Does promoting one behavior distract from others? Evidence from a field experiment *

Hannah (Trachtman) Luk-Zilberman †

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Abstract

The potential welfare benefits of motivating people to vaccinate their children, consume healthy foods, or use clean cookstoves are enormous. Recent research has uncovered many interventions that cost-effectively improve such behaviors, but most research evaluates one intervention in isolation on target outcomes. As such, despite evidence that effort and attention can be costly, there is little research on whether generic interventions can have negative spillovers on other behaviors. I propose a simple framework that organizes evidence on costly effort and attention into three mechanisms with distinct policy implications. I test the model's predictions using an online experiment in which individuals receive combinations of messages and incentives for two healthy behaviors, meditation and meal tracking, which are measured daily via phone applications. I find that all three of the interventions in the experiment generate negative spillovers on the opposite behavior, reducing daily completion rates by between 19% and 29%. These effects imply that a policymaker who wants to raise both behaviors by 1 standard deviation faces costs that are 28% higher than expected once spillovers are taken into account. Estimating the parameters of the model reveals that spillovers act as a fixed cost, and do not necessarily rise with the strength of the intervention. This suggests that policymakers can potentially mitigate negative spillovers by tending toward strong interventions, all else being equal.

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†Department of Economics, The Hebrew University of Jerusalem, Mt. Scopus, Jerusalem, 9190501, Israel. hannah.lukzilberman@mail.huji.ac.il

1 Introduction

Seemingly small changes in behaviors like routine childhood immunization, healthy eating, and the use of clean cookstoves—among others—can lead to big economic benefits.[1] Given their large returns, low take-up of such behaviors is often seen as a puzzle that merits intervention. In recent years, both traditional interventions like "sin taxes" (O'Donoghue and Rabin, 2006), as well as non-traditional instruments like "nudges" (Thaler and Sunstein, 2009), have been widely used. Many have been shown to be effective and cost-effective with respect to their target outcomes, and the possibility of shifting behaviors with such high stakes has inspired an outpouring of interventions and evaluations.[2] In particular, the use of nudges—which are often cheap, unimposing, and relatively easy to implement—has grown dramatically, in developed and developing countries, across many domains, and implemented by public and private actors alike.

Most evaluations measure the behavior targeted by the intervention and little else, unless there are specific reasons to expect spillovers.[3] But there may be general reasons to worry about spillovers. In particular, attention and effort—even cognitive effort—may be costly or limited in ways that interact with policy design. Do interventions encouraging one behavior divert attention away from other behaviors? Might interventions promoting difficult behaviors reduce effort spent on other behaviors? Can interventions that rely on education, information, or prompts overload beneficiaries? In fact, evidence suggests that all of these things are possible. Yet there is little research on whether or not they should be real policy concerns.

This paper aims to answer two questions. First, can generic behavior change interventions impose negative spillovers on other behaviors or interventions? Second, what is the mechanism, and what does it imply for policy?

I begin with a simple framework to clarify the potential drivers of spillovers, and to generate

[1]For example, see Afshin et al. (2019), Institute for Health Metrics and Evaluation (2018), and Zhou et al. (2014).

[2]For example, soda taxes have shown to reduce sugar consumption due to soda by 18% on average, and by 40% among young people age 13-21 (Dubois et al., 2019). Prompting people to write down a plan for getting the flu vaccine raised immunization rates by 13% (Milkman et al., 2011). Other interventions have proven less effective: encouraging the use of clean cookstoves has proved to be much more difficult than expected (Hanna et al., 2016).

[3]See for example the literature on whether retirement savings policies crowd out other types of savings, e.g. Chetty et al. (2014). Another example is the literature on the general equilibrium effects of cash transfers and other anti-poverty programs, e.g. Egger et al. (2019); Muralidharan et al. (2017)

predictions that will motivate an experiment. A decision-maker (DM) has two behaviors available to her, x and y. Doing each behavior generates a return, but requires mental effort, which is costly. An outside actor can subsidize effort to one or both behaviors with incentives or SMS messages.

The model formalizes three mechanisms that could potentially cause these interventions to generate spillovers: "overload," "diversion," and "depletion." Overload captures limits to information processing, which might result in interventions interfering negatively with one another, by overwhelming the individual with stimuli. Diversion captures limits to executive function, short-term, or working memory that prevent people from keeping more than one behavior at the top of mind. Depletion captures costly time as well as costly mental effort; the idea that engaging in certain behaviors depletes cognitive resources. Importantly, in my model it is also inseparable from (positive or negative) complementarity in the utility function.

The ideas and phenomena that overload, diversion, and depletion encompass are not new: they are supported by an abundance of evidence in psychology and neuroscience. But it turns out that distinguishing between these three mechanisms, the way I define them in this paper, is key for understanding the policy implications of spillovers. Specifically, in the case of depletion, an intervention's target effects are responsible for its spillovers, since it is the act of doing the target behavior that requires costly effort. On the other hand, in the case of diversion, an intervention's spillovers can be unrelated to its target effects. Interventions may divert attention away from other behaviors regardless of the extent to which they improve the target behavior. Thus, spillovers driven by diversion act as "fixed costs" of the intervention, which means that we can potentially raise welfare by shifting toward stronger interventions that spread the fixed spillover over a larger target effect. Finally, if spillovers are driven by overload, we can reduce them by shifting toward interventions that place fewer demands on information processing.

The model incorporates these three mechanisms, and generates three predictions about how they relate to comparative statics of behavior with respect to messages and incentives. This motivates an experiment design with five treatment groups: a control group, a group that gets messages about behavior x, a group that gets messages about behavior y, a group that gets both sets of messages, and a group that gets incentives for behavior y. I run an online experiment, recruiting 3,845 individuals via Facebook Ads that promote a study about daily meditation (behavior x) and nutritional monitoring (behavior y). Participants took a baseline survey and downloaded two smartphone applications, one for tracking meditation, and the other for logging meals. Upon verifying that they downloaded both applications, they were enrolled, randomized, and informed of their treatment assignment via email. Treatments began immediately thereafter, lasting four weeks. Participants in treatment groups with only x or y messages received twice daily messages, which included information about the benefits of the behavior as well as reminders. Participants in the group with both x and y messages received the union of both message sets, resulting in four daily messages. Lastly, participants in the y incentive group received an expected reward for every day they successfully logged their meals. I continued to measure behavior via the phone applications for an additional four weeks after the end of treatment, after which participants were sent a final survey.

The reduced form results show large target effects of both message and incentive interventions. Meditation messages raised the rate of meditation by 8.8 percentage points (almost double the control rate) and nutrition messages raised the rates of meal logging by 16.6 percentage points (more than double the control rate). Incentives for meal logging had an even larger effect, raising rates of meal logging by 38.1 percentage points (more than triple the control rate). But all three treatments imposed substantial spillovers on the opposite behavior, as measured by comparisons with the control group. Messages about meditation reduced meal logging by 2.4 percentage points (19%), messages about nutrition reduced meditation by 2.8 percentage points (29%), and incentives for meal logging reduced meditation by 2.5 percentage points (27%). The group with both sets of messages also did worse than the group with just meditation or just meal-logging messages, by 2.2 and 5.0 percentage points, respectively. There is no evidence of an interaction effect between the two sets of messages.

How costly are the observed spillovers? Suppose we want to increase both behaviors by one standard deviation. My results imply that once spillovers are taken into account, increasing both meditation and meal logging by one standard deviation costs 28% more than it would in the absence of spillovers.

As is made clear by the predictions of the model, the fact that all three interventions generated negative spillovers on the opposite behavior provides strong evidence of either diversion, depletion, or both. On the other hand, the fact that there is no evidence of a negative interaction between the two sets of messages indicates no evidence of overload in this context. Finally, the fact that all three interventions generate similarly sized spillovers suggests that depletion alone cannot drive the results. If it did, the more effective interventions should have evoked more costly effort and generated larger spillovers. This conclusion is supported by the fact that the covariance between meditation and meal logging does not vary across treatments. If depletion accounted for the full spillover, any intervention that successfully promoted the target behavior should also cause depletion and reduce the covariance between the two behaviors.

I confirm these conjectures by adding structure to the model and using classical minimum distance to estimate its 15 parameters with 22 moments. Consistent with the reduced form tests, I find that estimates of the parameters capturing both overload and depletion are not statistically different from zero. The estimates of both message and incentive diversion are negative, and the former is statistically significant. Again, consistent with the reduced form results, these estimates suggest that diversion is a key driver of spillovers in the case of message interventions. Given that depletion encompasses both costly time and effort, and that it is inseparable from complementarity in the utility function, we cannot draw any confident conclusions from its parameter estimate (especially given its large standard error). However, the conclusion that depletion cannot fully account for the observed spillovers is sufficient to draw a key policy implication.

Namely, the fact that interventions can divert attention even when they do not positively affect their target behaviors implies that the resulting spillovers act as "fixed costs." A natural implication of this is that two interventions that are equally cost-effective for one behavior will not be equivalent once multiple behaviors are taken into account. Rather, strong interventions that spread fixed spillover costs over a larger target effect are preferable. Specifically, imagine a hypothetical intervention that is 60% as strong as the one run in the experiment. For this intervention, my parameter estimates imply that increasing both meditation and meal logging by one standard deviation costs 54% more than it would in the absence of spillovers. On the other hand, a hypothetical intervention that is three times as strong as the one in my experiment mitigates the cost of spillovers substantially: raising both behaviors by one standard deviation is only 9.5% more costly in the presence of spillovers. Thus, my results indicate that strong (and expensive) interventions may be more cost-effective than weak (and inexpensive) ones, all else being equal, once multiple behaviors are taken into account.

Finally, the fact that I found no evidence of overload suggests that we have little reason to worry about high stimulus interventions. However, I do find evidence of overload with respect to the reading of messages: participants in the group with both sets of messages were less likely to respond to a surprise raffle (sent by SMS from the same number) than participants in groups with just one set of messages. I interpret this as evidence that overload caused people to read fewer messages, but did not prevent them from noticing the prompts and ultimately doing the behaviors.

This paper makes three key contributions. First, it organizes cross-disciplinary evidence on costly mental effort and attention into a framework that illuminates how such costs might mediate behavioral interventions. Second, it provide the first evidence that generic interventions can produce negative spillovers on other behaviors. Third, by using the framework to decompose spillovers in a useful way, it provides insight as to how interventions might be deliberately designed to mitigate spillovers in the future.

I begin in Section 2 by describing the model and its predictions for behavior with respect to incentives and messages. In Section 3 I describe the experiment. In Section 4 I present orthogonality tests, descriptive statistics, and reduced form results. In Section 5 I describe and estimate the structural model. In Section 6 I explore the policy implications. In Section 7 I show additional results on opting out, reading of messages, and expectations, and explore the role of time constraints. In Section 8 I conclude.

2 Framework

I use a simple framework to derive comparative statics of behavior with respect to messages and incentives. In this section I introduce the model, discuss the relevant evidence for costly effort and attention, and generate three predictions that will be testable with the reduced form results of the experiment. In Section 5 I add additional structure, which enables me to estimate key parameters associated with different mechanisms, run counterfactuals, and explore the policy implications of my results.

2.1 Set-Up

I consider an agent who chooses how much cognitive effort a_j to invest in two behaviors, x and y.[4] She receives return u_j per unit of effort invested in action $j \in \{x, y\}$. She also receives u_{xy} per joint unit invested, allowing for (positive or negative) complementarity between behaviors x and y.[5] The agent faces some effort cost, which I denote by the function C. Let the agent's utility over effort be $U(a_x, a_y) = a_x u_x + a_y u_y + a_x a_y u_{xy} - C(a_x, a_y)$, her returns from exerted effort minus its costs. She chooses a_x and a_y to maximize her utility.

$$\max_{a_x, a_y} \left\{ a_x u_x + a_y u_y + a_x a_y u_{xy} - C(a_x, a_y) \right\} \tag{1}$$

Effort is a latent variable; I observe only whether the agent does actions x and y in each period. I will assume that due to random error, I observe the agent undertaking action j as long as $a_j + \xi_j > 0$, where ξ_x and ξ_y are i.i.d. shocks that are independent from one another and conditionally uniformly distributed. The probability of observing, for example, $x = 1$ is thus $Pr(a_x + \xi_x > 0)$.

Effort costs cause the agent to expend less effort than she otherwise would. However, an outside actor can introduce an intervention w_j for behavior j, where the intervention can be either messages m or incentives z ($w_j \in \{m_j, z_j\}$). Let the modified cost function be $C(a_x, a_y, w_x, w_y)$. Let the marginal cost of a_x, C_1, be denoted as $c^x(a_x, a_y, w_x, w_y)$ and the marginal cost of a_y, C_2, be denoted as $c^y(a_y, a_x, w_y, w_x)$. I make the following assumptions about the cost function. First, I assume that $c_1^x > 0$ and $c_1^y > 0$ to ensure the existence of a local maximum. Second, I assume that

[4] I use "a" instead of "e" to emphasize that this is cognitive effort, which can also be understood as attention according to some definitions. For example, Chun et al. (2011) define attention in a way that includes everything from information processing to executive function to self-control. Throughout the paper I will use the terms "attention" and "cognitive effort" interchangeably.

[5] This can also capture the idea of "moral licensing," that people aim to maintain their positive self image, and thus engaging in something "good" can license one to subsequently engage in something "bad," or vice versa (e.g. Wertenbroch (1998); Strahilevitz and Myers (1998); Khan and Dhar (2006); Dolan and Galizzi (2015)).

both target messages and target incentives act as effort subsidies for the target behavior, reducing the marginal cost of effort: $c_3^x < 0$, $c_3^y < 0$.[6]

I define *overload* to be the possibility that $c_{34}^x > 0$ or $c_{34}^y > 0$ in the case of a message intervention ($w = m$). This means that y messages interfere with the subsidy produced by x messages (and vice versa). I define *diversion* to be the possibility that $c_4^x > 0$ or $c_4^y > 0$. Diversion thus operates like a tax: messages or incentives about behavior y increase the marginal cost of effort to x (and vice versa). I allow for the possibility of both message and incentive diversion. Lastly, I define *depletion* to be the possibility that $c_2^x = c_2^y > 0$. (From now on I will write $c_2^x = c_2^y$ as simply c_2^x.) This means that the marginal cost of exerting effort to do behavior x is increasing in the effort expended on behavior y, and vice versa. These definitions—overload, diversion, and depletion—are new, and have been constructed for the specific purpose of understanding intervention spillovers and their policy implications. However, the concepts and phenomena they encompass are not new at all. In the next section, I discuss relevant theory and evidence, from psychology and other disciplines, that underpin the concepts of overload, diversion, and depletion.

2.2 Overload, Diversion, and Depletion in the Literature

To better understand the concepts of overload, diversion, and depletion, I employ a taxonomy of attention that distinguishes between external versus internal attention (Chun et al., 2011).[7] External attention refers to the selection and modulation of external information, and the storing of that information in the brain.[8] Internal attention, on the other hand, refers to the selection and modulation of content that has already been stored in the brain. It includes the attention required to think about, plan, and make decisions about an action—including executive function, working memory, and long-term memory. It also includes the cognitive effort and self-control required to

[6] I assume that the outside actor will not implement both messages and incentives. The framework does not have interesting implications for interactions between messages and incentives, so I do not implement this treatment in my experiment.

[7] The roots of this taxonomy go back to 1890, when William James distinguished between "passive" and "active" attention. This basic division has persisted over the years, with several variants—bottom-up versus top-down attention, stimulus-driven versus goal-driven attention, exogenous versus endogenous attention, and finally external versus internal, which is what I use here.

[8] It can be directed to the sensory modalities of sight, hearing, touch, smell, and taste, and it can be used to perceive the world across space ("spatial attention") or time ("temporal attention").

carry out a task.

I build on this taxonomy to describe the possible ways in which interventions that require little time and no physical effort could impose negative spillovers, summarized in Figure 1. Suppose there is an intervention, say a text message, about some behavior x. External attention is used to modulate that stimulus and store it in our brains. Internal attention is then used to think about x, and ultimately do x. Now, suppose there is also a text message about some different behavior y. Three things might happen. First, limits to external attention, or limited information processing, might cause the y stimulus to interfere with the x stimulus. This is what I have defined to be "overload." Second, limits to working or short-term memory might cause the y stimulus to divert attention toward y and away from x, reducing the likelihood of doing x (regardless of whether or not there is any x stimulus). This is what I have defined to be "diversion." Finally, if the y stimulus works, causing us to do y and to exert costly cognitive effort or time, we might be subsequently less likely to do x. This is what I have defined to be "depletion." In short, overload can be summarized as "stimulus y affects stimulus x;" diversion can be summarized as "stimulus y affects behavior x," and depletion can be summarized as "behavior y affects behavior x."

There is ample research on overload across several disciplines. The fact that people are limited in their ability to process stimuli is so well-established that words like "selection" are commonplace in the psychology literature; the relevant question is not whether we select which stimuli to process but how.[9] Starting with Sims' (2003) model of rational inattention, many researchers have explored the implications of these information processing constraints for economic theory (e.g. Falkinger (2008); Eliaz and Spiegler (2011); Masatlioglu et al. (2012); Manzini and Mariotti (2012); Bordalo et al. (2012, 2013); Gabaix (2014); Schwartzstein (2014); De Clippel et al. (2014)). We have evidence that people have difficulty processing all of the information about the products they buy (Lacetera et al., 2012), all of the choices available to them (Chernev et al., 2015), and all of the dimensions of their production processes (Hanna et al., 2014).[10] We know less about how

[9]See for example one seminal paper which shows that limits to external attention are modality-specific: people are unable to attend to two visual or two auditory streams, but better able to attend to one of each (Duncan et al., 1997).

[10]In particular, the "information overload" literature in marketing has documented a hump-shaped relationship between the quantity of information that consumers have about products, and the "decision quality" of their ultimate purchase (Hwang and Lin, 1999; Edmunds and Morris, 2000; Eppler and Mengis, 2004).

information processing constraints mediate behavioral interventions. We do have some evidence that people become habituated or desensitized to alerts, designed to promote some behavior, over time.[11]

The possibility of diversion is also well supported in research across several disciplines. Experiments on working memory show that focusing on one thing often comes at the expense of something else.[12] Studies on "attentional capture" show that irrelevant stimuli can easily draw people's attention away from a task at hand (Yantis and Jonides, 1984).[13,14] Indeed, text message reminders have been used to capture attention and draw it to important economic choices like savings and electricity consumption people (e.g. Karlan et al. (2016); Taubinsky (2013); Allcott and Rogers (2014); Rogers and Milkman (2016)). Do we have any evidence that such interventions can divert attention away from other things? Hall and Madsen (2020) found that highway safety campaigns displaying roadside fatality counts is so effective at seizing attention that it actually *increases* the number of traffic crashes. And Medina (2017) found that sending SMS reminders to bank clients does effectively reduces late fees paid by 11%, but it also increases overdraft fees paid by 11%, resulting in a net loss for some.[15]

With regard to depletion, the opportunity cost of time is of course well appreciated in economics, as is the idea of costly physical effort, but less so the idea of costly cognitive effort, which is well-demonstrated in the psychology literature. For example, performance on difficult tasks tends to increase with incentives (Botvinick and Braver, 2015), and when given a choice between tasks that require high and low cognitive effort, participants tend to prefer the latter (Dunn et al.,

[11]For example, in medicine, as the use of electronic medical records and attendant automatic alerts to provide "decision support" have become widespread, there has been extensive discussion of "alert fatigue," the idea that physicians become habituated to alerts over time. One SMS program designed to alert physicians to new clinical trials found that response rates declined 2.7% every two weeks (Embi and Leonard, 2012)

[12]For example, people are capable of holding only limited sets of digits or words at a time (Miller, 1956; Luck and Vogel, 1997)

[13]The types of stimuli most likely to achieve attentional capture are novel stimuli (i.e. an unexpected SMS), emotionally salient stimuli (i.e. footage of a humanitarian crisis) and stimuli associated with rewards (i.e. a plate of cookies placed in front of you) (Fawcett et al., 2015; Chun et al., 2011).

[14]One relevant example is the phenomenon of "intention cost," or reduced performance (and brain activity) in a current task as a result of thinking ahead to a future task (Burgess et al., 2003; Gonen-Yaacovi and Burgess, 2012).

[15]The spillover is perhaps unsurprising given that both behaviors (paying bills and not going into overdraft) draw from the same budget. Nonetheless, it indicates the diversion of attention toward timely payment, and away from overdraft avoidance.

2016).[16] A rich economics literature has developed around one type of costly cognitive effort: self-control (e.g. Gul and Pesendorfer (2001, 2004); Noor (2007); Ozdenoren et al. (2012)), and has inspired creative interventions like commitment devices (Bryan et al., 2010) and temptation bundling (Milkman et al., 2014). If cognitive effort can be costly, as all of this research suggests, then there may well be unintended consequences of intervening to induce difficult behaviors. However, there is little economics or policy research on the question.[17]

2.3 Predictions

I now derive comparative statics of effort levels a_x^* and a_y^* with respect to messages and incentives, which will be randomly varied in the experiment. Since I have assumed that ξ_x and ξ_y are conditionally uniformly distributed, I can estimate them with linear probability models of behavior on treatments.[18]

Importantly, I will be unable to separately identify $c_2^x = c_2^y$ from u_{xy}, meaning that I will not be able to distinguish between (positive or negative) complementarities in the utility function and depletion in the effort cost function. For this reason, in the remainder of the paper, I will drop the u_{xy} term, and simply note that whenever I refer to depletion, I am really referring to depletion net of any complementarity in the utility function. This will have important implications for the interpretation of my results, which I will discuss in Sections 5 and 6.

I define a *spillover* to be the negative response of a behavior to a non-target intervention. If $\frac{\partial a_x^*}{\partial m_y} < 0$, it constitutes a *message spillover*, and if $\frac{\partial a_x^*}{\partial z_y} < 0$, it constitutes an *incentive spillover*. I define *interference* to be a negative interaction between two interventions. If $\frac{\partial^2 a_x^*}{\partial m_x \partial m_y} < 0$, it constitutes *interference*, and if both $\frac{\partial^2 a_x^*}{\partial m_x \partial m_y} < 0$ and $\frac{\partial^2 a_y^*}{\partial m_x \partial m_y} < 0$, it constitutes *interference in both directions*. I obtain three key predictions.

Proposition 1. *Either message (incentive) diversion or depletion is a necessary condition for message*

[16] Typically, the more automatic the task—the closer it is to some "default" behavior—the less effort it requires (Shenhav et al., 2017). Given that the policy goal of "behavior change" inherently asks people to move away from their defaults, it may also inherently require cognitive effort.

[17] Cognitive effort might be even more costly for those living under scarcity (Mani et al., 2013; Shah et al., 2012), which makes these questions particularly relevant when designing interventions for the poor.

[18] Probit and logit models give similar results.

(incentive) spillovers, and the presence of both message (incentive) diversion and depletion is a sufficient condition for message (incentive) spillovers.

Proof. The proof is straightforward from the expression for $\frac{\partial a_x^*}{\partial w_y}$, Equation 9 in Appendix A.1. $\square$

Proposition 1 implies that if I find message or incentive spillovers, they must be due to some type of limited internal attention. If I find neither, then the conclusion is ambiguous.

The second prediction requires two additional assumptions. First, I assume that, with the exceptions of c_{34}^x and c_{34}^y, the second derivatives of c^x and c^y are zero.[19] Second, I assume that $c_{34}^x = c_{34}^y$; namely, that the overload effect is symmetric across different behaviors. This implies that if two messages are sent, one about x and one about y, the first will interfere with subsidy generated by the second on y just as much as the second interferes with the subsidy generated by the first on x.[20]

Proposition 2. *Assume that $c_{34}^x = c_{34}^y$ and that all other second derivatives of c^x and c^y are zero. Then overload is a necessary condition for interference in both directions, and a sufficient condition for interference in one direction.*

Proof. See Appendix A.2. $\square$

The implication of Proposition 2 is as follows. If I find interference in both directions, it must be due to overload. If I find interference in neither direction, then there is no evidence of overload. And if I find interference in just one direction, the conclusion is ambiguous (and the second assumption does not hold).

The third prediction will help us distinguish between diversion and depletion.

Proposition 3. *Assume that $c_1^x = c_1^y$. If diversion is absent but depletion is present, then*

$$\frac{\partial a_i^*/\partial m_j^*}{\partial a_i^*/\partial m_i^*} = \frac{\partial a_i^*/\partial z_j^*}{\partial a_i^*/\partial z_i^*} \tag{2}$$

[19] I have no reason to believe that these derivatives are zero, but no reason to believe otherwise, as economic intuition tells us nothing about the signs of these derivatives.

[20] Ultimately I can check this assumption by testing whether $\frac{\partial^2 a_x^*}{\partial m_x \partial m_y} = \frac{\partial^2 a_y^*}{\partial m_x \partial m_y}$, and indeed this test is not rejected at the 5% level (p=0.09).

Proof. See Appendix A.3. □

In words, if diversion is absent but depletion is present, then the ratio between spillover and target effects generated by an intervention is fixed across our two intervention types: messages and incentives. This means that if we can reject equality of these ratios, we can be confident that spillovers are driven at least in part by diversion. The intuition for this proposition is the following. Recall that I have defined depletion to be the possibility that cognitive resources (effort, self-control, executive function) required to take action are costly. Since a spillover driven by depletion operates through the target action, we should expect interventions with large positive target effects to also have large negative spillovers, and interventions with small positive target effects to have small negative spillovers. These spillover/target effect ratios should be constant across different interventions.

3 Experiment Design

The experiment design is displayed in Table 1.[21] The control group received no intervention. Group 2 received only messages about behavior x, and Group 3 received only messages about behavior y. Group 4 received messages about behavior x as well as messages about behavior y. Group 5 received incentives for behavior y.

Behaviors x and y were daily meditation and nutritional self-monitoring. These behaviors were chosen for four reasons. First, they are important health behaviors for the sample frame (young Americans). A recent meta-analysis in the Journal of the American Medical Association found that meditation programs improved anxiety by 0.38 SDs at 8 weeks (and 0.22 at 3-6 months), improved depression by 0.30 SDs at 8 weeks (and 0.23 at 3-6 months), and reduced pain by 0.33 SDs at 8 weeks (Goyal et al., 2014). The use of smartphone apps for nutritional self-monitoring and feedback have been linked to weight loss (Wharton et al., 2014), which is associated with many health benefits.[22]

[21] The study was registered at the AEA RCT registry with a pre-analysis plan. Please see Appendix H for details.

[22] In the screening process, I asked participants if they would feel comfortable using a nutrition tracking app, and asked those who have struggled with eating disorders or body image issues in the past to consider this carefully.

Second, both behaviors can be measured objectively at high frequency via pre-existing smartphone applications. The meditation application allowed participants to access a wide variety of guided meditations or meditate on their own, and recorded details about each meditation session. In the nutritional monitoring application, participants inputted information about the meals they ate and then tracked various measures of the nutritional quality of their diet. Third, both behaviors require minimal amounts of time. The average meditation session was 21 minutes, but meal logging only took 11 minutes per day on average. Although this does not rule out the time constraint as a driver of spillovers, it does make it less likely. I will address this possibility in Section 7.3. Finally, these two behaviors are not obviously related to one another in any utility function, though this will not be an identifying assumption.

The 3845 participants were recruited using Facebook advertisements, targeting adults age 18-35 living in the U.S. (see Appendix B Figure A1). Upon clicking the link, participants underwent a brief screening that ensured they (1) had an iPhone or android; (2) were over 18; (3) were interested in working on wellness habits like meditation and tracking nutrition; and (4) were willing to download the two free applications. They then provided informed consent and proceeded to Survey 1, which took about 15 minutes. The first part of Survey 1 provided instructions for downloading the two apps (which were also emailed upon survey completion). Participants were instructed that in order to enroll, they would need to download both apps within 24 hours. Participants were then asked questions on demographics, electronic notifications, and preferences/experiences surrounding meditation and nutritional monitoring.

Participants who were verified to have downloaded both apps were then randomized to treatments, re-randomizing on gender, age, whether or not they had a college degree, daily notifications, whether or not they meditated in the last month, and whether or not they tracked their meals in the last month. These participants then received an enrollment confirmation email with their treatment assignment, a link to Survey 2, and other details about the study. Survey 2 required about five minutes and contained questions about participants' expectations of each behavior, conditional on their treatment assignment.

Importantly, when informed of their treatment assignment, participants were told that "this assignment was completely random, and has nothing to do with your survey responses or the relative importance of meditation, exercise, nutrition, and sleep." The purpose of this was to rule out an alternative potential source of spillovers or interference: the possibility that participants infer the relative benefits of the behaviors from their treatment assignment.[23] We also tell participants that, "Depending on your above assignment, we may (or may not) be encouraging you to meditate and/or log your meals, but your ultimate use of the apps is entirely up to you." The purpose of this was to avoid experimenter demand effects, and prevent participants from feeling obligated to engage in behaviors assigned to be treated (perhaps with motives of reciprocity or adherence to some imagined authority).

Each message program included twice-daily text messages: one simple reminder to do the behavior, and one longer message with information about some proven benefits to the behavior, as demonstrated in Table 2. As the table demonstrates, the timing of meditation vs. nutrition messages and information vs. reminder messages alternated in a balanced fashion. Messages were sent at either 7am and 7pm or at 8am and 8pm, alternating on a daily basis, and scheduled so that the group that received both meditation and nutrition messages never received them at exactly the same time (it was always the case that one message was at 7 and the other at 8). The purpose of this was to avoid capturing mechanical interference due to the simultaneous arrival of messages. There were 14 distinct messages, and 27 days of treatment, so each message (save one) was sent twice over the course of the program. The full set of messages is shown in Appendix Table A1. Participants were told during consent, in the enrollment email, and at the start of treatment that they could opt out any time by replying “STOP” to the relevant number.

We might be concerned that promoting meditation and meal logging together in one experiment artificially bundles the two behaviors in a way that does not reflect the way interventions are typically implemented in the real world. Ultimately this possibility will not be separable from depletion, and I will discuss what this implies for the interpretation of my results in Section 5. Still, to mimic the real world as much as possible, I give the meditation and meal logging programs separate names (Remindful and eNOMerate, respectively), and send the messages from different

[23] This mechanism is potentially important, but cannot be well studied in this kind of experimental context, since many participants already assume assignment is random.

phone numbers.

Incentives took the form of a raffle. Participants were informed in their enrollment email that they would earn one green lottery ticket for every day they successfully do the behavior, and one red lottery ticket every day that they do not. We informed them that at the end of the four weeks, we would draw one of their tickets, and each winning ticket would be worth a $10 Amazon gift certificate. Every Sunday during the program, participants received an email updating them about the tickets earned the previous week. They also received an email informing them when the program ended.

Four weeks after the end of treatment, participants received Survey 3 via email. Survey 3 included questions about meditation and nutritional monitoring outside of the assigned apps, the timing of behaviors, some measures of mental health and diet, and quizzes about the information content of any message program they received. For further details about the experiment protocol see Appendix B. For further details about attrition, see Appendix C.

Table 3 shows means and standard deviations of key variables across treatments, as well as an F-test of the joint significance all treatment variables. The re-randomization procedure ensured that the first variables were balanced across treatments, and the rest of the variables are highly balanced as well. Overall, the sample was overwhelmingly female (93%), mostly college educated (71%), with an average age of 27. Participants receive on average 51 notifications daily, 36 of which are messages, 10 of which are updates, 4 of which are reminders, and 1 of which was classified as "other." (See Appendix **??**, Figure A5 for details.) On average, participants perceived meal logging to be slightly more important than meditation and slightly more difficult, but the main difference in the behaviors is that meditation is perceived to be much more "fun" than meal logging. Most participants had experience with both meditation and meal logging. With respect to meditation, 90% had meditated before, 57% had done so on a daily basis, and 46% had done so in the last month. With respect to meal logging, 90% had logged their meals before, 87% had done so on a daily basis, and 32% had done so in the last month. (See Appendix **??**, Figure A6 for details.) These are people who have strong prior interest and experience in both behaviors, but who, for whatever reason, have not been engaged in them recently.

4 Reduced Form Results

I estimate linear probability models of the outcome on treatments at the individual-day level, where the outcome is 1 if the participant did the behavior on a given day. For meal logging, the behavior is having logged at least one meal and 0 otherwise. I define m_x (m_y) to be 1 if the individual received x (y) messages and 0 otherwise; m_x*m_y is 1 if the individual received both sets of messages. I include a vector of controls that consists of the variables used for re-randomization: whether or not the participant is female, whether or not they completed college, daily notifications, whether or not they meditated in the month prior to the study, and whether or not they logged a meal in the month prior to the study. I also include day fixed effects, and cluster standard errors at the individual level. Coefficients on message treatments represent intent-to-treat effects, as some participants chose to stop receiving messages.

The results are shown in Table 4. (See Appendix **??** Table A4 for estimates reported as treatment effects, and Figures A7 through A11 for depictions of the raw data.) During the treatment period, both sets of messages doubled the rates of their target behaviors: meditation messages raised the meditation rate from 9.4% to 18.2%, and nutrition messages raised the meal logging rate from 11.8% to 28.4%. Message treatments also had negative spillovers on non-target behaviors: participants getting only nutrition messages meditated 29% less than the control group (6.6% relative to 9.4%) and participants getting only meditation messages logged meals 19% less than the control group (9.4% relative to 11.8%). The group with both sets of messages did worse than the group with just meditation or just meal-logging messages, by 2.2 and 5.0 percentage points, respectively. There is no evidence of any interaction between the two sets of messages, however, neither for meditation nor for meal logging. Incentives for meal logging had large target effects, more than quadrupling the rate of meal logging (from 11.8% to 50%). They also had negative spillover effects, reducing meditation by 27% (6.9% relative to 9.4%). Given Proposition 1, the fact that m_x, m_y, and z_y all generated negative spillovers on the non-target behaviors implies that participants are subject to either diversion, or depletion, or both. And given Proposition 2, the fact that we find no strong evidence of interference in either direction implies that there is no evidence of overload.

In the post-treatment period, effects of messages on target behaviors persisted, at 28% and 18% the size of their treatment-period magnitude for meditation and meal logging, respectively. Target effects of incentives also persisted, at 10% the size of their treatment period effect. Importantly, negative spillover effects of meal logging messages and incentives on meditation rates persisted at almost 100% of their treatment period effects. There is no evidence, however, that spillovers of meditation messages on meal logging persisted. Figures A7 through A11 in the Appendix portray the raw data over the course of the treatment and post-treatment period. Engagement in both behaviors, and treatment effects, both diminish over time.

I test Proposition 3 by comparing spillover/target ratios across messages and incentives for behavior y. This comparison gives spillover/target ratios of 0.07 and 0.17 for y incentives and messages, respectively, and I can reject that the ratios are equal (p=0.03). This suggests that diversion is likely to be present, and that depletion cannot account for all of the spillovers we observe.

The data provide an additional test (which will be an additional source of identification) that was outside the scope of the simple model with homogeneous agents. Since we have defined depletion to be the possibility that doing one behavior raises the marginal cost of doing the other, we should expect that in the presence of depletion (net of complementarity in the utility function), doing x should negatively co-vary with doing y. With heterogeneous agents, however, the covariance between x and y also reflects any underlying covariance in preferences about meditation and meal logging. We can separate the two by comparing the covariance between x and y across treatments. Specifically, if there is no depletion (net of complementarity), then the covariance between x and y should be the same across all treatments, since it only reflects co-varying preferences which do not change with treatments. However, in the presence of depletion (net of complementarity), the covariance should vary across treatments, treatments induce different amounts of action, resulting in different amounts of depletion.

Table 5 shows the effects of treatments on the covariance between x and y. Our static model does not specify whether the predictions about the covariance refer to the covariance of x and y across people, or within a person over time. Both are plausible: depletion can conceivably cause people who meditate to be unlikely to also log their meals; it can also conceivably cause people who meditate on one day not to log their meals on the same day. Therefore, I check both: Column (1) looks at the covariance over individuals (within days) and Column (2) looks at the covariance over days (within individuals). If depletion drives spillovers, we should expect both sets of messages, as well as incentives, to have negative effects on the covariance. I see no evidence of this, supporting the conclusion that depletion (net of complementarity) cannot fully explain the spillovers we observe.

5 Structural Estimation

In the section that follows, I parameterize and estimate the model in order to quantify the contribution of each mechanism to the observed spillovers and draw out the policy implications.

5.1 Structural Model

Let $C(a_x, a_y, w_x, w_y) = f(a_x, a_y) - s^x(w_x, w_y)a_x - s^y(w_x, w_y)a_y$, where $f(a_x, a_y) = \frac{1}{2}\alpha(a_x^2 + a_y^2) + \rho a_x a_y$. Let the effort subsidy in the case of x messages be $s^x(m_x, m_y) = \nu_x m_x + \gamma m_x m_y + \theta_m m_y$, and in the case of y messages, $s^y(m_y, m_x) = \nu_y m_y + \gamma m_x m_y + \theta_m m_x$. Let the effort subsidy in the case of incentives be $s^x(z_x, z_y) = \lambda z_x + \theta_z z_y$ for x and $s^y(z_y, z_x) = \lambda z_y + \theta_z z_x$ for y. I thus assume that subsidies targeting behavior j reduce the marginal cost of effort to j by a fixed amount s^j. I allow target messages to have different effort subsidies depending on the behavior (allowing different ν_x and ν_y) but I assume that non-target messages impose the same tax regardless of the behavior (fixed θ_m, γ for x and y). I also impose that $c_1^x = c_1^y$.[24] In this parameterization, ρ captures depletion (net of complementarity), θ_m and θ_z capture diversion for messages and incentives, respectively, and γ captures overload. In my experiment I will only have incentives for y, so z_x will always be zero and λ will represent the target effect of incentives for y.

[24] I allow x and y to have different baseline returns (μ_x) and responses to target messages (ν_x, ν_y). All other differences between x and y (including any differences between c_1^x and c_1^y) are not identified, and will be loaded onto the aforementioned parameters.

In each period, I can thus write the agent's problem as:

$$\max_{a_x,a_y}\Big\{a_xu_x+a_yu_y-\Big(\frac{1}{2}\alpha(a_x^2+a_y^2)+\rho a_xa_y \\ -a_x(\nu_xm_x+\gamma m_xm_y+\theta_m m_y+\theta_z z_y)-a_y(\nu_ym_y+\gamma m_ym_x+\lambda z_y+\theta_m m_x)\Big)\Big\}$$

Let a^*_{xit} represent the optimal attention paid to behavior x by individual i in period t, with a^*_{yit} defined similarly. I allow for individual heterogeneity in u_x and u_y. I let $u_{xi}=\mu_x+\epsilon_{xi}$, and I normalize μ_y to 1, so that $u_{yi}=1+\epsilon_{yi}$. I assume that ϵ_{xi} and ϵ_{yi} are jointly normal, with mean zero, variances $\sigma^2_{\epsilon_x}$ and $\sigma^2_{\epsilon_y}$, and covariance $\sigma_{\epsilon_x\epsilon_y}$. I also allow for individual heterogeneity in the effects of target messages, so that $\nu_x=\phi_x+\delta_{xi}$ and $\nu_y=\phi_y+\delta_{yi}$. I assume that δ_{xi} and δ_{yi} are jointly normal, with mean zero, variances $\sigma^2_{\delta_x}$ and $\sigma^2_{\delta_y}$, and covariance $\sigma_{\delta_x\delta_y}$. I define $a^*_{xi}=E_{\xi_x}[a^*_{xit}]$, which I can estimate in the data as: $\hat{a}^*_{xi}=\frac{1}{T}\sum_{t=1}^{T}x_{it}$. I define a^*_{yi} and $\hat{a}^*_{yi}$ similarly.

5.2 Estimation

I estimate the model using classical minimum distance. I use the following 22 moments: the control means of a^*_{xi} and a^*_{yi}, the treatment effects on a^*_{xi} and a^*_{yi}, the control variances of a^*_{xi} and a^*_{yi}, the main and interaction effects of m_x and m_y on the variances of a^*_{xi} and a^*_{yi}, the control covariance of a^*_{xi} and a^*_{yi}, and the main and interaction effects of m_x and m_y on the covariance of a^*_{xi} and a^*_{yi}.[25]

The 22 moments as functions of the 15 parameters are written out in Appendix D. The simple expressions make it relatively straightforward to see how each parameter is identified. Most important is the identification of the relationship between α and ρ, which will allow us to distinguish depletion from diversion. This relationship is identified by the four target and spillover effects $\frac{\partial a^*_x}{\partial m_x}$, $\frac{\partial a^*_x}{\partial m_y}$, $\frac{\partial a^*_y}{\partial m_y}$, and $\frac{\partial a^*_y}{\partial m_x}$. The intuition is the same as that of Proposition 3: if ρ is zero, then we expect $\frac{\partial a^*_x}{\partial m_y}=\frac{\partial a^*_y}{\partial m_x}$. If ρ is positive, then the difference between $\frac{\partial a^*_x}{\partial m_y}$ and $\frac{\partial a^*_y}{\partial m_x}$ should reflect the difference between $\frac{\partial a^*_y}{\partial m_y}$ and $\frac{\partial a^*_x}{\partial m_x}$, since both are driven by differences between ϕ_x and ϕ_y. The relationship between α and ρ is also identified by the differences in the covariance between a_{xi}

[25] I do not use the effects of incentives on variances and covariances because for the sake of simplicity, I have not allowed for heterogeneity in the incentive attention subsidy.

and a_{yi} across treatments. The intuition is the same as that described in Section 4: if ρ is zero, then there should be no difference in the covariance across treatments; if ρ is positive, then treatments that induce higher x or y should also induce a lower covariance.

Once I have pinned down the relationship between α and ρ, I can use the control group means to separately identify α and ρ. The remaining parameters are straightforward to identify once α and ρ are known. Specifically, ϕ_x, ϕ_y, λ, θ_m, and θ_z are identified by the three target effects and three spillover effects of messages and incentives. The identification of the diversion parameters depends critically on ρ having already been pinned down. γ is (over-)identified by the two interference effects; this arises mechanically from the way overload was defined in the model. σ_{ϵ_x}, σ_{ϵ_y}, and σ_{xy} are identified by the variances and covariances of a_{xi} and a_{yi} in the control group, and σ_{phi_x}, σ_{phi_y}, and $\sigma_{\phi_x \phi_y}$ are identified by the variances and covariances of a_{xi} and a_{yi} across treatments.

Let ζ represent the vector of q parameters and let $m(\zeta)$ represent the r moments as functions of the parameters. The minimum-distance estimator selects parameters $\hat{\zeta}$ that minimize the expression $(m(\zeta) - \hat{m})'W(m(\zeta) - \hat{m})$. For the weighing matrix W I use the diagonal of the inverse of the variance-covariance matrix of the moments.[26] I estimate the variance of $\hat{\zeta}$ as $(\hat{G}'W\hat{G})^{-1}(\hat{G}'W\hat{\Lambda}W\hat{G})(\hat{G}'W\hat{G})^{-1}$, where $\hat{G} = \Delta_\zeta m_n(\hat{\zeta})$ (the matrix of derivatives of the moments with respect to parameters, evaluated at the estimated parameters) and $\hat{\Lambda} = Var(\hat{m})$.

The maximized value of the objective function is asymptotically distributed as $\chi^2(r - q)$, so the critical value for an over-identification test of model fit is 2.17. The maximized value of the objective function is 12.3, so the test is rejected. Figure A3 compares the actual moments and predicted moments. The high test statistic is driven principally by moment 12, $\frac{\partial var(ax)}{\partial my}$. It turns out that in the data, this estimate is significantly negative, at -0.015 (0.005). In the model, the variance is indeed predicted to fall, but only by a very small amount. Since the economic magnitude of this deviation is small, and since this moment is not critical to the identification of the parameters of interest, I do not consider this to be strong evidence that the model is wrong.

[26]With small samples, using the full variance-covariance (VC) matrix results in biased estimates (Altonji and Segal, 1996). Indeed, in this case, similar estimates are obtained using the diagonal of the VC matrix or the identity matrix, while different estimates are obtained using the full VC matrix. I show estimates using the identity matrix in Appendix E.

I report parameter estimates in Table 6. There is not strong evidence of overload, as $\hat{\gamma}$ is negative but not significantly different from zero. We cannot say much about depletion (net of complementarity), as $\hat{\rho}$ is positive but its standard error is very high. Message diversion is negative and statistically significant; incentive diversion is closer to zero and has a much higher standard error. These results confirm what we saw in the reduced form results: that diversion is an important driver of spillovers. In Appendix E Table A3, I present estimates using the same moments and parameters, but using the identity matrix as the weighing matrix. The estimates are very similar and the main conclusions are consistent. In Appendix E Figure A4 I examine the sensitivity of the four key parameter estimates to moments as in Andrews et al. (2017).

What can we say about the presence or absence of depletion? Recall that ρ potentially encompasses three things: time constraints, costly cognitive effort, and complementarity between behaviors x and y. There are thus several forces that might push ρ upwards. The time constraint might be binding, despite the choice of non-time-consuming behaviors, or people might create artificial time constraints within mental accounts that do bind (i.e. allotting 30 minutes per day for wellness). Perhaps moral licensing causes negative complementarity between these two positive health behaviors in the utility function. The fact that the experiment artificially bundled these two behaviors might cause negative complementarity between the behaviors. Or, finally, engaging in these behaviors might require costly mental effort that depletes internal resources. There are also several forces that might push ρ downwards. There could be positive complementarity in the utility function between behaviors x and y: meditation might cause people to care more about their health, raising the utility from logging meals. It is also possible that the fact that the experiment artificially bundled these two behaviors created a reminder effect, if the act of meditating also reminded people to log their meals, reducing the mental effort of doing so. These possibilities, as well as the high standard error around the estimate of ρ, make it difficult to say anything about depletion. Importantly, and fortunately, the key policy implications are tied not to the presence or absence of depletion, but to whether it *alone* can explain the spillovers we observe. In other words, the confirmed presence of diversion is what allows us to draw the key conclusion for policy, which I discuss in detail in Section 6.

6 Policy Implications

How do these results inform policy? In this section I aim to answer two questions. First, how costly are spillovers? Second, how does the costliness of spillovers vary with the features of the intervention? I focus on two features, as described in the introduction: the reliance of the intervention on information, and the effectiveness or strength of the intervention. To answer both policy questions, I will exploit the following statistic: if it costs \$1 to increase x and y by one standard deviation in the absence of spillovers, how much does it cost in the presence of spillovers? In other words, assuming that we care about increasing both x and y by the same amount (and abstracting from the complicated question of the welfare benefits of different amounts of different behaviors), how much more costly are interventions once spillovers are taken into account? More formally, I define $\hat{G}$ to be:

$$\hat{G} \equiv \frac{\tilde{w}_x^S + \tilde{w}_y^S}{\tilde{w}_x^{NS} + \tilde{w}_y^{NS}} \tag{3}$$

I define $\tilde{w}_i^S$ is the intervention size needed to increase target behavior i by 1 standard deviation in the presence of spillovers, and $\tilde{w}_i^{NS}$ is the intervention size needed to increase target behavior i by 1 standard deviation in the absence of spillovers. Specifically, I define $\tilde{w}_x^S$ and $\tilde{w}_y^S$ to be the solutions to the following equations:

$$\sigma_x = \frac{\partial a_x^*}{\partial w_x}\tilde{w}_x^S + \frac{\partial a_x^*}{\partial w_y}\tilde{w}_y^S \tag{4}$$

$$\sigma_y = \frac{\partial a_y^*}{\partial w_y}\tilde{w}_y^S + \frac{\partial a_y^*}{\partial w_x}\tilde{w}_x^S \tag{5}$$

I define $\tilde{w}_x^{NS}$ and $\tilde{w}_y^{NS}$ to be the solutions to the same equations in the absence of spillover effects (i.e. when $\frac{\partial a_x^*}{\partial w_y} = \frac{\partial a_y^*}{\partial w_x} = 0$). Since I did not find strong evidence of overload, I do not allow for any interaction between messaging interventions. This keeps the analysis simple, and means that $\hat{G}$ can be seen as a lower bound for the relative cost of spillovers.

To answer the first question—how costly are spillovers?—I rely only on my reduced form estimates of $\hat{\sigma_x}$, $\hat{\sigma_y}$, $\frac{\partial \hat{a_x^*}}{\partial w_x}$, $\frac{\partial \hat{a_x^*}}{\partial w_y}$, $\frac{\partial \hat{a_y^*}}{\partial w_y}$, and $\frac{\partial \hat{a_y^*}}{\partial w_x}$. I compute $\hat{G}$ and report standard errors using 500 bootstrapped samples of 600 observations, stratified by treatment. I find $\hat{G}$ to be 1.277 (S.E. 0.143), meaning that increasing both meditation and meal logging by 1 standard deviation is 28% more costly in the presence of spillovers. I can reject that $\hat{G}$ is different from 1 at the 10% significance level ($p = 0.053$).

With respect to the second question, I begin by addressing the question of interventions that rely heavily on information. Since I found minimal evidence of overload (γ was significantly different from zero), and no significant differences between message and incentive diversion, there is no evidence to suggest that policymakers should be concerned about interventions with large amounts of stimuli or information. In Section 7 I use some additional data to shed light on this question, and revisit the policy implications accordingly.

For the remainder of this section I focus on the question of how the effectiveness or strength of the intervention affects the costliness of spillovers. Recall that spillovers driven by diversion, unlike those driven by depletion, can exist even when the intervention is not effective. In other words, a high-impact intervention will not necessarily create larger spillovers than a low-impact one. Suppose we have a high-impact intervention that is equally cost-effective to a low-impact intervention when one behavior is considered. In the presence of diversion, the former is predicted to be more cost-effective when multiple behaviors are considered. In what follows I focus on the case of messaging interventions, as the uncertainty around incentive diversion is too high to allow for reasonably confident policy conclusions.

Since this question relies on the relative contributions of diversion and depletion to spillovers, I begin by using my parameter estimates to decompose the observed spillovers. Recall the expressions for the spillover moments:

$$\frac{\partial E a_x^*}{\partial m_y}\Big|_{m_x=0} = \frac{\alpha\theta_m - \rho\phi_y}{\omega} \tag{6}$$

$$\frac{\partial E a_y^*}{\partial m_x}\Big|_{m_y=0} = \frac{\alpha\theta_m - \rho\phi_x}{\omega} \tag{7}$$

In each expression, the first term represents the component of the spillover driven by diversion, and the second term represents the component of the spillover driven by depletion. Using my parameter estimates, and bootstrapping standard errors, I find that diversion explains 85.6% of spillovers generated by m_y on x (S.E. 67), and 92.3% of spillovers generated by m_x on y (S.E. 32). The standard errors are large, but for the latter, a 95% confidence interval does not contain zero and we can rule out the possibility that depletion alone accounts for it.

Next, I turn to the role of intervention strength. I run counterfactual simulations in which I scale ϕ_x and ϕ_y by (the same) multiplier τ, where I allow τ to run from 0.6 to 3.[27] I then use the rest of my parameter estimates to simulate treatment effects, and plug them into Equations 6 and 7 to compute the same statistic, $\hat{G}$. This allows me to compute the costliness of spillovers when both interventions are between 60% and three times as strong as they were in my experiment.

An important caveat when interpreting this calculation is that although I know that the extent of diversion does not *necessarily* depend on the strength of the intervention, I cannot say that the two are unrelated. In fact, it is plausible that on average, stronger interventions do generate more diversion. In the meantime, I assume that strength and diversion are unrelated, so the results should be interpreted accordingly.

Figure 2 shows the results; the shaded region represents standard errors. On the x-axis I vary τ, the multiplier by which I scale ϕ_x and ϕ_y. On the y-axis I report $\hat{G}$. When interventions are 60% as strong as the ones in my experiment, intervening to raise both x and y is about 54% more costly in the presence of spillovers, relative to the case with no spillovers. When interventions are twice as strong as the ones in my experiment, raising both behaviors is 13.6% more costly in the presence of spillovers. And when interventions are three times as strong as the ones in my experiment, raising both behaviors is only 9.5% more costly in the presence of spillovers. Thus, the prominent role of diversion has an important policy upshot: when targeting multiple behaviors, strong interventions are more cost-effective than weak ones, because they generate less negative spillover per unit of target impact achieved.

[27] The pattern is the same for interventions with $\tau < 0.6$, but the standard errors become very high, so I exclude this range from the plot.

7 Additional Evidence and Alternative Explanations

7.1 Should we be worried about overload?

Since I found only minimal evidence of overload, and no significant differences between message and incentive diversion, there is no evidence to suggest that policymakers should be concerned that interventions reliant on stimuli or information crowd one another out in terms of behavior. However, I can use two additional sources of data to check to see whether receiving messages about both behaviors affected the likelihood of opting out or the reading of messages.

To opt-out, participants simply had to reply "STOP" to the same number from which they were receiving messages. They were informed of this in the consent form, by email upon treatment assignment, as well as in the first text message they received. Table 7 examines whether participants receiving meditation messages ever opted out (Column 1), and whether participants receiving nutrition messages ever opted out (Column 2), where the omitted group has one set of messages, and the treatment group has both. Note that here we are capturing both spillover and interaction effects.

Baseline levels of opt-out were relatively high, at 14.5% for the meditation-only group and 16.8% for the nutrition-only group. There is no evidence that participants receiving both sets of messages opted out more frequently than those with just one. For opt-out of the meditation program, the coefficient has the opposite sign. For opt-out of the nutrition program, the coefficient has the correct sign but is not statistically different from zero ($p = 0.11$).

To understand the extent to which participants actually read and internalized the content of messages, I use two additional measures that I collected during the experiment. The first measure is whether or not participants responded to a surprise raffle sent to participants with messages via SMS.[28] The message said: "Hi from Remindful/eNOMerate! We are offering a surprise raffle for a USD 20 Amazon gift card. To enter, tap [link] and press send. Msg & dta rates may apply." Each message participant received a maximum of one raffle message over the course of the experiment. Roughly half of message participants received the raffle on day 10 or 11 (halfway through the

[28] Due to an implementation error, we are missing this data from 592 participants with messages. These participants accidentally received messages with a broken link, and so we do not know whether they responded or not.

second week) and the other half received the raffle on day 20 or 21 (at the end of the third week). Participants receiving both messages were randomly assigned to receive either the eNOMerate raffle or the Remindful raffle.

The second indicator of internalization of messages is knowledge about meditation and nutrition, as measured by the percentage of questions answered correctly on a quiz administered at the end of the study, one month after the end of treatment. The quiz consisted of true/false questions on information provided in the messages, with additional options to answer "I remember seeing this message but I do not remember the details" or "I do not remember seeing this message." Participants received 1 point for every correct answer, 0 point for every incorrect or "I do not remember seeing this message" answer, and 0.5 points for answering "I remember seeing this message but I do not remember the details." They were unaware of this scoring system, and had no explicit incentives to perform well. Both raffle response rates and quiz scores are restricted to participants with messages (I do not quiz groups on information they did not receive), so the omitted group has one set of messages, and the treatment group will have both.

Table 8 displays the results. Raffle response rates were low even in the groups with one set of messages, at 31% and 26% for meditation and meal logging raffles, respectively (but higher conditional on not opting-out, at 36% and 31% respectively). The group with both sets of messages was about 30% less likely to respond to both raffles, suggesting that they were reading messages at a lower rate. This might explain why these groups also did slightly worse on the knowledge quizzes, as demonstrated in columns (2) and (4), though these effects are not significant. The coefficient of interest does not change substantially when I restrict the sample to those who did not opt out, suggesting that the bulk of the effect is driven by participants who continued to receive messages throughout the treatment period.

Interestingly, this result indicates overload with respect to the reading of messages. It appears that the people receiving both sets of messages (2 daily about x and 2 daily about y) simply stopped reading them, relative to those receiving just one set. How is this consistent with the fact that we did not find evidence of overload with respect to behavior? Suppose the messages worked mostly as prompts, and not as information. Then just seeing or hearing the notification (and not actually reading the message) may have been sufficient to induce action and create the treatment effects we observe. Then, if prompts do not generate overload, but information does, we would expect to see overload with respect to the reading of messages, but not with respect to behavior, which is exactly what we see. So although our interventions did not appear to generate overload with respect to behavior, we may have reason to worry about interventions that rely on extensive information provision. More research is needed to understand how different features of information content—its difficulty, its length, its timing—affect an intervention's potential to generate interference.

We might also worry that the failure to find evidence of interference masks important heterogeneity across subjects. For example, we might expect interference effects to differ across participants with different amounts of *total* notifications.[29] In Appendix G I estimate treatment effects separately for participants with baseline notifications above and below the median. I find no evidence of heterogeneity by the baseline amount of notifications, but it should be noted that the experiment was not powered to detect these effects.

7.2 Did people expect spillovers?

During Survey 2, immediately after treatment assignment, but before treatment began, I collected data on the expectations of participants. Table 9 shows regressions of individual-level expectations about rates of meditation (Column (1)) and meal logging (Column (3)) on treatment assignment. Columns (2) and (4) show the differences between expected and actual rates of meditation and meal logging, respectively. Participants in the control group grossly over-predict their rates of both behaviors: they expect to meditate 38% of the time when actually they do so 9.4% of the time, and they expect to log their meals 52% of the time when they actually do so 11.8% of the time. Participants who receive only meditation messages predict their meditation rates to be even higher than the control group, but the level of over-prediction is similar, resulting in an "expected target effect" (11%) that is actually quite close to the true target effect (8.8%). Participants who receive

[29]The effect could go in either direction. In Appendix G I provide an example of how my model might be modified to generate predictions about heterogeneous spillover effects by total notifications. However, we would need more assumptions to generate a prediction about interference, and to take potential selection effects into account.

only nutrition messages also predict their meal-logging rates to be higher than the control group, but here they over-predict less, resulting in an "expected target effect" (12%) that is significantly lower than the true target effect (16.6%).

"Expected spillover effects" of nutrition messages and incentives on meditation are small and positive (though neither of these effects are significant). Participants thus significantly over-estimate spillover effects by 5 percentage points in both cases (i.e. they under-estimate negative spillovers by 5 percentage points). In the case of the spillover of meditation messages on meal logging, we cannot conclude that participants correctly predicted the negative spillover (the expected effect is about 70% of the true spillover, at -0.017, but the standard error is high), but there is no evidence that they under-estimated spillovers either.

7.3 Can time constraints account for spillovers?

As discussed in Section 5, the parameter ρ encompasses several things: time constraints, costly cognitive effort, and complementarity between x and y. The fact that the estimate for ρ was not significantly different from zero thus tells us little about the importance of time constraints. What can we conclude, then? First, most basically, the reduced form results show that even behaviors that take very little time can generate spillovers on other behaviors. Second, the structural estimation suggests that ρ (and therefore time constraints) cannot *alone* explain the observed spillovers.

As an additional check of the second conclusion, I use my estimates to compute the implied elasticity of substitution between meditation and meal logging in the case that spillovers are driven fully by the time constraint.[30]

Suppose agents have CES utility, $u(h_x, h_y) = (\alpha h_x^\rho + (1-\alpha)h_y^\rho)^{(1/\rho)}$, where h_x and h_y are daily hours spent on meditation and meal logging, respectively. It can be easily shown that the cross-price elasticity of meditation with respect to meal logging ϵ_{xy} is equal to $(\sigma - 1) * s_y$, where $s_y = \frac{h_i p_i}{Y}$, the share of income spent on y, and $\sigma = \frac{1}{1-\rho}$, the elasticity of substitution. In our case, $s_y = \frac{h_i}{24}$, if I let p_i represent the value of 1 hour of time.

I can use my estimates to approximate a lower bound for ϵ_{xy}. Table A6 shows that daily

[30] This exercise does not account for more sophisticated models of time constraints within mental accounts (i.e. allotting 30 minutes a day to wellness).

average minutes meditated fell from 1.8 to 0.98, a 45% decrease, in the presence of incentives for meal logging. The incentives had an expected value of $0.37 per successful day (with at least one meal logged), so I assume the price of meal logging fell by this amount. I approximate the price of meal logging to be $20 * (3.78/60) = 1.26$, assuming that the average hourly wage for a college graduate is $20, and that the minimum necessary time spent on meal logging was 3.78 minutes (the time required to log one meal as reported in the final survey, which is a lower bound for the time spent on daily meal logging). Together, this implies that incentives for meal logging reduced the price of meal logging by 29.4%, and that the cross price elasticity, ϵ_{xy}, is equal to 1.53. To calculate h_y, I use the time required to log all of one's meals as reported in the final survey, 11.82 minutes (an upper bound for the time spent on daily meal logging) and multiply it by the average daily rate of logging at least one meal across all groups, which was 0.14. Combining everything, I find that in order to explain the spillovers we see, the elasticity of substitution would need to be at least 1331. In summary, these behaviors take so little time that in a standard model, the elasticity of substitution would need to be unrealistically high in order for the time constraint to explain the observed spillovers.

8 Conclusion

A growing literature has documented a wide variety of interventions that shift behavior and generate meaningful economic impacts. This paper has demonstrated the importance of studying these interventions in a broader context, taking into account how they affect other interventions and other behaviors. In particular, costly mental effort and attention can cause interventions to have negative spillovers on seemingly unrelated behaviors. These spillovers do not necessarily grow with the effectiveness of the intervention, suggesting that small-scale, highly-effective interventions may be generally preferable to large-scale, less effective ones, all else being equal. This paper provides evidence that generic spillovers are possible, but raises many additional questions about the generalizability of these effects to other behaviors, contexts, and interventions.

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Table 1: Experiment Design

Group	Description	Messages $m_x = 1$	Messages $m_y = 1$	Incentives $z_y = 1$	N
1	ctrl				814
2	x messages	x			763
3	y messages		x		803
4	x & y messages	x	x		822
5	y incentives			x	643
					3845

Notes: The experiment included five treatment groups: a control group, a group that received only messages about meditation (x), a group that received only messages about meal logging (y), a group that received messages about both meditation (x) and meal logging (y), and a group that received incentives to log meals (y).

Table 2: Example Messages, Day 1

Group	Time	Msg 1	Msg 2
med only	8AM	Remember to meditate today! Try the 3-minute breathing space by Mark Williams on [meditation app]!	
	3PM	A meta-analysis in a top medical journal reviewed 47 studies and found systematic evidence that meditation reduces depression and anxiety! (Goyal et al. 2014)	
med & nut	8AM	Remember to meditate today! Try the 3-minute breathing space by Mark Williams on [meditation app]!	Logging meals can help with weight loss (Burke et al. 2011)! And people are better at meal-logging when they use apps like [meal logging app] (Wharton et al. 2014).
	3PM	A meta-analysis in a top medical journal reviewed 47 studies and found systematic evidence that meditation reduces depression and anxiety! (Goyal et al. 2014)	Take one minute to log your meals using [meal logging app] today!

Notes: This table shows the messages that were sent on Day 1, for treatment groups 2 (med only) and 4 (med & nut), as an example. (Treatment group 3 received the same nutrition messages as group 4, but without the meditation messages.) Each message program included twice-daily text messages: one simple reminder to do the behavior, and one longer message with information about some proven benefits to the behavior. Messages were sent at either 7am and 7pm or at 8am and 8pm, alternating on a daily basis. There were 14 distinct messages, and 27 days of treatment, so each message (save one) was sent twice over the course of the program. The full set of messages is shown in the Appendix in Table A1.

Table 3: Orthogonality Check

	control	mx	my	mx & my	zy	F-test, joint sig
female†	0.93	0.93	0.93	0.93	0.93	1.00
	0.25	0.25	0.25	0.25	0.26	
went to college†	0.71	0.72	0.71	0.71	0.72	0.99
	0.45	0.45	0.45	0.45	0.45	
age	27.46	27.43	27.20	27.74	27.11	0.19
	5.72	6.05	5.22	5.48	4.90	
daily notifications†	52.59	53.32	52.70	54.16	53.40	0.99
	70.13	83.67	78.54	74.26	70.23	
meditated daily, ever	0.56	0.59	0.56	0.56	0.57	0.79
	0.50	0.49	0.50	0.50	0.49	
meditated daily, last month†	0.47	0.46	0.46	0.46	0.46	0.99
	0.50	0.50	0.50	0.50	0.50	
logged meals, ever	0.88	0.87	0.86	0.87	0.89	0.50
	0.33	0.34	0.35	0.34	0.32	
logged meals, last month†	0.33	0.33	0.32	0.33	0.33	0.99
	0.47	0.47	0.47	0.47	0.47	
importance, x - y	-0.44	-0.49	-0.42	-0.45	-0.47	0.99
	3.36	3.30	3.39	3.28	3.26	
difficulty - fun, x - y	-2.48	-2.76	-2.64	-2.54	-2.54	0.83
	4.93	5.07	5.35	5.16	5.10	

Notes: Means and standard deviations of ten variables measured in the baseline survey. † indicates variables used in the re-randomization procedure. F-test of the joint significance of treatments is reported in last column. *Daily notifications* includes all notifications the participant receives across all devices and all applications, where a notification is defined as anything that generates an alert (including SMS and email). *Importance,* $x - y$ is the "importance" of meditation, self-reported on a scale from 1 to 10, minus that of meal logging. *Difficulty,* $x - y$ is the "difficulty" of meditation, self-reported on a scale from 1 to 10, minus that of meal logging. *Fun,* $x - y$ is the "fun" of meditation, self-reported on a scale from 1 to 10, minus that of meal logging. I report only the difference between difficulty and fun, since both contribute to the experience of doing the behavior.

Table 4: Reduced Form Results

	Treatment Period		*Post-Treatment Period*	
	Meditated (x) (1)	Logged Meal (y) (2)	Meditated (x) (3)	Logged Meal (y) (4)
mx	0.088***	-0.024**	0.024***	-0.010
	(0.011)	(0.010)	(0.009)	(0.006)
my	-0.028***	0.166***	-0.025***	0.029***
	(0.008)	(0.013)	(0.007)	(0.008)
mx*my	0.006	-0.026	0.009	-0.002
	(0.014)	(0.018)	(0.011)	(0.010)
zy	-0.025***	0.381***	-0.024***	0.037***
	(0.009)	(0.016)	(0.007)	(0.008)
mx + mx*my	0.094	-0.050	0.034	-0.013
	(0.009)	(0.014)	(0.007)	(0.008)
my + mx*my	-0.022	0.140	-0.016	0.027
	(0.011)	(0.012)	(0.009)	(0.007)
Ctrl Mean	0.094	0.118	0.054	0.033
Ctrl SD	0.291	0.323	0.227	0.178
Obs	102905	102905	102499	102499

Notes: OLS regressions at the individual-day level of daily meditation (0/1) and daily logging of at least one meal (0/1) on treatments. mx (my) is 1 if the individual received x (y) messages and 0 otherwise; mx*my is 1 if the individual received both sets of messages. The specification includes controls for the five baseline variables on which re-randomization was based (female, college, daily notifications, whether individual meditated in last month, whether individual logged meal in last month) as well as day fixed effects. Standard errors clustered at individual level. One, two, and three stars indicate q-values of 1%, 5%, and 10% respectively; q-values calculated according to the Benjamini Hochberg step-down procedure, considering all tests in the table (but excluding linear combinations of coefficients).

Table 5: Treatment Effects on Covariances

	Treatment Period	
	Cov(x,y) over people (within day) (1)	Cov(x,y) over time (within people) (2)
mx	0.001	0.002
	(0.004)	(0.001)
my	-0.002	0.001
	(0.004)	(0.001)
mx X my	0.021***	0.007***
	(0.005)	(0.002)
zy	-0.003	-0.001
	(0.004)	(0.001)
Ctrl Mean	0.019	0.008
Ctrl SD	0.138	0.084
Obs	102905	102905

Notes: OLS regressions at the individual-day level of the covariance between daily meditation (0/1) and daily logging of at least one meal (0/1) on treatments. Column 1 reports the covariance over people within a day (do people who meditate on a particular day also tend to log their meals?). Column 2 reports the covariance over days within a person (does someone who meditates a lot over the treatment period also log their meals a lot over the treatment period?). mx (my) is 1 if the individual received x (y) messages and 0 otherwise; mx*my is 1 if the individual received both sets of messages. The specification includes controls for the five baseline variables on which re-randomization was based (female, college, daily notifications, whether individual meditated in last month, whether individual logged meal in last month) as well as day fixed effects. Standard errors clustered at individual level. One, two, and three stars indicate q-values of 1%, 5%, and 10% respectively; q-values calculated according to the Benjamini Hochberg step-down procedure, considering all tests in the table (but excluding linear combinations of coefficients).

Table 6: Parameter Estimates

Parameter	*Description*	*Estimate*	*Standard Error*
α	slope of marginal cost of effort	8.239	0.664
$\boldsymbol{\rho}$	**depletion**	**0.173**	**0.468**
μ_x	return to x	0.792	0.067
ϕ_x	x message attn subsidy	0.726	0.124
$\boldsymbol{\gamma}$	**overload**	**-0.047**	**0.109**
$\boldsymbol{\theta_m}$	**message diversion**	**-0.200**	**0.075**
λ	y incentive attn subsidy	3.130	0.335
ϕ_y	y message attn subsidy	1.354	0.195
σ_x	S.D. of ϵ_x, heterogeneity in return to x	1.594	0.113
σ_y	S.D. of ϵ_y, heterogeneity in return to y	1.748	0.069
$\boldsymbol{\theta_z}$	**incentive diversion**	**-0.145**	**0.184**
σ_{xy}	covariance of ϵ_x and ϵ_y	1.000	0.358
σ_{ϕ_x}	S.D. of δ_x, heterogeneity in message subsidy	1.128	0.212
σ_{ϕ_y}	S.D. of δ_y, heterogeneity in message subsidy	1.778	0.230
$\sigma_{\phi_x \phi_y}$	covariance of δ_x and δ_y	1.000	0.297

Notes: Estimates of structural parameters and their standard errors. I use classical minimum distance with 15 moments from the experiment to estimate the 15 parameters; see details in the text. Parameters linked to the three key mechanisms are written in bold.

Table 7: Treatment Effects on Opting Out of Message Programs

	Opted Out Ever, Meditation Msgs (x) (1)	Opted Out Ever, Nutrition Msgs (y) (2)
mx & my	-0.029	0.030
	(0.017)	(0.019)
mx-only Mean	0.071	
mx-only SD	0.257	
my-only Mean		0.086
my-only SD		0.281
Obs	1585	1625

Notes: OLS regressions of whether individual ever opted out of each messaging program on treatments. Restricted to individuals with a message treatment. Omitted groups are mx-only (Column 1) and my-only (Column 2). mx & my is 1 if the individual received both sets of messages. The specification includes controls for the five baseline variables on which re-randomization was based (female, college, daily notifications, whether individual meditated in last month, whether individual logged meal in last month). One, two, and three stars indicate q-values of 1%, 5%, and 10% respectively; q-values calculated according to the Benjamini Hochberg step-down procedure, considering all tests in the table.

Table 8: Treatment Effects on Attention to Messages: Reading Rate and Memory of Content

	Raffle Response Meditation (x) (1)	Knowledge Score Meditation (x) (2)	Raffle Response Nutrition (y) (3)	Knowledge Score Nutrition (y) (4)
Panel A: Unconditional				
mx & my	-0.089**	-0.010	-0.081**	-0.008
	(0.028)	(0.006)	(0.028)	(0.007)
mx-only Mean	0.307	0.298		
mx-only SD	0.461	0.086		
my-only Mean			0.263	0.300
my-only SD			0.440	0.100
Obs	998	859	931	847
Panel B: Conditional on Not Opting-Out				
mx & my	-0.087**	-0.005	-0.084**	-0.005
	(0.033)	(0.006)	(0.034)	(0.007)
mx-only Mean	0.356	0.307		
mx-only SD	0.479	0.079		
my-only Mean			0.309	0.308
my-only SD			0.463	0.098
Obs	825	762	771	748

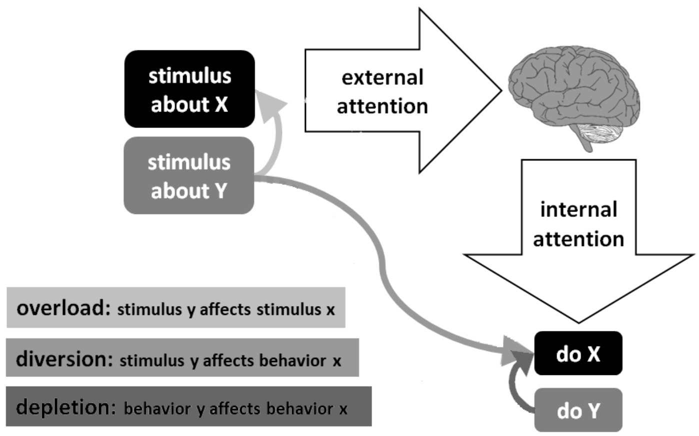
Notes: OLS regressions of whether individual responded to surprise raffle (Columns 1 and 3) and score on knowledge quiz (Columns 2 and 4). *Raffle response* is 1 if the individual responded to the raffle and 0 otherwise. *Score on knowledge quiz* is calculated using answers to a true/false quiz administered in the endline survey on the information provided in the messaging program. Participants received 1 point for every correct answer, 0 point for every incorrect or "I do not remember seeing this message" answer, and 0.5 points for answering "I remember seeing this message but I do not remember the details." The score is the fraction of 14 possible points received. Columns 1 and 3 are restricted to individuals with a message treatment who received a functional raffle message (592 did not due to an implementation error). Columns 2 and 4 are restricted to individuals with a message treatment who took the endline survey. Omitted groups are mx-only (Column 1) and my-only (Column 2). mx & my is 1 if the individual received both sets of messages. The specification includes controls for the five baseline variables on which re-randomization was based (female, college, daily notifications, whether individual meditated in last month, whether individual logged meal in last month). One, two, and three stars indicate q-values of 1%, 5%, and 10% respectively; q-values calculated according to the Benjamini Hochberg step-down procedure, considering all tests in the table.

Table 9: Baseline Expectations of Behaviors by Treatment

	Expected Meditation (x) (1)	Expected - Actual Meditation (x) (2)	Expected Meal Logging (y) (3)	Expected - Actual Meal Logging (y) (4)
mx	0.113***	0.008	-0.017	0.009
	(0.012)	(0.015)	(0.017)	(0.018)
my	0.017	0.053***	0.122***	-0.042**
	(0.012)	(0.013)	(0.014)	(0.018)
mx X my	-0.027	-0.029	0.037	0.052*
	(0.017)	(0.021)	(0.021)	(0.025)
zy	0.021	0.053***	0.213***	-0.223***
	(0.013)	(0.014)	(0.015)	(0.021)
mx + mxmy	0.085	-0.020	0.019	0.061
	(0.012)	(0.014)	(0.013)	(0.018)
my + mxmy	-0.010	0.024	0.159	0.010
	(0.012)	(0.016)	(0.015)	(0.018)
Ctrl Mean	0.384	0.278	0.519	0.382
Ctrl Mean S.E.	(0.009)	(0.010)	(0.012)	(0.012)
Obs	2891	2876	2891	2886

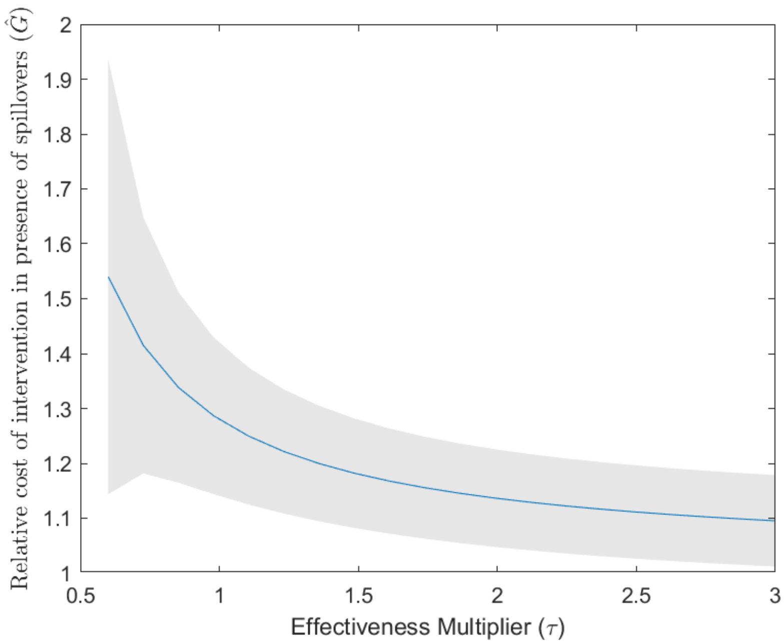
Notes: OLS regressions of expected rates of behavior over treatment period, measured at baseline (Columns 1 and 3) and difference between individual's expected and actual rate (Columns 2 and 4). The specification includes controls for the five baseline variables on which re-randomization was based (female, college, daily notifications, whether individual meditated in last month, whether individual logged meal in last month) as well as day fixed effects. Standard errors clustered at individual level. One, two, and three stars indicate q-values of 1%, 5%, and 10% respectively; q-values calculated according to the Benjamini Hochberg step-down procedure, considering all tests in the table (but excluding linear combinations of coefficients).

Figure 1: Overload, Depletion, and Diversion in a Taxonomy of Limited Attention



Notes: An illustration of the possible ways in which interventions could impose negative spillovers, building on a taxonomy of limited attention from Chun et al. (2011). Suppose there is an intervention, say a text message, about some behavior x, and a second intervention about some behavior y. Limits to external attention, or limited information processing, might cause the y stimulus to interfere with the x stimulus ("overload"). Limits to working or short-term memory might cause the y stimulus to divert attention toward y and away from x, reducing the likelihood of doing x ("diversion"). Finally, if the y stimulus works, causing us to do y and to exert costly cognitive effort or time, we might be subsequently less likely to do x ("depletion").

Figure 2: Quantifying Costs of Spillovers by Intervention Effectiveness



Notes: A depiction of how the costliness of spillovers varies with intervention strength. On the x-axis, I plot a multiplier τ by which I scale ϕ_x and ϕ_y, the strength of the intervention. I allow τ to run from 0.6 (an intervention 60% as strong as the one I ran in the experiment) to 3 (an intervention 3 times as strong as the one I ran in the experiment). (The pattern is the same for interventions with $\tau < 0.6$, but the standard errors become very high, so I exclude this range from the plot.) On the y-axis, I plot $\hat{G}$, which answers the question: if it costs $1 to increase x and y by one standard deviation in the absence of spillovers, how much does it cost in the presence of spillovers? To compute $\hat{G}$, I use my parameter estimates to simulate treatment effects for each level of τ, and plug them into Equations 6 and 7. The shaded region represents standard errors, bootstrapped with 500 samples of 600 observations, stratified by treatment

Appendices

A Mathematical Proofs

A.1 Comparative Statics

$$\frac{\partial a_x^*}{\partial w_x} = \frac{-c_1^y c_3^x + c_2^x c_4^y}{c_1^x c_1^y - c_2^{x2}} \tag{8}$$

$$\frac{\partial a_x^*}{\partial w_y} = \frac{-c_1^y c_4^x + c_2^x c_3^y}{c_1^x c_1^y - c_2^{x2}} \tag{9}$$

$$\frac{\partial a_y^*}{\partial w_y} = \frac{-c_1^x c_3^y + c_2^y c_4^x}{c_1^x c_1^y - c_2^{x2}} \tag{10}$$

$$\frac{\partial a_y^*}{\partial w_x} = \frac{-c_1^x c_4^y + c_2^y c_3^x}{c_1^x c_1^y - c_2^{x2}} \tag{11}$$

$$\begin{aligned}\frac{\partial^2 a_x^*}{\partial m_x \partial m_y} &= \frac{c_1^y c_{34}^x - c_2^x c_{34}^y - c_1^y \left((c_{11}^x \frac{\partial a_x}{\partial m_y} + c_{12}^x \frac{\partial a_y}{\partial m_y}) \frac{\partial a_x}{\partial m_x} + (c_{21}^x \frac{\partial a_x}{\partial m_y} + c_{12}^x \frac{\partial a_y}{\partial m_y}) \frac{\partial a_y}{\partial m_x} \right)}{c_1^x c_1^y - c_2^{x2}} \\ &\approx \frac{c_1^y c_{34}^x - c_2^x c_{34}^y}{c_1^x c_1^y - c_2^{x2}}\end{aligned} \tag{12}$$

$$\begin{aligned}\frac{\partial^2 a_y^*}{\partial m_x \partial m_y} &= \frac{c_1^x c_{34}^y - c_2^x c_{34}^x - c_1^x \left((c_{11}^y \frac{\partial a_y}{\partial m_x} + c_{12}^y \frac{\partial a_x}{\partial m_x}) \frac{\partial a_y}{\partial m_y} + (c_{21}^y \frac{\partial a_y}{\partial m_x} + c_{12}^y \frac{\partial a_x}{\partial m_x}) \frac{\partial a_x}{\partial m_y} \right)}{c_1^x c_1^y - c_2^{x2}} \\ &\approx \frac{c_1^x c_{34}^y - c_2^x c_{34}^x}{c_1^x c_1^y - c_2^{x2}}\end{aligned} \tag{13}$$

A.2 Proof of Proposition 2

Re-write Equations 12 and 13 as the following, incorporating the assumption that $c_{34}^x = c_{34}^y$.

$$\frac{\partial^2 a_x^*}{\partial m_x \partial m_y} \approx \frac{-c_{34}^x(c_1^y - c_2^x)}{c_1^x c_1^y - c_2^{x2}} \tag{14}$$

$$\frac{\partial^2 a_y^*}{\partial m_x \partial m_y} \approx \frac{-c_{34}^x(c_1^x - c_2^x)}{c_1^x c_1^y - c_2^{x2}} \tag{15}$$

Recall that we have assumed $c_1^x > 0$ and $c_1^y > 0$. For the case where $c_2^x \leq 0$, we can quickly see from Equations 14 and 15 that $c_{34}^x = c_{34}^y > 0 \implies$ interference in both directions, and interference in either direction implies $c_{34}^x = c_{34}^y > 0$.

For the case where $c_2^x > 0$, recall that for the existence of a local maximum we have also assumed that $c_1^x c_1^y - c_2^{x2} > 0$ which implies that either $c_1^x > |c_2^x|$ or $c_1^y > |c_2^x|$. Then we can see that if there is interference in both directions ($\frac{\partial^2 a_x^*}{\partial m_x \partial m_y} < 0$ and $\frac{\partial^2 a_y^*}{\partial m_x \partial m_y} < 0$), then it must be true that $c_{34}^x = c_{34}^y > 0$. And if $c_{34}^x = c_{34}^y > 0$, there must be interference in at least one direction ($\frac{\partial^2 a_x^*}{\partial m_x \partial m_y} < 0$ or $\frac{\partial^2 a_y^*}{\partial m_x \partial m_y} < 0$).

A.3 Proof of Proposition 3

If we set $c_4^x = c_4^y = 0$, using Equations 9 and 10, we have the following:

$$\frac{\partial a_x / \partial w_y}{\partial a_y / \partial w_y} = -\frac{c_2^x}{c_1^y} \tag{16}$$

Since neither c_2 nor c_1 vary with the intervention type, this expression remains the same for both messages ($w = m$) and incentives ($w = z$). Thus the ratio between spillover and target effects must be the same for messages about behavior y and incentives for behavior y.

B Experiment Protocol

I recruited participants using the below Facebook ad, targeting people in the U.S. age 18-65 and allowing the algorithm to train to maximize "conversions," or successful completions of Survey 1.

Figure A1: Facebook Ad Used to Recruit Participants

The first part of Survey 1 was an eligibility test in which participants had to verify six things: (1) ownership of iPhone or Android phone; (2) age 18 or over; (3) interested in working on wellness habits like daily meditation and tracking your nutrition; (4) willing to download two (free) wellness-related smartphone apps for the study; (5) comfortable potentially using a nutrition tracking app;[31] (6) have not already participated in the study. Participants then provided electronic consent, which included consenting to receive SMS messages associated with the study. The consent form also described the rewards for participation in the study: entrance into a raffle for a $20 Amazon gift card for participation in Survey 1 and app download; and entrance into a raffle for a $50 Amazon gift card for participation in Survey 2 or Survey 3. (If they completed both they were entered twice.)

[31]We received feedback that some participants who had struggled with eating disorders or body image issues in the past were ultimately uncomfortable using the meal tracking app.

The next part of Survey 1 was the app download. Participants were given two options: to either download the two apps now, or to download them after finishing the survey. Either way, they were told that they had to download both apps within 24 hours of completing the survey in order to be enrolled. They were told that if they downloaded the apps after 24 hours, they could still enroll, but they should email us to let us know. They were then shown a screen with instructions about how to download each app, which were also emailed to them upon survey completion. These instructions included a temporary assigned password, which enabled us to access their data for the duration of the study.

As described in the paper, the rest of the survey included basic demographic questions; questions on past meditation, exercise, meal tracking, and sleep; questions about the full set of notifications, across devices and apps, received by the participant; and questions about the participant's perceived importance, difficulty, and "fun" of meditation, exercise, meal tracking, and sleep. Upon completion of Survey 1, participants were sent an email repeating the instructions for how to download the apps, including their assigned passwords.

Every day, we verified whether new Survey 1 participants for whom 24 hours had elapsed since survey completion (plus old Survey 1 participants who emailed us) downloaded both apps. Those who did were randomized to one of the five treatments, using a script that re-randomized to ensure balance across the full sample. They were then sent an enrollment confirmation email, the key parts of which are displayed in Figure A2.[32]

For example, participants received to receive messages about meditation only were told, "you have been randomly assigned to receive messages about meditation with [app], as part of our Remindful program." The goal is to make clear that the behavior we have in mind is not meditation generally, but meditation specifically with the assinged app. Below the treatment assignment, participants were given a paragraph describing the benefits of each behavior they were assigned treatment for.

The link to Survey 2 was included in the enrollment email. It first reminded participants of their treatment assignment, and then asked them how many days per week they "hoped" and

[32]See Supplementary Materials for the full email.

Figure A2: Enrollment Confirmation Email

Welcome to the Yale Wellness and Technology Study! You successfully downloaded the apps and are officially enrolled. This email contains lots of information about the study. You can refer back to it throughout the study if you have questions.

Here is a brief summary of what it contains:

1. Your (random) assignment to messages or incentives for one or more wellness behaviors. **You were assigned: .** See below for details!
2. The **link to Survey 2**, which will expire in 24 hours. This survey is not mandatory, but takes only a few minutes, and if you participate in time, we'll enter your name into our second raffle (for a $50 Amazon gift card).
3. Survey 1 raffle results
4. A reminder of your password for [redacted]
5. Study duration and how to withdraw

Be well and let us know if you have questions.

Best,
Hannah

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1. Your (Random) Assignment to Messages or Incentives
You have been randomly assigned to receive . [INSERT TREATMENT]

Remember, because this is an experiment, this assignment was completely random. It has nothing to do with your survey responses, or with how important we think meditation, exercise, nutrition, and sleep are. (They're all important!)

Regardless of any programs you were or were not assigned above, your ultimate use of [redacted] is entirely up to you. You are welcome but not obligated to use these apps for the study, so please use them as much or as little as you'd like. The accounts just have to stay active (with the correct email and password) for the duration of the study.

2. Link to Survey 2
Here is the link to Survey 2, which will expire in 24 hours. This survey is not mandatory, but it takes just 3-5 minutes, and if you fill it out, we'll enter your name into our second raffle for a

"expected" to meditate and log their meals using the study apps. Finally, the enrollment email included Survey 1 raffle results and information about the study duration and how to withdraw. It reminded participants that at any time, they can opt out any SMS message program by replying STOP, without withdrawing from the study.

Table A1 shows the full set of possible messages. The first column contains all of the messages received by any participant assigned to m_x, and the second column contains all of the messages received by any participant assigned to m_y. Each message was sent twice throughout the program (except for messages 14 and 28, which were sent just once). The first 14 rows contain the informational messages, and the second 14 rows contain the reminder/encouragement messages. (A participant assigned to, say, m_x received 2 messages per day—one informational, one reminder—over 27 days, so 54 total messages.) As mentioned in the paper, the two daily messages were sent in the morning (either 7am or 8am) and in the evening (either 7pm or 8pm). The timing of meditation vs. nutrition messages and information vs. reminder messages alternated in a balanced fashion as shown in Table 2. Messages were sent using the platform Slicktext.

Table A1: Full Table of Messages

	Meditation	Nutritional Monitoring
1	Evidence from 47 studies suggests that meditation reduces depression and anxiety! (Goyal et al. 2014)	Fact: more than 102 million American adults have high cholesterol, and 35 million are at risk for heart disease as a result (CDC 2013).
2	Did you know that meditation actually changes the physical structures of the brain (Fox et al. 2014)?	Did you know that potassium helps keep your blood pressure low and your heart healthy? The CDC recommends 4700mg of potassium daily for adults age 19-50.
3	Fun fact: for people with insomnia, meditation improves nightly sleep time, and helps people fall asleep faster! (Gross et al 2011)	37.7% of Americans reported that they consume fruits less than once per day! 22.6% report the same for vegetables (CDC 2013). Make sure it's not you!
4	Aetna, a Fortune 500 company, claims that its meditation program made employees more productive, saving $3,000 per employee per year!	90% of Americans consume too much sodium (NHANES 2009-2012), which is a risk factor for heart disease! Many more foods have salt than you might expect!
5	Did you know that meditation programs combat depression almost as effectively as antidepressants? (Kuyken et al. 2008)	Over 15 years, people who consumed >25% of calories as added sugar were twice as likely to die from heart disease as those who consumed <10% (Yang et al. 2014)
6	Did you know that people can use meditation to reduce their physical pain? (Zeidan et al. 2011)	38% of U.S. adults are obese today, relative to 15% in 1980 (NHANES 2013-2014). Log your meals to keep track of your diet!
7	Fun fact: evidence suggests that meditation improves relationship satisfaction! (Sedlmeier et al. 2012)	Logging meals can help with weight loss (Burke et al. 2011)! And people are better at meal-logging when they use apps like [meal tracking app] (Wharton et al. 2014).
8	Meditation programs have been shown to reduce stress levels for people with high blood pressure! (Rainforth et al. 2008)	Less than 3% of Americans meet the daily recommended fiber intake (NHANES 2003-2006). Fiber can lower cholesterol and reduce the risk of heart disease
9	Fun fact: the part of the brain responsible for memory actually looks different in people who meditate! (Fox et al. 2014)	The American Heart Association says daily consumption of added sugar should be <25g for women and <38g for men. Yet the average American consumes 82g daily.
10	Did you know that General Mills runs 7-week meditation programs for its executives? Participants say they work more productively and make better decisions.	A host of studies suggest that nutrition is the most important factor in weight management – much more important than exercise (e.g. Johns et al. 2014).
11	Meditation has so many health benefits that today, 79% of medical schools offer some element of mindfulness training (Buchholz 2015)	Are you eating enough whole grains? Find out! Whole grains reduce the risk of diabetes; refined carbohydrates actually increase the risk! (AIEssa et al. 2015)
12	Did you know that 18.1% of adults in the U.S. experience some type of anxiety disorder? Meditation has proven to help! (Goyal et al. 2014)	Moderately active women between 21-40 should be consuming 2200-2000 calories per day (and men 2600-2800). Do you? Find out by tracking meals with [meal tracking app]!
13	Did you know that 35% of firms had mindfulness classes in 2017, and another 26% are considering them for the future (National Business Group on Health)?	>100 million Americans have diabetes or prediabetes (Nat'l Diabetes Stats Report 2017). Eating whole grains, and reducing sugar & trans fats, reduces the risk
14	Fun fact: meditation increases the thickness of your prefrontal cortex, the area of your brain associated with attention and self-awareness (Fox et al. 2014)	Fact: many companies are having their employees track their nutrition via smartphone apps as part of wellness programs. Jump on the bandwagon!
15	Greetings from Remindful! Try Tara Brach's Vipassana (Basic) meditation on [meditation app]!	Greetings from eNOMerate! Remember to log your meals today with [meal tracking app], if you haven't already!
16	Hello from Remindful! We hope you had a great day. Try Manoj Dias' Basic Breath Meditation on [meditation app]!	Hello from eNOMerate! We hope you had a great day. Take 5 minutes to log your meals with [meal tracking app]!
17	Hope you had a healthy, happy day from Remindful. You'll feel great if you end the day with some meditation! [meditation app] makes it easy.	Hope you had a healthy, happy day from eNOMerate. You'll feel great if you end the day by logging your meals! [meal tracking app] makes it easy.
18	Remindful wishes you a great evening! Remember to take care of yourself, and find a few minutes to meditate with [meditation app].	eNOMerate wishes you a great evening! Remember to take care of yourself, and find a few minutes to log your meals with [meal tracking app]!
19	Good evening from Remindful! You told us you were interested in meditation! So let's get on it. Try something new on [meditation app]!	Good evening from eNOMerate! You told us you were interested in monitoring your nutrition! So let's get on it. [meal tracking app] makes it simple!
20	Hi from Remindful! Are you meditating daily with [meditation app]? Keep the habit up!	Hi from eNOMerate! Are you logging your meals daily with [meal tracking app]? Keep the habit up!
21	Just another friendly hello, and reminder to meditate with [meditation app], from Remindful. Try the 3-minute breathing space by Mark Williams on [meditation app]!	Just another friendly hello, and reminder to log your meals with [meal tracking app], from eNOMerate! ;)
22	Greetings from Remindful! Remember to meditate today with [meditation app], if you haven't already!	Greetings from eNOMerate! Remember to log your meals today with [meal tracking app], if you haven't already!
23	Hello from Remindful! We hope you had a great day. Take 5 minutes to meditate with [meditation app]!	Hello from eNOMerate! We hope you had a great day. Take 5 minutes to log your meals with [meal tracking app]!
24	Hope you had a healthy, happy day from Remindful. You'll feel great if you end the day with some meditation! [meditation app] makes it easy.	Hope you had a healthy, happy day from eNOMerate. You'll feel great if you end the day by logging your meals! [meal tracking app] makes it easy.
25	Remindful wishes you a great evening! Remember to take care of yourself, and find a few minutes to meditate with [meditation app].	eNOMerate wishes you a great evening! Remember to take care of yourself, and find a few minutes to log your meals with [meal tracking app]!
26	Good evening from Remindful! You told us you were interested in meditation! So let's get on it. Try something new on [meditation app]!	Good evening from eNOMerate! You told us you were interested in monitoring your nutrition! So let's get on it. [meal tracking app] makes it simple!
27	Hi from Remindful! Are you meditating daily with [meditation app]? Keep the habit up!	Hi from eNOMerate! Are you logging your meals daily with [meal tracking app]? Keep the habit up!
28	Just another friendly hello, and reminder to meditate with [meditation app], from Remindful! ;)	Just another friendly hello, and reminder to log your meals with [meal tracking app], from eNOMerate! ;)

The incentive treatment was described initially in the enrollment email as the following. "You will earn a green raffle ticket from eNOMerate for every day that you log at least one meal with FatSecret, and a red raffle ticket for every day that you don't. To receive a ticket, you must log a meal on the day that you ate it. Every Sunday, for the duration of the program, we will let you know via email how many tickets you've accumulated. At the end, we will pull one of your tickets, and if it's green, you will win a $10 Amazon gift certificate. So if you log your meals every day, you will definitely get the gift certificate. If you log your meals half of the time, you will get it with 50% odds. And if you never log your meals, you definitely won't get it. (This is separate from the raffles for survey completion.) The program will begin tomorrow and will last exactly 27 days."

Each Sunday, participants in the incentive treatment received an email informing them of the total green and red tickets they had accumulated. At the end of the treatment period, they were sent a final email informing them of their total tickets, and then later sent the results of the raffle. Ultimately 52% of participants won the raffle.

At the end of the treatment period, all participants received an email informing them that any treatment programs they were in would now end, but that they should keep their app accounts intact with their assigned passwords for another four weeks, when they would receive a wrap-up email from us with a link to Survey 3.

After four weeks a final email was sent, concluding the study and providing a link to Survey 3. In Survey 3, we first ask how much they meditated without the assigned apps, about the timing of their meditation, and whether they felt like meditation came at the expense of any other activity. We then do the same for meal logging, with the additional question of how long it took them to log their meals each day. We then ask whether they set up any additional notifications for either behavior. Next, we ask questions about their mental health and diet. Finally, we administer an informational quiz, asking a true/false question about each informational message the participant received. At the end of Survey 3, participants were told to change their passwords for the two apps.

C Attrition and Survey Participation

In total 5,845 people filled out Survey 1, meaning that 66% of Survey 1 participants ultimately downloaded both apps and enrolled in the study. Of the 3,885 participants who enrolled, 40 ultimately dropped out, amounting to 1%, and resulting in a final sample of 3,845. Table A2 shows that there was no evidence of differential attrition by treatment.

Table A2: Attrition Rates by Treatment

	control	mx	my	mx & my	zy	F-test, joint sig
attrited	0.009	0.008	0.011	0.012	0.012	0.870
	0.092	0.088	0.105	0.109	0.110	

Notes: Means and standard deviations. F-test of joint significance reported in last column.

In terms of survey participation, of our 3,845 study participants, 2,891 completed Survey 2 (75.2%), and 2,145 completed Survey 3 (55.8%).

D Moment Conditions

$$M_1 = \frac{\partial E a^*_{xi}}{\partial m_x}\Big|_{m_y=0} = \frac{\alpha\phi_x - \rho\theta_m}{\omega}$$

$$M_2 = \frac{\partial E a^*_{xi}}{\partial m_y}\Big|_{m_x=0} = \frac{\alpha\theta_m - \rho\phi_y}{\omega}$$

$$M_3 = \frac{\partial^2 E a^*_{xi}}{\partial m_x \partial m_y} = \frac{\gamma(\alpha - \rho)}{\omega}$$

$$M_4 = \frac{\partial E a^*_{xi}}{\partial z_y} = \frac{\alpha\theta_z - \lambda\rho}{\omega}$$

$$M_5 = E\big[a^*_{xi}\big|m_x = 0, m_y = 0, z_y = 0\big] = \frac{\alpha\mu_x - \rho}{\omega}$$

$$M_6 = \frac{\partial E a^*_{yi}}{\partial m_x}\Big|_{m_y=0} = \frac{\alpha\theta_m - \rho\phi_x}{\omega}$$

$$M_7 = \frac{\partial E a^*_{yi}}{\partial m_y}\Big|_{m_x=0} = \frac{\alpha\phi_y - \rho\theta_m}{\omega}$$

$$M_8 = \frac{\partial^2 E a^*_{yi}}{\partial m_x \partial m_y} = \frac{\gamma(\alpha - \rho)}{\omega}$$

$$M_9 = \frac{\partial E a^*_{yi}}{\partial z_y} = \frac{\alpha\lambda - \rho\theta_z}{\omega}$$

$$M_{10} = E\big[a^*_{yi}\big|m_x = 0, m_y = 0, z_y = 0\big] = \frac{\alpha - \rho\mu_x}{\omega}$$

$$M_{11} = \frac{\partial Var(a^*_{xi})}{\partial m_x} = \frac{\alpha^2\sigma^2_{\phi_x}}{\omega^2}$$

$$M_{12} = \frac{\partial Var(a^*_{xi})}{\partial m_y} = \frac{\rho^2\sigma^2_{\phi_y}}{\omega^2}$$

$$M_{13} = \frac{\partial^2 Var(a^*_{xi})}{\partial m_x \partial m_y} = \frac{-2\alpha\rho\sigma_{\phi_x\phi_y}}{\omega^2}$$

$$M_{14} = Var(a^*_{xi})\big|_{m_x=0, m_y=0, z_y=0} = \frac{\alpha^2\sigma^2_x + \rho^2\sigma^2_y - 2\alpha\rho\sigma_{xy}}{\omega^2}$$

$$M_{15} = \frac{\partial Var(a^*_{yi})}{\partial m_x} = \frac{\rho^2\sigma^2_{\phi_x}}{\omega^2}$$

$$M_{16} = \frac{\partial Var(a^*_{yi})}{\partial m_y} = \frac{\alpha^2\sigma^2_{\phi_y}}{\omega^2}$$

$$M_{17} = \frac{\partial^2 Var(a^*_{yi})}{\partial m_x \partial m_y} = \frac{-2\alpha\rho\sigma_{\phi_x\phi_y}}{\omega^2}$$

$$M_{18} = Var(a^*_{yi})\big|_{m_x=0,m_y=0,z_y=0} = \frac{\alpha^2\sigma_y^2 + \rho^2\sigma_x^2 - 2\alpha\rho\sigma_{xy}}{\omega^2}$$

$$M_{19} = \frac{\partial Cov(a^*_{xi}, a^*_{yi})}{\partial m_x} = \frac{-\alpha\rho\sigma^2_{\phi_x}}{\omega^2}$$

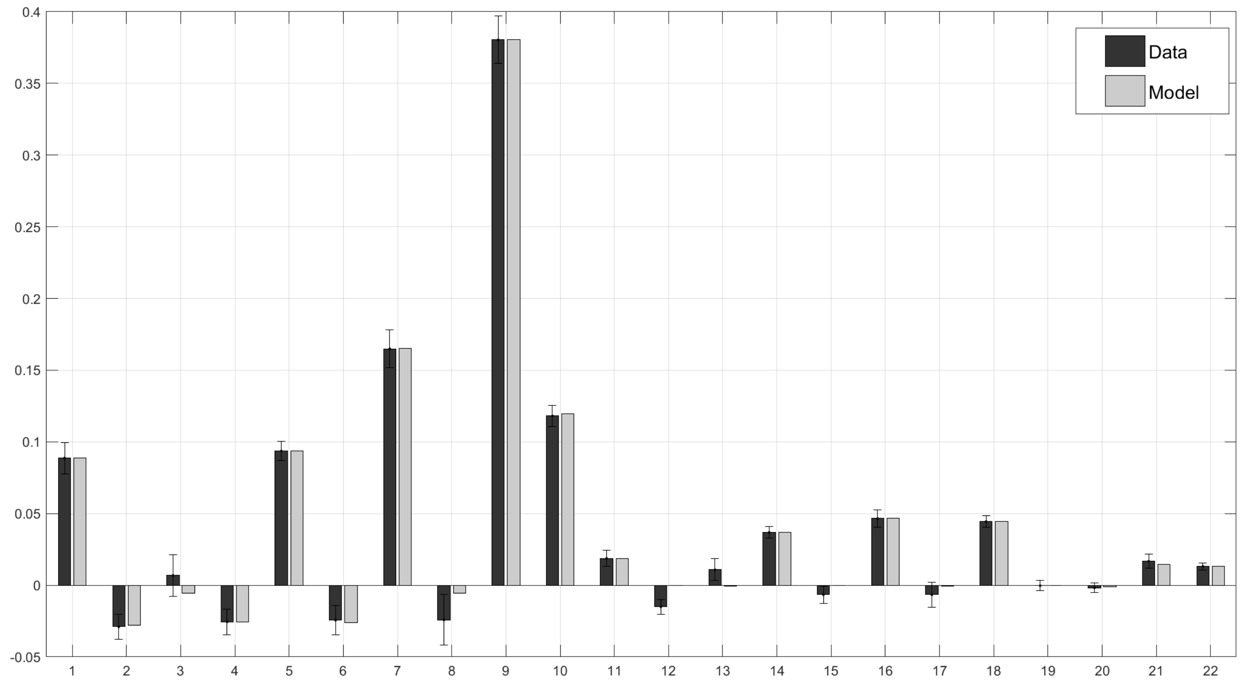
$$M_{20} = \frac{\partial Cov(a^*_{xi}, a^*_{yi})}{\partial m_y} = \frac{-\alpha\rho\sigma^2_{\phi_y}}{\omega^2}$$

$$M_{21} = \frac{\partial^2 Cov(a^*_{xi}, a^*_{yi})}{\partial m_x \partial m_y} = \frac{(\alpha^2 + \rho^2)\sigma_{\phi_x\phi_y}}{\omega^2}$$

$$M_{22} = Cov(a^*_{xi}, a^*_{yi})\big|_{m_x=0,m_y=0,z_y=0} = \frac{-\rho(\alpha\sigma_x^2 + \alpha\sigma_y^2) + (\alpha\alpha + \rho^2)\sigma_{xy}}{\omega^2}$$

E Estimation Robustness Checks and Sensitivity

Figure A3: Model Fit



Notes: Comparison of actual moments to predicted moments.

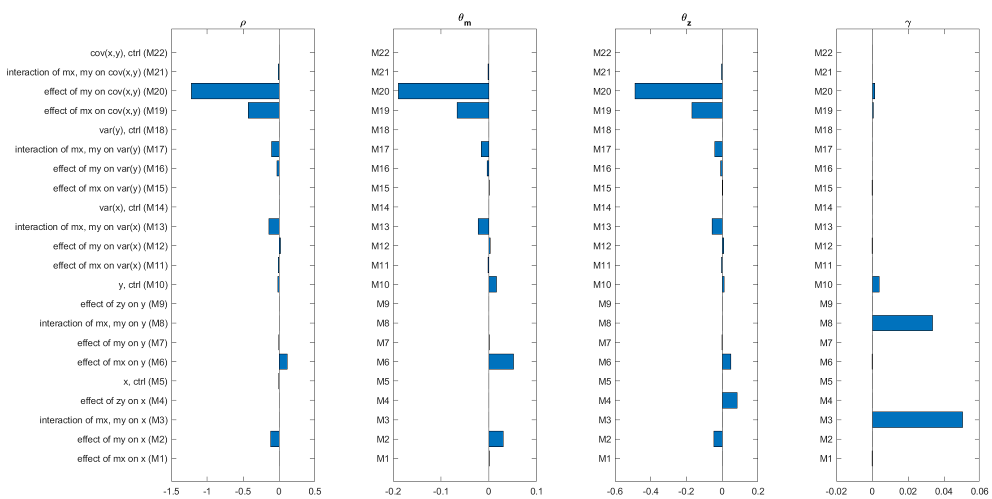
In Figure A3 I illustrate the model fit by comparing the moments in the data to those predicted by the model. In Table A3 I show parameter estimates using the identity matrix as the weighing matrix, instead of the diagonal of the inverse of the variance-covariance matrix as in the benchmark specification. In Figure A4 I plot the sensitivity matrix, as defined by Andrews et al. (2017), showing only the four key parameters. Each element of the matrix represents how a one percentage point increase in each moment affects each estimated parameter. We can see that ρ is most sensitive to the treatment effects of messages on the covariance, as well as to the main target effects, spillover effects, and interaction effects. As we would expect, θ_m and θ_z are sensitive to the same moments as ρ, but mostly in the opposite direction, since spillovers must be due to either diversion or depletion. (The bars go in the same direction because depletion is captured by $\rho > 0$ but diversion is captured by $\theta < 0$.) The only major exception is the effect of m_y on x, which pushes

Table A3: Estimates using Identity Matrix

Parameter	*Description*	*Estimate*	*Standard Error*
α	slope of marginal cost of effort	8.301	0.574
$\boldsymbol{\rho}$	**depletion**	**0.159**	**0.711**
μ_x	return to x	0.796	0.080
ϕ_x	x message attn subsidy	0.732	0.110
$\boldsymbol{\gamma}$	**overload**	**-0.073**	**0.113**
$\boldsymbol{\theta_m}$	**message diversion**	**-0.202**	**0.133**
λ	y incentive attn subsidy	3.154	0.289
ϕ_y	y message attn subsidy	1.365	0.163
σ_x	S.D. of ϵ_x, heterogeneity in return to x	1.605	0.101
σ_y	S.D. of ϵ_y, heterogeneity in return to y	1.760	0.078
$\boldsymbol{\theta_z}$	**incentive diversion**	**-0.152**	**0.281**
σ_{xy}	covariance of ϵ_x and ϵ_y	0.992	0.439
σ_{ϕ_x}	S.D. of δ_x, heterogeneity in message subsidy	1.136	0.193
σ_{ϕ_y}	S.D. of δ_y, heterogeneity in message subsidy	1.791	0.185
$\sigma_{\phi_x\phi_y}$	covariance of δ_x and δ_y	1.000	0.362

Notes: Parameter estimates using the identity matrix as the weighing matrix, instead of the diagonal of the inverse of the variance-covariance matrix as in the benchmark specification.

Figure A4: Sensitivity of Estimates to Moments

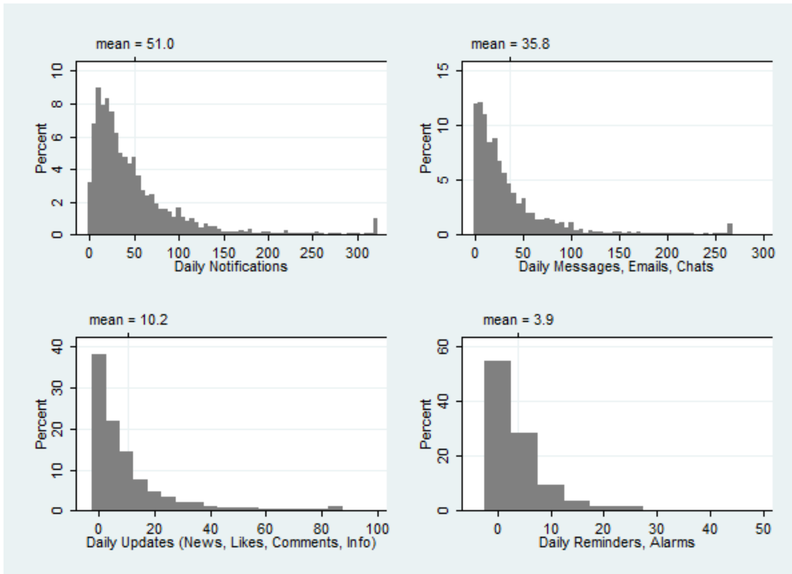


Matrix depicting sensitivity of parameter estimates to moments, as defined by Andrews et al. (2017), showing only the four key parameters. Each element of the matrix represents how a one percentage point increase in each moment affects each estimated parameter.

both θ_m and ρ toward zero. This is intuitive: the closer this spillover gets to zero, the smaller are both estimates of diversion and depletion. The effect of m_x on y pushes them in opposite directions (θ_m toward zero; ρ away from zero) because this moment is being used to separate ρ from θ_m: the nearer to zero is this spillover, the smaller is the spillover-target ratio for x, bringing it closer to the spillover-target ratio for y and thus making ρ larger. As would be expected. γ is most sensitive to the message interaction moments, though it is sensitive to all of the other moments through ρ and α.

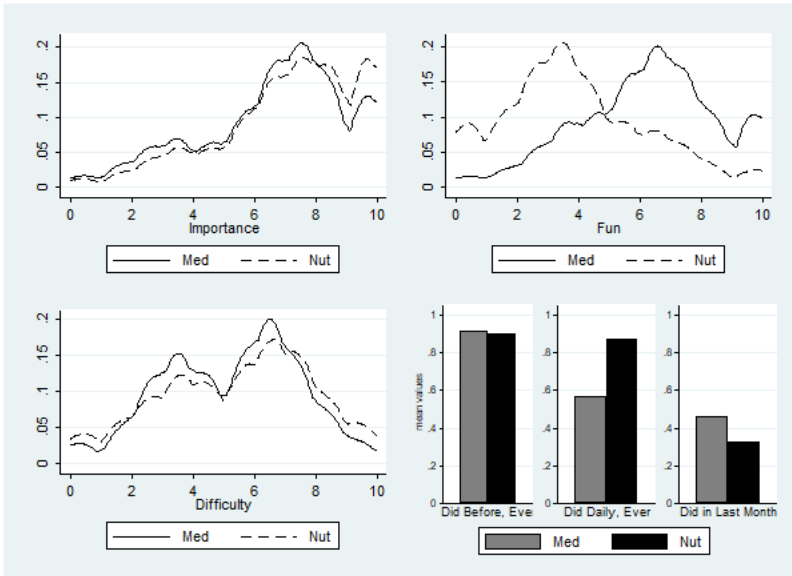
F Additional Data & Analysis

Figure A5: Daily Notifications (after winsorizing at 99%)



Notes: The distribution of daily notifications, as self-reported in the baseline survey. Participants were asked to list all apps that send notifications across all devices, and then to estimate daily notifications for each app. The top-left plot shows total notifications, and the subsequent plots break notifications down by type.

Figure A6: Preferences and Experience, Meditation & Meal Logging



Notes: The distribution of baseline responses to questions about self-reported importance, fun, and difficulty of each behavior, on a scale from 1 to 10. The most notable difference between the two behaviors is that participants believe that meditation will be more "fun" than meal logging. In the bottom-right plot I depict the self-reported experience with each behavior. The first comparison shows the fraction of participants who ever did the behavior before, the second shows the fraction of participants who ever did the behavior *daily* before, and the third shows the fraction of participants who did the behavior in the last month.

Figure A7: Meditation Messages vs. Control

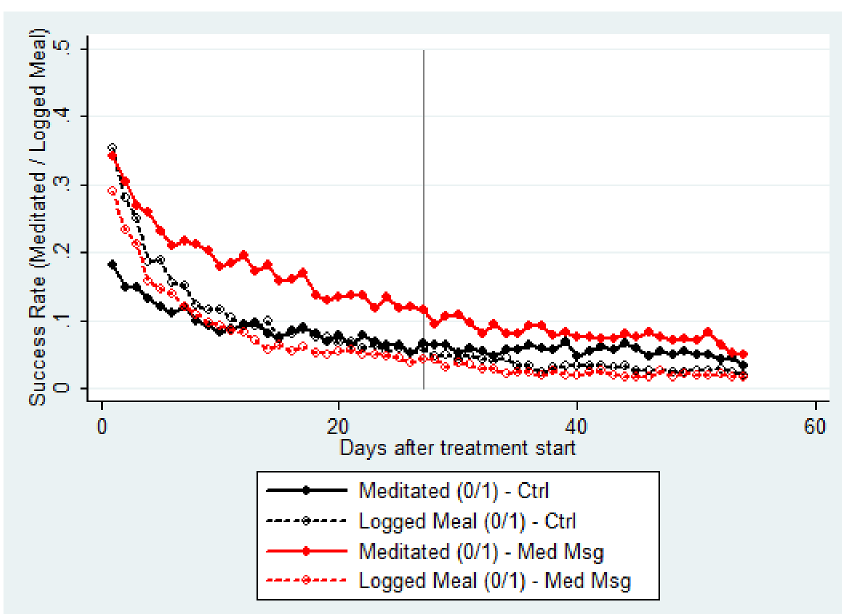


Figure A8: Nutrition Messages vs. Control

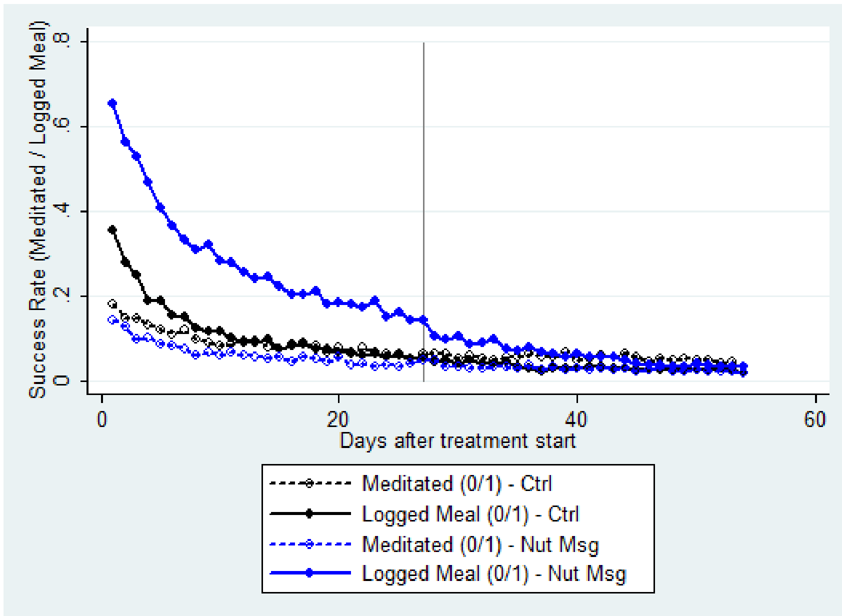


Figure A9: Meditation + Nutrition Messages vs. Meditation Messages Only

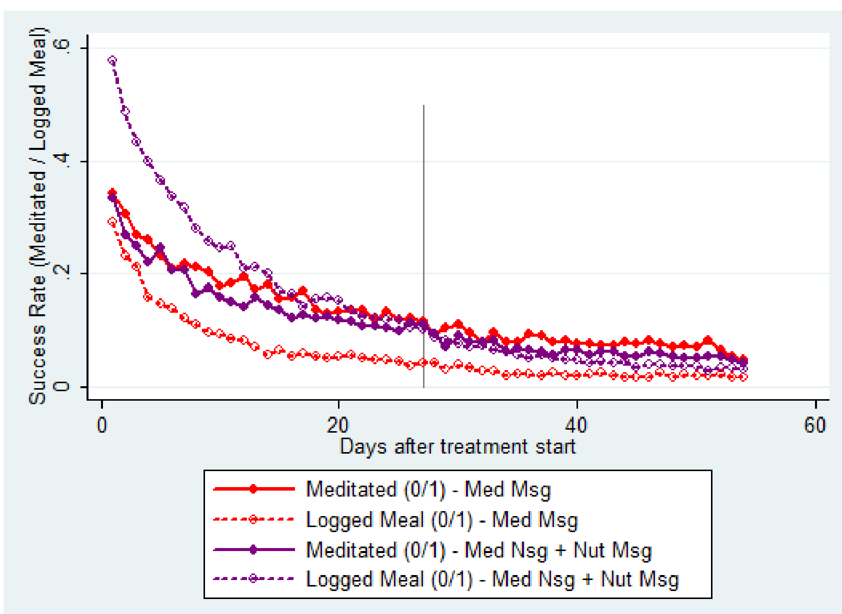


Figure A10: Meditation + Nutrition Messages vs. Nutrition Messages Only

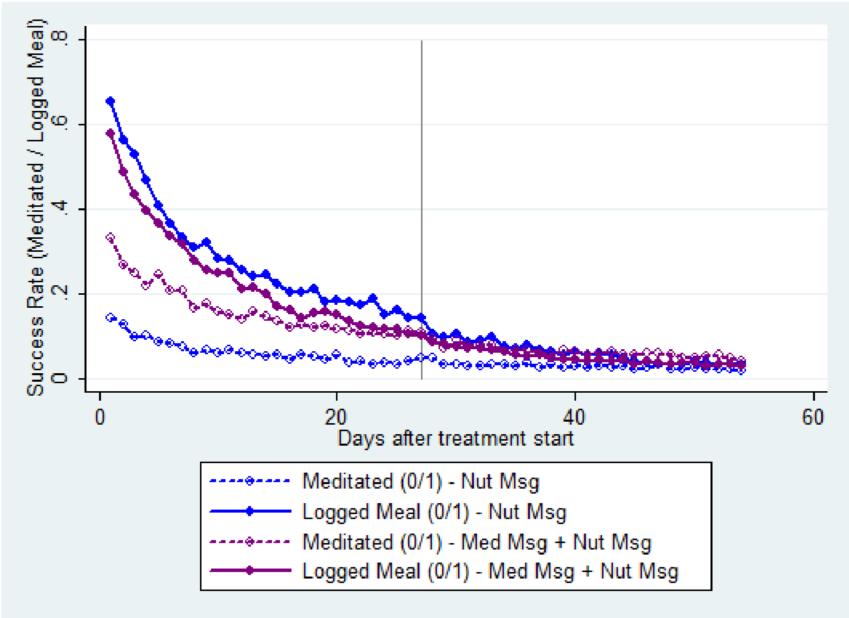


Figure A11: Nutrition Incentives vs. Control

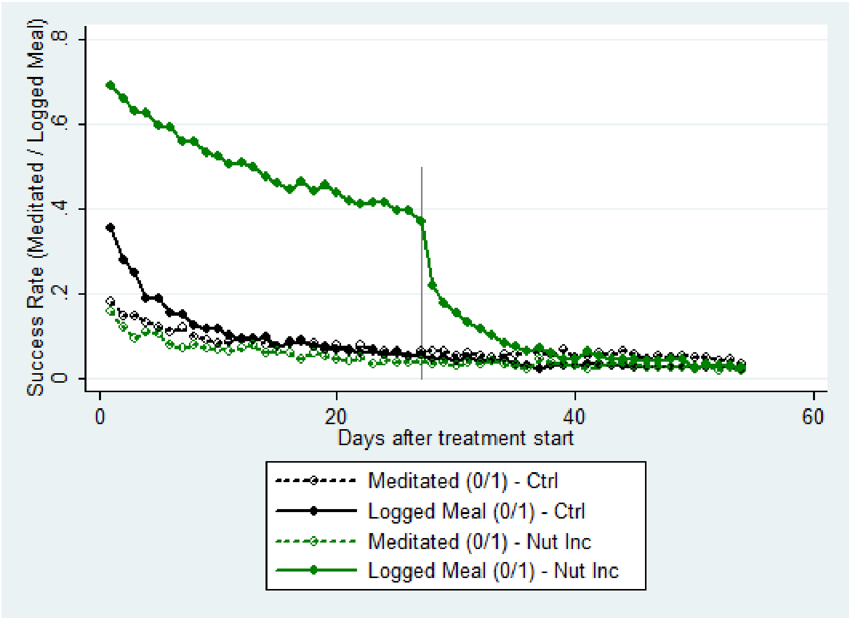


Table A4: Reduced Form Results Reported as Treatment Effects

	Treatment Period		*Post-Treatment Period*	
	Meditated (X) (1)	Logged Meal (Y) (2)	Meditated (X) (3)	Logged Meal (Y) (4)
mx only	0.088***	-0.024**	0.024***	-0.010*
	(0.011)	(0.010)	(0.009)	(0.006)
my only	-0.028***	0.166***	-0.025***	0.029***
	(0.008)	(0.013)	(0.007)	(0.008)
mx & my	0.066***	0.116***	0.009	0.017**
	(0.010)	(0.012)	(0.008)	(0.007)
zy	-0.025***	0.381***	-0.024***	0.037***
	(0.009)	(0.016)	(0.007)	(0.008)
mx only - mx & my	0.022	-0.140	0.016	-0.027
	(0.011)	(0.012)	(0.009)	(0.007)
my only - mx & my	-0.094	0.050	-0.034	0.013
	(0.009)	(0.014)	(0.007)	(0.008)
Ctrl Mean	0.094	0.118	0.054	0.033
Ctrl SD	0.291	0.323	0.227	0.178
Obs	102905	102905	102499	102499

Notes: OLS regressions with estimates for the four treatment groups as defined in the experiment design, rather than treatments and interactions. Apart from this, the specification is identical to that of Table 4.

Table A5: Reduced Form Results, Including Use of Other Apps

	Treatment Period		*Post-Treatment Period*	
	Meditated (x) (1)	Logged Meal (y) (2)	Meditated (x) (3)	Logged Meal (y) (4)
mx	0.242***	-0.032	0.028*	-0.005
	(0.032)	(0.037)	(0.014)	(0.010)
my	-0.027	0.397***	-0.014	0.059***
	(0.029)	(0.036)	(0.011)	(0.013)
mx X my	0.023	-0.084	0.015	-0.013
	(0.046)	(0.051)	(0.019)	(0.018)
zy	-0.011	0.450***	-0.029**	0.172***
	(0.030)	(0.036)	(0.011)	(0.018)
mx + mxmy	0.265	-0.116	0.043	-0.018
	(0.034)	(0.036)	(0.013)	(0.015)
my + mxmy	-0.003	0.313	0.001	0.046
	(0.036)	(0.037)	(0.015)	(0.012)
Ctrl Mean	0.346	0.557	0.064	0.047
Ctrl SD	0.460	0.574	0.245	0.211
Obs	2131	2119	3805	3805

Notes: OLS regressions of an individual-level outcome variable that takes into account the use of other meditation and meal logging apps on treatments. At the final survey, we ask participants how many days they did the behaviors using other apps during the treatment and post-treatment period. I inflate mean meditation and meal logging rates for the duration of the period according to the number of days in which other apps were reported to be used. The specification includes controls for the five baseline variables on which re-randomization was based (female, college, daily notifications, whether individual meditated in last month, whether individual logged meal in last month). One, two, and three stars indicate q-values of 1%, 5%, and 10% respectively; q-values calculated according to the Benjamini Hochberg step-down procedure, considering all tests in the table (but excluding linear combinations of coefficients).

Table A6: Treatment Effects on Health Outcomes

	Avg Daily Min Meditated (1)	Standardized PHQ4 Score (2)	Standardized M. Health Score (3)	Fraction Weight Goal Achieved (4)	Standardized Diet Score (5)
mx	0.778**	-0.010	0.079	-0.060	0.103
	(0.288)	(0.065)	(0.070)	(0.057)	(0.065)
my	-0.861***	-0.007	0.100	0.012	0.224***
	(0.225)	(0.066)	(0.067)	(0.065)	(0.070)
mx X my	0.528	-0.069	0.006	0.003	-0.120
	(0.364)	(0.092)	(0.101)	(0.078)	(0.097)
zy	-0.818***	0.006	-0.058	-0.019	0.302***
	(0.234)	(0.068)	(0.066)	(0.055)	(0.072)
mx + mxmy	1.306	-0.079	0.085	-0.057	-0.017
	(0.222)	(0.066)	(0.072)	(0.054)	(0.072)
my + mxmy	-0.333	-0.076	0.106	0.015	0.104
	(0.286)	(0.065)	(0.075)	(0.037)	(0.068)
Ctrl Mean	1.800	0.000	0.000	0.132	0.000
Ctrl Mean S.D.	5.633	1.000	1.000	0.964	1.000
Obs	3826	2145	2141	1651	2142

Notes: Health outcomes, including (1) average daily minutes meditated; (2) standardized score from the PHQ4, a four-item anxiety and depression questionnaire; (3) standardized response to "How would you describe your mental health now, relative to before you started the study?"; (4) fraction of weight goal achieved (self-reported), and (5) standardized response to "How would you describe your diet now, relative to before you started the study?" Regressions include controls for the five baseline variables on which re-randomization was based. One, two, and three stars indicate q-values of 1%, 5%, and 10% respectively; q-values calculated according to the Benjamini Hochberg step-down procedure, considering all tests in the table (but excluding linear combinations of coefficients).

G Heterogeneity

I look at two potential sources of heterogeneity in spillover effects. The first is by baseline notifications. Since the vast majority (92%) of notifications received by our sample participants are messages or updates, and not associated with a particular behavior, I simply modify the model to include a new type of message, m_w, but do not allow for any action associated with w (i.e. a_w). I make the same assumptions as before. The components of the cost function become: $f(a_x, a_y) = \frac{1}{2}\alpha(a_x^2 + a_y^2) + \rho a_x a_y$ and $s^x(m_x, z_x, m_y, z_y, m_w, z_w) = \nu_x m_x + \gamma m_x m_y + \gamma m_x m_w + \lambda z_x + \theta_m m_y + \theta_m m_w + \theta_z z_y$. I find the effect of m_w on spillovers to be:

$$\frac{\partial a_x}{\partial m_y}|_{m_w=1} - \frac{\partial a_x}{\partial m_y}|_{m_w=0} = \frac{-\rho\gamma}{\alpha^2 - \rho^2} \tag{17}$$

In the presence of both depletion ($\rho > 0$) and overload ($\gamma < 0$), notifications are predicted to push spillovers of m_y on x toward zero. The intuition is that distracting messages m_w interfere with m_y due to overload, causing them to generate less depletion, and thus less of a spillover, than they otherwise would. Since I found no strong evidence of either depletion or overload, I should not expect to see strong heterogeneous effects by baseline notifications. I do not attempt to predict the continuous effect of additional notifications, which would require many more assumptions.

The most accurate test of the above prediction would be to look at people who have at least one other daily notification versus people who do not. Unfortunately only 12 participants have fewer than one other daily notification, and even cutting the data in half does not provide satisfactory power to detect small effects. Instead I opt for the test with the highest power, interacting each treatment with whether participants have notifications that are above or below the median. Table A7 shows the results. As expected, there is limited evidence of heterogeneity, though for all of the above reasons this is by no means a conclusive test.

I do not attempt to use the model to predict heterogeneity by baseline experience, as it is not clear how experience should enter the model. It likely reflects both preferences, which would enter through u, as well as accumulated habits, which would likely affect the cost of attention. Instead, I try to run the simplest possible test of heterogeneity by baseline experience, in the hope

Table A7: Heterogeneous Treatment Effects by Baseline Notifications

	Meditated (x) (1)	Logged Meal (y) (2)
mx	0.099***	-0.032*
	(0.016)	(0.015)
my	-0.043***	0.167***
	(0.012)	(0.019)
mx X my	0.012	-0.005
	(0.021)	(0.026)
zy	-0.028*	0.397***
	(0.013)	(0.024)
highnotif	-0.013	-0.012
	(0.014)	(0.015)
mx X highnotif	-0.024	0.017
	(0.021)	(0.021)
my X highnotif	0.031	-0.002
	(0.017)	(0.026)
mx X my X highnotif	-0.010	-0.041
	(0.028)	(0.035)
zy X highnotif	0.005	-0.031
	(0.018)	(0.033)
mx + mxmy	0.111	-0.038
	(0.013)	(0.021)
my + mxmy	-0.032	0.162
	(0.017)	(0.018)
(mx + mxmy) X highnotif	-0.030	-0.020
	(0.020)	(0.030)
(my + mxmy) X highnotif	0.020	-0.040
	(0.020)	(0.020)
Ctrl Mean	0.094	0.118
Ctrl SD	0.291	0.323
Obs	102905	102905

Notes: OLS regressions of treatment-period behaviors on treatments and interactions with a binary measure of whether daily notifications are above or below the median. Includes controls for the five baseline variables on which re-randomization was based (female, college, daily notifications, whether individual meditated in last month, whether individual logged meal in last month) as well as day fixed effects. Standard errors clustered at individual level. One, two, and three stars indicate q-values of 1%, 5%, and 10% respectively; q-values calculated according to the Benjamini Hochberg step-down procedure, considering all tests in the table (but excluding linear combinations of coefficients).

that it might provide insight for future models.

I construct an experience score in which the participant gets 1 point if he/she has ever done the behavior, another point if he/she has attempted to do it daily before, and another point if he/she has done it in the last month, for a minimum score of zero and a maximum of three. Table A8 shows the results by whether participations are above or below the median in their experience with the outcome behavior in question. (In Column 1, experience represents meditation experience; in Column 2, experience represents meal logging experience.) I find no evidence of heterogeneity by experience, but again, the data is under-powered to detect small effects.

Table A8: Heterogeneous Effects by Baseline Experience in Outcome Behavior

	Meditated (x) (1)	Logged Meal (y) (2)
mx	0.093***	-0.021
	(0.021)	(0.026)
my	-0.002	0.133***
	(0.014)	(0.030)
mx X my	-0.045	0.015
	(0.027)	(0.044)
zy	-0.009	0.358***
	(0.015)	(0.045)
experience	-0.008	0.004
	(0.011)	(0.028)
mx X experience	-0.003	-0.001
	(0.011)	(0.012)
my X experience	-0.013	0.016
	(0.008)	(0.014)
mx X my X experience	0.026	-0.020
	(0.015)	(0.020)
zy X experience	-0.008	0.011
	(0.009)	(0.020)
mx + mxmy	0.048	-0.006
	(0.017)	(0.036)
my + mxmy	-0.047	0.148
	(0.023)	(0.033)
(mx + mxmy) X experience	0.020	-0.020
	(0.010)	(0.020)
(my + mxmy) X experience	0.010	0.000
	(0.010)	(0.010)
Ctrl Mean	0.094	0.118
Ctrl SD	0.291	0.323
Obs	102905	102905

Notes: OLS regressions of treatment-period behaviors on treatments and interactions with a binary measure of whether baseline experience in the outcome behavior was above or below the median. The experience measure went from 0 to 3, where participants earned 1 point for having ever done it before, 1 point for having done it daily before, and 1 point for having done it in the last month. Includes controls for the five baseline variables on which re-randomization was based (female, college, daily notifications, whether individual meditated in last month, whether individual logged meal in last month) as well as day fixed effects. Standard errors clustered at individual level. One, two, and three stars indicate q-values of 1%, 5%, and 10% respectively; q-values calculated according to the Benjamini Hochberg step-down procedure, considering all tests in the table (but excluding linear combinations of coefficients).

H Deviations from Pre-Analysis Plan

This study was registered at the AEA RCT Registry under the title "Nudges in Equilibrium" with RCT ID AEARCTR-0002435. In this section I describe any differences between the final paper and the pre-analysis plan.

Importantly, the main experiment design and sample size did not change substantially between the pre-analysis plan and the experiment. Slight differences in sample size across treatment groups are due to the fact that we randomized within cohorts using fixed proportions, and did not have full control over the total numbers. The slight rise in the total sample is also due to being unable to exactly control the size of the final cohort.

In terms of the analysis, the reduced form specifications are the same. One important difference is that ultimately we used meditation and meal logging *with* the assigned apps as the outcome in our main specification, rather than incorporating self-reports of meditation and meal-logging with other apps, as planned. The reason for this is twofold. First, it was actually a mistake to plan to incorporate self-reports, because the behavior we promoted in both message and incentive treatment was the behavior using the specified app, not the behavior generally. Second, ultimately only 56% of participants completed Survey 3—much less than hoped—so incorporating self-reports from this survey reduced our power significantly. Table A5 shows the results of the specification stated in the pre-analysis plan. The key coefficients of interest are not substantially different, but there is insufficient power to draw the same conclusions.

I do not include in the paper all of the sub-group analyses as described in the pre-analysis plan, since they are generally insufficiently powered. I also changed the measurement tool for mental health, substituting the PHQ4 for the General Well-Being Schedule, since feedback from the first participants suggested that the 18-item questionnaire was too time-consuming, and was reducing the likelihood of completion.

Finally, the model changed in several ways since the pre-analysis plan. The most important change was to ultimately include three mechanisms (overload, diversion, and depletion), instead of just two (overload and depletion, originally called limited external and internal attention). I made this change because after getting the data, I realized that the original model simply did not fit because it did not include diversion, which is what I ultimately find to be the main driver of spillovers. As a result of this change, many smaller, subsequent changes had to be made to the model as well, which explains the rest of the differences.