Attention, Neglect, and Persuasion

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Abstract

I show experimentally that information persuades not only by shifting beliefs but also by redirecting attention. Participants in my experiment repeatedly decide between multi-attribute "goods." In this setting, I show that the responsiveness of choices to the changing values of these attributes is a sufficient statistic for participants' attention. At baseline, participants' are far from the full-attention "rational" benchmark, reacting starkly differently across attributes to equivalent changes in their values. Randomly providing information about the value of one attribute, even when that information is already known and transparently redundant, greatly increases responsiveness to the attribute it describes. This increase comes in large part at the expense of distracting attention away from other attributes, suggesting a capacity constraint for attention. Furthermore, information about a positive attribute boosts demand for its associated good—despite on average providing neutral (and in any case already known) news—which I show implies that inattention takes the form of neglect rather than reliance on a prior. These forces can combine to produce paradoxical responses to correcting beliefs: reducing overoptimism about an attribute nonetheless boosts demand for its associated good.

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1 Introduction

How does information persuade? The traditional view in economics is that messages—from advertisers selling a product, mentors dispensing advice, or experimenters conducting RCTs—affect behavior solely by shifting recipients' beliefs. But many messages appear to convey information that their intended audience already knows: that a well-known soft drink tastes refreshing, that immigrants are different than you, that a religion promises salvation. Evidently, these messages work: but how? One hypothesis is that they persuade by shifting what their recipients pay attention to. If so, what must be true about how attention is allocated for these messages to change people's behavior in the way their senders intend?

In this paper, I study these questions experimentally. I first show that choice data are generally insufficient for identifying attention unless agents' preferences and beliefs are known. This observation motivates my focus on a novel experimental task where such control is possible. Participants repeatedly face a binary choice between two abstract "goods". One good offers participants an amount of money with certainty plus a lottery that pays a larger sum with a small probability. The alternative good has six attributes (three types of coins and three boxes whose value depends on their color), each of which contributes to the participant's payment if she chooses that option. Crucially, preferences over attributes are known ex ante, since they all ultimately map onto cash, and participants' beliefs can be measured by asking them about this mapping (i.e., checking their comprehension and memory). Random variation in each good's attributes then allows me to estimate empirical responsiveness to their exogenously changing values, which I show is a sufficient statistic to estimate attention.

I find first that, even in this simplified choice environment (compared to real-world economic decisions), the average participant is far from the full-attention "rational" benchmark: equivalent changes in the value of different attributes produce significantly different demand responses. Compared to the attribute participants most respond to, choices react between 25% and 89%

less to equal-value changes in other attributes ($p < 0.01$ for all comparisons). These distortions appear despite the vast majority of participants being able, in unincentivized debriefing questions, to correctly describe how the possible values of every attribute would contribute to their earnings. Very similar average distortions appear even among participants who are perfectly informed in this way, suggesting that deviations from the rational benchmark stem not from incorrect beliefs but from failures of full attention.

Next, a subset of participants receive information telling them about the value of one randomly selected attribute. Recall that most participants are already perfectly well-informed about these values. However, their inattentiveness at baseline raises the possibility that such information might persuade them to change their choices by shifting which attributes they focus on. Indeed, I find that this information starkly increases responsiveness to the attribute it describes. These effects are large; responsiveness to an attribute increases by 70% ($p < 0.01$) on average in response to information describing it. I find similar effects (78% increase, $p < 0.01$) when restricting the data to the (large) majority of participants who already know this information, and thus these effects appear to operate primarily by redirecting participants' attention rather than by correcting their beliefs.

These results show that information shifts attention, but three additional results shed light on the underlying drivers of attention and on how these effects operate. First, I find evidence of attention *spillovers*: information about one attribute boosts attention to it in part by decreasing responsiveness to other attributes (by 8% in the full sample, $p = 0.09$, and 9% among participants with correct beliefs, $p = 0.06$). This spillover effect, though smaller than the direct effect on the attribute the information describes, applies to many more attributes. The total spillover effect, summing across all these attributes, therefore amounts to 80% of the direct effect ($p = 0.03$). Because the direct and indirect effects operate in opposite directions and are comparable in magnitude, the total effect on attention across all attributes is small and statistically indistinguishable from zero ($p = 0.62$). These findings point to an attentional capacity constraint. They imply that a communicator (a policymaker, an advertiser) potentially faces tradeoffs when trying to manipulate attention through information provision: directing focus toward a particular feature distracts from others.[1]

Second, information about an attribute boosts demand for its associated good by about 2 percentage points ($p < 0.01$), despite the fact that information provision is (known to be) random and thus uncorrelated with the value of the attribute or of the good as a whole. I show that this effect depends on what I call the "default" value: how an attribute is treated when the agent fails to attend to it. The fact that the effect is positive suggests that this default is *zero*. That is, when failing to attend to an attribute, agents fail to incorporate it into their decision at all, rather than simply relying on its expected or average value. Inattention therefore looks like *neglect*, rather than shrinkage toward a prior.

Third, though the attentional effect of information is large, it is also fragile. Information about one attribute continues to increase attention to it (by 90%, $p < 0.01$) even after it is no longer displayed, but only so long as no other information takes its place. If new information about a different attribute begins to appear, attention to the original attribute falls by 43% ($p < 0.01$), almost completely reverting to the level it would have maintained absent any information (an insignificant 8% difference, $p = 0.54$). These patterns corroborate my interpretation that attention drives these results and speak against alternative mechanisms such as risk aversion, caution, experimenter demand, and any belief-based persuasion channel.

Finally, these effects of information on attention can combine to mask and even overturn their effects on beliefs. Recall that one of the alternatives participants can choose has a lottery associated with it. If they choose this option, they get to roll five six-sided dice, earning an additional bonus if their roll adds up to 12 or less. Participants substantially overestimate the odds of winning this lottery, with the average participant believing she has a 27% chance of

[1]This result is reminiscent of Altmann et al. (2021), who find that incentivizing one task reduces engagement in another task. My results show that these sorts of cognitive spillovers arise even within individual decisions and without modifying the objective incentives that participants face.

winning, whereas the true odds are close to 10%. Nonetheless, information that alerts participants of the true odds of winning—and therefore provides bad news relative to their priors—actually *boosts* demand for the option that includes the lottery by 5.1 percentage points ($p < 0.01$). The explanation of this paradoxical result is that the information, in addition to telling participants about the unfavorable odds, also points their attention toward the lottery. Because it is a positive attribute of the good, which participants may otherwise neglect, this boosts demand despite the bad news it conveys. Adding evidence to this view, an almost identical piece of information that reminds participants about the lottery but does not provide the odds of winning boosts demand even more (by 9.1 compared to 5.1 percentage points, $p = 0.03$).[2]

My focus on an controlled experimental environment is intentional, as it allows me to cleanly identify the attention channel through which information might persuade. First, in field settings, it is typically impossible to rule out belief-based channels even if information changes choices in "paradoxical" ways.[3] Second, how to estimate (or even define) attention is unclear unless agents have linear preferences, which my experiment induces. Third, my experiment allows me to measure preferences for, beliefs about, and attention paid to every dimension that is relevant for participants' choices. Finally, it also lets me provide information that is explicitly randomized and obviously not a signal that recipients should change their beliefs about anything else. All four of these features are crucial for untangling the beliefs and attention effects of information provision.

This paper contributes first to a literature studying non-traditional persuasion (for a review of more standard Bayesian persuasion, see Kamenica 2019).

[2]In addition, the difference in effects between the informative and non-informative reminders is larger for participants with particularly erroneous beliefs ($p < 0.01$).

[3]For example, a message about the current level of inflation might change recipients' beliefs about many things besides just the narrow fact it describes: e.g., about how important inflation is for economic outlook, about how important the sender *thinks* inflation is (inducing demand effects), about other facts like possible government responses or political repercussions, and so on. Similar concerns arise even if recipients already know the information the sender's message conveys or even if the message does not convey any hard information at all (e.g., the fact that a message is trying to make inflation salient to the recipient might reasonably change their beliefs).

For example, previous work has studied how information senders can influence recipients' actions by shifting the analogies they use to solve problems (Mullainathan et al., 2008), making messages simpler or more visually appealing (Bertrand et al., 2010), invoking other-regarding preferences (Coffman & Niehaus, 2020), or shifting the causal model people employ (Schwartzstein & Sunderam 2021, Aina 2023, Barron & Fries 2023). The distinction between belief-based and non-belief based persuasion is not always sharp, but my results provide a particularly clear example of the latter: providing information that recipients already know (and, given the experimental environment, cannot shift their beliefs about anything else) redirects attention and thereby choices.

Second, my evidence contributes to a growing body of theoretical work investigating how agents allocate attention when decisions have multiple relevant features.[4] My result that obviously uninformative messages have such large (but fragile) attentional effects points toward theories that give a central role to contextual factors such as visual prominence (e.g., Bordalo et al. 2022; Bordalo, Conlon, et al. 2023). Attentional capacity constraints appear in multiple existing theories (e.g., Gabaix 2014, Bordalo et al. 2022), though to my knowledge my experimental evidence on their importance is novel. Next, my result that individuals downweight unattended-to attributes rather than relying on their priors is consistent with models of neglect; for example, though Koszegi & Szeidl (2013), Bordalo et al. (2022), and Bushong et al. (2021) make different assumptions about why various attributes escape attention, all assume that inattention takes the form of neglect. This is in contrast to some theories of rational inattention, which typically assume that absent attention agents fall back on their priors (see Mackowiak et al. 2022 for a review). To my knowledge, my evidence testing between these possibilities is novel to this literature.

In addition to theoretical and lab-experimental studies, some applied work studies how people appear to neglect "shrouded attributes," and how reminders

[4]For experiments testing some of these models, see among others Dertwinkel-Kalt et al. (2017), Frydman & Mormann (2017), Li & Camerer (2022), Somerville (2022), and Dean & Neligh (2023).

about such attributes can affect choices (e.g., Chetty et al. 2009, Brown et al. 2010, Allcott & Taubinsky 2015, Bradley & Feldman 2020). My results show that similar results apply even to aspects of decisions that are not, in any straightforward sense, hidden from decision makers. Further, unlike in my experiment, these studies necessarily estimate relative attention to only a small subset (usually a single pair) of a product's relevant attributes (price, taxes, hidden fees, etc.), and thus they cannot speak to total attention allocation or distraction.

Finally, my findings speak to a small literature looking at how information can influence attention (variously defined). For example, Bettinger et al. (2021) find that non-specific messages about school attendance lead parents to later seek out information about their children's schooling. Golman et al. (2021) and Quispe-Torreblanca et al. (2022) study how people may avoid information or situations that would require thinking about unpleasant beliefs. Compared to these studies, I employ a distinct notion of attention: the extent to which already-held beliefs about a relevant feature of a decision are employed in a given choice. There are also some examples of information provision experiments with "backlash" effects (e.g., Barrera et al. 2020, Colonnelli et al. 2023 Alesina et al. 2023). In addition to being able to more cleanly separate the beliefs and attention channels through which information may affect choices (which as discussed above is difficult in naturalistic contexts), my results also shed light on how the psychology of attention must operate for attention to underlie such effects.

2 Theoretical Framework

I first describe a simple model to motivate the experimental design. Assume an agent decides whether to purchase a good characterized by a vector of attributes $\vec{a}$ (for example, a car has a price, certain safety features, fuel economy, etc.). Her utility from purchasing the good is linear in these attributes, according to equation 1:

$$v = \sum_{k=1}^{K} v_k a_k \tag{1}$$

However, I assume that the perceived value of the good depends on which attributes the agent pays attention to. Let $\theta_k \in [0, 1]$ indicate the extent to which the agent attends to dimension k. To the extent that she does, she incorporates her belief $\widehat{a_k}$ about the level of attribute k, which may differ from its true level a_k. To the extent that she neglects k, she uses a default value $\overline{a_k}$. Combining these assumptions, I define her attention-weighted perceived valuation of the good, which I denote by u, using equation 2:

$$u = \sum_{k=1}^{K} \theta_k v_k \widehat{a_k} + (1 - \theta_k) v_k \overline{a_k} \tag{2}$$

Finally, assume that there is some noise $\epsilon \sim F$ such that she purchases the good whenever $u + \epsilon > 0$. Then the probability that i purchases the good is $F(u)$. For simplicity, I assume that F is uniformly distributed in the relevant range: i.e., $F''(u) = 0$.

Note that this basic formulation is in principle compatible with many different theories of attention. For example, attention could be driven (i.e., θ could be determined) by focusing, relative thinking, salience, memory of the same or similar products/choice scenarios, or noisy information-processing about attributes (Koszegi & Szeidl 2013, Bushong et al. 2021, Bordalo et al. 2022, Bordalo, Burro, et al. 2023, Gabaix 2019, Yang & Krajbich 2022).

Is attention identifiable from choice data in such a setting? Consider equation 3, which compares how responsive demand for the good is to changes in the level of two attributes k and j.[5]

$$\frac{dF(u)/da_k}{dF(u)/da_j} = \underbrace{\frac{v_k}{v_j}}_{\text{Preferences}} \times \underbrace{\frac{d\widehat{a_k}/da_k}{d\widehat{a_j}/da_j}}_{\text{Beliefs}} \times \underbrace{\frac{\theta_k}{\theta_j}}_{\text{Attention}} \tag{3}$$

As is intuitive, equation 3 makes clear that demand responses depend on how much the agents cares about attributes (preferences), how much she knows about changes in those attributes (beliefs), and how attentive she is to each of them. It also makes clear that if agents' preferences and beliefs are known ex ante, then relative demand shifts are a sufficient statistic for (relative) attention. This is the setting that the experiment described below aims to construct.

We can now ask what effect information provision will have on agents' choices. To model this, suppose we can employ an information treatment t, which I take to be continuous for illustration purposes (e.g., perhaps t denotes the forcefulness or credibility of the information). Assume for simplicity that t only changes beliefs about one attribute a_k. Equation 4 then follows:

$$\frac{d}{dt}\left[\frac{dF(u)}{da_k}\right] = F'(u)v_k\left(\underbrace{\theta_k\frac{d}{dt}\left[\frac{d\widehat{a_k}}{da_k}\right]}_{\text{Effect on Beliefs}} + \underbrace{\frac{d\widehat{a_k}}{da_k}\frac{d\theta_k}{dt}}_{\text{Effect on Attention}}\right) \tag{4}$$

The first term of equation 4 captures a "traditional" belief-based persuasion effect: information might increase how responsive demand is to an attribute because it affects how responsive *beliefs* are to that attribute. For example, an agent might switch from being ignorant of the level of k ($\frac{d\widehat{a_k}}{da_k} = 0$) to being fully informed ($\frac{d\widehat{a_k}}{da_k} = 1$). The second term captures an attention effect: information might increase attentiveness to the attribute it describes, over and above any effect on beliefs. Note that in the extreme case where beliefs are already correct and thus do not react to information ($\frac{d}{dt}\left[\frac{d\widehat{a_k}}{da_k}\right] = 0$), choices

[5]Equation 3 makes a few implicit, but substantive, assumptions. Namely, it assumes that changes in the level of one attribute do not change beliefs about the levels of *other* attributes, the amount of attention paid to any attribute, or the default values $\overline{a_k}$.

only react through this attentional channel.

In addition to asking how information affects the responsinveness of demand to changes in attributes, we can also ask how it might affect the total level of demand, as given by equation 5:

$$\frac{dF(u)}{dt} = F'(u)\left[\underbrace{\theta_k v_k \frac{d\widehat{a}_k}{dt}}_{\text{Effect on Beliefs}} + \underbrace{\sum_{f=1}^{K} v_f \frac{d\theta_f}{dt}(\widehat{a}_f - \overline{a}_f)}_{\text{Effect on Attention}} \right] \tag{5}$$

Here again we see two terms corresponding respectively to a beliefs channel and an attention channel. First, of course, if the information increases the agent's belief about the level of attribute k (and she has a positive preference v_k for it), this will increase demand. Note that if the agent's beliefs are unbiased and information is randomly assigned, such that on average it does not change her beliefs about the value of k, then information will have no average beliefs-based effect. As we will see, this is (approximately) the case for some information in my experiment.

The second term of equation 5 says that, even absent an effect on beliefs, information will boost demand if it tends to increase attention to attributes that are higher than their "default" value $\overline{a}_f$. Thus any such effects depend crucially on how attributes are treated when they are neglected. Suppose again that information is randomly assigned, such that on average it arrives when attributes are equal to their expected value. If the default is this expected value (as in some models of optimized limited attention, see Gabaix 2019), then such information should have no average effect. If instead inattention takes the form of neglect or downweighting, as in other models (e.g., Koszegi & Szeidl 2013, Bushong et al. 2021, Bordalo, Conlon, et al. 2023), then information can have attentional effects on choices even when it does not focus the agent on especially positive attributes. Simply increasing attention to attributes that are positive *at all* (not necessarily unusually so) can boost demand. The experiment described below in part aims to test between these two possibilities.

3 Experimental Design

3.1 Option A vs Option B

Participants were recruited through Prolific to participate in an online survey (see Appendix B for details on recruitment, sample, compensation, comprehension checks, and pre-registration). The main part of the experiment asked participants to repeatedly choose between two options, labelled Option A and Option B, for how their bonus payment would be calculated. They made 80 such choices, and one of these was randomly chosen at the end of the experiment to actually determine their bonus payment. The order in which the two options were presented (Option A on the left and Option B on the right, or vice versa) was randomized across participants but held fixed throughout the experiment. Figure A.I shows screenshots of two such choices.

Option A had two "attributes." First, it listed an amount of money that would, with certainty, be added to participants' bonus if they chose this option. The exact amount (though participants were not told this) was chosen independently across choices from a normal distribution with a mean of \$0.40 and a standard deviation of \$0.20 (with a minimum of \$0.00). Second, if they chose Option A in the decision that was randomly selected to be implemented, they also got to roll five virtual six-sided dice. If the sum of these rolls added up to 12 or less, an extra \$1.00 or \$2.00 (randomized across participants) was added to their bonus. Right after the instructions page that described this lottery to them, participants were asked their belief about the percent chance of winning such a lottery. The average [median] answer was 27% [20%], significantly higher than the true chance ($p < 0.01$), which is approximately 10%.

Option B had six attributes. I will later look at the impact of providing information about one of these attributes on how responsive participants are to them (more details below). They were therefore designed to be as similar to each other as possible while remaining distinct enough to be considered separately. Three of these attributes were listed numbers of coins (pennies, nickels, and dimes), the value of all of which would be added to their bonus if they chose Option B. There were always 2, 12, or 22 pennies; 1, 3, or 5 nickels;

and 0, 1, or 2 dimes. Notice that the three coins therefore took on almost the same range of monetary values and differed from choice to choice by similar amounts (always 0, 10, or 20 cents different). Further, the participant pool was restricted to people living in the US for whom the value of these coins is familiar (which I confirm below).

The other three attributes of Option B were colored boxes (arranged vertically such that there was a top, middle, and bottom box), each of which could take on one of three colors (a different three colors for each box). At the beginning of the experiment, before any other instructions, participants were asked to rank each set of three colors according to "how much you like them." Whichever color they ranked highest then added \$0.20 to their bonus, the one they ranked second added \$0.10, and the one they ranked last added \$0.00. Participants were told this was how the survey chose these values. Notice that all the colored boxes therefore take on a similar range of monetary values as each other and as the coins, and they differ from choice to choice by similar amounts (again, always 0, 10, or 20 cents different). The values of the colors were assigned according to participants' preferences to make them easy to remember (which I confirm below)

The values of five of the six attributes of Option B were chosen randomly and independently across each choice, with each of the three possible values being equally likely to be chosen. The sixth attribute, randomly selected, was "frozen" at one particular value for the entire experiment. This value was also equally likely to be any of the three possible values for the attribute, but simply did not vary from choice to choice.

3.2 Information about Attributes

Unknown to participants, the 80 choices were divided into four periods, each of which lasted for 20 choices. Periods differed in whether and what type of information participants in different treatment groups were provided while they made their choices. Table 1 summarizes each period for the four different treatment groups.

During Period 1, participants simply chose between Options A and B, as described above, without receiving any additional information. During Period 2 (choices 21 to 40), 80% of respondents (Treatments 2, 3, and 4) began to see information at the top of the screen about a randomly selected attribute of Option B (chosen with equal likelihood from among the five non-frozen attributes). This information told participants how much the particular value of that attribute in the current choice was worth. For example, it might read, "Remember, a gold top box adds $0.20!" or "Remember, 12 pennies add $0.12!" This message would change as the value of the relevant attribute changed across choices. Participants were told (truthfully) that any information they were provided was chosen randomly and thus was not related to how useful it would be for their decision.

During Period 3 (choices 41 to 60), respondents who received no information in Period 2 continued to see no information. Among participants who received information in Period 2 (Treatments 2-4), Treatment 2 reverted to seeing no information in Period 3. Treatment 3 continued to see information about the same attribute as in Period 2. Treatment 4 was instead shown information about a new attribute (picked at random from the remaining four non-frozen attributes).

Period 4 did not provide any information about the attributes of Option B. Rather, and within treatment groups, participants were randomized to either receive no information or to receive one of two messages about the lottery associated with Option A. The first message, which I call "Lottery," simply reminded participants about the lottery (which, though described in the instructions and comprehension checks, was not mentioned on the later decision screens). The second message, "Lottery + Odds," was almost identical but included the numerical odds of winning. In particular, the "Lottery + Odds" message read "Remember, Option A also comes with a 10% chance to win an additional $1 [or $2] prize!" with smaller text at the bottom of the screen telling them the details of how the lottery worked. The "Lottery" message was identical except "10%" did not appear. Because the lottery did not vary from choice to choice, the message remained unchanged at the top of the screen for the entirety of Period 4.

3.3 Beliefs about Attributes

After making all 80 decisions, participants were asked whether they knew how each possible value of Option B's attributes contributed to their bonus payment. In particular, they were first asked how much money a penny, a nickel, and a dime were worth. Reassuringly, 95% of participants get all of these questions correct. Next, for each of the colored boxes associated with Option B (recall there was a top, middle, and bottom box), they were asked to match each possible color to its monetary value (\$0.00, \$0.10, \$0.20). For each box, between 87 and 89% of participants get all three values correct. Seventy-four percent of participants get all 12 questions (three coins, and three values for each of the three boxes) right. Note that this high accuracy appears despite these questions being unincentivized. We see similar levels of accuracy (indeed slightly higher, 75%) among participants who received no relevant information about the attributes during the 80 choices (Treatment 1). This high level of accuracy likely in part reflects the fact that the mapping between colors and money depended on participants' preferences over colors (and thus participants could reconstruct values by thinking about these preferences).

4 Results

4.1 Empirical Strategy

For most of the results below, I estimate variants on equation 6 by OLS:

$$ChoseOptionB_{i,t} = \beta_0 T_{i,t} + \sum_k \beta_k a_{i,k,t} T_{i,t} + \mu_i + \epsilon_{i,t} \tag{6}$$

In the above equation, $ChoseOptionB_{i,t}$ indicates whether participant i chose Option B in decision t. $T_{i,t}$ indicates some treatment status (e.g., whether/what type of information was visible for i during t). $a_{i,k,t}$ is the monetary value of attribute k in that choice, where there are eight possible attributes: the three coins, three colored boxes, the certain payment in Option A, and the value of the lottery in Option A. I define $a_{i,k,t}$ for each attribute such that β_k should be positive for all of them (i.e., I multiply Option A's attributes by negative one). In practice, I often add multiple attributes together (e.g., the value of all the colored boxes, or all non-frozen Option B attributes) to increase power and interpretability. I also recenter all attribute values such that they have mean zero. Finally, I also usually include individual-fixed effects μ_i, which could represent person-specific biases toward one or the other option, or heterogeneity in default values $\overline{a_k}$.

Estimating equation 6 lets us answer several questions. First, it tells us how responsive demand for Option B is to the values of various attributes within any given treatment (i.e., any information environment). Recall from equation 3 that responsiveness to attributes reveals a combination of 1) preferences over attributes, 2) beliefs about changes in the attributes, and 3) relative attention to attributes. By design, the experiment fixes preferences over attributes, as each attribute of Option B is straightforwardly worth a certain amount of money.[6] In addition, I measure participants' beliefs about attributes. I can thus ask whether differential responsiveness to attributes is due to differential beliefs or to inattention. Similarly, I can ask whether changes in this responsiveness across treatment groups is due either to treatment effects on beliefs or on attention, as in equation 4.

Next, the first term in equation 6 tells us how demand responds on average to information about an attribute, pooling across the particular values that attribute takes on. Recall from equation 5 that this effect depends both on whether information shifts attention and on what the "default" value is (i.e., how an attribute is treated when it is neglected).

Note that equation 6 has a clear "rational" benchmark: if agents' pay

[6] In principle, the presence of the lottery for Option A raises the possibility that risk aversion could affect how participants are willing to trade-off between Options A and B. In practice, as we will see, participants are *more* responsive to the certain payment in Option A than to Option B's attributes, the opposite of what we would expect if risk aversion were a substantial factor in participants' decisions. Since this issue would not substantially affect interpretation any of the main results, I henceforth assume participants are risk-neutral.

equal attention to all attributes, and if they have correct beliefs about those attributes, then demand responses should all be equal (i.e., $\beta_k = \beta_j$ for all k and j). Further, information should have no effect ($\beta_0 = 0$ and β_k should not depend on treatment).

4.2 Baseline Attention

Is attention systematically distorted at baseline (i.e., in Period 1 before participants received additional information about any attribute)? Column 1 of Table 2 shows estimates of equation 6 (without individual-fixed effects or treatment dummies) for four attributes: Option A's certain monetary value, the subjective value of Option A's lottery, Option B's coins, and Option B's colored boxes. To calculate each participants' subjective value of Option A's lottery, I multiply its monetary prize by participants' stated priors about their odds of winning.

We see large differences across attributes in this average measure of attentiveness. Compared to Option B's colored boxes, participants are 113% more attentive to the value of Option B's coins and 180% more attentive to Option A's certain value. They appear least attentive to Option A's lottery (which, recall, was not visually represented on each decision screen), as the coefficient on it is only 31% of that on Option B's colored boxes.[7] For each pair of attributes, we can reject that the responsiveness of demand is equal ($p < 0.01$ for all pairwise comparisons).

By construction, these differences cannot be due to differences in how participants actually value these attributes. They could, however, be due to misperceptions about how each attribute would contribute to their bonus payment (e.g., not remembering what each colored box is worth). To explore this possibility, column 3 of Table 2 restricts the analysis to the "correct-beliefs sample," the 74% of participants who correctly reported, in the unincentivized questions at the end of the survey, the value of each type of coin and the val-

[7]This coefficient could suffer from attenuation bias to the extent that participants' reported beliefs about the lottery are noisy.

ues of each possible colored box.[8] We see similar estimates to those from the full sample: these particpants are 147% more responsive to Option A's certain value, 92% more responsive to Option B's coins, and 69% less responsive to Option A's lottery than they are to Option B's colored boxes. Again, we can reject equality of responsiveness for each pair of attributes ($p < 0.01$). Thus, these differences appear to be driven by selective attention, rather than by mistaken beliefs.

Next, recall that a randomly chosen attribute of Option B was frozen at its initial value throughout the whole experiment. Column 2 shows that participants are 26% less responsive to this attribute than to the attributes that were changing for them from decision to decision ($p = 0.08$). We see similar relative inattention among the correct-beliefs sample (38%, $p = 0.02$).

Though it was not the primary purpose of the experiment, we can nonetheless ask whether the differences in responsiveness across attributes at baseline are what we might expect from existing theories of attention allocation. Bordalo et al. (2020) assume that features grab attention to the extent that they stand out relative to prior experiences, consistent with the fact that changing attributes drew more attention than the frozen attribute. Gabaix (2019) assumes that agents allocate their attention trading off the cognitive costs of doing so with the benefits to be gained. Consistent with this idea, participants paid less attention to Option B's colored boxes, whose values were plausibly more cognitively costly to retrieve, than to its coins. In addition, the certain value of Option A (which had a larger variance and therefore greater benefits to attending to it) garnered more attention. This latter effect is also in the spirit of the focusing model of Koszegi & Szeidl 2013 (though that model considers range within choice sets, not variance over time). Though no individual model captures all the differences across attributes that I estimate, I view them as broadly consistent with a combination of these explanations, corroborating my interpretation that inattention drives these patterns.

[8] Some of these participants, between Period 1 and belief elicitation, saw information telling them about one (Treatments 2 and 3) or two (Treatment 4) attributes of Option B. Table A.I shows similar results even for participants in Treatment 1, who were never provided information about Option B's attributes.

4.3 Responsiveness to Information

I now turn to the question of how providing information about Option B's attributes affects participants' choices. Given that the large majority of respondents are able to recall how each attribute's possible values contribute to the total value of Option B, it might be natural to think that such information should have little effect. However, recall from equations 4 and 5 the possibility that information might affect choices even without any effect on beliefs to the extent that it changes the relative attention that agents place on different attributes.

To investigate this possibility, we can compare participants in treatments group 2-4, who received information in Period 2 about a randomly selected attribute of Option B, to those in Treatment 1, who continued to see no additional information. In practice, the experiment implemented these treatments by choosing, for each person regardless of treatment group, a random "Target" attribute of Option B. In Treatments 2-4, participants then began to receive information about this attribute in Period 2. I can therefore compare responsiveness to this target attribute depending on whether participants were (Treatments 2-4) or were not (Treatment 1) receiving information about it.

Table 3 shows OLS estimates of equation 6, where the three attributes are the certain value of Option A, the target attribute of Option B, and the non-target attributes of Option B (all summed together). Column 1 pools data from all participants for Periods 1 and 2. We see, first, that the information has a large effect on responsiveness to the target attribute, increasing by 70% the attention participants pay to it (from 0.074 to 0.126, $p < 0.01$). We see similar sized (and indeed, directionally larger) effects if we restrict the sample to respondents who, in the unincentivized questions at the end of the survey, correctly identify how each attribute of Option B contributes to their bonus (column 4 of Table 3). This result is consistent with the information primarily operating by changing how much attention participants pay to different attributes, rather than through its effect on their beliefs.

Columns 2-3 and 5-6 of Table 3 split the sample by whether the target attribute was a coin or colored box. We see that effects are larger for information about the colored boxes ($p < 0.05$ for both the full and correct-beliefs samples), suggesting that attention effects are larger when baseline attention is lower. However, even information about coins significantly affects the difference in responsiveness to target vs non-target attributes. Because it is obvious (and the later unincentivized questions confirm) that participants are already perfectly aware that, say, two dimes are worth $0.20, the most natural interpretation of these results is that the information shifts attention.

In addition to boosting attention to the target attribute, Table 3 shows that it *lowered* attention paid to the non-target attributes of Option B (by 8% in the full sample, $p = 0.09$, and by 9% in the correct-beliefs sample, $p = 0.06$). This finding suggests an attentional capacity constraint, whereby (at least for some participants) the information boosted attention toward the target attribute by distracting attention away from other attributes. Table A.II shows similar regressions but interacting treatment status additionally with the value of Option A's lottery and with the value of the "frozen" Option B attribute.[9] Column 1 shows that while attention to the target attribute increases by 0.048 ($p < 0.01$) in response to information describing it, the total attention paid to all other attributes decreases by 0.039 ($p = 0.03$), or 80% of the effect on the target attribute. The increase in total attention, including all target and non-target attributes, is therefore an insignificant 0.009 ($p = 0.615$). We see similar effects among the correct-beliefs sample (column 4).

The effects described so far concern how information affects the responsiveness of demand to various attributes. The uninteracted effect of the information in columns 1 and 4 of Table 3 shows that information about the target attribute also had a significantly positive average effect on demand for Option B (by 2.2pp or 2.0pp for the full and correct-beliefs samples, $p < 0.01$ for both comparisons).[10] These effects appear despite the information being uncorrelated with the value of the attribute it described. That is, on average the treatment provided neutral information (which, in any case, most partic-

[9] We remove fixed effects from these regressions since these attributes do not vary from choice to choice for a given individual.

[10] Recall that the values of each attribute in Table 3 are recentered to have mean zero, so the main effect of information can be interpreted as the effect at the mean of these values.

ipants already knew) about the value of the target attribute. This result is consistent with the default value—how an attribute is treated when it is not attended to—being *zero* rather than agents' priors about its value. Consistent with this effect operating through attention, these effects are larger for information about colored boxes than about coins ($p = 0.03$ and $p = 0.07$ for the full and correct-beliefs samples), corresponding to the larger boost in attention for these attributes compared to coins.

4.4 Dynamics of Attentional Effects

How stable are these attentional effects? Table 4 shows estimates of equation 6 using data from only Period 3, where again the three attributes are the certain value of Option A, the target attribute of Option B, and the non-target attributes of Option B (all summed together). The four columns restrict attention to each of the four treatment groups. We see that having received information about the target attribute still has a significant effect on attention paid to it, even when this information is no longer visible (Treatments 2 vs 1, 0.119 vs 0.064, $p < 0.01$). This increased attention is almost identical to the attention paid to the target attribute when the information is still visible (Treatments 2 vs 3, 0.119 vs 0.122, $p = 0.81$). In contrast, when information about a new attribute begins appearing (Treatment 4), the effect of the previous information disappears entirely: attention to the target attribute reverts to a very similar level as if participants had never received the information (0.064 vs 0.069 in Treatments 1 vs 4, $p = 0.54$) and much less than if the new information had not appeared (0.119 vs 0.069 in Treatments 2 vs 4, $p < 0.01$). These results suggest that, while information can have large effects on attention and that these effects can outlast the information itself, they are also quite fragile.[11]

[11] Interestingly, there is no clear distraction effect in Period 3 as there was in Period 2. That is, attention to the non-target attributes in Period 3 is not noticeably or statistically significantly lower in Treatments 2 and 3 than in Treatment 1. This suggests that with practice (i.e., many repeated similar decisions in the same information environment) participants may be able to overcome the distracting effect of the information about the target attribute. However, these comparisons are somewhat underpowered, so caution is warranted

Note that these dynamics are inconsistent with other interpretations of what could be driving the main "attentional" effects. First, one might worry that our correct-beliefs sample, though they can accurately report the value of each attribute after the experiment, did not have this knowledge during Period 2. Or perhaps they are simply uncertain about each attribute's value (perhaps especially so for colored boxes); risk aversion (or caution à la Cerreia-Vioglio et al. 2015) might then lead them to be less reactive to them. The effect of information could then be driven by changing their beliefs or reducing their uncertainty. But notice that both these explanations would predict that information about the target attribute should continue to boost responsiveness to it even when information about a new attribute is provided in Period 3 (Treatment 4). Instead we see responsiveness to the target attribute revert to its baseline level, consistent with an attention interpretation.

Next, one might worry that the effects in Period 2 are driven by a form of experimenter demand. For example, perhaps (despite the instructions telling them otherwise) participants believed information appeared during decisions in which the attribute described was especially important. If so, we should expect responsiveness to the target attribute to decrease when information about it ceased to appear in Period 3 (Treatment 3). Instead, we see the same boost in responsiveness in Treatment 3 as in Treatment 2 (for whom information about the target attribute continued to appear), consistent with information shifting attention toward the attribute it describes until new information redirects focus toward something else.

4.5 The Interaction between Attention and Beliefs

Thus far, I have focused on the effect of information in cases where it communicates only what decision makers already know. Clearly, this is a special case, as information is often provided at least in part with the aim of changing recipients' beliefs. In this section, I explore the interaction between the effect of information on attention and its effect on beliefs. To do so, I

when interpreting them.

look at Period 4 of the experiment. Recall that during Period 4, participants were randomly (and independently of their treatment group) sorted into three groups. A third of participants saw no additional information in Period 4. Another third were shown the "Lottery" message, which reminded them that Option A also came with a lottery that added \$1 or \$2 to their bonus if their roll of five 6-sided dice add up to 12 or less. The final third of participants received the almost identical "Lottery + Odds" message, which additionally included the fact that such a lottery pays off about 10% of the time, much lower than participants' priors (mean 27%, median 20%, both significantly different from 10% at $p < 0.01$).

What effect should we expect the "Lottery + Odds" mesage to have on demand for Option A? As shown in equation 5, there may be two competing effects. First, in one sense this information clearly conveys bad news about Option A: it should reduce participants' beliefs about the value of the lottery, and hence of Option A. But second, if participants would otherwise fail to pay attention to the lottery, the information's attentional effect depends on the lottery's default value $\overline{a}$. The results in 4.3 suggested that this default is at least sometimes zero. If so, then boosting attention toward the lottery could nonetheless boost demand for Option A by pointing attention toward one of its positive attributes (even if it is not as positive as participants' priors suggested).

Table 5 shows OLS estimates where the dependent variable is whether participants chose Option A (which included the lottery), pooling all decisions across all periods of the experiment. I regress this variable on the certain value of Option A, the total value of all six of Option B's attributes, individual-fixed effects, an indicator variable for whether participants saw the information about the lottery that did not include odds, and a similar indicator for the information that also included the odds of winning. In column 1, we see that the "Lottery + Odds" message significantly *boosted* demand for Option A by 5.1pp, ($p < 0.01$), despite (in a sense) delivering bad news about it for the average participants.

We can disentangle the beliefs and attention effects of the "Info + Odds" treatment by comparing it to the "Info" treatment, which (plausibly) increased attention to the lottery without reducing beliefs about its odds. Column 1 Table 5 shows that this message boosted demand for Option A by even more (9.1pp, $p < 0.01$). This result suggests that the "pure" attention effect is quite large, enough to countervail the significant beliefs effect of 4.0pp (9.1pp minus 5.1pp, $p = 0.03$).

In column 2, I additionally interact these indicators with the error (beliefs minus truth) in participants' priors about the odds of winning the lottery. For participants who do not receive information about the odds of winning, we see directionally larger effects for respondents who overestimated the odds of winning by more ($p = 0.20$). In contrast, for participants who also learned the true odds of winning, the interaction term is negative and statistically significant ($p < 0.01$), as we would expect from the information correcting misperceptions. The main effect of both interventions—the effect for participants whose beliefs are correct, which we can interpret as the pure attentional effect—is large and positive (around 8 percentage points, $p < 0.01$, for both), and statistically indistinguishable from each other ($p = 0.72$).

5 Discussion

In this paper, I show experimentally that information affects choices by shifting attention, and that these effects can be large enough to overturn the traditional beliefs-based channel of persuasion. Further, these attentional shifts appear despite the information in my experiment often being transparently unhelpful ("12 pennies are worth \$0.12"), suggesting that in other contexts salient messages may redirect attention even when recognized as manipulative. For example, advertisers' incentives are obvious to consumers, and yet they still often provide clearly redundant or repetitive information to potential customers.

Other aspects of my results shed further light on how attention is allocated. First, they suggest that attention is capacity constrained: shifting focus to one attribute distracts from others. This dynamic may have implications for policymakers hoping to improve decision-making by providing information or reminders: the net benefit of such interventions will depend on what attributes or considerations such messages crowd out.

Second, they suggest that attention is fragile. Information starkly shifts what agents attend to, but new information can quickly undo such effects. Again, this may provide lessons for policy makers, suggesting that to be effective reminders need to be provided close to the moment where a relevant decision is being made. An open question is what factors contribute to the longevity of attention effects, and how these forces interact with similar fragility in belief updating (e.g., see Graeber et al. 2022).

Finally, my results suggest that inattention takes the form of *neglect*: features that escape attention appear to drop out of agent's decision procedures entirely, rather than simply being treated as having some expected or average value. Clearly, such effects have limits (e.g., when failing to think about the importance of oxygen, we continue to breathe), and exploring these boundaries is an important question for future work. But my results suggest that at least sometimes failures to pay attention will lead us away from a sensible default, with implications for when and how reminders and information will improve decisions.

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Table 1: Information across Treatment Groups

	Treatment 1	Treatment 2	Treatment 3	Treatment 4
Pre-Decisions	⟵ Identical Instructions ⟶			
Period 1: Decisions 1-20	None	None	None	None
Period 2: Decisions 21-40	None	Random	Random	Random
Period 3: Decisions 41-60	None	None	Same as Period 2	New Random
Period 4: Decisions 61-80	⟵ None, Lottery, or Lottery + Odds ⟶			
N	134	239	114	103

Notes: This table describes the distribution of participants across treatment groups and the information presented to each group throughout the experiment. "Random" denotes information about one randomly chosen attribute of Option B. "Lottery" indicates information about the existence of the lottery associated with Option A, but not about its odds. "Lottery + Odds" indicates information that also included the numerical odds of winning Option A's lottery. The first row indicates that instructions were identical to all participants, regardless of treatment group. The row for Period 4 indicates that information about lotteries (or not) were randomly assigned independently of treatment group. The experiment sorted participants into treatment groups at the beginning of the survey, with 20% probability of being assigned to Treatments 1, 3, and 4, and a 40% probability of being assigned to Treatment 2. Variation in sample sizes from this distribution is due to chance.

Table 2: Attention at Baseline

	Full Sample		Correct-Beliefs Sample	
	(1)	(2)	(3)	(4)
Option A Value	0.128***	0.128***	0.126***	0.127***
	(0.002)	(0.002)	(0.002)	(0.002)
Option A Lottery	0.014***	0.014***	0.016***	0.017***
	(0.003)	(0.003)	(0.004)	(0.004)
Option B Coins	0.096***		0.098***	
	(0.004)		(0.004)	
Option B Boxes	0.045***		0.051***	
	(0.004)		(0.004)	
Option B Changing		0.073***		0.079***
		(0.002)		(0.003)
Option B Frozen		0.054***		0.049***
		(0.011)		(0.012)
Constant	0.531***	0.554***	0.562***	0.586***
	(0.014)	(0.014)	(0.016)	(0.016)
Observations	11,800	11,800	8,740	8,740
Individuals	590	590	437	437
R^2	0.35	0.34	0.35	0.34
p-value: Option A Value = Boxes	0.00		0.00	
p-value: Option A Lottery = Boxes	0.00		0.00	
p-value: Coins = Boxes	0.00		0.00	
p-value: Moving = Frozen		0.08		0.02

Notes: Each column shows OLS estimates of the equation 6 using the Period 1 decisions of all treatment groups, with the following modifications. First, they do not include treatment dummies or individual-fixed effects. Second, the attributes in columns 1 and 3 include the certain value of Option A ("Option A Value"), the subjective value of Option A's lottery ("Option A Lottery"), the sum of the values of Option B's coins ("Option B Coins"), the sum of the values of Option B's colored boxes ("Option B Boxes"). The subjective value of the lottery is calculated by multiplying the prize for winning the lottery with each participants' prior belief about their odds of winning (winsorized at the 90th percentile). Because the dependent variable is whether participants chose Option B, the certain value and lottery value of Option A are multiplied by negative one so that the expected sign of all coefficients is positive. The attributes in columns 2 and 4 are identical to columns 1 and 3 for Option A, but for Option B include, first, the sum of the values of the five changing attributes and, second, the value of the (randomly chosen) attribute that was frozen throughout the experiment at its initial value. Columns 1-2 include all participants, while columns 3-4 include only participants who correctly respond to unincentivized questions at the end of the survey about the monetary value of each coin and every possible colored box. Robust standard errors, clustered at the individual level, are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Effects of Information about Target Attribute

	Full Sample			Correct-Beliefs Sample		
	Pooled (1)	Coin (2)	Box (3)	Pooled (4)	Coin (5)	Box (6)
Info	0.022***	0.010	0.042***	0.022***	0.011	0.039***
	(0.007)	(0.010)	(0.010)	(0.008)	(0.011)	(0.011)
Target Attribute X No Info	0.074***	0.100***	0.047***	0.074***	0.096***	0.054***
	(0.005)	(0.006)	(0.006)	(0.005)	(0.007)	(0.007)
Target Attribute X Info	0.126***	0.123***	0.132***	0.132***	0.123***	0.143***
	(0.006)	(0.008)	(0.008)	(0.006)	(0.009)	(0.008)
Non-Target Attributes X No Info	0.075***	0.070***	0.080***	0.080***	0.073***	0.087***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Non-Target Attributes X Info	0.069***	0.063***	0.074***	0.073***	0.062***	0.082***
	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)
Option A Value X No Info	0.127***	0.132***	0.123***	0.125***	0.131***	0.121***
	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Option A Value X Info	0.129***	0.133***	0.125***	0.128***	0.133***	0.124***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	23,600	11,400	12,200	17,480	8,200	9,280
Individuals	590	285	305	437	205	232
R^2	0.51	0.52	0.51	0.52	0.53	0.52
p-value: Responsiveness to Target Unaffected by Info	0.00	0.01	0.00	0.00	0.02	0.00
p-value: Responsiveness to Non-Target Unaffected by Info	0.09	0.16	0.18	0.06	0.04	0.35
p-value: Target vs Non-Target Tradeoff Unaffected by Info	0.00	0.01	0.00	0.00	0.00	0.00
p-value: Same Effect on Tradeoff for Info about Coin vs Box			0.00			0.00

Notes: This table shows OLS estimates of the equation 6 using the Periods 1 and 2 decisions of all treatment groups. The attributes k include the certain value of Option A ("Option A Value"), the value of the randomly selected "target attribute" of Option B, and the sum of the values of the five other attributes of Option B. Because the dependent variable is whether participants chose Option B, the certain value of Option A are multiplied by negative one so that the expected sign of all coefficients is positive. "Info" is a dummy variable for being in Treatments 2-4, who during Period 2 received information about the value of the target attribute. Columns 1-3 include all participants, while columns 4-6 include only participants who correctly respond to unincentivized questions at the end of the survey about the monetary value of each coin and every possible colored box. Robust standard errors, clustered at the individual level, are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Dynamics of Information Effects

	Treatment 1 (1)	Treatment 2 (2)	Treatment 3 (3)	Treatment 4 (4)
Target Option B Attribute	0.064***	0.119***	0.122***	0.069***
	(0.010)	(0.007)	(0.010)	(0.012)
Non-Target Option B Attributes	0.071***	0.074***	0.070***	0.074***
	(0.005)	(0.004)	(0.006)	(0.006)
Option A Value	0.132***	0.131***	0.127***	0.135***
	(0.004)	(0.003)	(0.004)	(0.005)
Observations	2,680	4,780	2,280	2,060
Individuals	134	239	114	103
R^2	0.56	0.55	0.57	0.54
p-value: Responsiveness to Target Same as Treatment 1		0.00	0.00	0.74
p-value: Responsiveness to Target Same as Treatment 2			0.81	0.00
p-value: Responsiveness to Target Same as Treatment 3				0.00

Notes: This table shows OLS estimates of equation 6 using the Period 3 decisions of each treatment group (separately by column), with the following modifications. First, the attributes k include the certain value of Option A ("Option A Value"), the value of the randomly selected "target attribute" of Option B (which the information in Period 2, when presented, described), and the sum of the values of the five other attributes of Option B. Second, I do not include treatment dummies, since treatment is constant within columns. The certain value of Option A is multiplied by negative one, such that the expected sign of all coefficients is positive. Treatment 1 never received information about the target attribute. Treatments 2-4 received information about the target attribute in Period 2, but differed in the information presented during Period 3. During this period, Treatment 2 received no information, Treatment 3 continued to receive information about the target attribute, and Treatment 4 received information about a different attribute (randomly chosen from the remaining four non-frozen attributes of Option B). Robust standard errors, clustered at the individual level, are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Effect of Information on Beliefs and Attention

	(1)	(2)
Lottery Info without Odds	0.091***	0.077***
	(0.013)	(0.017)
Lottery Info including Odds	0.051***	0.085***
	(0.013)	(0.017)
Lottery Info without Odds X Error in Prior		0.099
		(0.076)
Lottery Info including Odds X Error in Prior		-0.217***
		(0.074)
Option A Value	0.128***	0.128***
	(0.002)	(0.002)
Option B Total Value	-0.076***	-0.076***
	(0.002)	(0.002)
Observations	47,200	47,200
Individuals	590	590
R^2	0.50	0.50
p-value: Main Effects of Information Equal	0.03	0.72
p-value: Interactions Equal		0.00

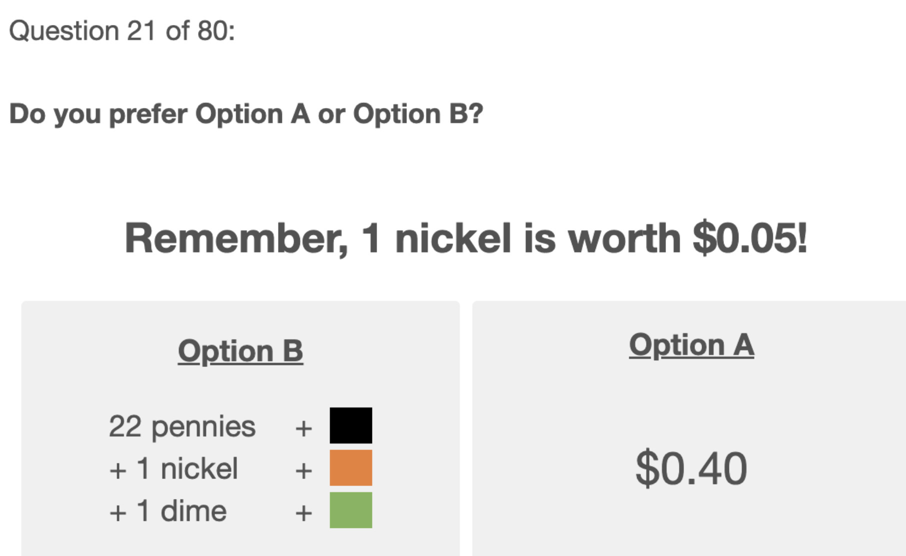
Notes: This table shows OLS regression estimates, pooling data from all Treatments and all Periods of the experiment. The dependent variable is a dummy indicating whether the participant chose Option A (which included the lottery). I regress this variable on the certain value of Option A ("Option A Value"), the sum of the values of all six of Option B's attributes ("Option B Total Value"), individual-fixed effects, and dummy variables indicating whether the participant was being reminded of the lottery, where this reminder either came without numerical information about the odds of winning ("Lottery Info without Odds") or with such information ("Lottery Info with Odds"). In Column 2, I additionally interact these dummy variables with the error (belief minus truth) in participants' previously reported beliefs about the odds of winning the lottery (winsorized at the 90th percentile). Robust standard errors, clustered at the individual level, are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

A For Online Publication: Supplementary Figures and Tables

Figure A.I: Main Experimental Task: Choosing between Options A and B

Panel 1: Example of Choice without Information

Panel 2: Example of Choice with Information



Notes: This figure gives two examples of the decision screen participants saw when choosing between Options A and B. Panel 1 is an example of a decision without any additional information being shown. Panel 2 shows a decision screen for a participant receiving information about one attribute of Option B (in this case, the number of nickels). The left-right placement of Option A vs B was randomized across participants but was constant throughout the experiment.

Table A.I: Attention at Baseline for Treatment 1 Correct-Beliefs Sample

	(1)
Option A Value	0.124***
	(0.006)
Option A Lottery	0.024***
	(0.009)
Option B Coins	0.092***
	(0.008)
Option B Boxes	0.055***
	(0.008)
Constant	0.611***
	(0.034)
Observations	2,020
Individuals	101
R^2	0.33
p-value: Option A Value = Boxes	0.00
p-value: Option A Lottery = Boxes	0.02
p-value: Coins = Boxes	0.00

Notes: This table shows OLS estimates of equation 6, except they do not include individual-fixed effects or treatment dummies. The attributes k include the certain value of Option A ("Option A Value"), the sum of the values of Option B's coins ("Option B Coins"), and the sum of the values of Option B's colored boxes ("Option B Boxes"). Because the dependent variable is whether participants chose Option B, the certain value and lottery value of Option A are multiplied by negative one so that the expected sign of all coefficients is positive. In addition, this table only includes data from participants in Treatment 1 (who received no information about any of Option B's attributes throughout the experiment) who also correctly responded to unincentivized questions at the end of the survey about the monetary value of each coin and every possible colored box. Robust standard errors, clustered at the individual level, are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A.II: Effects of Information about Target Attribute

	Full Sample			Correct-Beliefs Sample		
	Pooled (1)	Coin (2)	Box (3)	Pooled (4)	Coin (5)	Box (6)
Info	0.006	0.008	0.016	-0.001	-0.009	0.016
	(0.015)	(0.021)	(0.021)	(0.018)	(0.024)	(0.025)
Option A Value X No Info	0.128***	0.133***	0.123***	0.126***	0.131***	0.121***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Option A Value X Info	0.128***	0.133***	0.123***	0.127***	0.134***	0.121***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Option A Lottery X No Info	0.016***	0.010**	0.021***	0.019***	0.010*	0.026***
	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)
Option A Lottery X Info	0.013***	0.013**	0.013***	0.013***	0.010	0.017***
	(0.003)	(0.005)	(0.004)	(0.004)	(0.006)	(0.005)
Target Attribute X No Info	0.071***	0.095***	0.048***	0.073***	0.091***	0.054***
	(0.005)	(0.008)	(0.007)	(0.006)	(0.009)	(0.008)
Target Attribute X Info	0.119***	0.116***	0.133***	0.125***	0.117***	0.143***
	(0.006)	(0.009)	(0.008)	(0.007)	(0.010)	(0.009)
Other Changing Option B Attributes X No Info	0.074***	0.068***	0.079***	0.079***	0.073***	0.085***
	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Other Changing Option B Attributes X Info	0.069***	0.064***	0.072***	0.074***	0.064***	0.080***
	(0.003)	(0.005)	(0.004)	(0.004)	(0.006)	(0.005)
Frozen Option B Attribute X No Info	0.054***	0.052***	0.057***	0.050***	0.032*	0.067***
	(0.011)	(0.015)	(0.016)	(0.013)	(0.017)	(0.019)
Frozen Option B Attribute X Info	0.035***	0.033**	0.044***	0.031**	0.021	0.048***
	(0.011)	(0.016)	(0.016)	(0.013)	(0.019)	(0.018)
Observations	23,600	11,400	12,200	17,480	8,200	9,280
Individuals	590	285	305	437	205	232
R^2	0.346	0.356	0.343	0.355	0.358	0.361
Effect on Target	0.048	0.021	0.085	0.052	0.026	0.088
p-value: Effect on Target is Zero	0.000	0.038	0.000	0.000	0.026	0.000
Total Effect on Non-Targets	-0.039	-0.034	-0.048	-0.043	-0.041	-0.047
p-value: Total Effect on Non-Targets is Zero	0.026	0.193	0.047	0.032	0.175	0.085
Total Effect on All Attributes	0.009	-0.013	0.037	0.009	-0.015	0.041
p-value: Total Effect on All Attributes is Zero	0.615	0.641	0.159	0.667	0.636	0.183

Notes: This table shows OLS estimates of the equation 6 using the Periods 1 and 2 decisions of all treatment groups. The attributes k include the certain value of Option A ("Option A Value"), the subjective value of Option A's lottery ("Option A Lottery"), the value of the randomly selected "target attribute" of Option B, the sum of the values of the four other changing attributes of Option B, and the value of the frozen Option B attribute. Because the dependent variable is whether participants chose Option B, the certain value and lottery value of Option A are multiplied by negative one so that the expected sign of all coefficients is positive. "Info" is a dummy variable for being in Treatments 2-4, who during Period 2 received information about the value of the target attribute. Columns 1-3 include all participants, while columns 4-6 include only participants who correctly respond to unincentivized questions at the end of the survey about the monetary value of each coin and every possible colored box. The row showing the "Effect on Target" (and associated p-value) refers to the difference between the coefficient on "Target Attribute" with and without information. The row showing the "Effect on Non-Targets" (and associated p-value) first adds the coefficients for all attributes other than the target attribute (multiplying the coefficient on the other changing Option B attributes by four since there were four such attributes) and takes the difference between this value with and without information. "Total Effect on All Attributes" is analogous but also includes the target attribute. Robust standard errors, clustered at the individual level, are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

B For Online Publication: Data Appendix

Here I provide additional details on the experiment. Participants were recruited through Prolific to participate in a "Quick Survey on Decision Making." Potential survey-takers were not told anything of the content of the survey except that it was part of a research study and that it would be more difficult to complete if they struggled to tell colors apart. A total of 590 participants completed the survey during December 2022. All participants are US residents, the average participant is 41 years old, and 51% are women (recruiting was by design balanced on gender). The median respondent took 21 minutes to complete the experiment. The experiment paid a flat \$4.00 completion fee, plus any bonus that participants earned from their choices.

Participants had to correctly answer a series of comprehension checks in order to continue with the survey. If they initially answered a question incorrectly, they were forced to revise their answer before proceeding. I do not exclude anyone from the data for poor performance, but 95% of comprehension questions were answered correctly on the first try, suggesting a high level of engagement and understanding. Table B.I gives more details on these comprehension questions and provides a link to the experimental instructions.

Two differences between the preregistration and the analysis that appears in the main text bear mentioning. First, the preregistration mentions a more complicated way of testing whether information about the target attribute reduces attention paid to non-target attributes. In particular, it mentions that the survey, in addition to choosing a target attribute, also chose an "alternative" attribute. It was pointed out to me that the analysis in the main text (pooling together all attributes other than the one the information was about, rather than singling out just one) would provide both a simpler and much better-powered way of measuring the effect of target information on other attributes.

Second, the preregistration mentions a second experiment ($N = 211$) with the following differences from the experiment in the main text. First, there was no lottery associated with Option A. Second, the level of attributes was chosen such that the target attribute of Option B was always pivotal in deciding which option yielded the higher bonus. Just like in the main experiment, no one saw information in the first period. Unlike in the main experiment, in each of the remaining three periods, participants either saw no information, information about the (pivotal) target attribute, or information about a randomly selected non-target (and non-pivotal) attribute. This randomization occurred across periods and within participant.

This experiment was intended to test how the value of information depends on whether it directs attention toward pivotal or non-pivotal attributes. Table B.II shows an OLS regression where the dependent variable is an indicator for whether the participant chose the lower-value option. It regresses this variable on individual-fixed effects and indicators for whether the participant was receiving information about the pivotal target attribute and non-pivotal non-target attribute. We see that, while both types of information reduce the rate at which participants mistakenly choose the lower-value option, these effects are larger when the information is about a pivotal attribute ($p < 0.01$).

Table B.I: Comprehension Questions

	Topic	% Correct on First Attempt
Question #1	One choice is randomly chosen to be implemented	97.1%
Question #2	How bonus is calculated if Option A is chosen	87.4%
Question #3	Value of coins in Option B	98.3%
Question #4	Value of each color for top box	93.9%
Question #5	Value of each color for middle box	95.6%
Question #6	Value of each color for bottom box	96.5%
Question #7	Information is provided randomly	Not recorded*

Notes: The instructions that participants saw, along with the text of each comprehension question can be found at **this link**. *The fraction of participants who correctly answered question #7 on the first attempt was not recorded due to a coding error, so this statistic is unavailable.

Table B.II: Effects of Information about Pivotal and Non-Pivotal Information

	Mistake (1)
Information about Pivotal Attribute	-0.083***
	(0.011)
Information about Non-Pivotal Attribute	-0.046***
	(0.012)
Observations	16,880
Individuals	211
R^2	0.23
p-value: Information Effects Equal	0.00

Notes: This table shows an OLS regression where the dependent variable is an indicator for whether the participant chose the lower-value option. It regresses this variable on individual-fixed effects and indicators for whether the participant was receiving information about the pivotal target attribute and non-pivotal non-target attribute. Robust standard errors, clustered at the individual level, are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.