Is Attention Produced Optimally?

Theory and Evidence from Take-Up of Bandwidth Enhancements*

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June 2022

Abstract

This paper investigates whether people optimally value tools that reduce attention costs. We call these tools bandwidth enhancements (BEs) and characterize how demand for BEs vary with the pecuniary incentives to be attentive, under the null hypothesis of correct perceptions and optimal choice. We examine if the optimality conditions are satisfied in three experiments. The first is a field experiment ($n = 1373$) with an online education platform, in which we randomize incentives to complete course modules and incentives to utilize a plan-making tool to complete the modules. In the second experiment ($n = 2306$), participants must complete a survey in the future. We randomize survey-completion incentives and how long participants must wait to complete the survey, and we elicit willingness to pay for reminders. The third experiment ($n = 1465$) involves a psychometric task in which participants must identify whether there are more correct or incorrect mathematical equations in an image. We vary incentives for accuracy, elicit willingness to pay to reduce task difficulty, and examine the impact of learning and feedback. In all experiments, demand for reducing attention costs increases as incentives for accurate task completion increase. However, in all experiments—and across all conditions—our tests imply that this increase in demand is too small relative to the null of correct perceptions. These results suggest that people may be uncertain or systematically biased about their attention cost functions, and that this is not necessarily eliminated by experience and feedback.

*Bronchetti and Magenheim: Swarthmore College. Kessler: University of Pennsylvania and NBER. Taubinsky: UC Berkeley and NBER. Zwick: University of Chicago Booth and NBER. We thank Andrew Caplin, Mark Dean, Xavier Gabaix, Stephen O'Connell, Devin Pope, three grant reviewers at the Russell Sage Foundation, and seminar and conference participants for helpful comments and advice. We thank Alexander Hirsch, Stephanie Nam, Laila Voss, and Caleb Wroblewski for excellent research assistance. We gratefully acknowledge Mike Walmsley and CodeAvengers.com for their support with the education experiment. We gratefully acknowledge research funding from the Russell Sage Foundation, Swarthmore College, the Boettner Center, the Wharton School, the Wharton Behavioral Lab, and the Alfred P. Sloan Foundation. The first experiment was approved by the Swarthmore (covering Haverford and Muhlenberg), Bryn Mawr, Lafayette, and Ursinus IRBs, numbers: 14-15-065, R17-042, AY1617-12, 01-18-17. The second and third experiments were approved by the University of Pennsylvania IRB, number 832335. The opinions expressed in this paper are solely the authors', and do not necessarily reflect the views of any individual or institution listed above.

A large and rapidly growing body of work in economics and cognitive science shows that attention is costly and at least partly controlled (for recent reviews in economics, see Caplin, 2016; Maćkowiak et al., forthcoming; Gabaix, 2019). There is a growing recognition that, like many other types of costly effort decisions that economists have analyzed for decades, mental effort is also costly and deliberately deployed (Shenhav et al., 2017). A powerful modeling approach taken by the *rational inattention* literature is to assume that individuals choose their attention strategies optimally.

In this paper, we move beyond the question of whether people optimize attention within an environment, to investigate whether people optimally choose their attention environment. Specifically, we study whether individuals optimally invest in tools that reduce attention costs. This is a fundamental question for three reasons. First, because decisions over attention environments are pervasive. For example, people can choose whether to set reminders for themselves to reduce the cognitive costs of keeping things top of mind; how much to avoid distraction in their work environment because it impacts the cost of focusing mental effort; whether to avoid sellers with non-transparent and difficult-to-calculate fees; and whether to obtain outside advice for complex and cognitively-demanding tasks such as managing an investment portfolio, selling a home, or filing taxes. Second, because any dynamic model of costly attention (e.g., Sims, 2003; Matejka et al., 2017; Mackowiak et al., 2018) must be closed with an assumption about people's beliefs about their attention cost functions, empirical evidence on whether people are uncertain or systematically biased about their attention costs is a critical input into such modeling.[1] Third, failing to optimally choose an attention environment raises the possibility that individuals may fail to optimize their attention strategies even in static environments, due to uncertainty or systematic misperceptions about their attention cost functions.

The first contribution of this paper is a a methodology for testing whether people properly value tools to reduce demands on their *mental bandwidth*, i.e., the cost of being attentive. To fix terminology, we call these tools bandwidth enhancements (BEs). We then deploy this methodology in three complementary experiments. The first two experiments focus on people's ability to remain attentive to a future task. The third experiment explores an attention-demanding psychometric task, as in recent experiments on rational inattention models (Dean and Neligh, 2018; Caplin et al., 2020; Ambuehl et al., 2020). The BEs we study are a plan-making tool in the first experiment, a reminder tool in the second experiment, and making a

[1]See also de Oliveira et al. (2017), who provide an axiomatic treatment of rational inattention models by studying choices over menus. Such a characterization mechanically requires the assumption that people choose between attention environments optimally.

psychometric task easier in the third experiment.

Our theoretical approach builds on the Caplin et al. (2020) characterization of costly attention models with a competitive supply framework, and the insights that link bounded rationality and Slutsky symmetry introduced by Gabaix (2014). Our approach clarifies the difficulty with assessing whether people optimally value attention-cost reductions just by examining willingness to pay for BEs and how BEs affect behavior: individuals may particularly like or dislike a given BE for reasons unrelated to its impact on behavior, such as the nuisance of additional reminders. The main idea of our approach is to examine how pecuniary incentives to complete a task affect demand for BEs that aid task completion. The first prediction from our model is a precise condition on how willingness to pay for BEs changes with the pecuniary rewards for the task, under the null of correct perceptions. The second prediction is a form of a Slutsky symmetry condition, which states that the impact of task-completion incentives on take-up of the BE is equal to the impact of the price of the BE on the propensity to complete the task.[2] Our model is sufficiently general to apply to both dynamic and static settings, as well as to various assumptions about the set of feasible attention strategies, ranging from the flexible strategy spaces in rational inattention models to much more restrictive assumptions about how people choose attention. We obtain sharp results with minimal assumptions by utilizing the generalized envelope theorems developed by Milgrom and Segal (2002).

Guided by this framework, we carry out three experiments. The first experiment was run in the field with 1373 students and alumni from six Philadelphia-area colleges who enrolled in an 8-week online coding course. The experiment randomized incentives to complete three 15-minute coding lessons each week and randomized incentives to make a plan to complete three 15-minute course modules each week. Making a plan, the BE in this experiment, involved clicking a link that automatically created three 15-minute events in the participant's online calendar of choice for the following week and allowed the participant to rearrange the planned events to suit their schedule.

We document three key facts in our first experiment. First, use of our plan-making tool increased the likelihood of completing coding lessons, especially in the initial weeks. Second, take-up of our plan-making tool was elastic to the direct incentives for plan-making, but remained below 100 percent, even with the incentives. The combination of incomplete take-up and the positive elasticity suggests that the use of our tool imposes internal or "nui-

[2] See Gabaix (2019) for a discussion about exploiting Slutsky symmetry as an empirical strategy for testing limited attention. See Gabaix (2014) and Abaluck and Adams-Prassl (2021) for an implementation of such a test in the context of misperceived product attributes.

sance" costs on at least some individuals. Third, we find that take-up of our plan-making tool increased with incentives for *completing coding lessons*.

This third result is consistent with the *qualitative* prediction that optimizing individuals should value BEs more as the rewards for completing a task increase. At the same time, we estimate that the impact of completion incentives on plan-making is quantitatively too small relative to the Slutsky symmetry condition, suggesting that participants undervalue the plan-making tool. However, our confidence intervals are wide and do not permit us to reject the null of full optimality under correct perceptions.

Our second experiment is an online survey-completion experiment that elicits richer data that allows us to fully quantify the demand for a reminder technology and to test the first prediction from our model. The study was conducted on Amazon Mechanical Turk (MTurk) with 2306 participants. Study participants were offered a bonus (either \$3, \$4, \$11, or \$12) for completing a survey that would only be accessible for a week-long period after a delay (either 2 days, 1 week, 3 weeks, or 6 weeks). Prior to randomizing participants into one of the four possible delays and one of the four possible survey-completion rewards, we elicited participants' willingness to pay (WTP) for a set of three reminder emails, the BE in this experiment, for each possible delay-reward pair. Our procedure also generated exogenous variation in whether participants actually received the reminder emails, allowing us to estimate the effect of reminder emails on survey completion.

We find that survey completion increased with incentives and decreased with delay, while the impact of reminders decreased with incentives and increased with delay. The average impact of reminders on completing the survey was 29 and 16 percentage points for *low* (\$3 or \$4) and *high* (\$11 or \$12) task-completion incentives, respectively. Across the eight different possible delay-reward pairs, the impact of reminders ranged from -7 percentage points ($se = 6.00$) to 40 percentage points ($se = 5.88$).

This set of findings informs several hypotheses about attention in this type of setting. First, the negative effect of delay on task completion and the positive effect of delay on the impact of reminders is consistent with the attention/memory decay curves proposed by cognitive psychologists (see, e.g., Mullainathan, 2002; Ericson, 2017 for reviews). Second, the fact that, at high incentives, task completion was higher but the impact of reminders was lower is consistent with individuals exerting more costly attention to keep the task top of mind when stakes are higher.

Our second set of findings from our second experiment is that, while WTP for reminders increased with the size of the bonus for survey completion, the increase was too small relative to the null of correct perceptions of attention costs. The theory implies that under this null, an extra \$1 of task-completion incentives should increase WTP by approximately \$0.29 and \$0.16 in the *low* and *high* incentive groups, respectively.[3] Instead, WTP increased by \$0.07 ($se = 0.017$) and \$0.02 ($se = 0.047$) in those two groups. Using the variation in WTP and the effects of reminders across all eight conditions, we estimate a model of how people's perceived effects of reminders vary with the actual effects. We find that people uniformly underestimate the effects of reminders by 84 percent, rather than underestimating the effects when they are small and overestimating them when they are large.

Our third experiment, conducted on Prolific Academic with 1465 participants, expands the scope of our analysis in two ways. First, it involves a different domain of decision-making, illustrating the breadth and portability of our methods. Second, it allows us to study how learning and feedback affect people's perceptions of the value of BEs. Specifically, this experiment involved a series of tasks, first utilized by Ambuehl et al. (2020), in which participants were shown an image with a set of arithmetic equations that were either correct (e.g., $10+12=22$) or incorrect (e.g., $10+12=23$). Participants were asked to indicate whether an image contained more correct or more incorrect equations, and were rewarded for accuracy. All participants completed baseline tasks, in which the image had 100 equations and either 60 or 40 percent of equations were correct.

We randomly assigned participants to also do one of two less cognitively demanding versions of the task, the BEs in this experiment. In the *length* condition, the easier task involved only 10 equations. In the *discernibility* condition, the easier task had either 95 or 5 correct equations. Participants completed two blocks of seven tasks, with each block containing three baseline tasks, three easier tasks, and one task that might be affected by the participants' preferences. Analogous to the second experiment, we varied incentives for task completion (i.e., accuracy in this case) and, prior to each block, we elicited individuals' WTP to make the remaining task easier for the different incentive levels. Additionally, prior to the WTP elicitation in the second block, we randomly gave some participants feedback about their performance in the first block on the hard and easy tasks.

We find that participants were 19 ($se = 0.647$) and 26 ($se = 0.607$) percentage points more likely to accurately answer the easier tasks in the length and discernibility conditions, respectively, and these differences were nearly identical across both blocks. Under the null of correct perceptions, participants' WTP to decrease difficulty in the length and discernibility conditions should thus increase by approximately \$0.19 and \$0.26 with each extra dollar of accuracy incentives.[4] However, we find that in the first block, the WTP increases are

[3]This approximation is valid if an additional \$1 has negligible effects on behavior.

[4]See footnote 3.

only \$0.10 ($se = 0.034$) and \$$-0.01$ ($se = 0.030$) in the length and discernibility conditions, respectively. Thus, as in the other experiments, participants undervalue the BEs.

Our data from this experiment provide two additional lessons that complement the insights from the first two experiments. First, in the first block, participants' valuations of the discernibility BE are significantly more biased than their valuations of the length BE. This fact illustrates the potential context-specificity of BE valuations, and the need for portable methods that can quantify them across contexts. Second, we study the effects of learning and feedback. We find that in the second block, participants' WTP to make the task easier increases by only \$0.03 ($se = 0.025$) and \$0.03 ($se = 0.024$) for each extra dollar of accuracy incentives in the length and discernibility conditions, respectively. In the length condition, this result reflects a significant *decrease* relative to the first block. We show that this decrease is concentrated among people who received feedback that they performed at least as well in the longer version of the task than the shorter version, which suggests that—on average—people overweighted experiences in which the length-decreasing BE did not improve performance. This pattern is consistent with recent work on mis-specified learning (e.g., Heidhues et al., 2018; Gagnon-Bartsch et al., 2021), which suggests that experience and feedback do not necessarily eliminate mistakes.

Our results contribute to the literature in several ways. First, we build on the supply theory framework developed by Caplin et al. (2020) to develop methods for assessing whether individuals optimally choose their attention environment. Broadly speaking, models of rational inattention—particularly when applied to dynamic environments—assume that individuals know their attention cost functions and thus would optimally invest in BEs. Thus, our method allows researchers to test key assumptions in models of rational inattention. Despite the recent proliferation of work on rational inattention, surprisingly little work in the economics literature has been devoted to individuals' understanding of the limitations of their attention.[5] Our experiments illustrate how our method can be applied both to the kinds of psychometric settings where rational inattention models have traditionally been tested, as well as to settings concerning behaviors such as education and health investments where the

[5]There is more work in economics on the optimality of individuals' information acquisition strategies. See, e.g., Gabaix et al. (2006); Hanna et al. (2014); Bartoš et al. (2016); Martin (2016); Dean and Neligh (2018); Ambuehl et al. (2020); Caplin et al. (2020); Morrison and Taubinsky (forthcoming). There is also a large psychological literature on metacognition, including work on how individuals act on their environments to create external triggers (e.g., calendar events or reminders) for delayed intentions (Gilbert, 2015a,b). This work finds that whether individuals utilize such tools depends on what they perceive to be the required internal cognitive demands that would otherwise be necessary, as well as the expected value of achieving the goal (Shenhav et al., 2013). These evaluations may be erroneous(Gilbert et al., 2020), leading to suboptimal decisions. We provide a quantitative toolbox for exact quantitative tests of whether people value BEs optimally, which can help advance the more qualitative psychological work on metacognition.

study of attention has been more reduced-form.

Second, a large body of work looks at the impact of BEs, such as planning prompts and reminders, on behaviors such as medical compliance, educational attainment, savings, loan repayment, wage reporting, voting, and charitable donation.[6] We advance this literature by studying individuals' demand for BEs. Our approach sheds light on whether provision of such BEs is efficient. If individuals valued these BEs optimally, then external provision of the BEs would be inefficient because the market already provides individuals with many opportunities to acquire reminder technologies and plan-making tools in the form of various smartphone and computer applications, online calendars, smart caps on pill bottles, and so on. As noted above, incomplete take-up of BEs does not by itself imply that people undervalue them, because in addition to any pecuniary costs, reminders and plan-making tools may carry private nuisance costs (Damgaard and Gravert, 2018), time costs, or detract scarce attention from other important tasks (Nafziger, 2020; Altmann et al., forthcoming).

Closest to our second experiment, Ericson (2011) and Tasoff and Letzler (2014) conduct lab experiments and find that individuals' willingness to pay for a rebate exceeds the expected returns. Their results suggest overestimation of future attention to the rebate and thus follow-through, although other biases, such as Tasoff and Letzler's (2014) proposed *weak cost-salience*, plausibly also play a role. Our approach and results from the second experiment complement Ericson (2011) and Tasoff and Letzler (2014) in a few ways. First, overconfidence about one's baseline level of attention need not imply under-appreciation of the incremental impact of BEs, and vice versa. Second, by directly estimating individuals' (mis)valuations of BEs, our method allows us to directly speak to how much take-up of BEs should be encouraged through subsidies or other interventions. Third, the richness of our second experiment provides new insights about variation in attention, such as our result that people are more attentive at higher stakes.

Last, our work relates to the broader literature that studies whether individuals' beliefs are well-calibrated. A common approach is to directly elicit individuals' beliefs. However, the beliefs that individuals state in an abstract elicitation are not necessarily the decision weights that individuals apply in all real-stakes decisions because of salience and context effects (see, e.g., Bernheim and Taubinsky, 2018 for a recent discussion). For example, many individuals know how large sales taxes are and what products they apply to, but still neglect to incorporate them into their decisions (Chetty et al., 2009; Taubinsky and Rees-

[6]See, e.g., Nickerson and Rogers (2010); Milkman et al. (2011); Altmann and Traxler (2014); Castleman and Page (2016); Bronchetti et al. (2015); Karlan et al. (2016a); Calzolari and Nardotto (2017); Damgaard and Gravert (2018); Marx and Turner (2019); Zhang et al. (2021). See also Carrera et al. (2018) and Oreopoulos et al. (forthcoming) for examples of null effects.

Jones, 2018). Similarly, individuals might have an abstract understanding of how BEs affect behavior yet still undervalue them in real-stakes decisions. Our approach is thus a useful complement to this other work.

Methodologically, this paper also contributes to recent work that tests for behavioral biases by measuring consumer surplus using two different approaches: (i) inferring it from how behavior responds to incentives and (ii) directly eliciting consumers' WTP to engage in the behavior at different incentives. In different domains where bounded self-control rather than bounded rationality is plausibly implicated, Allcott et al. (2022) and Carrera et al. (2022) follow this strategy to estimate the degree of time inconsistency.[7]

The paper proceeds as follows. Section 1 presents our theoretical framework. Sections 2-4 present the designs and results from our three experiments. Section 5 concludes.

1 Theoretical Framework

1.1 Attention Strategies and Payoffs

We consider individuals who are faced with a task, the successful completion of which requires both attentional inputs and possibly other auxiliary inputs. For example, in our first two experiments, individuals first choose attention strategies that determine their likelihood of being attentive to the task in the future, and conditional on being attentive to the task they choose whether or not to provide auxiliary inputs to complete the task. In our third experiment, individuals' choice of attention strategy affects their likelihood of correctly solving a cognitively-demanding task, and there are no auxiliary actions.

Formally, let S_a denote the set of possible attention strategies, with generic element s_a, and let S_o denote the set of strategies over auxiliary actions, with generic element of s_o. Let $z = 1$ indicate that the task is completed successfully, with $z = 0$ otherwise. The likelihood of $z = 1$ is given by $Q(s_a, s_o, \omega)$ where $\omega \in \Omega$ is a state of the world drawn from a prior μ. Individual i's utility function is given by $U_i = rz - K_i(s_a, s_o, \omega)$, where r is the financial reward for completing the task, and K_i is the net utility cost of choosing (s_a, s_o) in state ω.[8]

To ease exposition, we assume that the state is given by $\omega = (\omega_a, \omega_o) \in \Omega_a \times \Omega_o$ and that $Q(s_a, s_o, \omega) = Q_a(s_a, \omega_a) \cdot Q_o(s_o, \omega_o)$. For example, we conceptualize our first two ex-

[7]See also DellaVigna and Malmendier (2004) and Acland and Levy (2015) for early precursors to this strategy in the time-inconsistency context, and see Strack and Taubinsky (2022) for formal results about the robustness of this strategy for measuring limited self-control.

[8]Note that any non-pecuniary benefits from completing the task can be incorporated into the net utility cost function K.

periments settings where individual take actions to maintain attention to the task, with Q_a giving the probability that individuals are attentive to the task. If individuals are not attentive to the task, they cannot complete it. If they are attentive to the task, they can take auxiliary actions to complete the task. Thus, $Q(s_a, s_o)$ is multiplicatively separable. We also assume that K_i is additively separable, given by $K_i = K_{ai}(s_a) + K_{oi}(s_o, \omega_o)$, where it is without loss of generality to assume that K_{ai} does not depend on the state. The general framework covers a variety of settings where attention is implicated, as the examples below illustrate.

Dynamic decisions and sustained attention Suppose that in periods $t = 1, \ldots T$, individuals realize an attention outcome α_t, and choose an attention action a_t. Let $h_t = (\alpha_1, \ldots \alpha_{t-1}, a_1, \ldots a_{t-1})$ denote the period-t history, and let $A_t(h_t, \alpha_t)$ denote the period t set of available actions, allowing for the possibility that α_t is itself endogenous to h_t (and to an underlying state ω_a). Then strategies s_a are feasible plans for a choice of a_t after each realized history. For example, let $\alpha_t \in \{0, 1\}$ encode whether an individual is attentive to the task at the beginning of period t, so that $Q_a = Pr(\alpha_T = 1)$. At each point in time t, suppose that $Pr(\alpha_t = 1)$ is a function of the history h_t, and suppose that $A_t = A$ if $\alpha_t = 1$ and $A_t = \emptyset$ otherwise. That is, as in Ericson (2017) and Taubinsky (2014), individuals can take actions to affect their future attention to the task if they are presently attentive to it, but if they forget about the task than they cannot take action. The attention-sustaining actions might involve setting reminders for oneself, asking others for reminders, or engaging in internal "rehearsal" (e.g., Mullainathan, 2002).

If the individual is inattentive to the task in period T then she does not complete it; otherwise auxiliary actions s_o determine whether the individual completes the task. Suppose that the cost of completing the task is given by ω_o, which is observed in period T (if the individual is attentive), so that an individual's incremental utility from completing the task is $r - \omega_o$. Strategies s_o are then functions $s_o : \omega_o \to \{0, 1\}$, with 1 an indicator for choosing to complete the task conditional on being attentive. The individual's optimal strategy s_o is then a cutoff rule, where $s_o(\omega_o) = 1$ if and only if $r \geq \omega_o$.

Rational inattention in cognitively-demanding tasks Consider a cognitively demanding task, like the kind employed in experiments testing rational inattention (Dean and Neligh, 2018; Caplin et al., 2020; Caplin, 2021), where the person must allocate mental effort to identify the state $\omega \in \{1, \ldots N\}$. The possible actions are messages $m \in M = \{1, \ldots, N\}$, and $z = 1$ if and only if $m = \omega$. The decision maker can receive signals γ from a set of cardinality at least N, and an attention strategy is any joint distribution s over signals and states that is consistent with the prior, so that $\int_\gamma s(\gamma,\omega)d\gamma = \mu(\omega)$. The cost of the information strategy is $K_a(s)$, which for tractability is sometimes assumed to be proportional to the expected mutual information between the state and the signal. The message m sent by the decision maker is the most likely state given the realized posterior.

As shown by Matějka and McKay (2015) and others, such rational inattention models can be equivalently reformulated such that a feasible strategy is any joint distribution over actions and states that is consistent with the prior. That is, S_a is the set of all probability distributions s over $\Omega \times M$ such that $\sum_m s(m,\omega) = \mu(\omega)$ for each ω. Under this definition, $Q(s_a,\omega) = s(m,\omega)/\mu(\omega)$.

1.2 A Simplifying Restatement

Building on Caplin et al. (2020), we perform a change-of-variable to reduce the dimensionality of the individual's optimization problem, which leads to a particularly straightforward interpretation of attention costs.

In the framework we have presented thus far, individual i solves the optimization problem

$$\max_{(s_a,s_o)\in S_a\times S_o} \mathbb{E}\left[rQ_a(s_a,\omega_a)Q_o(s_o,\omega_o) - K_{ai}(s_a) - K_{oi}(s_o,\omega_o)\right] \tag{1}$$

where the expectation is taken with respect to the prior μ. Define $\underline{q}_a := \inf_{s_a}\mathbb{E}Q_a(s_a,\omega_a)$ and $\bar{q}_a := \sup_{s_o}\mathbb{E}Q_a(s_a,\omega_a)$, and define $\underline{q}_o$ and $\bar{q}_o$ analogously. Define $\bar{K}_{ai}(q) = \inf_{s_a}\{K_{ai}(s_a)|\mathbb{E}Q_a(s_a,\omega_a) \geq q\}$ and $\bar{K}_{oi}(q) = \inf_{s_o}\{\mathbb{E}K_{oi}(s_o,\omega_o)|\mathbb{E}Q_o(s_o,\omega_o) \geq q\}$. Then the optimization problem in (1) is equivalent to

$$\max_{(q_a,q_o)\in[\underline{q}_a,\bar{q}_a]\times[\underline{q}_o,\bar{q}_o]} \left[rq_aq_o - \bar{K}_{ai}(q_a) - \bar{K}_{oi}(q_o)\right] \tag{2}$$

We formalize the notion of equivalence in Lemma 1 below.

Lemma 1. *Suppose that (s_a^*,s_o^*) is a solution to (1). Then $(q_a^* = \mathbb{E}Q(s_a^*,\omega_a), q_o^* = \mathbb{E}Q(s_o^*,\omega_o))$ is a solution to (2). Conversely, if (q_a^*,q_o^*) is a solution to (2) then there exist (s_a^*,s_o^*) that are a solution to (1), with $q_a^* = \mathbb{E}Q(s_a^*,\omega_a)$ and $q_o^* = \mathbb{E}Q(s_o^*,\omega_o)$.*

In other words, we can reformulate the individual's decisions as a two-dimensional optimization problem, with a one-dimensional attentional input q_a and a one-dimensional auxiliary input q_o, at costs $\bar{K}_{ai}(q_a)$ and $\bar{K}_{oi}(q_o)$, respectively. Lemma 1 shows that the functions $\bar{K}_{ai}, \bar{K}_{oi}$ are sufficient statistics for an individual's surplus: there are many different economic environments that can generate the same aggregate cost functions $\bar{K}_{ai}$ and $\bar{K}_{oi}$, and an individual's surplus depends only on these aggregate cost functions. This result allows us to focus all discussion below on $\bar{K}_{ai}$ and $\bar{K}_{oi}$, and to omit other details of the attention proces.

We refer to $\bar{K}_{ai}$ as the attention production function, which parallels standard models of competitive supply (Caplin et al., 2020). For example, if the aggregate cost functions are differentiable, then the optimal choice of (q_a, q_o) equates marginal benefits and marginal costs, so that $\bar{K}'_{ai}(q_a) = rq_o$ and $\bar{K}'_{oi}(q_o) = rq_a$.

We make one regularity assumption, which is that there exists a solution to (1), and therefore (2). We do not make additional assumptions about differentiability, continuity, or convexity, but Lemma 2 below shows that the aggregate cost functions $\bar{K}$ will be convex whenever the individual's objective function in (1) is concave. For example, in the special case of rational inattention in cognitively demanding tasks, the individual's optimization problem is concave under the common assumption that attention costs are proportional to mutual information (e.g., Sims, 2003; Matějka and McKay, 2015). In Appendix Lemma A.1, we show that convexity of the cost functions is sufficient to ensure differentiability of many statistics of interest, which facilitates the first-order conditions in Theorem 1 in the next subsection.

Lemma 2. *Suppose that $S_a \times S_o$ is a convex subset of $\mathbb{R}^n$. $\bar{K}_{ai}(q_a)$ is strictly convex in q_a if $\mathbb{E}Q_a(s_a, \omega_a)$ is concave in s_a and $K_{ai}(s_a)$ is convex in s_a, with one of these strict. $\bar{K}_{oi}(q_o)$ is strictly convex in q_o if $\mathbb{E}Q_o(s_o, \omega_o)$ is concave in s_a and $\mathbb{E}K_{oi}(s_o)$ is convex in s_o, with one of these strict.*

1.3 Choice of Attention Technology

We now introduce an initial choice of whether to simplify the attentional demands required to successfully complete the task. In our first two experiments, this involves planning prompts and reminders, respectively. In our third experiment this is a choice of whether to make the task less cognitively demanding. In addition to our specific experimental settings, this formalism can also apply to settings where people exert mental effort under a piece-rate incentive scheme (e.g., Dean, 2019; Kaur et al., 2021; Bessone et al., forthcoming), and may have a choice of task difficulty, decision aids, or the level of distraction in the environment.

Formally, individual i first makes a choice $j \in \{0, 1\}$ between attention cost functions $\bar{K}^0_{ai}$ and $\bar{K}^1_{ai}$.[9] We think of $\bar{K}^1_{ai}$ as constituting a bandwidth enhancement (BE) over $\bar{K}^0_{ai}$. We let p

[9] In principle, we could formalize this choice as part of the attention strategy s_a. However, because this is initial choice is the main choice that is observable to the analyst, we formally distinguish it from the other

denote the incremental cost of choosing $j=1$ over $j=0$, and we assume—consistent with our experiments—that it is incurred at the same time as the variable reward r. In our first experiment, $-p$ corresponds to the incentives we create for choosing our plan-making tool, while in our second and third experiments, p is the price of reminders or making the task easier. [10] We think of the BEs in our experiments as increasing the likelihood of success for a given attention cost, which is equivalent to decreasing the marginal cost of attentional inputs: $\bar{K}^1_{ai}(q'_a) - \bar{K}^1_{ai}(q_a) < \bar{K}^0_{ai}(q'_a) - \bar{K}^0_{ai}(q_a)$ for all $q'_a > q_a$. The decrease in marginal costs does not preclude the possibility that the BEs may cary nuisance costs, formalized as $\bar{K}^1_{ai}(0) > \bar{K}^0_{ai}(0)$.

Define

$$V_i^j(r) := \max_{(q_a,q_o)\in[\underline{q}_a,\bar{q}_a]\times[\underline{q}_o,\bar{q}_o]} \left[r q_a q_o - \bar{K}^j_{ai}(q_a) - \bar{K}_{oi}(q_o) \right]$$

as the indirect utility given attention production function j and incentives r. It is optimal for i to choose attention technology $j=1$ if and only if $V_i^1(r) - p \geq V_i^0(r)$. Our main result characterizes testable restrictions of optimal choice using measurable statistics of aggregate behavior. The first statistic is the willingness to pay (WTP) for technology $j=1$; that is, the highest p at which $j=1$ is preferred to $j=0$. If the nuisance cost of $j=1$ is sufficiently high, this statistic can be negative, even if $j=1$ lowers the marginal cost of attention and thus increases task-completion probability. Average WTP is given by

$$\bar{W}(r) := \mathbb{E}_i \left[V_i^1(r) - V_i^0(r) \right].$$

The other key statistics, which are also at the population level, are $Pr(j=1|p,r)$, the probability of individuals choosing technology $j=1$ given financial incentives p and r; $Pr(z=1|p,r)$, the probability of individuals successfully completing the task given incentives p and r; and $Pr(z=1|j,r)$, the probability of successfully completing the task when exogenously assigned attention technology j.

Lemma A.1 in Appendix A.1 shows that the statistics defined above are (almost everywhere) differentiable under mild assumptions. Differentiability of the cost functions $\bar{K}^j$ is not necessary for these statistics to be differentiable.

attention-affecting choices allowed by our general model.

[10] Individual differences in K_i^0 and K_i^1 could result from individual differences in baseline attentiveness; differences in how well-suited the BE is to an individual's needs; differences in the nuisance costs of reminders, and the personal and social costs of failing to execute a plan that one creates; or (in reduced-form) differences in the indirect costs of having one's attention to other activities reduced.

Theorem 1. *Assume that individuals choose attention strategies optimally. Define*

$$D(z=1|r) := Pr(z=1|j=1,r) - Pr(z=1|j=0,r).$$

For any r and $\Delta > 0$, average WTP for the BE satisfies

$$\bar{W}(r+\Delta) - \bar{W}(r) = \int_{x=r}^{r+\Delta} D(z=1|x)dx. \tag{3}$$

Moreover, $\bar{W}(r)$ is differentiable almost everywhere, differentiable at any point where $D(z=1|r)$ is continuous, differentiable everywhere if the cost functions $\bar{K}_{ai}^{j}$, $\bar{K}_{oi}$ are strictly convex, and satisfies

$$\frac{d}{dx}\bar{W}(x)|_{x=r} = D(z=1|r). \tag{4}$$

at any point of differentiability. At any pair (p,r) where $Pr(z=1|p,r)$ and $Pr(j=1|p,r)$ are continuously differentiable, the likelihood of choosing technology $j=1$ and the likelihood of completing the task satisfy

$$\begin{aligned}\frac{d}{dr}Pr(j=1|p,r) &= -\frac{d}{dp}Pr(z|p,r) && (5)\\ &= -\frac{d}{dp}Pr(j=1|p,r)D(z=1|r). && (6)\end{aligned}$$

Although we make minimal assumptions about the economic environment and individuals' utility functions, we obtain the sharp characterization of Theorem 1 by utilizing Milgrom and Segal's (2002) envelope theorems for general choice sets. Equation (4) of Theorem 1 states that, if technology $j=1$ increases individuals' likelihood of choosing $z=1$ by, e.g., 10 percentage points under incentive r, then a small increase dr in r should increase individuals' average willingness to pay for $j=1$ by approximately $dr \times 0.1$. Equation (3) is an integral version of equation (4) that does not require differentiability. Appendix A.4 provides an instructive graphical illustration of this result, using standard concepts from competitive supply.

However, the condition in equation (4) requires rich data that is difficult to collect in some field settings, and that we do not have in our first experiment. Equation (5) builds on equation (4) by characterizing how the probability of choosing $j=1$ and the probability of $z=1$ are related to each other. The condition in equation (5) formalizes the basic intuition that if attention is allocated optimally, then increasing the incentives for $z=1$ should increase the desire to adopt a technology that increases the likelihood of $z=1$. But while the qualitative comparative static could be consistent with individuals under- or overvaluing the benefits of BEs, the quantitative condition clarifies exactly how much individuals should seek BEs that increase task completion. The condition in equation (6) is a restatement of the condition in (5) that reveals the connection to equation (4) by utilizing the function $D(z=1|r)$.

The condition in (5) is a variation on the Slutsky symmetry condition that cross-price elasticities of compensated demand functions must be equal to each other, and is analogous to the tests of sparse-max decision making derived in Gabaix (2014). Intuitively, $-\frac{d}{dp}Pr(z=1|p,r)$ is an indication of how adoption of technology $j=1$ affects the probability of choosing $z=1$. In our online education experiment, this derivative is the average impact of our plan-making incentives on the likelihood of completing course modules. The higher this number is, the higher the impact of our plan-making tool on the likelihood of completing the course modules will be. And the higher the impact of the plan-making tool, the higher the impact of a small change in r on its value, as formalized in the first part of Theorem 1. This translates to a higher derivative $\frac{d}{dr}Pr(j=1|p,r)$.[11]

The first condition in Theorem 1 is a limit result in the sense that it applies to marginal changes in the task-completion incentive. Corollary 1 below clarifies how this condition can be used to evaluate "small" but not "vanishing" changes.

Corollary 1. *Assume that agents choose attention technologies optimally. Then*

$$\Delta \min_{x\in[r,r+\Delta]} D(z=1|x) \leq \bar{W}(r+\Delta)-\bar{W}(r) \leq \Delta \max_{x\in[r,r+\Delta]} D(z=1|x). \tag{7}$$

If $D(z=1|x)$ *is smooth on* $[r,r+\Delta]$ *and terms of order* $\frac{d}{dr^2}D(z=1|r)(\Delta^3)$ *are negligible, then*

$$\bar{W}(r+\Delta)-\bar{W}(r) \approx \frac{\Delta}{2}\cdot\left(D(z=1|r)+D(z=1|r+\Delta)\right). \tag{8}$$

Corollary 1 shows that if the change Δ in incentives is sufficiently small, then, roughly speaking, we can take a first-difference approximation to condition (4) by replacing $\frac{d}{dx}\bar{W}(x)|_{x=r}$

[11]Note that any data set that can be used to test condition (4) can be used to test the Slutsky symmetry condition as well. To see this, first note that eliciting individuals' WTP for $j=1$ at incentive r is equivalent to eliciting the demand curve for $j=1$ at incentive r, which means that this data set identifies $Pr(j=1|p,r)$ for all p and for each task-completion incentive r utilized in the experiment. Thus, $\frac{d}{dr}Pr(j=1|p,r)$ and $\frac{d}{dp}Pr(j=1|p,r)$ are identified in this data set. Second, the right-hand-side term of (4), $D(z=1|r)$, is identified by assumption. Thus, all of the statistics necessary to test (6), and therefore also (5), are available. Note, however, that the Slutsky symmetry conditions use strictly less data than condition (4): these conditions consider the function $Pr(j=1|p,r)$ only in a neighborhood around a single incentive level p, while condition (4) considers $Pr(j=1|p,r)$ across all possible values p for which $Pr(j=1|p,r)\in(0,1)$.

with $(\bar{W}(r+\Delta)-\bar{W}(r))/\Delta$. For example, if, as in our second and third experiments, the analysts finds that the change Δ negligibly affects the likelihood of successful task completion, then condition (7) implies that $\bar{W}(r+\Delta)-\bar{W}(r)\approx\Delta D(z=1|r)$ for optimizing agents.

The heuristic approximation above can be further refined in several ways. First, under the assumption that $D(z=1|x)$ is monotonic on $[r,r+\Delta]$, condition (7) shows that $\Delta\min\{D(z=1|r),D(z=1|r+\Delta)\}$ is a robust lower bound for $\bar{W}(r+\Delta)-\bar{W}(r)$, which is helpful for analysis attempting to rigorously show that $\bar{W}(r+\Delta)-\bar{W}(r)$ is too low relative to the optimizing benchmark. Second, under the assumption that $D(z=1|x)$ is locally linear on $[r,r+\Delta]$—which is a justifiable assumption whenever the impacts of Δ are "small—condition (8) provides a better approximation to $\bar{W}(r+\Delta)-\bar{W}(r)$. [12]

1.3.1 Remarks and Qualifications

Differences in fixed costs, $\bar{K}_i^1(0)-\bar{K}_i^0(0)$, may result from the potential nuisance costs of attention-improving technologies, which is consistent with negative WTP for reminders by some individuals in our second experiment. Thus, the value of a BE cannot be equated with its impact on the change in expected earnings, $rPr(z=1)$. Simply documenting that, for example, individuals' valuations for a reminder that increases their chance of earning \$10 by 10% is smaller than \$1 is not a rejection of correct valuation of the reminder, because nuisance costs could decrease the value of the reminder. Our more robust test focuses instead on how individuals' valuations of the BE change as the pecuniary incentives for being attentive change.

Second, note that condition (4) is a test of whether individuals correctly value the BE *on the margin*: whether individuals correctly perceive the effects of the BE at the current incentive level r. The statistic $\bar{W}(r)$, however, captures individuals' perceptions of the difference in total costs, which includes $K_i^1(0)-K_i^0(0)$. The impact on $\bar{W}(r)$ of additional treatments like opportunities for learning and feedback—as in our third experiment—can provide some insight about biases in the perception of total costs, and how those might differ from biases about the effects of the BE on the margin.

Third, note that we have assumed that utility is quasi-linear in the financial incentives. This is a plausible assumption for the small stakes featured in our experiment, as non-negligible deviations from this assumption would imply implausible levels of risk aversion for higher incentives (Rabin, 2000). Appendix E shows that incorporating standard estimates of risk aversion in our model negligibly impacts the quantitative implications of Theorem 1,

[12] Note that Δ need not necessarily be small. What is important is that $D(z=1|r+\Delta)-D(z=1|r)$ is small. For example, if this statistic is zero, then the condition in Corollary 1 is exact even for large Δ.

and thus does not confound our empirical conclusions.

1.4 Empirical Tests of Theorem 1 and Their Interpretation

A failure to verify the optimality conditions in Theorem 1 could result for the following reasons, which we discuss in turn below.

E1. Individuals mischaracterize (either due to incorrect beliefs or other forms of bounded rationality) their attention production process. That is, they mischaracterize $Q(s_{a,}s_o,\omega)$ or $K_i(s_{a,}s_o,\omega)$ on a positive measure of states ω.

E2. Individuals have biased priors μ.

E3. In a given experiment, the states ω are not realized independently across individuals. For example, there is uncertainty about a fixed parameter of the economic environment.[13]

Deviations due to E1 or E2 imply systematic misperceptions. Systematic biases have been documented in a variety of domains of decision making, including other types of costly effort decisions (e.g., DellaVigna and Pope, 2017; Hoffman and Burks, 2020). These explanations are also equivalent in the simplified representation (2), as both lead to biased beliefs about the aggregate cost functions $\bar{K}^j$. In experiments such as our first two, it is difficult to differentiate between E1 and E2 because the state space is not specified or observed by the analyst. In our third experiment, which follows standard protocols that test rational inattention models, E2 may be less likely as the experimenter specifies the state space, the prior distribution over states, and the mapping from states and actions to financial rewards. The only way in which E2 can apply to such experimental protocols is if the state space is, contrary to the experimenter's efforts, richer than the one specified by the experimenter, for reasons we discuss below in reference to E3.

Deviations due to E3 do not necessarily involve systematic biases. To formalize E3, let ξ denote an environmental parameter such that the effects of the BE are given by $D(z=1|r,\xi)$. Let each individual i receive a signal ζ_i about $D(z=1|r,\xi)$, which is affiliated with ξ according to some joint distribution $H(\zeta,\xi)$, with marginals H_ζ and H_ξ. Suppose that individuals have an unbiased prior about the effects of the BE, centered around $\bar{D}(z=1|r) := \int D(z=1|r,\xi)dH_\xi(\xi)$, the average effect of the BE across the different possible environments. Then,

[13] We thank an anonymous referee for pointing out this mechanism and motivating our discussion of it.

if individuals are Bayesian, the martingale property of beliefs implies that individuals' posterior beliefs after receiving signal ζ_i, the perceived effect of the BE, $\tilde{D}_i(z=1|r,\zeta_i)$, must also be unbiased on average: $\mathbb{E}_{i,\xi}\left[\tilde{D}_i(z=1|r,\zeta_i)\right]=\bar{D}(z=1|r)$.

For example, ξ could capture environmental features that determine the efficacy of a reminder. As shown in our second experiment, reminders have larger effects when the task is further off into the future. Individuals may have a correct prior about how effective reminders are on average, but may not have learned how effective reminders are for a specific task-completion delay. In our third experiment, individuals may have a correct understanding of how generally difficult it is to identify whether there are more correct or incorrect arithmetic calculations, based on similar tasks they have done in the past, but there may be correlated uncertainty about exactly how much more difficult it is to complete the task with one hundred rather than ten equations. Formalizing this type of uncertainty requires specifying a richer state space.

An analyst applying Theorem 1 to an experiment that features only one particular realization of ξ would fail to verify the the condition of the Theorem, since individuals' decisions would satisfy $\frac{d}{dr}\bar{W}(r)=\mathbb{E}_i\left[\tilde{D}_i(z=1|r,\zeta_i)|\xi\right]$ rather than $\frac{d}{dr}\bar{W}(r|\xi)=D(z=1|r,\xi)$. Concretely, suppose that individuals have a normal prior with variance σ_0^2, and that they receive a signal about $D(z=1|r,\xi)$ that is normally distributed around $D(z=1|r,\xi)$ with variance σ_1^2. Then individuals' perceptions, and thus by Theorem A.1 their WTPs, satisfy

$$\begin{aligned}\frac{d}{dr}\bar{W}(r|\xi) &= \mathbb{E}_i\left[\tilde{D}_i(z=1|r,\zeta_i)|\xi\right]\\ &= (1-\theta)\bar{D}(z=1|r)+\theta D(z=1|r,\xi),\end{aligned} \tag{9}$$

where $1-\theta=\sigma_1^2/(\sigma_0^2+\sigma_1^2)$ is the degree of Bayesian shrinkage toward the prior mean $\bar{D}(z=1|r)$. This implies that individuals undervalue the BE when it is more effective than average, in the sense that $D(z=1|r,\xi)>\bar{D}(z=1|r)$. But individuals also overvalue the BE when it is less effective than average, in the sense that $D(z=1|r,\xi)<\bar{D}(z=1|r)$. Thus, showing that individuals overestimate or underestimate the effects of a BE in one particular decision environment does not imply that the miscalibration is systematic.

Importantly, if the assumption that $\bar{D}(z=1|r)$ is an unbiased prior mean is relaxed, then equation (9) is mathematically equivalent to a "meta-inattention" model in the spirit of the attribute-misperception model of Gabaix (2014). In this interpretation, $\bar{D}$ is some default perception that people "anchor" on. Thus, equation (9) is a convenient parametrization that can also capture systematic biases as in E1 and E2.

Estimating equation (9) can provide at least suggestive evidence for differentiating between E3 and systematically-biased perceptions. In data sets where there is exogenous variation in conditions ξ that generate variation in the efficacy of the BE, equation (9) can be estimated simply through the linear regression model

$$\frac{d}{dr}\bar{W}(r|\xi) = \beta_0 + \beta_1 D(z=1|r,\xi). \tag{10}$$

The coefficient β_1 identifies θ, which implies that $\bar{D}(z=1|r) = \beta_0/(1-\beta_1)$. That is, $\beta_0/(1-\beta_1)$ is a sufficient statistic for the behavioral implications of the prior μ, including any possible biases in the prior. To illustrate how the coefficients can help differentiate between E3 and the systematic biases in E1 and E2, suppose that it is known that the effects of reminders are generally non-negative.[14] Then detecting strongly positive effects in some conditions suggests that $\bar{D}(z=1|r) > 0$, and thus a finding that $\beta_0 = 0$ suggests that people systematically underestimate the effects of reminders because they anchor on the erroneous default perception of null effects.

Implications for Models of Costly Attention To summarize, failure to verify the conditions of Theorem 1 generates several possible implications. One is that individuals are systematically mis-calibrated about their attention cost functions or have biased priors (E1 and E2). A different possibility, as captured by E3, is that even in highly-controlled empirical studies where the experimenter attempts to specify the states and probabilities—such as our third experiment—the individuals' subjective state space is richer than what has typically been assumed. Given that state-dependent stochastic choice (SDSC) data is a key empirical object for testing and estimating rational inattention models (Caplin and Dean, 2015; Caplin, 2016), this possibility raises intriguing challenges for this agenda.

Direct Versus Indirect Reasons for Misperceptions of BE Effects Individuals might misperceive the value of BEs for direct reasons—misperceiving $\bar{K}_a^1 - \bar{K}_a^0$—or due to indirect reasons—misperceiving $\bar{K}_o$ in economic environments that involve auxiliary actions. The latter possibility is not implicated in our third experiment, but is in principle possible in our first two experiments. If individuals underestimate $\bar{K}_o$, then they will overestimate their optimal choice of q_o, and thus *over*estimate the returns to higher attention and thus to the BE. The converse holds if individuals underestimate $\bar{K}_o$. However, because most plausible biases about the costs of auxiliary actions in our first two experiments—such as underestimating how busy one is in the future (e.g., the *planning fallacy* articulated in Kahneman and Tver-

[14] I.e., an unbiased prior puts little weight on negative effects.

sky, 1982)—lead people to underestimate $\bar{K}_o$, under-valuation of BEs in these experiments cannot be plausibly explained without direct misperceptions of $\bar{K}_a^1 - \bar{K}_a^0$.

2 Online Education Experiment

Our first experiment was designed around the Slutsky symmetry test in equation (5) of Theorem 1. It was run in the fall of 2018. We partnered with Code Avengers, an online platform for learning to code, to offer participants a free, eight-week course in three different programming languages (HTML/CSS, Javascript, and Web Dev).[15] Screenshots of all experimental instructions are in Screenshots Appendix F.1.

2.1 Design and Implementation

2.1.1 Participant Pool

We recruited students and recent alumni from six Philadelphia-area colleges using an email campaign. Enrollees were eligible to be included in our study if they reported in the onboarding survey that they regularly used either Google Calendar or Apple's iCal as an electronic calendar. Perhaps due to the relative youth of the participant pool, usage rates were high, at around 60–70 percent. Recruitment resulted in a pool of 1373 study-eligible participants.[16]

2.1.2 Implementation

Just before the 8-week course began, participants received an introductory email with information on their treatment assignment. This email also contained a recommendation that participants aim to complete three, 15-minute sessions of the coding course per week, a prompt to encourage participants to make a plan for when they would do the coding lessons, and a link to make plans for working on the coding lessons, which would be created in their electronic calendars. Participants who were eligible for financial rewards were informed that they would be paid their cumulative earnings in the form of an Amazon gift card at the end of the 8-week period.

Over 90 percent of participants opened the initial emails informing them of the incentives they faced (i.e., their treatment), giving us confidence that most were aware of the incentives

[15]These languages are commonly used tools for building modern web sites. See http://www.codeavengers.com for more details.

[16]Appendix Table A.1 presents characteristics of the participant pool. Females, first-years, and seniors were most likely to participate.

for which they were eligible. As expected from random assignment of treatment, email opening rates were very similar across treatments, ranging from 88 to 91 percent.

After the course had begun, all participants received a reminder email at the start of each week. The reminder email contained the same recommendation, planning prompt, and link to create plans as the initial email.

2.1.3 Experimental Design

The experiment consisted of a control group and five treatment arms, with varying levels of incentives for plan making and/or coding task completion. Participants assigned to the control group received the initial and reminder emails encouraging them to plan and complete the coding lessons and offering them the plan-making tool, but they were not eligible for financial rewards.

Those randomly assigned to the two *Pay-to-Plan* treatments received either $1 or $2 for making a plan for when to do their coding lessons that week (i.e., clicking the plan-making link within the weekly email). In the two *Pay-to-Code* treatments, participants received either $2 or $5 for completing three 15-minute sessions of the coding course during the week. Finally, participants in the *Combination* treatment arm were paid $1 for making a plan plus $2 if they completed three 15-minute sessions of the coding course during the week. Participants could earn these amounts each week, regardless of what they had done in previous weeks. In addition, making a plan did not restrict when a participant could do the coding lessons (i.e., participants in the *Pay-to-Code* and *Combination* treatments could complete the 15-minute sessions at any time during the week and still earn their coding-task incentives, regardless of whether or not they made a plan or when they had scheduled the three 15-minute sessions).

To measure plan making, we tracked whether a participant clicked on the provided plan-making link to create calendar events for when they planned to complete the 15-minute coding sessions.[17] Consistent with our theoretical framework, this observable plan-making is not the only available bandwidth enhancement (BE), or even the only available plan-making opportunity. For example, some participants might have other means of making plans or might directly edit their calendars without using our link. However, nearly 40 percent of the control group clicked to make a plan in the first week, despite receiving no financial rewards for doing so, and participants with higher incentives for completing the coding task were more likely to use the plan-making tool, implying that our plan-making tool was not

[17] When participants clicked on the plan-making link, they were given three default times, which they could change. This default ensured that as long as a participant clicked on the link, a calendar event would be created.

a perfect substitute for the plan making individuals would do otherwise.[18] This may be because the act of making a plan by using our link generates an internal cue, as theorized in the implementation intentions literature (Gollwitzer and Sheeran, 2006).

To measure completion of the coding coursework, we received real-time, backend data from Code Avengers on the number of minutes participants spent actively working on their coding coursework each day. The session timer stopped running after approximately 30 seconds of inactivity within the course. Once they had completed 15 minutes of active work, participants were notified with a pop-up that congratulated them but did not prevent or discourage them from continuing.

2.2 Results

2.2.1 Empirical Framework

Our primary analysis focuses on measuring the effect of plan-making and coding-task incentives on plan making and coding task completion. We estimate treatment effects using regressions of the form:

$$y_{ict} = \beta T_{ict} + \alpha_c + \alpha_t + \gamma X_i + \varepsilon_{ict}, \tag{11}$$

where y_{ict} measures either plan making or completing at least $\tau \in \{0, 10, 20, 30, 40, 45, 50, 60\}$ minutes in week t for participant i at campus c. We include fixed effects α_c for campus interacted with student status (i.e., current student or alumni), which was the level at which we randomized. We also control for course week α_t and a vector of participant characteristics X_i, but random assignment implies that these additional controls should not affect our estimated treatment effects. Our preferred measure of treatment T_{ict} is value in dollars of the participant's incentive, which assumes a linear relationship between the incentive and behavior. We also consider a specification with indicators for different incentive sizes. We estimate regressions separately for the Pay-to-Plan sample, which includes the control group and the two *Pay-to-Plan* treatments, and the Pay-to-Code sample, which includes the control group and the two *Pay-to-Code* treatments.

[18] Our theoretical framework only requires that the plan-making tool we offer is not a perfect substitute to other forms of planning individuals already undertake. Heterogeneity in attention cost functions accommodates the possibility that some participants who use our plan-making tool simply substituted from creating their own calendar reminders while others who use our plan-making tool would not have created a plan themselves.

2.2.2 Plan-making Incentives

In Table 1, we estimate the impacts of plan-making incentives on plan making and on coding task completion. In the context of our model, these specifications measure $\frac{d}{dp}Pr(z=1|p,r)$ and $\frac{d}{dp}Pr(j=1|p,r)$. The analysis sample includes 705 participants and eight pooled weekly observations per participant. In Panel A, we estimate the effect of plan-making incentives on the propensity to plan in week 1, weeks 1 to 4, and weeks 1 to 8. Multiple-week outcomes average the indicator for whether a participant made a plan (or completed the coding task) in each week. In Panel B, we estimate the effect of plan-making incentives on the propensity to complete at least 20 minutes or at least 45 minutes of coding during week 1, weeks 1 to 4, and weeks 1 to 8, respectively. Although our financial incentives were specifically for completing at least 45 minutes of the coding task (i.e., the three 15-minute sessions), we also include the 20-minute benchmark in the main tables and text to show robustness. Appendix Tables A.2 and A.3 consider other time thresholds: 0, 10, 30, 40, 50, and 60 minutes per week. Our interpretation of the results is consistent with the evidence from these alternative thresholds.

The results indicate strong impacts of plan-making incentives on plan making, and modest impacts of plan-making incentives on coding task completion. For each \$1 of plan-making incentive, participants increase their plan making by 11.6 percentage points (se = 1.3) on average over the eight weeks of the study, an increase of 140% relative to the control group mean of 8.2 percentage points. Plan-making effects are 18.0 percentage points (se = 2.0) in week 1, and 14.2 percentage points (se = 1.4) on average over weeks 1 to 4, which suggests an attenuated response over the course of the study. However, the control mean falls even more quickly, from 38.1% in week 1, to 15.0% in the first four weeks, to 8.2% over the full study, such that the relative impact of plan-making incentives increases over time. Panel A of Appendix Table A.4 shows the effects of the \$1 and \$2 plan-making incentives separately.

The treatment effect of plan-making incentives on coding task completion is more modest but still meaningful. Focusing on course completion of at least 45 minutes a week, we find that \$1 of plan-making incentive increases coding task completion by 3.8 percentage points (se = 1.8) in week 1, an increase of 22% relative to the control group mean of 17.4 percentage points. However, the effect declines to a marginally significant 1.7 percentage points (se = 1.2) over weeks 1 to 4, and to a statistically insignificant 0.6 percentage points (se = 0.9) over weeks 1 to 8. In Panel C, we combine the plan making and coding task completion estimates in an instrumental variables estimation of the effect of plan making on

Table 1: The Effect of Plan-Making Incentives on Plan Making and Task Completion

A. The Effect on Plan Making (First Stage)

	(1) Week 1	(2) Weeks 1-4	(3) Weeks 1-8
Plan Incentive	0.180*** (0.020)	0.142*** (0.014)	0.116*** (0.013)
Obs.	705	705	705
R^2	0.137	0.163	0.131
Control Mean	0.381	0.150	0.082
Controls	Yes	Yes	Yes
Campus FE	Yes	Yes	Yes

B. The Effect on Coding Task Completion (Reduced Form)

	(1) >20 (1)	(2) >20 (1-4)	(3) >20 (1-8)	(4) >45 (1)	(5) >45 (1-4)	(6) >45 (1-8)
Plan Incentive	0.040** (0.020)	0.028** (0.013)	0.013 (0.011)	0.038** (0.018)	0.017 (0.012)	0.006 (0.009)
Obs.	705	705	705	705	705	705
R^2	0.057	0.049	0.051	0.036	0.035	0.041
Control Mean	0.280	0.212	0.158	0.174	0.156	0.116
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Campus FE	Yes	Yes	Yes	Yes	Yes	Yes

C. The Effect of Plan Making on Coding Task Completion (IV)

	(1) >20 (1)	(2) >20 (1-4)	(3) >20 (1-8)	(4) >45 (1)	(5) >45 (1-4)	(6) >45 (1-8)
Plan Making	0.221** (0.105)	0.194** (0.087)	0.114 (0.086)	0.213** (0.096)	0.118 (0.076)	0.053 (0.074)
Obs.	705	705	705	705	705	705
R^2	0.147	0.174	0.133	0.092	0.120	0.085
Control Mean	0.280	0.212	0.158	0.174	0.156	0.116
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Campus FE	Yes	Yes	Yes	Yes	Yes	Yes

This table estimates the effect of plan-making incentives ("Plan Incentive") on plan making and coding task completion. Panel A shows the effect of plan-making incentives (in dollars) on whether participants made a plan. Column (1) shows the effect of plan-making incentives in week 1 of the experiment. Column (2) shows the average effect for the weeks 1–4. Column (3) shows the average effect for all weeks. Panel B shows the effect of plan-making incentives on coding task completion. Columns (1-3) show the effect on an indicator variable for whether or not the participant worked on the task for more than 20 minutes: Column (1) estimates the effect over week 1, Column (2) over weeks 1–4, and Column (3) over all weeks. Columns (4-6) show analogous estimates for an indicator for whether or not the participant worked on the task for more than 45 minutes. Panel C shows the 2SLS estimates instrumenting for whether participants made a plan using the plan-making incentive as an instrument. The dependent variables are the same as in Panel B. Standard errors are in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

coding task completion. Making a plan increases the probability of coding task completion by 21 to 22 percentage points in week 1, an 81% to 124% increase relative to control group means. This large effect is precisely estimated for week 1 and weeks 1 to 4 but diminishes over the full experimental period. Overall, the results point to the value of plan making for people who have some intrinsic motivation to complete the coding sessions. Panel B of Appendix Table A.4 shows the effects of the $1 and $2 plan-making incentives separately.

The decrease in treatment effects over time is not surprising, as many participants appear to attrit out of the coding course. Appendix Figure A.2 plots control group means for plan-making and coding task completion over the weeks of the experiment. Engagement in the first two weeks of the study is relatively high in the absence of monetary incentives—control group participation hovers between 20 and 30 percent. However, many participants disengage from both the plan-making tool, which falls close to zero by week 3, and from continuing the coding course, which falls to 10 percent participation by week 5, suggesting that motivation for the coding course diminished over time. In the context of our model, this implies that participants' estimates of $B(r)$ diminished as participants received additional signals about the course.[19]

2.2.3 Coding-task Incentives

Table 2 estimates the impacts of coding-task incentives on plan making and coding task completion. In the context of our model, these specifications measure $\frac{d}{dr}Pr(z=1|p,r)$ and $\frac{d}{dr}Pr(j=1|p,r)$. The analysis sample includes 714 participants and eight pooled weekly observations per participant. Following the structure of Table 1, in Panel A we estimate the effect of coding-task incentives on the propensity to plan in week 1, weeks 1 to 4, and weeks 1 to 8. In Panel B, we estimate the effect of coding-task incentives on the propensity to complete at least 20 minutes or at least 45 minutes of coding during week 1, weeks 1 to 4,

[19] Note that this by itself does not imply a deviation from optimal Bayesian decision making. As a simple illustration, suppose that for each participant, the beliefs about $B(r)$ take the form of a Bernoulli random variable that takes on the values $\bar{B}>0$ with probability 0.2 and $\underline{B}<0$ with probability 0.8, such that $0.2\bar{B}+0.8\underline{B}>0$. Then participants would initially sign up given the positive expectation of $B(r)$. But if the realizations of B are independently distributed across participants, 80 percent of them would attrit after discovering that $B=\underline{B}$. And if the realizations of B are positively correlated across participants, then in "bad" states the number of participants attriting could be much larger than what participants initially expected. For example, if the realizations are perfectly correlated, and the prior is that $Pr(B=\bar{B})=0.8$, then in the state $B=\underline{B}$ the participants who attrit will have ex-ante expected to complete the course with 80 percent chance. This does not pose a threat to our theoretical results about tests of optimal valuation of BEs because the experiment involves weekly measures of engagement with the planning tool. Thus, even if participants initially overestimated their enthusiasm about the course, they had the opportunity to adjust those expectations before the next planning decision.

and weeks 1 to 8, respectively.

Coding-task incentives have substantial effects on coding task completion, as shown in Panel B. We estimate that each $1 of coding-task incentive increases completion rates for 45-minutes in week 1 by 3.5 percentage points ($se = 0.8$), an increase of 20% relative to the control group mean of 17.4 percentage points. For the $2-incentive and $5-incentive groups,

Table 2: The Effect of Coding-Task Incentives on Plan Making and Task Completion

A. The Effect on Plan Making

	(1) Week 1	(2) Weeks 1-4	(3) Weeks 1-8
Task Incentive	0.025***	0.010**	0.007**
	(0.009)	(0.004)	(0.003)
Obs.	714	714	714
R^2	0.050	0.058	0.049
Control Mean	0.381	0.150	0.082
Controls	Yes	Yes	Yes
Campus FE	Yes	Yes	Yes

B. The Effect on Coding Task Completion

	(1) >20 (1)	(2) >20 (1-4)	(3) >20 (1-8)	(4) >45 (1)	(5) >45 (1-4)	(6) >45 (1-8)
Task Incentive	0.038***	0.031***	0.025***	0.035***	0.024***	0.020***
	(0.009)	(0.006)	(0.005)	(0.008)	(0.006)	(0.005)
Obs.	714	714	714	714	714	714
R^2	0.043	0.059	0.069	0.041	0.057	0.075
Control Mean	0.280	0.212	0.158	0.174	0.156	0.116
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Campus FE	Yes	Yes	Yes	Yes	Yes	Yes

This table estimates the effect of coding-task incentives ("Task Incentive") on plan making and coding task completion. Panel A shows estimates of the effect of coding-task incentives (in dollars) on whether or not participants made a plan. Column (1) shows the effect of coding-task incentives week 1 of the experiment. Column (2) shows the average effect over weeks 1–4. Column (3) shows the effect over all weeks. Panel B shows the effect of coding-task incentives on coding task completion. Columns (1-3) show the effect on an indicator variable for whether or not the participant worked on the task for more than 20 minutes: Column (1) estimates the effect over week 1, Column (2) over weeks 1–4, and Column (3) over all weeks. Columns (4-6) show analogous estimates for an indicator for whether or not the participant worked on the task for more than 45 minutes. Standard errors are shown in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

this coefficient implies an increase in the probability of coding task completion of 7 and 17.5 percentage points, respectively, or 40% and 101% relative to the control mean of 17.4 percentage points. Again, the treatment effects diminish over time to 2.4 percentage points ($se = 0.6$) per \$1 over weeks 1 to 4, and to 2.0 percentage points ($se = 0.5$) per \$1 over the eight weeks of the study.[20]

A more novel result is that coding-task incentives also increase the probability of plan making, as shown in Panel A. Column 1 shows that for each \$1 of coding-task incentive, participants increase their plan making by 2.5 percentage points ($se = 0.9$) in week 1, by 1.0 percentage point ($se = 0.4$) in weeks 1 to 4, and by 0.7 percentage points ($se = 0.3$) over the eight weeks of the study. Relative to the control group means of 38, 15, and 8 percentage points, these correspond to plan making increases of 6.6%, 6.7%, and 8.5% per \$1 of plan-making incentive.

2.2.4 Symmetry Test

Participants clearly recognize the potential value of plan making in helping them achieve their coding course participation. But do they value plan making enough? To answer this question, we compare the cross-price elasticities estimated in the Pay-to-Plan and Pay-to-Code samples, implementing the test in Equation (5) of Theorem 1. The coefficients for \$1 of plan-making incentives on coding task completion are 0.039, 0.017, and 0.006 in week 1, weeks 1 to 4, and weeks 1 to 8, respectively. The analogous coefficients for \$1 of coding-task incentives on plan making are 0.025, 0.010, and 0.006. The difference in coefficients provides our first test of under-planning, delivering estimates of 0.014 ($se = 0.019$), 0.007 ($se = 0.012$), and -0.0004 ($se = 0.009$), respectively.[21] The positive sign of the differences, particularly in the early weeks of the study, hints at the possibility that participants might undervalue plan making. However, the standard errors are too wide to draw strong conclusions from this data about whether participants plan optimally.

Figure 1 plots week-by-week coefficients for plan-making and coding-task incentives to

[20] We exclude the *Combination* treatment from our main analysis and separately evaluate whether this treatment exhibits complementarity effects (i.e., whether combining a \$1 plan-making incentive with a \$2 coding-task incentive induces plan making or coding effects that are significantly different from the \$1 *Pay-to-Plan* or \$2 *Pay-to-Code* treatments in isolation). For weeks 1 to 8, the *Combination* treatment effect on plan making is 26.7 percentage points ($se = 2.6$) compared to 23.9 percentage points ($se = 2.7$) for the \$1 *Pay-to-Plan* treatment (p-value of difference = 0.31). The *Combination* treatment effect on average course completion is 3.8 percentage points ($se = 2.4$) compared to 4.6 percentage points ($se = 2.1$) for the \$2 *Pay-to-Code* treatment (p-value of difference = 0.72). Thus, we find no statistically significant complementarity effect of the *Combination* treatment.

[21] Standard errors for coefficient differences are estimated via seemingly unrelated regression.

illustrate how the effect of incentives evolves over the course of the experiment. The effect of coding-task incentives on plan making is consistently close to zero (after week 1) and tightly estimated. In contrast, the effect of plan-making incentives on coding task completion is positive for the first half of the study and then decays toward zero, with relatively wider confidence intervals.[22] This provides suggestive evidence of under-planning.

Findings from Experiment 1. *Take-up of our plan-making tool increased with incentives for completing coding lessons, but the ratio of cross-price effects for plan-making and task completion suggests that participants undervalued plan-making. The impact of task-completion incentives on planning-prompt demand was 74% as large as the ex-post optimal benchmark implies.*

3 Online Survey-completion Experiment

Complementing our first experiment, we ran a survey-completion experiment on Amazon's Mechanical Turk platform (MTurk). The experiment is tightly tied to the test in equation (4) of Theorem 1, described in Section 1. The test states that for individuals who optimally invest in bandwidth enhancements (BEs), a $1 increase in the incentive for task completion must increase willingness to pay for such a technology by $1 times its efficacy (i.e., by the change in the probability of task completion due to the BE). Screenshots of all experimental instructions are in Screenshots Appendix F.2.

3.1 Design and Implementation

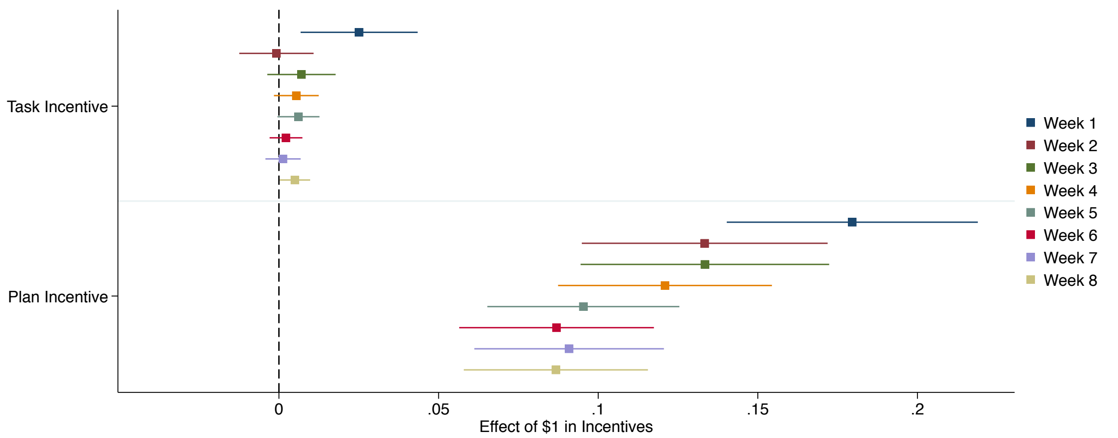
Participants were recruited to complete part 1 of the study each weekday between September 7 and September 24 of 2021. Our recruitment material informed potential participants that part 1 of the study would require 15 minutes of time immediately (for which participants were paid a guaranteed $2.50 and had the possibility of earning a bonus), and that they would be invited to participate in part 2 of the study at a later date for additional compensation by accessing a website provided to them in part 1 of the study.

When participants clicked to begin the study, they were told that part 2 of the study—a survey that needed to be completed in one sitting of approximately 20 minutes—would only be available starting on some day in the future to be randomly determined during part 1. Participants were told that they would have a one-week window to complete it.

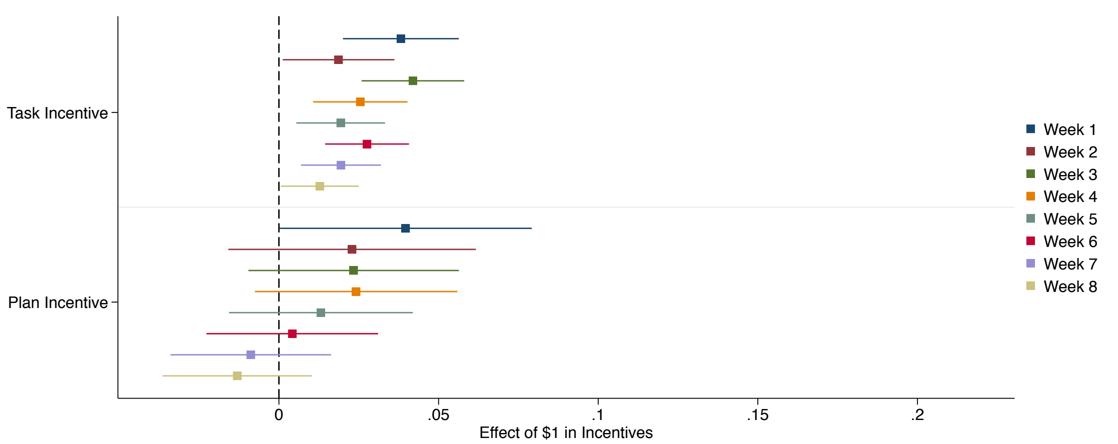
[22]The difference in standard errors across treatments is due to higher variance in coding-task incentives ($0, $2, and $5) relative to plan-making incentives ($0, $1, and $2).

Figure 1: The Effect of Incentives on Plan Making and Coding Task Completion

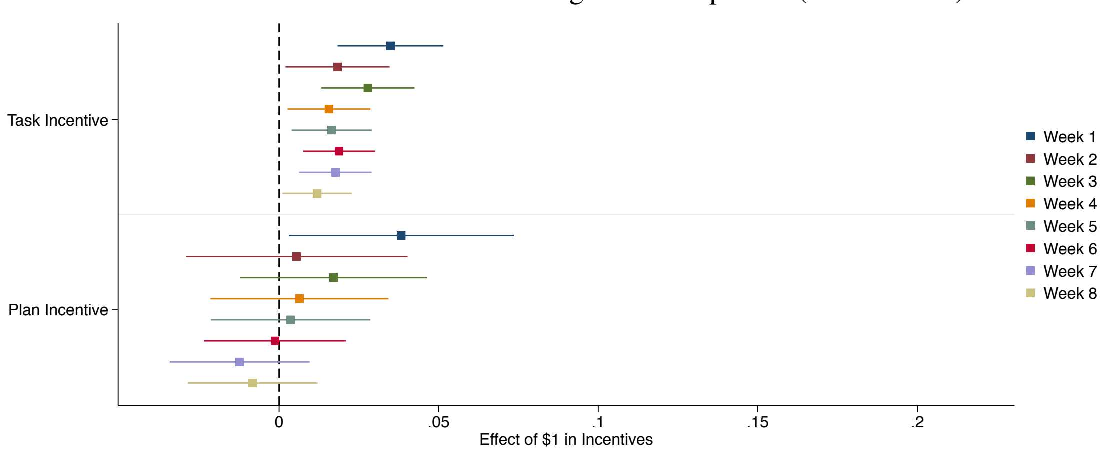
A. The Effect of Incentives on Plan Making



B. The Effect of Incentives on Coding Task Completion (>20 Minutes)



C. The Effect of Incentives on Coding Task Completion (>45 Minutes)



This figure shows estimates for the effect of incentives on plan making and coding task completion for each week of the study. Panel A shows estimates of the effect of incentives on whether or not participants made a plan. Panels B and C show the effect of incentives on completing at least 20 minutes and at least 45 minutes, respectively, of coding during the week. Whiskers report 95% confidence intervals around each estimate.

The first part of the study elicited participants' willingness to pay (WTP) for a set of three reminder emails (i.e., the BE in this experiment) that would come during the one-week window in which participants would be able to complete the survey. The goal was to generate data that would allow us to directly measure how much more participants were willing to pay for reminder emails as the incentive to complete the survey increased.

To ensure that participants understood the specific details of the reminder emails, we explained that the emails would come at 12 p.m. ET on the first, middle, and final days of the one-week window in which they could complete the survey.[23] Participants were told that emails would be sent using the MTurk email system—which MTurk uses for communicating with workers on its platform—so participants did not have to provide an email address and so the reminder emails would be unlikely to go to spam. Participants were told that the link to the survey would be included in the reminder emails so that initiating the survey would be as easy as clicking a link in the email. Participants were also explicitly told that they would not receive any reminders to complete the survey unless they were selected to receive these three reminder emails. We also clarified what the part-2 survey would look like (i.e., answering 40 hypothetical questions about gambles), and provided two example questions, in order to reduce ambiguity about the future tasks.[24]

Participants were informed that the survey would only be available starting in either 2 days, 1 week, 3 weeks, or 6 weeks, and that each delay was equally likely to be selected. Participants also learned that their incentive for completing the survey would be either \$3, \$4, \$11, or \$12. For ease of exposition, we refer to \$3 and \$4 as *low incentives* and \$11 and \$12 as *high incentives*. For each of the 16 combinations of the four possible incentive amounts and four possible delays, participants faced an incentivized multiple price list (MPL) that traded off part-1 bonus payments (up to \$4, in 25-cent increments, for the low-incentive MPLs and up to \$12, in 75-cent increments, for the high-incentive MPLs) against being sent the three reminder emails to complete the survey. Participants were informed that all possible bonus rewards, including part-1 and part-2 bonus payments, would be paid at the same point in time, after the one-week window to complete the survey ended.[25] Participants

[23]For example, for participants who completed part 1 of the study on September 7, the 2-day-delay window was open from September 9–15. To any participants in the 2-day-delay group who were selected to receive reminder emails, we sent the emails at 12 p.m. ET on September 9, 12, and 15.

[24]In order to participate in the study, participants needed to correctly answer questions demonstrating their understanding of the compensation structure, the tasks in part 2 of the study, and the conditions for receiving reminders (i.e., they had to answer "True" to the statement: "You will not receive any reminders to complete part 2 of the study unless you are selected to get them in this part of the study."). Participants were also shown an MPL attention check screen that was used to remove participants who might click through the MPLs without reading the instructions.

[25]Part-1 bonus payments were paid out at the same time as any part-2 bonus payments, three days after

were randomly selected to either first respond to the eight low-incentive MPLs (the "low-incentive block") or to the eight high-incentive MPLs (the "high-incentive block"). Within each block, the eight MPLs were shown to participants in a random order.

Because nuisance costs can lead participants to have negative WTP for the reminders, the MPL allowed participants to report both positive and negative willingness to pay for the reminder emails.[26] Participants were told that whichever incentive amount was randomly selected for them ($3, $4, $11, or $12) would be the bonus they would receive for completing the survey. In addition, they were told that for the randomly selected incentive amount, there was a 10% chance that one of the rows of that MPL would be randomly selected (each with equal probability) and that whatever the participant chose in that row would be implemented (i.e., they would receive whatever part 1 bonus payment was indicated in their choice, and they would receive the reminder emails if they chose the option on the left). Because testing the optimality conditions in Theorem 1 also requires estimating the effect of the reminder emails on completing the survey, we did not guarantee that one of the MPL rows would be selected. Instead, we randomized 45% of participants to receive the reminder emails and 45% of participants not to receive the reminder emails, regardless of their MPL choices. We use this random variation to estimate the effect of reminder emails on completing the survey. We randomly assign reminder emails in this way, and estimate the effect of reminders using this sample, in order to avoid potential selection bias that might arise if there were a correlation between WTP for reminders and the rate at which individuals completed the survey.

the end of the one-week window to complete the survey, mitigating concerns that part-1 bonuses would be viewed as being paid immediately, which might have made them particularly valuable from the perspective of a quasi-hyperbolic discounter.

[26]Consistency on an MPL requires a participant to always choose the option on the left, always choose the option on the right, or switch from choosing the option on the left to choosing the option on the right in one row of the MPL. Our MPL was implemented to allow participants to choose a single cross-over point, thus enforcing consistency in choices. Use of single-cross-over MPLs is common in the experimental literature as they make the decision faster and easier for participants. The main concern is failing to identify participants who are clicking randomly through the study (i.e., those who would likely be identified as inconsistent on the MPL if required to make a selection in each row). This concern is mitigated in our setting because of our extensive attention checks.

3.2 Results

3.2.1 Sample

A total of 2743 individuals fully completed the first part of the study.[27] Additionally, we make the conservative sample restriction to limit all of our analysis to individuals whose WTPs were never top-coded at the smaller incentive (i.e., \$3 or \$11) or bottom-coded at the larger incentive (i.e., \$4 or \$12).[28] Mechanically, these top-coded and bottom-coded individuals cannot increase WTP when the task-completion incentive rises, which could lead to an attenuation bias in our estimates of how WTP for reminders changes with task-completion incentives. Given the wide range of values offered in the MPL, only 8.37% and 8.12% of responses were top-coded on the low-incentive and high-incentive MPLs, respectively, and 0.80% and 0.54% of responses were bottom-coded on the low-incentive and high-incentive MPLs, respectively. In what follows, we report on data from the remaining 2306 participants. Our restriction is conservative because it can only increase our estimates of how WTP for reminders varies with incentives; indeed, without this restriction, the point estimates are slightly lower.

3.2.2 Impact of Reminders on Survey Completion

As described in Section 3.1, we randomized 90% of participants to either get or not get the reminder emails, regardless of their reported WTP. This randomization allows us to generate an estimate of the effect of the reminders on survey completion at each delay. In addition, since we independently randomized the incentive level for completing the survey, we can estimate the effect of reminders at low and high incentive levels.

Figure 2 presents this data. Panel A shows the rate at which participants complete the survey at each delay, and by whether participants receive reminders. Panel B summarizes the treatment effect of receiving reminders at each delay and incentive level. Without reminders, completion rates decrease with delay at both high and low incentives. With re-

[27] This number does not include the 1854 participants who were automatically screened out of the study (and prevented from participating further) because they failed attention checks, ensuring our pool of participants understood the instructions in our experiment. It also excludes 36 individuals who were excluded for having an invalid MTurk ID or the 36 individuals who had technical issues in the display of MPL screens or recording of the data (e.g., being shown the wrong combination of incentives and delays or not receiving a link to the part-2 survey).

[28] We define *top-coded* participants as those who chose the option on the right in each row, indicating a WTP for reminders of more than \$4 (on the low-incentive MPLs) or \$12 (on the high-incentive MPLs). *Bottom-coded* participants chose the option on the left in each row, indicating a WTP for reminders of less than $-\$4$ or $-\$12$.

minders, however, the impact of delay on completion rates is much smaller. This translates into an increasing impact of reminders as delay increases, as shown in Panel B.

Table 3 quantifies these results. Column (1) shows that receiving the reminders increases the likelihood that participants complete the survey by 23 percentage points. The estimate on High Incentive shows that participants who receive high incentives to complete the survey are 7 percentage points more likely to complete the survey than those who receive low incentives. The coefficient on ln(P2 Delay) implies that participants are less likely to complete the survey as the delay increases. Column (2) shows that reminders have a significantly smaller effect at high incentives, but a significantly larger effect at longer delays. Column (3) shows that because reminders have very small effects in the short-delay / high-incentive conditions (Panel B of Figure 2), the impact of delay on the effects of reminders is particularly large in the high-incentive condition.[29] Columns (4) and (5) show that neither the point estimates nor the standard errors of the column (2) and (3) models change when (i) including fixed effects for when participants begin the study or (ii) doing two-way clustering by when participants started part 1 and by when part 2 was available.[30] Column (6) shows that a $1 change in the incentives is small enough to not significantly affect behavior, which allows us to utilize Corollary 1.

As formalized in Appendix A.5, the negative interaction between reminders and high incentives suggests that the effect of the high incentives on survey completion was at least in part due to individuals choosing a higher level of attention in the absence of reminders. In our model, higher incentives increase the likelihood that individuals complete the task conditional on being attentive. However, since reminders increase the likelihood of being attentive, there would then be a *positive* interaction between reminders and incentives. Instead, if higher incentives increase individuals' effort to be attentive even in the absence of reminders, then there is less need for reminders, leading to a negative interaction.

3.2.3 How WTP Changes with the Incentive to Complete the Survey

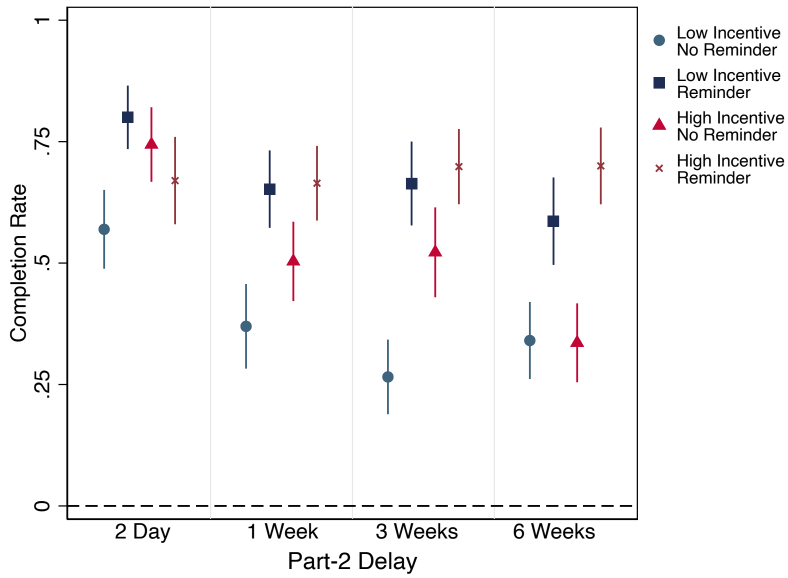
Figure 3 presents the average WTP for reminder emails for each part-2 incentive level at each of the four delays. Participants are willing to pay significantly more for reminders at the high incentives (i.e., $11 and $12, shown on the right of each panel) than at the low incentives

[29]Appendix Table A.5 presents a less parametric regression analysis that separately estimates the effect of each of the possible delays, as well as its interaction with reminders, on survey completion.

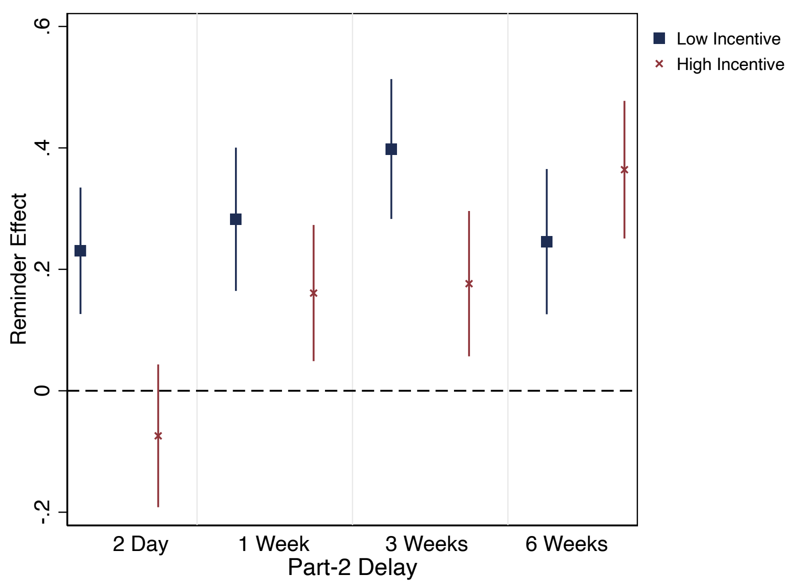
[30]We do not include fixed effects for part-2 survey availability because certain start dates are possible only in the 2-day condition, which means that the full set of fixed effects is not separately identified from the other covariates. However, because clustering by part-2 survey start date does not change standard errors, this is unlikely to matter.

Figure 2: Completion Rates and Treatment Effects of Reminders

A. Completion Rates by Incentive Level, Delay Type, and Reminders



B. Treatment Effect of Reminders by Incentive Level and Delay



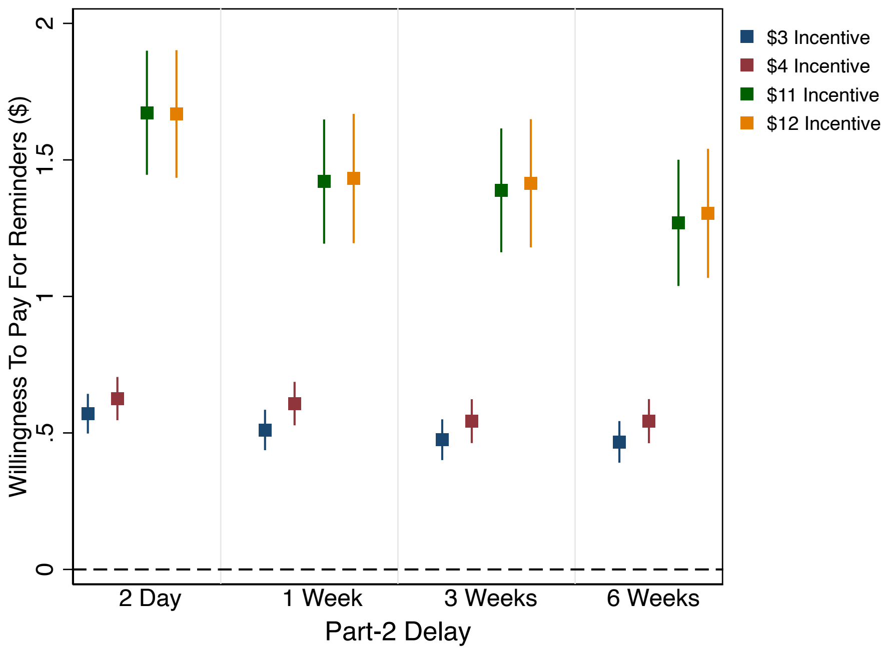
Panel A shows the part-2 survey completion rate and how it varies with the incentive for completing the survey, the amount of time after which part 2 of the survey became available (i.e., the part-2 delay), and whether the participant received reminders. Panel B shows the point estimates from a regression of the part-2 survey completion rate on whether the participant received a reminder. Both panels only include participants who were randomly assigned to receive or not receive reminders. The lines represent 95% confidence intervals.

Table 3: The Effect of Incentive, Delay, and Reminders on Part-2 Survey Completion

	Completed Part-2 Survey					
	(1)	(2)	(3)	(4)	(5)	(6)
Received Reminder	0.23***	0.12**	0.23***	0.12**	0.22***	0.23***
	(0.021)	(0.051)	(0.064)	(0.055)	(0.058)	(0.021)
High Incentive	0.07***	0.13***	0.21***	0.13***	0.20***	0.07***
	(0.021)	(0.029)	(0.066)	(0.031)	(0.063)	(0.021)
Ln(P2 Delay)	-0.07***	-0.10***	-0.08***	-0.10***	-0.08***	-0.07***
	(0.009)	(0.013)	(0.017)	(0.020)	(0.022)	(0.009)
Received Reminder × High Incentive		-0.13***	-0.38***	-0.11***	-0.32***	
		(0.042)	(0.094)	(0.039)	(0.070)	
Received Reminder × Ln(P2 Delay)		0.07***	0.02	0.07***	0.02	
		(0.018)	(0.025)	(0.020)	(0.025)	
High Incentive × Ln(P2 Delay)			-0.03		-0.03	
			(0.025)		(0.025)	
Received Reminder × High Incentive			0.11***		0.09***	
× Ln(P2 Delay)			(0.036)		(0.031)	
Extra $1						0.00
						(0.021)
Constant	0.57***	0.62***	0.59***	0.62***	0.59***	0.57***
	(0.028)	(0.036)	(0.045)	(0.054)	(0.057)	(0.029)
Observations	2,076	2,076	2,076	2,076	2,076	2,076
Number of Participants	2,076	2,076	2,076	2,076	2,076	2,076
S.E. Clustered by P1 & P2 Date				X	X	
P1 Date FE				X	X	

This table estimates how survey completion varies with reminders, the natural log of delay (in days), and whether participants are offered high incentives (i.e., $11 or $12) or low incentives (i.e., $3 or $4) to complete the survey. This table only includes participants who were randomly assigned to receive or not receive reminders. Columns (4) and (5) reproduce Columns (2) and (3) with fixed effects for the date that part 1 of the study was taken and with standard errors clustered for the date the participant completed part 1 and the date part 2 was made available to them. Column (6) reproduces Column (1) but estimates the impact of an extra $1 of incentive for completing the survey (i.e., the incentive being $4 or $12). Standard errors are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Figure 3: Willingness to Pay for Reminders



This figure shows mean willingness to pay for the reminders across different experimental conditions. The lines represent 95% confidence intervals with standard errors clustered at the participant level.

(i.e., $3 and $4, shown on the left of each panel). Additionally, WTP for reminders is higher at the $4 incentive than at the $3 incentive.

We formalize the results from Figure 3 in Table 4, combining data from all four incentive levels and all four delays to estimate how average WTP changes with the incentive to complete the survey.[31] The coefficient on Extra $1 is the impact on willingness to pay for reminders of increasing the survey-completion incentive from $3 to $4. The coefficient on High Incentive compares average WTP at $3 to average WTP at $11. The coefficient on Extra $1 × High Incentive compares the impact on WTP of increasing the incentive by $1 when the incentives are high (i.e., going from $11 to $12) to the impact when incentives are low (i.e., going from $3 to $4).

Column (1) shows that as the incentive to complete the survey increases from $3 to $4, participants are on average willing to pay around 7 cents more for the reminders. However, the interaction on *Extra* $1 × *High Incentive* is negative and similarly sized, implying that when incentives are high, the extra dollar of incentive does not lead to an increase of WTP.

[31] Appendix Table A.6 replicates Table 4 for the 90% of participants who either receive or do not receive the reminder emails based on random assignment. As one would expect from the fact that this 90% is randomly selected, estimates are nearly identical to those in Table 4.

That the coefficient on *High Incentive* is large and positive shows that WTP is on average much higher when incentives are high.[32]

Column (2) includes a covariate for the delay until the part-2 survey, Ln(P2 Delay), as well as the interaction Ln(P2 Delay) $\times$ Extra \$1. The coefficient on Ln(P2 Delay) reveals that participants are willing to pay less for reminders when the task is further out in the future, although the coefficient on Ln(P2 Delay) $\times$ Extra \$1 is directionally positive. Column (3) shows that these affects are amplified at the high incentive level. Column (4) returns to the specification in Column (1) but shows that controlling for the day participants completed part 1 of the study does not impact our estimates. Finally, Columns (5) and (6) replicate the specifications in (1) and (2) using a Tobit model to account for participants who were top-coded at the higher incentive levels within each group (\$4 or \$12). The results are quantitatively and qualitatively very similar in columns (5) and (6).[33]

3.2.4 Do Participants Invest in Reminders Optimally?

Taken together, the results show that, for a \$1 increase in the incentive for completing the survey, participants are on average willing to pay around 7 cents more for the reminders at low incentive levels but only 2 cents more at high incentive levels.

Part 1 of Theorem 1 states that if participants are optimally investing in the BE, then a small increase dr in incentives for completing the survey should increase WTP for the BE by approximately dr times the increase in the likelihood of survey completion due to the BE. As Column (6) of Table 3 shows, a \$1 change in incentives does not have a large effect on task completion, which implies that we can apply the test in part 1 of Corollary 1 to a \$1 change in incentives. On average, reminders had a 29 and a 16 percentage point effect on survey completion in the low-incentive and high-incentive conditions, respectively. This is significantly larger than the respective \$0.07 and \$0.02 changes in WTP with respect to an

[32]While not as natural a test of the theory since it spans a much larger increase in incentives, the 93 cent increase in WTP reflects an \$8 increase in the incentive level, or $\frac{93}{8} = 11.63$ cents per dollar, which is not that much larger than the 7 cent increase identified above. An additional difference that confounds this particular analysis, however, is that the MPL we use to elicit WTP for the high incentive levels was different (i.e., contained 33 rows where WTP increased in 75-cent increments) than the MPL for the low incentive levels (i.e., where WTP increased in 25-cent increments). For this reason as well, the estimate on *Extra* \$1 is the more natural test of the theory.

[33]Appendix Table A.7 shows that the results about WTP for reminders do not differ (at conventional levels of statistical significance) when we restrict to the first 2, 4, 6, or 8 MPL screens that participants encountered. This suggests that the within-subject design did not introduce demand or anchoring effects that altered our estimates. Appendix Table A.8 presents a less-parametric regression analysis that separately estimates the effect of each of the possible delays, as well as its interaction with the Extra \$1 covariate, on WTP for reminders.

Table 4: The Effect of Incentive and Delay on Willingness to Pay for Reminders

	WTP for Reminders ($)					
	(1)	(2)	(3)	(4)	(5)	(6)
Extra $1	0.07***	0.05	0.06*	0.07***	0.10***	0.07
	(0.017)	(0.051)	(0.036)	(0.017)	(0.018)	(0.052)
High Incentive	0.93***	0.93***	1.14***	0.93***	0.93***	0.93***
	(0.077)	(0.077)	(0.112)	(0.077)	(0.077)	(0.077)
Extra $1 × High Incentive	-0.06	-0.06	-0.08	-0.06	-0.07	-0.07
	(0.048)	(0.048)	(0.109)	(0.048)	(0.048)	(0.048)
Ln(P2 Delay)		-0.08***	-0.03***			-0.08***
		(0.022)	(0.012)			(0.022)
Extra $1 × Ln(P2 Delay)		0.01	0.00			0.01
		(0.021)	(0.014)			(0.021)
High Incentive × Ln(P2 Delay)			-0.09**			
			(0.035)			
Extra $1 × Ln(P2 Delay)			0.01			
× High Incentive			(0.041)			
Constant	0.51***	0.69***	0.59***	0.51***	0.50***	0.69***
	(0.032)	(0.057)	(0.041)	(0.032)	(0.032)	(0.057)
Observations	36,896	36,896	36,896	36,896	36,896	36,896
Number of Participants	2,306	2,306	2,306	2,306	2,306	2,306
Specification	OLS	OLS	OLS	OLS	Tobit	Tobit
P1 Date FE				X		

This table estimates the effect of incentive, the natural log of delay (in days), and having an incentive in the high-incentive pair on the willingness to pay for reminders. The extra $1 variable is an indicator for an incentive of $4 or $12. Participants in the high-incentive pair had a completion incentive of $11 or $12, compared to the low-incentive pair of $3 or $4. The "High Incentive" variable is an indicator for whether participants were in the high-incentive pair group. Column (1) shows OLS estimates for incentive, having an incentive in the high-incentive pair, and incentive interacted with having an incentive in the high-incentive pair; Column (2) maintains the specification in Column (1) and adds the natural log of delay; Column (3) adds an interaction between incentive, the natural log of delay, and an indicator for the high-incentive pair, as well as an interaction between the natural log of delay and the high-incentive pair, and an interaction between the natural log of delay and incentive; Column (4) shows Column (1) with fixed effects for the date that part-1 of the survey was taken; Column (5) uses the same variates as in Column (1) and shows Tobit estimates with censors at -$4 and $4 for the low-incentive group and censors at -$12 and $12 for the high-incentive group; Column (6) uses the same variates as in Column (2) and shows Tobit estimates with censors at -$4 and $4 for the low-incentive group and censors at -$12 and $12 for the high-incentive group. Standard errors, clustered at the participant level, are in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

additional \$1 incentive in these conditions (Wald test $p < 0.01$).[34]

We can also apply approximation (8) of Corollary 1 across each of the eight different incentive-delay pairs (i.e., {*low* and *high*} $\times$ {2 days, 1 week, 3 weeks, and 6 weeks}) generated in our experiment to estimate equation (10) from Section 1.4. A trivial application of the corollary is that under those same assumptions, equation (10) can be rewritten as

$$\bar{W}(r+\Delta|\xi) - \bar{W}(r|\xi) = \beta_0 + \beta_1 \frac{D(z=1|r,\xi) + D(z=1|r+\Delta,\xi)}{2}. \tag{12}$$

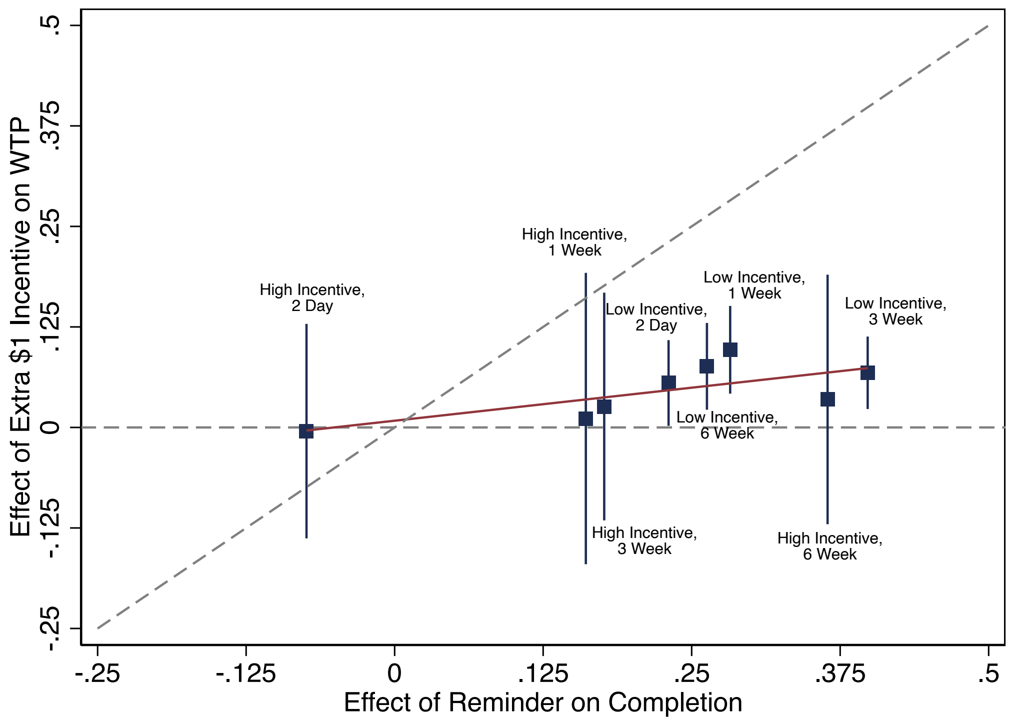
We estimate this equation by treating the eight different experimental conditions as variation in ξ. We then regress the change in WTP with respect to a \$1 change in incentives on the estimated effect of reminders, $D(z=1|r,\xi)$, in each condition. Formally, this procedure is equivalent to a two-stage least squares (2SLS) estimator, where the eight different experimental conditions are instruments for the effects of reminders, and where the dependent variable is the change in WTP with respect to a \$1 change in incentives.

Figure 4 provides a visualization of the second stage of this 2SLS estimator. On the *x*-axis, this figure shows the estimated effect of reminders on survey completion rates as reported in Panel B of Figure 2. On the *y*-axis, this figure shows the estimated increases in willingness to pay for the reminders as the incentive increases by \$1, together with the 95% confidence intervals of the estimates. If participants were optimally valuing the reminder technology, the WTP for the reminder would be on the 45-degree line (e.g., such that when reminders increase survey completion by 25 percentage points, the willingness to pay for the reminders increases by \$0.25 with a \$1 increase in incentives). Instead, our estimates are far from the 45-degree line. Estimated WTP is below the 45-degree line for seven of the eight estimates, and the 95% confidence intervals exclude the 45-degree line in six of the estimates. An estimate of equation (12) yields $\beta_0 = 0.01$ and $\beta_1 = 0.16$. That is, perceptions of the effects of reminders are attenuated toward a prior mean of approximately 0, by a factor of 84 percent. In line with the discussion in Section 1.4, this evidence may be more consistent with a model in which people's prior (or "default," in the sense of Gabaix, 2014) perceptions of the effects of the BE in this setting are systematically biased. For a prior mean of approximately 0 to be an unbiased prior, it would have to be that reminders often have no effect, and sometimes even have negative effects on task completion.

[34] Appendix Table A.17 explores whether risk aversion can explain the deviations in experiment 2 between willingness to pay and the effect of reminders on survey completion. We find no difference in willingness to pay for reminders when comparing participants with relatively high versus low risk aversion, measured in terms of the number of risky choices they select in gambles in the part 2 survey. These results hold both for the low incentive and high incentive groups. Thus, risk aversion does not appear to drive our results.

Findings from Experiment 2. *Willingness to pay for reminders increased with the size of the bonus for survey completion, but the increase was too small relative to the null of correct valuation of attention costs. Specifically, the bias parameter* θ *from the parametric model in (9) is estimated to be* 0.16*; that is, the responsiveness of valuations for reminders was 16% as large as the ex-post optimal benchmark implies.*

Figure 4: Effect of Reminders on Completion vs. Effect of Extra $1 on WTP



This figure displays estimates and standard errors for the effect of reminders on whether part 2 of the survey was completed, and the effect of an increase of $1 in the part-2 survey incentive on the willingness to pay for reminders. The figure only includes participants who were randomly assigned to receiving or not receiving reminders. The lines represent 95% confidence intervals, which are computed from the standard errors clustered by the date part 1 of the study was taken.

4 Learning Experiment

Results from our first two experiments reveal that individuals appear to undervalue bandwidth enhancements (BEs). A key question is whether individuals can learn to accurately value BEs through experience with them. We examine this in our third experiment. In addition, we illustrate the versatility of our methods by deploying them in a very different setting: a design that builds on the Ambuehl et al. (2020) paradigm and resembles other psychometric tasks that have been used to generate state-dependent stochastic choice data for testing rational inattention theories (Dean and Neligh, 2018; Caplin et al., 2020; Caplin, 2021). Experimental instructions are in Screenshots Appendix F.3.

4.1 Design and Implementation

We ran the learning experiment on Prolific Academic in October and November of 2021.[35] The study involved a series of tasks. In each task, participants were presented with an image that showed a series of equations that were either correct (e.g., $10+12=22$) or incorrect (e.g., $10+12=23$). Participants were asked to indicate whether the image contained more correct or more incorrect equations with an incentive for accuracy. The *baseline task* included an image with 100 equations about which participants were told that either 60% of the equations were correct (and 40% incorrect) or 40% of the equations were correct (and 60% incorrect).[36] Participants completed two blocks of seven tasks each, and at least three tasks in each block were baseline tasks. One task of the 14 tasks the participant completed was randomly selected, and participants were paid based on their accuracy in that task as described below.

Before completing each block of seven tasks, participants were asked for their willingness to pay to make the task easier (i.e., to take advantage of a BE). In the *length* arm, participants could shorten the task so that there were only 10 equations in the image, rather than 100 equations. In the *discernibility* arm, participants could make the fraction of correct equations either 95% correct (and 5% incorrect) or 5% correct (and 95% incorrect), rather than 60% or 40%. Of the seven tasks in the block, three were baseline tasks and three were easy tasks (i.e., shorter tasks in the *length* arm and more discernible tasks in the *discernibility* arm). If the remaining task in the block was randomly selected for payment, then the participant's choices (i.e., responses on a multiple price list, as described below) would determine

[35] In the recruitment materials, potential participants were informed that the study would require 20 minutes of their time, for which they would receive a guaranteed \$2.50. They were also informed that they would have the possibility of completing a bonus, and that the study had to be completed on a desktop or laptop computer using Chrome or Firefox as their web browser, which was necessary to ensure participants could see the tasks that were part of this study.

[36] The images were automatically constructed with randomly generated equations following the protocol in Ambuehl et al. (2020). The computer selected the number of equations (e.g., 100 in the baseline tasks) and randomly selected one of the possible fractions to be the percentage of correct equations (e.g., either 60% or 40% in the baseline tasks). The two numbers on the left side of the equation were each randomly selected from the range 1 to 99. For the correct equations, the true result appeared on the right side. For the incorrect equations, a number was randomly drawn from the range 1 to 5, which was either added to or subtracted from the true result at random. The equations were then shuffled for display.

whether that task was a baseline task an easier task.[37]

Following the design of our survey-completion experiment, we elicited willingness to pay to reduce attention costs for different incentives for accuracy in the task. Participants were informed that the computer would randomly and independently select an accuracy bonus—paid to a participant for providing an accurate answer in the task—of either \$2, \$3, or \$4 for each block of seven tasks.[38] This procedure allowed us to elicit a participant's WTP to make the task easier for three different accuracy bonus levels (i.e., before they knew which accuracy bonus would be relevant for that block). In particular, we elicited participants' willingness to pay to make the task easier using a set of three multiple price lists (MPLs). The interface of the MPLs was similar to that in the survey experiment.

A key design feature of this experiment is the opportunity to learn about the value of making the task easier. We do this by having participants experience both baseline and easy tasks in the first block of seven tasks (the baseline and easy tasks were presented in a random order). Participants then repeat the exercise—providing willingness to pay to make the task easier at each of the three accuracy incentive levels—for the second block of tasks.

The amount of feedback participants receive about their performance in the first block varies by treatment. In the *control* treatment, participants were not provided with information about their performance in the first block. In the *feedback* treatment, by contrast, participants were told the fraction of baseline and easy tasks they answered accurately. Moreover, participants were told how those accuracy levels translated to expected earnings at the three different levels of incentives.

In all MPLs, the options for willingness to pay ranged from $-\$4$ to \$4. This range is analogous to the range in the survey-completion experiment, where the highest possible MPL amount corresponded to the highest reward for task completion. Analogous to the survey-completion experiment, we make the same conservative sample restriction to limit to individuals who were never top-coded at the smallest incentive (i.e., \$2) or bottom-coded at the largest incentive (i.e., \$4). The logic behind this restriction is the same as in the previous experiment: since these individuals cannot increase their willingness to pay as the accuracy bonus increases, it is possible that including these participants could lead to an attenuation bias in estimates of the effect of the accuracy bonus on willingness to pay for a BE.

[37] If the task was not randomly selected for payment, then the difficulty of the task would be chosen at random. This protocol ensured that the multiple price list responses only affected the task and generated bonus payments in the case when participants were going to be paid based on their accuracy on that particular task.

[38] At the start of the block, participants were told which accuracy bonus had been randomly selected to apply for the block of seven tasks. Each task screen also reminded participants of the accuracy bonus. This bonus was paid if the participant provided an accurate answer on the task randomly selected for payment.

4.2 Results

Our analysis involves the 1465 participants who completed the study, and who were not top- or bottom-coded as described above.[39]

4.2.1 Impact of Task Difficulty on Accuracy

To analyze the causal effect of task difficulty on performance, we exclude data from tasks that were potentially endogenously determined by participants' WTP. That is, we exclude cases in which the "remaining task" in the block was selected to be the task that counts, and thus was affected by participants' preferences.

Table 5 presents OLS regressions that quantify the impacts of task difficulty on the likelihood of correctly identifying whether there are more correct or incorrect equations in the picture. The first two columns analyze performance in the first block, while the latter two columns analyze performance in the second block. Columns (1) and (3) show that performance did not differ significantly between the two blocks. In both blocks, participants correctly completed the baseline task approximately 70 percent of the time, were approximately 26 percentage points more likely to complete it correctly when the task was more discernible (i.e., had either 95% or 5% correct calculations), and were approximately 19 percentage points more likely to complete it correctly when the task was shorter (i.e., had only 10 equations). On average, the discernibility effect was moderately larger than the length effect: by 6.5 percentage points in block 1 (Chi-square test $p = 0.00$) and by 4.9 percentage points in block 2 (Chi-square test $p = 0.00$). Columns (2) and (4) show that variation in our incentives did not have a significant effect on performance. This is consistent with the survey-completion experiment, where a \$1 change in incentives was too small to have a significant effect, despite behavior being overall elastic (and thus responsive to large changes). This result implies that the incentive changes we analyze are sufficiently small to utilize Corollary 1.

Data on decision times is consistent with the baseline task being more difficult. On average, participants spent 87 seconds, 44 seconds, and 37 seconds on the baseline, shorter, and more discernible tasks, respectively. Appendix Figure A.4 presents the CDFs of response times across the three types of tasks. Appendix Figure A.3 shows that participants who spent

[39]This number does not include the 125 participants who were automatically screened out of the study (and prevented from participating further) because they failed attention checks, ensuring our pool of participants understood the instructions in our experiment. This number also does not include 13 individuals who encountered technical glitches. In addition, 520 participants were excluded from the analysis because in at least one set of their MPL elicitations they were either top-coded at the lowest incentive (\$2) or bottom-coded at the highest incentive (\$4).

Table 5: The Effect of Length and Discernibility on Getting a Task Correct

	Answered Task Correctly			
	(1)	(2)	(3)	(4)
More Discernible	0.26***	0.25***	0.26***	0.27***
	(0.010)	(0.008)	(0.010)	(0.008)
Shorter	0.19***	0.20***	0.21***	0.19***
	(0.011)	(0.009)	(0.012)	(0.010)
Length Arm	0.01		-0.04**	
	(0.015)		(0.015)	
Incentive ($)		-0.00		0.01
		(0.006)		(0.006)
Constant	0.71***	0.72***	0.71***	0.67***
	(0.010)	(0.018)	(0.010)	(0.018)
Observations	10,143	10,143	10,157	10,157
Number of Participants	1,465	1,465	1,465	1,465
Block	1	1	2	2

This table estimates the effect of shorter length (i.e., 10 calculations) and increased discernibility (i.e., 95% or 5% correct) on getting a task correct in block 1 and block 2. Tasks that had their difficulty determined by a participant's MPL choices have been excluded. The columns correspond to different regression specifications and blocks: Column (1) shows OLS estimates in block 1, Column (2) shows OLS estimates including the incentive level in dollars in block 1, and Columns (3) and (4) show analogous specifications for block 2. Standard errors, clustered at the participant level, are shown in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

more time on the tasks were more likely to answer them accurately, particularly in the more difficult, baseline task.

4.2.2 Willingness to Pay to Simplify Tasks

Table 6 presents OLS regressions that estimate how participants' WTP to make the tasks easier varies with accuracy incentives and other experimental manipulations. Column (1) shows that participants in the *length* arm increased their WTP to make the task shorter by approximately \$0.10 for every dollar of extra accuracy incentive in block 1. However, this effect goes down to approximately \$0.03 in block 2, which is a significant difference of approximately \$0.07 (Chi-square $p = 0.06$). In the *discernibility* arm, each \$1 of accuracy incentive increases WTP by $\$-0.01$ and \$0.03 cents in blocks 1 and 2, respectively. Neither of these is statistically significantly different from zero, nor are they different from each other. In block 1, the difference between the coefficients on incentive in the length and discernibility arms is \$0.11, which is statistically significant ($se = 0.045$, Chi-square $p = 0.017$).

Given that simplifying the task increased accuracy by approximately 19 to 26 percentage points in both arms, Theorem 1 implies that WTP for simplifying the task should increase by approximately \$0.19 to \$0.26. This is higher than the effects reported in Table 6. In each of the four block-arm pairs, the WTP increase is significantly smaller than the theoretical benchmark (Wald test $p < 0.01$ in all arms).

Columns (1) and (2) thus reveal three key insights. First, people in this experiment undervalue BEs, as in our first two experiments. Second, participants initially undervalue discernibility improvements more than length improvements. An ex-post rationalization is that decreasing the length of a task is a simple and relatively common form of simplification that most people are familiar with, whereas increasing discernibility is a more abstract and less-common form of simplification. Third, experience does not bring people's decisions more in line with the correct-perceptions benchmark formalized in Theorem 1. Directionally, people value discernibility improvements more in block 2, but this is not statistically significant at conventional levels. In fact, there is stronger evidence that in block 2 of the length arm, there is *more* deviation from the Theorem 1 benchmark than in block 1. The difference between the coefficients on Incentive $\times$ Block 2 in columns (1) and (2) is -0.11 ($se = 0.052$, Chi-square $p = 0.04$).

Columns (3) and (4) provide insight into why the deviation increases with experience in the length arm. As shown in column (3), the deviation increases primarily among the participants who received the feedback treatment. For participants in the control treatment, the coefficient on incentive increases by an insignificant 0.01 ($se = 0.050$). However, as the coefficient on the interaction Incentive $\times$ Block 2 $\times$ Feedback shows, the impact of experience is a statistically significant -0.16 ($se = 0.074$, $p = 0.034$) for participants in the feedback treatment. By contrast, feedback has no effect on participants in the discernibility arm, suggesting that participants in that arm have a very strongly held prior that discernibility would not affect their performance.

Columns (5) and (6) further explore the negative effect of feedback in the length arm. Column (5) restricts to participants who did not perform better on the shorter tasks than on the baseline tasks in block 1; column (6) restricts to participants who did perform better. The coefficient on Incentive $\times$ Block 2 is nearly identical in those two columns, implying that these two groups of participants were not differentially affected by experience in the control condition. However, the negative coefficient on Incentive $\times$ Block 2 $\times$ Feedback is twice as

Table 6: The Effect of Incentive, Block, and Feedback on Willingness to Pay for Easier Tasks

	Willingness to Pay ($)					
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive ($)	0.10***	-0.01	0.04	0.02	0.07	0.02
	(0.034)	(0.030)	(0.045)	(0.042)	(0.049)	(0.072)
Incentive ($) × Block 2	-0.07*	0.04	0.01	0.03	0.00	0.01
	(0.037)	(0.037)	(0.050)	(0.052)	(0.063)	(0.074)
Incentive ($)			0.11	-0.07	0.12	0.10
× Feedback			(0.067)	(0.060)	(0.089)	(0.099)
Incentive ($) × Block 2			-0.16**	0.01	-0.22**	-0.11
× Feedback			(0.074)	(0.074)	(0.100)	(0.108)
Block 2	0.15	-0.18	-0.09	-0.20	-0.16	-0.04
	(0.125)	(0.123)	(0.177)	(0.176)	(0.235)	(0.259)
Block 2 × Feedback			0.49*	0.05	0.87**	0.17
			(0.249)	(0.247)	(0.341)	(0.355)
Feedback			-0.22	0.22	-0.36	-0.11
			(0.248)	(0.213)	(0.342)	(0.353)
Constant	0.20	0.53***	0.31*	0.42***	0.32	0.31
	(0.124)	(0.107)	(0.172)	(0.147)	(0.212)	(0.260)
Observations	3,996	4,794	3,996	4,794	1,788	2,208
Number of Participants	666	799	666	799	298	368
Block 1 Acc. Diff.	All	All	All	All	≤ 0	> 0
Arm	Length	Discernibility	Length	Discernibility	Length	Length

This table estimates the effect of accuracy incentives, block order, and whether the participant received performance feedback on willingness to pay for an easier task (i.e., a shorter task in the length arm and a more discernible task in the discernibility arm). Columns (5) and (6) restrict participants by their block-1 accuracy difference between the baseline and easy tasks, which equals the difference between the percentage of easy tasks and baseline tasks answered correctly in block 1. The mean block-1 accuracy differences for participants in Columns (5) and (6) are -0.06 and 0.40, respectively. Column (1) shows OLS estimates for incentive, block, and the interaction of incentive and block order for participants in the length arm; Column (2) repeats this analysis in the discernibility arm; Column (3) maintains the specification in Column (1) and the restriction to participants in the length arm while adding whether feedback was received and the interactions between feedback, block, and incentive; Column (4) shows the OLS estimates in Column (3) for participants in the discernibility arm; Column (5) shows the OLS estimates in Column (3) for participants in the length arm who had a block-1 accuracy difference of less than or equal to 0; Column (6) shows the OLS estimates in Column (3) for participants in the length arm who had a block-1 accuracy difference of greater than 0. Standard errors, clustered at the participant level, are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

large in magnitude in column (5) than in column (6). This result suggests that a reason for the negative effect of the feedback treatment is participants being disappointed to learn that their block-1 performance was not higher on shorter tasks.[40]

The results about WTP in columns (3)–(6) of Table 6 cannot be explained by differences in block-2 performance among the different groups analyzed in those columns. Columns (1) and (2) of Appendix Table A.11 show that neither experience nor feedback altered participants' performance on the baseline versus easy tasks. Columns (3) through (5) of Appendix Table A.11 restrict to the length arm, and show that: (i) consistent with mean reversion, participants who did not perform better on the shorter tasks than on the baseline tasks in block 1 *improved* their relative performance on the easy tasks in block 2, and (ii) feedback did not reduce the difference in block-2 performance between the easy and baseline tasks among these participants. The overall negative effect of the feedback treatment suggests participants updated their beliefs in a quasi-Bayesian manner by overweighting disappointing experiences.

Interestingly, columns (3) and (5) of Table 6 also suggest that, while experience and feedback led participants to underestimate the effect of task simplification on their performance at the incentives in the experiment, it did increase their overall WTP to simplify the task. This result illustrates the theoretical discussion in Section 1.3.1 about how accurate perceptions of total costs (including fixed costs) are not characterized in Theorem 1—perceptions of the effects of the BE at the current incentive level r is not a sufficient statistic for perceptions of total costs. Appendix Table A.12 shows that participants who did not perform better on the shorter tasks than on the baseline tasks in block 1 spent almost two minutes longer on the baseline versus easy tasks in block 1. Subsequently, they spent approximately 30 to 45 seconds less time on the baseline versus easy tasks in block 2, relative to the participants who did perform better on the shorter tasks in block 1. This suggests that the participants who did not perform better on the shorter tasks in block 1 also incurred significantly larger total costs on the baseline tasks. The feedback treatment may have helped prime this realization by inducing participants to further reflect on the differences between the baseline and shorter tasks.[41] This illustrates the Section 1.3.1 discussion about how additional treatments such as our feedback treatment can provide additional insights into perceptions about total costs,

[40] Alternatively, it could be consistent with the suggestive evidence from column (6) of Appendix Table A.11 that feedback may have decreased performance for these participants—which suggests that these participants overall chose to adopt attention strategies that would decrease relative performance in the shorter task.

[41] Appendix Table A.12 also shows that feedback treatment had a small negative effect on the time taken on the baseline tasks in block 2 for participants who performed better on the shorter tasks than on the baseline tasks in block 1. There is no effect on participants who did not perform better on the shorter tasks in block 1.

complementing the tests in Theorem 1.[42]

Findings from Experiment 3. *The responsiveness of willingness to pay for the length BE was 50% as large as the ex-post optimal benchmark implies. Willingness to pay for the discernibility BE did not respond to incentive size at all, implying a responsiveness 0% as large as the ex-post optimal benchmark implies. Experience via feedback on past performance did not bring people's decisions more in line with the correct-valuations benchmark.*

5 Conclusion

While a large and growing literature shows that attention-increasing interventions such as reminders and plan-making tools can have significant effects on economically important behaviors, this literature rarely asks the question of whether individuals value and deploy these tools optimally. This paper addresses this question with three theory-driven, quantitative tests. We find that individuals' demand for attention-increasing tools is *qualitatively* consistent with the predictions of optimal management of limited attention,but is quantitatively inconsistent with fully optimizing choice of attention. This suggests that individuals are uncertain and/or systematically biased about their attention cost functions. While this under-valuation of bandwidth enhancements may be context dependent, our methods can be applied more broadly to explore how individuals value attention-increasing technologies across various domains.

Our methods are immediately portable to other settings where the impact of reminders and planning prompts has already been documented, such as in medical compliance, savings, loan repayment, and voting (see footnote 6 for references). In addition, as exemplified by our third experiment, our methodology can be used to test whether people understand their production functions for attention-consuming tasks in field settings such as those of Dean (2019), Kaur et al. (2021), or Bessone et al. (forthcoming). More generally, our tests could be applied to any setting that involves domains of behavior that feature "intermediate" actions. For example, our methods could be used to quantify whether students fully understand the relationship between studying and test performance, whether individuals understand the link between education and earnings, or whether individuals properly invest in "good habits."

Finally, our results that people do not select their attention environments optimally are consistent with the hypothesis that people might misoptimize their attention strategies in

[42] Appendix Tables A.9 and A.10 replicate Table 6 using Tobit models and dropping participants with the 10 percent fastest average task times by the length and discernibility arms separately. The results are quantitatively and qualitatively similar.

other ways. For example, people might misoptimize their choice of decision boundaries in sequential information acquisition problems (Reshidi et al., 2022); or people might not choose their signal structure optimally in complex games with asymmetric payoffs (as in, e.g., Suen, 2004). These and other possibilities suggest an exciting research agenda on the question of whether attention is produced optimally.

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Online Appendix

A Mathematical appendix

A.1 Additional Technical Results

We first introduce an additional lemma that characterizes the differentiability and continuity properties of the statistics employed in Theorem 1. The proof is given in Appendix A.3.

Lemma A.1. *Assume that individuals choose attention strategies optimally. Then*

1. $\bar{W}(r)$ *is differentiable almost everywhere.*

2. $Pr(z=1|j,r)$ *is increasing in* r *and differentiable almost everywhere.*

3. *Suppose that* $\bar{K}^1_{ai}(q'_a) - \bar{K}^1_{ai}(q_a) < \bar{K}^0_{ai}(q'_a) - \bar{K}^0_{ai}(q_a)$ *for all* i *and* $q'_a > q_a$*, meaning that the marginal costs to increasing attention are always lower under technology* j*. Then,* $Pr(j=1|p,r)$ *and* $Pr(z=1|p,r)$ *are both increasing* r*, decreasing in* p*, and almost everywhere differentiable in* r *and* p*.*

4. $\bar{W}(r)$ *is differentiable at any point* r *where* $Pr(z=1|j=1,r) - Pr(z=1|j=0,r)$ *is continuous in* r*.*

5. *Suppose that* $\bar{K}_{ai}$ *and* $\bar{K}_{oi}$ *are strictly convex for all* i*. Then* $\bar{W}(r)$ *is everywhere continuously differentiable.*

6. *Suppose that at* (p,r)*,* $Pr(j=1|p,r)$ *is continuously differentiable in* p *and that* $\bar{W}(r)$ *is continuously differentiable in* r*. Then* $Pr(j=1|p,r)$ *is continuously differentiable in* r *and* $Pr(z=1|p,r)$ *is continuously differentiable in* p*.*

Lemma A.1 allows us to express some of our main results in terms of marginal conditions without much loss of generality. This is for three reasons. First, because parts 1 and 2 of the lemma show that without any additional assumptions, two of they key statistics are differentiable everywhere except on a set of Lebesgue measure zero. Second, part 3 of the lemma shows that under the assumption that the BEs in our experiments work as intended—by increasing the likelihood of success for a given attention cost—the remaining statistics are differentiable almost everywhere as well. Part 4 of the Lemma concerns the condition that $Pr(z=1|j=1,r) - Pr(z=1|j=0,r)$ is continuous in r. This is a plausible condition in our experiments, where we find that $Pr(z=1|j,r)$ for $j \in \{0,1\}$ changes negligibly when we increase r by a small amount. Part (5) provides an alternative set of assumptions for differentiability of $\bar{W}(r)$, which is that the cost functions are convex. Finally, part (6) considers the mild assumption that $Pr(j=1|p,r)$ is differentiable in p. This is a natural condition on a demand function for BEs, and holds whenever the distribution of individual differences is smooth. For example, this condition holds when $\bar{K}^1_{ai}(q)-\bar{K}^0_{ai}(q)=\bar{K}^1_a(q)-\bar{K}^0_a(q)+\eta_i$, where η_i is a random variable with a smooth density function, and interpreted as a person-specific nuisance cost of the BE.

A.2 Preliminaries for Proofs of Main Results

A.2.1 Notation

By the reasoning analogous to that in the proof of Lemma 1, we can express the indirect utility functions as

$$V_i^j(r)=\max_{q\in[\underline{q},\bar{q}]}\left\{rq-\bar{K}_i^j(q)\right\}$$

where $\underline{q}=\underline{q}_a\underline{q}_o$, $\bar{q}=\bar{q}_a\bar{q}_o$, and $\bar{K}_i^j(q)=\inf_{q_a,q_o}\{K_{ai}(q_a)+K_{oi}^j(q_o)|q_aq_o\geq q\}$.

To further ease notation, define the functions $f_i^j(r,q)=rq-\bar{K}^j(q)$, so that $V_i^j(r)=\max_q f_i^j(r,q)$. Define $X_i^j(r)=\{q|f_i^j(r,q)=V_i^j(q)\}$ as the maximizers of f_i^j, and note that by assumption X_i^j is non-empty. Under the assumption of optimality, an individual's choice of q under technology j is a selection $q_i^j(r)$ from $X_i^j(r)$.

Define $\mathscr{V}_i(p,r)=\max\{V_i^1(r)-p,V_i^0(r)\}$, and define $\bar{\mathscr{V}}(p,r)=\mathbb{E}_i\mathscr{V}_i(p,r)$. We can write $\mathscr{V}_i(p,r)=\max_{q,j}\varphi_i(q,j,p,r)$ where

$$\varphi_i=j\left(rq-\bar{K}_i^1(q)-p\right)+(1-j)\left(rq-\bar{K}_i^0(q)\right)$$

Similarly, define $Y_i(p,r)=\{(q,j)|\varphi_i(q,j,p,r)=\mathscr{V}_i(p,r)\}$ as the maximizers of φ_i, which again is non-empty by assumption. An individual's choice of technology and completion probability is a selection $(j_i(p,r),q_i(r))\in Y_i(p,r)$. We define $Pr_i(z=1|p,r)$ as individual i's probability of successfully completing the task, given by $j_i(p,r)q_i^1(r)+(1-j_i(p,r))q_i^0(r)$.

A.2.2 Preliminary Lemmas

Lemma A.2. *$V_i^j(r)$ is strictly increasing in r. Any selection $q_i^j(r)$ is increasing in r.*

Proof. Consider $r_2 > r_1$. Then

$$\begin{aligned} V_i^j(r_2) &\geq f_i^j(r_2, q_i(r_1)) \\ &> f_i^j(r_1, q_i(r_1)) \\ &= V_i^j(r_1) \end{aligned}$$

which establishes the first claim. Next, note that by definition, we must have

$$\begin{aligned} r_2 q_i^j(r_2) - \bar{K}_i^j(q_i^j(r_2) &\geq r_2 q_i^j(r_1) - \bar{K}_i^j(q_i^j(r_1)) \\ \Leftrightarrow r_2 \left(q_i^j(r_2) - q_i^j(r_1) \right) &\geq \bar{K}_i^j(q_i^j(r_2)) - \bar{K}_i^j(q_i^j(r_1)) \end{aligned} \tag{13}$$

Similarly, we must have

$$r_1 \left(q_i^j(r_1) - q_i^j(r_2) \right) \geq \bar{K}_i^j(q_i^j(r_1)) - \bar{K}_i^j(q_i^j(r_2)) \tag{14}$$

Combining (13) and (14) implies that

$$\begin{aligned} r_2 \left(q_i^j(r_2) - q_i^j(r_1) \right) &\geq \bar{K}_i^j(q_i^j(r_2)) - \bar{K}_i^j(q_i^j(r_1)) \\ &\geq r_1 \left(q_i^j(r_2) - q_i^j(r_1) \right) \end{aligned}$$

Since $r_2 > r_2$, the above equality can only hold if $q_i^j(r_2) - q_i^j(r_1)$ is non-negative, which establishes the second part of the claim. □

Lemma A.3. *If $Pr(z = 1|p, r)$ is continuous then $\bar{\mathscr{V}}$ is differentiable in r. If $Pr(j = 1|p, r)$ is continuous then $\bar{\mathscr{V}}$ is differentiable in p.*

Proof. Define $x = ((j_i, q_i))_{i \in \mathscr{I}}$ as the tuple of strategies of all individuals $i \in \mathscr{I}$ in the data. Define $\varphi(x, p, r) = \mathbb{E}_i \varphi_i(j_i, q_i, p, r)$, and note that x is a maximizer of φ if (j_i, q_i) is a maximizer of φ_i for each i. Thus, $\bar{\mathscr{V}}(p, r) = \max_x \varphi(x, p, r)$. Now because $\varphi(x, p, r)$ is linear in r and p, and because $\frac{\partial}{\partial r}\varphi(x, p, r)$ and $-\frac{\partial}{\partial p}\varphi(x, p, r)$ are contained in the unit interval, it satisfies all assumptions of Theorem 3 of Milgrom and Segal (2002). Thus, $\bar{\mathscr{V}}(p, r)$ is left-and right-differentiable in both $r$and p, with the respective derivatives given by

$$
\begin{aligned}
\frac{d_-}{dr}\bar{\mathcal{V}}(p,r) &= \lim_{x\to r-} \mathbb{E}_i Pr_i(z=1|p,r) \\
&= \lim_{x\to r-} Pr(z=1|p,r) \\
\frac{d_+}{dr}\bar{\mathcal{V}}(p,r) &= \lim_{x\to r+} \mathbb{E}_i Pr_i(z=1|p,r) \\
&= \lim_{x\to r+} Pr(z=1|p,r) \\
\frac{d_-}{dp}\bar{\mathcal{V}}(p,r) &= \lim_{x\to p-} \mathbb{E}_i j_i(p,r)(-1) \\
&= \lim_{x\to p-} -Pr(j=1|p,r) \\
\frac{d_+}{dp}\bar{\mathcal{V}}(p,r) &= \lim_{x\to p+} \mathbb{E}_i j_i(p,r)(-1) \\
&= \lim_{x\to p-} -Pr(j=1|p,r)
\end{aligned}
$$

When $Pr(z=1|p,r)$ is continuous, the left and right limits are equal, and thus $\bar{\mathcal{V}}(p,r)$ is differentiable in r. Similarly, $\bar{\mathcal{V}}(p,r)$ is differentiable in p when $Pr(j=1|p,r)$ is continuous.

□

A.3 Proofs of Main Results

Proof of Lemma 1

Proof. Suppose first that (s_a^*, s_o^*) is a solution to (1), and define $q_a^* = \mathbb{E}Q(s_a^*, \omega_a)$ and $q_o^* = \mathbb{E}Q(s_o^*, \omega_o)$. Plainly, an individual maximizing (2) can achieve at least

$$\mathbb{E}\left[rQ_a(s_a^*, \omega_a)Q_o(s_o^*, \omega_o) - K_{ai}(s_a^*) - K_{ai}(s_o^*, \omega_o)\right]$$

by simply setting $q_a = q_a^*$ and $q_o = q_o^*$. We now show that the individual cannot do any better. By way of contradiction, assume that there exist (q_a', q_o') such that

$$rq_a'q_o' - \bar{K}_{ai}(q_a') - \bar{K}_{oi}(q_o') \geq \mathbb{E}\left[rQ_a(s_a^*, \omega_a)Q_o(s_o^*, \omega_o) - K_{ai}(s_a^*) - K_{ai}(s_o^*, \omega_o)\right] + \varepsilon$$

for some $\varepsilon > 0$. By definition of the $\bar{K}$ functions, there exist (s_a', s_o') such that $\mathbb{E}Q_a(s_a', \omega_a) \geq q_a'$, $\mathbb{E}Q_o(s_o', \omega_a) \geq q_o'$ and $K_{ai}(s_a') \leq \bar{K}_{ai}(q_a') + \varepsilon/4$, $K_{oi}(s_o') \leq \bar{K}_{oi}(q_o') + \varepsilon/4$. Thus,

$$\mathbb{E}\left[rQ_a(s_a', \omega_a)Q_o(s_o', \omega_o) - K_{ai}(s_a') - K_{ai}(s_o', \omega_o)\right] \geq \mathbb{E}\left[rQ_a(s_a^*, \omega_a)Q_o(s_o^*, \omega_o) - K_{ai}(s_a^*) - K_{ai}(s_o^*, \omega_o)\right] + \varepsilon/2$$

which contradicts the optimality of (s_a^*, s_o^*).

To prove the converse direction, note again that by definition of the $\bar{K}$ functions, for any $\varepsilon > 0$ there exist (s_a^*, s_o^*) such that $\mathbb{E}Q_a(s_a^*, \omega_a) \geq q_a^*$, $\mathbb{E}Q_o(s_o', \omega_a) \geq q_o^*$ and $K_{ai}(s_a^*) \leq \bar{K}_{ai}(q_a') + \varepsilon/2$, $K_{oi}(s_o^*) \leq \bar{K}_{oi}(q_o^*) + \varepsilon/2$. Thus,

$$\max_{(s_a,s_o)\in S_a\times S_o} \mathbb{E}\left[rQ_a(s_a,\omega_a)Q_o(s_o,\omega_o) - K_{ai}(s_a) - K_{ai}(s_o,\omega_o)\right] \geq rq_a^*q_o^* - \bar{K}_{ai}(q_a^*) - \bar{K}_{oi}(q_o^*) - \varepsilon$$

for any arbitrary ε, and thus

$$\max_{(s_a,s_o)\in S_a\times S_o} \mathbb{E}\left[rQ_a(s_a,\omega_a)Q_o(s_o,\omega_o) - K_{ai}(s_a) - K_{ai}(s_o,\omega_o)\right] \geq rq_a^*q_o^* - \bar{K}_{ai}(q_a^*) - \bar{K}_{oi}(q_o^*).$$

On the other hand, as we have already argued in the first part of the proof, the agent cannot find strategies (s_a, s_o) that obtain higher expected utility than $rq_a^*q_o^* - \bar{K}_{ai}(q_a^*) - \bar{K}_{oi}(q_o^*)$. $\square$

Proof of Lemma 2

Proof. We need to show that for any q_1, q_2 and $\alpha \in (0,1)$, $\bar{K}_a(\alpha q_1 + (1-\alpha)q_2) < \alpha\bar{K}_a(q_1) + (1-\alpha)\bar{K}_a(q_2)$. The argument for $\bar{K}_o$ is identical. By convexity of S_a, for any $q \in [\underline{q}_a, \bar{q}_a]$ there must be some $s \in S_a$ such that $\mathbb{E}Q(s,\omega_a) = q$. Thus, we can choose $s_1, s_2 \in S_a$ such that $s_1 \in \operatorname{argmin}_s\{K_a(s)|\mathbb{E}Q(s,\omega_a) \geq q_1\}$ and analogously for s_2. Now since $\mathbb{E}Q(\cdot,\omega_a)$ is concave, we have

$$\begin{aligned}
\bar{K}_a(\alpha q_1 + (1-\alpha)q_2) &\leq \bar{K}_a(\mathbb{E}Q(\alpha s_1 + (1-\alpha)s_2, \omega_a)) && (15)\\
&\leq K_a(\alpha s_1 + (1-\alpha)s_2) && (16)\\
&\leq \alpha K_a(s_1) + (1-\alpha)K_a(s_2) && (17)\\
&= \alpha\bar{K}_a\left(\mathbb{E}Q(s_1,\omega_a)\right) + (1-\alpha)\bar{K}_a\left(\mathbb{E}Q(s_1,\omega_a)\right) && (18)
\end{aligned}$$

Line (15) follows by the concavity of $\mathbb{E}Q(\cdot,\omega_a)$. Line (16) follows by the definition of $\bar{K}_a$. Line (17) follows by convexity of K_a, and line (18) follows by the definition of s_1 and s_2. $\square$

Proof of Lemma A.1

Proof. Part 1: In Lemma we have show that $V_i^j(r)$ is strictly increasing in r. Thus, $\mathbb{E}_iV_i^j(r)$ is strictly increasing in r, and thus differentiable almost everywhere. Thus, $\bar{W}(r) = \mathbb{E}_iV_i^1(r) -$

$\mathbb{E}_i V_i^0(r)$ is differentiable almost everywhere.

Part 2: We can write

$$\varphi_i(j,q,p,r) = rq - \psi_i(j,q,p,r)$$

where $\psi_i(j,q,p) = jp + j\left(\bar{K}_i^1(q) - \bar{K}_i^0(q)\right) + \bar{K}_i^0(q)$.

Consider $r_2 > r_1$. Then then optimal selections $j_i(r)$ and $q_i(r)$ satisfy

$$\begin{aligned} r_2 q_i(r_2) - \psi_i(j_i(r_2), q_i(r_2), p) &\geq r_2 q_i(r_1) - \psi_i(j_i(r_1), q_i(r_1), p) \\ \Leftrightarrow r_2\left(q_i(r_2) - q_i(r_1)\right) &\geq \psi_i(j_i(r_2), q_i(r_2), p) - \psi_i(j_i(r_1), q_i(r_1), p) \end{aligned}$$

Similarly,

$$r_1\left(q_i(r_1) - q_i(r_2)\right) \geq \psi_i(j_i(r_1), q_i(r_1), p) - \psi_i(j_i(r_2), q_i(r_2), p)$$

and thus

$$r_2\left(q_i(r_2) - q_i(r_1)\right) \geq r_1\left(q_i(r_2) - q_i(r_1)\right)$$

which can hold only if $q_i(r_2) - q_i(r_1) \geq 0$. Thus, $Pr_i(j=1|p,r)$ is increasing in r, and therefore $Pr(j=1|p,r)$ is increasing in r as well. The monotonicity implies almost everywhere differentiability.

Part 3: The assumption equivalently implies that $\bar{K}_i^1(q) - \bar{K}_i^0(q)$ is decreasing in q. Thus, $\psi_i(j,q,p)$, as defined above, is such that $\psi_i(1,q,p) - \psi_i(0,q,p)$ is decreasing in q. Since above we have show that $q_i(r)$ is increasing in q, this implies that the returns to choosing $j=1$ over $j=0$ are increasing in q.

Part 4: Define $x^j = (q_i^j)_{i\in\mathscr{I}}$ as the tuple of strategies of all individuals $i \in \mathscr{I}$ in the data given technology j. Define $f^j(x^j, r) = \mathbb{E}_i f_i^j(q_i, r)$, and note that x^j is a maximizer of f if q_i^j is a maximizer of f_i^j for each i. Thus, $\bar{W}(r) = \max_{x^1} f^1(x^1, r) - \max_{x^0} f^0(x^0, r)$. Now because $f^j(x,p,r)$ is linear in r, and because $\frac{\partial}{\partial r} f^j(x,p,r)$ is contained in the unit interval, it satisfies all assumptions of Theorem 3 of Milgrom and Segal (2002). Thus, $\bar{W}(r)$ is left- and right-differentiable in r, with the respective derivatives given by

$$\begin{aligned} \frac{d_-}{dr}\bar{W}(p,r) &= \lim_{x\to r-}\left(\mathbb{E}_i Pr_i(z=1|j=1,r) - \mathbb{E}_i Pr_i(z=1|j=0,r)\right) \\ &= \lim_{x\to r-} D(z=1|p,r) \\ \frac{d_+}{dr}\bar{W}(p,r) &= \lim_{x\to r+}\left(\mathbb{E}_i Pr_i(z=1|j=1,r) - \mathbb{E}_i Pr_i(z=1|j=0,r)\right) \\ &= \lim_{x\to r+} D(z=1|p,r) \end{aligned}$$

When $D(z=1|r)$ is continuous in r, the left and right limits are equal, and thus $\bar{W}(r)$ is continuously differentiable in r.

Part 5: If $\bar{K}_{ai}$ and $\bar{K}_{oi}$ are strictly convex for all i then $\bar{K}_i^j$, as defined in Appendix A.2.1, is strictly convex, by an argument identical to that in the proof of Lemma 2. Thus, each individual's optimal choice $q_i^j(r)$ is unique for each (j,r). Moreover, since convex functions are continuous, this implies that f_i^j is continuous. Thus, Corollary 4 of Milgrom and Segal (2002) implies that $V_i^j(r)$ is everywhere differentiable in r, with derivative $q_i^j(r)$. The claim then follows immediately.

Part 6: $Pr(j=1|p,r) = Pr\left(V_i^1(r) - V_i^0(r) - p \geq 0\right)$. Now since $V_i^j(r)$ is increasing, it has left and right derivatives everywhere. Thus, $Pr\left(V_i^1(r) - V_i^0(r) - p \geq 0\right)$ is left- and right-differentiable everywhere. Now by the assumption that $Pr(j=1|p,r)$ is continuously differentiable in p,

$$\begin{aligned}
\frac{d-}{dr} Pr\left(V_i^1(r) - V_i^0(r) - p \geq 0\right) &= \frac{d}{dp} Pr\left(V_i^1(r) - V_i^0(r) - p \geq 0\right) \frac{d-}{dr} \mathbb{E}_i\left(V_i^1(r) - V_i^0(r)\right) \\
&= \frac{d}{dp} Pr\left(V_i^1(r) - V_i^0(r) - p \geq 0\right) \frac{d-}{dr} \bar{W}(r) \\
\frac{d+}{dr} Pr\left(V_i^1(r) - V_i^0(r) - p \geq 0\right) &= \frac{d}{dp} Pr\left(V_i^1(r) - V_i^0(r) - p \geq 0\right) \frac{d+}{dr} \mathbb{E}_i\left(V_i^1(r) - V_i^0(r)\right) \\
&= \frac{d}{dp} Pr\left(V_i^1(r) - V_i^0(r) - p \geq 0\right) \frac{d+}{dr} \bar{W}(r)
\end{aligned}$$

Thus, $Pr\left(V_i^1(r) - V_i^0(r) - p \geq 0\right)$ is continuously differentiable in r if $\bar{W}(r)$ is continuously differentiable in r.

Next, to show that $Pr(z=1|p,r)$ is continuously differentiable in p, note that it is given by

$$Pr(j=1|p,r)Pr(z=1|j=1,r) + (1 - Pr(j=1|p,r))Pr(z=1|j=0,r)$$

Thus, $Pr(z=1|p,r)$ is continuously differentiable in p if $Pr(j=1|p,r)$ is continuously differentiable in p. □

Proof of Theorem 1

Proof. Since f_i^j is a linear function of r, all assumptions of Theorem 2 of Milgrom and Segal (2002) are satisfied for f_i^j. Moreover, note that $\frac{\partial}{\partial r} f_i^j = q$. Thus, if $q_i^j(r)$ is an individual's optimal choice under technology j, we have that

$$V_i^j(r+\Delta) - V_i^j(r) = \int_{x=r}^{x=r+\Delta} q_i^j(r)dr.$$

Now

$$\begin{aligned}
\bar{W}(r+\Delta) - \bar{W}(r) &= \mathbb{E}_i\left[V_i^1(r+\Delta) - V_i^0(r+\Delta)\right] - \mathbb{E}_i\left[V_i^1(r) - V_i^0(r)\right] \\
&= \mathbb{E}_i\left[V_i^1(r+\Delta) - V_i^1(r)\right] - \mathbb{E}_i\left[V_i^0(r+\Delta) - V_i^0(r)\right] \\
&= \mathbb{E}_i \int_{x=r}^{x=r+\Delta} q_i^1(x)dx - \mathbb{E}_i \int_{x=r}^{x=r+\Delta} q_i^0(x)dx \\
&= \int_{x=r}^{x=r+\Delta} \mathbb{E}_i q_i^1(x)dx - \int_{x=r}^{x=r+\Delta} \mathbb{E}_i q_i^0(x)dx \\
&= \int_{x=r}^{x=r+\Delta} D(z=1|x)dx
\end{aligned}$$

This completes the proof of (3). It follows immediately that $\bar{W}'(r) = D(z=1|r)$ at all points of differentiability, and the conditions for where $\bar{W}$ is differentiable follow from Lemma A.1.

To prove the statement in (5), note that $\bar{\mathscr{V}}$ is differentiable in both $r$and p by Lemma A.3. In particular, application of the Envelope Theorem 3 of Milgrom and Segal (2002) in the proof of that Lemma showed that

$$\begin{aligned}
\frac{d}{dr}\bar{\mathscr{V}}(p,r) &= Pr(z=1|p,r) \\
\frac{d}{dp}\bar{\mathscr{V}}(p,r) &= -Pr(j=1|p,r)
\end{aligned}$$

Now when $Pr(z=1|p,r)$ and $Pr(j=1|p,r)$ are continuously differentiable, the cross-partials $\frac{d}{dp}\frac{d}{dr}\bar{\mathscr{V}}(p,r)$ and $\frac{d}{dr}\frac{d}{dp}\bar{\mathscr{V}}(p,r)$ are continuous and therefore must be equal to each other. This implies that

$$\frac{d}{dp}Pr(z=1|p,r) = -\frac{d}{dr}Pr(j=1|p,r)$$

Last, note that

$$
\begin{aligned}
Pr(z=1|p,r) &= Pr(j=1|p,r)\left(D(z=1|r)+Pr(z=1|j=0,r)\right)\\
&\quad + Pr(j=0|p,r)Pr(z=1|j=0,r)\\
&= \left(Pr(j=1|p,r)+Pr(j=0|p,r)\right)Pr(z=1|j=0,r)\\
&\quad + Pr(j=1|p,r)D(z=1|r)\\
&= Pr(z=1|j=0,r)+Pr(j=1|p,r)D(z=1|r).
\end{aligned}
$$

Since $Pr(z=1|j=0,r)$ and $D(z=1|r)$ are not functions of p, we thus have that

$$
\frac{d}{dp}Pr(z=1|p,r) = \frac{d}{dp}Pr(j=1|p,r)D(z=1|r).
$$

□

A.4 Graphical Illustration

Figure A.1 illustrates the intuition graphically for a representative individual, for the case in which the marginal costs are linear. For simplicity, we assume that there are no auxiliary actions, and that $\bar{K}^0(0)=\bar{K}^1(0)=0$. In this case, the likelihood of executing the task equals the chosen level of attention q. In analogy to standard theories of competitive supply, individuals' choice of q with attention technology j is determined by the intersection of the marginal benefit curve r and the marginal cost curve $\frac{\partial}{\partial q}\bar{K}^j$. As in theories of competitive supply, the total surplus of an individual with technology $j=0$ at incentive r is equal to the area of triangle OAD. Similarly, the total surplus of an individual with technology $j=1$ is equal to the area of triangle OAF. Increasing the incentives r by an amount Δ increases surplus by an amount ABCD under technology $j=0$, and by an amount ABEF under technology $j=1$. The change in WTP for technology $j=1$ is thus given by the area DCEF. The area of DCEF is equal to the height, Δ, multiplied by the average of the lengths of DF and CE, which is

$$
\left(D(z=1|r)+D(z=1|r+\Delta)\right)/2.
$$

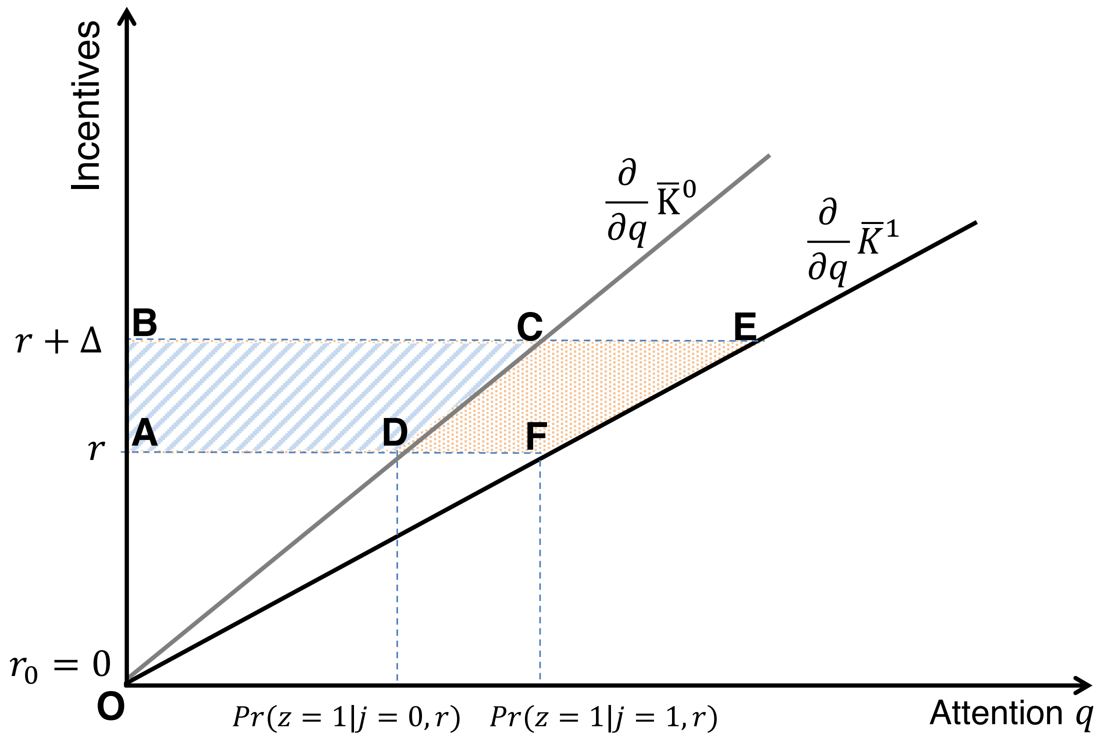
This gives the expression in Corollary 1.

In the limit of very small Δ, the difference between $Pr(z=1|j,r)$ and $Pr(z=1|j,r+\Delta)$ becomes negligible, and thus the area of ABCD can be expressed as

$$
\Delta \cdot D(z=1|r)
$$

which leads to first-order condition for $\bar{W}(r)$ in Theorem 1, after dividing by Δ.

Figure A.1: Illustration of Theorem 1



This figure illustrates equation (4) of Theorem 1. The top line (in gray) plots the marginal costs of attention under technology $j = 0$, while the bottom line (in black) plots marginal costs under technology $j = 1$. The area DCEF corresponds to the change in WTP for technology $j = 1$ over $j = 0$ when the financial incentive is increased from r to $r+\Delta$.

A.5 Interaction Between Incentives and Reminders

Let $q^0_{ai}(r)$ and $q^1_{ai}(r)$ be the chosen levels of attention given cost functions $\bar{K}^0$ and $\bar{K}^1$, respectively, and incentive level r. Let $q_{oi}(r)$ denote the auxiliary completion probability conditional on being attentive. Set $\Delta q_{ai}(r) = q^1_{ai}(r) - q^0_{ai}(r)$, and suppose that it is non-negative, meaning that the BE increases attentiveness. The impact of the BE on task completion depends on incentives r as follows,

$$\frac{d}{dr}D_i(z=1|r) = \frac{d}{dr}\left[\Delta q_{ai}(r)\cdot q_{oi}(r)\right]$$
$$= \left(\frac{d}{dr}\Delta q_{ai}(r)\right) q_{oi}(r) + \Delta q_{ai}(r) q'_{oi}(r) \quad (19)$$

Under optimally-chosen auxiliary actions, q_{oi} is increasing in r. Moreover, since the BE increases task-completion, $\Delta q_{ai}(r) \geq 0$. Thus, equation (19) can be negative only if $\frac{d}{dr}\Delta q_{ai}(r) < 0$, meaning that the BE and incentives are substitutes in people's attention allocation decisions. If attention is chosen optimally, $q^j_{ai}(r)$ is non-decreasing in r, and in fact any plausible model would make that implication. Combining this property with $\frac{d}{dr}\Delta q_{ai}(r) < 0$ implies that $q^0_{ai}(r)$ must be strictly increasing in r.

B Additional Results for Experiment 1

Table A.1: Participant Characteristics (Experiment 1)

Students		Alumni	
First-year	0.28	2017	0.22
	(0.45)		(0.41)
Sophomore	0.22	2016	0.18
	(0.41)		(0.39)
Junior	0.23	2015	0.21
	(0.42)		(0.41)
Senior	0.28	2014	0.19
	(0.45)		(0.39)
		2013	0.20
			(0.40)
Female	0.65	Female	0.70
	(0.48)		(0.46)
Male	0.31	Male	0.27
	(0.46)		(0.44)
Non-binary or no answer	0.04	Non-binary or no answer	0.03
	(0.20)		(0.21)
N	686	N	687

This table presents summary statistics for the participants in experiment 1, split between student and alumni groups. These participants were randomized to our various treatments as described in the main text. The Pay-to-Code sample includes 496 participants divided between \$2 and \$5 incentive arms. The Pay-to-Plan sample includes 487 participants divided between \$1 and \$2 incentive arms. The remaining participants include 218 control participants and 172 participants assigned to the *Combination* treatment.

Table A.2: The Effect of Coding-Task Incentives on Task Completion

	(1) Week 1	(2) Weeks 1-4	(3) Weeks 1-8
>0	0.036***	0.032***	0.026***
	(0.009)	(0.007)	(0.006)
Obs.	714	714	714
R^2	0.039	0.059	0.064
Control Mean	0.385	0.278	0.210
>10	0.037***	0.034***	0.027***
	(0.009)	(0.007)	(0.006)
Obs.	714	714	714
R^2	0.047	0.067	0.072
Control Mean	0.339	0.243	0.179
>30	0.036***	0.027***	0.023***
	(0.009)	(0.006)	(0.005)
Obs.	714	714	714
R^2	0.043	0.053	0.068
Control Mean	0.239	0.186	0.138
>40	0.038***	0.026***	0.021***
	(0.009)	(0.006)	(0.005)
Obs.	714	714	714
R^2	0.044	0.058	0.074
Control Mean	0.183	0.161	0.119
>50	0.032***	0.022***	0.017***
	(0.008)	(0.005)	(0.004)
Obs.	714	714	714
R^2	0.037	0.058	0.071
Control Mean	0.165	0.142	0.107
>60	0.027***	0.019***	0.013***
	(0.008)	(0.005)	(0.004)
Obs.	714	714	714
R^2	0.044	0.066	0.066
Control Mean	0.138	0.118	0.093
Controls	Yes	Yes	Yes
Campus × Student FE	Yes	Yes	Yes

This table presents estimates for the effect of coding-task incentives (in dollars) on task completion. Each panel of the table corresponds to an analysis of whether participants completed at least that number of minutes of the coding task in a given week. The columns correspond to different periods during the experiment over which the effect of the incentives is tested: Column (1) shows the effect in week 1, Column (2) shows the effect for weeks 1-4, and Column (3) shows the effect over all weeks. In Column (1), the dependent variable is an indicator for whether a participant completed at least that many minutes of the coding task in the first week. In Columns (2) and (3), the dependent variable is the mean of the indicators, constructed as in Column (1), for each of the weeks being considered. Each panel-by-column corresponds to a separate specification, and thus 18 distinct specifications are shown in the table. Standard errors are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table A.3: The Effect of Plan-Making Incentives on Task Completion

	(1) Week 1	(2) Weeks 1-4	(3) Weeks 1-8
>0	0.037*	0.029*	0.014
	(0.022)	(0.015)	(0.012)
Obs.	705	705	705
R^2	0.041	0.040	0.046
Control Mean	0.385	0.278	0.210
>10	0.037*	0.027*	0.014
	(0.021)	(0.014)	(0.011)
Obs.	705	705	705
R^2	0.045	0.045	0.046
Control Mean	0.339	0.243	0.179
>30	0.045**	0.023*	0.010
	(0.020)	(0.013)	(0.010)
Obs.	705	705	705
R^2	0.054	0.042	0.045
Control Mean	0.239	0.186	0.138
>40	0.036**	0.019	0.008
	(0.018)	(0.012)	(0.009)
Obs.	705	705	705
R^2	0.034	0.036	0.041
Control Mean	0.183	0.161	0.119
>50	0.034*	0.015	0.005
	(0.018)	(0.011)	(0.008)
Obs.	705	705	705
R^2	0.035	0.039	0.042
Control Mean	0.165	0.142	0.107
>60	0.027*	0.013	0.002
	(0.016)	(0.010)	(0.008)
Obs.	705	705	705
R^2	0.044	0.038	0.042
Control Mean	0.138	0.118	0.093
Controls	Yes	Yes	Yes
Campus × Student FE	Yes	Yes	Yes

This table presents estimates for the effect of plan-making incentives (in dollars) on task completion. Each panel of the table corresponds to an analysis of whether participants completed at least that number of minutes of the task in a given week. The columns correspond to different periods during the experiment over which the effect of the incentives is tested: Column (1) shows the effect in week 1, Column (2) shows the effect for weeks 1-4, and Column (3) shows the effect for all weeks. In Column (1), the dependent variable is an indicator for whether a participant completed at least that many minutes of the coding task in the first week. In Columns (2) and (3), the dependent variable is the mean of the indicators, constructed as in Column (1), for each of the weeks being considered. Each panel-by-column corresponds to a separate specification, and thus 18 distinct specifications are shown in the table. Standard errors are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.4: The Effect of Plan-Making Incentives on Task Completion (2SLS)

A. The Effect on Plan Making (First Stage)

	(1) Week 1	(2) Weeks 1-4	(3) Weeks 1-8
$1 Plan	0.282***	0.285***	0.240***
	(0.048)	(0.033)	(0.030)
$2 Plan	0.368***	0.297***	0.242***
	(0.033)	(0.028)	(0.025)
Obs.	705	705	705
R^2	0.144	0.189	0.157
Control Mean	0.381	0.150	0.082
Controls	Yes	Yes	Yes
Campus FE	Yes	Yes	Yes

B. The Effect on Coding Task Completion (Reduced Form)

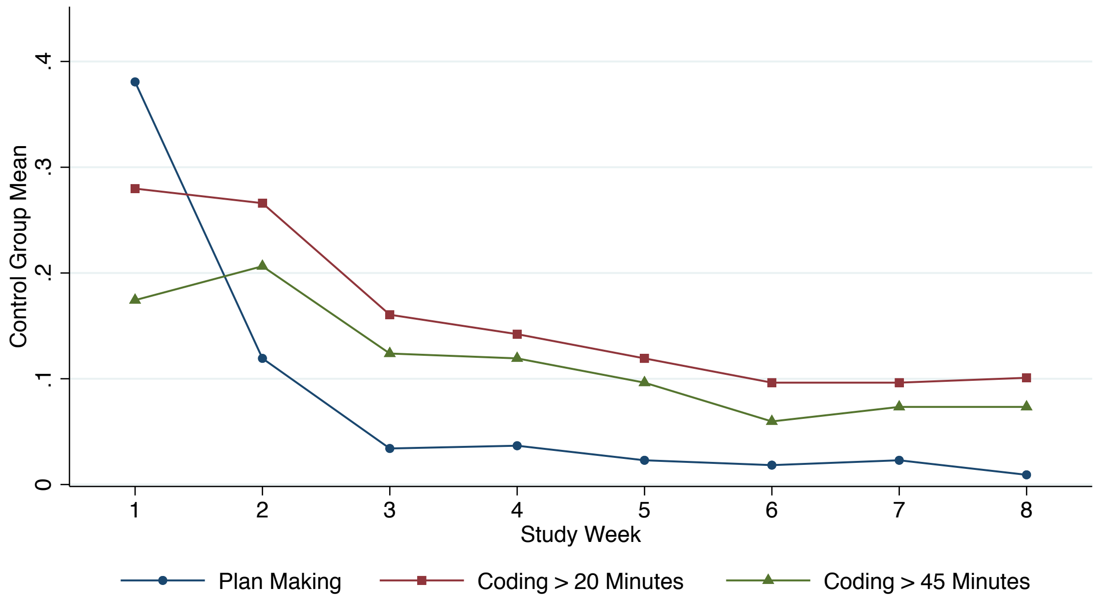
	(1) >20 (1)	(2) >20 (1-4)	(3) >20 (1-8)	(4) >45 (1)	(5) >45 (1-4)	(6) >45 (1-8)
$1 Plan	0.034	0.017	0.013	0.034	-0.000	0.005
	(0.048)	(0.032)	(0.025)	(0.043)	(0.027)	(0.021)
$2 Plan	0.079*	0.054**	0.026	0.076**	0.032	0.012
	(0.040)	(0.027)	(0.021)	(0.036)	(0.023)	(0.018)
Obs.	705	705	705	705	705	705
R^2	0.057	0.049	0.051	0.036	0.035	0.041
Control Mean	0.280	0.212	0.158	0.174	0.156	0.116
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Campus FE	Yes	Yes	Yes	Yes	Yes	Yes

C. The Effect of Plan Making on Coding Task Completion (IV)

	(1) >20 (1)	(2) >20 (1-4)	(3) >20 (1-8)	(4) >45 (1)	(5) >45 (1-4)	(6) >45 (1-8)
Plan Making	0.203**	0.146*	0.092	0.197**	0.076	0.041
	(0.102)	(0.080)	(0.078)	(0.093)	(0.070)	(0.066)
Obs.	705	705	705	705	705	705
R^2	0.143	0.151	0.120	0.091	0.094	0.076
Control Mean	0.280	0.212	0.158	0.174	0.156	0.116
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Campus FE	Yes	Yes	Yes	Yes	Yes	Yes

This table shows estimates for the effect of plan-making incentives on plan making and task completion using treatment dummies rather than a linear plan-making incentive variable. Panel A shows estimates of the effect of plan-making incentives on whether or not participants made a plan. Column (1) shows the effect of plan-making incentives in week 1. Column (2) shows the average effect over weeks 1-4. Column (3) shows the average effect over all weeks. Panel B shows the effect of plan-making incentives on task completion. Columns (1)–(3) show the effect on an indicator variable for whether or not the participant worked on the coding task for more than 20 minutes: Column (1) estimates the effect over week 1, Column (2) over weeks 1-4, and Column (3) over all weeks. Columns (4)–(6) show analogous estimates, but for an indicator variable for whether or not the participant worked on the task for more than 45 minutes each week. Panel C shows the 2SLS estimates instrumenting for whether or not participants made a plan using the plan-making treatment dummies as instruments. The dependent variables are the same as those in Panel B. Standard errors are shown in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

Figure A.2: Experiment 1 Control Group Means (Week-by-Week)



This figure shows control group means for plan making and completing at least 20 minutes or at least 45 minutes of the coding course for each week of the study.

C Additional Results for Experiment 2

Table A.5: The Effect of Incentive, Delay, and Reminders on Part-2 Survey Completion (Categorical Delay)

	Completed Part-2 Survey		
	(1)	(2)	(3)
Received Reminder	0.23***	0.15***	0.16***
	(0.021)	(0.045)	(0.044)
High Incentive	0.08***	0.14***	0.13***
	(0.021)	(0.029)	(0.031)
1-Week Delay	-0.16***	-0.22***	-0.22***
	(0.029)	(0.041)	(0.072)
3-Week Delay	-0.17***	-0.27***	-0.27***
	(0.030)	(0.042)	(0.067)
6-Week Delay	-0.21***	-0.32***	-0.30***
	(0.030)	(0.041)	(0.063)
Received Reminder × High Incentive		-0.13***	-0.11***
		(0.042)	(0.040)
1-Week Delay × Received Reminder		0.13**	0.10
		(0.058)	(0.075)
3-Week Delay × Received Reminder		0.20***	0.20***
		(0.060)	(0.057)
6-Week Delay × Received Reminder		0.22***	0.18***
		(0.059)	(0.063)
Constant	0.55***	0.59***	0.59***
	(0.025)	(0.032)	(0.043)
Observations	2,076	2,076	2,076
Number of Participants	2,076	2,076	2,076
S.E. Clustered by P1 & P2 Date			X
P1 Date FE			X

This table estimates how survey completion varies with reminders, delay, and whether participants are offered high incentives (i.e., \$11 or \$12) or low incentives (i.e., \$3 or \$4) to complete the survey. The 2-day delay variable is omitted so the 2-day delay is the excluded group. This table only includes participants who were randomly assigned to receive or not receive reminders. Column (3) reproduces Column (2) with fixed effects for the date that part 1 of the study was taken and with standard errors clustered for the date the participant completed part 1 and the date part 2 was made available to them. Standard errors are shown in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

Table A.6: The Effect of Incentive and Delay on Willingness to Pay for Reminders (Only Participants Randomized for Reminders)

	WTP for Reminders ($)					
	(1)	(2)	(3)	(4)	(5)	(6)
Extra $1	0.07***	0.08	0.06	0.07***	0.10***	0.11*
	(0.018)	(0.055)	(0.038)	(0.018)	(0.019)	(0.055)
High Incentive	0.96***	0.96***	1.14***	0.96***	0.96***	0.96***
	(0.082)	(0.082)	(0.118)	(0.082)	(0.082)	(0.082)
Extra $1 × High Incentive	-0.05	-0.05	-0.01	-0.05	-0.06	-0.06
	(0.050)	(0.050)	(0.116)	(0.050)	(0.051)	(0.051)
Ln(P2 Delay)		-0.07***	-0.04***			-0.07***
		(0.022)	(0.013)			(0.023)
Extra $1 × Ln(P2 Delay)		-0.00	0.01			-0.00
		(0.022)	(0.014)			(0.022)
High Incentive × Ln(P2 Delay)			-0.08**			
			(0.036)			
Extra $1 × Ln(P2 Delay)			-0.02			
× High Incentive			(0.044)			
Constant	0.51***	0.68***	0.59***	0.51***	0.51***	0.68***
	(0.034)	(0.060)	(0.044)	(0.034)	(0.034)	(0.060)
Observations	33,216	33,216	33,216	33,216	33,216	33,216
Number of Participants	2,076	2,076	2,076	2,076	2,076	2,076
Specification	OLS	OLS	OLS	OLS	Tobit	Tobit
P1 Date FE				X		

This table estimates how willingness to pay for reminders varies with the natural log of delay (in days) and incentives to complete the survey. This table only includes participants who were randomly assigned to receive or not receive reminders. The High Incentive variable is an indicator for being asked about an incentive of $11 or $12. The Extra $1 variable is an indicator for being asked about an incentive of $4 or $12. Column (4) reproduces Column (1) with fixed effects for the date that part 1 of the survey was taken; Columns (5) and (6) reproduce Columns (1) and (2) using Tobit estimates with censors at -$4 and $4 for the low-incentive group and censors at -$12 and $12 for the high-incentive group. Standard errors, clustered at the participant level, are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table A.7: The Effect of Incentive and Delay on Willingness to Pay for Reminders by MPL

	WTP for Reminders ($)			
	(1)	(2)	(3)	(4)
Extra $1	0.15*	0.08	0.11***	0.09***
	(0.078)	(0.050)	(0.034)	(0.026)
High Incentive	0.58***	0.68***	0.79***	0.77***
	(0.193)	(0.154)	(0.143)	(0.139)
Extra $1 × High Incentive	0.18	0.02	-0.10	-0.02
	(0.244)	(0.150)	(0.106)	(0.077)
Constant	0.45***	0.49***	0.50***	0.51***
	(0.063)	(0.053)	(0.048)	(0.046)
Observations	4,612	9,224	13,836	18,448
Number of Participants	2,306	2,306	2,306	2,306
First T MPLs	$T = 2$	$T = 4$	$T = 6$	$T = 8$

This table estimates how willingness to pay for reminders varies with incentives to complete the survey. The High Incentive variable is an indicator for being asked about an incentive of $11 or $12. The Extra $1 variable is an indicator for being asked about an incentive of $4 or $12. Column (1) shows these estimates when limited to the first 2 MPLs participants are asked about, Column (2) shows these estimates when limited to the first 4 MPLs, Column (3) shows these estimates when limited to the first 6 MPLs, and Column (4) shows these estimates when limited to the first 8 MPLs. Standard errors, clustered at the participant level, are shown in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

Table A.8: The Effect of Incentive and Delay on WTP for Reminders (Categorical Delay)

	WTP for Reminders ($)				
	(1)	(2)	(3)	(4)	(5)
Extra $1	0.07***	0.05	0.07***	0.10***	0.08
	(0.017)	(0.047)	(0.017)	(0.018)	(0.047)
High Incentive	0.93***	0.93***	0.93***	0.93***	0.93***
	(0.077)	(0.077)	(0.077)	(0.077)	(0.077)
Extra $1 × High Incentive	-0.06	-0.06	-0.06	-0.07	-0.07
	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)
1-Week Delay		-0.16***			-0.16***
		(0.054)			(0.055)
3-Week Delay		-0.19***			-0.19***
		(0.062)			(0.062)
6-Week Delay		-0.25***			-0.25***
		(0.067)			(0.067)
1-Week Delay × Extra $1		0.03			0.03
		(0.073)			(0.074)
3-Week Delay × Extra $1		0.02			0.02
		(0.068)			(0.068)
6-Week Delay × Extra $1		0.03			0.03
		(0.068)			(0.068)
Constant	0.51***	0.66***	0.51***	0.50***	0.65***
	(0.032)	(0.047)	(0.032)	(0.032)	(0.047)
Observations	36,896	36,896	36,896	36,896	36,896
Number of Participants	2,306	2,306	2,306	2,306	2,306
Specification	OLS	OLS	OLS	Tobit	Tobit
P1 Date FE			X		

This table estimates how willingness to pay for reminders varies with incentives to complete the survey. The High Incentive variable is an indicator for being asked about an incentive of $11 or $12. The Extra $1 variable is an indicator for being asked about an incentive of $4 or $12. Column (2) maintains the specification in Column (1) and adds controls for delay; Column (3) shows Column (1) with fixed effects for the date that part 1 of the survey was taken; Columns (4) and (5) reproduce Columns (1) and (2) using Tobit estimates with censors at -$4 and $4 for the low-incentive group and censors at -$12 and $12 for the high-incentive group. Standard errors, clustered at the participant level, are shown in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

D Additional Results for Experiment 3

Table A.9: Replication of Table 6 with Tobit Models

	Willingness to Pay ($)					
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive ($)	0.11***	-0.00	0.05	0.03	0.08*	0.03
	(0.035)	(0.031)	(0.046)	(0.042)	(0.052)	(0.073)
Incentive ($) × Block 2	-0.07*	0.04	0.01	0.04	0.00	0.02
	(0.038)	(0.037)	(0.051)	(0.052)	(0.065)	(0.076)
Incentive ($) × Feedback			0.11	-0.07	0.12	0.11
			(0.069)	(0.061)	(0.093)	(0.100)
Incentive ($) × Block 2			-0.16**	0.01	-0.22**	-0.11
× Feedback			(0.076)	(0.075)	(0.103)	(0.110)
Block 2	0.14	-0.18	-0.10	-0.21	-0.17	-0.06
	(0.126)	(0.124)	(0.178)	(0.177)	(0.236)	(0.260)
Block 2 × Feedback			0.49*	0.05	0.89***	0.17
			(0.251)	(0.248)	(0.343)	(0.359)
Feedback			-0.23	0.22	-0.37	-0.12
			(0.250)	(0.214)	(0.347)	(0.354)
Constant	0.17	0.51***	0.29*	0.40***	0.28	0.30
	(0.125)	(0.107)	(0.172)	(0.148)	(0.212)	(0.260)
Observations	3,996	4,794	3,996	4,794	1,788	2,208
Number of Participants	666	799	666	799	298	368
Participant B1 Acc. Diff.	All	All	All	All	≤ 0	> 0
Arm	Length	Discernibility	Length	Discernibility	Length	Length

This table replicates Table 6 but presents Tobit estimates with censors at 4 and -4 for the effect of accuracy incentive, block order, and whether the participant received feedback on their block 1-performance on willingness to pay for an easy task (i.e., a task with shorter length in the length arm, or a task with increased discernibility in the discernibility arm). Standard errors, clustered at the participant level, are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table A.10: Replication of Table 6, Dropping the 10% Fastest Participants

	Willingness to Pay ($)					
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive ($)	0.10***	-0.01	0.04	0.02	0.08	0.01
	(0.037)	(0.033)	(0.051)	(0.046)	(0.061)	(0.076)
Incentive ($) × Block 2	-0.07*	0.03	0.02	0.03	0.03	0.02
	(0.041)	(0.040)	(0.055)	(0.056)	(0.075)	(0.078)
Incentive ($) × Feedback			0.12*	-0.07	0.14	0.12
			(0.073)	(0.066)	(0.096)	(0.103)
Incentive ($) × Block 2			-0.19**	0.00	-0.31***	-0.12
× Feedback			(0.082)	(0.081)	(0.117)	(0.112)
Block 2	0.16	-0.12	-0.13	-0.15	-0.24	-0.06
	(0.138)	(0.133)	(0.198)	(0.189)	(0.277)	(0.275)
Block 2 × Feedback			0.59**	0.08	1.24***	0.20
			(0.274)	(0.266)	(0.397)	(0.371)
Feedback			-0.15	0.17	-0.30	-0.06
			(0.270)	(0.231)	(0.389)	(0.369)
Constant	0.17	0.50***	0.24	0.42***	0.18	0.28
	(0.135)	(0.115)	(0.192)	(0.160)	(0.252)	(0.275)
Observations	3,438	4,314	3,438	4,314	1,344	2,094
Number of Participants	573	719	573	719	224	349
Participant B1 Acc. Diff.	All	All	All	All	≤ 0	> 0
Arm	Length	Discernibility	Length	Discernibility	Length	Length

This table replicates Table 6 on the effect of accuracy incentive, block order, and whether the participant received feedback on their block 1-performance on willingness to pay for an easy task (i.e., a task with shorter length in the length arm, or a task with increased discernibility in the discernibility arm) after dropping participants in the top 10% of fastest task times in the length arm and the top 10% of fastest task times in the discernibility arm. Standard errors, clustered at the participant level, are shown in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

Table A.11: The Effect of Block and Feedback on Block Accuracy Difference

	Accuracy Difference Between Easy and Baseline Tasks					
	(1)	(2)	(3)	(4)	(5)	(6)
Block 2	0.03	0.01	0.20***	-0.13***	0.19***	-0.10***
	(0.021)	(0.018)	(0.018)	(0.020)	(0.027)	(0.029)
Feedback	0.03	-0.01			0.02	0.03
	(0.023)	(0.020)			(0.016)	(0.023)
Block 2 × Feedback	-0.04	-0.01			0.00	-0.07*
	(0.030)	(0.025)			(0.036)	(0.039)
Constant	0.18***	0.26***	-0.06***	0.40***	-0.07***	0.39***
	(0.016)	(0.014)	(0.008)	(0.012)	(0.013)	(0.016)
Observations	1,332	1,598	596	736	596	736
Number of Participants	666	799	298	368	298	368
Participant B1 Acc. Diff.	All	All	≤ 0	> 0	≤ 0	> 0
Arm	Length	Discernibility	Length	Length	Length	Length

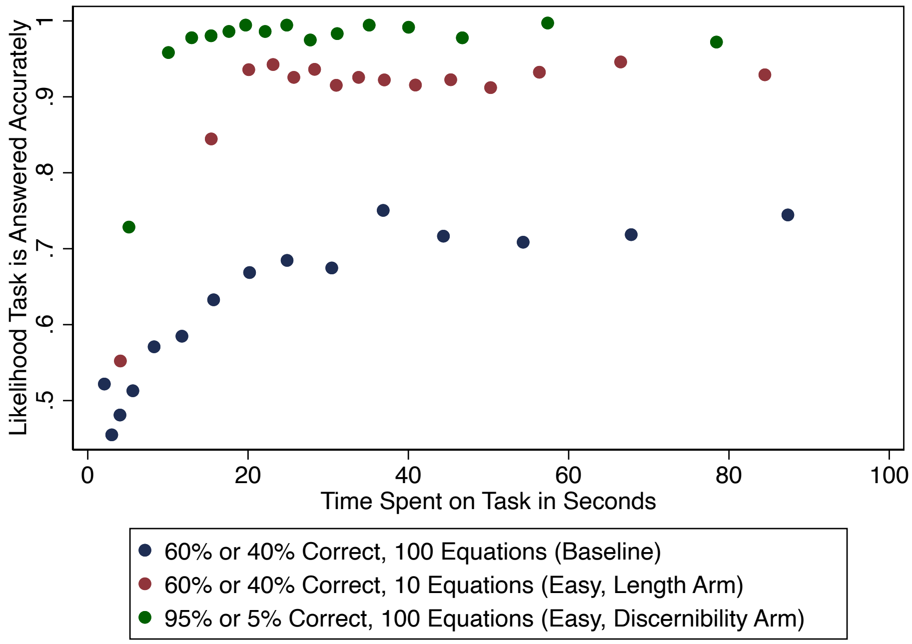
This table estimates the effect of block order and feedback on the accuracy difference between easy and baseline tasks within a block. The accuracy difference is constructed by taking the difference between the percentage of easy tasks answered correctly and the percentage of baseline tasks answered correctly in a block. Column (1) shows OLS estimates for participants in the length arm; Column (2) shows OLS estimates for participants in the discernibility arm; Columns (3) and (5) restrict to participants in the length arm who had a block-1 accuracy difference less than or equal to 0 (i.e., who were at least as accurate in the baseline tasks as in the easy tasks); Columns (4) and (6) restrict to participants in the length arm who had a block-1 accuracy difference greater than 0 (i.e., who were more accurate in the easy tasks than the baseline tasks). Standard errors, clustered at the participant level, are shown in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

Table A.12: The Effect of Block, Feedback, and Accuracy Difference on Within-Block Time Spent on Baseline vs. Easy Tasks

	Average Difference in Time Spent on Baseline vs. Easy Tasks, By Block					
	(1)	(2)	(3)	(4)	(5)	(6)
Block 2	-21.72***	-13.78**	-19.24***	-11.59**	-16.63***	-9.04*
	(4.125)	(6.078)	(3.562)	(5.444)	(3.144)	(4.740)
B1 Acc. Diff ≤ 0	115.45***	106.94***	103.16***	93.86***	79.81***	74.50***
	(10.006)	(14.582)	(8.194)	(11.926)	(6.141)	(9.036)
Block 2 $\times$ B1 Acc. Diff ≤ 0	-43.62***	-54.99***	-33.48***	-42.74***	-16.53***	-26.72***
	(7.547)	(11.468)	(5.928)	(8.959)	(4.665)	(6.987)
Feedback		3.53		3.40		6.12
		(9.958)		(8.729)		(7.343)
Block 2 $\times$ Feedback		-16.07*		-15.47**		-15.34**
		(8.211)		(7.074)		(6.236)
B1 Acc. Diff ≤ 0 $\times$ Feedback		17.26		18.86		10.78
		(19.988)		(16.347)		(12.243)
Block 2 $\times$ B1 Acc. Diff ≤ 0		23.01		18.74		20.61**
$\times$ Feedback		(15.056)		(11.823)		(9.290)
Constant	19.23***	17.48**	16.51***	14.83**	11.79***	8.76
	(4.978)	(7.384)	(4.366)	(6.615)	(3.673)	(5.423)
Observations	1,332	1,332	1,332	1,332	1,332	1,332
Number of Participants	666	666	666	666	666	666
Winsorized at T Seconds	No	No	$T = 300$	$T = 300$	$T = 180$	$T = 180$

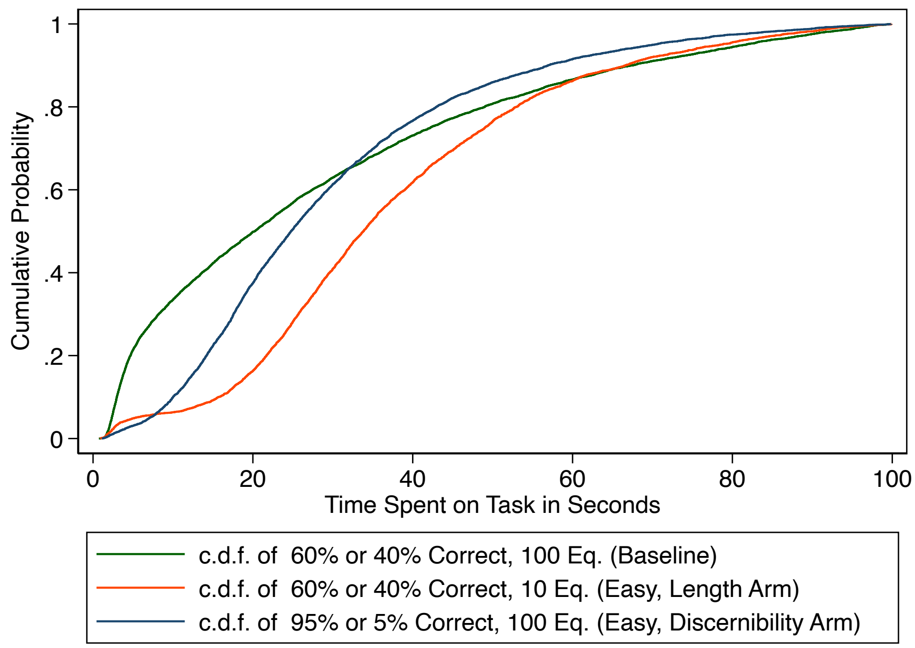
This table estimates the effect of block, feedback and accuracy difference on the average difference in time spent on baseline vs. easy tasks for participants in the length arm. The dependent variable is constructed by taking the difference between average time spent on the baseline tasks in a block and the average time spent on the easy tasks in the same block. By-block accuracy difference is constructed by taking the difference between the percentage of easy tasks answered correctly and the percentage of baseline tasks answered correctly in a block. Column (3) maintains the specification in Column (1) and winsorizes at 300 seconds; Column (4) maintains the specification in Column (2) and winsorizes at 300 seconds; Column (5) maintains the specification in Column (1) and winsorizes at 180 seconds; Column (6) maintains the specification in Column (2) and winsorizes at 180 seconds. Standard errors, clustered at the participant level, are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Figure A.3: The Likelihood of Answering Tasks Accurately by Time Spent, Dropping Observations > 100 Seconds



This figure includes binned scatterplots that displays how accuracy varies with the time spent on the three types of tasks in seconds after dropping responses in which a participant spent more than 100 seconds on a task. Here, observations have been separated into 15 equal-sized bins in each binned scatterplot.

Figure A.4: CDFs of Time Spent on Task by Length and Discernibility (Dropping Observations > 100 Seconds)



This figure displays the CDFs of time spent on a task in seconds by task type after dropping responses in which a participant spent more than 100 seconds on a task.

E Implications of Risk Aversion

E.1 Calibration exercises

Let π_1 denote the probability of completing the task with BE, and let π_0 denote the probability of completing the task without the BE. Suppose that individuals value the BE optimally, but are potentially risk averse. Let z denote the initial wealth of an individual before starting the experiment. We consider two cases.

Case 1: Constant absolute risk aversion Let α be the CARA parameter. Analogous to before, an application of the Envelope Theorem implies that a marginal increase dr in r increases the individual's expected utility by $\pi_1 e^{-\alpha(z+r)}dr$ in the presence of the BE, and by $\pi_0 e^{-\alpha(z+r)}dr$ in the absence of a BE. A marginal increase dw in w, where $w > 0$ and thus is a payment received in the absence of the BE, increases an individual's expected utility by $\pi_0 e^{-\alpha(z+w+r)}dw + (1-\pi_0)e^{-\alpha(z+w)}dw$ in the absence of the BE. Dividing through by $e^{-\alpha z}$

implies that the the impact of r on the WTP for a BE is given by

$$\frac{dw}{dr} = \frac{\pi_1 e^{-\alpha r} - \pi_0 e^{-\alpha r}}{\pi_0 e^{-\alpha(w+r)} + (1-\pi_0)e^{-\alpha w}} \tag{20}$$

We use equation (20) to estimate how WTP for a BE changes when the reward for (accurate) task completion increases by \$1. In experiment 2, we set $r = \$3$ for the low incentive conditions and $r = \$11$ for the high incentive conditions. We set π_0 and π_1 to match the empirical completion probabilities in the 8 delay$\times$incentive conditions in Figure 2a. We set w to match the average WTP for the BE at the \$3 or \$11 incentive values in the 8 delay$\times$incentive conditions in Figure 3.

In experiment 3, we set $r = \$2$, and we set w to match the average WTP for the BE at the $r = \$2$ incentive value, in each of the 4 conditions corresponding to either the length or discernibility arm, and either block 1 or block 2.

We draw prior work to consider the following values of α: Using insurance decisions, Cohen and Einav (2007) estimate $\alpha \approx (0.00087, 0.0019)$, Handel (2013) estimates $\alpha \approx (0.00019, 0.000325)$, and Sydnor (2010) estimates $\alpha \approx 0.002$. Chetty (2006) estimates a constant relative risk aversion coefficient of 0.7 from labor supply elasticities, which translates to $\alpha \approx 0.0007$ if payday borrowers have \$1000 monthly "uncommitted" (in the sense of Chetty and Szeidl (2007)) consumption. Using relatively small-stakes gambles, von Gaudecker et al. (2011) estimate $\alpha \approx 0.03$, and Holt and Laury (2002) estimate $\alpha \approx 0.2$. For studies that provide a range, we take the midpoint of the range.

Table A.13 below considers experiment 2 data under the hypothesis that people value BEs optimally but are risk averse. The table presents estimates of how the WTP for the BE would change when r increases by \$1, across the 8 delay$\times$incentive conditions, and across the different values of α summarized above. We set $\alpha = 0$ in the first row, to benchmark to the quasilinear case assumed in the body of the paper. The very last row in the table, separated by double lines, presents our empirical estimates.

Table A.13: Experiment 2, Effect of Extra \$1 Incentive on WTP by Delay $\times$ Incentive Condition and α (CARA parameter)

	2 Day $\times$ Low Incentive	1 Week $\times$ Low Incentive	3 Weeks $\times$ Low Incentive	6 Weeks $\times$ Low Incentive	2 Day $\times$ High Incentive	1 Week $\times$ High Incentive	3 Weeks $\times$ High Incentive	6 Weeks $\times$ High Incentive
$\alpha = 0$	0.23	0.28	0.40	0.25	−0.07	0.16	0.18	0.36
$\alpha \approx (0.00087, 0.0019)$ Cohen and Einav (2007)	(0.23, 0.23)	(0.28, 0.28)	(0.40, 0.40)	(0.25, 0.24)	(−0.07, −0.07)	(0.16, 0.16)	(0.18, 0.18)	(0.36, 0.36)
$\alpha \approx (0.00019, 0.000325)$ Handel (2013)	(0.23, 0.23)	(0.28, 0.28)	(0.40, 0.40)	(0.25, 0.25)	(−0.07, −0.07)	(0.16, 0.16)	(0.18, 0.18)	(0.36, 0.36)
$\alpha \approx 0.002$ Sydnor (2010)	0.23	0.28	0.40	0.24	−0.07	0.16	0.18	0.36
$\alpha \approx 0.0007$ Chetty (2006)	0.23	0.28	0.40	0.25	−0.07	0.16	0.18	0.36
$\alpha \approx 0.03$ von Gaudecker et al. (2011)	0.23	0.27	0.38	0.23	−0.07	0.14	0.15	0.30
$\alpha \approx 0.2$ Holt and Laury (2002)	0.19	0.21	0.27	0.17	−0.03	0.04	0.05	0.07
Empirical estimates	0.06	0.10	0.07	0.08	−0.00	0.01	0.03	0.03

Notes: This table presents estimates of how the WTP for the BE would change when r increases by \$1, across the 8 delay × incentive conditions and across the values of α summarized above.

Table A.14 below considers experiment 3 data under the hypothesis that people value BEs optimally but are risk averse. The table presents estimates of how the WTP for the BE changes when r increases by \$1, across the 4 conditions corresponding to either the length or discernibility arm, and either block 1 or block 2. As in Table A.13, we consider the different values of α summarized above, as well as the risk-neutral $\alpha = 0$ in the first row. The very last row in the table, separated by double lines, presents our empirical estimates.

Table A.14: Experiment 3, Effect of Extra \$1 Incentive on WTP by Arm $\times$ Block and α (CARA parameter)

	Length Arm $\times$ Block 1	Discernibility Arm $\times$ Block 1	Length Arm $\times$ Block 2	Discernibility Arm $\times$ Block 2
$\alpha = 0$	0.20	0.26	0.21	0.26
$\alpha \approx (0.00087, 0.0019)$ Cohen and Einav (2007)	(0.20, 0.20)	(0.26, 0.26)	(0.21, 0.21)	(0.26, 0.26)
$\alpha \approx (0.00019, 0.000325)$ Handel (2013)	(0.20, 0.20)	(0.26, 0.26)	(0.21, 0.21)	(0.26, 0.26)
$\alpha \approx 0.002$ Sydnor (2010)	0.20	0.26	0.21	0.26
$\alpha \approx 0.0007$ Chetty (2006)	0.20	0.26	0.21	0.26
$\alpha \approx 0.03$ von Gaudecker et al. (2011)	0.19	0.26	0.21	0.26
$\alpha \approx 0.2$ Holt and Laury (2002)	0.19	0.25	0.20	0.25
Empirical estimates	0.10	−0.01	0.03	0.03

Notes: This table presents estimates of how the WTP for the BE would change when r increases by \$1, across the 4 arm × block conditions and across the values of α summarized above.

Case 2: Constant relative risk aversion Let ρ be the CRRA parameter. Analogous to above, simple algebra shows that

$$\frac{dw}{dr} = \frac{\pi_1 - \pi_0}{\pi_0 \left(\frac{z+r}{z+w+r}\right)^{\rho} + (1-\pi_0)\left(\frac{z+r}{z+w}\right)^{\rho}} \tag{21}$$

To study the potential impacts of risk aversion, we consider the upper-bound value of $\rho = 1.37$, which Holt and Laury (2002) clarify implies a level of risk aversion that individuals with such a parameter should "stay in bed." Holt and Laury (2002) show that very few individuals exhibit such a value of risk aversion.

Table A.15below considers experiment 2 data under the hypothesis that people value BEs optimally but are risk averse. Utilizing equation (21), table presents estimates of how the WTP for the BE would change when r increases by \$1, across the 8 delay$\times$incentive conditions. The first row corresponds to the risk-neutral benchmark of $\rho = 0$. The subsequent rows consider $\rho = 1.37$ and vary assumptions about initial wealth z. The very last row in the table, separated by double lines, presents our empirical estimates.

Table A.16 is analogous, but considers the 4 conditions corresponding to either the length or discernibility arm, and either block 1 or block 2.

Table A.15: Experiment 2, Effect of Extra \$1 Incentive on WTP by Delay $\times$ Incentive Condition, initial wealth and ρ(CRRA parameter)

	2 Day × Low Incentive	1 Week × Low Incentive	3 Weeks × Low Incentive	6 Weeks × Low Incentive	2 Day × High Incentive	1 Week × High Incentive	3 Weeks × High Incentive	6 Weeks × High Incentive
$\rho = 0$	0.23	0.28	0.40	0.25	−0.07	0.16	0.18	0.36
$\rho = 1.37$, $z = 10$	0.21	0.24	0.32	0.20	−0.06	0.10	0.11	0.19
$\rho = 1.37$, $z = 100$	0.23	0.28	0.39	0.24	−0.07	0.15	0.17	0.34
$\rho = 1.37$, $z = 1000$	0.23	0.28	0.40	0.25	−0.07	0.16	0.18	0.36
$\rho = 1.37$, $z = 100000$	0.23	0.28	0.40	0.25	−0.07	0.16	0.18	0.36
Empirical estimates	0.06	0.10	0.07	0.08	−0.00	0.01	0.03	0.03

Notes: This table presents estimates of how the WTP for the BE would change when r increases by \$1, across the 8 delay × incentive conditions and across the values of ρ and z summarized above.

Table A.16: Experiment 3, Effect of Extra \$1 Incentive on WTP by Delay $\times$ Incentive Condition, initial wealth and ρ(CRRA parameter)

	Length Arm $\times$ Block 1	Discernibility Arm $\times$ Block 1	Length Arm $\times$ Block 2	Discernibility Arm $\times$ Block 2
$\rho = 0$	0.20	0.26	0.21	0.26
$\rho = 1.37, z = 10$	0.19	0.25	0.20	0.25
$\rho = 1.37, z = 100$	0.19	0.26	0.21	0.26
$\rho = 1.37, z = 1000$	0.20	0.26	0.21	0.26
$\rho = 1.37, z = 100000$	0.20	0.26	0.21	0.26
Empirical estimates	0.10	−0.01	0.03	0.03

Notes: This table presents estimates of how the WTP for the BE would change when r increases by \$1, across the 4 arm × block conditions and across the values of ρ and z summarized above.

E.2 Risk Aversion and Willingness to Pay in Experiment 2

Appendix Table A.17 explores whether the deviations in experiment 2 between willingness to pay and the effect of reminders on task completion depend on participant risk aversion. People are valuing reminders by selecting between a sure amount of money and a reminder that only increases the likelihood of a monetary reward. If risk aversion is sufficiently high, then risk aversion, rather than suboptimal attention, could account for these results.

We estimate how willingness to pay for reminders varies with the participant's level of risk aversion as measured in experiment 2, part 2 and with completion of the survey. The risk aversion variables are derived from participant answers to 10 questions in the second part of experiment 2, in which participants were asked to select between receiving a "for sure" amount of money and receiving a higher amount of money with 50% probability (see [fig:cash risk aversion question example] for an example). The Above Median Risky Choice Total variable is an indicator that takes the value of 1 if the number of times that a participant selected the uncertain choice is higher than the sample median. The Fraction of Risky Choices variable is the number of times a participant selected the uncertain choice divided by the total number of questions.

Column (1) shows that there is no difference in willingness to pay for reminders when comparing participants with relatively high versus low risk aversion in terms of the number of risky choices they select in part 2. These results hold both for the low incentive and high incentive groups. Column (2) recasts these results using the fraction of risky choices selected as the interaction term. Again, we see no interaction effect, which suggests that risk aversion is not driving participants' willingness to pay for reminders. In the case of the low incentive group, we can make an even stronger statement: an extreme risk-seeking individual (i.e., with the fraction of risky choices equal to 1), would still value reminders with a 95% confidence interval well below the estimated effect of reminders on task completion.[43] Finally, column (3) confirms that the subset of participants who complete part 2, and thus for whom we can measure risk aversion, have statistically indistinguishable willingness to pay for reminders as compared to the participants who do not complete part 2. This fact supports our extrapolation of the risk aversion results from columns (1) and (2) to characterize the full participant pool.

[43] The 95% confidence interval for the low incentive group for the effect of a $1 increase in incentives on WTP is $[0.008, 0.208]$ and the effect of reminders for this group is 0.29. For the high incentive group, the analogous calculations give $[-0.064, 0.464]$ and 0.16; thus, for this group, the confidence interval is too wide to permit a sharp test of the statement.

Table A.17: Effect of Risk Aversion (as Measured in Experiment 2 Part-2 Survey) on WTP

	WTP for Reminders ($)		
	(1)	(2)	(3)
Above Median Risky Choice Total	0.04		
	(0.085)		
Extra $1	0.08***	0.07**	0.05*
	(0.025)	(0.030)	(0.030)
High Incentive	0.96***	0.99***	1.04***
	(0.128)	(0.152)	(0.125)
Above Median Risky Choice Total × Extra $1	0.02		
	(0.042)		
Above Median Risky Choice Total × High Incentive	-0.15		
	(0.209)		
Extra $1 × High Incentive	-0.13	-0.20**	0.04
	(0.077)	(0.093)	(0.083)
Above Median Risky Choice Total × Extra $1 × High Incentive	0.09		
	(0.117)		
Fraction of Risky Choices		0.09	
		(0.131)	
Fraction of Risky Choices × Extra $1		0.03	
		(0.070)	
Fraction of Risky Choices × High Incentive		-0.23	
		(0.310)	
Fraction of Risky Choices × Extra $1 × High Incentive		0.29	
		(0.189)	
Completed Part-2 Survey			-0.04
			(0.068)
Completed Part-2 Survey × Extra $1			0.04
			(0.037)
Completed Part-2 Survey × High Incentive			-0.14
			(0.165)
Completed Part-2 Survey × Extra $1 × High Incentive			-0.15
			(0.104)
Constant	0.49***	0.48***	0.53***
	(0.054)	(0.064)	(0.052)
Observations	20,912	20,912	33,216
Number of Participants	1,307	1,307	2,076

This table estimates how willingness to pay for reminders varies with level of risk aversion and with completion of the survey. The risk aversion variables are derived from participant answers to 10 questions in the the part-2 survey of experiment 2, in which participants were asked to select between receiving a "for sure" amount of money and receiving a higher amount of money with 50% probability (see Figure A.5 for an example of how these questions were presented). The Above Median Risky Choice Total variable is an indicator for whether the number of times that a participant selected the uncertain choice is higher than the median. The Fraction of Risky Choices variable is the fraction of times a participant selected the uncertain choice. Columns (1) and (2) focus on the effect of risk-seeking behavior and are restricted to participants who were never top-coded at $3 or $11 and who were never top-coded at $4 or $12, as in all of the willingness-to-pay analysis. Column (3) focuses on the representativeness of willingness-to-pay responses for those who complete the part-2 survey, since all participants included in Columns (1) and (2) completed part-2 of the survey. Standard errors, clustered at the participant level, are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Figure A.5: Example Risk Aversion Question

Question 11 out of 40:

Would you rather have $59 for sure or a 50% chance of $90?

- $59 for sure
- a 50% chance of $90

Participants answered 10 questions in the same format as this, where the first value was randomly drawn from the integers between 35 and 65, and the second value was randomly drawn from the integers between 90 and 100.

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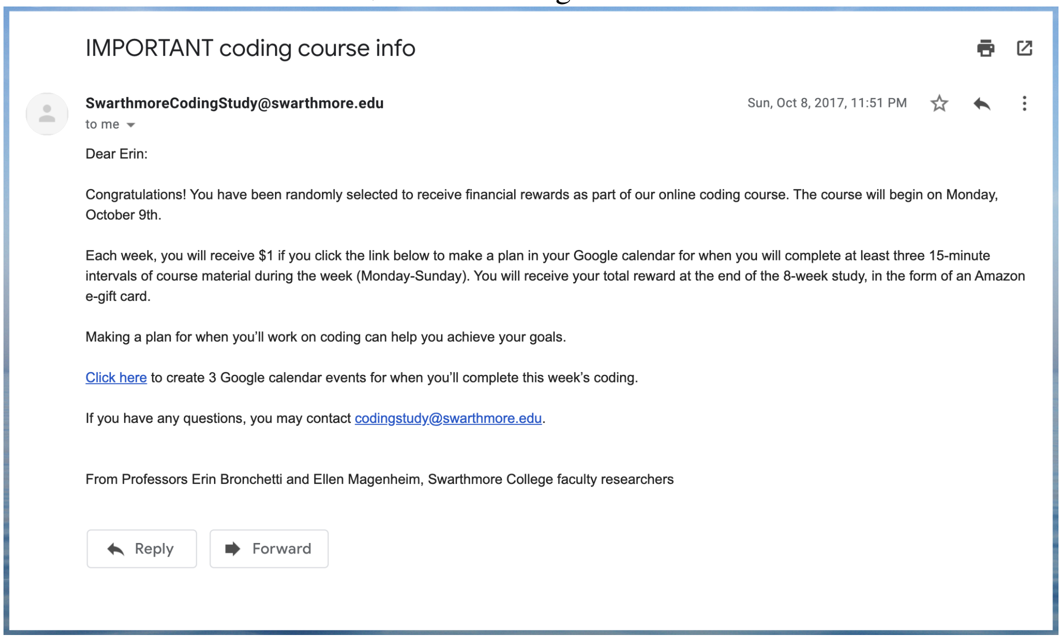
F Screenshots Appendix

F.1 Experiment 1

Figure S.1: *Pay-to-Plan* Treatment Emails, Week 1

$1 Plan-Making Incentive



IMPORTANT coding course info

SwarthmoreCodingStudy@swarthmore.edu Sun, Oct 8, 2017, 11:51 PM
to me

Dear Erin:

Congratulations! You have been randomly selected to receive financial rewards as part of our online coding course. The course will begin on Monday, October 9th.

Each week, you will receive $1 if you click the link below to make a plan in your Google calendar for when you will complete at least three 15-minute intervals of course material during the week (Monday-Sunday). You will receive your total reward at the end of the 8-week study, in the form of an Amazon e-gift card.

Making a plan for when you'll work on coding can help you achieve your goals.

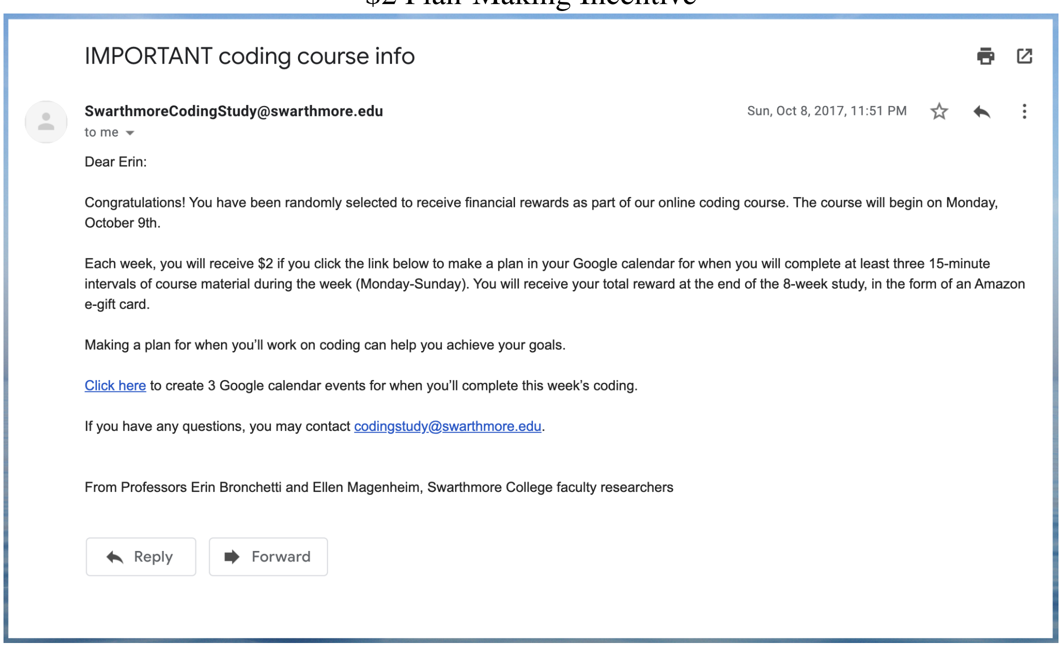
Click here to create 3 Google calendar events for when you'll complete this week's coding.

If you have any questions, you may contact codingstudy@swarthmore.edu.

From Professors Erin Bronchetti and Ellen Magenheim, Swarthmore College faculty researchers

Reply Forward

$2 Plan-Making Incentive



IMPORTANT coding course info

SwarthmoreCodingStudy@swarthmore.edu Sun, Oct 8, 2017, 11:51 PM
to me

Dear Erin:

Congratulations! You have been randomly selected to receive financial rewards as part of our online coding course. The course will begin on Monday, October 9th.

Each week, you will receive $2 if you click the link below to make a plan in your Google calendar for when you will complete at least three 15-minute intervals of course material during the week (Monday-Sunday). You will receive your total reward at the end of the 8-week study, in the form of an Amazon e-gift card.

Making a plan for when you'll work on coding can help you achieve your goals.

Click here to create 3 Google calendar events for when you'll complete this week's coding.

If you have any questions, you may contact codingstudy@swarthmore.edu.

From Professors Erin Bronchetti and Ellen Magenheim, Swarthmore College faculty researchers

Reply Forward

Figure S.2: *Pay-to-Code* Treatment Emails, Week 1

$2 Coding-Task Incentive

IMPORTANT coding course info

SwarthmoreCodingStudy@swarthmore.edu Sun, Oct 8, 2017, 11:50 PM
to me

Dear Erin:

Congratulations! You have been randomly selected to receive financial rewards for completing parts of the online coding course. The course will begin on Monday, October 9th.

Each week, you will receive $2 if you complete at least three 15-minute intervals of course material during that week (Monday-Sunday). You will receive your total reward at the end of the 8-week study, in the form of an Amazon e-gift card.

Making a plan for when you'll work on coding can help you achieve your goals.

Click here to create 3 Google calendar events for when you'll complete this week's coding.

If you have any questions, you may contact codingstudy@swarthmore.edu.

From Professors Erin Bronchetti and Ellen Magenheim, Swarthmore College faculty researchers

Reply Forward

$5 Coding-Task Incentive

IMPORTANT coding course info

SwarthmoreCodingStudy@swarthmore.edu Sun, Oct 8, 2017, 11:50 PM
to me

Dear Erin:

Congratulations! You have been randomly selected to receive financial rewards for completing parts of the online coding course. The course will begin on Monday, October 9th.

Each week, you will receive $5 if you complete at least three 15-minute intervals of course material during that week (Monday-Sunday). You will receive your total reward at the end of the 8-week study, in the form of an Amazon e-gift card.

Making a plan for when you'll work on coding can help you achieve your goals.

Click here to create 3 Google calendar events for when you'll complete this week's coding.

If you have any questions, you may contact codingstudy@swarthmore.edu.

From Professors Erin Bronchetti and Ellen Magenheim, Swarthmore College faculty researchers

Reply Forward

Figure S.3: *Combined* and Control Group Emails, Week 1

Combined Treatment ($1 Plan-Making and $2 Coding-Task Incentive)

IMPORTANT coding course info

SwarthmoreCodingStudy@swarthmore.edu Sun, Oct 8, 2017, 11:51 PM
to me

Dear Erin:

Congratulations! You have been randomly selected to receive financial rewards as part of our online coding course. The course will begin on Monday, October 9th.

Each week, you will receive $1 if you click the link below to make a plan in your Google calendar for when you will complete at least three 15-minute intervals of course material during the week (Monday-Sunday).

Each week, you will also receive $2 if you complete at least three 15-minute intervals of course material during that week (Monday-Sunday).

You will receive your total reward at the end of the 8-week study, in the form of an Amazon e-gift card.

Making a plan for when you'll work on coding can help you achieve your goals.

Click here to create 3 Google calendar events for when you'll complete this week's coding.

If you have any questions, you may contact codingstudy@swarthmore.edu.

From Professors Erin Bronchetti and Ellen Magenheim, Swarthmore College faculty researchers

Reply Forward

Control Group

IMPORTANT coding course info

SwarthmoreCodingStudy@swarthmore.edu Sun, Oct 8, 2017, 11:29 PM
to me

Dear Erin:

Congratulations on enrolling in our free, 8-week coding course in HTML, Java, and WebDev! The course will begin on Monday, October 9th.

Each week, we encourage you to complete at least three 15-minute intervals of course material sometime during the week. Making a plan for when you'll work on coding can help you achieve your goals for the course.

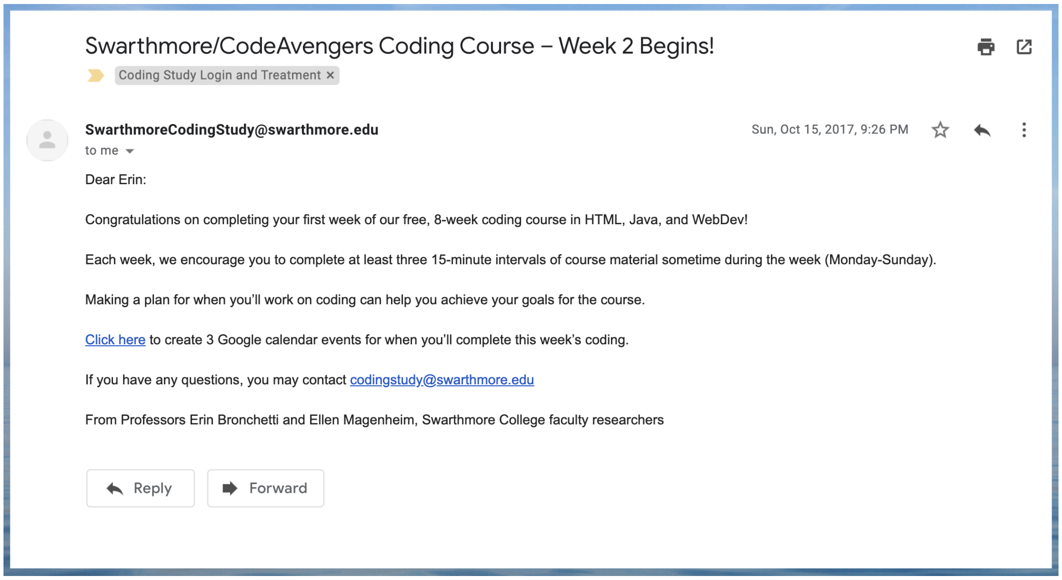
Click here to create 3 Google calendar events for when you'll complete this week's coding.

If you have any questions, you may contact codingstudy@swarthmore.edu.

From Professors Erin Bronchetti and Ellen Magenheim, Swarthmore College faculty researchers

Reply Forward

Figure S.4: Weekly Reminder Email, All Groups



Swarthmore/CodeAvengers Coding Course – Week 2 Begins!

Coding Study Login and Treatment ×

SwarthmoreCodingStudy@swarthmore.edu

to me

Sun, Oct 15, 2017, 9:26 PM

Dear Erin:

Congratulations on completing your first week of our free, 8-week coding course in HTML, Java, and WebDev!

Each week, we encourage you to complete at least three 15-minute intervals of course material sometime during the week (Monday-Sunday).

Making a plan for when you'll work on coding can help you achieve your goals for the course.

Click here to create 3 Google calendar events for when you'll complete this week's coding.

If you have any questions, you may contact codingstudy@swarthmore.edu

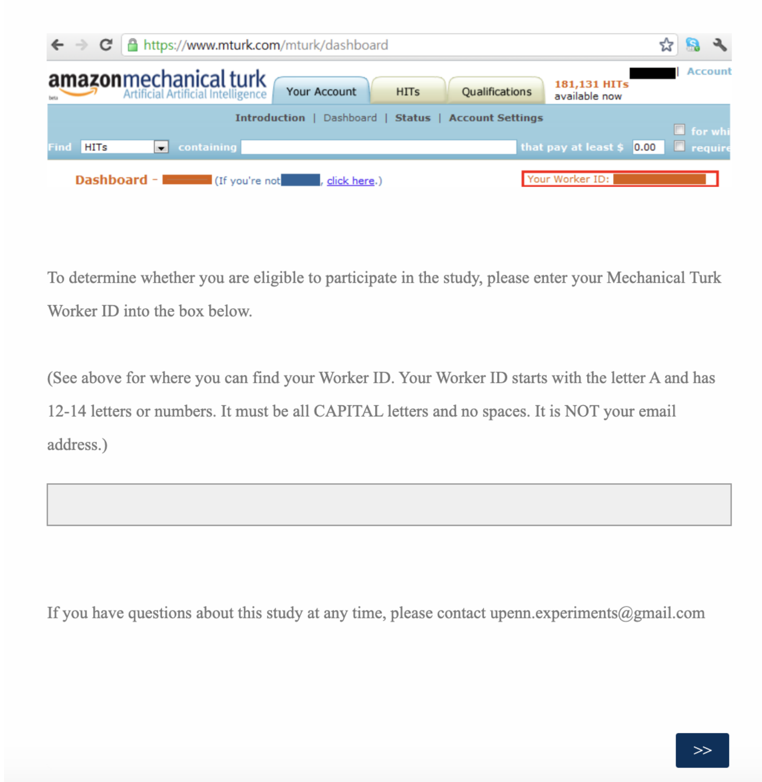
From Professors Erin Bronchetti and Ellen Magenheim, Swarthmore College faculty researchers

Reply Forward

F.2 Experiment 2

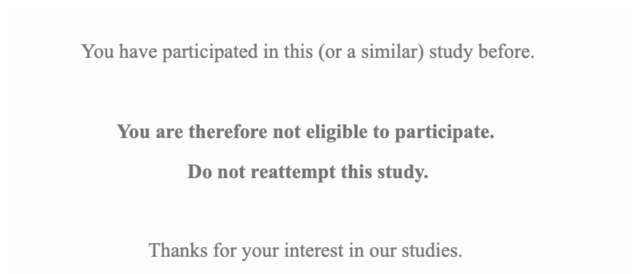
F.2.1 Part 1

Figure S.5: Eligibility Screen



This is the screen in which the participant entered their MTurk ID.

Figure S.6: If Ineligible Screen



If participants had participated in the study at an earlier date, they were shown this screen and excluded from participating.

Figure S.7: Consent Form

This is a consent form. Please read and click below to continue.

Study Background and Purpose: This study examines decision-making. Your participation in this research will take approximately 15 minutes today and you will have the option to complete an additional task for approximately 20 minutes during a one-week window between Sunday, October 3rd and Friday, November 19th.

What Happens in this Research Study: If you decide to participate, you will answer questions.

Payments: There are no known costs to you for participating in this research study except for your time. Upon completion of the survey, you will be given a code that you can submit to MTurk so that you can receive your payment. You will be paid $2.50 for completing the entire survey today, and you will have the possibility of earning an additional bonus payment.

Confidentiality: Your data will be anonymous and will not be linked to your identity.

Voluntary Participation: Participating in this research is voluntary. You can withdraw from the study at any time.

Contact: If you have questions, concerns, or complaints regarding this research, please contact the researcher at upenn.experiments@gmail.com. If a member of the research team cannot be reached or you want to talk to someone other than those working on the study, you may contact the Office of Regulatory Affairs with any question, concerns or complaints at the University of Pennsylvania by calling (215) 898-2614.

Agreement to Participate: By clicking to continue, you are indicating that you have read this consent form and that you voluntarily agree to participate in the study.

>>

Figure S.8: Attention Check (first attempt)

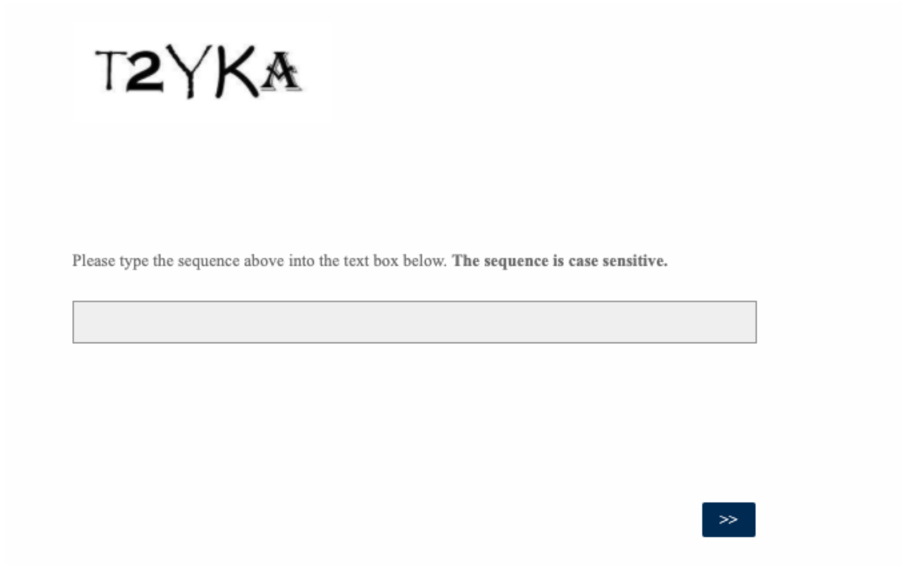
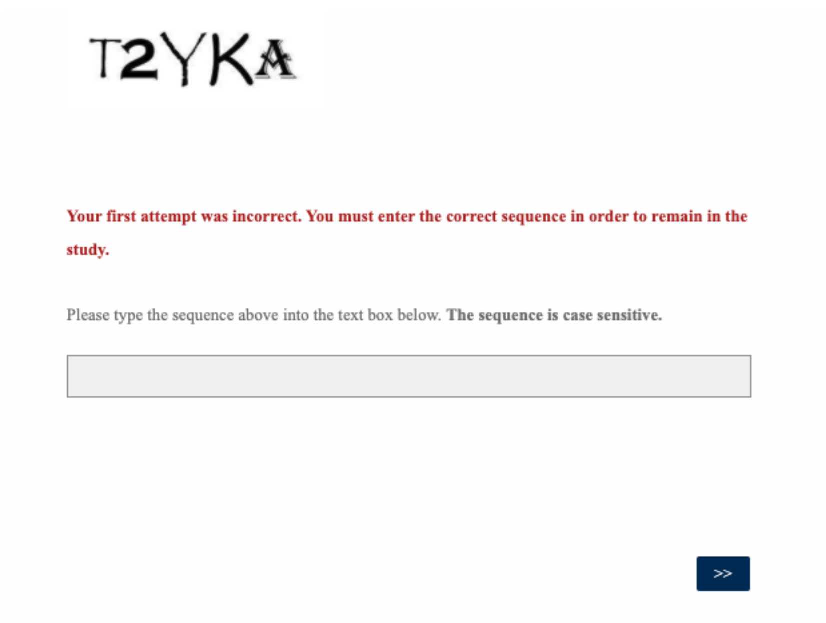


Figure S.9: Attention Check (second attempt)



If participants answered the attention check question incorrectly the first time, they saw this screen which warned them that failure to enter the sequence correctly would remove them from the study.

Figure S.10: Instructions, Screen 1

Thank you for participating in this HIT. This HIT has two parts. Part 1 of the study should take 15 minutes to complete right now. **For completing part 1 of the study, you will earn $2.50. You may also earn a "part 1 bonus" payment.**

In part 2 of the study, you will be asked to complete a survey that will take you about 20 minutes and must be completed in one sitting. **If you complete part 2 of the study, you will earn a "part 2 bonus" payment.** You cannot complete part 2 of the study now. You can only complete part 2 at some point in the future. (We will explain the details of when you can complete part 2 of the study on the next screen.)

In part 2 of the study, we will ask you **40 hypothetical questions** about whether or not you would take a particular gamble. These questions are **hypothetical**, which means what you choose in these questions will not affect your payment in any way.

- We will ask you 20 hypothetical questions about gambles over money. For example, you might be asked to choose between: (a) $50 for sure, or (b) a 50% chance of $110 and a 50% chance of $0.
- We will ask you 20 hypothetical questions about gambles over lottery tickets (where there are 1,000 lottery tickets, one of which earns a prize of $1,000). For example, you might be asked to choose between: (a) 50 tickets for sure, or (b) a 50% chance of 110 tickets and a 50% chance of 0 tickets.

You must answer all 40 hypothetical questions to complete part 2 of the study and earn a part 2 bonus payment.

Figure S.11: Understanding Questions for Instructions, Screen 1

You must answer this question correctly in order to remain in the study. Which of the following statements describes the bonus payments in this study?

- ○ In this study, I will earn $2.50 for sure in part 1. I will not have the opportunity to earn any bonus payments.
- ○ In this study, I will earn $2.50 for sure in part 1. I may also earn a "part 1 bonus" payment. I will earn a "part 2 bonus" payment if I complete part 2 of the study.
- ○ In this study, I will earn $2.50 for sure in part 1. I may also earn a "part 1 bonus" payment. I will also earn $2 for sure in part 2.

You must answer this question correctly in order to remain in the study. What will you do in part 2 of the study?

- ○ I will answer an unknown number of questions about an unknown topic.
- ○ I will answer **40 hypothetical questions** about gambles, and what I choose **will not affect** my payment (except that I must answer all 40 questions to earn a "part 2 bonus" payment).
- ○ I will answer **40 questions** about gambles, and what I choose in each question **will affect** my "part 2 bonus" payment.

>>

Figure S.12: Instructions, Screen 2

As was noted on the last screen, you cannot complete part 2 of the study now. You can only complete part 2 at some point in the future.

In particular, at the end of part 1 of the study, you will receive a link to complete part 2 of the study. The link will become active at 12:01am Eastern Time (ET) on some day in the future and remain active until 11:59pm Eastern Time (ET) a week later. The day that the link will become active will be randomly determined for you during this part of the study. **You are required to complete part 2 of the study in one sitting within that week to earn your part 2 bonus payment.** That is, you must click the link during the week that it is active and complete the survey within one hour of initially clicking the link to earn your part 2 bonus payment.

In this part of the study, we will ask you questions about how you value receiving a set of three reminder emails to complete part 2 of the study during the week that it is available for you to complete. **Based on random chance and your choices in this part of the study, you may receive a set of three reminder emails through the MTurk platform.**

If you do not receive this set of three reminder emails, we will not send you any reminders to complete part 2 of this study.

Figure S.13: Understanding Questions for Instructions, Screen 2

You must answer this question correctly in order to remain in the study. Is the following statement true or false? You will not receive any reminders to complete part 2 of the study **unless** you are selected to get them in this part of the study.

- True
- False

>>

Figure S.14: Instructions, Screen 3

If you receive the set of three reminder emails to complete part 2 of the study, we will send you three emails through the MTurk platform. Each email will include a link to access part 2 of the study.

We will send you the first reminder email at 12pm ET on the first day of the week that part 2 of the study is available to you.

We will send you the second reminder email at 12pm ET on the fourth day of the week that part 2 of the study is available to you, halfway through the window to complete part 2 of the study.

We will send you the third reminder email at 12pm ET on the last day of the week that part 2 of the study is available to you, twelve hours before the window to complete part 2 of the study closes.

>>

Figure S.15: Instructions, Screen 4

We will now ask you to make decisions about how you value getting the set of three reminder emails to complete part 2 of the study.

You will go through 16 screens of decisions about how you value the reminder emails. There are four possible part 2 bonus payments that you may be eligible to receive and four possible one-week windows in which part 2 of the study may become available to you. (These four possible bonuses and four possible weeks generate 16 possible combinations.) Each possible combination is equally likely to be selected.

Each screen will apply to a different possible part 2 bonus payment for completing part 2 of the study and a different week when part 2 of the study might be available to you.

Because they are randomly selected, you cannot affect which part 2 bonus payment you are eligible to receive or which week part 2 of the study will be available to you. Instead, you will make decisions on the following screens assuming that a given part 2 bonus payment and a given week have been randomly selected. Your responses for a given part 2 bonus payment and week only apply if that part 2 bonus payment and that week are randomly selected.

What this means is that it is in your best interest to make each decision carefully and honestly.

In addition to the decisions you make on the following screens, there is a chance that the computer will randomly assign you to either get the set of three reminder emails or not get the set of three reminder emails.

Any part 1 bonus payment you may earn today and any part 2 bonus payment you may earn from completing part 2 of the study will be sent to you three days after the week in which you may complete part 2 of the study ends.

>>

Figure S.16: Instructions, Screen 5

There will be 16 screens with decisions. On each screen, there will be a table of 33 rows. Each of the rows is its own separate decision. Any one of the decisions could be the one that is selected to determine your outcomes from this study. If one of the decisions is selected, then:

- If you choose the option on the left, you will NOT get the three reminder emails. You will also receive the part 1 bonus payment listed in the option on the left.
- If you choose the option on the right, you will get the three reminder emails. You will also receive the part 1 bonus payment listed in the option on the right.

On each of the 16 screens, you will be asked to indicate which option you prefer in each decision. To speed things up, we have made it so that you can simply click on the decision where you want to switch from choosing the option on the left to the option on the right. What you choose for each decision will be highlighted in orange.

Figure S.17: Understanding Questions for Instructions, Screen 5

You must answer this question correctly in order to remain in the study. If one of the decisions is selected and you chose the option on the LEFT in that decision, what would happen?

- ○ You will NOT get three reminder emails. You will get the amount of money listed in the option on the left.
- ○ You will get three reminder emails. You will get the amount of money listed in the option on the right.

You must answer this question correctly in order to remain in the study. If one of the decisions is selected and you chose the option on the RIGHT in that decision, what would happen?

- ○ You will NOT get three reminder emails. You will get the amount of money listed in the option on the left.
- ○ You will get three reminder emails. You will get the amount of money listed in the option on the right.

>>

Figure S.18: Multiple Price List Attention Check

Before you make any decisions, we have one more attention check. To signal to us that you are reading all instructions, simply click the continue button below, without clicking on anything in any of the 33 rows below. If you click on any of the rows below, you will be excluded from the study. The decisions will start on the screen after this one.

	NO REMINDERS +		GET REMINDERS +
DECISION 1:	$12.00	OR	$0.00
DECISION 2:	$11.25	OR	$0.00
DECISION 3:	$10.50	OR	$0.00
DECISION 4:	$9.75	OR	$0.00
DECISION 5:	$9.00	OR	$0.00
DECISION 6:	$8.25	OR	$0.00
DECISION 7:	$7.50	OR	$0.00
DECISION 8:	$6.75	OR	$0.00
DECISION 9:	$6.00	OR	$0.00
DECISION 10:	$5.25	OR	$0.00
DECISION 11:	$4.50	OR	$0.00
DECISION 12:	$3.75	OR	$0.00
DECISION 13:	$3.00	OR	$0.00
DECISION 14:	$2.25	OR	$0.00
DECISION 15:	$1.50	OR	$0.00
DECISION 16:	$0.75	OR	$0.00
DECISION 17:	$0.00	OR	$0.00
DECISION 18:	$0.00	OR	$0.75
DECISION 19:	$0.00	OR	$1.50
DECISION 20:	$0.00	OR	$2.25
DECISION 21:	$0.00	OR	$3.00
DECISION 22:	$0.00	OR	$3.75
DECISION 23:	$0.00	OR	$4.50
DECISION 24:	$0.00	OR	$5.25
DECISION 25:	$0.00	OR	$6.00
DECISION 26:	$0.00	OR	$6.75
DECISION 27:	$0.00	OR	$7.50
DECISION 28:	$0.00	OR	$8.25
DECISION 29:	$0.00	OR	$9.00
DECISION 30:	$0.00	OR	$9.75
DECISION 31:	$0.00	OR	$10.50
DECISION 32:	$0.00	OR	$11.25
DECISION 33:	$0.00	OR	$12.00

>>

Figure S.19: Example Multiple Price List Instructions with Incentive Level of $12 and Delay of 2 Days

Question 1 of 16

If the part 2 bonus payment is randomly selected to be $12 and if part 2 of the study is available starting in 2 days, one of the decisions below could be the one that is selected to determine your outcomes from this study. In the table below, each of the 33 rows is its own separate decision. If one of the decisions below is selected, then:

- If you choose the option on the left, you will NOT get the three reminder emails. You will also receive the part 1 bonus payment listed in the option on the left.
- If you choose the option on the right, you will get the three reminder emails. You will also receive the part 1 bonus payment listed in the option on the right.

Please indicate which option you prefer in each decision. To speed things up, we made it so that you can simply click on the decision where you want to switch from choosing the option on the left to the option on the right. What you choose in each decision will be highlighted in orange.

(Note that you cannot click on the submit button until you have selected an answer.)

To summarize, for this set of decisions:

- The part 2 bonus payment is randomly selected to be **$12**.
- Part 2 of the study is randomly selected to be available in **2 days**. Thus, part 2 of the study will be available starting on **Sunday, October 3rd**.
- Each row is its own separate decision, so make sure that the option selected in each row is your preferred of the two options in that row.

Click to Review Information about Reminder Emails and Payment

If you receive the three reminder emails, they would be sent to you at:
12pm ET on Sunday, October 3rd, the day part 2 of the study opens.
12pm ET on Wednesday, October 6th, halfway through the window to complete part 2 of the study, and
12pm ET on Saturday, October 9th, twelve hours before the window to complete part 2 of the study closes.

Any part 1 bonus payment you may earn today and any part 2 bonus payment you may earn from completing part 2 of the study between 12:01am ET on Sunday, October 3rd and 11:59pm ET on Saturday, October 9th will be sent to you on Tuesday, October 12th, three days after the week to complete part 2 of the study ends.

This figure shows the text that appears on the screen of a multiple price list decision. (The next figure shows the actual multiple price list that participants faced.) Participants saw a version of these screen 16 times for every combination of the four possible incentives and four possible delays. The order of these 12 MPLs was randomized at the participant level. In addition to the instruction text, the page has a clickable button between the instructions and the multiple price list which summarizes information about the reminder emails and payment; the figure above shows what it looks like when the button has been pressed.

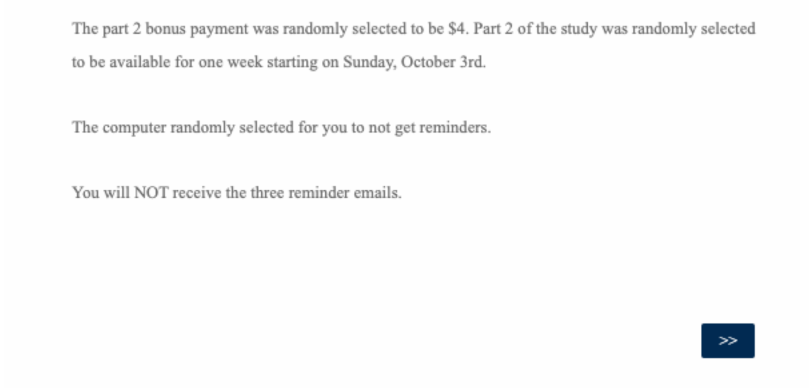
Figure S.20: Example Multiple Price List with Incentive Level of $12 and Delay of 2 Days

	NO REMINDERS +		GET REMINDERS +
DECISION 1:	$12.00	OR	$0.00
DECISION 2:	$11.25	OR	$0.00
DECISION 3:	$10.50	OR	$0.00
DECISION 4:	$9.75	OR	$0.00
DECISION 5:	$9.00	OR	$0.00
DECISION 6:	$8.25	OR	$0.00
DECISION 7:	$7.50	OR	$0.00
DECISION 8:	$6.75	OR	$0.00
DECISION 9:	$6.00	OR	$0.00
DECISION 10:	$5.25	OR	$0.00
DECISION 11:	$4.50	OR	$0.00
DECISION 12:	$3.75	OR	$0.00
DECISION 13:	$3.00	OR	$0.00
DECISION 14:	$2.25	OR	$0.00
DECISION 15:	$1.50	OR	$0.00
DECISION 16:	$0.75	OR	$0.00
DECISION 17:	$0.00	OR	$0.00
DECISION 18:	$0.00	OR	$0.75
DECISION 19:	$0.00	OR	$1.50
DECISION 20:	$0.00	OR	$2.25
DECISION 21:	$0.00	OR	$3.00
DECISION 22:	$0.00	OR	$3.75
DECISION 23:	$0.00	OR	$4.50
DECISION 24:	$0.00	OR	$5.25
DECISION 25:	$0.00	OR	$6.00
DECISION 26:	$0.00	OR	$6.75
DECISION 27:	$0.00	OR	$7.50
DECISION 28:	$0.00	OR	$8.25
DECISION 29:	$0.00	OR	$9.00
DECISION 30:	$0.00	OR	$9.75
DECISION 31:	$0.00	OR	$10.50
DECISION 32:	$0.00	OR	$11.25
DECISION 33:	$0.00	OR	$12.00

>>

This figure shows the multiple price list that participants saw on the same screen as the text in the previous figure. This is the high incentive treatment; the other version of this multiple price list has a maximum bonus of $4 and increments by $0.25, instead of a maximum bonus of $12 and increments of $0.75 as above.

Figure S.21: Part 2 Information: No reminder emails



The part 2 bonus payment was randomly selected to be $4. Part 2 of the study was randomly selected to be available for one week starting on Sunday, October 3rd.

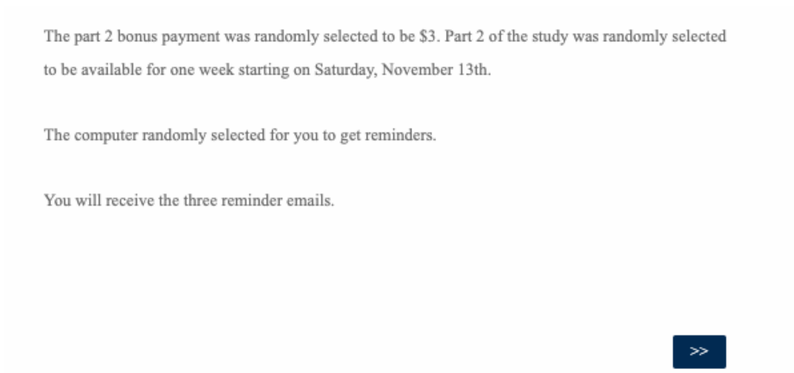
The computer randomly selected for you to not get reminders.

You will NOT receive the three reminder emails.

>>

Participants who would not receive reminder emails were shown a version of this screen after completing the 16 multiple price list screens; the part 2 bonus and availability was randomly selected and varies across participants.

Figure S.22: Part 2 Information: Reminder emails



The part 2 bonus payment was randomly selected to be $3. Part 2 of the study was randomly selected to be available for one week starting on Saturday, November 13th.

The computer randomly selected for you to get reminders.

You will receive the three reminder emails.

>>

Participants who would receive reminder emails were shown a version of this screen after completing the 16 multiple price list screens; the part 2 bonus and availability was randomly selected and varies across participants.

Figure S.23: Link Screen: No reminder emails

The following link will take you to part 2 of the study and be active from 12:01am ET on Sunday, October 3rd to 11:59pm ET on Saturday, October 9th.

https://wharton.qualtrics.com/jfe/form/SV_7QgPF0gZN4ePx8q

If you complete part 2 of the study during this week, you will receive an additional part 2 bonus payment of $4.

Please make a note of this link. You will not receive any reminders to complete part 2 of the study.

Participants who would not receive reminder emails were shown this screen at the end of part 1 of the study.

Figure S.24: Link Screen: Reminder emails

The following link will take you to part 2 of the study and be active from 12:01am ET on Saturday, November 13th to 11:59pm ET on Friday, November 19th.

https://wharton.qualtrics.com/jfe/form/SV_cwg2HYah4aDCOMK

If you complete part 2 of the study during this week, you will receive a part 2 bonus payment of $3.

Please make a note of this link. You will also be sent this link when you are sent reminder emails to complete part 2 of the study.

Participants who would receive reminder emails were shown this screen at the end of part 1 of the study.

Figure S.25: Demographic Information

To complete part 1 of this study, please answer the short follow-up survey below. None of your answers on this follow-up survey will influence your payments in any way.

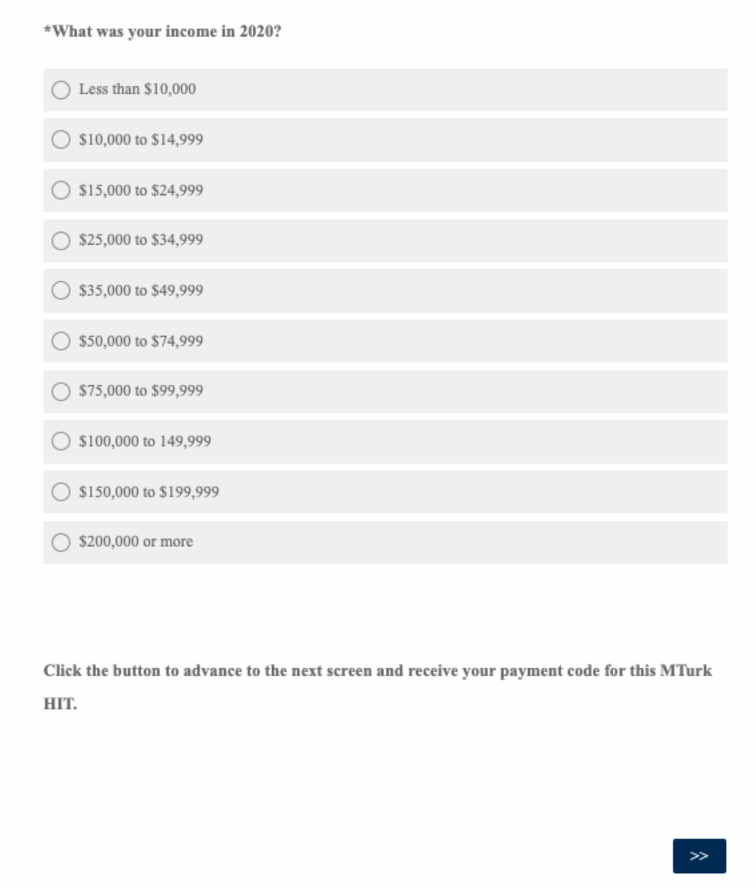
***How old are you (in years)?**

***What is your gender?**

- Male
- Female
- Other
- Prefer not to say

***Please select the highest level of education that you have completed.**

- Elementary School
- Middle School
- High School or equivalent
- Some college
- College Graduate with Associate's Degree (2 year)
- College Graduate with Bachelor's Degree (4 year)
- Master's Degree (MS)
- Doctoral Degree (PhD)
- Professional Degree (MD, JD, etc.)
- Other



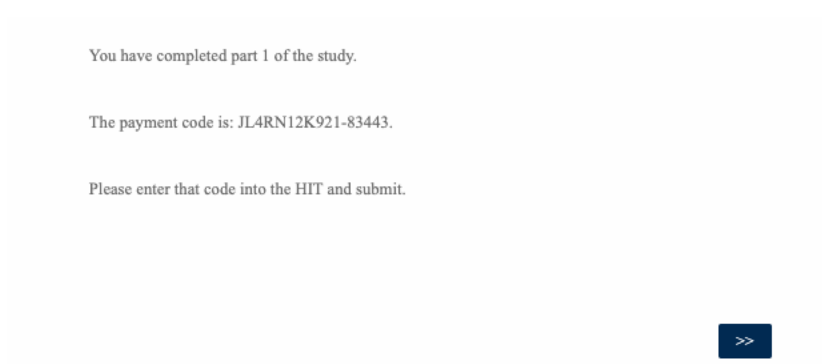
***What was your income in 2020?**

- Less than $10,000
- $10,000 to $14,999
- $15,000 to $24,999
- $25,000 to $34,999
- $35,000 to $49,999
- $50,000 to $74,999
- $75,000 to $99,999
- $100,000 to 149,999
- $150,000 to $199,999
- $200,000 or more

Click the button to advance to the next screen and receive your payment code for this MTurk HIT.

>>

Figure S.26: Final Screen



You have completed part 1 of the study.

The payment code is: JL4RN12K921-83443.

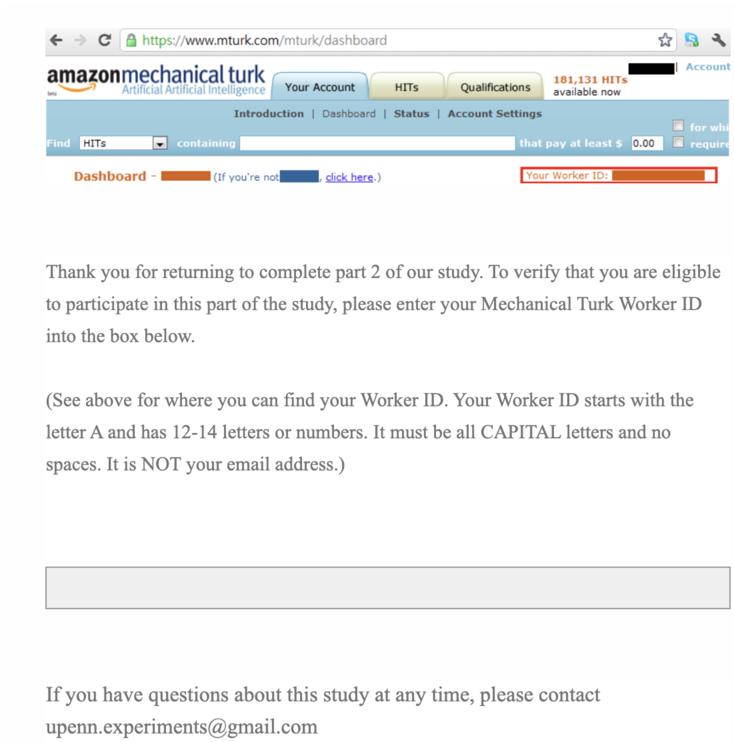
Please enter that code into the HIT and submit.

>>

Participants saw their MTurk payment code as their final screen before exiting the study.

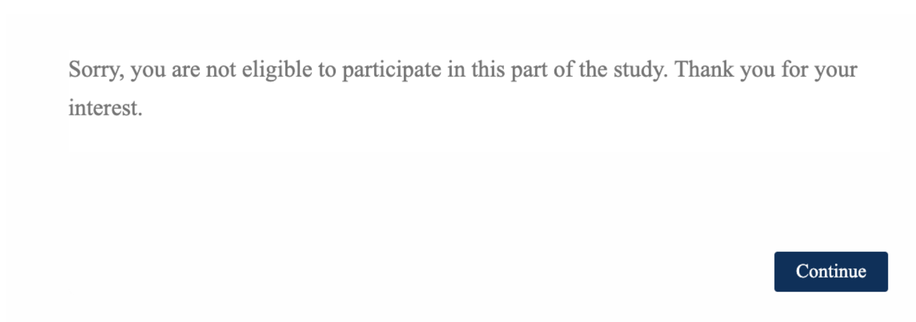
F.2.2 Part 2

Figure S.27: Eligibility Screen



This is the screen in which the participant entered their MTurk ID. In order to proceed, the participant had to enter then MTurk ID they used for part 1 of the study.

Figure S.28: If Ineligible Screen



If a participant entered an MTurk ID that did not match one used for part 1 of the study, they saw this screen and were automatically screened out.

Figure S.29: Introduction Screen

Thank you for returning to complete part 2 of this study. If you complete this part you will receive a $3 bonus. We anticipate that this part will take approximately 20 minutes. You must complete it in one sitting (i.e., within one hour from when you launched the survey).

In this part of the study, you will be asked 40 hypothetical questions. While these questions will not affect your bonus payment for this part of the study, please take them seriously and answer them carefully.

Continue

Participants received this introduction after the eligibility check. The bonus amount shown was based on the bonus randomly assigned to them in part 1 of the study. Participants who took more than one hour to complete the study were treated as not having completed part 2 of the study.

Figure S.30: Instructions for Gambles over Money

On each of the next 20 screens, we will ask you **hypothetical** questions about your preferences for gambles over **money**.

When you are ready, click the continue button to advance to the first of the 20 screens.

Continue

The order was randomized such that some participants saw the gambles over money first, while other participants saw the gambles over lottery tickets first.

Figure S.31: Example Question about Gambles over Money

Question 2 out of 40:

You are given $100. Would you rather keep what you have or accept a gamble with a 50% chance of losing $99 and a 50% chance of gaining $53?

- Keep what I have
- Accept a gamble with a 50% chance of losing $99 and a 50% chance of gaining $53

Participants answered 20 questions similar to the one shown here.

Figure S.32: Instructions for Gambles over Lottery Tickets

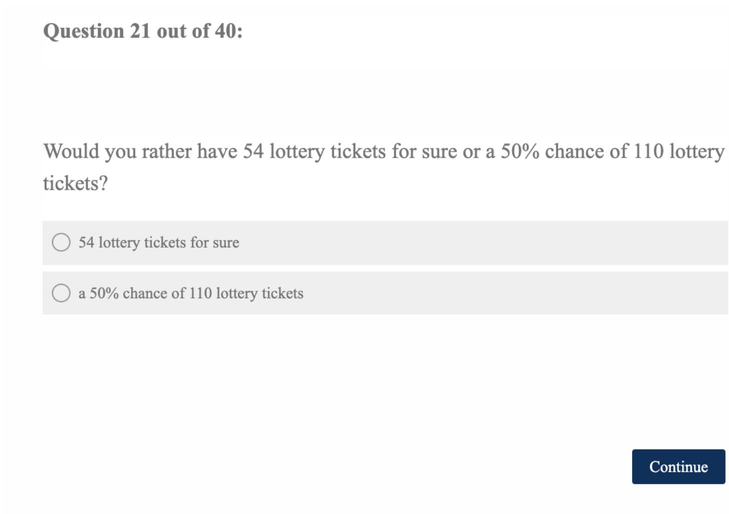
On each of the next 20 screens, we will ask you **hypothetical** questions about your preferences for gambles over **lottery tickets**.

Imagine that there are 1,000 lottery tickets total, and exactly one of them is the winning ticket. The winning ticket earns a prize of $1,000. So if you have all 1,000, you win $1,000 for sure. If you have 500, you win $1,000 half the time. If you have 100, you win $1,000 one out of every ten times.

When you are ready, click the continue button to advance to the first of the 20 screens.

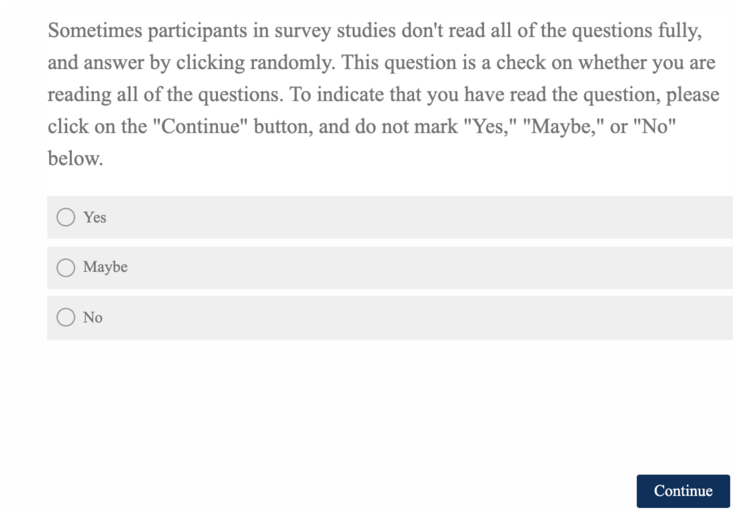
Continue

Figure S.33: Example Question about Gambles over Lottery Tickets



Participants answered 20 questions similar to the one shown here.

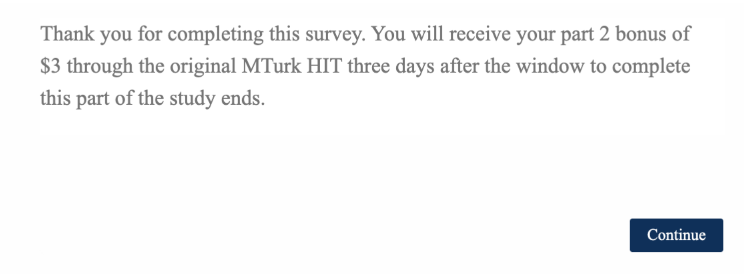
Figure S.34: Attention Check Question



Participants were asked the above attention check question to assess whether they were paying attention, but this question did not affect payments in any way.

Participants were then asked for their demographic information on screens identical to those in Figure S.25.

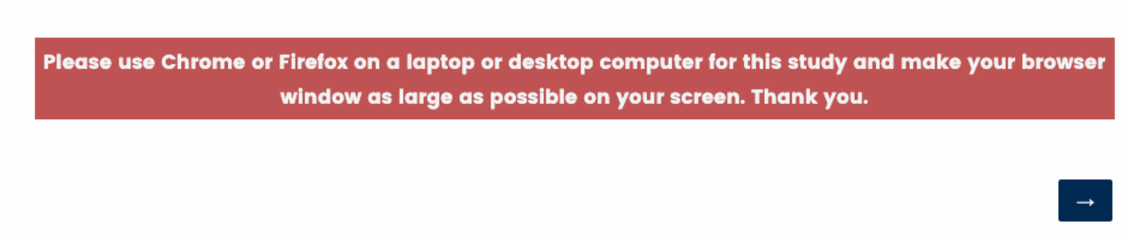
Figure S.35: Final Screen



Participants saw this screen before exiting the survey. The bonus amount shown was based on the bonus randomly assigned to them in part 1 of the study.

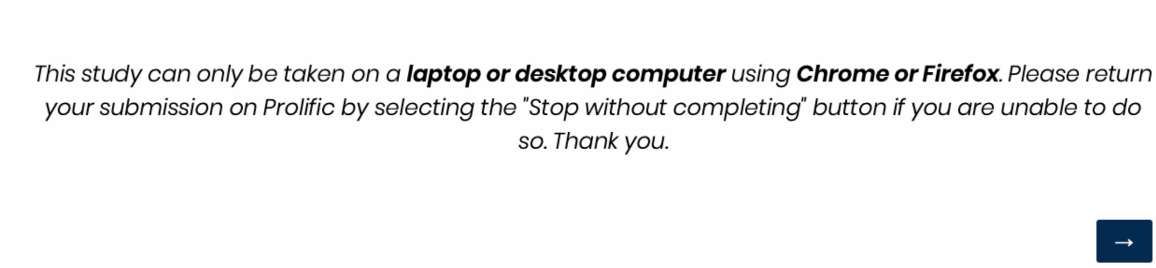
F.3 Experiment 3

Figure S.36: Introduction Screen



Participants were informed of the browser and device restrictions of the study, and were advised to maximize the size of the browser window.

Figure S.37: Browser or Device Ineligibility Screen



If participants had opened the study on a mobile phone or on browser other than Chrome or Firefox, they were shown this screen and excluded from participating.

Figure S.38: Attention Check (first attempt)

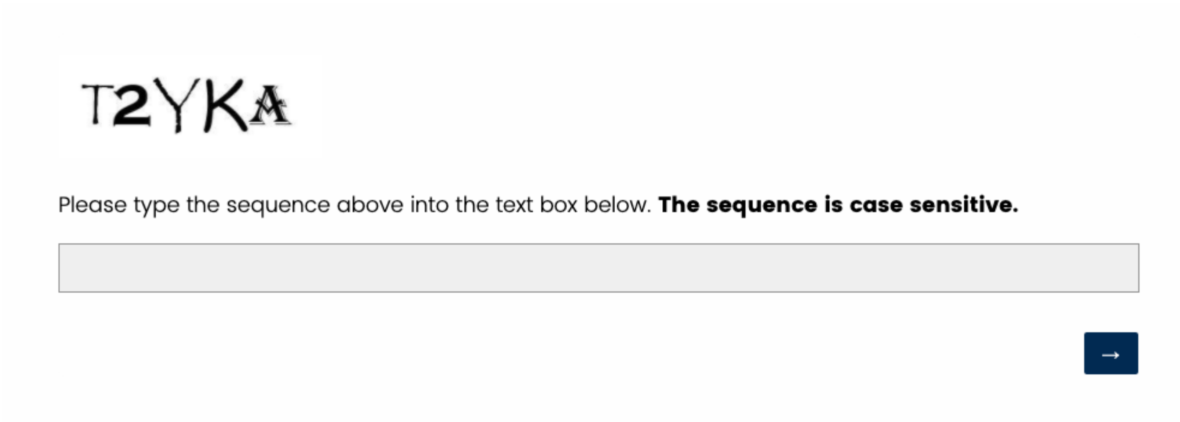
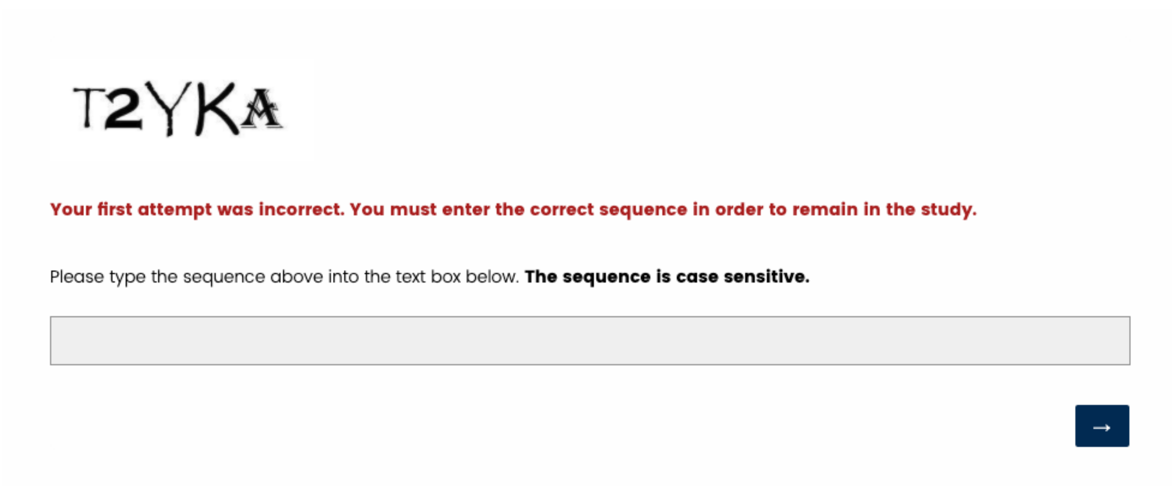


Figure S.39: Attention Check (second attempt)



If participants answered the attention check question incorrectly the first time, they saw this screen which warned them that failure to enter the sequence correctly would remove them from the study.

Figure S.40: Prolific ID Entry

Before we begin, please enter your Prolific ID below.

As you complete the rest of the study, if you encounter freezing that prevents you from continuing, please refresh or restart your browser.

→

Figure S.41: Consent Form

This is a consent form. Please read and click below to continue.

Study Background and Purpose: This study examines how much effort people put into simple tasks with different incentive schemes. Your participation in this research will take approximately 20 minutes.

What Happens in this Research Study: If you decide to participate, you will be asked to do the following:

- Complete a series of arithmetic tasks, where you will receive monetary rewards for accuracy.
- Answer a few questions about which types of arithmetic tasks you want to complete.

Payments: There are no known costs to you for participating in this research study except for your time. Upon completion of the survey, you will be redirected to Prolific. You will be paid $2.50 for completing the entire survey today, and you will have the possibility of earning an additional reward.

Confidentiality: Your data will be anonymous and will not be linked to your identity.

Voluntary Participation: Participating in this research is voluntary. You can withdraw from the study at any time.

Contact: If you have questions, concerns, or complaints regarding this research, please contact the researchers at upenn.experiments@gmail.com. If a member of the research team cannot be reached or you want to talk to someone other than those working on the study, you may contact the Office of Regulatory Affairs with any question, concerns or complaints at the University of Pennsylvania by calling (215) 898-2614.

Agreement to Participate: By clicking to continue, you are indicating that you have read this consent form and that you voluntarily agree to participate in the study.

Figure S.42: Instructions, Screen 1

Instructions

There are 2 blocks of math tasks in this study, and each block involves 7 math tasks.

For each math task, you will see a picture like the one below, which lists multiple equations. Some equations are correct; others are incorrect. For example, in the picture below, the first two equations are incorrect and the third one is correct.

$2 + 55 = 54$
$46 + 12 = 53$
$14 + 10 = 24$
$12 + 62 = 74$
$24 + 60 = 84$
$69 + 12 = 81$
$59 + 27 = 86$
$13 + 59 = 74$
$9 + 77 = 90$
$18 + 42 = 60$

In each math task, the computer will select the fraction of correct equations from two equally likely possibilities. You will be told the two possibilities, but not what the computer selected. For example, in the picture above, you might be told that either 60% of the equations (6/10) are correct, or that 40% of the equations (4/10) are correct. It will be your job to determine which of these is true.

The computer will randomly select one of the 14 math tasks to be the task that counts. If you give an accurate answer in the task that counts, you will receive a reward.

The total number of equations that appear in a picture might vary between math tasks. The possibilities for the fraction of correct equations might also vary between math tasks.

Figure S.43: Instructions, Screen 2

(a) Length Arm

Instructions, continued

In all of the 7 math tasks that you see in this block, the fraction of correct equations is 60% or 40%. The computer will randomly select the reward for giving an accurate answer in this block to be $2, $3, or $4.

In 3 of the math tasks, the picture will have 10 equations. In 3 of the math tasks, the picture will have 100 equations. For the remaining math task, you may have a chance to choose whether the picture has 10 equations or has 100 equations. These 7 math tasks will be presented in random order. We will explain how you make this choice on the next screen.

(b) Discernibility Arm

Instructions, continued

In all of the 7 math tasks that you see in this block, the pictures will have 100 equations. The computer will randomly select the reward for giving an accurate answer in this block to be $2, $3, or $4.

In 3 of the math tasks, the computer will randomly (and with equal chance) select whether the fraction of correct equations is 60% or 40%. In 3 of the math tasks, the computer will randomly (and with equal chance) select whether the fraction of correct equations is 95% or 5%. For the remaining math task, you may have a chance to choose whether the computer randomly selects between 60% or 40% correct or whether the computer randomly selects between 95% and 5% correct. These 7 math tasks will be presented in random order. We will explain how you make this choice on the next screen.

Participants saw one of the above instruction screens, depending on the arm they were assigned to.

Figure S.44: Instructions, Screen 3

(a) Length Arm

If the remaining math task is selected as the task that counts, one of the decisions you make on an upcoming screen will determine whether the remaining math task in the block has 10 equations or 100 equations. (If the remaining math task is not selected as the task that counts, the computer will select whether the remaining math task has 10 equations or 100 equations.)

On the upcoming screen, you will see three sets of decisions. Each set of decisions is in a table with 33 rows, and each row is its own decision. **Any one of the decisions could be chosen to count.** If your decision is chosen to count:

- If you choose the option on the left, you will complete a math task with 100 equations. You will also receive the additional money listed in the option on the left (this money is in addition to any reward you receive for giving an accurate answer in the math task).
- If you choose the option on the right, you will complete a math task with 10 equations. You will also receive the additional money listed in the option on the right (this money is in addition to any reward you receive for giving an accurate answer in the math task).

Each set of decisions applies to a given reward for giving an accurate answer to the associated math task. That is, a decision from that table will only be selected if that reward for accuracy is randomly selected (and the remaining math task is selected as the task that counts).

What this means is that it is in your best interest to make each decision carefully and honestly.

(b) Discernibility Arm

If the remaining math task is selected as the task that counts, one of the decisions you make on an upcoming screen will determine whether the computer will randomly select between 60% or 40% correct equations or between 95% or 5% correct equations for the remaining math task in the block. (If the remaining math task is not selected as the task that counts, the computer will select whether the remaining math task has 60% or 40% correct equations or has 95% or 5% correct equations.)

On the upcoming screen, you will see three sets of decisions. Each set of decisions is in a table with 33 rows, and each row is its own decision. **Any one of the decisions could be chosen to count.** If your decision is chosen to count:

- If you choose the option on the left, you will complete a math task in which the computer will randomly select between 60% or 40% correct equations. You will also receive the additional money listed in the option on the left (this money is in addition to any reward you receive for giving an accurate answer in the math task).
- If you choose the option on the right, you will complete a math task in which the computer will randomly select between 95% or 5% correct equations. You will also receive the additional money listed in the option on the right (this money is in addition to any reward you receive for giving an accurate answer in the math task).

Each set of decisions applies to a given reward for giving an accurate answer to the associated math task. That is, a decision from that table will only be selected if that reward for accuracy is randomly selected (and the remaining math task is selected as the task that counts).

What this means is that it is in your best interest to make each decision carefully and honestly.

Participants saw one of the above instruction screens, depending on their arm.

Figure S.45: Comprehension Questions, Screen 3

(a) Length Arm

You must answer this question correctly in order to remain in the study. If the associated math task is selected as the task that counts and you chose the option on the LEFT in the randomly selected decision, what would happen?

- ○ You will complete a math task with 100 equations. You will also receive the additional money listed in the option on the left (this money is in addition to any reward you receive for giving an accurate answer in the math task).
- ○ You will complete a math task with 10 equations. You will also receive the additional money listed in the option on the right (this money is in addition to any reward you receive for giving an accurate answer in the math task).

You must answer this question correctly in order to remain in the study. If the associated math task is selected as the task that counts and you chose the option on the RIGHT in the randomly selected decision, what would happen?

- ○ You will complete a math task with 100 equations. You will also receive the additional money listed in the option on the left (this money is in addition to any reward you receive for giving an accurate answer in the math task).
- ○ You will complete a math task with 10 equations. You will also receive the additional money listed in the option on the right (this money is in addition to any reward you receive for giving an accurate answer in the math task).

(b) Discernibility Arm

You must answer this question correctly in order to remain in the study. If the associated math task is selected as the task that counts and you chose the option on the LEFT in the randomly selected decision, what would happen?

- ○ You will complete a math task with 100 equations. You will also receive the additional money listed in the option on the left (this money is in addition to any reward you receive for giving an accurate answer in the math task).
- ○ You will complete a math task with 10 equations. You will also receive the additional money listed in the option on the right (this money is in addition to any reward you receive for giving an accurate answer in the math task).

You must answer this question correctly in order to remain in the study. If the associated math task is selected as the task that counts and you chose the option on the RIGHT in the randomly selected decision, what would happen?

- ○ You will complete a math task with 100 equations. You will also receive the additional money listed in the option on the left (this money is in addition to any reward you receive for giving an accurate answer in the math task).
- ○ You will complete a math task with 10 equations. You will also receive the additional money listed in the option on the right (this money is in addition to any reward you receive for giving an accurate answer in the math task).

On the same screen as Figure S.44, participants answered one of the sets of comprehension questions above depending on their arm. If at least one of the questions was answered incorrectly, the participant was removed from the study.

Figure S.46: Multiple Price List Attention Check

Before you make any decisions, we have one more attention check. To signal to us that you are reading all instructions, simply click the continue button below, without clicking on anything in any of the 33 rows below. If you click on any of the rows below, you will be excluded from the study. The decisions will start on the screen after this one.

	60% OR 40% CORRECT +		95% OR 5% CORRECT +
DECISION 1:	$4.00	OR	$0.00
DECISION 2:	$3.75	OR	$0.00
DECISION 3:	$3.50	OR	$0.00
DECISION 4:	$3.25	OR	$0.00
DECISION 5:	$3.00	OR	$0.00
DECISION 6:	$2.75	OR	$0.00
DECISION 7:	$2.50	OR	$0.00
DECISION 8:	$2.25	OR	$0.00
DECISION 9:	$2.00	OR	$0.00
DECISION 10:	$1.75	OR	$0.00
DECISION 11:	$1.50	OR	$0.00
DECISION 12:	$1.25	OR	$0.00
DECISION 13:	$1.00	OR	$0.00
DECISION 14:	$0.75	OR	$0.00
DECISION 15:	$0.50	OR	$0.00
DECISION 16:	$0.25	OR	$0.00
DECISION 17:	$0.00	OR	$0.00
DECISION 18:	$0.00	OR	$0.25
DECISION 19:	$0.00	OR	$0.50
DECISION 20:	$0.00	OR	$0.75
DECISION 21:	$0.00	OR	$1.00
DECISION 22:	$0.00	OR	$1.25
DECISION 23:	$0.00	OR	$1.50
DECISION 24:	$0.00	OR	$1.75
DECISION 25:	$0.00	OR	$2.00
DECISION 26:	$0.00	OR	$2.25
DECISION 27:	$0.00	OR	$2.50
DECISION 28:	$0.00	OR	$2.75
DECISION 29:	$0.00	OR	$3.00
DECISION 30:	$0.00	OR	$3.25
DECISION 31:	$0.00	OR	$3.50
DECISION 32:	$0.00	OR	$3.75
DECISION 33:	$0.00	OR	$4.00

→

Participants saw one last attention check in which they were asked to click "continue" without making any choices to demonstrate they were reading the instructions. Participants who selected a choice were automatically removed from the study. The screen above was presented to participants in the discernibility arm; participants in the length arm saw the same table, but with the headers "100 Equations +" and "10 Equations +".

Figure S.47: Multiple Price List Instructions

(a) Length Arm

Choosing the remaining math task in Block 1

Recall that the reward for answering the math tasks accurately could be either $2, $3, or $4. The first table of decisions below is about what kind of math task you would like to answer (and how strong your preference is) if the accuracy reward is $2. The second table is about what you would like if the accuracy reward is $3. The third table is about what you would like if the accuracy reward is $4.

In each of the three tables below, each of the 33 rows is its own separate decision. If the reward is randomly selected to be the one listed at the top of the table and the remaining math task in this block is selected as the task that counts, then for the randomly selected decision:

- If you choose the option on the left, the math task will have 100 equations. You will also receive the **additional** money (i.e., in addition to the bonus you would get if you accurately answer the math task) listed in the option on the left.
- If you choose the option on the right, the math task will have 10 equations. You will also receive the **additional** money (i.e., in addition to the bonus you would get if you accurately answer the math task) listed in the option on the right.

The different amounts of additional money in each decision of each table are for assessing how much more or less you prefer to answer a 10-equation math task over a 100-equation math task. For example, the more likely you think you are to accurately answer a 10-equation math task, the more you might want to choose the options in the right column of each table. And because higher accuracy rewards make answering tasks accurately more important, the point where you cross over from choosing options on the left to choosing options on the right might then be higher up in tables with higher accuracy rewards. The opposite would apply if you think you are more likely to accurately answer a 100-equation math task.

Please indicate which option you prefer in each decision in each table. To speed things up, we made it so that in each table you can simply click on the decision where you want to switch from choosing the option on the left to the option on the right. The more you prefer the math task in the right column, the higher up you should switch from options on the left to options on the right. The more you prefer the math task in the left column, the lower down you should switch from options on the left to options on the right. What you choose in each decision will be highlighted in orange. (Note that you cannot click on the submit button until you have selected your decisions in each table.)

(b) Discernibility Arm

Choosing the remaining math task in Block 1

Recall that the reward for answering the math tasks accurately could be either $2, $3, or $4. The first table of decisions below is about what kind of math task you would like to answer (and how strong your preference is) if the accuracy reward is $2. The second table is about what you would like if the accuracy reward is $3. The third table is about what you would like if the accuracy reward is $4.

In each of the three tables below, each of the 33 rows is its own separate decision. If the reward is randomly selected to be the one listed at the top of the table and the remaining math task in this block is selected as the task that counts, then for the randomly selected decision:

- If you choose the option on the left, the computer will randomly select between 60% or 40% correct equations. You will also receive the **additional** money (i.e., in addition to the bonus you would get if you accurately answer the math task) listed in the option on the left.
- If you choose the option on the right, the computer will randomly select between 95% or 5% correct equations. You will also receive the **additional** money (i.e., in addition to the bonus you would get if you accurately answer the math task) listed in the option on the right.

The different amounts of additional money in each decision of each table are for assessing how much more or less you prefer to answer a math task with 95% or 5% correct equations over a math task with 60% or 40% correct equations. For example, the more likely you think you are to accurately answer a math task with 95% or 5% correct equations, the more you might want to choose the options in the right column of each table. And because higher accuracy rewards make answering tasks accurately more important, the point where you cross over from choosing options on the left to choosing options on the right might then be higher up in tables with higher accuracy rewards. The opposite would apply if you think you are more likely to accurately answer a math task with 60% or 40% correct.

Please indicate which option you prefer in each decision in each table. To speed things up, we made it so that in each table you can simply click on the decision where you want to switch from choosing the option on the left to the option on the right. The more you prefer the math task in the right column, the higher up you should switch from options on the left to options on the right. The more you prefer the math task in the left column, the lower down you should switch from options on the left to options on the right. What you choose in each decision will be highlighted in orange. (Note that you cannot click on the submit button until you have selected your decisions in each table.)

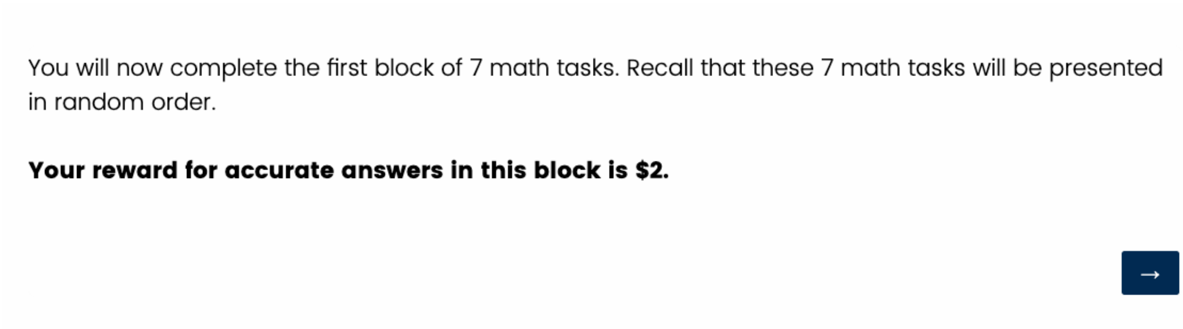
Participants saw one of the above instruction screens, depending on the arm they were assigned to.

Figure S.48: Multiple Price Lists

	$2 ACCURACY REWARD				$3 ACCURACY REWARD				$4 ACCURACY REWARD		
	100 EQUATIONS +		10 EQUATIONS +		100 EQUATIONS +		10 EQUATIONS +		100 EQUATIONS +		10 EQUATIONS +
#1:	$4.00	OR	$0.00	#1:	$4.00	OR	$0.00	#1:	$4.00	OR	$0.00
#2:	$3.75	OR	$0.00	#2:	$3.75	OR	$0.00	#2:	$3.75	OR	$0.00
#3:	$3.50	OR	$0.00	#3:	$3.50	OR	$0.00	#3:	$3.50	OR	$0.00
#4:	$3.25	OR	$0.00	#4:	$3.25	OR	$0.00	#4:	$3.25	OR	$0.00
#5:	$3.00	OR	$0.00	#5:	$3.00	OR	$0.00	#5:	$3.00	OR	$0.00
#6:	$2.75	OR	$0.00	#6:	$2.75	OR	$0.00	#6:	$2.75	OR	$0.00
#7:	$2.50	OR	$0.00	#7:	$2.50	OR	$0.00	#7:	$2.50	OR	$0.00
#8:	$2.25	OR	$0.00	#8:	$2.25	OR	$0.00	#8:	$2.25	OR	$0.00
#9:	$2.00	OR	$0.00	#9:	$2.00	OR	$0.00	#9:	$2.00	OR	$0.00
#10:	$1.75	OR	$0.00	#10:	$1.75	OR	$0.00	#10:	$1.75	OR	$0.00
#11:	$1.50	OR	$0.00	#11:	$1.50	OR	$0.00	#11:	$1.50	OR	$0.00
#12:	$1.25	OR	$0.00	#12:	$1.25	OR	$0.00	#12:	$1.25	OR	$0.00
#13:	$1.00	OR	$0.00	#13:	$1.00	OR	$0.00	#13:	$1.00	OR	$0.00
#14:	$0.75	OR	$0.00	#14:	$0.75	OR	$0.00	#14:	$0.75	OR	$0.00
#15:	$0.50	OR	$0.00	#15:	$0.50	OR	$0.00	#15:	$0.50	OR	$0.00
#16:	$0.25	OR	$0.00	#16:	$0.25	OR	$0.00	#16:	$0.25	OR	$0.00
#17:	$0.00	OR	$0.00	#17:	$0.00	OR	$0.00	#17:	$0.00	OR	$0.00
#18:	$0.00	OR	$0.25	#18:	$0.00	OR	$0.25	#18:	$0.00	OR	$0.25
#19:	$0.00	OR	$0.50	#19:	$0.00	OR	$0.50	#19:	$0.00	OR	$0.50
#20:	$0.00	OR	$0.75	#20:	$0.00	OR	$0.75	#20:	$0.00	OR	$0.75
#21:	$0.00	OR	$1.00	#21:	$0.00	OR	$1.00	#21:	$0.00	OR	$1.00
#22:	$0.00	OR	$1.25	#22:	$0.00	OR	$1.25	#22:	$0.00	OR	$1.25
#23:	$0.00	OR	$1.50	#23:	$0.00	OR	$1.50	#23:	$0.00	OR	$1.50
#24:	$0.00	OR	$1.75	#24:	$0.00	OR	$1.75	#24:	$0.00	OR	$1.75
#25:	$0.00	OR	$2.00	#25:	$0.00	OR	$2.00	#25:	$0.00	OR	$2.00
#26:	$0.00	OR	$2.25	#26:	$0.00	OR	$2.25	#26:	$0.00	OR	$2.25
#27:	$0.00	OR	$2.50	#27:	$0.00	OR	$2.50	#27:	$0.00	OR	$2.50
#28:	$0.00	OR	$2.75	#28:	$0.00	OR	$2.75	#28:	$0.00	OR	$2.75
#29:	$0.00	OR	$3.00	#29:	$0.00	OR	$3.00	#29:	$0.00	OR	$3.00
#30:	$0.00	OR	$3.25	#30:	$0.00	OR	$3.25	#30:	$0.00	OR	$3.25
#31:	$0.00	OR	$3.50	#31:	$0.00	OR	$3.50	#31:	$0.00	OR	$3.50
#32:	$0.00	OR	$3.75	#32:	$0.00	OR	$3.75	#32:	$0.00	OR	$3.75
#33:	$0.00	OR	$4.00	#33:	$0.00	OR	$4.00	#33:	$0.00	OR	$4.00

This figure shows the multiple price list that participants in the length arm saw on the same screen as the text in Figure S.47. Participants in the discernibility arm saw the same display, but with the headers in Figure S.46 i.e., "60% or 40% correct +" and "95% or 5% correct +".

Figure S.49: Block 1 Start Screen



After completing the MPLs, participants were told the reward for accurate answers before starting the first block. The value given for the reward for accurate answers varied based on the incentive the participant had been assigned to.

Figure S.50: Task Examples

(a) Baseline

In this math task, there are 100 equations and 60% or 40% are correct. If you accurately identify whether there are more correct or incorrect equations in this math task, and this math task is randomly selected to be the task that counts, you will receive a **$2 reward**.

Please select which of the two possibilities below accurately describes equations in this math task:

59 + 36 = 95	56 + 18 = 73	9 + 56 = 61	42 + 39 = 84	21 + 1 = 22
80 + 8 = 88	79 + 19 = 100	51 + 12 = 62	60 + 10 = 70	27 + 3 = 26
1 + 48 = 45	2 + 13 = 15	33 + 13 = 41	14 + 85 = 95	2 + 62 = 64
20 + 9 = 25	40 + 53 = 95	23 + 24 = 47	61 + 28 = 89	46 + 3 = 46
38 + 31 = 66	28 + 27 = 53	5 + 14 = 20	12 + 38 = 50	22 + 49 = 71
54 + 1 = 55	11 + 45 = 56	20 + 26 = 48	4 + 91 = 95	20 + 48 = 68
64 + 19 = 83	2 + 4 = 4	15 + 78 = 90	72 + 3 = 72	2 + 16 = 16
55 + 14 = 72	22 + 18 = 45	47 + 31 = 78	77 + 20 = 97	14 + 56 = 70
62 + 13 = 75	5 + 2 = 4	42 + 18 = 63	33 + 8 = 46	35 + 14 = 49
45 + 30 = 72	37 + 54 = 96	61 + 29 = 87	18 + 44 = 62	84 + 4 = 88
81 + 18 = 99	18 + 81 = 95	3 + 37 = 44	4 + 85 = 93	37 + 4 = 45
41 + 52 = 91	46 + 45 = 93	18 + 43 = 57	36 + 27 = 66	58 + 39 = 97
45 + 26 = 73	58 + 6 = 67	30 + 28 = 60	40 + 59 = 98	50 + 6 = 56
63 + 4 = 67	15 + 71 = 86	24 + 38 = 62	26 + 43 = 68	10 + 21 = 30
45 + 50 = 95	16 + 60 = 71	2 + 13 = 12	45 + 48 = 92	64 + 29 = 93
19 + 9 = 33	49 + 7 = 56	78 + 3 = 81	26 + 8 = 30	68 + 6 = 71
65 + 31 = 92	19 + 51 = 75	23 + 17 = 40	5 + 16 = 21	18 + 57 = 75
23 + 1 = 20	19 + 59 = 80	54 + 23 = 77	18 + 6 = 29	34 + 36 = 70
11 + 16 = 27	4 + 77 = 76	24 + 54 = 78	33 + 43 = 76	12 + 82 = 96
69 + 18 = 89	36 + 51 = 85	96 + 1 = 102	68 + 28 = 94	70 + 4 = 70

- ○ There are **60 correct** equations and **40 incorrect** equations
- ○ There are **40 correct** equations and **60 incorrect** equations

(b) Easier Length Version

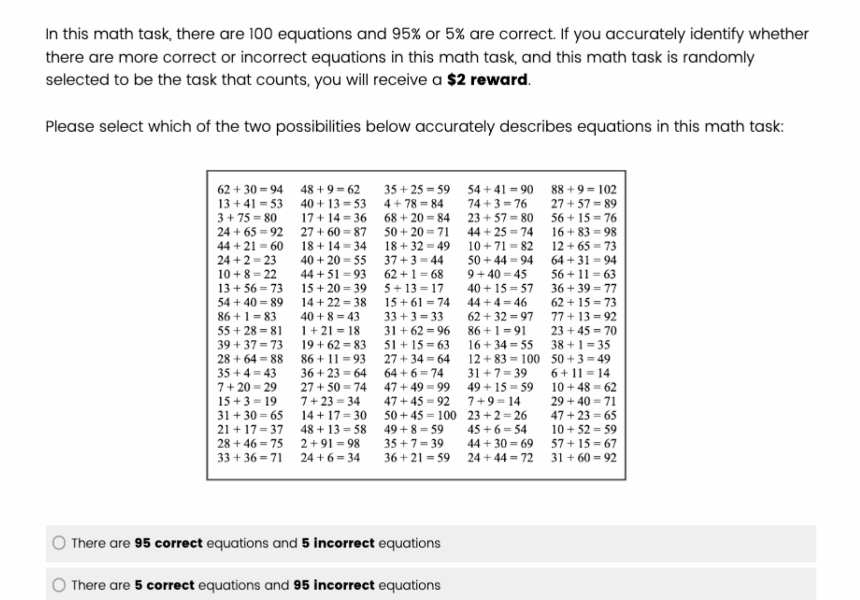
In this math task, there are 10 equations and 60% or 40% are correct. If you accurately identify whether there are more correct or incorrect equations in this math task, and this math task is randomly selected to be the task that counts, you will receive a **$2 reward**.

Please select which of the two possibilities below accurately describes equations in this math task:

23 + 1 = 24
52 + 24 = 71
66 + 1 = 67
8 + 46 = 55
52 + 30 = 82
61 + 15 = 77
25 + 62 = 88
30 + 8 = 42
46 + 46 = 97
73 + 2 = 75

- ○ There are **6 correct** equations and **4 incorrect** equations
- ○ There are **4 correct** equations and **6 incorrect** equations

(c) Easier Discernibility Version



In this math task, there are 100 equations and 95% or 5% are correct. If you accurately identify whether there are more correct or incorrect equations in this math task, and this math task is randomly selected to be the task that counts, you will receive a **$2 reward**.

Please select which of the two possibilities below accurately describes equations in this math task:

62 + 30 = 94	48 + 9 = 62	35 + 25 = 59	54 + 41 = 90	88 + 9 = 102
13 + 41 = 53	40 + 13 = 53	4 + 78 = 84	74 + 3 = 76	27 + 57 = 89
3 + 75 = 80	17 + 14 = 36	68 + 20 = 84	23 + 57 = 80	56 + 15 = 76
24 + 65 = 92	27 + 60 = 87	50 + 20 = 71	44 + 25 = 74	16 + 83 = 98
44 + 21 = 60	18 + 14 = 34	18 + 32 = 49	10 + 71 = 82	12 + 65 = 73
24 + 2 = 23	40 + 20 = 55	37 + 3 = 44	50 + 44 = 94	64 + 31 = 94
10 + 8 = 22	44 + 51 = 93	62 + 1 = 68	9 + 40 = 45	56 + 11 = 63
13 + 56 = 73	15 + 20 = 39	5 + 13 = 17	40 + 15 = 57	36 + 39 = 77
54 + 40 = 89	14 + 22 = 38	15 + 61 = 74	44 + 4 = 46	62 + 15 = 73
86 + 1 = 83	40 + 8 = 43	33 + 3 = 33	62 + 32 = 97	77 + 13 = 92
55 + 28 = 81	1 + 21 = 18	31 + 62 = 96	86 + 1 = 91	23 + 45 = 70
39 + 37 = 73	19 + 62 = 83	51 + 15 = 63	16 + 34 = 55	38 + 1 = 35
28 + 64 = 88	86 + 11 = 93	27 + 34 = 64	12 + 83 = 100	50 + 3 = 49
35 + 4 = 43	36 + 23 = 64	64 + 6 = 74	31 + 7 = 39	6 + 11 = 14
7 + 20 = 29	27 + 50 = 74	47 + 49 = 99	49 + 15 = 59	10 + 48 = 62
15 + 3 = 19	7 + 23 = 34	47 + 45 = 92	7 + 9 = 14	29 + 40 = 71
31 + 30 = 65	14 + 17 = 30	50 + 45 = 100	23 + 2 = 26	47 + 23 = 65
21 + 17 = 37	48 + 13 = 58	49 + 8 = 59	45 + 6 = 54	10 + 52 = 59
28 + 46 = 75	2 + 91 = 98	35 + 7 = 39	44 + 30 = 69	57 + 15 = 67
33 + 36 = 71	24 + 6 = 34	36 + 21 = 59	24 + 44 = 72	31 + 60 = 92

○ There are **95 correct** equations and **5 incorrect** equations

○ There are **5 correct** equations and **95 incorrect** equations

This figure shows examples of the three types of tasks that participants faced. Participants in the length arm saw the task types in (a) and (b), and participants in the discernibility arm saw the task types in (a) and (c).

Figure S.51: End of Block 1

(a) No Feedback, Both Length and Discernibility Arms

You have now completed the first block of the study. You will now complete the second block, which will be very similar to the first.

(b) Feedback, Length Arm

You have now completed the first block of the study. You will now complete the second block, which will be very similar to the first.

Here is a summary of how you did in the first block:

- Out of the 4 math tasks with 10 equations, you answered **50%** (2 out of 4) accurately.
- Out of the 3 math tasks with 100 equations, you answered **33%** (1 out of 3) accurately.

(c) Feedback, Discernibility Arm

You have now completed the first block of the study. You will now complete the second block, which will be very similar to the first.

Here is a summary of how you did in the first block:

- Out of the 4 math tasks with 95% or 5% correct equations, you answered **75%** (3 out of 4) accurately.
- Out of the 3 math tasks with 60% or 40% correct equations, you answered **33%** (1 out of 3) accurately.

After completing the tasks in block 1, participants saw one of the screens above, depending on their arm and whether they were randomly assigned to receive feedback on their performance. Sub-figures (b) and (c) are examples of what participants would have seen since the number of easy and baseline tasks and the percent of each answered correctly varied by participant.

Figure S.52: Screen Before Second Set of MPLs

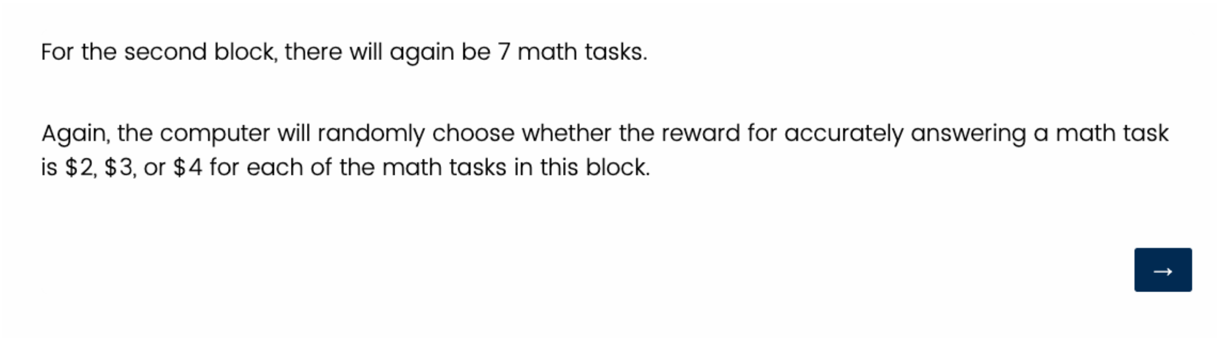


Figure S.53: Multiple Price Lists, Feedback

Choosing the remaining math task in Block 2

Recall that the reward for answering the math tasks accurately could be either $2, $3, or $4. The first table of decisions below is about what kind of math task you would like to answer (and how strong your preference is) if the accuracy reward is $2. The second table is about what you would like if the accuracy reward is $3. The third table is about what you would like if the accuracy reward is $4.

In each of the three tables below, each of the 33 rows is its own separate decision. If the reward is randomly selected to be the one listed at the top of the table and the remaining math task in this block is selected as the task that counts, then for the randomly selected decision:

- If you choose the option on the left, the math task will have 100 equations. You will also receive the **additional** money (i.e., in addition to the bonus you would get if you accurately answer the math task) listed in the option on the left.
- If you choose the option on the right, the math task will have 10 equations. You will also receive the **additional** money (i.e., in addition to the bonus you would get if you accurately answer the math task) listed in the option on the right.

The different amounts of additional money in each decision of each table are for assessing how much more or less you prefer to answer a 10-equation math task over a 100-equation math task. For example, the more likely you think you are to accurately answer a 10-equation math task, the more you might want to choose the options in the right column of each table. And because higher accuracy rewards make answering tasks accurately more important, the point where you cross over from choosing options on the left to choosing options on the right might then be higher up in tables with higher accuracy rewards. The opposite would apply if you think you are more likely to accurately answer a 100-equation math task.

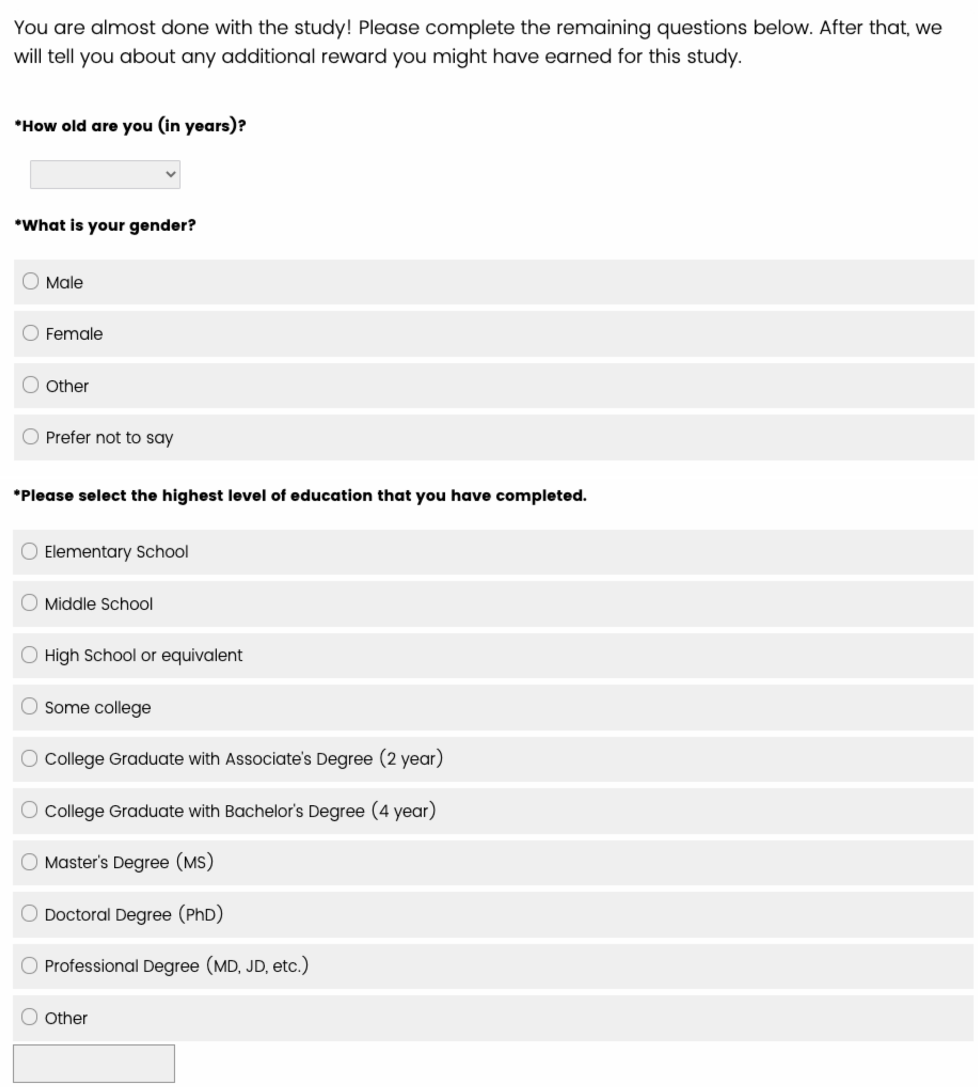
Please indicate which option you prefer in each decision in each table. To speed things up, we made it so that in each table you can simply click on the decision where you want to switch from choosing the option on the left to the option on the right. The more you prefer the math task in the right column, the higher up you should switch from options on the left to options on the right. The more you prefer the math task in the left column, the lower down you should switch from options on the left to options on the right. What you choose in each decision will be highlighted in orange. (Note that you cannot click on the submit button until you have selected your decisions in each table.)

Recall that in block 1, you accurately answered 2 out of 4 of the 10-equation math tasks, and 1 out of 3 of the 100-equation math tasks. This means that if your performance in block 2 is the same as in block 1, you will, on average, earn the following amounts:

If the accuracy reward is $2		**If the accuracy reward is $3**		**If the accuracy reward is $4**	
100-equation math task	10-equation math task	100-equation math task	10-equation math task	100-equation math task	10-equation math task
Earn $0.67	**Earn $1.00**	**Earn $1.00**	**Earn $1.50**	**Earn $1.33**	**Earn $2.00**

If participants were in the feedback treatment, the MPL screen preceding block 2 included the sentence beginning "Recall that..." and the table of expected earnings. (This is an example of a participant in the length arm; for a participant in the discernibility arm, the table headings were "60% or 40% correct equations" and "95% or 5% correct equations.") If the participant was not in the feedback treatment, the instructions were identical to Figure S.47. The MPLs themselves were identical to those in block 1, as pictured in Figure S.48.

Figure S.54: Demographic Information



You are almost done with the study! Please complete the remaining questions below. After that, we will tell you about any additional reward you might have earned for this study.

***How old are you (in years)?**

***What is your gender?**

- Male
- Female
- Other
- Prefer not to say

***Please select the highest level of education that you have completed.**

- Elementary School
- Middle School
- High School or equivalent
- Some college
- College Graduate with Associate's Degree (2 year)
- College Graduate with Bachelor's Degree (4 year)
- Master's Degree (MS)
- Doctoral Degree (PhD)
- Professional Degree (MD, JD, etc.)
- Other

***What was your income in 2020?**

- ○ Less than $10,000
- ○ $10,000 to $14,999
- ○ $15,000 to $24,999
- ○ $25,000 to $34,999
- ○ $35,000 to $49,999
- ○ $50,000 to $74,999
- ○ $75,000 to $99,999
- ○ $100,000 to $149,999
- ○ $150,000 to $199,999
- ○ $200,000 or more

Figure S.55: Final Screen

(a) If the chosen task was not determined by choices in the MPL

Thank you for completing the experiment.

The math task we have randomly selected to determine your reward is math task 1 from block 2.

In the chosen math task, there were 100 equations, of which 40% were correct; so, there were fewer correct equations than incorrect ones. You said that there were fewer correct equations than incorrect ones. This is accurate. Thus, you do get the $3 accuracy reward that was associated with this math task.

This was one of the math tasks that was selected by the computer, and thus the only reward associated with this math task is the reward for getting it right.

When done viewing this information, please advance to the next screen. This will end the study, redirect you to Prolific, and automatically record your response as complete. (You must advance to the next page for your response to be recorded as complete.)

(b) If the chosen task was determined by choices in the MPL

Thank you for completing the experiment.

The math task we have randomly selected to determine your reward is math task 5 from block 2.

In the chosen math task, there were 100 equations, of which 40% were correct; so, there were fewer correct equations than incorrect ones. You said that there were more correct equations than incorrect ones. This is inaccurate. Thus, you do not get the $3 accuracy reward that was associated with this math task.

This was one of the 2 math tasks where you indicated preferences for the possible fraction correct. The randomly chosen row from your decisions in block 2 that was used to select this math task was the one where you chose between (a) "60% or 40% correct + bonus of $3.75" and (b) "95% or 5% correct + bonus of $0." You chose "60% or 40% correct + bonus of $3.75". Thus, you get an additional $3.75 reward.

When done viewing this information, please advance to the next screen. This will end the study, redirect you to Prolific, and automatically record your response as complete. (You must advance to the next page for your response to be recorded as complete.)

Participants saw one of the above screens that displayed their study outcomes. If the task selected to count for payment was not one whose type was determined by the participant's decisions in the multiple price lists, the participant saw the top panel. If the task selected to count for payment was one whose type was determined by the participant's decisions in the multiple price lists, the participant saw the bottom panel.