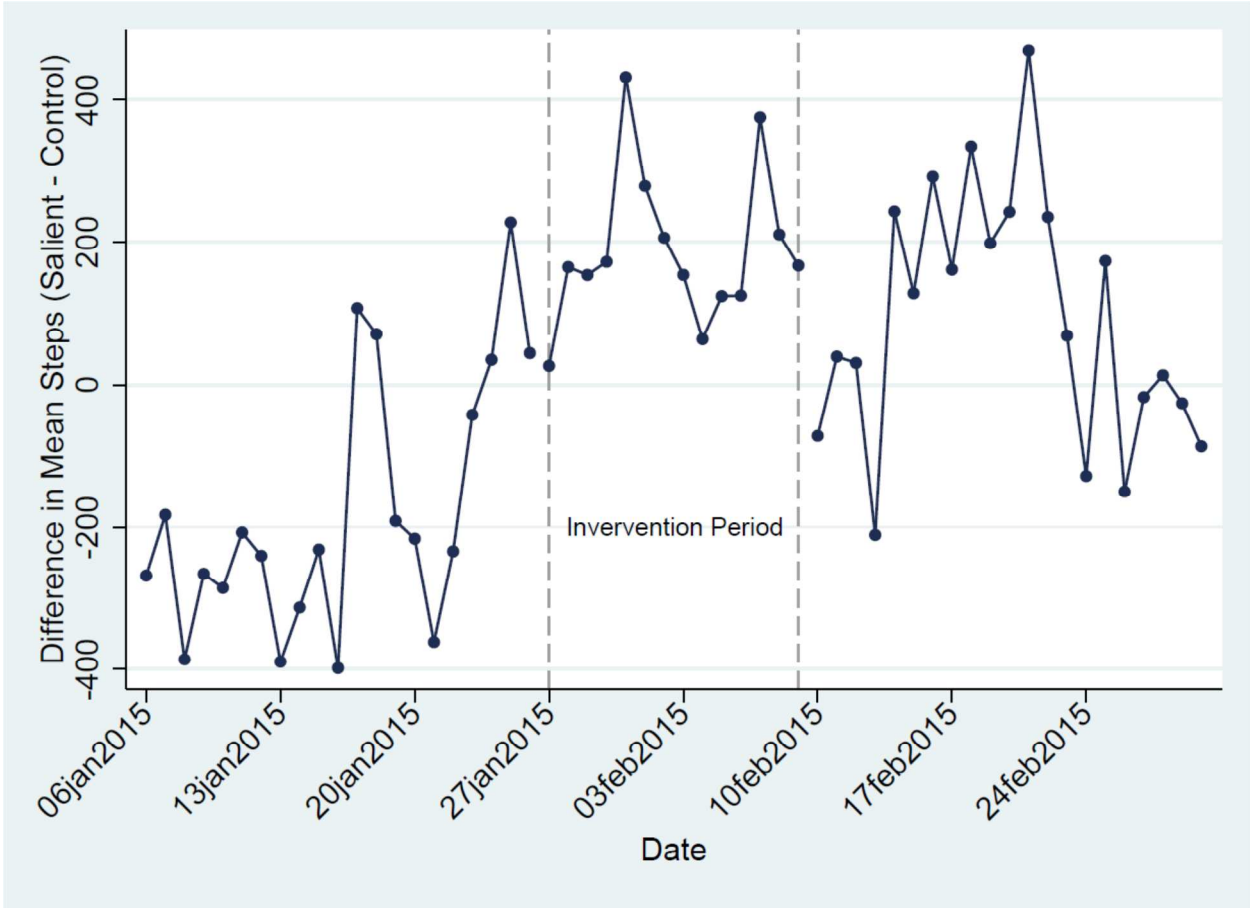


Table S1: This table reports coefficient estimates from an ordinary least squares regression model (see Equation 1) predicting daily steps taken by a given user. Robust standard errors are clustered by participant-day-of-week and reported in parentheses. The analyzed data include observations of participants' daily steps from the 21 days before the intervention, 14 days during the intervention, and 21 days after the intervention. Model 1 is estimated using all observations. Model 2 is estimated using observations with daily steps greater than 2,000.

	Model 1	Model 2
(Conspicuous Incentives Condition) x (Treatment Period)	451.8*** (77.72)	226.2*** (77.94)
(Conspicuous Incentives Condition) x (0-2 Weeks Post-Treatment)	416.7*** (79.47)	208.1*** (80.01)
(Conspicuous Incentives Condition) x (3 Weeks Post-Treatment)	230.7** (91.18)	93.42 (96.90)
Fixed effects for day of the year	Yes	Yes
Fixed effects for (user) x (day of the week)	Yes	Yes
Observations	115,080	85,789
R-squared	0.512	0.476
Clusters	14,385	13,760
Observations Included	All	Steps > 2000

Table S2: 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-day-of-week 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. Model 1 is estimated using all observations. Model 2 is estimated using observations with daily steps greater than 2,000.

	Model 1	Model 2
(In Experiment) x (Treatment Period)	-110.04* (49.84)	52.08 (50.85)
(In Experiment) x (0-2 Weeks Post-Treatment)	-413.82*** (91.89)	-198.22 (125.30)
(In Experiment) x (3 Weeks Post-Treatment)	-130.16* (65.39)	141.74* (68.97)
(Conspicuous Incentives Condition) x (Treatment Period)	367.06*** (58.34)	228.33*** (58.83)
(Conspicuous Incentives Condition) x (0-2 Weeks Post-Treatment)	331.92*** (61.89)	212.08*** (62.07)
(Conspicuous Incentives Condition) x (3 Weeks Post-Treatment)	145.98 (77.33)	96.00 (82.90)
Fixed effects for day of the year	Yes	Yes
Fixed effects for (user) x (day of the week)	Yes	Yes
Observations	235,424	172,117
R-squared	0.33	0.26
Clusters	29,428	28,008
Observations Included	All	Steps > 2000

Table S3: 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. Daily step data were Winsorized at the 99% level (observations of steps above the 99th percentile of steps were set equal to the 99th percentile). Model 1 is estimated using all observations. Model 2 is estimated using observations with daily steps greater than 2,000.

	Model 1	Model 2
(Conspicuous Incentives Condition) x (Treatment Period)	348.86*** (103.11)	210.55** (77.54)
(Conspicuous Incentives Condition) x (0-2 Weeks Post-Treatment)	328.57** (116.80)	210.18* (85.78)
(Conspicuous Incentives Condition) x (3 Weeks Post-Treatment)	137.85 (135.90)	85.22 (106.33)
Fixed effects for day of the year	Yes	Yes
Fixed effects for (user) x (day of the week)	Yes	Yes
Observations	115,080	85,789
R-squared	0.52	0.49
Clusters	2,055	2,029
Observations Included	All	Steps > 2000

Table S4: This table reports coefficients 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-day-of-week 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. Daily step data were Winsorized at the 99% level (observations of steps above the 99th percentile of steps were set equal to the 99th percentile). Model 1 is estimated using all observations. Model 2 is estimated using observations with daily steps greater than 2,000.

	Model 1	Model 2
(Conspicuous Incentives Condition) x (Treatment Period)	348.86*** (56.38)	210.55*** (54.73)
(Conspicuous Incentives Condition) x (0-2 Weeks Post-Treatment)	328.57*** (59.89)	210.18*** (57.67)
(Conspicuous Incentives Condition) x (3 Weeks Post-Treatment)	137.85 (72.47)	85.22 (73.70)
Fixed effects for day of the year	Yes	Yes
Fixed effects for (user) x (day of the week)	Yes	Yes
Observations	115,080	85,789
R-squared	0.52	0.49
N_clust	14,385	13,760
Observations Included	All	Steps > 2000

Table S5: 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 participants 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 the matched holdout group. Daily step data were Winsorized at the 99% level (observations of steps above the 99th percentile of steps were set equal to the 99th percentile). Model 1 is estimated using all observations. Model 2 is estimated using observations with daily steps greater than 2,000.

	Model 1	Model 2
(In Experiment) x (Treatment Period)	-99.81	66.69
	-87.06	-66.04
(In Experiment) x (0-2 Weeks Post-Treatment)	-346.56***	-101.7
	-102.66	-75.47
(In Experiment) x (3 Weeks Post-Treatment)	-145.21	124.48
	-119.43	-91.31
(Conspicuous Incentives Condition) x (Treatment Period)	348.86***	209.92**
	-103.08	-77.63
(Conspicuous Incentives Condition) x (0-2 Weeks Post-Treatment)	328.57**	209.97*
	-116.78	-85.87
(Conspicuous Incentives Condition) x (3 Weeks Post-Treatment)	137.85	84.5
	-135.87	-106.44
Fixed effects for day of the year	Yes	Yes
Fixed effects for (user) x (day of the week)	Yes	Yes
Observations	235,424	172,117
R-squared	0.53	0.51
Clusters	4,204	4,144
Observations Included	All	Steps > 2000

Table S6: 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-day-of-week and reported in parentheses. The analyzed data include observations of study 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 matched holdout group. Daily step data were Winsorized at the 99% level (observations of steps above the 99th percentile of steps were set equal to the 99th percentile). Model 1 is estimated using all observations. Model 2 is estimated using observations with daily steps greater than 2,000.

	Model 1	Model 2
(In Experiment) x (Treatment Period)	-99.81* (47.79)	66.69 (47.14)
(In Experiment) x (0-2 Weeks Post-Treatment)	-346.56*** (51.86)	-101.70* (49.64)
(In Experiment) x (3 Weeks Post-Treatment)	-145.21* (62.42)	124.48* (62.46)
(Conspicuous Incentives Condition) x (Treatment Period)	348.86*** (56.38)	209.92*** (54.80)
(Conspicuous Incentives Condition) x (0-2 Weeks Post-Treatment)	328.57*** (59.88)	209.97*** (57.74)
(Conspicuous Incentives Condition) x (3 Weeks Post-Treatment)	137.85 (72.46)	84.50 (73.80)
Fixed effects for day of the year	Yes	Yes
Fixed effects for (user) x (day of the week)	Yes	Yes
Observations	235,424	172,117
R-squared	0.53	0.51
Clusters	29,428	28,008
Observations Included	All	Steps > 2000

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2
3 **Figure S1.** Kickoff email.
4

5 Users in the conspicuous incentives condition received the following email on Jan. 26, 2015 (the day
6 before incentives began):
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9
10 Subject Line: New Program to Encourage You to Walk (earn Bonus Points)

11
12 Message:
13

14
15
16
17 The AchieveMint logo is displayed in white text on a pink rectangular background.

18
19
20
21  Bonus Points Opportunity
22

23
24
25 New Program to Encourage You to Walk (earn Bonus Points)
26

27 Tomorrow is the first day of a two week walking program designed in partnership with
28 experts at Harvard and the University of Pennsylvania to get you moving. Tomorrow and
29 every day after that for the next two weeks, we'll encourage you to walk by multiplying the
30 points you earn for walking by 40.
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38 To push you to walk more, your bonuses from Achievemint over the next two weeks will
39 follow this schedule:
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JANUARY 2015

Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
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25	26	27	28	29	30	31
		40x	40x	40x	40x	40x

FEBRUARY 2015

Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
1	2	3	4	5	6	7
40x	40x	40x	40x	40x	40x	40x
8	9	10	11	12	13	14
40x	40x					
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](#).

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1
2
3 **Figure S2.** Reminder emails.
4

5 Users in the conspicuous incentives condition received the following reminder email every other day (day
6
7 1, day 3, ..., day 13) during the 14-day incentive program.
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9
10 Subject Line: Program to Increase Walking (Earn Bonus Points Today and Tomorrow)

11
12 Message:
13

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16
17 The AchieveMint logo is displayed in white text on a pink rectangular background.
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19

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21  Bonus Points Opportunity
22

23
24
25 Program to Increase Walking (Earn Bonus Points Today and Tomorrow)
26

27 You are in the middle of a two week walking program designed in partnership with experts
28 at Harvard and the University of Pennsylvania to get you moving. Today and tomorrow,
29 we'll encourage you to walk: all points you earn for walking will be multiplied by 40.
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36 To push you to walk more, your bonuses from Achievemint over the next two weeks will
37 follow this schedule:
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JANUARY 2015

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

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](#).

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