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Attention, intentions, and follow-through in preventive health behavior: Field experimental evidence on flu vaccination☆

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ABSTRACT

Preventive health behaviors like flu vaccination have important benefits, but compliance is poor, and the reasons are not fully understood. We conducted a large study across six colleges ($N = 9358$), with a methodology that offers an unusual opportunity to look at three potential factors: inattention to information, informed intentions to not comply, and problems following through on intentions. We also tested three interventions in an RCT. We find that inattention to information is not the primary driver of low take-up, while informed decisions to not get the vaccine, but also lack of follow-through, are important factors. A financial intervention increased take-up and had persistent, positive effects on intentions for vaccination in future years. Two low-cost "nudges" did not increase vaccination rates, although the peer endorsement nudge increased exposure to information, especially if aligned with social networks.

1. Introduction

Vaccination against influenza is a public good with important economic implications (e.g., increased workplace productivity, reduced absenteeism, lowered health care costs), as well as substantial private benefits.[1] As with many other recommended preventive health behaviors, however, compliance is poor; the fraction of the population that is vaccinated against the flu each year is well below the CDC's stated goal (CDC, 2013a).[2] The problem of low vaccination rates is not limited to the flu, but is observed for a variety of communicable diseases (CDC, 2013b). Similarly, low rates of participation are problematic, in terms of health outcomes and health care costs, for a range of other beneficial health behaviors, like medication adherence (Volpp et al., 2008b), recommended cancer screenings (Weller et al., 2009), physical exercise (Charness and Gneezy, 2009; Royer et al., 2012), or healthy diet (Dansinger et al., 2005).

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[1] Seasonal influenza has a high mortality rate, with 1 in 10,000 Americans dying each year from flu and its complications; for those over 65, the rate is 1 in 20 (Ward, 2014). Seasonal flu also results in more than 200,000 hospitalizations each year (Thompson et al., 2003), and over 75 million missed work days (Benson and Marano, 1998).

[2] The CDC recommends every adult get a yearly flu vaccine, preferably early in the fall.

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A challenge in improving compliance with recommended preventive health behaviors is that the underlying decision making has largely remained a black box. Low take-up could be caused by individuals' uncertainty about the benefits of an activity (Madrian and Shea, 2001; Duflo et al., 2006; Kessler, 2011), or inattention to relevant information (Karlan et al., 2010; Kast et al., 2011; Altmann and Traxler, 2012). Alternatively, people may be informed, but still have a strong aversion to the activity, either due to some aspect of preferences, or a strongly held prior belief that benefits are low. Or it could be that individuals *do* intend to undertake the activity, but fail to follow through on those plans. This could reflect deviations from the perfect rationality assumptions, such as present-biased preferences, or imperfect memory (O'Donoghue and Rabin, 1999; DellaVigna and Malmendier, 2006; Duflo et al., 2011; Milkman et al., 2011; Royer et al., 2012). A better understanding of the roles of these factors would shed light on the most relevant models for explaining low take-up, and help guide design of policy interventions.

The first main contribution of this paper is to partially lift the veil on the reasons for low take-up of the flu vaccine. Data for understanding decision making would ideally include: a way to assess whether individuals attend to information; a measure of intentions; and data on actual behavior, allowing the study of follow-through on intentions. Our study uses a methodology that combines these features. An electronic messaging system tracks whether subjects open e-mails about flu vaccines, provides an indicator for in-depth reading of e-mails, and elicits self-reported intentions to get vaccinated. We then match the e-mail campaign data to a second data source: information from campus health centers on whether or not individuals actually came in and got the vaccine. Subjects were not informed about being in a research study, to minimize Hawthorne effects (Levitt and List, 2011).

As is always the case, even with field experiments, the generalizability of results may be affected by the specific population and setting studied, but studying college students offers several offsetting advantages. The population is attractive due to the possibility to obtain data from the campus health centers, and to have a large sample size ($N = 9358$). To our knowledge, ours is the largest study on flu vaccination decisions to date. College students are also themselves a highly policy-relevant population for studying flu vaccination, as they are poised to enter the workforce, and formation of good habits is especially valuable if it occurs early in life.[3]

Our results reveal that lack of information, or inattention to information, are not the key factors determining low take-up of vaccine. Instead, many of those who are informed nevertheless express an intention to not get the vaccine, and among those who *do* plan to get the vaccine, many fail to follow through on these intentions. One explanation for the latter result could be that stated positive intentions were inflated, as a way to impress the health center (social desirability bias), but this seems unlikely because the health center is known to observe eventual decisions anyway.[4] Another possibility is that these individuals truly intended to come in for the vaccine, but had trouble following through on their own plans.[5]

A second contribution of the paper is an experimental test of different types of interventions for improving flu vaccine take-up. Within each campus, subjects were randomized into one of four conditions. In the control condition, students received a series of three e-mails (one initial e-mail and two reminders) from the campus health center, which provided information about how to obtain a vaccine on campus. In the three interventions, subjects received the same number and timing of e-mails as in the control group, but the e-mail content was different.

The first intervention involved a modest financial incentive for getting the vaccine at the campus health center ($30 minus any out-of-pocket cost of the vaccine), which was received within two weeks of getting the vaccine to help increase the immediacy of the reward. Our results show that the incentive is associated with a significantly higher rate of opening e-mails, reading conditional on opening, stating positive intentions conditional on reading, and following through on intentions to get the vaccine. Ultimately, the incentive had a substantial positive impact on actual vaccine take-up.

A challenge that often arises in field experiments testing the impact of incentives is that the observed response might reflect substitution. For example, a discount on healthy foods in a specific grocery store could lead to an increase in sales, but actually just reflect reduced purchases on healthy foods at other stores. In the case of our study, substitution would mean the incentive just shifted students from off-campus to on-campus vaccination. To better assess whether incentives truly affect participation we built into the study a third phase of data collection, a large-scale survey fielded among the subject population after the flu season was over, which measured (self-reported) vaccination rates on and off campus during the whole year. We find that even allowing for substitution there was a substantial increase in overall take-up

[3] Although the young may be less vulnerable to influenza than the elderly, greater efficacy of vaccine among younger adults can imply that vaccination of the young has positive externalities for the elderly (Ward, 2014). College students might be more responsive to certain interventions, such as financial incentives, than the general population, due to lower income levels. On the other hand, they are a relatively healthy and less vulnerable population, and thus it may be more difficult to motivate college students to get the vaccine, which would make our treatment estimates a lower bound.

[4] Stating intentions to come in for the vaccine, and then not following through, would not be a good strategy for making a good impression, so that the bias might even work against stating intentions to get vaccinated.

[5] One explanation for such dynamic inconsistency is quasi-hyperbolic discounting (see Laibson, 1997), such that individuals have extreme discounting of tomorrow relative to today, but have only mild discounting (standard, exponential discounting) when evaluating any two adjacent future periods. In the present, individuals postpone getting vaccinated, because the cost is immediate and the benefits accrue only in the future. When thinking about the future, their relatively mild discounting may cause them to plan to get vaccinated in the future. Preference reversals can occur, however, once the future becomes the present, because then the immediate cost will make it once again unattractive to get vaccinated. Here, present-bias could lead to perpetual postponement of getting vaccinated, even though the individual's current period self does want to get the vaccine (in the future).

(34 percent, relative to control). Interestingly, the survey results are suggestive that the incentive treatment had a particularly large impact among those who were not vaccinated the prior year, and it increased intentions to get vaccinated in the future, when incentives will not be present. In combination these results could be consistent with "habit formation," or with the incentive generating persistent effects by signaling the social value of the flu vaccine (e.g., Vesterlund, 2003).

We also tested two novel, low-cost "nudge" interventions. In one intervention, we tested a way that an institution might try to harness peer effects, while still allowing the institution (e.g., health center) to control the nature and timing of information provided: by scripted endorsements (from volunteers), saying that they thought flu vaccination was a good idea. While this yields a conservative test of peer endorsements, relative to more personalized messages exchanged between peers on campus, it mimics a type of intervention already used by many organizations in other contexts (e.g., encouraging votes or donations in political campaigns). Importantly, it also allows us to keep other information in the e-mail, and timing of e-mails, the same as control, isolating the pure effect of this type of endorsement in a way that would not be possible if subjects endogenously chose recipients and/or information content.[6] We collected information about social networks of endorsers, to study the implications of social ties for endorsement effects. In the other intervention e-mails were augmented with a 3–4 s audio clip of a sick person coughing, as a way to potentially make sickness more salient at the time of forming intentions about vaccination. Previous evidence shows that images of sickness, e.g., posted on washroom walls, can increase preventive health behaviors like hand washing (e.g., Porzig-Drummond et al., 2009). Our aim was to test an audio version of this hypothesis, in the domain of online messaging, shortly before someone was asked to make a plan about getting vaccinated.

These nudges did not affect vaccine take-up (with more than 9000 observations we were powered to detect even modest effects). The endorsement was, however, associated with increased opening of emails, with a similar impact to that of the incentive condition. The effect was especially strong if the peer happened to be a close friend of the recipient, indicating the importance of social networks for spreading information. As we discuss below, our design addresses some of the problems with establishing causality of peer effects (e.g., Manski, 1993).

Our results have a number of further implications. While incentives were shown to be a powerful motivator, the evidence that there is some substitution from off-campus vaccination illustrates the importance of targeting financial interventions towards individuals who would not otherwise get vaccinated, in order to increase cost-effectiveness.[7] The impact of the peer endorsements on e-mail opening suggests that this approach might be useful, if combined with other interventions, to maximize exposure to information. While we find that a scripted endorsement was not effective for increasing take-up, other approaches to harnessing peer effects that sacrifice control of information transmission, such as simply exhorting individuals to post about vaccination on their Facebook pages, might be effective. The finding that salience did not affect intention formation may reflect special challenges posed by the online context, where receiving the treatment requires clicking on a link.

Our study contributes to a literature on understanding the decision making underlying potentially self-defeating behaviors, including under-saving, postponing medical check-ups, poor academic performance, and lack of exercise (Altmann and Traxler, 2012; Charness and Gneezy, 2009; Kast et al., 2011; Wong, 2008). The paper also adds to a literature on policy interventions focused on behavioral economics and health. Reminders have been shown to influence behavior in a variety of domains, including vaccination rates (Briss et al., 2000; Szilagyi et al., 2000), and augmenting reminders with implementation prompts leads to an additional increase in vaccine take-up (Milkman et al., 2011). We incorporated reminders and a version of intention prompts into our control condition, and investigated the ability of other interventions to improve outcomes relative to this benchmark. A recent study on testimonials and generic drugs finds limited impact, providing converging evidence that this approach may not be effective (Beshears et al., 2013). Evidence on financial incentives and vaccination is scarce, but an exception is Banerjee et al. (2010), who show that a financial incentive equivalent to one day's wages increases take-up of a childhood immunization package in rural India (N = 1640).[8] Our study is different from the previous literature in the way that it sheds light on the black box of decision making, yielding insights that may help guide development of new interventions focused on preferences and follow-through on intentions. We also contribute a test of novel interventions for increasing flu vaccination. We provide evidence of a strong positive effect of an incentive intervention, an effect that does not appear to be explained by substitution from off-campus to on-campus vaccination.

The rest of the paper is organized as follows. Section 2 describes the design of the study. Section 3 presents the empirical results on the reasons for low take-up of flu vaccine, and on differences across treatments in the rates of email opening/reading, intentions to get the vaccine, and vaccination. Section 4 concludes.

[6] See Kessler (2011) for experimental evidence on how social endorsements affect contributions to public goods in the form of donations to charity. See also Falk and Ichino (2006), Mas and Moretti (2009), and Bandiera et al. (2010).

[7] The benefits in terms of reduced incidence of flu need to be monetized, however, and compared to the costs, in order to fully assess the cost effectiveness of the incentive approach. For a recent analysis of the benefits of flu vaccination, see Ward (2014).

[8] A different approach, making it the default to for college employees to have a flu vaccine appointment, increased take-up by about 35 percent (Chapman et al., 2010). A related literature studies a range of preventive health behaviors, such as smoking cessation, exercise, weight loss, or medication adherence (e.g., see Volpp et al., 2008a, 2009; Charness and Gneezy, 2009; Royer et al., 2012, and others).

Table 1
Characteristics of campuses included as experimental sites.

Study site	Number of subjects	Number of peer endorsers	Vaccination period	Vaccine availability	Cost of vaccine to students/parents	Payment method
College 1	1363	72	October 3–December 3	Walk-in or appointment	$20	Billable to student account
College 2	1145	53	October 1–December 3	All-day clinics on October 1 and 3 or appointment	$35	Billable directly to insurance
College 3	1175	62	October 22–December 3	Walk-in or appointment	$25	Billable to student account
College 4	1489	71	October 3–December 3	2-h clinics on 10/3, 10/18, 11/6 or appointment	$20	Must be paid in cash or check
College 5	1951	89	October 9–December 3	Walk-in or appointment	$20	Must be paid in cash or check
College 6	2233	84	October 1–December 3	Walk-in or appointment	$20	Billable to student account

2. Design of the study

2.1. Field setting and study participants

The study took place at six undergraduate colleges located within a 90-mile radius of Philadelphia, Pennsylvania, with student populations ranging from approximately 1100 to 2500 students. We conducted the study at small colleges because students are relatively likely to have social ties with a randomly chosen peer on campus, which is relevant for our peer endorsement intervention.

We observe subjects in the environment in which they usually undertake the tasks of interest (opening/reading e-mails, getting a flu vaccine), and subjects do not know they are being studied. Students are automatically included in our study population if they are enrolled for the fall 2012 semester, are not studying abroad, and are 18 or older.[9,10] This natural field experiment approach "combines the most attractive elements of the experimental method and naturally occurring data: randomization and realism" (List, 2011).

We conducted the initial phase of the study during the height of flu vaccine season for 2012, in the months of October and November. On each campus, the experimental period began when the first round of e-mails was sent, just prior to the beginning of flu vaccination at the campus health center. The earliest start date was October 1, and the e-mail campaign and study period concluded on all campuses on December 3, 2012. Table 1 describes differences across campuses in the timing of flu vaccination, the availability of the vaccine, and the cost of the vaccine to students/parents. At all of the schools, students are free to request an appointment for a flu vaccine at any time during health center operating hours. However, two campuses conduct dedicated flu shot clinics on a few publicized dates, while the other four campuses have a policy that allows students to "walk in" for a vaccine during regular hours, without an appointment. On four campuses, the cost of the flu vaccine to students can be directly billed to their student accounts (often paid by parents) or to their health insurance providers. The vast majority of students choose this payment method; these students pay no out-of-pocket cost and may perceive the vaccine's effective price to be zero. Two colleges require students to pay the charge by cash or check at the time of the vaccine.

2.2. Measuring e-mail readership, intentions, and vaccine take-up

We commissioned a customized e-mail platform for the purposes of this study, which permits tracking several outcomes related to e-mail viewing and readership. First, we can observe that a student has opened an e-mail if the student: (1) views the e-mail in HTML, (2) views the e-mail in plain text but accepts the request to "display images" (which is displayed in an eye-catching way at the top of the e-mail), or (3) clicks on any link in the e-mail. We will underestimate the opening rate if some students view e-mails in plain text and do not display images or click links. This underestimation should occur at

[9] Our study received a waiver of informed consent from the relevant IRBs, by meeting the requirements specified in the federal guidelines.
[10] A group of students on each campus was recruited prior to the experiment and offered the option to serve as peer endorsers of flu vaccines. These students are excluded from the subject pool of e-mail recipients. We describe this group of students further below.

random across treatment and control groups.[11] We measure opening with an indicator for whether the student opened *at least one* of the three e-mails.

Of course, that a student opens an e-mail does not necessarily mean that he/she reads or absorbs the information in the message. To check whether subjects made it to the end of the e-mail content, there was solicitation at the end of the message: students were asked "to help the student health center's planning" by reporting whether or not they planned to get a flu vaccine on campus. The e-mail instructed students that by clicking either of two links (*YES, I think I will probably get a flu vaccine* or *NO, I do not think I will get a flu vaccine*), they would be able to enter in a lottery to win one of five $100 rewards (per campus) to be issued at the end of the semester. This measure will understate e-mail readership to the extent that students read the information about flu vaccines but do not click on these links. On the other hand, because these links were at the bottom of the e-mail messages, when we observe a student clicking on one of these links, we can feel more confident that he/she was exposed to the content of the e-mail. From here on, when we refer to reading e-mails as opposed to opening e-mails, we use this measure of readership.

Second, we measure students' intentions regarding whether to get a vaccine at the campus health center by tracking responses to this question (i.e., whether students click 'Yes' or 'No').[12] Thus, we can observe the rate at which informed students make a decision (or intend) to get a flu vaccine, at the time of receiving information, and we can test whether the treatments differentially impact intentions. By matching e-mail tracking data with data on vaccinations, we can determine whether students follow through on their reported intentions, and test for treatment impacts on follow-through.

Our second phase of data collection involved obtaining flu vaccination records from the six college campuses on a bi-weekly schedule. These records identify students who were vaccinated and the dates of their vaccinations.[13]

The third and final phase of our study was implemented after the academic year, to capture vaccination decisions even after the main flu season. In this survey we collected self-reported information from students on all six campuses, regarding their flu vaccine decisions, the location and timing of their vaccines (if applicable), and their intentions about future vaccination.

2.3. Treatment-control contrast

Our study compares treatment conditions involving incentives, social endorsements, and salience, to a control condition in which subjects receive only information and reminders (see Appendix A for treatment and control e-mails). A student in the control group receives an initial e-mail approximately 12–24 h before flu vaccination begins on his campus. The e-mail appears to come from the campus health center. It begins with a statement that the American College Health Association (ACHA) recommends getting a flu vaccine each year and provides information about how to obtain a vaccine on campus (e.g., dates, hours, cost, and methods of payment). It also contains text emphasizing the personal benefits of flu vaccination as well as the positive externalities of vaccination. About two to three weeks later, we send a second e-mail that is identical to the initial message and serves as a reminder. After another 2–3 weeks, a final reminder e-mail is sent.

In all three treatments, e-mails and reminders are sent to students with exactly the same timing as in the control condition. The e-mails include the same information about how to get a flu vaccine as in the control e-mails, but the first few lines of the message are altered to reflect the treatment. Fig. 1 displays how the control and treatment e-mails might appear in a recipient's inbox. Through differences in the e-mail's first sentence, the e-mail sender, and the subject line, treatment-control contrast is achieved even before e-mails are opened.

Financial Reward Treatment ('*Incentive*'): Subjects in the incentive treatment receive an e-mail from the student health center with the subject line, "*Flu vaccine (get $30!)*" The first lines of the e-mail inform students that they have been randomly selected to receive a payment of $30 in cash if they get a flu vaccine at the campus health center on or before December 3, 2012. To minimize the time between vaccination and reward, and maximize the ability of the intervention to increase *near-term* benefits of vaccination, the message promises that $30 in cash will be placed in an envelope in the student's mailbox within 1–2 weeks of receiving the vaccine. That the e-mail comes from the campus health center and provides multiple ways to contact the health center to ask questions, may help strengthen the credibility of this promise. Other content is the same as in control, with two identical reminders over the next 4–6 weeks.

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[11] One exception is that students in the Salience/Coughing treatment have an additional opportunity to click on a link and be tracked as having opened the e-mail. However, we find no evidence of an impact of the coughing treatment on e-mail opening.

[12] Once students report their intentions, they continue to receive any remaining reminder e-mails, and have an opportunity to report intentions again. We use whether an individual ever clicks 'Yes' as our measure of intentions.

[13] While this research qualified for a waiver of HIPAA requirements, three of the health centers asked students to sign a statement releasing the information that they got a flu vaccine to researchers at Swarthmore College. If the incentive treatment, for example, caused students on these campuses to be more likely to sign this consent form, this could bias our incentive effect estimate upward (as we would be more likely to observe students in the incentive group getting vaccines). Evidence from our follow-up survey suggests this was not an important source of bias. We also run the vaccine regressions separately for campuses which required consent and those which did not, and find no meaningful differences in treatment effects.

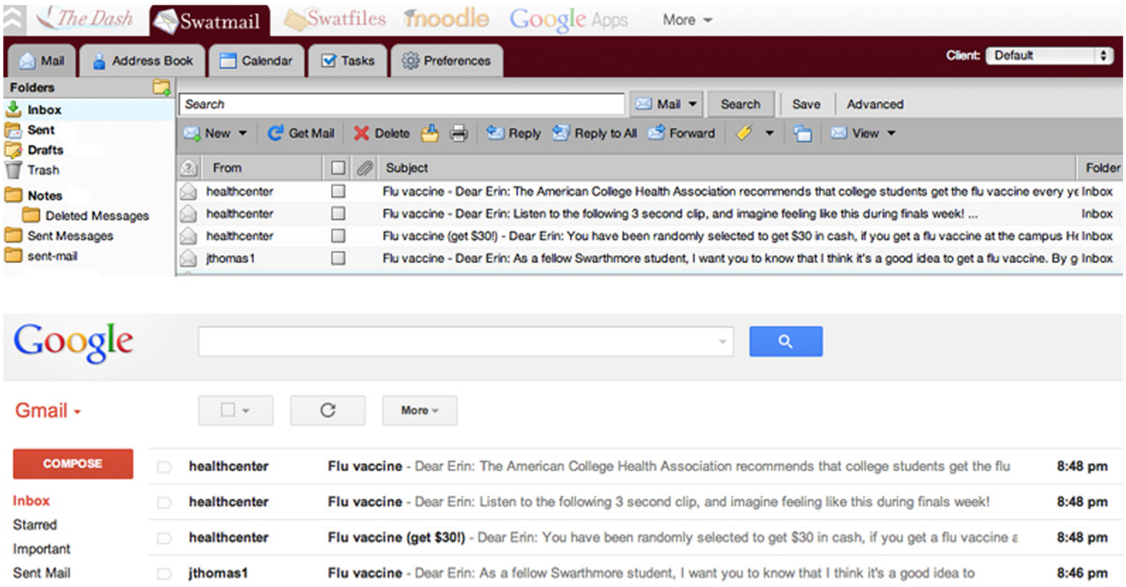

Fig. 1. Appearance of control and treatment e-mails in inbox.

Social Endorsement Treatment ('*Peer*'): A second treatment harnesses the potential importance of a peer recommendation. In this condition the initial e-mail that students receive comes from a peer on campus; the subject line is as in the control ("*Flu vaccine*"), but the *From* field of the student's inbox displays a peer's name or unique campus e-mail id. Moreover, the first line of the e-mail reads, "*As a fellow* [College Name] *student, I want you to know that I think getting a flu vaccine is a good idea.*" Importantly, the two reminder e-mails that come over the next several weeks are identical to the initial e-mail, but they come from different peer endorsers.[14] We describe the group of volunteer peer endorsers further below.

We match peer endorsers *randomly* to recipients in the peer condition, for several reasons. First, it would have been logistically challenging to allow endorsers to endogenously choose recipients: to preserve the treatment-control contrast we would have needed to offer them a list of possible recipients from the randomly-assigned peer treatment group, rather than allowing them to choose freely. If allowed to select recipients freely, subjects could not be prevented from selecting ones in the control group, confounding the RCT. Second, our approach ensures that all recipients in the peer treatment receive exactly the same number and timing of e-mails as recipients in the control group, isolating the effect of the sender identity. Third, the random assignment allows us to remove some obstacles to assessing the causal impact of social ties or friendship for the power of peer effects. Because peer endorsements sometimes went to friends and sometimes to strangers, we can study whether a match with a friend has a differential effect than with a stranger. Moreover, because there are friends of peer endorsers who are *not* in the peer treatment group, we can control away the potential confound that friends of volunteer vaccine endorsers might respond favorably to emails about vaccination simply because the friends have similar preferences. If we find that matches with friends have stronger effects, this provides an evidence base for the importance of social networks and future interventions that target social networks.

Salience Treatment ('*Coughing*'): Our final treatment aims to make the benefits of vaccination more *salient* to students. The e-mails sent to students in this group come from the campus health center and have a subject line of "*Flu vaccine*." Each e-mail begins with the text, "*Listen to this 3-second clip, and imagine feeling like this during finals week!*," after which is a link to an audio file of a sick person coughing. Once a student clicks on the embedded link to listen to the audio clip, the link expires. This restriction limits the potential for treatment contamination via forwarding of the e-mail, and mitigates the concern that students in other conditions may be exposed to the salience treatment. Again, students also receive two identical reminder e-mails, so each has a total of three opportunities to listen to the coughing audio clip.

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[14] One practical reason for this decision was to increase the likelihood that some students in the peer endorsement treatment group would (at random) receive at least one message from a close friend, allowing us to distinguish whether a message through existing social ties had a greater impact than a message from another peer on campus. A second practical reason had to do with recruiting peer endorsers. We worried that students would feel uncomfortable participating if their names were used to send repeated messages to the same fellow student.

Table 2
Mean observable student characteristics by treatment group.

	Means by treatment group				p-values on t tests of equal means					
	(1) Control	(2) Incentive	(3) Peer	(4) Coughing	Col. 1=2	Col. 1=3	Col. 1=4	Col. 2=3	Col. 2=4	Col. 3=4
Female	0.582	0.565	0.563	0.583	0.24	0.17	0.94	0.86	0.21	0.15
	(0.493)	(0.496)	(0.496)	(0.493)						
International student	0.055	0.055	0.056	0.050	0.99	0.94	0.40	0.94	0.39	0.35
	(0.228)	(0.229)	(0.229)	(0.217)						
Freshman	0.270	0.260	0.269	0.258	0.42	0.96	0.36	0.46	0.90	0.39
	(0.444)	(0.439)	(0.444)	(0.438)						
Sophomore	0.265	0.253	0.269	0.283	0.35	0.73	0.18	0.20	0.02	0.31
	(0.441)	(0.435)	(0.444)	(0.450)						
Junior	0.210	0.226	0.211	0.218	0.18	0.91	0.49	0.22	0.52	0.57
	(0.407)	(0.418)	(0.408)	(0.413)						
Senior	0.255	0.261	0.250	0.241	0.62	0.69	0.27	0.37	0.11	0.48
	(0.436)	(0.439)	(0.433)	(0.428)						
Is friends with any sender	0.164	0.163	0.155	0.172	0.92	0.40	0.48	0.46	0.41	0.12
	(0.370)	(0.369)	(0.362)	(0.377)						
Has out-of-pocket cost	0.375	0.376	0.363	0.357	0.95	0.41	0.20	0.37	0.17	0.64
	(0.484)	(0.484)	(0.481)	(0.479)						
Zip code median income	9.015	8.964	9.836	9.095	0.64	0.47	0.47	0.80	0.23	0.15
	(3.560)	(3.621)	(3.521)	(3.627)						
Observations	2337	2353	2332	2336						

Note: Sample sizes for means for zip code median income are 2168, 2171, 2172, and 2154, respectively, because zip codes are missing for international students.

A risk of this type of intervention is that it relies on a hyperlink to an audio file, which could trigger suspicion or concern about "spam" among recipients.[15] One technical limitation we encountered when designing this intervention is that the subject had to be taken to a new browser window upon clicking the audio link. Although it required only a single click to return to the e-mail, there was a small cost of continuing reading that was not present in other conditions.

We present evidence of successful random assignment in Table 2. There is only one significant difference in observable characteristics of student e-mail recipients according to treatment status; sophomores were more likely to wind up in the salience condition than in the incentive treatment group. Results of auxiliary regressions in which treatment status is the dependent variable also suggest that student characteristics were balanced across control and treatment groups.

Our power calculations suggest that the sample size (and the size of each experimental arm) is sufficient to detect a 3.2-percentage point increase in flu vaccination, relative to the baseline vaccination rate for the control group of 9 percent, after correcting for multiple comparisons.[16] Differences across campuses (Table 1) and across individuals within campuses may also affect statistical power. We return to the issue of statistical power when interpreting our results below.

2.4. Peer flu vaccine endorsers

Prior to the experiment, we recruited students on each campus to participate as "peer flu vaccine endorsers" for their campus. We intentionally aimed to recruit peer vaccine endorsers whose names were likely to be recognized by fellow students, perhaps because they were leaders on campus. Before the beginning of the fall semester, we recruited students who arrive early to campus, like athletes, student residential advisors (RAs), student academic mentors, and members of the student orientation staff.

The recruitment procedure involved inviting students to participate in a two-part research experience that would take them about 10 min. Students were compensated for their time with $30 in cash and a chocolate chip cookie. The first part of the research involved a paper survey including questions about his/her roles on campus, class year, leadership qualities, and social networks. We asked students to list the first and last names of their 10 closest friends on campus.

The second part of the experience involved describing the flu vaccination field experiment to students and asking whether they would like to participate by allowing us to use their names as if they were the senders of the

[15] For example, recent research by Goldstein et al. (2014) has highlighted that certain features (e.g., motion, animation or "seems like spam") of Internet ads make them annoying and increase the odds that people will take steps to avoid them.

[16] We initially predicted somewhat higher power; our ex-ante calculations were based on an expected sample size of 10,000 and an expected baseline rate of vaccination of 6 percent.

Table 3
Mean characteristics of student peer vaccine endorsers.

Peer endorser trait	College 1	College 2	College 3	College 4	College 5	College 6	Full sample
Leader on campus (1–7)	5.264	4.816	5.234	4.986	5.276	5.964	5.300
	(1.300)	(1.523)	(1.330)	(1.634)	(1.273)	(1.017)	(1.380)
Others listen to opinion (1–7)	5.847	5.796	5.859	5.831	5.782	5.880	5.833
	(0.959)	(0.645)	(0.852)	(0.676)	(0.882)	(1.183)	(0.901)
NCAA athlete	0.444	0.490	0.609	0.803	0.713	0.313	0.563
	(0.500)	(0.505)	(0.492)	(0.401)	(0.455)	(0.467)	(0.497)
Student advisor or mentor	0.667	0.612	0.188	0.296	0.322	0.349	0.394
	(0.475)	(0.492)	(0.393)	(0.460)	(0.470)	(0.480)	(0.489)
Number of friends listed (of 10)	9.361	9.429	8.953	9.521	9.195	9.012	9.232
	(1.447)	(1.137)	(1.598)	(1.080)	(1.561)	(1.979)	(1.535)
Male	0.431	0.673	0.000	0.394	0.425	0.434	0.387
	(0.499)	(0.474)	(0.000)	(0.492)	(0.497)	(0.499)	(0.488)
Freshman	0.028	0.184	0.281	0.155	0.207	0.084	0.153
	(0.165)	(0.391)	(0.453)	(0.364)	(0.407)	(0.280)	(0.360)
Sophomore	0.389	0.306	0.172	0.239	0.322	0.157	0.263
	(0.491)	(0.466)	(0.380)	(0.430)	(0.470)	(0.366)	(0.441)
Junior	0.194	0.306	0.188	0.268	0.253	0.337	0.258
	(0.399)	(0.466)	(0.393)	(0.446)	(0.437)	(0.476)	(0.438)
Senior	0.389	0.204	0.359	0.338	0.218	0.422	0.326
	(0.491)	(0.407)	(0.484)	(0.476)	(0.416)	(0.497)	(0.469)
Observations	72	49	64	71	87	83	426

e-mails in the peer endorsement treatment.[17] We placed no pressure on students to participate in this way and reassured them that they would receive the $30 and cookie, regardless of their decision. Nonetheless, 83 percent agreed to participate.

Mean characteristics of the group of peer vaccine endorsers are shown in Table 3. As expected, collegiate athletes, student advisors, and females are disproportionately represented. The pre-recruitment survey asked students to respond to the statements, "*I feel that I am a leader on campus*," and "*My friends and peers seem to value my opinion and listen to my advice*," with ratings on a scale from 1 (strongly disagree) to 7 (strongly agree). The average scores among peer vaccine endorsers were 5.3 and 5.8, respectively, indicating that vaccine endorsers generally feel they are campus leaders whose advice and opinions are important to peers. There are some important differences in the average characteristics of endorsers across campuses. We include campus dummies in all but the most parsimonious regressions in Section 3, and we test for differences in the peer endorsement effect according to the traits of the endorser.

3. Results

3.1. What accounts for low flu vaccine take-up?

Fig. 2 sheds light on how individuals engaged with the e-mail campaign and approached the vaccination decision. The first panel of the figure shows unconditional probabilities of e-mail opening, e-mail reading, intentions to get vaccinated, and vaccination behavior, for the control group. We find similar qualitative results across all treatment conditions, but the treatments do differentially impact the fraction of subjects reaching each stage of the decision-making process. We discuss these differential effects further below.

An initial question is about the role of inattention. Can an e-mail campaign effectively deliver information about flu vaccines to college students, or do students simply ignore such e-mails? The baseline (control group) rate of e-mail opening in our experiment is 67.4 percent; that is, the majority of students will open an institutional message about a preventive health decision, like flu vaccination. Notably, even among individuals who do not open the e-mails, there could be those who are informed about flu vaccination from other sources. This is another reason that the rate of e-mail opening we observe is likely a lower bound for exposure to relevant information, reinforcing the impression that low take-up is not explainable solely by lack of exposure to information. In terms of e-mail readership, Fig. 2 shows that about two-thirds (66 percent) of control group members who open the e-mail thoroughly read its content (the unconditional probability of reading is 44 percent).

Once subjects have opened and read the e-mails, we can assess how many of these individuals, whom we know to be informed, plan to get vaccinated. Fig. 2 reveals that just over half (56 percent) of students who have read the e-mail

[17] Students quickly understood the idea and likened it to receiving e-mails from "Barack Obama," "Beyonce," or "Ann Romney," during the 2012 presidential campaign.

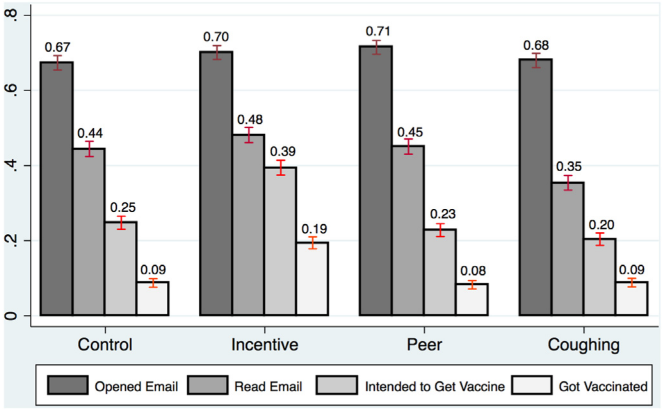

Fig. 2. Rates of email opening, reading, plans to get vaccinated, and vaccination (Unconditional probabilities; by treatment group).

(indicated by reporting whether or not they intend to get a vaccine), respond that they think they will probably get a flu vaccine at the health center. This is despite the fact that this statement of intention is fairly weak and involves no actual commitment on the part of the student. Thus, for many individuals, their preferences do not lead them to decide in favor of getting the vaccine. This could reflect many factors, from the perceived effort cost of getting the vaccine, to concerns about side effects. Of those who say they do not intend to come in for the vaccine, 99.6 percent follow through on that plan.

Finally, among students who report to the health center that they intend to come in for a flu vaccine, we can observe how many in fact show up. In this case we find a rather low rate of follow-through, of only 27 percent.[18] One possible explanation would be that students are for some reason more likely to lie about their intentions, in the case that they say they intend to get the vaccine (but are truthful about negative intentions). Yet it is not clear what subjects would gain by falsely representing their intentions. The health center observes eventual vaccination decisions anyway, so stating positive intentions and then not following through is not a good way to impress the health center; to the extent that "social desirability bias" (see King and Bruner, 2000) affects responses, it might actually tend to reinforce follow-through on positive intentions.[19] An alternative explanation is that people *do* mean it when they say they intend to get the vaccine, but then have trouble following through. This might reflect present-biased preferences (e.g., due to hyperbolic time discounting) or forgetfulness, but could also be explained by frequent updating of beliefs or by preferences that are unstable for reasons not owing to time discounting.

These results provide insight into the challenges to increasing compliance with recommended preventive health behaviors, like flu vaccination. In particular, they show that lack of information is probably not the leading cause of low vaccine take-up. Rather, intentions to not get the vaccine, despite being informed, and lack of follow-through on intentions to get vaccinated, play important roles.

3.2. Treatment effects on email opening and reading

We first investigate whether any of the three treatment interventions caused students to be more likely to open e-mails about flu vaccination. Recall that some treatment-control contrast is achieved even before e-mails are opened in that the e-mails appear differently in a recipient's inbox (Fig. 1).

The first panel of Table 4 contains results from regressions explaining the decision to open an e-mail about flu vaccination (columns (1)–(4)).[20] The dependent variable is an indicator for whether the student opened any of the three e-mails that we sent. Column (1) is a parsimonious regression on treatment dummies alone, while column (2) adds campus dummies. In column (3) we add an interaction between the peer treatment indicator and an indicator that equals one if the e-mail recipient was listed as one of the 10 closest friends of any of his three peer vaccine endorsers (i.e., if any of the three endorsement emails came from a close friend), along with controls for student and campus characteristics. This model also includes an indicator that captures the effect of being a friend of any of the vaccine endorsers (regardless of one's treatment group). Note that randomly pairing endorsers with recipients allows us to estimate the separate effects of being friends with

[18] Note that the unconditional probability of vaccination for the control group, divided by the unconditional probability of positive intentions, is higher than 27 percent because some students get vaccinated without ever reporting intentions.

[19] Perhaps students hope the health center will attribute their lack of follow-through to a decision to get vaccinated off campus, but this seems unlikely to be a powerful motive. Also, students are asked about plans to get it at the health center, to help the health center's planning. This makes lack of follow-through (saying they will come but then going off-campus instead) socially undesirable.

[20] Marginal effects from probit estimation are remarkably similar in both magnitude and significance to OLS estimates. Results available upon request.

Table 4
Effects of treatments on email opening and readership.

	(1) Opened email	(2) Opened email	(3) Opened email	(4) Opened email	(5) Read email	(6) Read email	(7) Read email	(8) Read email
Incentive	0.027**	0.027**	0.039**	0.042**	0.037***	0.036**	0.052***	0.052***
	(0.013)	(0.013)	(0.016)	(0.017)	(0.014)	(0.014)	(0.017)	(0.018)
Peer	0.041***	0.039***	0.038***	0.044***	0.007	0.004	0.006	0.006
	(0.014)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)
× Peer is close friend			0.187**	0.179*			−0.114	−0.106
			(0.092)	(0.093)			(0.098)	(0.098)
Coughing	0.006	0.003	0.004	0.008	−0.090***	−0.094***	−0.093***	−0.087***
	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)
Female			0.071***	0.073***			0.093***	0.098***
			(0.010)	(0.010)			(0.011)	(0.011)
International student			0.053**	–			0.072***	–
			(0.021)				(0.023)	
Freshman			0.058***	0.048***			0.050***	0.037***
			(0.013)	(0.014)			(0.014)	(0.014)
Junior			0.016	0.011			−0.001	−0.004
			(0.014)	(0.014)			(0.015)	(0.015)
Senior			0.012	0.007			0.014	0.015
			(0.013)	(0.014)			(0.014)	(0.014)
Is friends with any sender			0.008	0.004			0.005	0.001
			(0.013)	(0.014)			(0.014)	(0.014)
Has out-of-pocket cost			−0.098***	−0.167***			−0.118***	−0.146***
			(0.018)	(0.018)			(0.019)	(0.019)
Out-of-pocket × incentive			−0.030	−0.026			−0.037	−0.030
			(0.023)	(0.023)			(0.024)	(0.025)
Zip code income (10,000s)				0.004***				−0.006***
				(0.001)				(0.001)
Constant	0.674***	0.755***	0.687***	0.664***	0.444***	0.529***	0.454***	0.513***
	(0.010)	(0.015)	(0.018)	(0.022)	(0.010)	(0.016)	(0.019)	(0.024)
Campus dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	9358	9358	9358	8665	9358	9358	9358	8665

Standard errors in parentheses; columns (4) and (8) include the subjects for whom we have zip code data and therefore exclude international students; median income for each student's zip code is obtained from the 2010 Census. Two campuses have an out-of-pocket cost of $20, which students must pay by cash or check at the time of the vaccine. Outcome variable in columns (1)–(4) is indicator for opening an email; outcome in columns (5)–(8) is indicator for reading email thoroughly, as indicated by clicking link at bottom of email to be eligible to win a financial prize.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

an endorser (and potentially sharing similar views and preferences), from the effect of receiving an e-mail from a sender who is a friend. In column (4) we verify that results are robust to adding a control for median income in the home zip code of the student. The results in these regressions, and in other regressions reported in the paper, are robust to adjusting standard errors for correlation within campus.

The results in Table 4 indicate that the incentive and peer endorsement treatments both significantly increased the probability of email opening, by approximately 4 percentage points relative to control. However, we find that the interaction of the peer treatment with an indicator for whether the recipient was a close friend of the endorser is large and statistically significant. A peer message raises opening by about 22 percentage points when the peer is a close friend, outperforming both messages from non-friends ($p = 0.05$) and the incentive treatment ($p = 0.04$). This suggests that targeting peer endorsements within social networks might amplify their power. As displayed in Table A1, other characteristics of peer endorsers (e.g., whether the endorser was the same gender as the recipient, a student advisor/mentor, an athlete, or an upperclassman) were not found to impact the peer treatment effect on email opening (or on other vaccine outcomes). We also tested whether receiving an email from a close peer *first*, versus in the second or third emails, differentially impacted attention to the email. We did not find any evidence that the timing of the message from a close friend matters.[21] These estimates are imprecise, however, due to the relatively small number of subjects who receive a peer endorsement from a close friend, so we are hesitant to draw strong conclusions. The salience treatment, which included an audio clip of a sick person coughing, did not impact e-mail opening, relative to the control.

Next, we turn to a stricter indicator of attention to information about the flu vaccine, measuring whether a student gets to the stage of both opening the e-mail and clicking on a link at the end of the e-mail (in response to an offer of a chance

[21] Results available upon request.

to win $100). Results from regressions explaining the prevalence of opening combined with in-depth reading are displayed in the second panel of Table 4 (columns (5)–(8)). These results indicate that the financial incentive significantly increased the joint probability of opening an e-mail and reading the e-mail (as indicated by clicking on a link at the bottom of the message), by about 4 percentage points. Thus, the positive impact of the incentive continues to the level of both opening and reading e-mails.

Although the peer endorsement treatment impacted the rate at which students opened e-mails about vaccines, Table 4 shows that there is no significant difference, relative to control, at the stage of opening and reading. This indicates that the probability of reading e-mails, conditional on having opened, must be smaller in the peer condition than in control. One potential explanation is that recipients were turned off to reading the information once they realized that the email was a form email, rather than a personalized message. This interpretation would be consistent with the large, negative coefficients on the interaction with whether the peer was a close friend (although we note these are imprecisely estimated). The salience (coughing) treatment is associated with a *reduction* in the joint probability of opening and reading the e-mail, relative to control. Again, this implies a negative impact on the conditional reading probability. One possibility is that students in this treatment group clicked on the link to hear the audio clip and then did not return to the e-mail to click on the links at the end, because there was a small "cost" of doing so. However, those who click on the coughing link are found to be more likely to click links at the end of the e-mail than those who do not click the coughing link, inconsistent with this explanation. Alternatively, seeing the initial link, and the solicitation to click it, might cue some students to think about "spam," and be unwilling to click later links, even if they read the e-mails. This direct negative impact of the coughing intervention on the decision to click a link could explain this result.

In summary, a financial incentive increased the rate at which subjects opened and read e-mails with health information. While including an interesting link and making e-mails appear as if they were sent by a recognizable person are common approaches, our results suggest their impacts on information receipt may be limited, in that they may not generate in-depth reading even if they foster opening of e-mails.

3.3. Treatment effects on flu vaccine intentions and vaccine take-up

Table 5 shows how the three treatments are related to whether students report making a decision, or having an intention, to get a flu vaccine on campus, and to whether students actually get vaccinated. The results in columns (1)–(4) show the causal impacts of the interventions on the joint probability that subjects open, read, *and* click the link *YES, I think I will probably get a flu vaccine*.

We find the financial incentive causes a much higher proportion of individuals to *decide* to get a flu vaccine. Across specifications students in the incentive group are about 15–20 percentage points more likely than those in the control to open, read, and indicate that they *do* plan to get a vaccine. This estimate reflects an increase of more than 50 percent, relative to the mean rate for the control group. We also note that intentions are lower on campuses with out-of-pocket costs, consistent with a price effect, and that the impact of the incentive is also smaller when the out-of-pocket cost of the vaccine is greater, as shown by the negative and significant interaction term. In contrast, those in the peer endorsement group and coughing group are less likely than control to reach the stage of opening, reading, and deciding to get the vaccine.[22,23]

The final outcome of interest is whether subjects obtain the flu vaccine. We present estimates of treatment effects on the unconditional probability students get the flu vaccine in the second panel of Table 5 (columns (5)–(8)). The main finding is that the incentive treatment causes a substantial increase (11 percentage points) in the likelihood of vaccination relative to the control group. Other things equal, students in the incentive treatment group are *twice* as likely as those in the control group to be vaccinated. The other two treatments, on the other hand, have no overall impact on flu vaccine take-up. Notably, these null effects are precisely estimated: 95 percent confidence intervals suggest treatment effects no larger than a 1–2 percentage point increase in vaccination. Such an effect is smaller than one fifth the size of the incentive treatment effect.

On some campuses, students getting a flu vaccine at the health center were required to pay an out-of-pocket cost of 20 dollars, in cash or check, at the time of vaccination. This out-of-pocket cost effectively reduced the size of the incentive, to a modest financial reward of $10. Our results in column (7) suggest that even this modest incentive meaningfully improved

[22] The coefficients on the peer treatment are around 2 percentage points, and may fail to be statistically significant due to lack of power. However, the estimated peer effects on vaccination, in columns (5)–(8), are quite small in magnitude, and estimated with sufficient precision to reject meaningfully large, or policy-relevant, impacts.

[23] While most students in the salience treatment group do not open the link to hear the sound of coughing, a natural question regards the subsequent behavior of those who do. Of those who open the email and click on the audio link, 32 percent report positive intentions, compared to 29 percent of those who open the email but do not open the link. Interestingly, though, the rate of vaccination among those who both open and click on the link is lower (11 percent) than among those who open but do not listen (12 percent). We strongly caution against interpreting these statistics as effects of the coughing clip, given the potential for positive self-selection (of those with high initial interest in vaccination) into clicking on the audio link.

Table 5
Effects of treatments on plans to get a vaccine and vaccine take-up.

	(1) Intended to get vaccine	(2) Intended to get vaccine	(3) Intended to get vaccine	(4) Intended to get vaccine	(5) Got vaccine	(6) Got vaccine	(7) Got vaccine	(8) Got vaccine
Incentive	0.147***	0.146***	0.164***	0.163***	0.107***	0.107***	0.124***	0.125***
	(0.013)	(0.013)	(0.015)	(0.016)	(0.009)	(0.009)	(0.011)	(0.011)
Peer	−0.020	−0.022*	−0.020	−0.018	−0.005	−0.006	−0.005	−0.007
	(0.013)	(0.013)	(0.013)	(0.013)	(0.009)	(0.009)	(0.009)	(0.010)
× Peer is close friend			−0.093	−0.090			−0.050	−0.053
			(0.088)	(0.088)			(0.063)	(0.063)
Coughing	−0.044***	−0.046***	−0.045***	−0.036***	0.001	0.000	0.001	−0.000
	(0.013)	(0.013)	(0.013)	(0.013)	(0.009)	(0.009)	(0.009)	(0.009)
Female			0.035***	0.036***			0.014**	0.017**
			(0.010)	(0.010)			(0.007)	(0.007)
International student			0.000	–			−0.012	–
			(0.020)				(0.015)	
Freshman			0.058***	0.054***			0.033***	0.031***
			(0.012)	(0.013)			(0.009)	(0.009)
Junior			0.010	0.008			0.014	0.012
			(0.013)	(0.013)			(0.009)	(0.010)
Senior			0.031**	0.031**			0.020**	0.020**
			(0.012)	(0.013)			(0.009)	(0.009)
Is friends with any sender			0.016	0.015			0.002	0.004
			(0.013)	(0.013)			(0.009)	(0.009)
Has out-of-pocket cost			−0.141***	−0.091***			−0.068***	−0.026**
			(0.017)	(0.017)			(0.012)	(0.012)
Out-of-pocket × incentive			−0.046**	−0.041*			−0.046***	−0.051***
			(0.021)	(0.022)			(0.015)	(0.016)
Zip code income (10,000s)				−0.002				0.002*
				(0.001)				(0.001)
Constant	0.248***	0.326***	0.274***	0.289***	0.087***	0.125***	0.097***	0.083***
	(0.009)	(0.014)	(0.017)	(0.021)	(0.006)	(0.010)	(0.012)	(0.015)
Campus dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	9358	9358	9358	8665	9358	9358	9358	8665
R-squared	0.028	0.044	0.049	0.047	0.022	0.030	0.033	0.032

Standard errors in parentheses; columns (4) and (8) include the subjects for whom we have zip code and therefore exclude international students; median income for each student's zip code is obtained from the 2010 Census. Outcome in columns (1)–(4) is an indicator for having clicked 'Yes,' indicating intentions to get the vaccine. Outcome in columns (5)–(8) is indicator for having been vaccinated.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table 6
Effects of treatments on self-reported vaccination, location, and timing.

	(1) Got shot on campus (October–November)	(2) Got shot anywhere (October–November)	(3) Got shot on campus	(4) Got shot anywhere	(5) Plans to get shot next year	(6) Got shot anywhere, no shot last year	(7) Got shot anywhere, got shot last year
Incentive	0.174***	0.088***	0.226***	0.104***	0.062*	0.186***	0.004
	(0.023)	(0.029)	(0.026)	(0.033)	(0.032)	(0.040)	(0.042)
Peer	0.014	0.030	0.032	0.028	0.043	0.070*	−0.016
	(0.023)	(0.029)	(0.026)	(0.032)	(0.032)	(0.039)	(0.042)
Coughing	−0.003	−0.005	0.019	0.007	0.052	0.054	−0.052
	(0.023)	(0.029)	(0.026)	(0.033)	(0.032)	(0.040)	(0.042)
Constant	0.130***	0.259***	0.189***	0.497***	0.618***	0.242***	0.782***
	(0.021)	(0.026)	(0.023)	(0.029)	(0.029)	(0.036)	(0.037)
Campus FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1877	1877	1877	1877	1877	998	879
R-squared	0.050	0.014	0.062	0.019	0.020	0.040	0.018

Standard errors in parentheses. The survey was conducted online among students at all six campuses. All of the colleges had appointments and walk-in hours for flu vaccines. Two of the colleges also held dedicated flu vaccine clinics on specific days during the study period.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

flu vaccination rates. All else equal, a net incentive equal to $10 raised flu vaccine rates by 7.8 percentage points, compared to an increase of 12.4 percentage points caused by the larger payment of $30.[24]

Finally, while we have focused on results using the pooled sample across campuses, it is noteworthy that similar qualitative treatment effects are observed at each campus individually. In particular, the incentive treatment roughly doubles take-up on every campus, whereas the peer and coughing treatments have little impact on take-up.

3.4. Robustness check: vaccine location and timing

The results so far indicate that the incentive treatment raised vaccine take-up at campus health centers by about 11 percentage points during the study period of October and November, more than doubling the observed rate of vaccination relative to control. However, recall that our measure of flu vaccination only reflects vaccines that were obtained at the campus health center; we do not observe when a student gets a flu vaccine off campus (e.g., in a pharmacy at a store like CVS or Target, or from a private doctor on a visit home). This raises a question about the estimated incentive treatment effect because students in this group were only eligible to receive the $30 payment if they received their vaccines at the campus health center. In the most extreme case, the incentive may only have affected the behavior of students who would have otherwise gotten vaccinated at an off-campus location, causing them instead to get their vaccines on campus. While this would appear as a positive treatment effect in the data, it would reflect only substitution from off-campus to on-campus vaccination, and *no* increase in overall vaccination. Notably, this is a type of challenge that is often faced in field experiments that implement incentives for an activity.

To address the substitution issue we built into the study a third phase of data collection: a large-scale survey ($N=1877$) during the summer of 2013, designed to elicit information about students' flu vaccination decisions both off and on campus, and during all times of the calendar year, not just during our experiment. We delivered the survey electronically to students at all six colleges in the study. The survey collected student names, allowing us to identify to which treatment group a respondent was assigned during our field experiment. Students who participated in the survey were entered in a lottery with a 1 in 10 chance of winning a prize of at least $50. Students received one e-mail inviting them to participate, and the survey was available for only 1 week (due to budget considerations). Nevertheless the survey responses amount to about 20 percent of the entire population. Compared to the main experimental sample, survey respondents were more likely to be female, first-year students, have a friend who is a peer endorser, and live on a campus without an out-of-pocket cost for vaccination, all of which are positively correlated with flu vaccination in Table 5. However, we find no evidence of differential response rates across treatments, which is promising for relative treatment differences in the survey matching those observed in the experimental data; response rates are very similar, and differences are not statistically significant.[25]

Table 6 displays the effects of the treatments on various self-reported flu vaccine outcomes. In column (1) we estimate effects of the interventions on the likelihood a student reports getting vaccinated at the campus health center during October or November (the study period). While the control group vaccination rate (13 percent) is somewhat higher than what we observed in the health center data (9 percent), the *relative* size of the incentive effect is similar to what we find in the experimental data; the incentive more than doubles the rate of take-up relative to control.

In column (2) we show the impact of the incentive on vaccination anywhere during the study period, including off-campus, so that we have the impact on total take-up of vaccine during October/November. If all of the incentive effect at campus health centers reflected substitution, we would expect to see no impact of the incentive treatment when considering all flu vaccinations. Instead, the incentive raised the overall vaccination rate during October and November by 8.8 percentage points (a 34 percent increase relative to control). While the incentive caused a substantial increase in overall take-up, this estimate is appreciably smaller in magnitude than what we obtain by looking only at the health center data, consistent with substitution from off-campus to on-campus vaccination.

Another possibility is that the incentive treatment effect could reflect substitution over time. The fact that the incentive was available only until December 3rd might have caused some students to get the vaccine during those months, instead of at a later date. In the most extreme case, all of the observed impact of the incentive on flu vaccination on campus during October and November could be a shift in the timing of vaccination rather than an increase in total take-up. In one sense this would still be a useful outcome, in that the CDC recommends getting the vaccine earlier than December or January. Nevertheless, we use the data on vaccination throughout the year to address this issue.

Column (3) of Table 6 shows that the incentive had a large and significant impact on the rate of vaccinations on campus, considering the entire academic year from September 2012 to May 2013. If all of the incentive effect were due to

[24] In results not shown here, we also tested whether the presence of an out-of-pocket cost altered the effects of the other two treatments. However, the coefficients on the relevant interactions were generally small in magnitude and were never jointly significant.

[25] The rates were 19.8 percent for control, 20.2 percent for incentive, 21.0 percent for peer, 19.7 percent for coughing.

substitution over time, by contrast, we would expect to see no impact on total vaccination, but rather an impact only during October and November. We also find that take-up during the time period after November is similar in the incentive group to the control group, indicating that little substitution took place; substitution would imply a lower rate of take-up for the incentive group in months after our experiment. Column (4) yields a similar conclusion for vaccinations at all locations at all times: total take-up of vaccine during the year was significantly higher for the incentive group than the control. Nonetheless, compared to the doubling of vaccine take-up that we observe in the health center vaccination data, the magnitude of the incentive effect is much smaller, reflecting a 21 percent increase relative to control group take-up. In short, while the ex-post survey data do not allow us to perfectly account for potential substitution effects, taken together our evidence strongly suggests a positive effect of the incentive treatment on overall vaccination rates.

The survey also allows clarifying a final issue, which is how rates of follow-through might change with broader definitions of follow-through. As mentioned earlier, among people stating that they did not plan to get the vaccine at the health center, we found that essentially zero did so during our study period, using objective vaccine outcomes from the health center. Using our survey data, we can check whether this is also true for months after the study period is over. We find that take-up is also close to zero in those later months, so that overall negative intentions were fully implemented. The survey also shows that some of these individuals did get vaccinated off campus, but this is fully consistent with stated intentions, since we asked about plans to get vaccinated on campus. Among those stating an intention to come in for the vaccine, by contrast, we observed just 27 percent following through during our study using the health center data (the fraction is 29 percent among control group respondents to our follow-up survey).

A broader definition of follow-through might include those getting the vaccine off campus during the study period; these individuals did get the vaccine in a timely manner, albeit not at the health center as planned. Using the survey data and the definition of follow-through including both on- and off-campus vaccination, we find that the rate of follow-through during the recommended window (corresponding to the months of our study period) rises to 45 percent. Broadening the definition still further, we could include those who do eventually get the vaccine, but rather late, in the months following our study. Including such cases adds another 21 percentage points to the follow-through rate. Nonetheless, even with this rather generous definition of follow-through, more than 30 percent of those who reported intentions to get the flu vaccine did not ever follow through on those plans.

3.5. Testing for longer-term impacts and heterogeneous treatment effects

The survey also asked about plans to get the vaccine next year and vaccination decisions in the prior year, allowing us to address questions about the persistence of treatment effects, and effects on those who do not traditionally get the vaccine.

Column (5) of Table 6 shows the impact of the different interventions on intentions to get the vaccine next year. Interestingly, individuals in the incentive condition have a significantly higher rate of planning to get the vaccine in the future ($p = 0.054$), than those in control. This is suggestive that the incentive induced individuals who were not in the habit of being vaccinated to try the vaccine, thereby establishing a new pattern of behavior. As opposed to traditional models of habit formation, an alternative mechanism is that the incentive could have generated persistent effects is by signaling the social value of the flu vaccine, thereby changing preferences (e.g., as in models of seed money donations in charitable giving (Vesterlund, 2003)). Other interventions did not significantly impact intentions to get vaccinated in the future, although statistical power is reduced by the smaller sample size here. One caveat is that students who experienced the incentive might have wrongly believed that the incentive would be present again next year. The survey did not say that incentives would be available, but it also did not explicitly rule this out. Mitigating this belief is the fact that the incentive intervention e-mails emphasized that the individual had been randomly selected, which means that getting the incentive next year would not be certain, even if the program would again be in place. Nevertheless, we cannot rule out that some of the effect may reflect such beliefs.

If the incentive did generate new habits, this requires that it caused individuals who did not get the vaccine in the past to start getting the vaccine. The effect of the incentive is especially large among respondents who did not get the flu shot in the previous year (column (6)), with take-up being 75 percent higher than control after taking into account substitution from off campus and over time. In contrast, there is no statistically significant effect among those who were vaccinated in the prior year (column (7)). The fact that the incentive was effective among individuals who did not get the vaccine previously is encouraging for the potential cost-effectiveness of monetary incentive approaches, which would ideally be targeted toward populations where vaccination rates are traditionally low, to minimize payments to those who would have gotten vaccinated anyway.[26]

[26] We caution that a policy providing incentives only to those who did not get vaccinated in the prior year might also have perverse effects, causing some subjects to avoid vaccination in hopes of being eligible for a financial reward in the following year.

The results in column (6) also reveal a marginally significant, positive impact of the peer endorsement treatment on vaccinations among those who did not get the vaccine last year ($p = 0.08$). We tested whether the peer messages similarly increased vaccination on campus and/or during the study period for this group, but found they did not. Perhaps peer endorsements have a delayed impact on students' vaccine decisions; while we find no effect of these messages on *intentions* to get vaccinated or vaccinations during the study period, messages may be memorable, and affect student decisions later.

This finding raises the question of whether peer endorsements, or the other treatments, work differently for some groups of students than for others. For example, Card and Giuliano (2013) find that peer effects on decisions to engage in risky behavior are larger for females than males.[27] We use the information we have on subjects' traits (gender, class year, and income) to look for evidence of heterogeneity in treatment effects. We report these results in Table A2. Generally speaking, we do not find substantial differences in vaccination decisions across the sub-samples we consider. Two exceptions have to do with effects of treatments on attention to information: the incentive impacts reading of emails more for males than females, and the coughing intervention impacts email opening for males but not females, and for freshmen and sophomores but not juniors and seniors. Note that we find no differences in the effects of peer endorsements by subject traits. Our information on subject characteristics is limited, however, and we believe future work should explore the question of which populations are most responsive to the types of nudges we have tested here.

4. Conclusions

We study flu vaccination decisions in response to an online messaging campaign on six college campuses, and provide some of the first evidence on different components of the problem of low immunization rates. While information exposure is not perfect, observed e-mail opening and reading rates are perhaps surprisingly high. On the other hand, we find low rates of intentions to get a flu vaccine among those who are observed to be informed, and even among those who state an intention to be vaccinated, less than half follow through. Interestingly, in exploring heterogeneity in follow-through, we find that lack of follow-through is associated with lower college GPA, even controlling for measures of cognitive ability like SAT and ACT scores.[28] That is, those who fail to follow-through on stated intentions to get vaccinated are also those who struggle more generally to implement beneficial long-term goals, i.e., academic success. We suggest that this relationship warrants further work. While the relationship we observe cannot be determined to be causal, future research might explore the roles of cognitive ability and non-cognitive skills in determining outcomes like attention to information and follow-through on one's own intentions.

In a randomized field experiment, we test the effectiveness of three interventions – a financial incentive, a peer endorsement, and a salience treatment – to address these challenges. We find that the financial incentive significantly raises the overall vaccination rate, relative to the control. This impact appears to work through attention to information, intentions, and follow-through. The positive effects of the incentive are upheld even when the net value of the reward is reduced to just 10 dollars. The lower-cost nudges do not affect overall vaccine take-up, although receiving a message from a peer significantly increases opening of e-mails containing health information.

A natural question is whether our $30 incentive outperforms other, lower-cost interventions which have been shown to increase flu vaccines. For example, Milkman et al. (2011) find that implementation prompts raise flu vaccination among employees at a large firm by 4.2 percentage points, relative to a baseline rate of 33.1 percent (i.e., a 13 percent increase). Chapman et al. (2010) analyze the effect of setting default appointments for flu vaccination on vaccine decisions among university employees, and find a 12-percentage point increase, on a baseline of 33 percent (i.e., a 36 percent increase in vaccination). While these samples of adult employees are different from our sample of college students, which had a much lower baseline rate of vaccination during the study period, our results suggest the incentive raised vaccination by about as much (34 percent) as default appointments in the Chapman et al. study, and by more than implementation prompts in the Milkman et al. (2011) experiment, after accounting for substitution from off-campus to on-campus vaccination. The relatively high financial cost of incentives may make this intervention more appropriate for populations where initial take-up is low, and where there are logistical or other challenges to implementing a default of vaccination.

While we provide evidence that one type of automated, impersonal peer endorsement does not positively impact intentions to get vaccinated or final vaccination decisions, we caution against concluding that peer endorsements, in general, could not be effective in this context. In particular, personal messages of peer endorsement that are generated and distributed endogenously through existing social networks, while difficult to test in a randomized field experiment, might have more substantial impacts on pro-social behavior like flu vaccination. Similarly, given that the intervention including an audio link of a sick person coughing appeared to discourage thorough reading

[27] Others have also documented gender differences in peer effects, including Stinebrickner and Stinebrickner (2006), Argys and Rees (2008), and Kling et al. (2007).

[28] Results available upon request.

of the emails (as measured by clicking on the Yes/No hyperlinks), future work might consider alternate salience treatments that signal the costs of getting the flu (i.e., the benefits of vaccination) without relying on hyperlinks.

Our results call for further research on the nature of decision making underlying preventive health behaviors more broadly. One example is poor performance in taking medication at the prescribed frequency and quantity. Individuals are provided with information about the benefits of such medication, and yet a strikingly large fraction stop taking medication too soon, significantly raising the risk of future health problems, harming themselves and also increasing health care costs (Volpp et al., 2008b). This behavior remains a puzzle. Measuring information exposure, intentions, and behavior of such individuals, could shed light on the nature of the problem, and provide a better evidence base for choosing interventions. If follow-through is the main problem, then interventions focused on reminders, commitment devices, and incentives, could be most helpful. But if intentions are opposed to medication adherence, other types of interventions are needed, e.g., ones focused on dispelling false beliefs about side effects.

Appendix A.

From : healthcenter@swarthmore.edu
Subject : Flu vaccine
To : ebronch1@swarthmore.edu
Reply To : healthcenter@swarthmore.edu

Dear Erin :

The American College Health Association recommends that college students get the flu vaccine every year.

By getting vaccinated you will protect yourself, and fellow students, from getting sick. No one wants to get sick, especially at crucial times like midterms or finals.

It is easy to get a flu vaccine on campus. It is safe, and now is the right time!

You can get the vaccine at Worth Health Center beginning Wednesday, October 3rd (tomorrow!), at any time. You do not need an appointment! The cost is $20 and can be billed to your student account or paid by cash/check.

To help the student health center's planning, please let us know: Do you plan to come in for a flu vaccine?

By responding below (clicking EITHER yes or no), you can enter to win one of five $100 prizes. Prizes will be distributed to winners' student mailboxes at the beginning of finals week.

YES, I think I will probably get a flu vaccine: Click here.

NO, I do not think I will get a flu vaccine: Click here.

It is also possible to make a flu vaccine appointment if you want to, on T/W/Th between 1:30 and 4:00 p.m. This helps minimize wait times. If you have any questions about flu vaccination, please contact the Health Center at 610-328-8058.

Stay Healthy!

From Health Center

From : healthcenter@swarthmore.edu
Subject : Flu vaccine (get $30!)
To : ebronch1@swarthmore.edu
Reply To : healthcenter@swarthmore.edu

Dear Erin:

You have been randomly selected to get $30 in cash, if you get a flu vaccine at the campus Health Center by December 3rd. Get a flu vaccine, and the Health Center will put a sealed envelope with $30 in your student mailbox. You will get the money within 1 to 2 weeks after you get vaccinated. (This reward is only available to students in this randomly selected group.)

The American College Health Association recommends that college students get the flu vaccine every year.

By getting vaccinated you will protect yourself, and fellow students, from getting sick. No one wants to get sick, especially at crucial times like midterms or finals.

It is easy to get a flu vaccine on campus. It is safe, and now is the right time!

You can get the vaccine at Worth Health Center beginning Wednesday, October 3rd (tomorrow!), at any time. You do not need an appointment! The cost is $20 and can be billed to your student account or paid by cash/check.

To help the student health center's planning, please let us know: Do you plan to come in for a flu vaccine?

By responding below (clicking EITHER yes or no), you can enter to win one of five $100 prizes. Prizes will be distributed to winners' student mailboxes at the beginning of finals week.

YES, I think I will probably get a flu vaccine: Click here.

NO, I do not think I will get a flu vaccine: Click here.

It is also possible to make a flu vaccine appointment if you want to, on T/W/Th between 1:30 and 4:00 p.m. This helps minimize wait times. If you have any questions about flu vaccination, please contact the Health Center at 610-328-8058.

Stay Healthy!

From Health Center

From : jdoe1@swarthmore.edu
Subject : Flu vaccine
To : ebronch1@swarthmore.edu
Reply To : jdoe1@swarthmore.edu

Dear Erin:

As a fellow Swarthmore student, I want you to know that I think it's a good idea to get a flu vaccine.

By getting vaccinated you will protect yourself, and fellow students, from getting sick. No one wants to get sick, especially at crucial times like midterms or finals.

It is easy to get a flu vaccine on campus. It is safe, and now is the right time!

You can get the vaccine at Worth Health Center beginning Wednesday, October 3rd. You do not need an appointment! The cost is $20 and can be billed to your student account or paid by cash/check.

To help the student health center's planning, please let them know: Do you plan to come in for a flu vaccine?

By responding below (clicking EITHER yes or no), you can enter to win one of five $100 prizes. Prizes will be distributed to winners' student mailboxes at the beginning of finals week.

YES, I think I will probably get a flu vaccine: Click here.

NO, I do not think I will get a flu vaccine: Click here.

It is also possible to make a flu vaccine appointment if you want to, on T/W/Th between 1:30 and 4:00 p.m. This helps minimize wait times. If you have any questions about flu vaccination, please contact the Health Center at 610-328-8058.

Stay Healthy!

From John Doe

From : healthcenter@swarthmore.edu
Subject : Flu vaccine
To : ebronch1@swarthmore.edu
Reply To : healthcenter@swarthmore.edu

Dear Erin:

Listen to the following 3 second clip, and imagine feeling like this during finals week!

Click here.

The American College Health Association recommends that college students get the flu vaccine every year.

By getting vaccinated you will protect yourself, and fellow students, from getting sick. No one wants to get sick, especially at crucial times like midterms or finals.

It is easy to get a flu vaccine on campus. It is safe, and now is the right time!

You can get the vaccine at Worth Health Center beginning Wednesday, October 3rd (tomorrow!), at any time. You do not need an appointment! The cost is $20 and can be billed to your student account or paid by cash/check.

To help the student health center's planning, please let us know: Do you plan to come in for a flu vaccine?

By responding below (clicking EITHER yes or no), you can enter to win one of five $100 prizes. Prizes will be distributed to winners' student mailboxes at the beginning of finals week.

YES, I think I will probably get a flu vaccine: Click here.

NO, I do not think I will get a flu vaccine: Click here.

It is also possible to make a flu vaccine appointment if you want to, on T/W/Th between 1:30 and 4:00 p.m. This helps minimize wait times. If you have any questions about flu vaccination, please contact the Health Center at 610-328-8058.

Stay Healthy!

From Health Center

Table A1
Heterogeneity in effects of peer endorsement, by sender traits.

	(1) Opened email	(2) Opened email	(3) Read email	(4) Read email	(5) Clicked yes	(6) Clicked yes	(7) Got vaccine	(8) Got vaccine
Incentive	0.042**	0.043**	0.052***	0.052***	0.163***	0.163***	0.125***	0.124***
	(0.017)	(0.017)	(0.018)	(0.018)	(0.016)	(0.016)	(0.011)	(0.011)
Peer	0.063**	0.144***	−0.008	0.053	−0.028	−0.028	−0.007	0.008
	(0.030)	(0.056)	(0.031)	(0.059)	(0.028)	(0.053)	(0.020)	(0.038)
×Peer is close friend	0.180*	0.186**	−0.107	−0.100	−0.090	−0.091	−0.053	−0.052
	(0.093)	(0.093)	(0.098)	(0.098)	(0.088)	(0.088)	(0.063)	(0.063)
×Sender is same gender	−0.021	−0.022	0.016	0.018	0.011	0.009	0.001	−0.000
	(0.030)	(0.030)	(0.032)	(0.032)	(0.028)	(0.028)	(0.020)	(0.020)
×Peer is NCAA athlete		−0.045		−0.032		0.016		0.019
		(0.030)		(0.032)		(0.029)		(0.021)
×Peer is student advisor		−0.029		0.002		−0.018		−0.008
		(0.023)		(0.024)		(0.022)		(0.016)
×Peer is junior or senior		−0.026		−0.031		−0.004		−0.032
		(0.039)		(0.041)		(0.037)		(0.027)
×Peer is leader on campus		0.018		0.002		0.006		0.019
		(0.021)		(0.023)		(0.020)		(0.015)
×Others listen to peer		−0.011		−0.022		0.001		−0.010
		(0.021)		(0.023)		(0.020)		(0.015)
Coughing	0.008	0.008	−0.087***	−0.087***	−0.036***	−0.036***	−0.000	−0.000
	(0.014)	(0.014)	(0.015)	(0.015)	(0.013)	(0.013)	(0.009)	(0.009)
Female	0.073***	0.073***	0.098***	0.098***	0.036***	0.036***	0.017**	0.017**
	(0.010)	(0.010)	(0.011)	(0.011)	(0.010)	(0.010)	(0.007)	(0.007)
Freshman	0.048***	0.048***	0.037***	0.038***	0.054***	0.054***	0.031***	0.031***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)	(0.009)	(0.009)
Junior	0.011	0.011	−0.004	−0.004	0.008	0.008	0.012	0.012
	(0.014)	(0.014)	(0.015)	(0.015)	(0.013)	(0.013)	(0.010)	(0.010)
Senior	0.007	0.007	0.015	0.015	0.031**	0.031**	0.020**	0.020**
	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)	(0.009)	(0.009)
Is friends with any sender	0.004	0.005	0.001	0.001	0.015	0.015	0.004	0.004
	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)	(0.009)	(0.009)
Out-of-pocket cost, Y/N?	−0.167***	−0.167***	−0.146***	−0.146***	−0.091***	−0.093***	−0.026**	−0.028**
	(0.018)	(0.018)	(0.019)	(0.019)	(0.017)	(0.017)	(0.012)	(0.012)
Out-of-pocket × incentive	−0.026	−0.027	−0.031	−0.031	−0.041*	−0.040*	−0.051***	−0.049***
	(0.023)	(0.023)	(0.025)	(0.025)	(0.022)	(0.022)	(0.016)	(0.016)
Zip code median income (10,000s)	0.004***	0.004***	−0.006***	−0.006***	−0.002	−0.002	0.002*	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.664***	0.665***	0.513***	0.513***	0.289***	0.290***	0.083***	0.085***
	(0.022)	(0.022)	(0.024)	(0.024)	(0.021)	(0.021)	(0.015)	(0.015)
Observations	8665	8665	8665	8665	8665	8665	8665	8665
R-squared	0.025	0.025	0.043	0.043	0.047	0.047	0.032	0.032

Standard errors in parentheses; all models include same controls as in Columns (4) and (8) in Tables 4 and 5. Sample includes subjects for whom we have zip code data and therefore excludes international students; median income for each student's zip code is obtained from the 2010 Census. Two campuses have an out-of-pocket cost of $20, which students must pay by cash or check at the time of the vaccine. Outcome variable in columns (1)–(4) is indicator for opening an email; outcome in columns (5)–(8) is indicator for reading email thoroughly, as indicated by clicking link at bottom of email to be eligible to win a financial prize.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table A2
Heterogeneity in response to treatments, by subject characteristics (coefficients that are statistically different at $p < 0.10$ are highlighted in bold).

	Female students				Male students			
	(1) Opened email	(2) Read email	(3) Clicked yes	(4) Got vaccine	(5) Opened email	(6) Read email	(7) Clicked yes	(8) Got vaccine
Incentive	0.021	**0.025**	0.158***	0.144***	0.071***	**0.088*****	0.170***	0.099***
	(0.022)	(0.024)	(0.021)	(0.015)	(0.026)	(0.027)	(0.024)	(0.017)
Peer	0.046**	0.012	−0.009	−0.010	0.043**	0.001	−0.030	−0.003
	(0.018)	(0.020)	(0.018)	(0.013)	(0.022)	(0.022)	(0.020)	(0.014)
×Peer is close friend	0.166	−0.114	−0.109	−0.102	0.196	−0.098	−0.061	0.070
	(0.108)	(0.118)	(0.106)	(0.076)	(0.179)	(0.181)	(0.161)	(0.117)
Coughing	−**0.014**	−0.081***	−0.028	−0.010	**0.038***	−0.095***	−0.045**	0.014
	(0.018)	(0.020)	(0.018)	(0.013)	(0.022)	(0.022)	(0.020)	(0.014)
Constant	0.705***	0.581***	0.290***	0.111***	0.704***	0.549***	0.332***	0.076***
	(0.029)	(0.031)	(0.028)	(0.020)	(0.033)	(0.034)	(0.030)	(0.022)
Observations	4919	4919	4919	4919	3746	3746	3746	3746

Table A2
(*Continued*)

	Freshmen &Sophomores				Juniors &seniors			
	(1) Opened email	(2) Read email	(3) Clicked yes	(4) Got vaccine	(5) Opened email	(6) Read email	(7) Clicked yes	(8) Got vaccine
Incentive	0.056**	0.054**	0.172***	0.116***	0.029	0.051**	0.155***	0.138***
	(0.023)	(0.025)	(0.022)	(0.016)	(0.024)	(0.025)	(0.023)	(0.017)
Peer	0.049**	0.010	−0.010	−0.018	0.041**	0.003	−0.028	0.007
	(0.019)	(0.020)	(0.018)	(0.013)	(0.021)	(0.022)	(0.019)	(0.014)
×Peer is close friend	0.271*	−0.102	−0.159	−0.081	0.120	−0.103	−0.039	−0.030
	(0.146)	(0.155)	(0.138)	(0.099)	(0.121)	(0.127)	(0.113)	(0.082)
Coughing	**0.032***	−0.086***	−0.035*	−0.003	**−0.017**	−0.088***	−0.036*	0.003
	(0.019)	(0.020)	(0.018)	(0.013)	(0.021)	(0.022)	(0.019)	(0.014)
Constant	0.685***	0.537***	0.314***	0.096***	0.654***	0.505***	0.294***	0.087***
	(0.029)	(0.031)	(0.028)	(0.020)	(0.031)	(0.033)	(0.029)	(0.021)
Observations	4576	4576	4576	4576	4089	4089	4089	4089

	Lower than median income				Higher than median income			
	(1) Opened email	(2) Read email	(3) Clicked yes	(4) Got vaccine	(5) Opened email	(6) Read email	(7) Clicked yes	(8) Got vaccine
Incentive	0.025	0.044*	0.182***	0.133***	0.061***	0.062**	0.146***	0.117***
	(0.024)	(0.025)	(0.022)	(0.016)	(0.023)	(0.025)	(0.022)	(0.016)
Peer	0.025	0.018	−0.022	−0.009	0.065***	−0.005	−0.016	−0.004
	(0.020)	(0.021)	(0.019)	(0.013)	(0.020)	(0.021)	(0.019)	(0.014)
×Peer is close friend	0.312**	−0.030	−0.125	0.026	0.095	−0.149	−0.071	−0.096
	(0.156)	(0.164)	(0.146)	(0.104)	(0.115)	(0.122)	(0.110)	(0.080)
Coughing	−0.011	−0.105***	−0.052***	0.003	0.028	−0.070***	−0.018	−0.004
	(0.020)	(0.021)	(0.019)	(0.013)	(0.019)	(0.021)	(0.019)	(0.014)
Constant	0.641***	0.480***	0.297***	0.054**	0.685***	0.500***	0.284***	0.079***
	(0.038)	(0.040)	(0.035)	(0.025)	(0.039)	(0.042)	(0.038)	(0.028)
Observations	4332	4332	4332	4332	4332	4332	4332	4332

Standard errors in parentheses, models contain same controls as in columns (4) and (8) of Tables 4 and 5. Results in bold are statistically different across compared samples. Panel 3 compares sample of students with zip code incomes lower than the median for their campuses to students with zip code incomes higher than the median for their campuses.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

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