How Do Habits Form? Experimental Evidence from Health Screenings*

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Abstract

We develop a framework to identify the mechanisms of habit formation and apply it to experimental data on annual health screenings from a three-year study of 4,799 university employees. We find strong habit formation from initial exposure: completing the first screening raised subsequent screening rates by 32.4–36.0 percentage points (84%–90%) in the second and third years. In contrast, completing a second screening had minimal effect on subsequent screenings. This pattern contradicts reinforcement mechanisms like addiction, where recent consumption drives the habit, and instead supports an experience-good model where consumers learn the value of screening through first exposure. Experimental and observational estimates yield markedly different conclusions, underscoring the value of research design for shaping inferences about the sources of habit formation.

Keywords: habit formation, wellness programs, health screenings, incentives

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1 Introduction

Habits, which emerge when past consumption increases current consumption, shape many social, economic, and health behaviors (Pollak, 1970; Becker, 1992). Distinguishing habit formation from other sources of behavioral persistence, such as fixed preferences or persistent economic conditions, poses a significant empirical challenge. Existing studies often address this challenge by leveraging exogenous variation in past behavior, such as through randomized financial incentives (e.g., Carrera et al., 2020; Hussam et al., 2022). However, these studies are typically not designed to disentangle the underlying mechanisms of habit formation. As a result, the observed habit is often attributed to a specific model, such as addiction or learning, without directly testing competing explanations.

While habit formation can be modeled in many different ways, a central distinction is whether habits are driven more by recent past consumption or by initial consumption. For example, addictive habits like cigarette smoking depend strongly on recent consumption, whereas developing a taste for mangoes might require only a single initial experience to establish a lasting habit. Understanding the specific mechanisms through which habits form and persist is crucial for designing effective taxes and incentives (Becker, Grossman and Murphy, 1994; Hintermann and Lange, 2013).

Our study develops a framework to distinguish between two broad classes of habit formation models: *reinforcement*, in which habits are primarily driven by recent consumption, and *initial exposure*, in which they depend mainly on initial consumption. The reinforcement class includes frameworks such as rational addiction (Becker and Murphy, 1988) and transaction costs (Klemperer, 1987), while the initial exposure class includes experience-good and learning models (Dupas, 2014; Banerjee et al., 2021). In a two-period setting, these classes are observationally equivalent, since initial and most recent consumption are one and the same. We show, however, that in settings with more than two time periods these two classes can be distinguished empirically, since initial and subsequent consumption can be independently identified and their relative effects on current demand directly compared.[1]

[1] A large literature has examined whether specific habit formation mechanisms—most notably addiction—reflect forward-looking behavior, i.e., whether consumers anticipate future consequences when making current decisions. While not the focus of our paper, we note that identifying forward-looking behavior also requires variation across multiple time periods to distinguish anticipatory responses from myopic ones.

We use our framework to study habit formation in annual biometric health screenings, a widely promoted health behavior encouraged by employers and health insurance programs.[2] As part of a large-scale randomized controlled trial, we offered individual-level financial incentives to 3,275 university employees ("Wave 1") each year for three years (2016–2018), paying those who completed a health screening. Incentives were rerandomized and announced at the start of each program year, generating variation across both initial and subsequent participation. We use this design to perform two complementary analyses. First, we conduct a reduced-form analysis that estimates contemporaneous ("direct") effects of incentives on screening completion as well as their persistence effects up to two years later. Second, we use the randomized incentives as instruments to estimate the causal effect of past screening completion on current completion. Because three years of randomization provide variation in both initial and recent screening behaviors, we can directly compare their relative influence on current screening completion. We further validate our findings among a second cohort of 1,524 employees ("Wave 2") who received randomized incentives for two years (2017–2018). The staggered and overlapping design of the two waves enables within-year comparisons of individuals with and without prior exposure, providing direct evidence on the role of initial versus recent screening experience in habit formation.

We find that all financial incentives—the initial, the second, and the third—had large immediate effects, boosting contemporaneous screening rates by 12.4–20.3 percentage points. The initial incentive also produced persistent effects in both waves, boosting future screening rates by 4.5–8.9 percentage points (10–24%) one year later and 4.4 percentage points (12%) two years later. By contrast, the second incentive showed a small and statistically insignificant persistence effect. In other words, screening in the third year was affected by the initial incentives in year one, but not by the more recent incentive in year two.

Next, we use our randomized incentives as instruments to estimate the causal effect of past screening completion on current screening completion.[3] We find strong evidence of habit

[2]To our knowledge, no prior study has examined habit formation in the context of biometric health screenings. Related work has documented habit formation in health behaviors such as exercise, bed net usage, and insurance enrollment, where it has been attributed to addiction-like dynamics or experience-good mechanisms (Charness and Gneezy, 2009; Dupas, 2014; Banerjee et al., 2021).

[3]Our definition of habit formation is consistent with Becker (1992) and Royer, Stehr and Sydnor (2015). It encompasses a variety of mechanisms, including addiction, switching costs, learning, and taste discovery.

formation from initial participation. Completing the first screening raised the likelihood of screening completion one year later by 32.4–36.0 percentage points (84–89%) in Waves 1 and 2, and raised completion two years later by 33.1 percentage points (90%) in Wave 1. The consistency of the one-year estimates across waves underscores the robustness of the first-time screening effect, while its stability over a two-year horizon indicates that the effect does not quickly decay. By contrast, completing the second screening raised third-year completion rates by only 6.3 percentage points, a small and statistically insignificant effect.

These patterns distinguish between the two classes of habit formation models introduced earlier. Our IV estimates show that the more recent second-year screening had a significantly smaller effect on Wave 1's likelihood of completing the third screening than the more distant first-year screening, rejecting a core prediction of the reinforcement class. Instead, the findings align with several predictions of the initial-exposure class of models: conditional on having been screened once, being screened twice does not further increase the probability of future participation; and the impact of completing a first screening is nearly identical whether it occurred one or two years earlier.

Finally, we demonstrate how understanding the mechanisms of habit formation can inform the design of cost-effective interventions. Consider the initial incentive to complete a screening in the first year of Wave 1. Ignoring habit formation, one would conclude that the incentive increased completion rates by 12.4 percentage points for a single year. Accounting for habit formation, however, increases the total effect to 21.3 percentage points over the two-year program period—72% higher than the one-year estimate. Notably, this habit formation effect is confined to the first "dose" of the intervention. The second dose increased contemporaneous completion rates by 20.3 percentage points but had no detectable effect on subsequent screenings. In other words, habit formation enhanced the cost-effectiveness of the first incentive but not the second. This finding contrasts with predictions from models such as addiction, where reinforcement would amplify the long-run effect of both incentives. More broadly, these results highlight the importance of not only identifying habit formation but also distinguishing among its underlying mechanisms when designing interventions.

Our study builds on a large literature documenting habit formation across a wide range of behaviors, including smoking, voting, energy conservation, and social-media use. A cen-

tral challenge in this work has been distinguishing habit formation from other sources of behavioral persistence (Brandon et al., 2017; Moore and Morris, 2024). Most studies assume rather than identify the underlying mechanism. (Hussam et al., 2022), for instance, interpret persistence in handwashing as an addiction but acknowledge that it may also reflect learning. Other studies have attributed habit formation to reinforcement (Becker, Grossman and Murphy, 1994; Gruber and Köszegi, 2001; Allcott and Rogers, 2014; Fujiwara, Meng and Vogl, 2016; Allcott, Gentzkow and Song, 2022), inertia (Handel, 2013), environmental cues (Giuntella, Saccardo and Sadoff, 2024), start-up costs (Carrera et al., 2020), switching costs (Polyakova, 2016), and learning through experience (Ackerberg, 2003; Dupas, 2014; Banerjee et al., 2021). A smaller set of studies has attempted to distinguish between different consumer learning mechanisms using structural methods (Ackerberg, 2001; Osborne, 2011).

Our paper advances this literature by introducing a randomized framework to distinguish two broad classes of habit-formation mechanisms, reinforcement and initial exposure, which encompass common models such as addiction and experience goods. A key innovation is the use of multiple rounds of rerandomized incentives to isolate the different persistence effects of repeated treatments. This design allows us to identify habit formation—the casual effect of past consumption on current consumption—while ruling out alternatives such as inattention or serially correlation preferences. It also enables a direct comparison of the effects of initial versus subsequent consumption on current demand, thereby distinguishing between the two mechanism classes. Beyond our specific setting, this experimental framework provides a general template for studying other models of behavioral persistence. For example, in industrial organization, switching costs and learning are often cited as important determinants of brand choice (Ackerberg, 2003; Farrell, 2007). In a typical switching cost model, current consumption depends on the immediately preceding period's consumption, whereas in an experience-good model, it depends on earlier consumption when learning is fast. By employing multiple rounds of rerandomized incentives, researchers can empirically separate these mechanisms without relying exclusively on structural assumptions.

We also contribute to the literature on habit formation in health behaviors, which has predominantly focused on high-frequency activities such as exercising.[4] A pioneering study

[4]Even outside the realm of health, few studies have examined habit formation in infrequent behaviors. A

by Charness and Gneezy (2009) finds that financial incentives for attending the gym for one month increased attendance for at least seven weeks after the intervention ended. More recent studies on exercise generally find weaker results: some fail to detect habit formation effects from simple incentives (Royer, Stehr and Sydnor, 2015; Patel et al., 2016; Carrera et al., 2018; Rohde and Verbeke, 2017), while others document short-run gains that fade within months (Volpp et al., 2008; Acland and Levy, 2015; Carrera et al., 2020). However, more sophisticated incentives, such as commitment or time-bundled contracts, have produced durable effects on gym attendance and walking (Royer, Stehr and Sydnor, 2015; Aggarwal, Dizon-Ross and Zucker, 2022). Evidence also extends to other health behaviors: financial incentives have produced lasting effects in children's healthy eating for up to three months (Loewenstein, Price and Volpp, 2016) and in daily handwashing for up to nine months (Hussam et al., 2022), though the effect was diminishing in the latter case. Our study adds to this literature by examining biometric health screenings, a low-frequency (annual) behavior. The results demonstrate that even infrequent health behaviors can become habitual, suggesting that habit formation may play an important role in sustaining preventive practices such as annual vaccinations.

Finally, our study contributes to the extensive literature on using financial incentives to promote healthy behaviors. Similar to our setting, prior work has examined low-frequency activities such as vaccinations (Stone et al., 2002; Campos-Mercade et al., 2021), cancer and cardiovascular screenings (Stone et al., 2002; Alsan, Garrick and Graziani, 2019), and workplace health screenings (Jones, Molitor and Reif, 2019; Song and Baicker, 2019). However, little is known whether incentives produce habit formation for these activities, which typically only occur once per year. One notable exception is Schneider et al. (2023), who find that a $24 financial incentive for the initial COVID-19 vaccine dose in Sweden did not significantly affect uptake of subsequent doses. In contrast, we study much larger incentives ($75–$200) in the context of annual health screenings and show that they produced economically large and statistically significant persistence effects.

The remainder of the paper is organized as follows. Section 2 presents a general model of habit formation and shows how its predictions differ depending on the underlying mechanism.

prominent exception is Fujiwara, Meng and Vogl (2016), who study habit formation in voting.

Section 3 provides background on our empirical setting and describes our data. Section 4 outlines our empirical strategy. Section 5 describes our results, and Section 6 concludes.

2 Model

We present a flexible framework for habit formation that nests two widely cited mechanisms—addiction and experience goods—both commonly invoked as important drivers of health-related behaviors (Gruber and Köszegi, 2001; Dupas, 2014; Banerjee et al., 2021; Hussam et al., 2022). This framework provides a common structure for analyzing how past consumption influences current choices and yields distinct, testable predictions for each mechanism.

2.1 Setup

In each period $t \leq T$, consumers choose consumption of a habit-forming good a_t, sold at price p_t, and a composite numeraire good c_t. Consumption of these goods, together with other factors x_t, produces flow utility $U(a_t, S_t, c_t, x_t)$. Past consumption of a_t affects current utility through S_t, the stock of past consumption:

$$S_t = g(a_{t-1}, a_{t-2}, \ldots, a_1) \tag{1}$$

Each period, consumers maximize utility subject to their budget constraint:

$$\max_{a_t, c_t} U(a_t, S_t, c_t, x_t), \tag{2}$$

$$\text{s.t.} \quad w_t = c_t + p_t a_t, \tag{3}$$

where income w_t is allocated between the two goods. For simplicity, we assume consumers are myopic: they ignore how current consumption affects future utility via the stock S_{t+1}.[5]

For tractability, we treat a_t as a continuous variable and assume utility is concave and

[5] In a forward-looking addiction model, current demand would also depend on expected future consumption. In a forward-looking experience-good model, individuals may choose to consume earlier in order to learn their preferences and improve future decisions. However, the qualitative predictions derived below remain unchanged under these forward-looking extensions. Because future incentives were not known to the individuals in our setting in advance, we abstract from forward-looking considerations in both models.

quadratic, a common specification in the addiction literature (Becker, Grossman and Murphy, 1994; Gruber and Köszegi, 2001; Reif, 2019).[6] As we show in Appendix A, solving the consumer's optimization problem (2) subject to the budget constraint (3) yields the following demand equation:

$$a_t = \kappa + \theta S_t + \pi p_t + \delta x_t, \tag{4}$$

where κ is a constant, π captures price sensitivity, and δ reflects the effect of x_t on demand. The parameter of interest, θ, reflects how the stock of past consumption affects current demand: $\theta > 0$ implies habit formation in behavior—higher past consumption raises current demand—while $\theta < 0$ indicates that past consumption reduces current demand, consistent with satiation or fatigue. Appendix A derives the conditions on preferences under which $\theta > 0$. Identifying θ requires exogenous variation in the stock S_t, which our randomized incentive design provides. Conceptually, our instruments shift earlier realizations of x_t, which in turn affect the current stock S_t.[7]

In what follows, we specify two alternative laws of motion for S_t—one corresponding to addiction, the other to experience goods—and derive their testable implications.

2.2 Addiction

To capture addiction, we follow Becker and Murphy (1988) and let past consumption depreciate geometrically:

$$S_{t+1} = (1 - d)\, S_t + a_t, \tag{5}$$

where $d \in (0, 1]$ is the rate of depreciation. Stable demand requires $\theta/d < 1$, ensuring that the effect of past consumption on demand does not grow without bound.

In a two-period setting ($T = 2$) with initial stock $S_0 = 0$—analogous to the two-year

[6] Appendix A presents a discrete-choice version of the model that yields a similar estimating equation.

[7] Equation (4) is a Frisch demand equation, which holds the marginal utility of income constant. Although the randomized incentives are financial in nature, they represent less than 1% of annual income for the median participant. Thus, we assume that any associated income effects are negligible.

window in Wave 2 of our study—combining equations (4) and (5) yields:

$$a_2 = \kappa + \theta a_1 + \pi p_2 + \delta x_2. \tag{6}$$

Extending to three periods ($T = 3$)—as in Wave 1 of our study—and substituting recursively yields:

$$a_3 = \kappa + \theta a_2 + \theta(1-d)a_1 + \pi p_3 + \delta x_3 \tag{7}$$

Equation (7) highlights a key empirical prediction: current demand depends on both recent and earlier consumption, but the effect of more recent consumption is stronger, i.e., $\theta > \theta(1-d)$. In our context, this result implies that current screening participation is more strongly influenced by last year's screening than by screening two years prior.[8]

2.3 Experience Goods

Habit formation can also arise through an experience-good mechanism, where consumers learn their true preferences only after trying the good (Erdem and Keane, 1996; Ackerberg, 2003; Israel, 2005). For example, a consumer who tries mango for the first time may discover they enjoy it—a realization that could lead to a lasting habit. In this model, initial consumption reveals information about taste, and subsequent consumption reflects this updated belief. This mechanism therefore predicts that present consumption depends only on whether the good was ever consumed, not on how recently it was consumed. We formalize this model as the special case where the stock of past consumption captures only the experience of prior use:

$$S_t = \begin{cases} 1 & \text{if } a_\tau > 0 \text{ for any } \tau < t, \\ 0 & \text{otherwise.} \end{cases}$$

When there are only two periods, the experience-good model with initial stock $S_0 = 0$ yields the same demand equation (6) as addiction. The models diverge, however, when there are at

[8] Alternatively, third-period demand can be expressed solely in terms of current and past exogenous variables, yielding the "reduced-form" persistence model described in Section 4.1.

least three periods: in the experience-good case, current demand depends only on whether the individual has ever consumed the good, not on the timing of that consumption. Formally:

$$a_3 = \kappa + \theta \mathbf{1}(a_1 + a_2 > 0) + \pi p_3 + \delta x_3. \tag{8}$$

The experience-good demand specification generates distinct, testable predictions that differentiate it from the addiction model discussed above. Estimating only θ in equation (8) cannot reveal this distinction, since both frameworks imply that past consumption increases current demand ($\theta > 0$). However, when data span at least three periods, two additional implications emerge that uniquely characterize the experience-good model.

First, the effect of additional past consumption is zero once the experience good has been tried. In other words, after an individual's initial experience, further prior use does not raise current demand. Empirically, those who consumed the good in both previous periods should be no more likely to consume now than those who consumed in exactly one prior period. This prediction contradicts the addiction model, which implies that more frequent past use reinforces current consumption.

Second, the timing of initial consumption is irrelevant in the experience-good model. Because the experience-good mechanism involves no depreciation, the effect of first-time consumption is identical whether the experience occurred earlier or more recently. Empirically, among individuals who have tried the good exactly once, those whose first use occurred in an earlier period should be just as likely to consume now as those whose first use occurred later. This again contrasts with addiction, which predicts that more recent consumption exerts a stronger influence.

Together, these implications yield one sharp equality (frequency) and one weaker inequality (timing), which we formalize below.

2.4 Unified Three-Period Framework

Both the addiction and experience good mechanisms impose structural restrictions on how third-period demand depends on past consumption. These restrictions can improve statistical power when estimating the models, but different specifications must be estimated to test

each framework separately. In this section, we show that a single, less restrictive specification can generate parameter estimates that discriminate between the two mechanisms. This approach allows both models to be evaluated within a unified empirical framework.

Our approach is to define the following mutually exclusive indicators of past consumption patterns:

$$D_1 \equiv \mathbf{1}\{a_2 > 0,\ a_1 = 0\},$$
$$D_2 \equiv \mathbf{1}\{a_2 = 0,\ a_1 > 0\},$$
$$D_3 \equiv \mathbf{1}\{a_2 > 0,\ a_1 > 0\},$$

and let $D_0 \equiv \mathbf{1}\{a_1 = 0,\ a_2 = 0\}$ denote the omitted reference category. Consider the following estimating equation:

$$a_3 = \kappa + \beta_1 D_1 + \beta_2 D_2 + \beta_3 D_3 + \pi p_3 + \delta x_3 + u_3 \tag{9}$$

Projecting the conditional mean from (8) onto the estimating equation (9) yields the key testable restriction implied by the experience-good model:

$$\beta_1 = \beta_2 = \beta_3 = \theta > 0. \tag{10}$$

This restriction captures both the frequency equality ($\beta_1 = \beta_2 = \beta_3$) and the timing equality ($\beta_1 = \beta_2$) that characterize the experience-good mechanism.

By contrast, under the addiction model, projecting (7) onto (9) yields:

$$\beta_1 = \theta\, \mathbb{E}[a_2 \mid D_1]$$
$$\beta_2 = \theta(1-d)\, \mathbb{E}[a_1 \mid D_2]$$
$$\beta_3 = \theta\, \mathbb{E}[a_2 \mid D_3] + \theta(1-d)\, \mathbb{E}[a_1 \mid D_3]$$

where $\mathbb{E}[a_i \mid D_j]$ denotes the mean of past consumption conditional on prior use pattern D_j.

These imply the testable restrictions:

$$\beta_3 = \beta_1 \frac{\mathbb{E}[a_2 \mid D_3]}{\mathbb{E}[a_2 \mid D_1]} + \beta_2 \frac{\mathbb{E}[a_1 \mid D_3]}{\mathbb{E}[a_1 \mid D_2]} \tag{11}$$

$$\beta_1 > \beta_2 \frac{\mathbb{E}[a_2 \mid D_1]}{\mathbb{E}[a_1 \mid D_2]}. \tag{12}$$

The first restriction captures the frequency difference, while the second represents the timing inequality. When a_t is binary (as in our empirical setting), these simplify to:

$$\beta_3 = \beta_1 + \beta_2, \tag{13}$$

$$\beta_1 > \beta_2. \tag{14}$$

The frequency restriction implied by addiction provides a sharp contrast with the experience-good model. Even if we fail to reject the experience-good equalities ($\beta_1 = \beta_2 = \beta_3$), the addiction model can still be rejected through its sharper prediction that $\beta_3 = \beta_1 + \beta_2$ (for binary a_t). By contrast, the timing implication under addiction is an inequality and thus not sharp—it remains compatible with failing to reject the experience-good model. Conversely, failing to reject the addiction restriction does not imply that the experience-good equalities hold. We therefore focus on the frequency-based restriction ($\beta_1 = \beta_2 = \beta_3$ versus $\beta_3 = \beta_1 + \beta_2$) as the central empirical test distinguishing the two mechanisms.

2.5 Discussion

Several studies link the experience-good model to health behaviors. Crawford and Shum (2005) and Dickstein (2021) model pharmaceutical demand and drug adherence under experiential learning about drug efficacy. Dupas (2014) subsidizes the purchase of antimalarial bed nets and attributes the subsequent increase in long-run demand to learning about the product's true value. Similarly, Banerjee et al. (2021) find that a one-time health insurance subsidy led to sustained enrollment, which they attribute to learning through experience. Related to experience goods is the concept of start-up costs, which have been cited as drivers of habit formation in exercise (Charness and Gneezy, 2009; Carrera et al., 2020). In these models, current demand is shaped by initial consumption but does not necessarily depend

on recent consumption.

To our knowledge, no prior study has implemented a research design to distinguish addiction from an experience-good model. Hussam et al. (2022), for instance, interpret persistence in handwashing as an addiction but acknowledge that it may also reflect learning.[9] Similarly, Banerjee et al. (2021) attribute persistence in health insurance enrollment to learning from initial exposure, but it is also possible that persistence is driven instead by more frequent or more recent interaction with the health system.

Our analysis demonstrates that addiction and experience-good models can be empirically distinguished when consumption data span three or more periods, including the initial period in which learning might plausibly occur. Specifically, if habit formation is driven by addiction, third-period demand will be more strongly influenced by second-period consumption than first-period consumption. Conversely, an experience-good model predicts the opposite pattern, where earlier consumption has a greater effect on current demand.

Other mechanisms, such as inertia and switching costs, can also drive habit formation. Handel (2013) and Polyakova (2016) argue that these mechanisms contribute to persistence in insurance plan enrollment. Like addiction, inertia and switching cost models predict that current demand should be most strongly related prior consumption from the most recent period. However, we believe these mechanisms are not relevant to our setting. Our subjects are required to make an active choice in each period, which rules out inertia as an explanation for our persistence. Furthermore, there is no clear source of switching costs in our context.

3 Institutional Setting

The Illinois Workplace Wellness Study is a large-scale randomized controlled trial designed to evaluate the effects of workplace wellness programs on employee health, behavior, and productivity, and to examine how incentives shape participation (Jones, Molitor and Reif, 2019; Reif et al., 2020). Conducted at the University of Illinois at Urbana-Champaign, the study randomized 3,300 benefits-eligible employees to a "wellness" group eligible for a two-

[9]They note that increased handwashing may cause people to "learn that handwashing leads to improvements in health, and therefore [to] update their beliefs on the returns to the behavior" (Hussam et al., 2022).

year workplace wellness program involving three annual health screenings from 2016 to 2018. The remaining 1,534 employees were assigned to a "non-wellness" group—ineligible for the wellness program but eligible for the 2017 and 2018 follow-up health screenings. We limit our analysis to subjects who remained continuously enrolled through the end of the intervention, resulting in a final sample size of 4,799.

Employees in the wellness group ($N = 3,275$), referred to as "Wave 1" members, were invited for on-campus biometric health screenings in the fall of 2016, 2017, and 2018. During these annual screenings, clinicians measured each employee's height, weight, waist circumference, and blood pressure, and administered a fingerstick blood test to check for cholesterol, triglyceride, and glucose levels. Employees received their results within minutes and reviewed them with a health coach.

Wave 1 members who completed a health screening in 2016 or 2017 were also invited to complete an online health risk assessment (HRA), a questionnaire designed to assess a person's health habits. Completing the HRA made them eligible to choose from various wellness activities offered throughout the academic year, including a self-paced walking program, weight management classes, and a tobacco cessation program. 34 percent of Wave 1 members completed at least one activity during the two-year program.

Employees assigned to the non-wellness group ($N = 1,524$), referred to as "Wave 2" members, were invited to complete health screenings in 2017 and 2018 so that researchers could compare their biometric health outcomes with those from the Wave 1 group (Reif et al., 2020). Unlike Wave 1 members, they were not eligible to participate in the initial (2016) health screening and were never invited to complete an online HRA or sign up for wellness activities.

Figure 1 presents the study's experimental design. Financial incentives were assigned randomly several weeks before each screening sign-up period. The annual rerandomization ensured that incentives were independent of previous incentives or employee outcomes, including past program participation. In 2016, Wave 1 employees were equally likely to receive an incentive of \$0, \$100, or \$200.[10] In 2017 and 2018, Wave 1 and Wave 2 employees were

[10]The 2016 incentives were paid only to employees who completed both a health screening and an online HRA. Of 1,900 participants who completed a health screening, 1,848 also completed an online HRA. In 2017, HRA completion was not required to receive the assigned incentive. In 2018, the HRA was not offered.

assigned with equal probability to receive either $0 or $125 (in 2017) and either $0 or $75 (in 2018) upon successfully completing a screening. The incentives in each year were assigned at the individual level using stratified random sampling, as detailed in Jones, Molitor and Reif (2019). The study's structure creates five distinct experiments—one per wave and year of randomization.

4 Empirical Strategy

Our empirical strategy encompasses two objectives. The first is to estimate the causal effects of financial incentives on health screening completion. We define the "direct effect" of incentives as their impact on same-year screening completion and the "persistence effect" as their impact on future completion. The five treatments (incentives) embedded in the study design depicted in Figure 1 all produce measurable direct effects. We can measure persistence effects for three of these treatments: the 2016 incentives' effects on Wave 1's 2017–2018 screenings, and the 2017 incentives' effects on 2018 screenings for both Wave 1 and Wave 2. Our second objective is to estimate habit formation, for which we use an IV approach that assumes the persistence effect operates through past completion. We draw on our model in Section 2 to inform our empirical specifications and tests in this case.[11]

It is uncertain whether financial incentives to complete a screening will increase or decrease future screening completion. If individuals prefer to only undergo screenings every few years—perhaps because they feel that more frequent screenings do not generate information of sufficient value—then an incentive that boosts current screening rates might reduce future rates due to intertemporal substitution. On the other hand, screenings could exhibit intertemporal complementarity and lead to habit formation. For instance, regular screenings allow individuals to monitor changes in biometrics over time. Completing an initial screening could also increase future screenings by sparking an individual's interest in health tracking or by making the logistics of scheduling and completing screenings more familiar and manageable. The parameter θ from Section 2, therefore, is of ambiguous sign, both in

[11] Our preregistered analysis plan specified estimating the effects of incentives on screening completion. We did not prespecify the specific models or mechanisms for habit formation examined in this paper.

our model and given these more general considerations.

Our experimental framework relies on the assumption that incentives are randomly assigned to participants. To validate this assumption, we test whether other screening incentives, baseline demographics, and survey variables jointly predict incentive amounts. Table 1 reports the averages of these variables (one per row) across the different treatment arms (one per column). We conduct joint balance tests for each of the five experiments in our study, represented by pairs of adjacent columns (columns (1)–(2), (3)–(4), etc.). The p-values from these tests, all 0.29 or greater, indicate that these variables collectively do not predict assigned incentives and support the null hypothesis of randomly assigned incentives in each experiment.

4.1 Persistence

We estimate the effects of past, current, and future financial incentives on screening completion using the following reduced-form regression model:

$$\text{SCREEN}_i^t = \alpha + \sum_{\tau=1}^{T} \beta_\tau \text{INCENTIVE}_i^\tau + \gamma X_i + \epsilon_i. \tag{15}$$

The outcome, SCREEN_i^t, is an indicator equal to 1 if individual i completed a health screening in event year $t \in \{1, 2, 3\}$, where event year 1 corresponds to 2016 for Wave 1 and 2017 for Wave 2. The focal explanatory variable is INCENTIVE_i^τ, an indicator equal to 1 if individual i was assigned a non-zero screening incentive in year τ, with τ ranging from 1 to $T = 3$ for Wave 1 and from 1 to $T = 2$ for Wave 2 (Figure 1). We estimate the equation separately for each wave and screening year, with individuals as the unit of observation, resulting in five separate regressions: three for Wave 1, and two for Wave 2.

The focal parameter, β_τ, represents the average treatment effect (ATE) of monetary incentives assigned in year τ on screening completion. An individual receives the assigned incentive in year τ only upon completing a health screening that year. Thus, equation (15) identifies the direct effect of incentives on screening completion when screening is measured in the same year as the incentive ($t = \tau$) and a persistence effect when screening is measured in a future year ($t > \tau$). Since future incentives were unknown to participants, we expect

$\beta_\tau = 0$ for $t < \tau$, providing a falsification test of our model.

Equation (15) includes multiple treatment variables. If the effect of an incentive depends on the receipt of past incentives, β_τ will capture a weighted average of ATEs across the counterfactual scenarios created by different combinations of incentives offered in previous years (Muralidharan, Romero and Wüthrich, 2023).[12] This weighted average is policy relevant because many real-world wellness programs and other similar interventions commonly offer incentives on an annual basis. In supplemental analyses, we extend equation (15) to include interaction terms.

In our baseline analysis, equation (15) does not include additional control variables, X_i. Because incentives were randomly assigned, controls are not necessary to remove bias in the focal estimate but may increase precision. In supplemental analyses, we adopt a "post-Lasso" control specification, selecting controls via the Lasso double-selection method of Belloni, Chernozhukov and Hansen (2014). The set of potential control variables includes baseline demographics, health survey responses, health behaviors, and claims-based measures of medical spending and usage, along with all their pairwise interactions.[13] Since randomization was performed at the individual level, we report conventional heteroskedasticity-robust standard errors (Abadie et al., 2023).

4.2 Habit Formation

We define habit formation as the causal effect of past screening completion on current screening completion. We estimate different specifications to test our alternative models of habit formation in Section 2. First, based on our demand estimation equation (6) in Section 2.2, we estimate the effect of a one-year lagged screening on current screenings using the following regression model:

$$\text{SCREEN}_i^t = \kappa + \theta_1 \text{SCREEN}_i^{t-1} + \gamma X_i + \epsilon_i. \tag{16}$$

[12] Future incentives do not influence this average, as they were unknown at the time and could not affect current screening decisions.

[13] For missing values, we impute means/modes and generate a variable that indicates missing values. The missing-value indicators are also included in the set of potential controls. Health behavior measures include participation in an annual running event and usage of campus recreational facilities. See Jones, Molitor and Reif (2019) for a detailed description of these variables.

The focal variables, SCREEN_i^t, are indicators equal to 1 if individual i completed a screening in year t. We estimate this equation separately for screening year 2017 for Wave 1 and screening year 2018 for Wave 2. The term X_i represents a vector of individual-specific control variables. Our baseline analysis includes the contemporary financial incentive, INCENTIVE_i^t, as a control variable because it is a strong predictor of SCREEN_i^t. In supplemental analyses, we report a specification that includes post-Lasso controls. Note that both the addiction model in Section 2.2 and the experience good model in Section 2.3 yield the same estimating equation for a two-period model. Therefore, this specification can identify habit formation, but will not distinguish between our two mechanisms.

We next model screening year 2018 for Wave 1, which represents the third year of participation. Within the context of the addiction model, we turn to equation (7) and estimate the following:

$$\text{SCREEN}_i^{2018} = \kappa + \theta_1 \text{SCREEN}_i^{2017} + \theta_2 \text{SCREEN}_i^{2016} + \gamma X_i + \epsilon_i. \tag{17}$$

Our addiction model carries the prediction that $\theta_1 > \theta_2$, that is the most recent consumption has a stronger effect on current consumption than more distant consumption.[14]

Our next set of three-period specifications test predictions of the experience good model in Section 2.3. Here we have the potential of distinguishing between the addiction and experience learning mechanisms. Our habit formation equation (8) informs the following IV regression:

$$\begin{aligned} \text{SCREEN}_i^{2018} = \kappa &+ \theta_4 \mathbf{1}\left(\text{SCREEN}_i^{2016} + \text{SCREEN}_i^{2017} \geq 0\right) \\ &+ \theta_5 \mathbf{1}\left(\text{SCREEN}_i^{2016} + \text{SCREEN}_i^{2017} = 2\right) + \gamma X_i + \epsilon_i. \end{aligned} \tag{18}$$

Under the experience good model, we predict $\theta_5 = 0$, that is, only the first screening matters

[14] This assumes we have positive persistence effects, as is the case in our setting. A more general prediction is that the absolute value of θ_3 will be smaller than that of θ_2.

for habit formation. To do so, we estimate the following alternative formulation:

$$\begin{aligned} \text{SCREEN}_i^{2018} = \kappa &+ \theta_6 \mathbf{1}\left(\text{SCREEN}_i^{2016} > 0\right) \\ &+ \theta_7 \mathbf{1}\left(\text{SCREEN}_i^{2017} > 0, \text{SCREEN}_i^{2016} = 0\right) + \gamma X_i + \epsilon_i. \end{aligned} \tag{19}$$

Under this alternative estimating equation, the experience good predicts that $\theta_6 = \theta_7$. In other words, the first screening is was determines habit formation, and it does not matter that it happen in 2016 or 2017.

Finally, we implement the unrestricted three-period model in equation (9), which relates 2018 screening behavior to mutually-exclusive indicators for prior screening behavior:

$$\begin{aligned} \text{SCREEN}_i^{2018} = \kappa &+ \beta_1 \mathbf{1}\left(\text{SCREEN}_i^{2016} > 0,\ \text{SCREEN}_i^{2017} = 0\right) \\ &+ \beta_2 \mathbf{1}\left(\text{SCREEN}_i^{2016} = 0,\ \text{SCREEN}_i^{2017} > 0\right) \\ &+ \beta_3 \mathbf{1}\left(\text{SCREEN}_i^{2016} > 0,\ \text{SCREEN}_i^{2017} > 0\right) + \gamma X_i + \epsilon_i. \end{aligned} \tag{20}$$

We implement each of the above models, for both the addiction and experience good models, in the next section below.

Ordinary least squares (OLS) estimation of equations (16)–(20) will produce biased estimates if past screening completion is correlated with unobserved determinants of current screening completion. This bias is likely to be positive, as many factors that increase screening propensity in one period, such as higher health consciousness or a more proactive attitude toward preventive care, are likely to persist and increase the probability of future screening completion. However, the bias could be negative if people typically wait one or more years between screenings.

To address the endogeneity issue between current and past screening behavior, we perform an IV estimation using the randomly assigned monetary incentives from prior years as instruments for past screening completion. When the outcome is the second screening for a given wave, there is a single endogenous regressor (initial screening completion) and one instrument (the initial incentive). When the outcome is the third screening, which happens only for Wave 1 in 2018, the model has two endogenous regressors. We instrument for these endogenous variables using their respective prior screening incentives and their interaction

terms as instruments. We estimate the model using two-stage least squares (2SLS) and report first-stage F-statistics using the method of Sanderson and Windmeijer (2016), which provides separate tests for weak identification for each endogenous regressor.

The IV analysis relies on the exclusion restriction that monetary incentives affect future screening completion solely through their influence on concurrent screening completion. We address potential violations of this restriction in Section 5.2. In models with a single endogenous regressor, we can interpret θ as the average causal effect of screening completion in the past among compliers, i.e., those induced to complete a health screening in a prior year by a financial incentive. This interpretation requires the standard monotonicity assumption for local ATE (LATE) interpretations of 2SLS.

When testing linear restrictions across equations with multiple endogenous regressors, we must further assume that the complier populations are comparable across those regressors. To assess this assumption, we proceed in two steps. First, we show that our specification satisfies the average conditional monotonicity and no cross effects conditions of Bhuller and Sigstad (2024), which ensures that each 2SLS estimand can be interpreted as a properly weighted LATE even in the presence of multiple treatments.[15] Second, we analyze the characteristics of compliers and show that the populations induced by the different instruments are similar across a wide range of observable dimensions.

5 Results

5.1 Persistence

Figure 2 reports annual health screening completion rates by incentive level and experiment wave. The direct effect of an incentive can be assessed by comparing the completion rates of groups that received high and low incentives in the year the incentive was initially introduced. The persistence effects of incentives given before 2018 (shown in panels A, B, and D) can be gauged by comparing completion rates in these groups in subsequent years. For the second

[15] For completeness, the appendix also reports estimates of the coefficients on the two endogenous regressors obtained from separate single-equation IV specifications, which preserve the standard one-instrument-per-treatment LATE interpretation.

and third incentives (in panels B, C, and E), completion rates before the incentives were assigned provide a falsification test for whether incentives were successfully rerandomized each year.

Panel A of Figure 2 shows that for Wave 1 subjects, the initial (2016) financial incentive increased completion of that year's health screening from 49.4% to 61.8%. The panel also indicates that this initial incentive led to positive persistence effects, as those who were offered it were about 4.5 and 4.4 percentage points more likely to complete a health screening in 2017 and 2018, respectively, than those who were not. Error bars confirm the statistical significance of these effects ($p < 0.05$). Panel D shows that the initial incentive offered to individuals in Wave 2 (in 2017) produced similar results, affirming the replicability of a persistent impact of an initial incentive. In contrast, panel B shows that the second incentive offered to Wave 1 subjects had a substantial direct effect but resulted in little to no persistence effect. Finally, the falsification tests in panels B, C, and E all reveal similar screening rates between high- and low-incentive groups in years before the incentive assignment, consistent with incentives being rerandomized annually.

Table 2 displays regression estimates from equation (15), analyzing the impact of financial incentives on current, future, and past screening completion rates. Each column is a separate regression, where the outcome is screening completion for a given wave and year. Rows report the effects of the incentives offered in each of the three years of the program. The direct effects on current completion, highlighted in gray, are shown along the diagonals in columns (1)–(3) for Wave 1 and columns (4)–(5) for Wave 2. The persistence effects on future completion are in bold, while the effects of incentives on past completion (in plain text) serve as falsification tests for the randomization process. Analogous post-Lasso estimates are reported in Table A.1.

The values in the first row of Table 2 corroborate the patterns shown in panel A of Figure 2: the 2016 incentive increased screening completion rates in that year by 12.4 percentage points and increased future completion rates in 2017 and 2018 by 4.5 and 4.4 percentage points, respectively. Across the five distinct experiments in the study, the direct effects ranged from 12.4 percentage points (an increase of 22%) to 27.6 percentage points (an increase of 71%), with all the effects being statistically significant ($p < 0.01$). First-

year incentives increased future screening completion in both waves by 4.4 to 8.9 percentage points ($p < 0.01$), revealing strong persistence effects. By contrast, the second incentive offered to Wave 1 had a small and statistically insignificant persistence effect of 1.2 percentage points (column (3), row 2). Finally, all falsification estimates, reported below the direct effect estimates in columns (1), (2), and (4), are small and statistically insignificant, as expected.

Table A.2 presents estimates from a generalized model incorporating interactions between the incentives assigned in different years.[16] We focus on the 2018 screening outcome, for which the number of previous incentive rounds is greatest. Column (1) of Table A.2 reproduces the baseline estimates from the third column of Table 2, while columns (2)–(5) report different combinations of interactions. Although including these interactions reduces statistical power, the estimates of the main effects remain largely unchanged. We do not detect any significant interaction effects, though we note that our lack of statistical power means we cannot rule out the possibility of meaningful interaction effects.

While all three persistence effects of the first incentives offered to the Wave 1 and Wave 2 groups are large and statistically significant, the effect of the second (2017) incentive on 2018 screening completion in the Wave 1 group is small and insignificant (see panel B of Figure 2 and column (3), row 2 of Table 2). We consider three possible reasons for this discrepancy. First, there may have been something unique about the 2017 screening that influenced our results. For instance, perhaps people had unusually poor screening experiences, resulting in a diminished persistence effect. If that were the case, we would expect Wave 2 subjects, who received the same screening treatment in 2017 as Wave 1 subjects, to exhibit similarly weak persistence effects. However, the 2017 incentive produced a positive persistence effect on 2018 screening completion for Wave 2 subjects (see panel D of Figure 2 and column (5), row 2 of Table 2).

Another possibility is that the 2017 incentive's direct effect on that year's screening completion may have been particularly weak for Wave 1 subjects, in which case we would expect the absolute magnitude of persistence to also be small. Comparing the direct effects in columns (2) and (4) of Table 2 provides some support for this hypothesis, though the

[16]The post-Lasso estimates, reported in Table A.3, are similar.

differences in these effect sizes are small. In addition, the direct effect of the 2017 incentive was larger than the direct effect of the 2016 incentive, which did result in persistence effects. These patterns suggest that a lack of persistence of the second incentive among Wave 1 was not a consequence of weak direct effects.

A third possibility is that only the 2016 incentive matters for persistence within the window we examine. Unlike Wave 2 subjects, Wave 1 subjects were allowed to participate in the 2016 health screenings. Thus, the 2017 incentive represented a second "dose" for Wave 1 but was the first dose for Wave 2. We consider this possibility in further detail in Section 5.2.

5.2 Habit Formation

5.2.1 Addiction Model

Table 3 presents habit formation estimates from our addiction model. Columns (1)–(3) report our IV estimates, where we instrument for past screening completion with randomly assigned past incentives. Columns (1) and (3), which estimate θ_1 from equation (6), show that completing the first health screening increased the likelihood of completing the second screening by 36.0 (84%) and 32.4 (89%) percentage points in Waves 1 and 2, respectively. These effects are both statistically significant ($p < 0.01$) and similar in magnitude. In Column (2), we are able to test our key prediction from the addiction model equation (7): that more recent screenings have a more positive impact on current screenings than more distant ($\theta_2 > \theta_3$). We estimate in row one of column (2), and find that completion of the first screening increased screening completion in the third year by 33.1 percentage points (90%) in Wave 1. This effect is statistically significant ($p < 0.01$). By contrast, our estimate of θ_2 in the second row of column (2) indicates that completing the second screening raised screening rates one year later by a small and statistically insignificant 6.3 percentage points (17%). We therefore reject our key prediction of the addiction model, that the more recent effect, θ_2, should be larger in magnitude than the more distant effect, θ_3. Post-Lasso estimates in Table A.4 provide similar results.

We formally test whether the habit formation effects from first and second screenings are equal for Wave 1, in column (2). These tests lead us to reject equality. The 95% confidence

interval for the second screening effect in row two of column (2), $[-8.0, 20.5]$, does not contain the point estimate of the effect of the first screening in row one of column (2), and vice versa. A formal test also rejects joint equality between the first- and second-year estimates in column (2), with $p < 0.1$. These IV results corroborate the conclusions of the reduced-form analysis in Section 5.1: habit formation produced by the initial health screening was equally strong in both waves, showed no decay after two years, and was greater than the habit formation produced by the second screening in Wave 1.

Columns (4) through (6), report simple OLS estimates of the various θ parameters. In this case, all four θ estimates are positive and statistically significant. However, these estimates are prone to upward bias because an individual's decision to complete a health screening is positively correlated over time. Table 3, therefore, demonstrates how not accounting for this bias could lead to incorrect inference regarding our models. Indeed, the positive bias of the OLS estimates lead to a confirmation of the prediction in the addiction model that $\theta_2 > \theta_3$ in column (5). Meanwhile, our IV estimates indicate the opposite.

5.2.2 Experience Goods

In Table 4 we turned to testing the predictions of our experience good model. Our IV estimates are in columns (1) through (3). In all cases, we estimate version of equation (**??**), using each wave of the experiment seprately, and then combining them. In column (2), for Wave 2, since there are only two periods, we are effectively only able to estimate θ_4 from equation (**??**). We find a positive and significant effect, and note that with only two periods this specification coincides with our equation (6), and therefore repeats the results from column (3) of Table 3.

We next turn to column (1), which uses Wave 1 and leverages three periods of data to estimate both θ_4 and θ_5. In the first row, we again find a positive effect of ever having at least one past screening, either one or two years prior, on the current screening of 43.2 percentage points (XX%), which is statistically significant ($p = 0.XX$)). However, we show in the next row that a having a second screening in the past yields no additional effect on one's current screening ($\theta_5 = -0.065$, $p = 0.XX$). These results are consistent with the predictions of the experience good model in Section 2.3. In column (3), we pool together the two waves,

using Waves 1 and 2 to estimate θ_4, and Wave 1 to again estimate θ_5, and continue to find similarly consistent results.

Finally, in columns (4)-(5) we show OLS estimates using the endogenously determined past screening choices to estimate the parameters of the experience good model. In this case, we again see a general upward bias in the estimates, and, furthermore, we reject the experience model prediction that $\theta_5 = 0$. We therefore find further evidence that relying on endogenous screening outcomes would lead to the opposite conclusion of the IV analysis.z

5.2.3 Alternative Explanations

Our exclusion restriction assumes that the effect of financial incentives on future screening completion operates through an increase in prior screening completion. Because we rerandomized incentives each year, correlation in assigned incentive amounts over time is not a threat to validity. However, it is possible that subjects may not have fully understood or paid attention to how their financial incentives changed over time. For example, if subjects assigned to the high-incentive group in 2016 and then the low-incentive group in 2017 mistakenly thought they would still receive a high incentive in 2017, our exclusion restriction would not hold. To investigate that possibility, we estimate the effect of completing the 2016 screening on 2018 completion for the subsample of subjects assigned to the $0 group in 2017. If inattention drove our estimates, we would expect a smaller treatment effect estimate for this subsample: confused subjects who attended the 2017 screening expecting a high incentive would have learned they were mistaken, thereby reducing their turnout for the 2018 screening.[17]

We report the results of this investigation in Table A.6. Column (1) shows that the 2016 incentive raised the 2018 completion rate by 4.4 percentage points in the full sample. When we limit the regression to those individuals who were assigned the $0 incentive in 2017, the point estimate rises to 5.4 percentage points, indicating that confusion regarding payment is not driving our main estimates. Columns (3) and (4) report the corresponding IV estimates. Here, the point estimate from the subsample is slightly smaller than the estimate from the

[17]The 2017 screening completion rate was 33.8% for the 1,091 people who were assigned to the high-incentive group in 2016 and the low-incentive group in 2017.

full sample, but the difference is not statistically significant. We obtain similar results if we include post-Lasso controls (Table A.7). Thus, we conclude that inattention does not explain our estimates.

Another potential threat to the exclusion restriction is the possibility that offering a financial incentive prompts individuals to seek information about biometric screenings, thereby increasing the likelihood of future screenings regardless of whether they completed the initial incentivized screening. Although we consider this possibility unlikely, we lack evidence to rule it out definitively. However, even if the exclusion restriction were to fail, the persistence estimates presented in Table 2 would remain valid.

5.3 Discussion

Overlooking habit formation significantly underestimates the effectiveness of financial incentives. For example, consider the initial (2016) incentive offered to Wave 1. Without accounting for habit formation, one would conclude that this incentive increased completion rates by 12.4 percentage points (or 406 people) for one year, as shown in column (1) of Table 2. However, the total effect over the two-year program period is an increase of 21.3 percentage points (or 698 people), which is 72% higher.[18] These figures only reflect the effect of habit formation over the two years of our intervention. Since we observed no significant decay in the effect during this period, it is reasonable to believe that the full effect would have been even greater had the program extended beyond two years.

While habit formation substantially increased the effectiveness of our health screening incentives, this effect applies only to the initial exposure. The second dose of incentives had no detectable effect on future screenings, suggesting that while initial incentives may be more effective than previously thought, the benefits of subsequent doses are unlikely to be underestimated by ignoring habit formation. This finding contrasts with predictions from a standard addiction model, where habit formation would increase the effectiveness of both doses.

Our results also shed light on the role of biased beliefs in shaping health behavior. We find

[18]Equivalently, each person induced by incentives to complete a screening in the first year is 36% more likely to complete a screening in the second year and 33% more likely in the third year (Table 3).

evidence of habit formation, driven by a one-shot mechanism such as an experiential learning model. This pattern is consistent with individuals initially holding negatively biased beliefs—for example, overestimating the costs or underestimating the benefits of screening participation—which are revised positively after the first screening. However, different patterns may arise in settings where biased beliefs take a different form. For instance, if individuals hold positively biased beliefs, such as overconfidence in their ability to maintain health, initial participation in a health behavior may lead to a decline in that behavior over time as expectations are tempered.

6 Conclusion

We show that even infrequent health behaviors like annual health screenings can become strongly habitual. Completing a health screening for the first time raises the probability of completing a future screening by over 30 percentage points. However, completing a second screening does not further reinforce this habit. This pattern contradicts the standard model of addiction, but is consistent with an experience-good model where consumers learn the value of health screenings.

The key to disentangling these two mechanisms is our novel experimental research design, which includes multiple rounds of rerandomized incentives that allow us to separately identify the effects of different past screening decisions. By contrast, prior work has generally relied on panel data coupled with quasi-experimental variation, a one-time experimental treatment, or structural modeling (Becker, Grossman and Murphy, 1994; Ackerberg, 2001; Gruber and Köszegi, 2001).

While our study focuses on individual-level behaviors, we also acknowledge that health outcomes are influenced by system-level forces, such as health care markets, regulations, and social norms, as well as individual-level decisions, such as diet and exercise. Our findings do not address whether interventions at the system or individual level are more effective, or whether they serve as complements or substitutes. These questions are beyond the scope of this study, but would be interesting avenues for future work.[19]

[19]See Chater and Loewenstein (2023) for a broader discussion.

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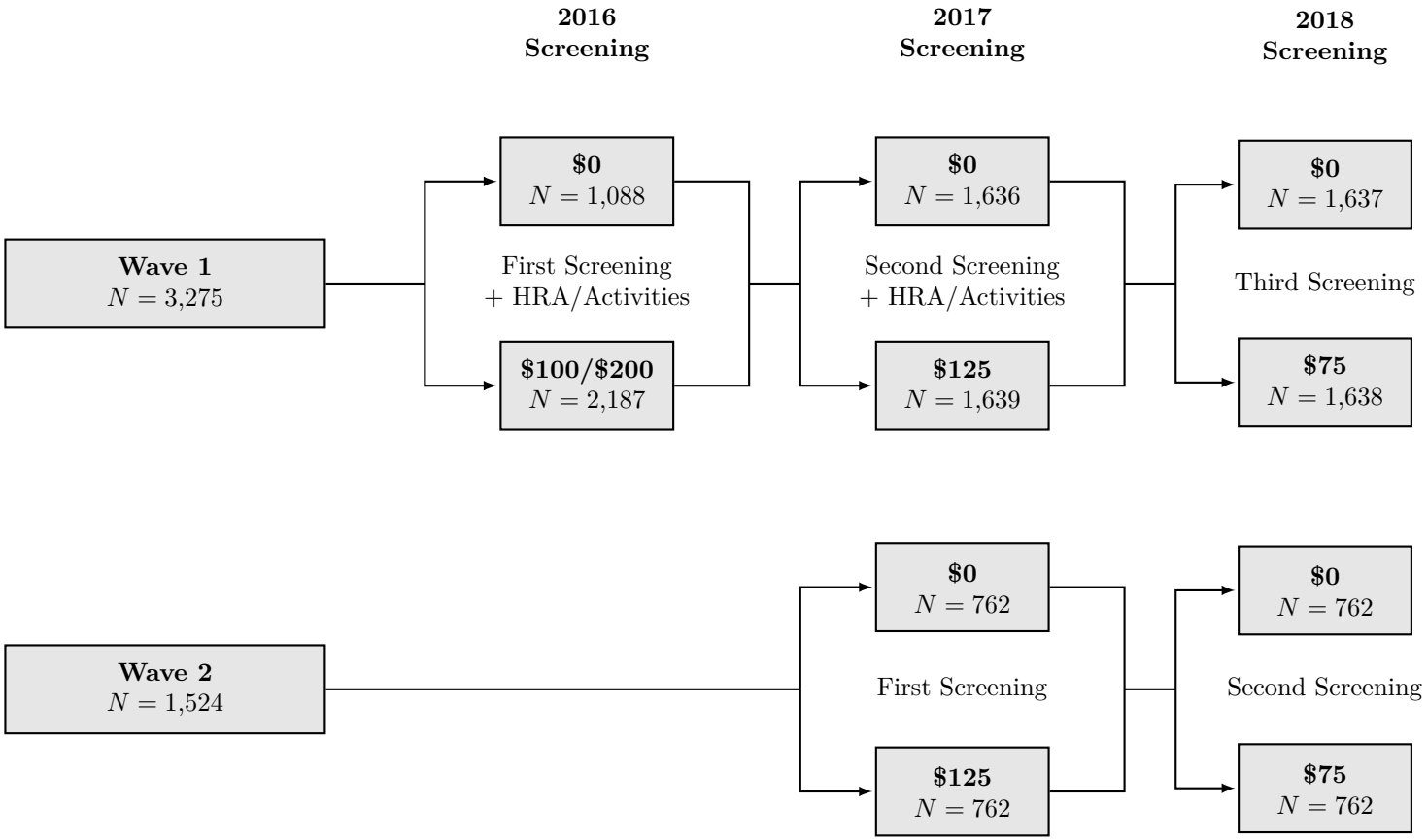
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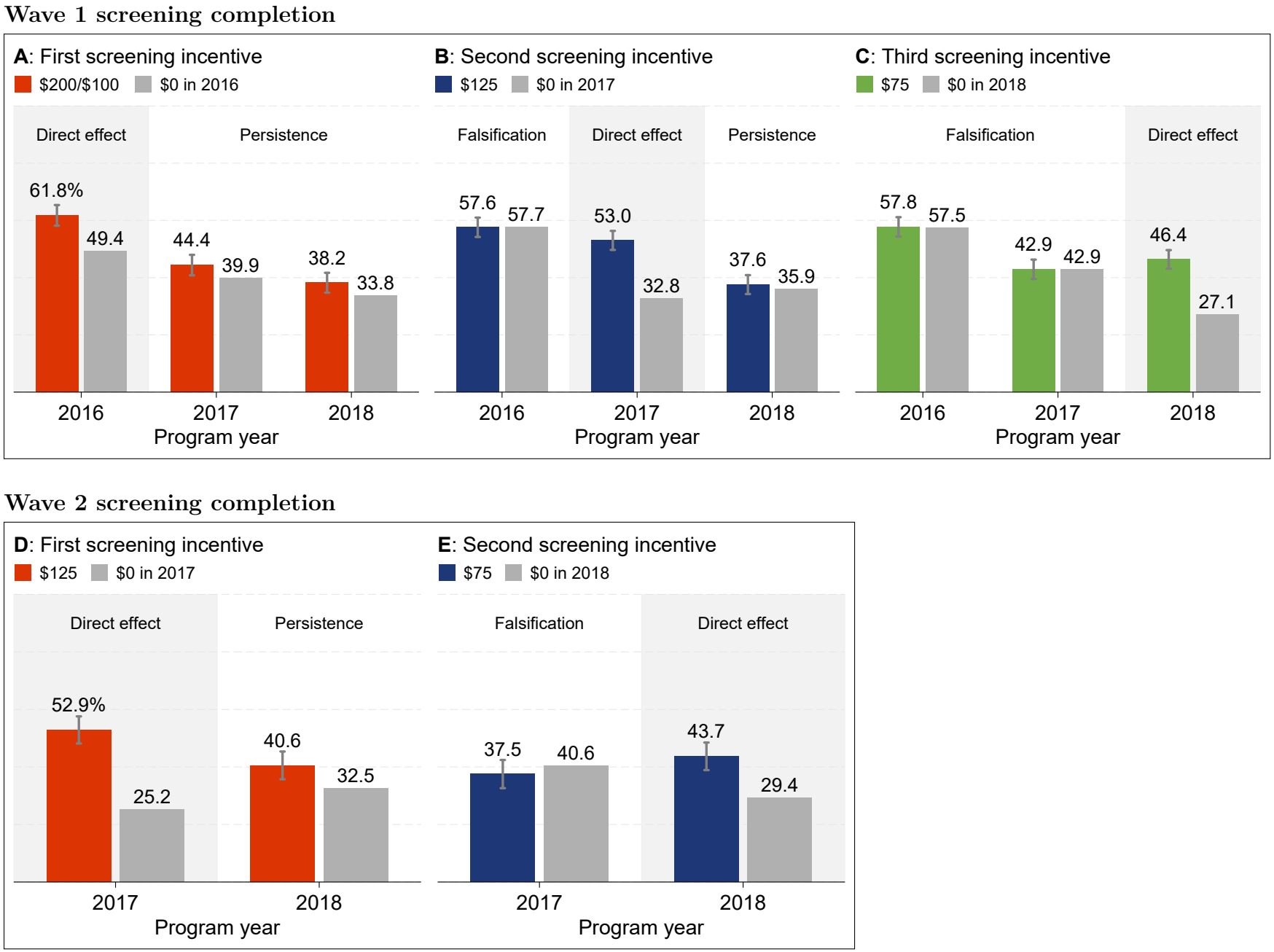
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Figure 1: Experimental Design of the Illinois Workplace Wellness Study



Notes: The figure depicts treatment assignments over time for subjects continuously enrolled through the end of the intervention. In 2016, the 4,799 subjects were randomly assigned to either Wave 1 or Wave 2. Wave 1 was invited to complete a biometric health screening in 2016, 2017, and 2018, while Wave 2 was invited only in 2017 and 2018. Subjects were randomly assigned to either a control ($0 incentive) or treatment (> $0 incentive) group a few weeks before each screening. Incentive assignments are uncorrelated across years.

Figure 2: Health Screening Completion Rates, by Incentive Groups



Notes: The figure panels report raw screening completion rates, by year and incentive level. Each panel reflects a specific wave and year of the incentive. Error bars show 95% confidence intervals of differences in screening rates between high- and low-incentive groups. Comparisons made one to two years after the assignment of incentives are labeled "persistence," while those made one to two years before the assignment are labeled "falsification."

Table 1: Balance Table

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Wave 1 screening incentives						Wave 2 screening incentives			
	First (2016)		Second (2017)		Third (2018)		First (2017)		Second (2018)	
	$200/$100	$0	$125	$0	$75	$0	$125	$0	$75	$0
A. Screening Incentive Variables										
First-year screening incentive	1.00	0.00	0.67	0.67	0.67	0.67	1.00	0.00	0.47	0.53
Second-year screening incentive	0.50	0.50	1.00	0.00	0.51	0.49	0.47	0.53	1.00	0.00
Third-year screening incentive	0.50	0.50	0.51	0.49	1.00	0.00				
B. Stratification Variables										
Male [admin]	0.43	0.43	0.43	0.43	0.43	0.43	0.43	0.42	0.43	0.42
Age 50+ [admin]	0.32	0.33	0.33	0.32	0.32	0.33	0.32	0.32	0.32	0.32
Age 37–49 [admin]	0.33	0.33	0.33	0.33	0.33	0.33	0.34	0.34	0.34	0.34
White [admin]	0.83	0.84	0.84	0.83	0.83	0.84	0.84	0.84	0.86	0.82
Salary Q1 (bottom quartile) [admin]	0.24	0.24	0.24	0.24	0.25	0.24	0.24	0.25	0.25	0.24
Salary Q2 [admin]	0.26	0.26	0.26	0.26	0.26	0.26	0.26	0.25	0.25	0.26
Salary Q3 [admin]	0.25	0.26	0.25	0.25	0.25	0.25	0.25	0.25	0.24	0.26
Faculty [admin]	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.19	0.20	0.20
Academic staff [admin]	0.44	0.44	0.44	0.44	0.44	0.44	0.44	0.44	0.44	0.44
C. 2016 Health Survey Variables										
Ever screened [survey]	0.89	0.90	0.88	0.90	0.89	0.89	0.88	0.89	0.88	0.89
Physically active [survey]	0.39	0.37	0.38	0.39	0.38	0.39	0.35	0.37	0.36	0.36
Trying to be active [survey]	0.82	0.80	0.81	0.81	0.81	0.81	0.83	0.81	0.81	0.84
Current smoker (cigarettes) [survey]	0.07	0.06	0.07	0.06	0.06	0.07	0.06	0.09	0.07	0.07
Current smoker (other) [survey]	0.09	0.07	0.09	0.08	0.08	0.09	0.07	0.10	0.09	0.08
Former smoker [survey]	0.19	0.20	0.18	0.21	0.18	0.21	0.20	0.19	0.19	0.20
Drinker [survey]	0.64	0.65	0.64	0.65	0.66	0.64	0.64	0.67	0.65	0.66
Heavy drinker [survey]	0.05	0.04	0.05	0.05	0.05	0.05	0.05	0.05	0.04	0.06
Chronic condition [survey]	0.72	0.74	0.73	0.72	0.72	0.73	0.74	0.72	0.73	0.73
Excellent or v. good health [survey]	0.59	0.61	0.60	0.61	0.61	0.59	0.59	0.59	0.57	0.60
Not poor health [survey]	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99
Physical problems [survey]	0.39	0.39	0.39	0.39	0.39	0.39	0.39	0.40	0.38	0.40
Lots of energy [survey]	0.33	0.33	0.33	0.33	0.35	0.30	0.31	0.31	0.29	0.33
Bad emotional health [survey]	0.29	0.29	0.29	0.28	0.28	0.30	0.30	0.32	0.31	0.30
Overweight [survey]	0.52	0.56	0.54	0.53	0.53	0.53	0.55	0.54	0.52	0.56
High BP/cholesterol/glucose [survey]	0.29	0.31	0.30	0.29	0.29	0.30	0.32	0.29	0.31	0.30
Sedentary [survey]	0.55	0.53	0.55	0.54	0.56	0.53	0.56	0.53	0.53	0.56
Sample size	2,187	1,088	1,639	1,636	1,638	1,637	762	762	762	762
Joint balance test (p-value)		0.61		0.98		0.56		0.81		0.29

Notes: The table reports group means. The joint balance test row reports the p-value from testing whether assignment to a positive screening incentive in the specified year (column label) is predicted by the row variables, excluding the incentive variable itself.

Table 2: Effect of Financial Incentives on Health Screening Completion

	(1)	(2)	(3)	(4)	(5)
	Wave 1			Wave 2	
	2016	2017	2018	2017	2018
2016 incentive ($200/$100)	0.124**	**0.045***	**0.044***		
	(0.018)	**(0.018)**	**(0.017)**		
2017 incentive ($125)	–0.001	0.203**	**0.012**	0.276**	**0.089****
	(0.017)	(0.017)	**(0.017)**	(0.024)	**(0.024)**
2018 incentive ($75)	0.002	–0.004	0.192**	–0.013	0.149**
	(0.017)	(0.017)	(0.017)	(0.024)	(0.024)
Mean outcome	0.576	0.429	0.368	0.390	0.365
Sample size	3,275	3,275	3,275	1,524	1,524

Notes: This table reports estimates of β_τ from Equation (15). The dependent variable is an indicator for whether a screening was completed in the year specified in the column header. Gray-shaded estimates indicate the contemporaneous effect of the incentive on screening completion. Bold-faced estimates indicate the persistence effect of the incentive on future screening completion, and plain-text estimates report a falsification test of an unannounced incentive on past screening completion. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table 3: Effect of Past Screening Completion on Current Screening Completion

	(1)	(2)	(3)
	Wave 1		Wave 2
	Screened 2017	Screened 2018	Screened 2018
A. IV estimates			
Screened 2016	θ_1: 0.360**	θ_2: 0.331**	
	(0.128)	(0.127)	
Screened 2017		θ_1: 0.063	θ_1: 0.324**
		(0.073)	(0.075)
First-stage F (Screened 2016)	45.6	48.1	
First-stage F (Screened 2017)		182.5	131.6
Mean outcome	0.429	0.368	0.365
Sample size	3,275	3,275	1,524
B. OLS estimates			
Screened 2016	θ_1: 0.467**	θ_2: 0.183**	
	(0.014)	(0.016)	
Screened 2017		θ_1: 0.430**	θ_1: 0.552**
		(0.017)	(0.022)
Mean outcome	0.429	0.368	0.365
Sample size	3,275	3,275	1,524

Notes: This table reports IV and OLS estimates of θ_1 and θ_2 from Equations (16) and (17). Dependent variable is an indicator for whether the 2017 (or 2018) screening was completed. Reinforcement-class mechanisms like addiction predict $\theta_1 > \theta_2$. All regressions control for contemporary financial incentives. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table 4: Effect of Initial and Second Screenings on 2018 Screening Completion

	(1)	(2)	(3)	(4)	(5)
	Wave 1	Wave 2	Waves 1 & 2	Wave 1	Waves 1 & 2
A. IV estimates					
Screened ever	θ_4: 0.361**	θ_4: 0.324**	θ_4: 0.334**	θ_4: 0.432*	θ_4: 0.337**
	(0.130)	(0.075)	(0.065)	(0.216)	(0.071)
Screened twice				θ_5: –0.065	θ_5: –0.011
				(0.158)	(0.104)
First-stage F (Screened ever)	14.9	131.6	44.1	11.8	51.7
First-stage F (Screened twice)				17.0	71.8
Mean outcome	0.368	0.365	0.367	0.368	0.367
Sample size	3,275	1,524	4,799	3,275	4,799
B. OLS estimates					
Screened ever	θ_4: 0.444**	θ_4: 0.552**	θ_4: 0.480**	θ_4: 0.229**	θ_4: 0.362**
	(0.013)	(0.022)	(0.011)	(0.017)	(0.014)
Screened twice				θ_5: 0.384**	θ_5: 0.310**
				(0.019)	(0.018)
Mean outcome	0.368	0.365	0.367	0.368	0.367
Sample size	3,275	1,524	4,799	3,275	4,799

Notes: This table reports IV and OLS estimates of θ_4 and θ_5 from Equation (18). Dependent variable is an indicator for whether the 2018 screening was completed. Initial-exposure mechanisms like experience goods predict $\theta_4 > 0$ and $\theta_5 = 0$. All regressions control for contemporary financial incentives, interacted with wave. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table 5: Effect of 2016 and 2017 First-Time Screenings on 2018 Screening Completion

	(1)	(2)	(3)
	Wave 1	Wave 2	Waves 1 & 2
A. IV estimates			
Screened 2016	θ_6: 0.389**		θ_6: 0.419**
	(0.133)		(0.123)
Screened 2017 only	θ_7: 0.180	θ_7: 0.324**	θ_7: 0.308**
	(0.224)	(0.075)	(0.072)
First-stage F (Screened 2016)	22.3		60.0
First-stage F (Screened 2017 only)	29.6	131.6	63.6
Mean outcome	0.368	0.365	0.367
Sample size	3,275	1,524	4,799
B. OLS estimates			
Screened 2016	θ_6: 0.449**		θ_6: 0.464**
	(0.014)		(0.014)
Screened 2017 only	θ_7: 0.408**	θ_7: 0.552**	θ_7: 0.503**
	(0.033)	(0.022)	(0.018)
Mean outcome	0.368	0.365	0.367
Sample size	3,275	1,524	4,799

Notes: This table reports IV and OLS estimates of θ_6 and θ_7 from equation (19). Dependent variable is an indicator for whether the 2018 screening was completed. Initial-exposure mechanisms like experience goods predict $\theta_6 = \theta_7$. All regressions control for contemporary financial incentives, interacted with wave. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table 6: Effects of Mutually Exclusive 2016 and 2017 Screening Choices on 2018 Screening Completion

	(1)	(2)
	IV	OLS
Screened 2016 only	β_1: 0.440**	β_1: 0.188**
	(0.139)	(0.018)
Screened 2017 only	β_2: 0.316**	β_2: 0.503**
	(0.075)	(0.018)
Screened twice	β_3: 0.403**	β_3: 0.628**
	(0.133)	(0.015)
Parameter hypothesis tests (p-values)		
Addiction: $\beta_1 + \beta_2 = \beta_3$	0.017	0.021
Experience: $\beta_1 = \beta_2 = \beta_3$	0.678	0.000
First-stage F (Screened 2016 only)	92.2	
First-stage F (Screened 2017 only)	70.3	
First-stage F (Screened twice)	91.3	
Mean outcome	0.367	0.367
Sample size	4,799	4,799

Notes: This table reports IV and OLS estimates of β_1, β_2, and β_3 from equation (9). Dependent variable is an indicator for whether the 2018 screening was completed. All regressions control for contemporary financial incentives, interacted with wave. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Online Appendix

"How Do Habits Form? Experimental Evidence from Health Screenings"

Damon Jones, David Molitor, and Julian Reif

A Habit Formation Model Derivations

A.1 Continuous a_t

We provide the full derivation of the various demand equations featured in Section 2. Following Reif (2019), we solve the consumer's optimization problem under the assumption that utility is concave and quadratic:

$$\begin{aligned} U\left(a_t, S_t, c_t, x_t\right) = &-\frac{1}{2}\left(u_{aa}a_t^2 + u_{ss}S_t^2 + u_{cc}c_t^2 + u_{xx}x_t^2\right) + u_{as}a_tS_t + u_{ac}a_tc_t + u_{ax}a_tx_t \\ &+ u_{sc}S_tc_t + u_{sx}S_tx_t + u_{cx}c_tx_t + u_aa_t + u_sS_t + u_cc_t + u_xx_t \end{aligned} \tag{A.1}$$

Habit formation is driven by the parameter u_{as}, which captures the strength of intertemporal complementarity and is assumed to be positive. Assuming agents are myopic, we have the following utility maximization problem:

$$\begin{aligned} \max_{a_t, c_t} \quad & U(a_t, S_t, c_t, x_t) \\ \text{s.t.} \quad & w_t = c_t + p_ta_t \end{aligned} \tag{A.2}$$

Maximizing utility with respect to c_t and subject to the budget constraint yields:

$$c_t = \frac{-\lambda + u_c + u_{ac}a_t + u_{sc}S_t + u_{cx}x_t}{u_{cc}} \tag{A.3}$$

where λ is the marginal utility of income. Substituting (A.3) into the utility function (A.1) yields a reduced-form utility in a_t, S_t, and x_t only:

$$U^*\left(a_t, S_t, x_t\right) = -\frac{1}{2}\left(b_{aa}a_t^2 + b_{ss}S_t^2 + b_{xx}x_t^2\right) + b_{as}a_tS_t + b_{ax}a_tx_t + b_{sx}S_tx_t + b_aa_t + b_sS_t + b_xx_t + b_k \tag{A.4}$$

where each b-parameter is a function of the original u-parameters. In particular,

$$b_{as} = \frac{u_{ac}u_{sc} + u_{as}u_{cc}}{u_{cc}}$$

Using the first-order condition $\partial U^*/\partial a_t = \lambda p_t$ yields the demand equation from the main text:

$$a_t = \kappa + \theta S_t + \pi p_t + \delta x_t \tag{A.5}$$

where:

$$\begin{aligned}\kappa &= \frac{b_a}{b_{aa}} \\ \theta &= \frac{b_{as}}{b_{aa}} > 0 \\ \pi &= \frac{-\lambda}{b_{aa}} < 0 \\ \delta &= \frac{b_{ax}}{b_{aa}}\end{aligned}$$

The slope on the stock variable S_t, θ, is identical across periods and captures the strength of habit formation. A sufficient condition for $\theta > 0$ is $b_{as} > 0$, which holds if $u_{ac}u_{sc} \geq 0$; a simple special case is additive separability in a_t and c_t.

A.2 Discrete a_t

We now extend the model to the case of a discrete choice. Let $a_t \in \{0, 1\}$ be a binary choice made every period. We amend utility with an additional, random component that is conditional on choosing $a_t = 1$:

$$\tilde{U}(a_t, S_t, x_t) = U^*(a_t, S_t, x_t) - a_t v_t \tag{A.6}$$

where v_t is a random shock with CDF $F_v(\cdot)$. In each period, a myopic agent will choose $a_t = 1$ if the decision yields higher utility:

$$\begin{aligned}a_t &= \mathbf{1}\left(\tilde{U}(1, S_t, x_t) - \tilde{U}(0, S_t, x_t) > 0\right) \\ &= \mathbf{1}\left(U^*(1, S_t, x_t) - U^*(0, S_t, x_t) > v_t\right) \\ &= \mathbf{1}\left(b_a + b_{as}S_t + b_{ax}x_t - \tfrac{1}{2}b_{aa} > v_t\right) \\ &= \mathbf{1}\left(\kappa + \theta S_t + \pi p_t + \delta x_t > v_t\right)\end{aligned}$$

The last step follows after dividing both sides by b_{aa}, which rescales both the latent index and the idiosyncratic error by the same positive constant. Because only the sign of the inequality determines the choice, this normalization leaves the implied choice probability unchanged. The constant term $-1/2$ merely shifts the location of v_t, which likewise has no effect on the probability. For notational simplicity, the rescaled and mean-shifted error is still denoted v_t.

It follows that the probability of choosing $a_t = 1$ will be:

$$\begin{aligned}\Pr\left(a_t = 1\right) &= \mathbb{E}\left[\mathbf{1}\left(\kappa + \theta S_t + \pi p_t + \delta x_t > v_t\right)\right] \\ &= F_v\left(\kappa + \theta S_t + \pi p_t + \delta x_t\right)\end{aligned} \tag{A.7}$$

Substituting the respective laws of motion for the habit stock—(5) or (2.3)—into the demand equation produces analogous expressions to those derived in the continuous model. In practice, we approximate the discrete demand in equation (A.7) with a linear probability model, which takes the same form as equation (A.5).

Table A.1: Effect of Financial Incentives on Health Screening Completion, Post-Lasso Controls

	(1)	(2)	(3)	(4)	(5)
	Wave 1			Wave 2	
	2016	2017	2018	2017	2018
2016 incentive ($200/$100)	0.133**	**0.046****	**0.045****		
	(0.018)	**(0.017)**	**(0.017)**		
2017 incentive ($125)	–0.003	0.200**	**0.014**	0.276**	**0.090****
	(0.016)	(0.016)	**(0.016)**	(0.023)	**(0.024)**
2018 incentive ($75)	–0.005	–0.007	0.188**	–0.002	0.154**
	(0.016)	(0.016)	(0.016)	(0.024)	(0.024)
Number of controls	53	40	42	40	42
Mean outcome	0.576	0.429	0.368	0.390	0.365
Sample size	3,275	3,275	3,275	1,524	1,524

Notes: This table reports estimates of β_τ from Equation (15). The dependent variable is an indicator for whether a screening was completed in the year specified in the column header. Each regression controls for a set of covariates selected by Lasso to predict the dependent variable in the full sample. "Number of controls" reports the number of selected covariates. Gray-shaded estimates indicate the contemporaneous effect of the incentive on screening completion. Bold-faced estimates indicate the persistence effect of the incentive on future screening completion, and plain-text estimates report a falsification test of an unannounced incentive on past screening completion. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table A.2: Effect of Financial Incentives on 2018 Health Screening Completion for Wave 1

	(1)	(2)	(3)	(4)	(5)
2016 incentive ($200/$100)	0.044*	0.054*	0.046*	0.044*	0.032
	(0.017)	(0.024)	(0.023)	(0.017)	(0.032)
2017 incentive ($125)	0.012	0.025	0.012	0.036	0.016
	(0.017)	(0.028)	(0.017)	(0.022)	(0.037)
2018 incentive ($75)	0.192**	0.193**	0.195**	0.216**	0.186**
	(0.017)	(0.017)	(0.028)	(0.023)	(0.039)
2016 incentive × 2017 incentive		–0.019			0.029
		(0.035)			(0.046)
2016 incentive × 2018 incentive			–0.003		0.045
			(0.035)		(0.049)
2017 incentive × 2018 incentive				–0.047	0.018
				(0.033)	(0.056)
2016 incentive × 2017 incentive × 2018 incentive					–0.096
					(0.069)
Mean outcome	0.368	0.368	0.368	0.368	0.368
Sample size	3,275	3,275	3,275	3,275	3,275

Notes: This table reports estimates of β_τ and its interactions from a version of Equation (15) that incorporates interactions between the different assigned treatments. The dependent variable is an indicator for whether a screening was completed in 2018. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table A.3: Effect of Financial Incentives on 2018 Health Screening Completion for Wave 1, Post-Lasso Controls

	(1)	(2)	(3)	(4)	(5)
2016 incentive ($200/$100)	0.045**	0.046	0.043	0.045**	0.013
	(0.017)	(0.024)	(0.023)	(0.017)	(0.031)
2017 incentive ($125)	0.014	0.014	0.014	0.035	–0.006
	(0.016)	(0.027)	(0.016)	(0.022)	(0.036)
2018 incentive ($75)	0.188**	0.188**	0.185**	0.209**	0.164**
	(0.016)	(0.016)	(0.028)	(0.023)	(0.039)
2016 incentive × 2017 incentive		–0.001			0.062
		(0.034)			(0.045)
2016 incentive × 2018 incentive			0.004		0.067
			(0.034)		(0.048)
2017 incentive × 2018 incentive				–0.042	0.042
				(0.032)	(0.055)
2016 incentive × 2017 incentive × 2018 incentive					–0.126
					(0.068)
Number of controls	42	42	42	42	42
Mean outcome	0.368	0.368	0.368	0.368	0.368
Sample size	3,275	3,275	3,275	3,275	3,275

Notes: This table reports estimates of β_τ and its interactions from a version of Equation (15) that incorporates interactions between the different assigned treatments. The dependent variable is an indicator for whether a screening was completed in 2018. Each regression controls for a set of covariates selected by Lasso to predict the dependent variable in the full sample. "Number of controls" reports the number of selected covariates. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table A.4: Effect of Past Screening Completion on Current Screening Completion, Post-Lasso Controls

	(1)	(2)	(3)
	Wave 1		Wave 2
	Screened 2017	Screened 2018	Screened 2018
A. IV estimates			
Screened 2016	θ_1: 0.364**	θ_3: 0.330**	
	(0.124)	(0.123)	
Screened 2017		θ_2: 0.064	θ_1: 0.318**
		(0.071)	(0.071)
Number of controls	40	42	42
First-stage F (Screened 2016)	49.9	52.4	
First-stage F (Screened 2017)		189.8	142.6
Mean outcome	0.429	0.368	0.365
Sample size	3,275	3,275	1,524
B. OLS estimates			
Screened 2016	θ_1: 0.431**	θ_3: 0.169**	
	(0.015)	(0.016)	
Screened 2017		θ_2: 0.412**	θ_1: 0.524**
		(0.018)	(0.023)
Number of controls	40	42	42
Mean outcome	0.429	0.368	0.365
Sample size	3,275	3,275	1,524

Notes: This table reports IV and OLS estimates of θ from Equations (16) and (17). Dependent variable is an indicator for whether the 2017 (or 2018) screening was completed. Each regression controls for a set of covariates selected by Lasso to predict the dependent variable in the full sample. "Number of controls" reports the number of selected covariates. All regressions control for contemporary financial incentives. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table A.5: Effect of Past Screening Completion on 2018 Screening Completion

	(1)	(2)	(3)
A. IV estimates			
Screened 2016 (θ_3)	0.331**	0.354**	
	(0.127)	(0.129)	
Screened 2017 (θ_2)	0.063		0.061
	(0.073)		(0.079)
First-stage F (Screened 2016)	48.1	45.6	
First-stage F (Screened 2017)	182.5		143.1
Mean outcome	0.368	0.368	0.368
Sample size	3,275	3,275	3,275
B. OLS estimates			
Screened 2016 (θ_3)	0.183**	0.383**	
	(0.016)	(0.015)	
Screened 2017 (θ_2)	0.430**		0.515**
	(0.017)		(0.015)
Mean outcome	0.368	0.368	0.368
Sample size	3,275	3,275	3,275

Notes: This table reports OLS and IV estimates of θ from Equation (17). The dependent variable is an indicator for whether the 2018 screening was completed. Columns (1) and (4) replicate columns (2) and (5) from Table 3. All regressions control for contemporary financial incentives. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table A.6: Effect of 2016 Financial Incentives and 2016 Health Screening Completion on 2018 Health Screening Completion, for Subjects Assigned a $0 Incentive in 2017

	(1)	(2)	(3)	(4)
	Reduced form		IV	
	Full sample	Subsample	Full sample	Subsample
2016 incentive ($200/$100)	0.044*	0.054*		
	(0.017)	(0.024)		
Completed first screening			0.354**	0.340*
			(0.129)	(0.143)
First-stage F (first screening)			45.6	37.3
Mean outcome	0.368	0.359	0.368	0.359
Sample size	3,275	1,636	3,275	1,636

Notes: Columns (1)–(2) report estimates of β_τ from Equation (15), while columns (3)–(4) report IV estimates of θ_τ from Equation (**??**). The dependent variable is an indicator for completing the 2018 health screening. Columns (1) and (3) include the full Wave 1 sample. Columns (2) and (4) limit the sample to Wave 1 subjects who were assigned the $0 incentive in 2017. The IV specification instruments for the endogenous regressor "Completed first screening" using an indicator for the 2016 financial incentive. All regressions control for contemporary financial incentives. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table A.7: Effect of 2016 Financial Incentives and 2016 Health Screening Completion on 2018 Health Screening Completion, for Subjects Assigned a $0 Incentive in 2017, Post-Lasso Controls

	(1)	(2)	(3)	(4)
	Reduced form		IV	
	Full sample	Subsample	Full sample	Subsample
2016 incentive ($200/$100)	0.045**	0.045		
	(0.017)	(0.024)		
Completed first screening			0.355**	0.292*
			(0.123)	(0.144)
Number of controls	42	42	42	42
First-stage F (first screening)			49.9	36.8
Mean outcome	0.368	0.359	0.368	0.359
Sample size	3,275	1,636	3,275	1,636

Notes: Columns (1)–(2) report estimates of β_τ from Equation (15), while columns (3)–(4) report IV estimates of θ_τ from Equation (**??**). The dependent variable is an indicator for completing the 2018 health screening. Columns (1) and (3) include the full Wave 1 sample. Columns (2) and (4) limit the sample to Wave 1 subjects who were assigned the $0 incentive in 2017. The IV specification instruments for the endogenous regressor "Completed first screening" using an indicator for the 2016 financial incentive. Each regression controls for a set of covariates selected by Lasso to predict the dependent variable in the full sample. "Number of controls" reports the number of selected covariates. All regressions control for contemporary financial incentives. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table A.8: Effect of 2017 Financial Incentives and 2017 Health Screening Completion on 2018 Health Screening Completion, by 2016 Financial Incentive

	Reduced form			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
2017 incentive ($125)	0.012	0.025	0.006			
	(0.017)	(0.028)	(0.020)			
Completed second screening				0.061	0.136	0.028
				(0.079)	(0.141)	(0.095)
Sample restrictions:						
None	X			X		
2016 low incentive only		X			X	
2016 high incentive only			X			X
First-stage F (second screening)				143.1	40.3	103.3
Mean outcome	0.368	0.338	0.382	0.368	0.338	0.382
Sample size	3,275	1,088	2,187	3,275	1,088	2,187

Notes: This table reports estimates of β_τ from Equation (15). The dependent variable is an indicator for whether a screening was completed in 2018. Column (1) replicates column (3) from Table 2. Column (2) omits the 2016 incentive regressor. Column (3) limits the sample to individuals who were assigned the low incentive ($0) in 2016. Column (4) limits the sample to individuals receiving a high incentive in 2016. All regressions control for contemporary financial incentives. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table A.9: Effect of Initial and Second Screenings on 2018 Screening Completion, Post-Lasso Controls

	(1)	(2)	(3)	(4)	(5)
	Wave 1	Wave 2	Waves 1 & 2	Wave 1	Waves 1 & 2
A. IV estimates					
Screened ever	θ_x: 0.356**	θ_x: 0.318**	θ_x: 0.327**	θ_x: 0.385	θ_x: 0.328**
	(0.127)	(0.071)	(0.064)	(0.218)	(0.070)
Screened twice				θ_y: –0.026	θ_y: –0.004
				(0.155)	(0.100)
Number of controls	42	42	42	42	42
First-stage F (Screened ever)	16.1	142.6	46.4	11.1	52.9
First-stage F (Screened twice)				15.2	64.2
Mean outcome	0.368	0.365	0.367	0.368	0.367
Sample size	3,275	1,524	4,799	3,275	4,799
B. OLS estimates					
Screened ever	θ_x: 0.412**	θ_x: 0.524**	θ_x: 0.448**	θ_x: 0.214**	θ_x: 0.341**
	(0.014)	(0.023)	(0.012)	(0.017)	(0.014)
Screened twice				θ_y: 0.366**	θ_y: 0.294**
				(0.020)	(0.018)
Number of controls	42	42	42	42	42
Mean outcome	0.368	0.365	0.367	0.368	0.367
Sample size	3,275	1,524	4,799	3,275	4,799

Notes: This table reports IV and OLS estimates of θ from Equation (18). Dependent variable is an indicator for whether the 2018 screening was completed. Each regression controls for a set of covariates selected by Lasso to predict the dependent variable in the full sample. "Number of controls" reports the number of selected covariates. All regressions control for contemporary financial incentives, interacted with wave. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table A.10: Effect of 2016 and 2017 First-Time Screenings on 2018 Screening Completion, Post-Lasso Controls

	(1)	(2)	(3)
	Wave 1	Wave 2	Waves 1 & 2
A. IV estimates			
Screened 2016	θ_x: 0.382**		θ_x: 0.404**
	(0.131)		(0.120)
Screened 2017, first time	θ_x: 0.172	θ_x: 0.318**	θ_x: 0.304**
	(0.222)	(0.071)	(0.070)
Number of controls	42	42	42
First-stage F (Screened 2016)	24.1		24.6
First-stage F (Screened 2017, first time)	29.9	142.6	66.2
Mean outcome	0.368	0.365	0.367
Sample size	3,275	1,524	4,799
B. OLS estimates			
Screened 2016	θ_x: 0.416**		θ_x: 0.431**
	(0.014)		(0.014)
Screened 2017, first time	θ_x: 0.381**	θ_x: 0.524**	θ_x: 0.474**
	(0.032)	(0.023)	(0.018)
Number of controls	42	42	42
Mean outcome	0.368	0.365	0.367
Sample size	3,275	1,524	4,799

Notes: This table reports IV and OLS estimates of θ from Equation (REF). Dependent variable is an indicator for whether the 2018 screening was completed. Each regression controls for a set of covariates selected by Lasso to predict the dependent variable in the full sample. "Number of controls" reports the number of selected covariates. All regressions control for contemporary financial incentives, interacted with wave. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.

Table A.11: Effect of Initial Screening (2016 or 2017) and Second Screening on 2018 Screening Completion

	(1)	(2)	(3)
	Wave 1	Wave 2	Waves 1 & 2
A. IV estimates			
Screened 2016	θ_x: 0.320		θ_x: 0.440**
	(0.240)		(0.139)
Screened 2017, first time	θ_x: 0.042	θ_x: 0.324**	θ_x: 0.316**
	(0.453)	(0.075)	(0.075)
Screened twice	θ_y: 0.073		θ_y: – 0.037
	(0.209)		(0.109)
First-stage F (Screened 2016)	13.1		92.2
First-stage F (Screened 2017, first time)	8.9	131.6	70.3
First-stage F (Screened twice)	11.0		117.8
Mean outcome	0.368	0.365	0.367
Sample size	3,275	1,524	4,799
B. OLS estimates			
Screened 2016	θ_x: 0.173**		θ_x: 0.188**
	(0.018)		(0.018)
Screened 2017, first time	θ_x: 0.407**	θ_x: 0.552**	θ_x: 0.503**
	(0.033)	(0.022)	(0.018)
Screened twice	θ_y: 0.440**		θ_y: 0.440**
	(0.020)		(0.020)
Mean outcome	0.368	0.365	0.367
Sample size	3,275	1,524	4,799

Notes: This table reports IV and OLS estimates of θ from Equation (REF). Dependent variable is an indicator for whether the 2018 screening was completed. All regressions control for contemporary financial incentives, interacted with wave. Robust standard errors are reported in parentheses, and */** indicates significance at the 5%/1% level.