University of Toronto
Department of Economics

Working Paper 607

Attention and Selection Effects

By Sandro Ambuehl, Axel Ockenfels and Colin Stewart

May 25, 2018

Attention and Selection Effects

Sandro Ambuehl, Axel Ockenfels, and Colin Stewart*

May 25, 2018

Abstract

Who participates in transactions when information about the consequences must be learned? We show theoretically that decision makers for whom acquiring and processing information is more costly not only respond more strongly to changes in incentive payments for participating but also decide to participate based on worse information. With higher payments, the pool of participants consists of a larger proportion of individuals who have a worse understanding of the consequences of their decision. We conduct a behavioral experiment that confirms these predictions, both for experimental variation in the costs of information acquisition and for various measures of information costs, including school grades and cognitive ability. These findings are relevant for any transaction combining a payment for participation with uncertain yet learnable consequences.

*Ambuehl: University of Toronto, UTSC and Rotman School of Management, 105 St George Street, Toronto, ON M5S 3E6, Canada, sandro.ambuehl@utoronto.ca. Ockenfels: University of Cologne, Department of Economics, Universitätsstrasse 22a, 50923 Cologne, Germany, ockenfels@uni-koeln.de. Stewart: University of Toronto, Department of Economics, 150 St. George Street, Toronto, ON M5S 3G7, Canada, colinbstewart@gmail.com. This paper was previously circulated under the title "For They Known Not What They Do: Selection through Incentives when Information is Costly." We are grateful to Roland Bénabou, Yoram Halevy, Collin Raymond, Roberto Weber and participants at various seminars and conferences for helpful comments and suggestions. This research has been approved by the University of Toronto's Research Ethics Board in protocol 34310. Viola S. Ackfeld, Rami Abou-Seido, Ruizhi Zhu, and Jiaqi Zou provided excellent research assistance. Ockenfels gratefully acknowledges support by the German Science Foundation through the DFG Research Unit Design & Behavior (FOR 1371). Ambuehl gratefully acknowledges support through a University of Toronto Connaught New Researcher Award and the Wynne and Bertil Plumptre Fellowship at the University of Toronto Scarborough. This project has received funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (grant agreement No 741409); the results reflect the authors' view, the ERC is not responsible for any use that may be made of the information it contains. This research was supported by the Social Sciences and Humanities Research Council of Canada.

1 Introduction

In many transactions that involve uncertain consequences, ranging from credit card adoption to human egg donation, monetary payments are offered to incentivize participation. Individuals can typically reduce their uncertainty by acquiring more information about these consequences (at the cost of time and mental effort). Yet there are vast differences, across both people and choice contexts, in how difficult it is to acquire and process information. In this paper we ask: how do information costs affect the way in which people respond to payments for participation? How do changes in these payments affect who participates and the quality of information these participants' decisions are based on? Answers to these questions are relevant whenever incentives for participation concern transactions that require information acquisition, in fields as diverse as consumer choice, finance, labor economics, and welfare economics, as outlined below.

We document two main theoretical results, which we derive from a standard model of information choice (see Matějka and McKay, 2015) and test experimentally. First, when information about a transaction is more costly to acquire and process, supply responds more strongly to payments for participation. This leads to a selection effect: as the payment for participation increases, the composition of participants shifts disproportionately toward people with higher information costs. Second, those for whom information is more costly make less-informed decisions. These results are based on sophisticated information choice behavior; to verify their applicability, we test them empirically in a laboratory experiment. Our experimental findings strongly support these predictions, both for experimentally varied information cost and for several individual-specific information cost measures, including educational background and cognitive ability.

Our theoretical and experimental investigations both concern the following setting. An agent will receive a fixed, known payment if and only if she chooses to participate in a transaction. *A priori*, the agent lacks information about the consequences of participating; whether participation is optimal depends on an unknown state of the world. She decides how much and what kind of information to obtain—at a cost—before committing to a decision.

Each of our main theoretical results arises from the response in information choice behavior to changes in the payment for participation. Our first result—that higher information costs increase responsiveness to incentive payments— corresponds to the idea that individuals with lower information costs arrive at firmer views regarding whether participating is the right action for them, and are thus less susceptible to influences such as incentive payments. This can be understood intuitively by considering extreme cases of information costs. On the one hand, if information costs are low, the agent chooses to obtain precise information and selects the optimal action with high probability in each state; changes in the payment for participation therefore have little effect on behavior. On the other hand, if information costs are high, the agent obtains imprecise information, making him responsive to changes in the amount of the payment. Consequently, increasing the payment leads to a disproportionate increase in the likelihood that higher-cost types participate relative to lower-cost types. Section 2 explains this mechanism in more detail.

Existing evidence on endogenous attention and information choice is scarce and does not speak directly to our predictions (Pinkovskiy, 2009; Cheremukhin, Popova, and Tutino, 2015; Bartoš, Bauer, Chytilová, and Matějka, 2016; Ambuehl, 2017; Dean and Neligh, 2017). We conduct an incentivized laboratory experiment to test these predictions. The advantages of using a laboratory experiment over other empirical methods are twofold. First, we aim to isolate the effects of information acquisition and processing and exclude alternative mechanisms that could generate similar results. Second, evaluating partially informed choice requires knowledge of the counterfactual decisions that subjects would make based on perfect information. Neither naturally occurring data nor a field experiment would be able to offer both of these opportunities.

In the main experimental task, 584 subjects each receive a payment of €2, €6, or €10 if they choose to participate in a gamble in which they lose either €0 or €12, with equal prior probability. Hence, as in the model, subjects know the payment amount, but have imperfect information about the downside of this transaction. After learning the payment amount, but before deciding whether to participate in the transaction, subjects can examine hard-to-process information about whether they will face a net gain or a net loss from participation. Subjects are shown a list of 60 solved addition problems, such as *23 + 45 = 68*. For subjects who will gain from taking the gamble, 35 of the additions are solved correctly and 25 are solved incorrectly; for subjects who will lose, these numbers are reversed. There is no time limit, enabling subjects to determine whether they will gain or lose with whatever degree of accuracy they desire. As in our model, subjects have a great deal of freedom in choosing their information; for example, they could demand a higher level of accuracy when the evidence initially points toward participation than when it points the other way. Importantly, better information costs time and effort—and more so for some subjects than for others.

We induce experimental variation in information costs by changing the total number of addition problems in the list while keeping the proportion of correct and incorrect calculations approximately constant. The longer the list of problems, the less information one obtains from checking a single calculation. We also employ three proxies for individual-level information acquisition costs. First, and most closely related to our theoretical framework, we elicit subjects' reservation prices for checking a given number of calculations (with a punishment imposed for insufficient checking quality). Second, for a measure determined independently of our experiment, we elicit information about subjects' educational background. Third, for a personality trait that is more widely predictive outside of our specific experiment, we measure cognitive ability using Raven's matrices.

Subjects' behavior conforms to our theoretical predictions for all four of our information cost measures. First, for experimental variation in information costs, we find that the likelihood of participation responds more strongly to changes in the participation payment in higher-cost treatments. Second, for all three elicited cost measures, we observe the predicted selection effects. In particular, as the payment for participation increases, the composition of individuals who agree to take the gamble shifts toward those with a higher reservation price for checking calculations, and those with a weaker background in mathematics. Moreover, the average Raven's IQ score amongst subjects who elect to take the gamble drops significantly as the payment for participation increases.[1]

A potential alternative explanation for the selection effects in our experiment could be that they arise due to some mechanism unrelated to information costs, such as more cognitively able individuals having different risk preferences or otherwise behaving in systematically different ways. To address this concern, we conduct a control treatment that eliminates endogenous information choice but is otherwise the same as the main treatments—of the list of 60 addition problems that reveal the state of the world, subjects can only see the first 20 (which bounds information from above), and the computer reveals the number of correct and incorrect calculations amongst those 20 to them (which bounds information from below). This treatment reveals no selection effects, thereby excluding such alternative explanations and pinpointing information acquisition and processing as the mechanism underlying our results.

Our experimental results also confirm the prediction that higher information acquisition costs lead to less-informed decisions (which are more likely to cause *ex post* regret). These results obtain in all of our treatment conditions and for all of our individual-level information cost measures. Finally, we show that the average reservation price of subjects who elect to take the gamble increases more quickly with respect to the participation payment when the list of addition problems is longer, thus indicating that differences across people become magnified for transactions whose consequences are generally more difficult to comprehend.

That our results hold both theoretically and experimentally suggests that the same mechanism will arise in other settings, and that the empirical results are not likely to be a mere artifact of a fortuitous choice of parameters in our experiment.

Applications extend to several subfields of economics. In the context of finance, our results suggest that an increase in expected returns will, *ceteris paribus*, lead to a disproportionate inflow of less-informed traders. In a labor context, our results suggest that larger signing bonuses will attract a different selection of workers; these workers will be more likely to regret having accepted the job as their decisions will, on average, be less informed. Finally, consider a take-it-or-leave-it offer to buy a good with two attributes, quality and price. A price is simply a negative payment, and is directly observable. Suppose quality must be learned through costly inspection. Our model and experimental results indicate that if quality becomes more difficult to observe, customers will respond more strongly to variation in price (unless the price is so extreme that consumers completely forego inspection of

[1] This statement applies as long as we do not incentivize performance on the Raven's test. Duckworth et al. (2011) present evidence that unincentivized scores are better predictors of economic outcomes.

quality altogether). We thus speak to a literature that studies how attention-related effects such as salience influence consumer choice (Kőszegi and Szeidl, 2012; Bushong, Rabin, and Schwartzstein, 2015; Bordalo, Gennaioli, and Shleifer, 2012, 2013).

Our results are also of interest regarding transactions for which payments are constrained by laws and regulations, such as human tissue donation, gestational surrogacy, and medical trial participation (Roth, 2007; Ambuehl, 2017). While we take *no normative stance*, a significant proportion of both professional ethicists (Faden and Beauchamp, 1986; Satz, 2010; Kanbur, 2004) and survey participants from the general population (Ambuehl and Ockenfels, 2017) insist that the decision to participate in such transactions should adhere to the principles of informed consent (DHEW 1978, The Belmont Report). According to these principles, the decision to participate in a transaction is ethically sound if it is made not only voluntarily, but also in light of all relevant information, properly comprehended. This information is understood to encompass both objective consequences and subjective well-being (Faden and Beauchamp, 1986), the idiosyncratic nature of which renders mere provision of information about typical consequences insufficient.[2] Our results show that payments for participation can be in direct conflict with the understanding that participants have about the consequences of participation. Further, the severity of this conflict grows with respect to both the amount of the payment and the difficulty of acquiring and processing information about the consequences of the transaction.

There are alternative mechanisms that can generate selection effects related to information. For instance, in a population with heterogeneous priors and no information acquisition, raising the payment for participation would lead to a selection of subjects with increasingly pessimistic priors. Unlike our model, this alternative predicts neither systematic selection based on persistent personality characteristics, nor systematic differences in the magnitude of the selection effect across contexts.[3] Another alternative mechanism consists of people drawing conclusions from the payment amount *per se*, for instance by making the transaction appear suspicious (Kamenica, 2008; Cryder et al., 2010). Depending on how a propensity for such inferences correlates with information acquisition costs, it could exacerbate or attenuate the mechanism we document. Finally, economic inequality is an independent source of selection effects that may be correlated with information. We discuss this mechanism in Section 5.

Our paper contributes to three main strands of literature. First, our work derives and tests important implications of a standard model of costly attention (see Caplin (2016) for a survey). Its mechanism is related to Ambuehl (2017), which shows that costly information acquisition makes rational Bayesians appear as though they were engaging in motivated reasoning, because higher par-

[2]Another issue in the definition of informed consent lies in what constitutes proper comprehension. Faden and Beauchamp (1986) maintain that "there must sometimes be an extrasubjective component to the knowledge base necessary for substantial understanding," but are intentionally imprecise about the minimal requirements for substantial understanding, claiming that "[a]ny exact placement of this line risks the criticism that it is 'arbitrary,' . . . and controversy over any attempt at precise pinpointing is a certainty."

[3]Selection in this alternative model relies on the absence of information acquisition. Appendix A.4 examines an extension of our model with heterogeneous priors, and shows that the effect of information acquisition tends to dominate over the effect of the heterogeneity in the priors.

ticipation payments cause them to skew their search for information in a way that increases their likelihood of participating. The selection effect we document here derives partially from the fact that such motivated reasoning is more pronounced when information costs are higher. We thus add to an emerging literature that explores the informational foundations of phenomena documented in behavioral economics, such as Woodford (2012a,b); Steiner and Stewart (2016) (probability weighting and prospect theory), Steiner, Stewart, and Matějka (2017); Dean, Kıbrıs, and Masatlioglu (2017) (status quo bias), Azfar (1999); Gabaix and Laibson (2017) (hyperbolic discounting), and Kőszegi and Matějka (2017) (mental accounting).

Second, we contribute to the burgeoning literature on repugnance as a constraint on markets. (Kahneman, Knetsch, and Thaler, 1986; Basu, 2003, 2007; Roth, 2007; Leider and Roth, 2010; Ambuehl, Niederle, and Roth, 2015; Elias, Lacetera, and Macis, 2015a,b, 2016; Ambuehl, 2017; Clemens, 2017; Exley and Kessler, 2017). Ambuehl and Ockenfels (2017) explicitly study individuals' ethical intuitions relating to the mechanisms we document in the current paper.

Third, by exploring how the effects of participation payments vary with personality characteristics, we also contribute to the literature on personality psychology and economics (Almlund, Duckworth, Heckman, and Kautz, 2011; Fréchette, Schotter, and Trevino, 2017), specifically traits related to motivation and cognitive ability (Benjamin, Brown, and Shapiro, 2013; Segal, 2012; Dohmen, Falk, Huffman, and Sunde, 2010; Borghans, Meijers, and Ter Weel, 2008; Burks, Carpenter, Goette, and Rustichini, 2009; Agarwal and Mazumder, 2013).

Importantly, although our theoretical predictions derive from a rational model of costly attention, our empirical results are orthogonal to the literature studying whether people reach the appropriate level of subjective certainty given the data they observe (Peterson and Beach, 1967; Tversky and Kahneman, 1974; El-Gamal and Grether, 1995; Holt and Smith, 2009; Ambuehl and Li, 2018; Buser, Gerhards, and van der Weele, 2016). What matters for our purposes is solely the action subjects take based on their information. Whether they are, at the point of action, more or less certain about the state of the world than Bayes' law implies they should be has no bearing on our behavioral results.[4]

The remainder of this paper proceeds as follows. Section 2 derives the theoretical predictions. Section 3 introduces the experiment design, and Section 4 presents the empirical findings. Finally, Section 5 suggests policy implications and discusses the scope and generalizability of our findings.

2 Theory

Setting An agent decides whether or not to participate in a transaction in exchange for a payment m. The agent is uncertain about the (utility) consequences of participation, which depend on an unknown state of the world $s \in \{G, B\}$. The state is good ($s = G$) with prior probability μ, and bad

[4] Section 4.5 does examine the relationship between objective posterior probabilities and subjective posterior beliefs in our experiment.

($s = B$) with the remaining probability $1 - \mu$. If the agent participates and the state is s, she obtains utility π_s. If she does not participate, she obtains utility 0. We assume $\pi_G + m > 0 > \pi_B + m$, making the choice problem nontrivial for the agent.

Before the agent decides whether or not to participate, she can acquire information about the state. Instead of placing restrictions on the kind of information the agent can acquire, we allow—as is typical in the rational inattention literature—for the agent to choose *any* information structure to learn about the state, with different structures incurring different costs.[5] (These costs can be psychological, physical, or some combination thereof.) For example, structures that provide more precise information have higher costs. Modeling information acquisition in this way captures the idea that there are many possible learning strategies varying not only in their precision but also in exactly how information depends on the state. The agent could, for example, choose to look for information that, if found, would strongly indicate that the state is good, but if not found would leave her quite uncertain; or she could similarly try to confirm the bad state (or both). Thus the agent can choose both how much and what kind of information to acquire.

More specifically, there is a fixed set of possible signal realizations (containing at least two elements), and the agent chooses the distribution of signals in each state of the world. As in much of the rational inattention literature, we assume the cost of information is proportional to the expected reduction in the Shannon entropy of the agent's belief about the state due to observation of the signal. The use of information costs proportional to reduction in entropy makes the model analytically tractable and allows us to draw on the characterization of the solution in Matějka and McKay (2015). We have verified numerically that our results also hold for a number of other cost functions; see Appendix B for details.

A strategy for the agent—which combines the information choice with the choice of an action for each signal realization—amounts to choosing the probability of participation in each state (Matějka and McKay, 2015). Under this interpretation, the cost of information is based on the difference in entropy between the prior belief μ and the posterior belief conditional on the agent's action; this is the cost associated with the least expensive information structure for implementing this strategy. Letting p_s denote the probability of participation in each state $s \in \{B, G\}$, the agent's posterior belief that the state is good is $\gamma_{\text{part}} := \mu p_G / \big(\mu p_G + (1-\mu) p_B\big)$ when she participates and $\gamma_{\text{abst}} := \mu(1-p_G)/\big(\mu(1-p_G) + (1-\mu)(1-p_B)\big)$ when she does not. The information cost associated with the strategy (p_G, p_B) is therefore proportional to

$$c(p_G, p_B) := h(\mu) - p h(\gamma_{\text{part}}) - (1-p) h(\gamma_{\text{abst}}),$$

[5]That the agent can acquire perfect information does not mean that the model only applies to cases in which the consequences of the transaction can be known for sure. Instead, the states should be interpreted as capturing all there is to know about the consequences: any uncertainty that cannot be reduced by further information acquisition can be incorporated into the states of the world. In this interpretation, π_G and π_B represent expected utilities from participation conditional on the best available information.

where $p := \mu p_G + (1-\mu)p_B$ is the *ex ante* probability of participation and $h(\gamma) := \gamma \log \gamma + (1-\gamma)\log(1-\gamma)$ is the entropy associated with belief γ.

The agent chooses (p_G, p_B) to maximize her expected utility

$$U(p_G, p_B; m) = \mu p_G(\pi_G + m) + (1-\mu)p_B(\pi_B + m) - \lambda c(p_G, p_B), \tag{1}$$

where $\lambda > 0$ is an information cost parameter. Let $\big(p_G(m,\lambda), p_B(m,\lambda)\big)$ denote the solution to this problem and let

$$p(m,\lambda) = \mu p_G(m,\lambda) + (1-\mu)p_B(m,\lambda)$$

be the corresponding *ex ante* participation probability. We refer to $p(\cdot, \lambda)$ as type λ's *supply function.*

Our model, like other rational inattention models, does not explicitly specify the source of the information cost. Costs could be incurred for acquiring, processing, or interpreting information, or some combination thereof; the exact source of this friction is behaviorally irrelevant. Similarly, uncertainty about the state of the world has several possible interpretations. In particular, it may capture risk that is idiosyncratic to the agent, including uncertainty about her own preferences.

The underlying assumption that the agent can choose *any* information structure merits discussion. One natural interpretation is that the agent acquires information over time according to a process by which he continuously updates his belief. The choice of p_G and p_B then corresponds to choosing threshold beliefs at which to stop learning and choose an action; thus, for example, a high threshold belief for participation corresponds to a small value of p_B. Morris and Strack (2017) show that optimal sequential learning is behaviorally equivalent to optimal choice in a rational inattention problem with binary states.[6]

Analysis Before we state our formal results, it is instructive to examine an example of the supply curves for different information cost parameters. Panel A of Figure 1 shows two such curves, for $\lambda = 0.1$ and $\lambda = 0.3$, with parameters $\mu = \frac{1}{2}$, $\pi_G = 0$, and $\pi_B = -1$. The participation probability of the high-cost type becomes positive only once the payment m crosses a lower threshold, which is higher than the corresponding threshold for the low-cost type. As long as the participation probabilities are strictly between 0 and 1, however, the high-cost type's probability responds more strongly to changes in the payment than does that of the low cost type. We also plot the proportion of high-cost types among those who choose to participate under the assumption that each of the types forms half of the total population. The proportion of high-cost types steadily increases with the payment until the high-cost type participates with probability 1.

The following proposition shows that these observations hold for general parameter values.

Proposition 1.

[6] Earlier work by Hébert and Woodford (2017) identifies a related connection.

(i) Suppose λ and m are such that $0 < p(m, \lambda) < 1$. Then

$$\frac{\partial}{\partial \lambda}\left[\frac{\partial p(m,\lambda)}{\partial m}\right] > 0.$$

(ii) Suppose λ is continuously distributed with support on some interval $[\underline{\lambda}, \overline{\lambda}]$ with $0 \leq p(m, \lambda) < 1$ for all $\lambda \in [\underline{\lambda}, \overline{\lambda}]$ and $p(m, \lambda) > 0$ for some $\lambda \in [\underline{\lambda}, \overline{\lambda}]$. Then for any increasing function $f : \mathbb{R} \longrightarrow \mathbb{R}$, $E[f(\lambda) \mid \text{participate}]$ is increasing in m.

Proposition 1 captures, in two different ways, the idea that increases in the payment m disproportionately affect those with higher information costs. While increasing the payment increases the likelihood of participation for any given type, the slope result in part (i) of the proposition says that this effect is stronger for higher-cost types. The selection result in part (ii) relates to applications more directly, showing that the composition of the pool of participants shifts toward types with higher costs as the payment increases.[7]

Note that the selection result applies as long as m is not so high that some type participates without acquiring any information. Unlike the slope result, which requires that the agent has an interior participation probability, the selection result allows for some types to abstain with certainty.

While the two parts of Proposition 1 are related, neither implies the other. Varying the cost parameter not only causes the slope effect identified in part (i), but also causes a level effect that may countervail the slope effect in terms of the composition of the pool of participants. The proofs of each part, which may be found in Appendix A, make use of the characterization of optimal choice behavior in Matějka and McKay (2015). In our model, their characterization leads to an explicit expression for the participation probability, which we can differentiate and sign. Part (ii) requires additional steps to handle the full distribution of types as well as the level effect noted above.

To gain some intuition for the result, consider the effect of marginal changes in the payment m on types that differ in the value of their information cost parameters. For types with very low cost, an increase in the payment has little effect: the agent obtains a precise signal, which makes her very likely to participate in the good state and abstain in the bad state. For types with very high cost, the decision to participate is necessarily based on limited information, making the agent responsive to changes in the payment. Similarly, intermediate types obtain partial information, leaving them somewhat responsive to changes in the payment, though less so than high-cost types. This intuition, though simple, neglects a crucial feature of the model: the probability of participation changes only if—and to the extent that—the agent changes her choice of information. It is this choice that responds to the change in payment m. As m increases, the gain from participation in the good state increases and the loss in the bad state decreases. Hence, the agent needs to be less convinced that the state is good in order to participate, and more convinced that the state is bad in order to abstain. By

[7]Equivalently, part (ii) shows that an increase in the payment m leads to a first-order stochastic dominance increase in the cost parameters of those agents who elect to participate.

choosing her information accordingly, she increases her probability of participating in both states, with a larger effect when information is less precise (and hence for higher-cost types).

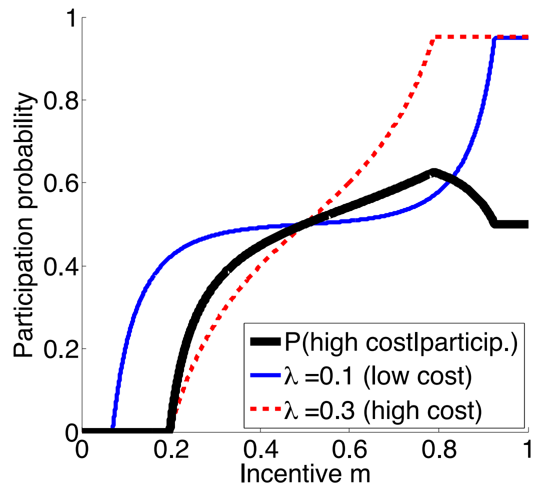


Figure 1: Supply curves predicted by the model with $\pi_G = 0$, $\pi_B = -1$ and $\mu = 0.5$. The proportion of participants having high costs is increasing up to the point at which the high-cost type participates with probability 1.

The next proposition shows that higher-cost types will make less well-informed decisions, and are thus more likely to regret their choices *ex post*. Let $\gamma^*_{\text{part}}(\lambda, m)$ and $\gamma^*_{\text{abst}}(\lambda, m)$ denote, for type λ, the posterior beliefs that the state is good when she chooses to participate and to abstain, respectively. Higher-cost types decide based on less information: both posterior beliefs become closer to the prior belief as the cost parameter increases. Since $\gamma^*_{\text{part}}(\lambda, m)$ is the probability that participating is the correct decision (conditional on type λ participating), a lower value of $\gamma^*_{\text{part}}(\lambda, m)$ corresponds to a higher likelihood of regret.

Proposition 2. *Suppose λ and m are such that $0 < p(m, \lambda) < 1$. Then $\frac{\partial}{\partial \lambda}\gamma^*_{\text{part}}(\lambda, m) < 0$ and $\frac{\partial}{\partial \lambda}\gamma^*_{\text{abst}}(\lambda, m) > 0$.*

The proof of this result is based on the concavification approach to rational inattention developed in Caplin and Dean (2013). The assumption that costs are proportional to the reduction in entropy is not necessary for this result: the proof immediately extends to the much larger class of posterior separable cost functions described in Caplin and Dean (2013).

The intuition for this result is straightforward. Whenever information is more expensive to acquire and process, it is optimal, *ceteris paribus*, to acquire and process less of it.

The magnitude of the effects identified in Proposition 1 depend on the context and, in particular, the difficulty of the information acquisition problem. The following result identifies a sense in which the magnitudes are larger in more opaque contexts (where acquiring information is more difficult for all types). More precisely, as we scale up the cost of information by some factor, the cross derivative of the participation probability with respect to m and λ increases.

Proposition 3. *Suppose λ and m are such that $0 < p(m, \lambda) < 1$. Then*

$$\frac{\partial}{\partial a}\bigg|_{a=1} \left[\frac{\partial}{\partial m}\frac{\partial}{\partial \lambda}\, p(m, a\lambda)\right] > 0.$$

A restatement of this result illuminates the (incomplete) intuition: individual differences lead to less pronouncedly different responses to payments for transactions for which information costs are lower. If the information costs approach zero, so do all agents' probabilities of making a suboptimal choice. Accordingly, no agent's behavior can respond much to changes in the payment in either state of the world, regardless of her individual-specific information cost parameter. Therefore, the slopes of the supply curves converge across the different types of agents.

Discussion and robustness Our results are robust to various extensions and alternatives that the above analysis abstracts from.

Risk aversion. Our model is presented based on the assumption of risk neutrality. A careful inspection of the proofs shows that they generalize to the case of agents who share the same nonlinear utility function u for money that is additively separable from the cost of information acquisition, so that the agent's expected utility is now given by $U(p_G, p_B; m) = \mu p_G u(\pi_G + m) + (1-\mu) p_B u(\pi_B + m) - \lambda \cdot c(p_G, 1 - p_B)$. If risk preferences are heterogeneous, however, they could both reinforce or countervail our results, depending on the correlation between risk preferences and cost of information acquisition.

Information cost function. Appendix B.1 displays the results of numerical simulations with alternative information cost functions. We have found no counterexamples for any cost function that is separable in posterior beliefs, such as Tsallis entropy, or the expected waiting time in the Wald (1947) sequential information acquisition problem. (For the class of Renyi-entropy cost functions, however, we have found isolated deviations.) Nor have we found counterexamples in a related model in which agents choose the precision of a normally distributed signal about the state; see Appendix B.2.

Heterogeneous priors. Our results are also robust to heterogeneity in prior beliefs, as long as all types have an interior participation probability. In fact, the probability that an agent with cost parameter λ participates depends only on the mean prior amongst all agents with that cost of information acquisition. By implication, all our comparative statics results on p generalize to the case of heterogeneous priors.[8]

Alternative interpretation. We have hitherto interpreted our setting as one with a known incentive payment and uncertain utility consequences of participation. Other interpretations are possible. In-

[8] See Appendix A.4 for a formal statement. Observe that this statement concerns the case in which some subjects truly face different priors than others (for instance due to idiosyncratic variation in risk), not the case of misperceptions about the data generating process.

deed, the main driver of our model is not the assumption that there is one activity with a safe payoff and another with an uncertain payoff. Instead, the relevant characterization is that a higher payment raises the payoff of one activity versus another in every state of the world. This holds regardless of the riskiness of each option.

3 Experiment design

We conduct an experiment to test the empirical validity of the predictions derived in the previous section. Existing empirical evidence does not address this question directly. We test the implications of the model rather than its primitives because it is the implications that are of substantive interest in applications. In particular, our experiment is not designed to draw conclusions about the extent of rationality, the form of the information cost function, or other primitives of the model.

In addition to varying information cost experimentally, we aim to link the theoretical predictions to subjects' personal characteristics, such as cognitive ability. Thus, we choose a real-effort information processing task in which subjects' psychological costs depend on their own preferences, skills, and perseverance. Either source of variation can be consistent with the theory in that the cost can be interpreted as arising from acquisition, processing, or interpretation of information.

Task Subjects decide whether to take a gamble in which they receive π_G if the state is good, or π_B if the state is bad. In exchange for taking the gamble, they receive a payment m, regardless of whether they win or lose—but only if they take it. The prior probabilities of the states are 50/50. Before deciding whether to take the gamble, but after learning the value of m, subjects obtain information about the state of the world in a way that is perfectly revealing, but costly to interpret. Specifically, they see a list of calculations as in panel A of Figure 2. The list comprises n two-digit addition problems with proposed solutions. If the state is good, k are solved correctly and $N - k$ are solved incorrectly. If the state is bad, the numbers of correct and incorrect solutions are reversed. Subjects are aware of this setting, and can examine each such list for as long as they desire.

This task affords subjects a large opportunity set of state-dependent stochastic choice probabilities. The more calculations a subject checks, the better is her information about the state of the world, and by extension, the payoff-maximizing betting decision. Moreover, subjects in our experiment can skew their information acquisition by searching more intensely if the initial calculations they have checked suggest they would lose than if they suggest they would win, similar to a researcher who scrutinizes criticisms of her work but readily accepts praise. There are many alternative approaches through which subjects may shape their information acquisition. They may choose how carefully to check any given addition, and which ones to check (perhaps attempting to check easier ones first). The information cost subjects incur depends on the approach they take.

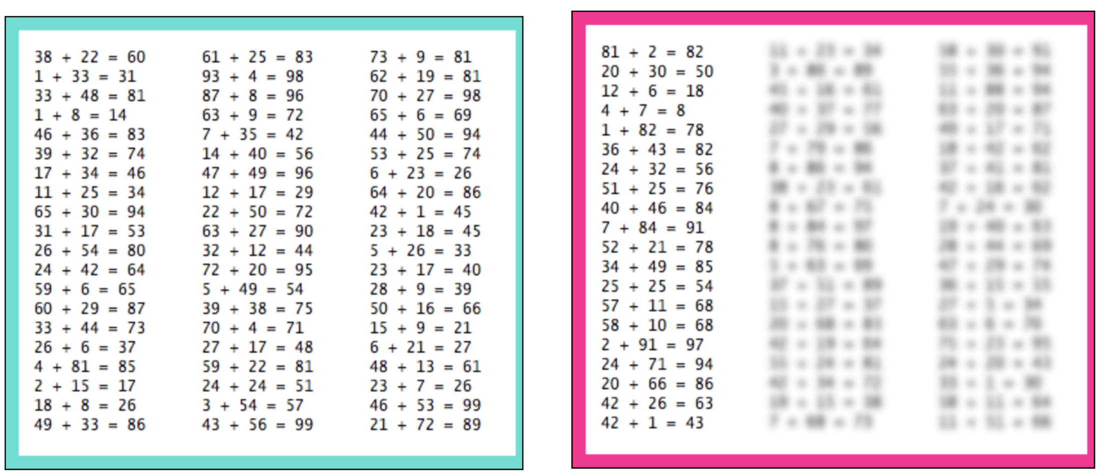
This task is suitable for testing our theoretical predictions as it satisfies the following four criteria.

First, it allows us to experimentally vary the cost of information acquisition. We do so by varying the number of calculations in a picture. By increasing the list length, and keeping the fraction of correct / incorrect calculations approximately constant, we ensure that checking any given calculation reveals less information about the state, thus making information acquisition more costly.

Second, it is plausible that individuals differ both in their ability and their willingness to extract information from a list of calculations. This generates natural variation in information acquisition costs. We measure this variation directly by eliciting subjects' reservation price to check a given number of calculations, by eliciting information about their choices and performance in school, and by testing their cognitive ability.

Third, the theoretical setting rests on the assumption that subjects can choose from a rich set of information structures that can be tailored to the specifics of the choice problem. It would not apply, for instance, if subjects can only choose to either acquire or forfeit a single piece of information about the state. In contrast, our task is designed to afford subjects this kind of rich information acquisition choice, as explained above.

Fourth, it is unlikely that our results are driven by subjects' reliance on some domain-specific, specialized cognitive system such as visuospatial perception.[9] This increases the number of settings to which our results plausibly apply.



(A) (B)

Figure 2: Panel A depicts the presentation of information about the state of the world in the main treatments (60 calculations). Panel B depicts the presentation for the *fixed information* treatment; in this treatments, subjects are explicitly told the number of correct and incorrect calculations in the visible part of the picture.

[9] Visuospatial perception occurs in a highly specialized, clearly localizable part of the brain with capacities that may be absent from other parts of the cognitive system. Experiments related to attention that rely on visuospatial perception are discussed, for example, in Woodford (2012a) and in Dean and Neligh (2017).

Treatments We set $\pi_G = 0$, $\pi_B = -12$, and vary the payment $m \in \{2, 6, 10\}$ in the *low*, *medium* and *high incentive* treatments, respectively. (All amounts are denominated in euros.) Note that for $m \leq 6$, any risk-averse subject who bases her participation decision on the prior alone would reject the gamble.

Our three *information cost* treatments vary the level of difficulty for information acquisition. The *low cost* treatment has 25 addition problems, of which 60% are correct (incorrect) in the good (bad) state; the *medium cost* treatment has 60 addition problems with 58.3% correct (incorrect) in the good (bad) state; and the *high cost* treatment has 100 addition problems with 55% of calculations correct (incorrect) in the good (bad) state.[10]

The *fixed information* treatment is an important control. In this treatment, subjects are shown a picture similar to that in the main treatments, but only part of it is visible, with the rest heavily blurred, as shown in panel B of Figure 2. A line of text above the picture explicitly informs the subject how many correct and incorrect calculations the visible part contains. Further examination of the picture would reveal no additional information. This effectively eliminates the possibility of, and thereby the costs associated with, endogenous information acquisition. This treatment allows us to demonstrate that our effects indeed arise due to differential information acquisition, and not merely because different people tend to draw different conclusions from the same set of stochastic information.[11] We fix the difference between the number of correct and incorrect calculations in the visible part of the picture such that, in a picture with a total of 60 expressions, they see 20 of which either 11 or 13 are correct (incorrect) in the good (bad) state. The associated Bayesian posterior beliefs are $P(s = good|11 \text{ correct}, 9 \text{ incorrect}) = 72.6\%$ and $P(s = good|13 \text{ correct}, 7 \text{ incorrect}) = 94.9\%$.

Each subject participates in 18 rounds of decision making in an individually randomized order, as summarized in Table 1. We anticipated that in the low incentive treatments subjects would likely refuse to take the gamble in the majority of cases. Hence, to obtain adequate statistical power, we oversampled these decisions. Subjects know that their earnings are determined by at most one randomly selected round.

After each of the 18 rounds, subjects indicate their subjective posterior belief that they have seen a good-state picture, incentivized by the mechanism proposed in Karni (2009) and Holt and Smith (2009), in which they may either win or lose €3. Subjects know from the start that there is an 80% chance that they will be paid according to one decision in one of these 18 rounds. They also know that in this case, there is an 80% chance that the selected decision will be a betting decision, and a 20% chance that it will be a belief elicitation decision, and never both. We chose to put the lion's share of the probability mass onto incentivizing the betting decision to ensure that it would be the main driver of information acquisition.

[10]In sessions 2, 3, and 4, the low-cost treatment used 30 calculations per picture, with 60% correct (incorrect) in the good (bad) state, and session 1 had 20, also with 60%.

[11]It is conceivable, for instance, that more mathematically inclined people would deviate from Bayesian updating to a lesser extent.

Information	Endogenous			Fixed
Difficulty	25	60	100	20 visible
Participation payment				
€ 2	2	2	2	2
€ 6	1	1	1	2
€ 10	1	1	1	2

Table 1: Type and number of decisions taken by each subject. Treatments were displayed in individually randomized order. States and pictures were drawn independently for each individual. For the fixed information treatments, the visible part of the picture contained either 11 or 13 of the majority type (correct or incorrect) of solution, and 9 or 7 of the minority type.

Individual measures After subjects complete the first part of the experiment, we elicit three proxies for individual-level information costs.

Reservation price for checking calculations. As a direct measure of information acquisition costs, we elicit subjects' reservation price for the opportunity to assess n addition problems for correctness in exchange for an additional payment, for each $n \in \{30, 60, 100, 200\}$. Subjects know that if they agree to check n calculations in exchange for money, and this decision is randomly selected for implementation, then they need to check at least 90% of them correctly. Otherwise, they not only lose the money they would have obtained for completing the task correctly, but also forfeit another €10 from their completion payment. For each value of n, a subject sees a separate list, and decides, on each line, whether to check the calculations in exchange for €p. In each list, p ranges from 0 to 10 in steps of 0.5, and also includes 0.25 and 0.75. Subjects are informed that one of these decisions will be selected for implementation in addition to the chosen decision from the main stage of the experiment.[12]

Educational background. Second, we elicit information about subjects' educational background in both mathematics and German literature. One reason for this is that eliciting educational background on two unrelated subjects helps us demonstrate how the effects we document relate to the costs of acquiring the information specific to our tasks. We expect that subjects' background and performance in mathematics will have predictive power for information costs, whereas background and performance related to German literature will not. Specifically, for both mathematics and German literature, we elicit high-school grades, as well as whether the subject has taken an honors class in the subject.

[12] We chose to disburse this payment in addition to other payments to make the experiment simpler to understand for subjects. While this design choice could in principle lead to income effects, those would countervail our hypothesis. Our first prediction, for instance, maintains that subjects with higher information acquisition costs will respond more strongly to the payment for taking the gamble. Accordingly, we predict a *positive* relationship between reservation prices for checking calculations and responsiveness to participation payments. If income effects were dominant, we would expect the opposite: If our hypothesis is true, then someone who has paid more attention in the main part of the experiment will be less responsive to participation payments, and will expect a higher payment from that stage. Income effects predict a lower marginal utility of money for such a person. This would reveal itself in a higher reservation price for checking a given number of calculations. Accordingly one would expect an attenuated, or even a *negative* relationship between reservation prices for checking calculations and responsiveness to incentive payments.

Additionally, we elicit whether subjects are currently enrolled in a STEM college major.[13] Another reason for eliciting educational background is that our other measures of attentional cost (reservation prices and Raven's matrices) are elicited during the same two hours in which a subject participates in the experiment, raising the possibility that variation in these costs may simply be caused by transient reasons such as having had a particularly good night's sleep. The variables relating to educational background, by contrast, have been determined over a long time frame.

Raven's matrices. Third, we measure cognitive ability. This is of interest because cognitive ability has been related to many different life outcomes. We use series I and the first 24 matrices of series II of Raven's Advanced Progressive Matrices (Raven, Raven, and Court, 1962). Each matrix in this test consists of 3 rows and 3 columns, and all but one of the cells display some geometrical figure. The test taker must then infer a rule about how items in columns and rows relate to each other and then select a candidate for the missing piece from 8 options. The test starts with simple geometric shapes and gradually increases in difficulty. We expect this standard measure of cognitive ability to be related to the cost of information acquisition in our decision tasks, as it is indicative of abilities like concentration and short-term memory. This measure is, however, less directly related to the experimental task than the elicitation of reservation prices for checking calculations. Correspondingly, we expect a weaker association.

Previous research has shown that measures of cognitive ability are predictive of different outcomes depending on whether subjects are incentivized for performance (Segal, 2012; Duckworth, Quinn, Lynam, Loeber, and Stouthamer-Loeber, 2011; Borghans, Meijers, and Ter Weel, 2008; Dessi and Rustichini, 2015). To explore this dependency, we perform two separate treatments. The *unincentivized IQ* condition corresponds to the fashion in which this test is normally administered: subjects are not given incentives for performance. In the *incentivized IQ* condition, there is a 10% chance that a subjects' payment from the experiment may be determined entirely by their performance in this test. In that case, she is paid €0.30 for each correctly solved matrix.

Risk preferences. As a control variable, we also elicit subjects' risk preferences. We use lists of decisions to elicit certainty equivalents of various gambles. Each decision is of the form *Win* €X *with chance* p *and lose* €Y *with chance* $1-p$ versus *win / lose* €Z *with certainty.* The structure of these decisions is the same as in our main treatments in which subjects also decide between a gamble and a certain payment. The lotteries we present are win 2 / lose 10, win 6 / lose 6, and win 10 / lose 2 with winning probabilities $p \in \{0.5, 0.75, 0.9\}$, resulting in a total of 9 lists. On each list, the certain option varies from *lose €10 with certainty* to *win €0 with certainty* in steps of €1.[14] Subjects in the *unincentivized IQ* and *incentivized IQ* conditions know from the start of the experiment that their

[13]We elicit subjects' current college major, and then classify them into STEM / non-STEM. We also elicit subjects' high school GPA. Because this is an average over many classes, including some that are relevant, and many that are presumably irrelevant to our task, we have no *ex ante* expectation.

[14]We enforced single switching, but subjects had to make an active choice on each line of each price list.

payment is determined by a risk preference elicitation question with a 20% or a 10% probability, respectively.

Implementation and payment Subjects learn that the experiment has three parts, two "decision making parts", labelled "A" (main tasks) and "B" (reservation price and risk preference elicitation), as well as a part involving "logical puzzles" (the Raven's matrices) they will complete in between. All gains are added to a budget of €15 and all losses are deducted. Subjects read all instructions on screen, and have to complete a comprehension check before they can move on to the decision-making part.[15] States of the world are drawn randomly and are i.i.d., and lists with correct and incorrect calculations are generated randomly on an individual level. To clearly delineate the rounds from each other, each list of calculations has a differently colored border, with colors randomly assigned on an individual level. Subjects then decide whether they want to "bet on the [color] picture". In an effort to minimize confusion, we presented subjects with a choice of taking a win $(\pi_G + m)$ / lose $|\pi_B + m|$ gamble, as opposed to offering them m to take a win π_G / lose $|\pi_B|$ gamble.[16] Appendix D.3 contains the experimental instructions and screenshots of the interface.

4 Experiment results

We ran the experiment with a total of 584 student subjects across 19 sessions in May and July 2017 at the University of Cologne's Laboratory for Economic Research.[17] Subjects spent about one and a half hours on the experiment, on average, for which they received an average total payment of €18.70. Subjects were permitted to leave as soon as they completed the experiment, regardless of whether others were still working on the experimental tasks. As our analyses will demonstrate, they paid attention to the stimuli.[18]

The experiment includes three different levels of information costs. In order to run simple interactions (as opposed to using dummy variables for each cost level), we assign a cardinal value to each treatment. For simplicity, we weigh each cost condition equally, and thus assign cost indices 1, 2, and 3 to pictures with 25, 60, and 100 calculations, respectively. To show comparisons that are independent of this assignment, we also display estimated coefficients involving comparisons between only two cost levels.

The experiment involves randomly drawing states of the world. Rather than simply relying on the law of large numbers and averaging across these states, we run weighted regressions such that in each

[15]Subjects must answer all of 12 true-false questions correctly, and hence are highly unlikely to pass the check by mere guessing.

[16]Hence, in the €2 incentive treatment, for instance, subjects would decide whether they want to participate in a win €2/ lose €10 gamble.

[17]We had obtained 300 subjects in May, and then decided to replicate the findings by roughly doubling the sample size. Appendix Table D.7 lists the details of each session. Before conducting any of the laboratory sessions, we had conducted two pilot studies on Amazon Mechanical Turk with largely similar results, which are available from the authors by request.

[18]Appendix D.2 analyzes order effects.

relevant cell, the weighted fraction of decisions for which the state is good *exactly* equals the prior of 50%.[19] Additionally, we include order and session fixed effects in all analyses. Details about the regression specifications are in Appendix C.

In Sections 4.1, 4.2, and 4.3, we study the empirical evidence for the three propositions, beginning with selection. We first use reservation prices for checking additional calculations as a measure of individual-specific information costs, as it is most directly associated with our experimental task. Section 4.4 then repeats the analyses using educational background and measures of cognitive ability as alternative measures of individual-specific information costs. Finally, Section 4.5 examines the relationship between subjective beliefs and objective posterior probabilities.

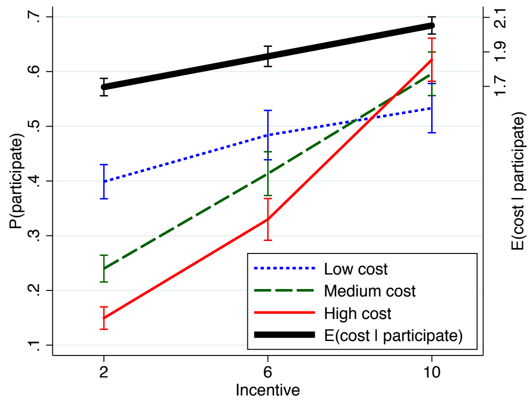
4.1 Selection

We begin by testing Proposition 1. Do higher payments for participation in the transaction lead to selection toward participants with higher information acquisition costs, as predicted by our model? Our data robustly confirm this prediction, both with experimentally induced variation in information costs (our *information cost* treatments), as well as with reservation prices for checking additional calculations as an individual-level measure of information acquisition costs. We also find robust evidence that higher information acquisition costs induce a more pronounced supply response, as predicted in part (i) of the proposition.

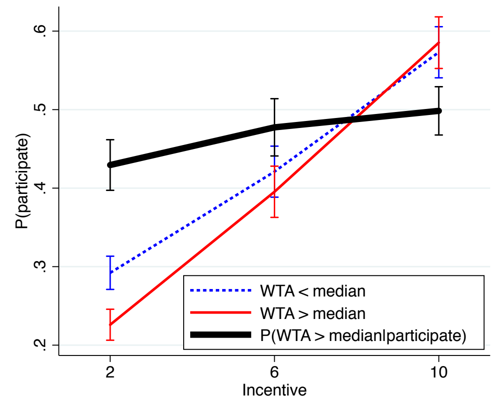
Experimental variation in information acquisition costs The solid bold line in Panel A of Figure 3 shows how the composition of information costs changes among subjects who accept the gamble as the payment increases from €2 to €8. It displays the average information cost conditional on the subject accepting the gamble, assigning numbers 1, 2, and 3 to the low, medium, and high-cost treatments, respectively. The slope of that curve is positive. The average information cost amongst subjects who choose to take the gamble is 1.7 for the low-incentive condition and increases to 2.05 in the high-incentive condition. Hence, consistent with our main prediction, as the participation payment increases, a subject who decides to participate in the gamble is more likely to come from a treatment in which information acquisition is more difficult. Additionally, the graph displays the supply curves for each cost level in our three *information cost* treatments, with the payment displayed on the horizontal axis. The supply curve is clearly steeper in the higher-cost treatments, consistent with part (i) of Proposition 1. It increases from 40% to just under 55% in the low-cost treatment, but from 15% to over 60% in the high-cost treatment. Hence, an €8 increase in the payment has a 15

[19] This weighting is necessary because in each cell we will typically have no more than a couple of hundred observations, so that deviations in the prior probability from the theoretical value of 50% that stem from randomly drawing the states in the range of multiple percentage points are still quite frequent. Whenever we redefine cells (for instance by considering subjects in the top and bottom halves of the reservation price distribution) we recalculate the weights to fit those cells.

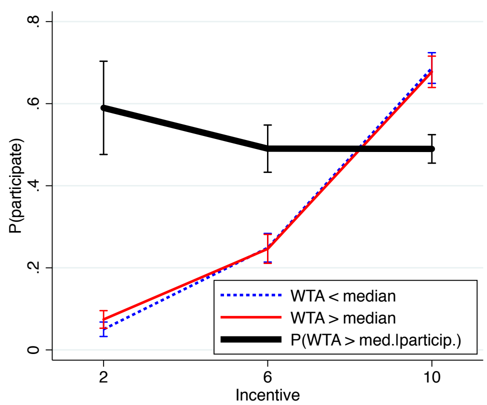
percentage point effect on supply in the low-cost treatment, and a 45 percentage point effect in the high-cost treatment.[20]



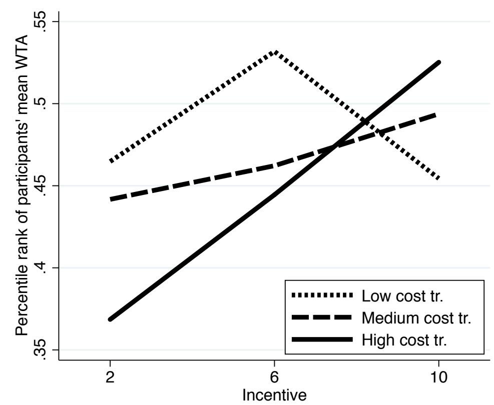
A. Information cost treatments, experimental variation in information acquisition costs.



B. Information cost treatments, measured variation in information acquisition costs.



C. Fixed information treatment, measured variation in information acquisition costs.



D. Information cost treatments, interaction between experimental and measured variation in information acquisition cost. Displayed is the mean percentile rank of WTA to check additional calculations conditional on participating in the gamble.

Figure 3: Supply curves by information cost. Bold solid lines display information acquisition costs conditional on participation. Thin solid or dashed lines display the unconditional probability of participation. In Panels B, C, and D, information costs are measured as willingness-to-accept (WTA) the checking of addition problems. In Panels B and C, subjects are classified as having below-median or above-median WTA. Panel D shows participants' average percentile rank in WTA.

Column 1 of Table 2 performs the corresponding econometric analyses. We use the information cost index as a dependent variable. We are interested in the composition of information costs amongst

[20]Observe that in the boundary case of completely costless information, the supply curve should be constant at 50%. In the case of prohibitively expensive information and risk-averse subjects, supply should be zero for the €2 and €6 payments. For the €10 payment, supply should be equal to the fraction of subjects willing to take a 50/50 win 10 / lose 2 gamble.

subjects who decide to take the gamble, and therefore include only those observations. We test whether an increase in the payment m changes the composition of information-cost treatments from which subjects select into the gamble in the predicted fashion. Indeed, the positive coefficient on the payment amount shows that as the payment increases, the average cost of subjects who decide to participate increases significantly. To check that these results do not depend on our choice of information-cost index, each of the bottom two rows of the table perform the same analysis including only two information cost treatments; in both cases, the results remain qualitatively the same.[21]

	(1)	(2)	(3)	(4)
	Selection		**Supply curves**	
Dependent variable	Mean picture size		Bet taken	
Inclusion criterion				
Bet taken	Yes	Yes		
Min examination time ≥ median		Yes		Yes
Payment index	0.185***	0.119***	0.001	-0.023
	(0.015)	(0.020)	(0.019)	(0.027)
Cost treatment			-0.127***	-0.105***
			(0.009)	(0.014)
Payment index × cost treatment			0.080***	0.062***
			(0.008)	(0.012)
Observations	2,645	1,434	7,008	3,504
Subjects	578	290	584	292
Subsamples	Coefficient on payment		Coefficient on interaction	
Only €2 and €6 payments	0.187***	0.092**	0.042**	0.032
	(0.031)	(0.038)	(0.015)	(0.022)
Only €6 and €10 payments	0.188***	0.167***	0.125***	0.095**
	(0.029)	(0.043)	(0.020)	(0.030)
Only small and medium picture	0.077***	0.048***	0.106***	0.075**
	(0.010)	(0.014)	(0.017)	(0.024)
Only medium and large picture	0.062***	0.041**	0.054***	0.050**
	(0.011)	(0.015)	(0.015)	(0.023)

Table 2: Tests of the predictions of Proposition 1, using experimental variation in information acquisition costs. For simplicity, information-cost-treatments are assigned values 1, 2, and 3. Payment index is coded as 1, 2, 3 for incentive amounts € 2, 6, 10, respectively. This assignment is without loss of generality when only two information-cost-treatments are included, as is the case in the bottom two rows.

While part (ii) of Proposition 1 holds as long as no agent participates in the transaction with probability 1, part (i) only holds if the optimal participation probability lies in the open interval

[21] The estimated magnitudes are smaller simply because the maximal difference in the cost index across the included treatments in the bottom two rows is 1, whereas it is 2 when all cost treatments are included.

(0, 1). Implicitly, this condition means that exogenous variation changes only *how* people acquire information (the intensive margin of information acquisition). Empirically, however, it is also possible that the magnitude of the payment for taking the gamble affects *whether* someone chooses to obtain any information at all (the extensive margin of information acquisition). Figure 4 presents the number of seconds subjects spent examining the list of addition problems, at the subject-round level. While the median time is a substantial 74 seconds, there is significant mass near zero. This suggests that in some rounds, some subjects indeed made their choices based on the prior alone.

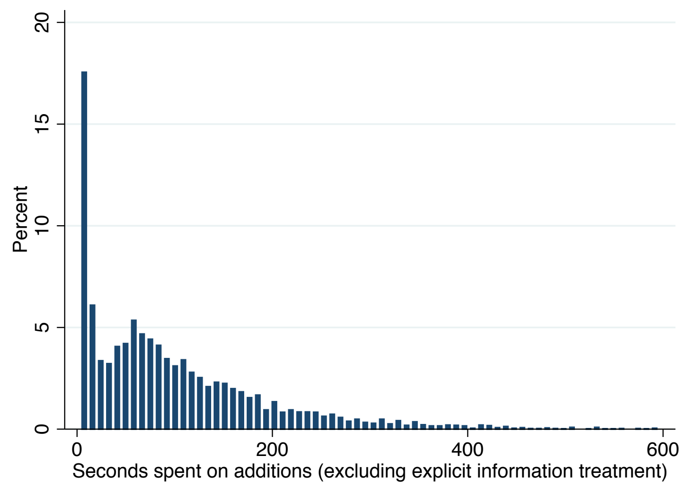


Figure 4: Distribution of seconds subjects spent examining the list of calculations across all three information cost conditions.

To isolate effects that are likely due to the intensive margin alone, column 2 of Table 2 replicates column 1 but including only the half of subjects who spent a non-negligible amount of time on *every* list of calculations they were shown. More specifically, for each subject, we find the minimum time spent across all rounds, excluding the fixed information treatment. The median of these minimum times is 7.3 seconds. Since this is enough time to check at least a handful of additions, these subjects' information acquisition was likely affected on the intensive margin alone. The predicted selection effect also obtains on this subsample.

Our results are not simply due to the fact that a sufficiently large increase in the payment m changes the prior-optimal action. To see this, notice that the prior optimal action will not change for payments of 2 or 6 for any risk-averse subject. As the fourth row from the bottom shows, our results appear, if anything, more pronounced if we only use data with the €2 and €6 payments for analysis.

As argued in Section 2, a change in information costs alters the supply response due to both a level effect and a slope effect, which may reinforce or countervail each other. To demonstrate that our results are to a substantial extent due to the slope effect, we now study the slopes of the supply curves directly. To do so, we estimate a linear probability model in which we regress an indicator of whether the subject takes the gamble on the payment amount, on the information cost index, and on the interaction between the two. The hypothesis that higher information costs induce a more pronounced supply response to variations in the incentive payment implies that the coefficient on the interaction term should be positive. Indeed, as column 3 shows, the estimated coefficient is positive and statistically significant. As column 4 shows, this result continues to hold if we include only those subjects with above-median minimum response times, who arguably respond to a change in the incentive payment for taking the gamble on the intensive margin of information acquisition alone. These two regressions include all nine information cost and incentive treatments, and thus impose linearity assumptions both on the shape of the supply curve and on its response to information costs. The bottom four rows list the estimated coefficient on the interaction on subsets of our treatment conditions that include either only two incentive treatments, or only two cost treatments. The predicted effect obtains in each of them.

Elicited variation in information acquisition cost We now test the predictions of Proposition 1 using variation in reservation prices for checking a given number of calculations.[22]

Panel B of Figure 3 groups subjects into two halves: one half consisting of those who more strongly dislike checking addition problems (high reservation price) and the other half those who are less averse to it (low reservation price). The solid bold line shows how the composition of participants in the gamble changes as the payment m increases, averaged across the three information cost treatments. As predicted, higher payments increase the fraction of high-cost types amongst those who elect to participate. Additionally, the graph displays the supply curve for each of the two groups of individuals, averaged across the information cost conditions. As predicted in part (i) of Proposition 1, the supply curve is steeper for the half of subjects with higher reservation prices.

Table 3 performs the corresponding econometrics. We begin by examining the composition of the pool of participants as a function of the payment m, and thus include only observations in which the subject takes the bet. For ease of interpretation, we rank subjects according to their mean reservation price, and use the percentile rank as the dependent variable. The percentile rank is an increasing function of subjects' (unobserved) marginal information acquisition cost, and thus presents a valid test of Proposition 1. Column 1 shows that an increase in the payment m by €4 increases the mean reservation price rank amongst those who select into the bet by 2.3 percentage points. While statistically significant, the effect sizes seem relatively minor. This analysis averages across information cost treatments; Section 4.3 shows that the effects are more pronounced in the high-cost treatments, as suggested by Proposition 3.

[22] Of those subjects who had to check a given number of calculations (according to the reservation price elicitation), 90.23% of subjects verified 90% or more correctly, and thus exceeded the quality threshold to be paid for (and avoid being punished for) solving this task well enough. This statistic is based on sessions 5–19, as in sessions 1–4 there was an error with recording the fraction of correctly verified calculations.

	(1)	(2)	(3)	(4)	(5)	(6)
		Selection			**Supply curves**	
Dependent variable		Mean reservation price (rank)			Bet taken	
Inclusion criterion						
Bet taken	Yes	Yes	Yes			
Minimal examination time $\geq$ median			Yes			Yes
Payment index	0.023***	0.020	0.014**	0.142***	-0.170***	0.081***
	(0.006)	(0.013)	(0.007)	(0.011)	(0.016)	(0.015)
Res. price $\geq$ median				-0.099***	-0.080***	-0.066*
				(0.026)	(0.027)	(0.038)
Payment index				0.042**	0.045	0.046
$\times$ (res. price $\geq$ median)				(0.019)	(0.028)	(0.029)
Controls (+ interactions)						
Risk aversion		Yes			Yes	
Fixed information treatment		Yes			Yes	
Method	OLS	OLS	OLS	OR-IV	OR-IV	OR-IV
Observations	2,645	3,808	1,434	7,008	10,512	3,504
Subjects	578	583	290	584	584	292
Subsamples		Coefficient on payment			Coefficient on interaction Payment $\times$ (res. price $\geq$ median)	
Only €2 and €6 payments	0.040***	0.057	0.026*	0.063*	0.071	0.071
	(0.012)	(0.036)	(0.014)	(0.034)	(0.048)	(0.050)
Only €6 and €10 payments	0.010	0.001	0.003	0.017	0.012	0.016
	(0.010)	(0.017)	(0.015)	(0.041)	(0.060)	(0.060)

Table 3: Tests of the predictions of Proposition 1 using reservation prices as a proxy for information acquisition costs. Payment index m is coded as 1, 2, 3 for incentive amounts € 2, 6, 10, respectively. Includes session, order, and cost treatment fixed effects. Controls for risk aversion are based on the rank of the mean certainty equivalent elicited. Next to this variable, the controls consist of interactions between this variable and (i) the payment amount for columns 1–3, and (ii) the first three variables listed in the first three rows in the above table for columns 4–6. Standard errors are clustered by subject, bootstrapped for columns 4–6. The coefficient on the payment amount in column 5 has a negative sign, which results from controlling for the fixed information treatment. The interpretation of this coefficient is that the supply curve in the information cost treatments is flatter than in the fixed information treatment.

It is conceivable that our effects arise not because different individuals acquire systematically different information, but merely because reservation prices for checking calculations happen to be correlated with some other personality characteristic, such as risk aversion or belief updating biases. If so, we should observe a similar selection effect in the fixed information treatments, in which subjects have no choice about what information to acquire.[23] Panel C of Figure 3 shows data from the fixed information treatment, regarding both the change of the composition of subjects as a function of the payment m, as well as the supply curves of the two groups. If anything, the selection effect now has the opposite sign, and the supply curves lie virtually on top of each other. Hence, we conclude that the effects in column 1 arise because individuals with different reservation prices for checking calculations choose to acquire different information, not because they respond differently to a given piece of information. To confirm this econometrically, column 2 of Table 3 replicates the analysis in column 1 including statistical controls for behavior in the fixed information treatment, as well as for risk preferences.[24] Our coefficient estimate of the interaction between payments and reservation prices is nearly unchanged, although the standard error more than doubles, causing a loss in statistical significance.

Finally, column 3 reruns the analysis in column 1 on the half of subjects with an above-median minimum examination time to isolate the effect that occurs through the intensive margin on information acquisition. The coefficient size is now estimated at 1.4 percentage points, and is statistically significant at the 5% level.

If we consider these effects excluding either the highest or the lowest incentive treatment, we see that they are mainly driven by the increase in the payment from €2 to €6. Recall that for both the €2 and €6 payments, rejection is the prior-optimal choice for any risk-averse subject. Hence, these results are not driven by a change in the prior-optimal choice.

Finally, columns 4–6 correspond to columns 1–3, but consider how reservation prices change the slope of the supply curve. Because reservation prices are potentially measured with noise, ordinary least squares estimates would suffer from attenuation bias.[25] For each of these variables we have multiple measurements, allowing us to use a subset of these measurements as instrumental variables for the others, according to the Obviously Related Instrumental Variables (OR-IV) estimator introduced in Gillen, Snowberg, and Yariv (2015).[26] In column 4, the effect size of 0.042 is statistically significant

[23] Individuals with low reservation prices for checking additional calculations, for instance, might differ not only by what information they choose to acquire, but also in what conclusions they draw from a given piece of information. The fixed information condition presents subjects with a given piece of information, so that if behavior in that condition differs across subjects with different reservation prices, it must be because subjects draw different conclusions from that same information. Hence, by controlling for behavior in those treatments, we isolate the effect of reservation price for checking additional calculations that arises through information acquisition alone.

[24] We interact each of the predictors in the top three rows of the Table (payment, res. price > median, and payment × (res. price > median)), as well as a constant with the control variable. Hence, we do not merely allow the level of the supply curves to differ depending on the controlled-for characteristics, but also the slopes. See Appendix C for details about this specification.

[25] Panel C of Figure 3 does *not* correct for attenuation bias.

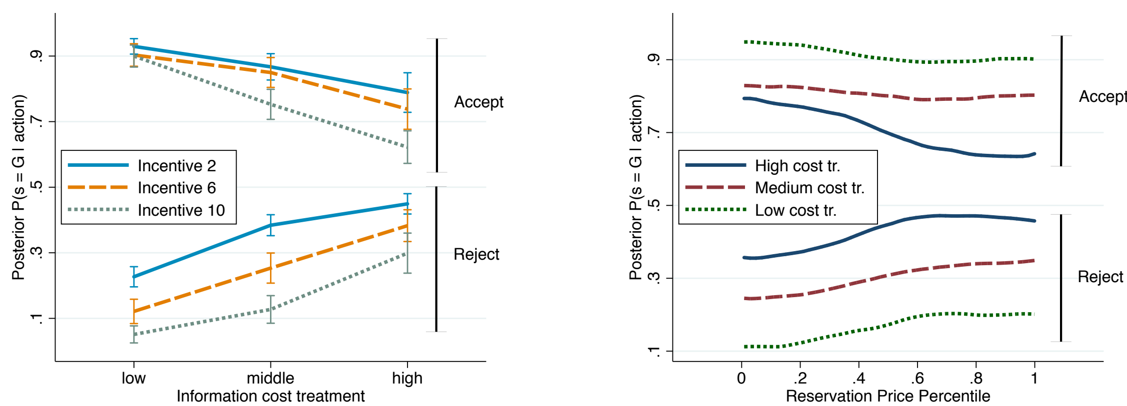
[26] Specifically, for each subject, we have four measured reservation prices; we predict each one using the remaining three. We then estimate a system of four equations, in which each equation includes one of the predicted measurements.

at the 5% level. In columns 5 and 6, the estimated magnitude slightly increases, showing that the effects are robust to these controls. The standard error of the estimates increase too, however, leading to a loss in significance. Again, subsample analysis shows that in all specifications, the results are more pronounced for low-to-medium incentives than for medium-to-high ones.

Overall, this collection of results provides strong empirical support for Proposition 1.

4.2 Posteriors

We now test our predictions on the comparative statics of chosen posterior probabilities, as stated in Proposition 2. Our dependent variables here are the frequencies with which the state is good conditional on whether the bet is accepted or rejected. These frequencies are estimates of the objective probabilities $P(s = G \mid accept)$ and $P(s = G \mid reject)$, which may or may not coincide with the subjective beliefs our participants hold at the time of taking an action. The latter are analyzed in Section 4.5.



(A) (B)

Figure 5: Posterior probabilities conditional on the subject's action (accept or reject). (A) By information cost treatment and incentive treatment. (B) By reservation price and information cost treatment. Moving average, Epanechnikov kernel, bandwidth 0.15.

For each of the nine treatments, Panel A of Figure 5 plots the fraction of times subjects won the bet if they had decided to take it. It also plots the fraction of times they would have won in case they chose to reject (corresponding to $P(s = G \mid reject)$). The curves tend towards the prior of 50% as information costs increase, and thus indicate that in the higher cost treatments, subjects make decisions based on less information, as predicted in Proposition 2. We also observe that incentive payments affect the posterior probabilities directly, replicating a result from Ambuehl (2017). With a higher payment, subjects accept the bet at posteriors that are closer to the prior (less informative),

Following Gillen, Snowberg, and Yariv (2015), we average across the estimated parameters by restricting the coefficient estimates to equal one another. We obtain standard errors by bootstrapping this procedure, clustering on the subject level.

but reject at posteriors that are further from the prior (more informative). Moreover, the magnitude of the comparative statics of information costs and that of incentive payments are quite similar; they are both on the order of 10 to 20 percentage points.

Column 1 of Table 4 displays the corresponding econometrics. For all observations in which subjects accept the gamble, we regress an indicator for being in the good state of the world on our information cost index (1, 2, and 3 for the low, medium, and high cost treatments, respectively). The estimates are displayed in the upper half of the table, and are all significantly negative, as predicted. The effect of increasing information costs is stronger for higher payments. The bottom half of the table performs the parallel analysis on the observations in which the subject refuses the gamble. Also as predicted, all estimated slope coefficients are significantly positive. Unlike for the accept-posteriors, the effect of an increase in information costs does not substantially depend on the payment for the reject-posteriors.

Panel B of Figure 5 displays the comparative statics of posterior probabilities regarding reservation prices for checking calculations. We average across incentive treatments, and split by information cost treatment. Subjects with higher reservation prices, and thus higher information acquisition costs, both accept and reject the gamble based on worse information. This effect is particularly pronounced in the high-cost treatment.

Econometrically, column 2 replicates column 1, with subjects' reservation prices instead of the information cost index as a measure of information acquisition costs.[27] Column 3 additionally controls for risk aversion as well as behavior in the fixed information treatment. In both specifications, our findings have the predicted signs. Regarding the accept-posteriors, we find that the magnitude of the effect is substantially larger for higher payments. Accordingly, our estimates are statistically significant in the €10 treatment (and for column 3 also for the €6 treatment), but not for the €2 treatment. For the reject-posteriors, we also find the predicted positive sign; this time, the magnitude is substantially larger for lower payments, with the corresponding implications regarding statistical significance.

These findings strongly support Proposition 2 in that higher information costs lead to less well-informed decisions.

[27] As in columns 1–3 of Table 3, we use OR-IV, as OLS estimates would suffer from attenuation bias.

	(1)	(2)	(3)	(4)	(5)
Dependent variable		Indicator $s = G$		Elicited belief	Deviation subj. vs. obj.
Cost variation	Experimental	WTA	WTA	Experimental	
Controls (incl. interactions)					
Risk aversion			Yes		
Fixed information			Yes		
Method	OLS	OR-IV	OR-IV	OLS	OLS
			Bet accepted		
Effect of cost increase by incentive treatment					
2€	-0.067***	-0.049	-0.043	-0.045***	0.021
	(0.015)	(0.047)	(0.046)	(0.009)	(0.014)
6€	-0.080***	-0.094	-0.124**	-0.058***	0.021
	(0.018)	(0.058)	(0.056)	(0.009)	(0.017)
10€	-0.140***	-0.115**	-0.112**	-0.091***	0.048***
	(0.016)	(0.054)	(0.054)	(0.007)	(0.015)
Observations	2,645	2,645	3,808	2,645	2,645
Subjects	578	578	583	578	578
			Bet rejected		
Effect of cost increase by incentive treatment					
2€	0.109***	0.193***	0.204***	0.083***	-0.026**
	(0.011)	(0.040)	(0.040)	(0.006)	(0.011)
6€	0.130***	0.090	0.098**	0.103***	-0.027*
	(0.016)	(0.055)	(0.049)	(0.009)	(0.016)
10€	0.123***	0.024	0.036	0.055***	-0.067***
	(0.016)	(0.056)	(0.056)	(0.009)	(0.017)
Observations	4,363	4,363	6,704	4,363	4,363
Subjects	580	580	583	580	580

Table 4: Tests of the predictions of Proposition 2. Includes session and order fixed effects. Controls for risk aversion include interactions between incentive-level dummies and ranks of subjects' mean certainty-equivalent. Similarly for controls for the fixed information treatment. Standard errors clustered by subject. Column 3 has a larger number of subjects and observations because the fixed information treatment is included only in that column. Asterisks are suppressed for constants.

4.3 Contextual information costs

We now test whether selection effects become stronger as we raise the contextual information acquisition cost, and then examine the formal prediction of Proposition 3.

Panel D of Figure 3 shows, for each information cost treatment, how the composition of subjects who elect to participate in the gamble changes with the payment m. Each line displays the fraction of subjects with an above-median reservation price for checking calculations. The selection effect in the high-cost condition is considerable: the proportion of high-cost participants rises from 37% to 55% as the payment increases from €2 to €10. Importantly, this increase is significantly more pronounced than in the medium-cost condition, where the fraction of high-cost participants increases from 44% to 49% over the same increase in the payment. Yet the selection effect is more attenuated in the low-cost condition, with nearly indistinguishable fractions of high-cost participants between the €2 and the €10 treatments. Unexpectedly, however, selection in the low-incentive treatment is non-monotonic.[28]

Table 5 displays the corresponding econometrics, using the same specifications as those in Table 3, with the important exception that all right-hand-side variables are interacted with the information-cost-treatment index (with the exception of session and order fixed effects). Columns 1–3 analyze the composition of those who elected to take the gamble. In all three columns, the coefficient on the interaction between cost treatment and payment amount is 0.021 and highly statistically significant, regardless of whether we include controls for risk aversion and behavior in the fixed information treatment (column 2) or whether we consider only subjects with an above-median minimal response time (column 3). Subsample analysis reveals significant effects when the low-cost treatment is excluded, but not when the high-cost treatment is excluded. Similarly, it reveals significant effects when the low incentive treatment is excluded, but not when the high incentive treatment is excluded. Both of these latter results are likely due to the non-monotonicity in the low-cost treatment.

Columns 4–6 formally examine the prediction of Proposition 3, which states that the effect of individual-level variation in information costs on the supply curve should increase with contextual information costs. Formally, the three-way interaction $\big($(res. price $\geq$ median) $\times$ payment $\times$ cost treatment$\big)$ should have a positive coefficient. Indeed, the estimated coefficient value in column 4 is 0.04, which is significant at the 5% level. Adding controls for risk aversion and behavior in the fixed information treatment leaves the coefficient estimate virtually unchanged, but causes an increase in its standard error (column 5). The estimated coefficient is slightly larger on the sample of subjects with an above-median minimal response time, and statistically different from zero at the 10% level.

Hence, experimental behavior aligns with the predictions of Proposition 3.

[28] Regressing the reservation price rank of those who select into the gamble on both the payment amount in the low-cost and medium-payment and high-payment conditions reveals that the decrease is significantly different from zero at $p = 0.09$.

	(1)	(2)	(3)	(4)	(5)	(6)
		Selection			**Supply curves**	
Dependent variable		Mean reservation price (rank)			Bet taken	
Inclusion criterion						
Bet taken	Yes	Yes	Yes			
Minimal examination time ≥ median			Yes			Yes
Payment index	-0.017	-0.017	-0.027	0.017	0.018	0.003
	(0.012)	(0.016)	(0.016)	(0.023)	(0.032)	(0.038)
Cost treatment						
× 1	-0.028***	-0.028***	-0.027**	-0.114***	-0.124***	-0.082***
	(0.010)	(0.010)	(0.012)	(0.012)	(0.017)	(0.021)
× payment index	0.021***	0.021***	0.021***	0.062***	0.050***	0.039**
	(0.006)	(0.006)	(0.008)	(0.011)	(0.016)	(0.018)
Res. price ≥ median						
× 1				-0.009	-0.007	0.058
				(0.050)	(0.066)	(0.077)
× payment index				-0.037	-0.037	-0.071
				(0.043)	(0.057)	(0.067)
× cost treatment				-0.032	-0.032	-0.062*
				(0.021)	(0.027)	(0.034)
× payment index × cost treatment				0.040**	0.041	0.058*
				(0.019)	(0.027)	(0.030)
Controls						
Risk aversion (+ interactions)		Yes			Yes	
Fixed information (+ interactions)		Yes			Yes	
Method	OLS	OLS	OLS	OR-IV	OR-IV	OR-IV
Observations	2,645	3,808	1,434	7,008	10,512	3,504
Subjects	578	583	290	584	584	292
Subsamples		Coefficient on interaction Payment × cost treatment			Coefficient on interaction payment × cost treatment × (res. price ≥ median)	
Only €2 and €6 payments	0.007	0.007	0.000	-0.057	-0.035	-0.057
	(0.013)	(0.014)	(0.017)	(0.039)	(0.051)	(0.060)
Only €6 and €10 payments	0.034**	0.033**	0.041**	0.156***	0.137**	0.191**
	(0.012)	(0.012)	(0.018)	(0.049)	(0.064)	(0.080)
Only small and medium picture	0.017	0.017	0.025	0.025	0.032	0.071
	(0.012)	(0.012)	(0.016)	(0.041)	(0.047)	(0.062)
Only medium and large picture	0.021***	0.026**	0.021	0.056	0.054	0.046
	(0.006)	(0.012)	(0.015)	(0.037)	(0.042)	(0.030)

Table 5: Tests of the predictions of Proposition 3. Treatment effects depending on costliness of the environment. Payment index is coded as 1, 2, 3 for incentive amounts € 2, 6, 10, respectively. Includes session and order fixed effects. Controls for risk aversion are based on the rank of the mean certainty equivalent elicited. Next to this variable, the controls consist of interactions between this variable with the seven variables listed in the first seven rows in the above table for columns 4–6, and with the first three variables for columns 1–3. Standard errors are clustered by subject, bootstrapped for columns 1–3.

4.4 Additional measures: Educational demographics and cognitive ability

Educational demographics Studying how the effects of incentives for participation differ depending on educational demographics serves two goals. First, comparing the effects of background in mathematics against background in German literature shows that the effects are due to context specific expertise. Skills and preferences for mathematics likely reduce the information processing and acquisition costs in our mathematics-related experimental tasks. Second, since subjects' educational backgrounds were determined long before taking part in our experiment, any differences in behavior capture the role of a persistent characteristic, rather than a transient effect such as being unusually tired for the day.

Column 1 of Table 6 tests Proposition 1. Each entry corresponds to a separate regression that follows the same specification as column 1 of Table 3, but using a different dependent variable. Specifically, on each row, we regress a specific educational demographic on the payment m, using only those subjects who have selected into the gamble, pooled across all information treatments. Additionally, we control for the German state (Bundesland) in which the subject completed high school, since standards and requirements differ across states. For ease of interpretation and comparison across measures, we use percentile ranks rather than levels for variables measured on a quasi-continuous scale.[29] Arguably, these percentile ranks are increasing functions of subjects' (unobserved) marginal information acquisition costs, and thus present a valid test of Proposition 1.

We find that raising the payment for participation by €4 lowers the chance that a subject selecting into the gamble is currently enrolled in a STEM major by 3 percentage points. Relatedly, it lowers the chance that the subject has taken a high school honors class in mathematics by 3.5 percentage points. Finally, it decreases the average participant's high school mathematics percentile rank by 1.1 points. Our control measure, German literature, shows none of, or the opposite of these selection effects, regarding both grades and having taken an honors class.[30] For completeness, we also report the effect of overall high-school GPA, which is an average of several academic subjects, including mathematics and German. Accordingly, we find no effect on that measure.

[29] 54.8% of our subjects are enrolled in a STEM major. Amongst those, 11.8% have taken an honors class in both mathematics and German, 29.9% have taken neither, 33.6% have taken only mathematics, and 24.7% have taken only German. Amongst those not enrolled in a STEM major, the respective numbers are 10.5%, 31.9%, 19.8%, and 37.9%.

[30] A potential explanation for the positive coefficient is that German high school students in some states are required to choose a fixed numbers of honors classes, and the choice of an honors class in German literature may therefore simply reflect a dislike of mathematics.

	(1)	(2)	(3)	(4)
Tested Proposition	1	2		3
Interpretation of coefficient	Effect of increase in payment by €4 on listed variable	Effect of listed variable on $P(s = G \mid \text{choice})$		Effect of interaction between cost treatment and payment on listed variable
Sample				
Bet choice	accept	accept	reject	accept
A. Educational demographics				
Effect expected				
College major STEM	-0.030**	0.062**	-0.005	-0.008
	(0.011)	(0.019)	(0.016)	(0.011)
HS maths honors class taken	-0.035**	0.041**	-0.034**	-0.012
	(0.011)	(0.020)	(0.017)	(0.011)
HS grade maths (%-ile rank)	-0.011*	0.153**	-0.082**	-0.013**
	(0.006)	(0.037)	(0.034)	(0.006)
No / opposite effect expected				
HS German honors class taken	0.015	-0.055**	0.024	0.008
	(0.011)	(0.021)	(0.017)	(0.011)
HS grade German (%-ile rank)	0.011*	-0.006	0.017	0.004
	(0.006)	(0.039)	(0.036)	(0.006)
Ambiguous				
HS GPA (%-ile rank)	0.001	0.076**	-0.070**	-0.003
	(0.006)	(0.035)	(0.03)	(0.006)
B. Cognitive ability (Raven's test)				
Unincentivized (%-ile rank)	-0.018**	0.180**	-0.172**	-0.007
	(0.007)	(0.052)	(0.047)	(0.007)
Incentivized (%-ile rank)	-0.001	0.095*	-0.007	0.017*
	(0.010)	(0.056)	(0.052)	(0.010)

Table 6: Tests of Propositions 1, 2, and 3, using alternative measures of subject-specific information acquisition costs. For comparability, all cardinal variables are transformed into percentile ranks. Columns 1 and 4 display the coefficients of regressions in which the characteristics listed in the rows are the *left hand side* variable, and the predictor is an increase in the payment m by €4. Columns 2 and 3 display the coefficients of regressions in which the state of the world is the dependent variable, and the characteristics listed in the rows are *right hand side* variables. Each row displays the estimates from a separate regression. Regressions concerning high school control for the state in which the subject attended high school. Regressions concerning cognitive ability control for time taken to complete the test.

Columns 2 and 3 test Proposition 2. In column 2, we regress, for each variable, an indicator of whether the state is good on that variable, amongst the subset of subjects take the bet. The resulting estimate informs us of how the variable under consideration changes the likelihood that the state of the world is good, conditional on taking the bet.[31] In column 3 we perform the same estimations, but on the subsample of subjects who reject the bet. As predicted, subjects with a stronger background in mathematics are more likely to win if they take the gamble, and more likely would have lost if they reject it. The subject who is top-ranked in high school mathematics, for instance, is 15.3 percentage points more likely to win conditional on taking the bet than is the bottom-ranked subject. Moreover, for our control measures related to German literature, we find either no effect or the opposite effects. Finally, the results regarding high school GPA take the same direction as those of mathematics background.

To test Proposition 3, we perform regressions of the same form as in column 1 of Table 5. We find a significant effect of the predicted sign regarding high school mathematics grades. The remaining estimates are directionally consistent with our prediction, but not statistically significantly different from zero.

Cognitive ability Results regarding cognitive ability are of interest because it is a potent predictor of important life outcomes. Observe that in our context, predicting selection effects based on cognitive ability is challenging. Our subjects are all students of the University of Cologne, and hence the subject pool is already rather selective regarding cognitive ability. This naturally attenuates the additional selection we can possibly generate through our experimental treatments. We explore both the *unincentivized IQ* treatment, and the *incentivized IQ* treatment. According to previous research we expect different predictive power (Segal, 2012; Duckworth, Quinn, Lynam, Loeber, and Stouthamer-Loeber, 2011; Borghans, Meijers, and Ter Weel, 2008; Dessi and Rustichini, 2015), but we have no *ex ante* hypothesis about the direction.

Regarding the *unincentivized IQ* treatment, we find that an increase in the payment m of €4 leads to a 1.8 percentile point reduction in the test score of subjects who elect to participate in the gamble. Additionally, conditional on taking the gamble, the top-ranked subject is 18 percentage points more likely to win than the bottom-ranked subject. Relatedly, if the top-ranked subject refuses the gamble, it is 17.2 percentage points less likely that she would have lost. Finally, while the coefficient in column 4 has the predicted sign, it is not statistically different from zero.

The *incentivized IQ* treatment has lower predictive power; the only statistically significant coefficient with the predicted direction pertains to the chance that a subject who opts for the gamble wins.

To the extent that the difference between incentivized and unincentivized scores on IQ tests is attributable to intrinsic motivation (Segal, 2012; Duckworth, Quinn, Lynam, Loeber, and Stouthamer-Loeber, 2011), our results suggest that within our experiment, intrinsic motivation is the more important determinant of behavior than intellectual ability. This appears plausible considering that our experiment involves university students solving elementary addition problems.

[31] Formally, we obtain an estimate of $P(s = G|\text{accept}, X)$ where X is the variable under consideration.

4.5 Subjective vs. objective posteriors

Finally, we compare the deviation between elicited and objective posterior beliefs across treatments.[32] This comparison reveals whether subjects choose what they intend to choose, or whether their choice is based on a misconception about objective facts, and thus possibly at odds with their own preferences. Importantly, we reiterate that the extent to which objective posterior probabilities and subjective beliefs align cannot be interpreted as evidence either for or against the behavioral predictions of the theoretical model. What matters regarding the behavioral predictions of the model is solely what actions subjects take based on the information they observe, as analyzed above. Given that choice, a behavioral test is unaffected by whether subjects are more or less certain about the state of the world compared to a Bayesian observing the same information. Deviations between objective and subjective posteriors are, however, of interest regarding some aspects of subjects' welfare.[33]

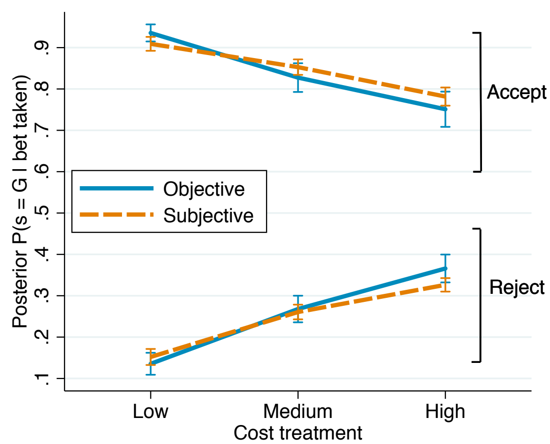


Figure 6: Objective vs. elicited beliefs. Averaged across incentive treatments.

Figure 6 displays objective and elicited posteriors for subjects who accepted the gamble. As shown in Section 4.2, decisions in the higher cost treatments are based on less information; the objective posteriors are closer to the prior. The graphs corresponding to elicited beliefs show that subjects are, on average, aware of this fact. However, subjects underestimate the extent to which their decision quality deteriorates in more difficult information contexts. In the high-cost treatment, the average subject who accepts the bet is overconfident about the chance of winning the gamble.

The corresponding econometric analyses are in columns 4 and 5 of Table 4. Column 4 shows that elicited posteriors significantly decrease as information costs rise, for subjects who accepted the

[32] After stating their posterior beliefs, subjects had the opportunity to return to the previous screen to change their decision whether to accept or refuse the bet. Overall, 1.05% of all decisions were changed, and 15.6% of subjects changed their decision at least once over the 18 rounds of the experiment.

[33] In the language of Bernheim and Rangel (2009), the comparison illuminates the question of whether subjects' choices—which our theoretical model describes accurately—should be considered as lying within or beyond the *welfare-relevant domain.*

bet. This is true within each incentive condition, and the opposite effect obtains for subjects who rejected the bet. Column 5 displays the difference between elicited posteriors and objective posteriors. Conditional on the subjects' choice of action, elicited beliefs deteriorate to a lesser extent than objective posteriors in each incentive condition. Statistical significance obtains for subjects who accept the bet when the incentive payment is high, in which case the effect is largest. The econometrics also confirm the respective effects for decisions in which the subject rejects the gamble.[34]

5 Discussion and Conclusion

Many economic transactions combine a monetary payment for participation in a transaction with consequences that are not entirely certain. This paper shows that individuals are more responsive to the payment when information about the consequences is costlier to acquire and process, regardless of whether this is because the transaction is objectively difficult to understand, or because of individual-specific differences. There is a corresponding selection effect: as the payment for participating in the transaction rises, the composition of subjects who elect to participate shifts towards those with higher information costs. These people also make less informed decisions.

While these findings are of interest in fields as diverse as consumer choice, finance, and labor economics, one policy application concerns the controversial topic of transactions for which incentive payments are limited by laws and guidelines (Becker and Elias, 2007; Roth, 2007; Ambuehl, 2017), such as living tissue donation or clinical trial participation. Our results highlight a conflict between incentive payments and the principles of informed consent. While we take no normative stance regarding these principles, we highlight that even for policy makers who subscribe to this principle, banning or limiting these payments is not necessarily the optimal response. One alternative consists of stringent informed consent requirements, perhaps coupled with an assessment of participants' comprehension. Another alternative involves reducing information costs through regulatory measures. In the domain of finance, for instance, the European Union now requires that retail investors interested in certain investment products must be provided with a standardized information sheet no longer than three pages describing the costs and risk/reward profile of the product.

A frequently voiced concern with payments for transactions like living tissue donation or gestational surrogacy is that they would disproportionately increase participation by the poor. This raises the question of how economic inequality interacts with the selection effects we document in this paper. The answer depends on context.[35] For example, economic inequality will compound the selection effects

[34] A potential confounder is the possibility that subjects' reported beliefs are biased toward the middle of the relevant half of the belief-elicitation scale. This exact effect has not been reported in the literature, but in contexts outside of information acquisition and belief updating, there is a well-known tendency for subjects to choose options towards the middle of multiple-decision lists (Andersen, Harrison, Lau, and Rutstrom, 2006).

[35] The dependence rests on two factors: which model elements scale with a change in the marginal utility of money, and whether wealthier individuals have higher or lower information costs. To see how, observe that our model consists of three elements: the consequences of participation π_G and π_B, the payment m (which we take to be monetary), and the information costs c (which we take to be non-monetary). If the consequences of participation do not scale with the

we document if the following two conditions hold. The first condition is that the utility consequences of participation, aside from the payment m, are the same for rich and poor individuals. This may be considered an appropriate assumption for transactions such as living tissue donation or gestational surrogacy wherein the consequences concern physical wellbeing. The second condition is that poorer individuals tend to have higher information costs. This is plausible to the extent that cognitive ability and education are correlated with socioeconomic status. Importantly, survey evidence suggests that concerns about the failure to comprehend the consequences of a transaction might be a driving force underlying ethical qualms with incentivizing the poor, rather than vice versa: on the topic of human egg donation, respondents in Ambuehl and Ockenfels (2017) are substantially more concerned about incentivizing women who have trouble understanding the risks and consequences of the procedure than about incentivizing poorer women *per se*.

marginal utility of money, the only factor affected by a change in that variable is the payment. For a poorer individual, a given change in the payment is more substantial (under the assumption of diminishing marginal utility of money). Accordingly, if poorer individuals tend to have higher costs of information acquisition, wealth heterogeneity compounds the selection effect of incentive payments in Proposition 1; otherwise it counteracts that effect. If the consequences do scale with the marginal utility of money, the only non-monetary variable is the information cost. Such an assumption is appropriate for purely financial transactions such as credit card contracts with shrouded fees. *Ceteris paribus*, being richer is now akin to having lower stakes in the transaction, at unchanged information costs. Accordingly, if wealth and information costs are negatively correlated, wealth heterogeneity may counteract the selection effect in Proposition 1; otherwise it will compound it.

References

Abeler, J., A. Falk, L. Goette, and D. Huffman (2011). Reference points and effort provision. *American Economic Review 101*(2), 470–92.

Agarwal, S. and B. Mazumder (2013). Cognitive abilities and household financial decision making. *American Economic Journal: Applied Economics 5*(1), 193–207.

Almlund, M., A. L. Duckworth, J. J. Heckman, and T. D. Kautz (2011). Personality psychology and economics. In *Handbook of the Economics of Education*, Volume 4, pp. 1–181. Elsevier.

Ambuehl, S. (2017). An offer you can't refuse? Incentives change how we inform ourselves and what we believe. CESifo Working Paper 6296.

Ambuehl, S. and S. Li (2018). Belief updating and the demand for information. *Games and Economic Behavior 109*, 21–39.

Ambuehl, S., M. Niederle, and A. E. Roth (2015). More money, more problems? Can high pay be coercive and repugnant? *American Economic Review, Papers & Proceedings 105*(5), 357–60.

Ambuehl, S. and A. Ockenfels (2017). The ethics of incentivizing the uninformed. A vignette study. *American Economic Review, Papers & Proceedings 107*(5), 91–95.

Andersen, S., G. W. Harrison, M. I. Lau, and E. E. Rutstrom (2006). Elicitation using multiple price list formats. *Experimental Economics 9*, 383–405.

Azfar, O. (1999). Rationalizing hyperbolic discounting. *Journal of Economic Behavior & Organization 38*(2), 245–252.

Bartoš, V., M. Bauer, J. Chytilová, and F. Matějka (2016). Attention discrimination: Theory and field experiments with monitoring information acquisition. *American Economic Review 106*(6), 1437–75.

Basu, K. (2003). The economics and law of sexual harassment in the workplace. *Journal of Economic Perspectives 17*(3), 141–157.

Basu, K. (2007). Coercion, contract and the limits of the market. *Social Choice and Welfare 29*(4), 559–579.

Becker, G. S. and J. J. Elias (2007). Introducing incentives in the market for live and cadaveric organ donations. *Journal of Economic Perspectives 21*(3), 3–24.

Benjamin, D. J., S. A. Brown, and J. M. Shapiro (2013). Who is 'behavioral'? Cognitive ability and anomalous preferences. *Journal of the European Economic Association 11*(6), 1231–1255.

Bernheim, B. D. and A. Rangel (2009). Beyond revealed preference: Choice-theoretic foundations for behavioral welfare economics. *Quarterly Journal of Economics 124*(1), 51–104.

Bordalo, P., N. Gennaioli, and A. Shleifer (2012). Salience theory of choice under risk. *Quarterly Journal of Economics 127*(3), 1243–1285.

Bordalo, P., N. Gennaioli, and A. Shleifer (2013). Salience and consumer choice. *Journal of Political Economy 121*(5), 803–843.

Borghans, L., H. Meijers, and B. Ter Weel (2008). The role of noncognitive skills in explaining cognitive test scores. *Economic Inquiry 46*(1), 2–12.

Burks, S. V., J. P. Carpenter, L. Goette, and A. Rustichini (2009). Cognitive skills affect economic preferences, strategic behavior, and job attachment. *Proceedings of the National Academy of Sciences 106*(19), 7745–7750.

Buser, T., L. Gerhards, and J. van der Weele (2016). Measuring responsiveness to feedback as a personal trait. Working paper.

Bushong, B., M. Rabin, and J. Schwartzstein (2015). A model of relative thinking. Working paper.

Caplin, A. (2016). Measuring and modeling attention. *Annual Reviews of Economics 8*, 379–403.

Caplin, A. and M. Dean (2013). Rational inattention, entropy, and choice: The posterior-based approach. Working paper.

Cheremukhin, A., A. Popova, and A. Tutino (2015). A theory of discrete choice with information costs. *Journal of Economic Behavior & Organization 113*, 34–50.

Clemens, M. (2017). Testing for repugnance in economic transactions: Evidence from guest work in the Gulf. Working paper.

Cryder, C. E., A. J. London, K. G. Volpp, and G. Loewenstein (2010). Informative inducement: Study payment as a signal of risk. *Social Science & Medicine 70*, 455–464.

Dean, M., Ö. Kıbrıs, and Y. Masatlioglu (2017). Limited attention and status quo bias. *Journal of Economic Theory 169*, 93–127.

Dean, M. and N. Neligh (2017). Experimental tests of rational inattention. Working paper.

Dessi, R. and A. Rustichini (2015). Strong intrinsic motivation. TSE Working Paper 567.

Dohmen, T., A. Falk, D. Huffman, and U. Sunde (2010). Are risk aversion and impatience related to cognitive ability? *American Economic Review 100*(3), 1238–1260.

Duckworth, A. L., P. D. Quinn, D. R. Lynam, R. Loeber, and M. Stouthamer-Loeber (2011). Role of test motivation in intelligence testing. *Proceedings of the National Academy of Sciences 108*(19), 7716–7720.

El-Gamal, M. A. and D. M. Grether (1995). Are people Bayesian? Uncovering behavioral strategies. *Journal of the American Statistical Association 90*(432), 1137–1145.

Elias, J., N. Lacetera, and M. Macis (2015a). Sacred values? The effect of information on attitudes toward payments for human organs. *American Economic Review, Papers & Proceedings 105*(5), 361–365.

Elias, J., N. Lacetera, and M. Macis (2016). Efficiency-morality trade-offs in repugnant transactions: A choice experiment. Working paper.

Elias, J. J., N. Lacetera, and M. Macis (2015b). Markets and morals: An experimental survey study. *PLoS ONE 10*(6).

Exley, C. L. and J. B. Kessler (2017). Equity concerns are narrowly framed: Why money cannot buy time. Working paper.

Faden, R. R. and T. L. Beauchamp (1986). *A History and Theory of Informed Consent*. Oxford University Press.

Fréchette, G. R., A. Schotter, and I. Trevino (2017). Personality, information acquisition, and choice under uncertainty: An experimental study. *Economic Inquiry 55*(3), 1468–1488.

Gabaix, X. and D. Laibson (2017). Myopia and discounting. NBER Working Paper.

Gillen, B., E. Snowberg, and L. Yariv (2015). Experimenting with measurement error: Techniques with applications to the Caltech cohort study. NBER Working Paper.

Hébert, B. and M. Woodford (2017). Rational inattention with sequential information sampling. Working paper.

Holt, C. A. and A. M. Smith (2009). An update on Bayesian updating. *Journal of Economic Behavior & Organization 69*, 125–134.

Kahneman, D., J. L. Knetsch, and R. Thaler (1986). Fairness as a constraint on profit seeking: Entitlements in the market. *American Economic Review 76*(4), 728–741.

Kamenica, E. (2008). Contextual inference in markets: On the informational content of product lines. *American Economic Review 98*(5), 2127–2149.

Kanbur, R. (2004). On obnoxious markets. In S. Cullenberg and P. Pattanaik (Eds.), *Globalization, Culture and the Limits of the Market: Essays in Economics and Philosophy*. Oxford University Press.

Karni, E. (2009). A mechanism for eliciting probabilities. *Econometrica 77*(2), 603–606.

Kőszegi, B. and F. Matějka (2017). Mental accounting economizes on information. Working paper.

Kőszegi, B. and A. Szeidl (2012). A model of focusing in economic choice. *Quarterly Journal of Economics 128*(1), 53–104.

Leider, S. and A. E. Roth (2010). Kidneys for sale: Who disapproves, and why? *American Journal of Transplantation 10*(5), 1221–1227.

Matějka, F. and A. McKay (2015). Rational inattention to discrete choices: A new foundation for the multinomial logit model. *American Economic Review 105*(1), 272–98.

Morris, S. and H. S. Shin (2002). Social value of public information. *American Economic Review 92*(5), 1521–1534.

Morris, S. and P. Strack (2017). The Wald problem and the equivalence of sequential sampling and static information costs. Working paper.

Peterson, C. R. and L. R. Beach (1967). Man as an intuitive statistician. *Psychological Bulletin 68*(1), 29–46.

Pinkovskiy, M. L. (2009). Rational inattention and choice under risk: explaining violations of expected utility through a Shannon entropy formulation of the costs of rationality. *Atlantic Economic Journal 37*(1), 99–112.

Raven, J. C., J. C. Raven, and J. H. Court (1962). *Advanced Progressive Matrices*. HK Lewis London.

Roth, A. E. (2007). Repugnance as a constraint on markets. *Journal of Economic Perspectives 21*(3), 37–58.

Satz, D. (2010). *Why Some Things Should Not Be For Sale: The Moral Limits of Markets*. Oxford University Press.

Segal, C. (2012). Working when no one is watching: Motivation, test scores, and economic success. *Management Science 58*(8), 1438–1457.

Steiner, J. and C. Stewart (2016). Perceiving prospects properly. *American Economic Review 106*(7), 1601–1631.

Steiner, J., C. Stewart, and F. Matějka (2017). Rational inattention dynamics: Inertia and delay in decision-making. *Econometrica 84*(2), 521–553.

Tversky, A. and D. Kahneman (1974). Judgment under uncertainty: Heuristics and biases. *Science 185*(4157), 1124–1131.

US Department of Health, Education, and Welfare (DHEW) (1978). *The Belmont report: Ethical principles and guidelines for the protection of human subjects of research.* National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research, Washington DC.

Wald, A. (1947). *Sequential Analysis.* New York: Wiley.

Woodford, M. (2012a). Inattentive valuation and reference-dependent choice. Working paper.

Woodford, M. (2012b). Prospect theory as efficient perceptual distortion. *American Economic Review 102*(3), 41–46.

ONLINE APPENDIX

Attention and Selection Effects

Sandro Ambuehl, Axel Ockenfels, and Colin Stewart

Table of Contents

A Proofs

A.1 Proof of Proposition 1

A.1.1 Proof of part (i)

For simplicity of notation, we omit the arguments from $p_s(m,\lambda)$ and $p(m,\lambda)$. A direct application of Theorem 1 in Matějka and McKay (2015) shows that for each $s \in \{G,B\}$, the state-contingent participation probabilities p_s are given by

$$p_s = \left[1 + \left(\frac{1}{p} - 1\right)\exp\left\{-\frac{1}{\lambda}(\pi_s + m)\right\}\right]^{-1}.$$

Substituting these expressions into the equation $p = \mu p_G + (1-\mu)p_B$ defining p and dividing both sides by p gives

$$1 = \frac{\mu}{p + (1-p)/g} + \frac{1-\mu}{p + (1-p)/b},$$

where $g := \exp\left((\pi_G + m)/\lambda\right)$ and $b := \exp\left((\pi_B + m)/\lambda\right)$. Note that, since $\pi_G + m > 0 > \pi_B + m$, $g > 1 > b$. Rearranging gives

$$-\mu\frac{g-1}{g\frac{p}{1-p}+1} = (1-\mu)\frac{b-1}{b\frac{p}{1-p}+1}.$$

Solving for $\frac{p}{1-p}$ then yields

$$\frac{p}{1-p} = -\frac{(1-\mu)(b-1)+\mu(g-1)}{(1-\mu)(b-1)g+\mu b(g-1)},$$

from which we obtain

$$p = -\frac{\mu}{b-1} - \frac{1-\mu}{g-1}. \tag{2}$$

Differentiating with respect to m gives

$$\frac{\partial p}{\partial m} = \frac{1-\mu}{(g-1)^2}\frac{g}{\lambda} + \frac{\mu}{(b-1)^2}\frac{b}{\lambda}.$$

Let A denote the first of the two terms on the right-hand side. We will show that $\frac{\partial A}{\partial \lambda} > 0$; a similar argument applies to the second term, thereby proving the result. We have

$$\frac{1}{1-\mu}\frac{\partial A}{\partial \lambda} = \frac{2g^2\log g}{\lambda^2(g-1)^3} - \frac{g\log g}{\lambda^2(g-1)^2} - \frac{g}{\lambda^2(g-1)^2},$$

which is positive if and only if

$$(g+1)\log g - g + 1 > 0.$$

The left-hand side of this inequality is equal to 0 when $g = 1$ and its derivative is positive everywhere. Therefore, the inequality holds for all $g > 1$, as needed.

A.1.2 Proof of part (ii)

Lemma 1. *Let X be a continuously distributed real-valued random variable and let $f : \mathbb{R} \longrightarrow \mathbb{R}_+$ and $g : \mathbb{R} \longrightarrow \mathbb{R}_+$ be such that $\frac{f(x)}{g(x)}$ is increasing in x and $E[f(X)] > 0$ and $E[g(X)] > 0$. Then*

$$\frac{E[Xf(X)]}{E[f(X)]} > \frac{E[Xg(X)]}{E[g(X)]}.$$

Proof. Let γ be the density of X. Let $\hat{f}(x) = f(x)\gamma(x)/E[f(X)]$ and $\hat{g}(x) = g(x)\gamma(x)/E[g(X)]$. Note that $\hat{f}$ and $\hat{g}$ are probability density functions. Since $\frac{f(x)}{g(x)}$ is increasing, so is

$$\frac{f(x)\gamma(x)}{E[f(X)]} \cdot \frac{E[g(X)]}{g(x)\gamma(x)} = \frac{\hat{f}(x)}{\hat{g}(x)}.$$

That is, $\hat{f}$ and $\hat{g}$ satisfy the monotone likelihood ratio property. In particular, the distribution associated with $\hat{f}$ first-order stochastically dominates that associated with $\hat{g}$. It follows that

$$\int_{-\infty}^{\infty} x\hat{f}(x)dx > \int_{-\infty}^{\infty} x\hat{g}(x)dx.$$

By definition of $\hat{f}$ and $\hat{g}$, this last inequality is equivalent to

$$\frac{E[Xf(X)]}{E[f(X)]} = \int_{-\infty}^{\infty} x\frac{f(x)\gamma(x)}{E[f(X)]}dx > \int_{-\infty}^{\infty} x\frac{g(x)\gamma(x)}{E[g(X)]}dx = \frac{E[Xg(X)]}{E[g(X)},$$

as needed. □

Lemma 2. *The function*

$$h(b, g) = -\left((b-1)g + b(g-1)\right)(b-1)(g-1) + (b-1)g(2b-g-1)\log g + (g-1)b(2g-b-1)\log b$$

is positive everywhere on the set $\Gamma = \{(b, g) \mid b \in (0, 1) \text{ and } g \in (1, \infty)\}$.

Proof. Note that $h(1, g) \equiv 0$, so it suffices to show that $h_b(b, g)$ is negative everywhere on Γ, where h_b denotes the partial derivative of h with respect to b. We have

$$h_b(b, g) = -(g-1)(4bg-5g-b+2) + (4b-g-3)g\log g + (g-1)(2g-2b-1)\log b.$$

In particular, $h_b(b,1) \equiv 0$. Hence h_b is negative everywhere on Γ if h_{bg} is. We have

$$h_{bg}(b,g) = -8bg + 9b + 9g - 10 + (4b - 2g - 3)\log g + (4g - 2b - 3)\log b.$$

Note that $h_{bg}(b,1) \equiv b - 1 + (1-2b)\log b$, which is negative for all $b \in (0,1)$. Hence h_{bg} is negative everywhere on Γ if h_{bgg} is. We have

$$h_{bgg}(b,g) = -8b + 7 + \frac{4b-3}{g} - 2\log g + 4\log b.$$

Note that

$$h_{bgg}\left(\frac{1}{4}, g\right) \equiv 5 + 4\log\left(\frac{1}{4}\right) - \frac{2}{g} - 2\log g,$$

which is negative for all $g > 1$ since $5 + 4\log(1/4) < 0$. Now note that

$$h_{bggb}(b,g) = -8 + \frac{4}{g} + \frac{4}{b}$$

is positive whenever $b < 1/4$ and $g > 1$. It follows that h_{bgg} is negative whenever $b \in (0, 1/4]$ and $g \in (1, \infty)$.

Now consider $b > 1/4$. Note that $h_{bgg}(b,1) \equiv 4(1 - b + \log b)$, which is negative for all $b \in (0,1)$. Note also that

$$h_{bggg}(b,g) = -\frac{4b-3}{g^2} - \frac{2}{g},$$

which, for $g > 1$, is negative if and only if $g > 3/2 - 2b$, which holds if $b > 1/4$ and $g > 1$. It follows that h_{bgg} is negative whenever $b \in (1/4, 1)$ and $g \in (1, \infty)$. Combining this with the above gives that h_{bgg} is negative everywhere on Γ, as needed. $\square$

We first argue that it suffices to show that, under the conditions stated in the proposition, $E[\lambda \mid \text{participate}]$ is increasing in m. To see this, note first that an equivalent statement of part (ii) of the proposition is that if m_1 and m_2 are such that $m_2 > m_1$ and $p(m_i, \lambda) \in [0,1)$ for all $\lambda \in [\underline{\lambda}, \overline{\lambda}]$ and $i = 1,2$, then the distribution of λ conditional on participation at m_2 first-order stochastically dominates (FOSDs) that at m_1. Let Ψ denote the distribution of λ. For each $i = 1,2$, let F_i denote the distribution function for λ conditional on participation at m_i. Note that F_1 and F_2 are continuous since Ψ is. Suppose that F_2 does not FOSD F_1; we will show that this implies that, for some distribution of λ satisfying the conditions of the proposition, $E[\lambda \mid \text{participate}]$ is not increasing in m. Then there exists some λ_0 such that $F_1(\lambda_0) < F_2(\lambda_0)$. By continuity of F_1 and F_2 and the fact that they agree at $\underline{\lambda}$ and $\overline{\lambda}$, there exists an interval $[a,b]$ containing λ_0 such that $F_1(a) = F_2(a)$, $F_1(b) = F_2(b)$, and $F_1(x) < F_2(x)$ for all $x \in (a,b)$. Thus, for each $\lambda \in (a,b)$,

$$F_1(\lambda|[a,b]) = \frac{F_1(\lambda) - F_1(a)}{F_1(b) - F_1(a)} = \frac{F_1(\lambda) - F_2(a)}{F_2(b) - F_2(a)} < \frac{F_2(\lambda) - F_2(a)}{F_2(b) - F_2(a)} = F_2(\lambda|[a,b]),$$

and hence $F_1(\cdot \mid [a,b])$ FOSDs $F_2(\cdot \mid [a,b])$. Note that $F_i(\cdot \mid [a,b])$ is the distribution of λ conditional on participation at m_i when the prior distribution of λ is $\Psi(\cdot \mid [a,b])$. It follows that, for the prior distribution $\Psi(\cdot \mid [a,b])$, $E[\lambda \mid \text{participate}]$ is higher at m_1 than it is at m_2, as needed.

We now show that $E[\lambda \mid \text{participate}]$ is indeed increasing in m. First suppose $p(m,\lambda) > 0$ for all $\lambda \in [\underline{\lambda}, \overline{\lambda}]$. We have

$$E[\lambda \mid \text{participate}] = \frac{E[\lambda p]}{E[p]}.$$

Differentiating with respect to m gives

$$\frac{\partial}{\partial m} E[\lambda \mid \text{participate}] = \frac{E[p]E\left[\lambda \frac{\partial p}{\partial m}\right] - E[\lambda p]E\left[\frac{\partial p}{\partial m}\right]}{(E[p])^2}.$$

This is positive if and only if the numerator is positive, which, since p and $\partial p / \partial m$ are positive for each λ, may be rewritten as

$$\frac{E\left[\lambda \frac{\partial p}{\partial m}\right]}{E\left[\frac{\partial p}{\partial m}\right]} > \frac{E[\lambda p]}{E[p]}.$$

By Lemma 1 (with $X = \lambda$, $f = \frac{\partial p}{\partial m}$, and $g = p$), it suffices to show that

$$\frac{1}{p}\frac{\partial p}{\partial m}$$

is increasing in λ. Differentiating with respect to λ gives

$$\frac{\partial}{\partial \lambda}\left(\frac{1}{p}\frac{\partial p}{\partial m}\right) = -\frac{1}{p^2}\frac{\partial p}{\partial \lambda}\frac{\partial p}{\partial m} + \frac{1}{p}\frac{\partial^2 p}{\partial \lambda \partial m}.$$

Thus it suffices to show that

$$p\frac{\partial^2 p}{\partial \lambda \partial m} > \frac{\partial p}{\partial \lambda}\frac{\partial p}{\partial m}. \tag{3}$$

Differentiating (2) gives

$$\begin{aligned}\frac{\partial p}{\partial \lambda}\frac{\partial p}{\partial m} &= \left(\frac{1-\mu}{(g-1)^2}\left(-\frac{g}{\lambda}\log g\right) + \frac{\mu}{(b-1)^2}\left(-\frac{b}{\lambda}\log b\right)\right)\left(\frac{1-\mu}{(g-1)^2}\frac{g}{\lambda} + \frac{\mu}{(b-1)^2}\frac{b}{\lambda}\right) \\ &= -(1-\mu)^2\frac{g^2\log g}{\lambda^2(g-1)^4} - \mu(1-\mu)\frac{bg\log b + bg\log g}{\lambda^2(b-1)^2(g-1)^2} - \mu^2\frac{b^2\log b}{\lambda^2(b-1)^4},\end{aligned} \tag{4}$$

and

$$\frac{\partial^2 p}{\partial \lambda \partial m} = (1-\mu)\left(\frac{g(g+1)\log g - g(g-1)}{\lambda^2(g-1)^3}\right) + \mu\left(\frac{b(b+1)\log b - b(b-1)}{\lambda^2(b-1)^3}\right).$$

Multiplying the latter by the expression for p in (2) and expanding leads to

$$
\begin{aligned}
p\frac{\partial^2 p}{\partial\lambda\partial m} = -(1-\mu)^2 &\left(\frac{g(g+1)\log g - g(g-1)}{\lambda^2(g-1)^4}\right) \\
&- \mu(1-\mu)\left(\frac{g(g+1)\log g - g(g-1)}{\lambda^2(b-1)(g-1)^3} + \frac{b(b+1)\log b - b(b-1)}{\lambda^2(b-1)^3(g-1)}\right) \\
&\qquad\qquad - \mu^2\left(\frac{b(b+1)\log b - b(b-1)}{\lambda^2(b-1)^4}\right). \qquad (5)
\end{aligned}
$$

Comparing the $(1-\mu)^2$ terms in (4) and (5), we see that the latter is larger if and only if

$$-(g(g+1)\log g - g(g-1)) > -g^2\log g,$$

or, equivalently, if

$$g-1-\log g > 0,$$

which holds for all $g>1$. Similarly, comparing the μ^2 terms in (4) and (5), we see that the latter is larger if and only if

$$b-1-\log b > 0,$$

which holds for all $b\in(0,1)$.

Finally, for the $\mu(1-\mu)$ terms, that in (5) is larger than that in (4) if and only if

$$-\left(\frac{g(g+1)\log g - g(g-1)}{\lambda^2(b-1)(g-1)^3} + \frac{b(b+1)\log b - b(b-1)}{\lambda^2(b-1)^3(g-1)}\right) > -\frac{bg\log b + bg\log g}{\lambda^2(b-1)^2(g-1)^2}.$$

Rearranging gives the equivalent inequality

$$
\begin{aligned}
&(b-1)(g-1)bg(\log b + \log g) \\
&\qquad > (b-1)^2\left(g(g+1)\log g - g(g-1)\right) + (g-1)^2\left(b(b+1)\log b - b(b-1)\right).
\end{aligned}
$$

Further rearranging leads to

$$-\left((b-1)g + b(g-1)\right)(b-1)(g-1) + (b-1)g(2b-g-1)\log g + (g-1)b(2g-b-1)\log b < 0,$$

which, by Lemma 2, holds for all $b\in(0,1)$ and $g\in(1,\infty)$.

Combining these three comparisons, we see that (3) holds for all b and g.

Now suppose $p(m,\lambda)=0$ for some $\lambda\in[\underline{\lambda},\overline{\lambda}]$. By Lemma 2 of Matějka and McKay (2015), for any such λ, $p=0$ maximizes

$$\mu\log(pg+1-p) + (1-\mu)\log(pb+1-p).$$

The corresponding first-order condition (evaluated at $p = 0$) is

$$\mu g + (1-\mu)b \leq 1. \tag{6}$$

Suppose this holds with equality; that is, suppose $\mu g + (1-\mu)b = 1$. The derivative of the left-hand side of (6) with respect to λ is

$$-\mu g \frac{\log g}{\lambda} - (1-\mu)b\frac{\log b}{\lambda}.$$

Since $f(x) = -x \log x$ is a strictly concave function, Jensen's Inequality implies that

$$-\mu g \frac{\log g}{\lambda} - (1-\mu)b\frac{\log b}{\lambda} < -\frac{1}{\lambda}(\mu g + (1-\mu)b) \log\left(\mu g + (1-\mu)b\right),$$

the right-hand side of which is equal to 0 whenever (6) holds with equality. It follows that if there is some λ for which $p = 0$, then there is a cutoff value $\tilde{\lambda}$ such that $p = 0$ if and only if $\lambda > \tilde{\lambda}$.

Since the result holds if $p > 0$ for all $\lambda \in [\underline{\lambda}, \overline{\lambda}]$, it also holds if we condition on $\lambda \in [\underline{\lambda}, \tilde{\lambda}]$. Removing this condition only strengthens the result since $\tilde{\lambda}$ is increasing in m (which follows from the fact that the left-hand side of (6) is increasing in m).

A.2 Proof of Proposition 2

Caplin and Dean (2013) show that the agent's choice problem is equivalent to the choice of posterior beliefs $(\gamma_{\text{part}}, \gamma_{\text{abst}})$ solving

$$\max_{\gamma_{\text{part}}, \gamma_{\text{abst}}, p \in [0,1]} pN_{\text{part}} + (1-p)N_{\text{abst}} \quad \text{s.t.} \quad p\gamma_{\text{part}} + (1-p)\gamma_{\text{abst}} = \mu, \tag{7}$$

where

$$\begin{aligned} & N_{\text{abst}} := -\lambda h(\gamma_{\text{abst}}) \\ \text{and} \quad & N_{\text{part}} := \gamma_{\text{part}}(\pi_G + m) + (1-\gamma_{\text{part}})(\pi_B + m) - \lambda h(\gamma_{\text{part}}) \end{aligned}$$

are the net utilities associated with the two posteriors (under the assumption that the agent abstains at γ_{abst} and participates at γ_{part}).

Caplin and Dean (2013) show that the solution to (7) is given by the posteriors γ_{part} and γ_{abst} that support the concavification of the upper envelope of the net utility functions, as in Aumann, Maschler, and Stearns (1995) and Gentzkow and Kamenica (2011), with $\gamma_{\text{part}} \geq \mu \geq \gamma_{\text{abst}}$. Under the assumption that each action is chosen with positive probability, these inequalities are strict, and participation is optimal at posterior γ_{part} while abstention is optimal at posterior γ_{abst}.

By concavification, the solution satisfies two conditions. First, the slopes of the tangent lines to the net utility function at γ_{abst} and γ_{part} must coincide:

$$-\lambda h'(\gamma_{\text{abst}}) = \Delta - \lambda h'(\gamma_{\text{part}}), \tag{8}$$

where $\Delta := \pi_G - \pi_B$. Second, the tangent line to the net utility function at γ_{abst} has the same value at γ_{part} as the net utility function itself:

$$-\lambda h(\gamma_{\text{abst}}) - (\gamma_{\text{part}} - \gamma_{\text{abst}})\lambda h'(\gamma_{\text{abst}}) = \Delta\gamma_{\text{part}} + \pi_B + m - \lambda h(\gamma_{\text{part}}). \tag{9}$$

Taking derivatives of (8) and (9) with respect to λ, we obtain

$$-h'(\gamma_{\text{abst}}) - \lambda h''(\gamma_{\text{abst}})\frac{\partial\gamma_{\text{abst}}}{\partial\lambda} = -h'(\gamma_{\text{part}}) - \lambda h''(\gamma_{\text{part}})\frac{\partial\gamma_{\text{part}}}{\partial\lambda} \tag{10}$$

and

$$\begin{aligned} -h(\gamma_{\text{abst}}) - \lambda h'(\gamma_{\text{abst}})\frac{\partial\gamma_{\text{abst}}}{\partial\lambda} = &-h(\gamma_{\text{part}}) - \lambda h'(\gamma_{\text{part}})\frac{\partial\gamma_{\text{part}}}{\partial\lambda} + \left(\frac{\partial\gamma_{\text{part}}}{\partial\lambda} - \frac{\partial\gamma_{\text{abst}}}{\partial\lambda}\right)\lambda h'(\gamma_{\text{abst}}) \\ &+ (\gamma_{\text{part}} - \gamma_{\text{abst}})\left(h'(\gamma_{\text{abst}}) + \lambda h''(\gamma_{\text{abst}})\frac{\partial\gamma_{\text{abst}}}{\partial\lambda}\right) + \Delta\frac{\partial\gamma_{\text{part}}}{\partial\lambda}. \end{aligned} \tag{11}$$

Cancelling $-\lambda h'(\gamma_{\text{abst}})\frac{\partial\gamma_{\text{abst}}}{\partial\lambda}$ from both sides of (11) and rearranging yields

$$h(\gamma_{\text{part}}) - h(\gamma_{\text{abst}}) = \frac{\partial\gamma_{\text{part}}}{\partial\lambda}\left[\lambda h'(\gamma_{\text{abst}}) - \lambda h'(\gamma_{\text{part}}) + \Delta\right] + (\gamma_{\text{part}} - \gamma_{\text{abst}})\left(h'(\gamma_{\text{abst}}) + \lambda h''(\gamma_{\text{abst}})\frac{\partial\gamma_{\text{abst}}}{\partial\lambda}\right).$$

By (8), the term in square brackets is equal to 0. Further rearranging yields

$$\begin{aligned} (\gamma_{\text{part}} - \gamma_{\text{abst}})\lambda h''(\gamma_{\text{abst}})\frac{\partial\gamma_{\text{abst}}}{\partial\lambda} &= h(\gamma_{\text{part}}) - h(\gamma_{\text{abst}}) - (\gamma_{\text{part}} - \gamma_{\text{abst}})h'(\gamma_{\text{abst}}) \\ &= \frac{1}{\lambda}(\Delta\gamma_{\text{part}} + \pi_B + m), \end{aligned} \tag{12}$$

where the second line follows by substituting from (9). Since participation is optimal at γ_{part}, we have $\Delta\gamma_{\text{part}} + \pi_B + m = \gamma_{\text{part}}\pi_G + (1 - \gamma_{\text{part}})\pi_B + m > 0$.

Rearranging (10) and substituting $h'(\gamma_{\text{part}}) - h'(\gamma_{\text{abst}}) = \frac{\Delta}{\lambda}$ from (8) leads to

$$\begin{aligned} \lambda h''(\gamma_{\text{part}})\frac{\partial\gamma_{\text{part}}}{\partial\lambda} &= \lambda h''(\gamma_{\text{abst}})\frac{\partial\gamma_{\text{abst}}}{\partial\lambda} - \frac{\Delta}{\lambda} \\ &= \frac{1}{\lambda}\left(\frac{\gamma_{\text{abst}}\pi_G + (1 - \gamma_{\text{abst}})\pi_B + m}{\gamma_{\text{part}} - \gamma_{\text{abst}}}\right), \end{aligned}$$

where the second equality subsitutes for $\lambda h''(\gamma_{\text{abst}})\frac{\partial\gamma_{\text{abst}}}{\partial\lambda}$ using (12). Because $h'' > 0$ and because the quantity on the right-hand side is negative, we conclude that $\frac{\partial\gamma_{\text{part}}}{\partial\lambda} < 0$.

A.3 Proof of Proposition 3

From equation (2) we have

$$p(m, a\lambda) = -\mu f(\pi_B + m, c\eta) - (1-\mu) f(\pi_G + m, c\eta),$$

where $\eta = 1/\lambda$, $c = 1/a$, and $f(x,\eta) = \frac{1}{e^{\eta x}-1}$. Thus it suffices to show that

$$\frac{\partial}{\partial c}\bigg|_{c=1}\left[-c\frac{1}{\lambda^2}\frac{\partial^2}{\partial\eta\partial m} f(x, c\eta)\right] \geq 0, \tag{13}$$

and that this inequality is strict for at least one $x \in \{\pi_B + m, \pi_G + m\}$. Differentiating the left-hand side leads to the equivalent expression

$$\frac{\partial}{\partial c}\bigg|_{c=1}\left[-\frac{c}{\lambda^2}\frac{e^{cx\eta}\left(cx\eta + 1 + e^{cx\eta}(cx\eta - 1)\right)}{(e^{cx\eta}-1)^3}\right]$$
$$= \frac{1}{8\lambda^2}\left(\sinh\left(\frac{z}{2}\right)\right)^{-4}\big(-1 + 2z^2 + (1+z^2)\cosh(z) - 3z\sinh(z)\big),$$

where $z = x\eta$. Because the above expression is symmetric (in the sense that each side yields the same value, regardless of whether it is evaluated at z or at $-z$, for all z), it suffices to show that it is positive whenever z is (it holds trivially for $z = 0$). This expression is positive if and only if

$$z^2(\cosh(z) + 2) + \cosh(z) > 1 + 3z\sinh(z).$$

Because $\cosh(z) \geq 1$ for all z, it suffices to show that $z^2\big(\cosh(z) + 2\big) > 3z\sinh(z)$, or, equivalently,

$$\cosh(z) + 2 > \frac{3}{z}\sinh(z). \tag{14}$$

To prove this inequality, we employ the fact that sinh and cosh are analytic functions. Inserting their series representations, we get

$$\frac{3}{z}\sinh(z) = \frac{3}{z}\sum_{k=0}^{\infty}\frac{z^{2k+1}}{(2k+1)!} = 3 + 3\sum_{k=1}^{\infty}\frac{z^{2k}}{(2k)!}\frac{1}{2k+1} \leq 3 + \sum_{k=1}^{\infty}\frac{z^{2k}}{(2k)!} = 2 + \cosh(z), \tag{15}$$

as needed.

Finally, the two sides of inequality (14) are equal only if $z = 1$. Because $\pi_B + m < \pi_G + m$, inequality (13) is strict for at least one $x \in \{\pi_B + m, \pi_G + m\}$.

A.4 Heterogeneous priors

Proposition 4. *(Robustness to dispersion in prior) Fix m, π_G, and π_B. Consider a population with joint distribution of priors and costs of information $f(\mu, \lambda)$ such that $0 < p(m, \lambda, \mu) < 1$ for all*

$(\lambda, \mu) \in \operatorname{supp}(f)$. *For each* λ, *let* $\nu(\lambda) = \int \mu f(\mu, \lambda) d\mu$. *Then,*

$$\int p(m, \lambda, \mu) f(\lambda, \mu) d\mu = p\big(\lambda, \nu(\lambda)\big)$$

Proof. Let $\rho_1 = P(s = G|\text{participate})$ and $\rho_0 = P(s = G|\text{abstain})$ denote the optimal posteriors. By the law of iterated expectations, $\mu = p\rho_1 + (1-p)\rho_0$. The participation probability can thus be written as a function of the chosen posteriors,

$$p(m; \lambda) = \frac{\mu - \rho_0}{\rho_1 - \rho_0} \tag{16}$$

Posterior separability implies that the optimal ρ_1 and ρ_0 are independent of μ as long as $0 < p(m; \lambda) < 1$. The claim thus follows from the fact that (16) is linear in μ. $\square$

B Simulations

B.1 Information cost functions

In this section we test the robustness of our main results, stated in Proposition 1, regarding alternative functional form assumptions on the costs of information acquisition. (Recall that Proposition 2 is formally valid for the entire class of posterior-separable cost functions.)

We simulate the model for the following four cost-of-information functions studied in the recent theoretical literature on decision making under rational inattention (Caplin, Dean, and Leahy, 2017; Morris and Strack, 2017). In each case, the cost of the information associated with a pair of state-contingent choice probabilities (p_G, p_B) is given by $c(p_G, p_B) = h(\mu) - ph(\gamma_{\text{part}}) - (1-p)h(\gamma_{\text{abst}})$, where γ_{part} and γ_{abst} are the posteriors in case of participation and abstention, respectively. The cost functions differ by the functional form of h, which can take the following forms.

- Shannon costs: $h_{\text{Shannon}}(x) = x\log(x) + (1-x)\log(1-x)$.
- Logit costs: $h_{\text{logit}}(x) = x\text{logit}(x) + (1-x)\text{logit}(1-x)$, where $\text{logit}(y) = \log\left(\frac{y}{1-y}\right)$.
- Tsallis costs: $h_{\text{Tsallis}}(x,\sigma) = \frac{1}{\sigma-1}\left(x(1-x^{\sigma-1}) + (1-x)(1-(1-x)^{\sigma-1})\right) = \frac{1}{\sigma-1}\left(1 - x^{\sigma} - (1-x)^{\sigma}\right)$ for $\sigma \in \mathbb{R}$, $\sigma \neq 1$. Note that as $\sigma \to 1$, $h_{\text{Tsallis}}(x,\sigma) \to h_{\text{Shannon}}(x)$.
- Renyi costs: $h_{\text{Renyi}}(x,\sigma) = \frac{1}{\sigma-1}\log\left(x^{\sigma} + (1-x)^{\sigma}\right)$, for $\sigma > 0$, $\sigma \neq 1$. Note that as $\sigma \to 1$, $h_{\text{Renyi}}(x,\sigma) \to h_{\text{Shannon}}(x)$.

Our analytical results apply to the case of Shannon costs, which we include here for reference. The logit case is of interest because it corresponds to the Wald (1947) sequential information acquisition problem with linear time costs (Morris and Strack, 2017). Tsallis entropy is of interest because the selection of parameter σ allows us to differentially vary the relative cost of marginal changes in the posterior depending on the distance between the posterior and the prior. In our simulations, $\sigma = 2$ is a case in which the relative cost of adjusting posteriors that are near the prior is low (h has a U-shaped appearance), and $\sigma = 0.1$ is a case in which that relative cost is high (h has more of a V-shaped appearance). Renyi entropy is of interest because it is not separable across states. We parametrize these costs with $\sigma = 2$.

The results are shown in Figure B.7, which displays supply curves and the fraction of high-cost individuals amongst participants for three different prior probabilities, $\mu \in \{0.1, 0.5, 0.9\}$. We derive the fraction of high-cost participants under the assumption that both types are equally prevalent in the population. In each of the first four cases, the supply curve is steeper for the high-cost type than for the low-cost type as soon as it is interior for both types, paralleling the analytical result for the case of Shannon costs in Proposition 1 (i). For the case of Tsallis entropy, we additionally observe that if $\sigma = 2$ and information acquisition costs are low ($\lambda = 0.2$), the supply curve is flat at the level of the prior belief μ. This indicates perfect information acquisition. The fifth case, Renyi costs, is different. For this cost function, the low-cost type sometimes responds more strongly to a change in incentive payments than does the high-cost type. This tends to occur near regions of perfect information acquisition.

Regarding the robustness of part (ii) of Proposition 1, we again find in each of the first four cases that the fraction of high-cost individuals among participants monotonically increases until incentive payments are so high that high-cost individuals participate with probability one. Again, behavior with Renyi costs exhibits a pattern different from that under Shannon costs; the composition of participants no longer changes monotonically as the incentive payment increases, even in regions in which both types participate with an interior probability. These results are suggestive regarding the extent of the generality of the results we have analytically derived for the Shannon case.

B.2 Normal signals

Models that employ noisy signals about an imperfectly known state of the world often consider the case of normally distributed signals (e.g. Morris and Shin (2002)). Here, we explore the robustness of our findings in a case with normal signals.

Setting As in the main text, an agent decides whether or not to participate in a transaction in exchange for a payment m. There are two states $s \in \{G, B\}$ with prior distribution $P(s = G) = \mu$. If the agent participates in state s, he receives utility $\pi_s + m$, which is positive if $s = G$ and negative otherwise. Non-participation gives utility 0.

The information acquisition technology differs from that in the main text. The agent observes a stochastic signal n that is normally distributed. If $s = G$, the mean of the signal is 1, if $s = B$, the mean is 0. The agent chooses the variance σ^2 of the signal. We assume that lower variance is more costly and, for simplicity, consider the functional form $c(\sigma) = \lambda \frac{1}{\sigma}$, where λ captures individual heterogeneity in information acquisition costs. As in the main text, information acquisition costs are discounted from the agent's utility.

Analysis The posterior belief of the agent after observing signal realization n is given by

$$Pr(s = G|n) = \frac{\mu \frac{1}{\sqrt{2\pi}\sigma} \exp(-\frac{(n-1)^2}{2\sigma^2})}{\mu \frac{1}{\sqrt{2\pi}\sigma} \exp(-\frac{(n-1)^2}{2\sigma^2}) + (1-\mu)\frac{1}{\sqrt{2\pi}\sigma} \exp(-\frac{n^2}{2\sigma^2})} = \frac{\mu}{\mu + (1-\mu)\exp(\frac{1-2n}{2\sigma^2})}.$$

Conditional on n, the agent will participate if

$$(\pi_G + m)Pr(s = G|n) + (\pi_B + m)Pr(s = B|n) \geq 0$$
$$\iff n \geq \frac{1}{2} - \sigma^2 \log \gamma,$$

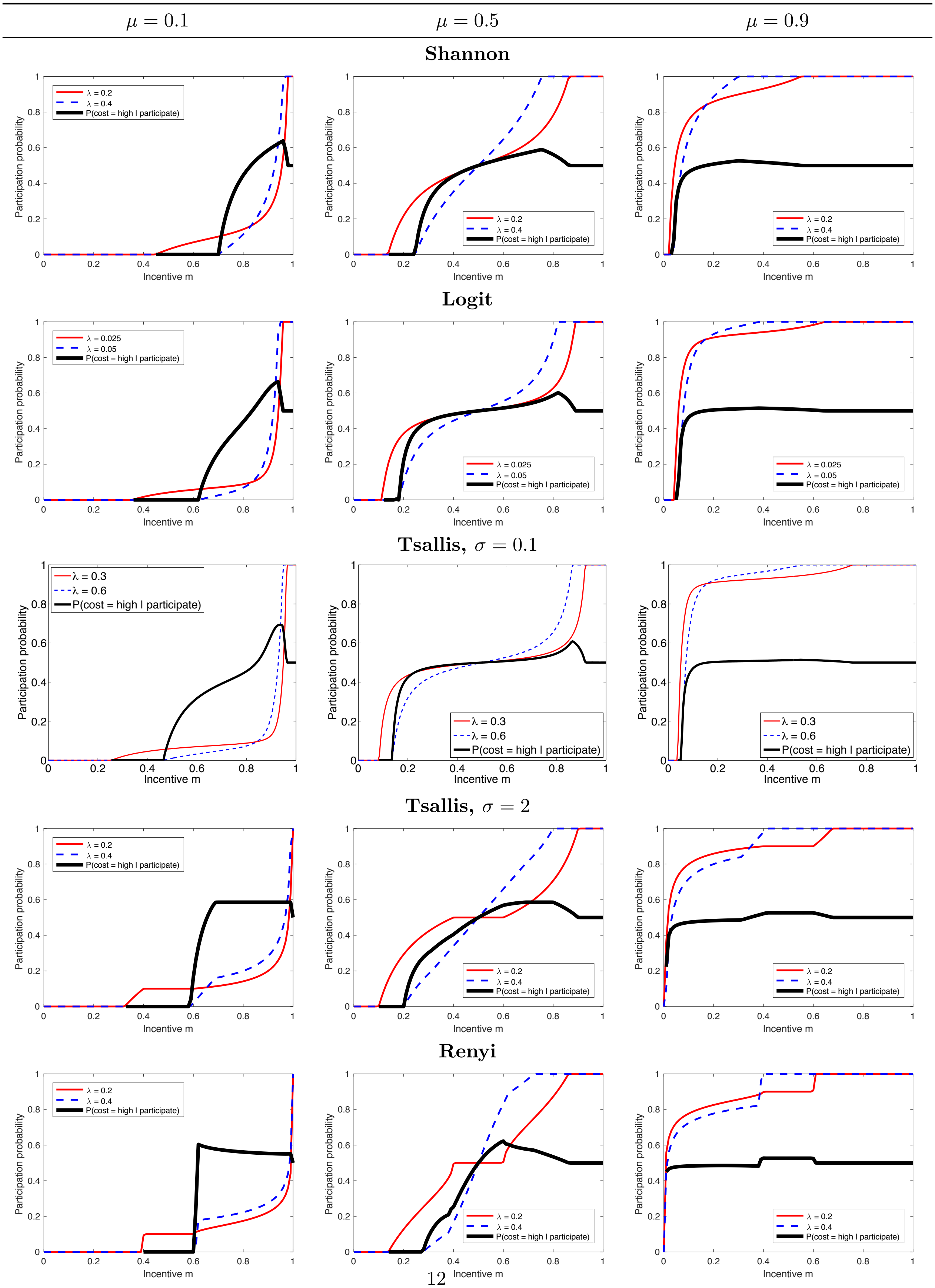


Figure B.7: Simulation tests of Proposition 1 with Shannon, logit, Tsallis, and Renyi cost functions.

where $\gamma = -\frac{\mu(\pi_G+m)}{(1-\mu)(\pi_B+m)}$. If the agent finds it optimal to base her decision on a positive amount of information, the state-dependent participation probabilities are thus given by $p_G = 1-\Phi\left(\frac{-\frac{1}{2}-\sigma^2\log\gamma}{\sigma}\right)$ and $p_B = 1-\Phi\left(\frac{\frac{1}{2}-\sigma^2\log\gamma}{\sigma}\right)$. If the agent finds it optimal to reach a decision without acquiring any information, she will participate with probability 1 if $\mu(\pi_G+m)+(1-\mu)(\pi_B+m)\geq 0$, and abstain otherwise.

Simulation Figure B.8 shows the supply curves implied by this model for $\mu \in \{0.1, 0.5, 0.9\}$, and two levels of λ each. The figures are consistent with both parts of Proposition 1. First, supply increases more steeply for the high-cost type whenever it is interior. Second, as long as neither type participates with probability 1, the probability that a participant is a high-cost type increases with the payment m. We have not found any counterexamples for a wide range of alternative parameter values we have checked.

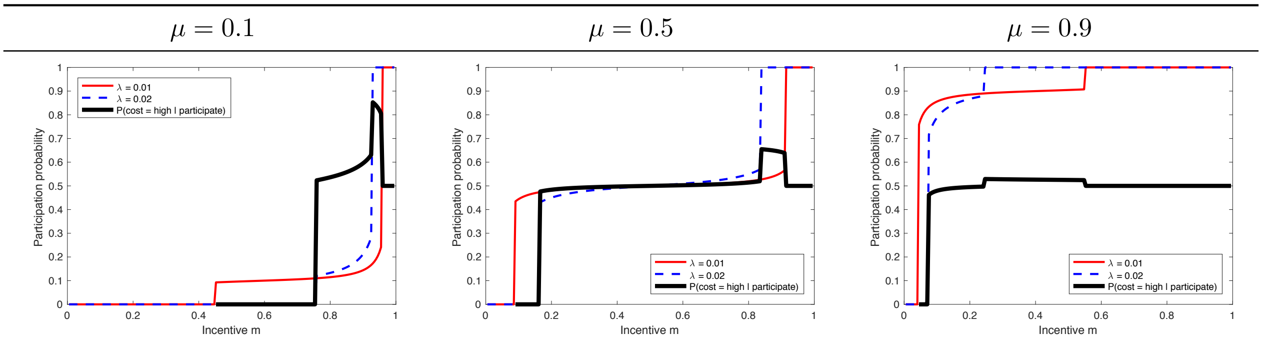


Figure B.8: Simulation tests of Proposition 1 in a model with normal signals.

C Regression Specifications

Here, we detail the specifications of the regressions in the main text. In all specifications, we cluster standard errors on the subject level.

Selection Let $m \in \{1, 2, 3\}$ be an index corresponding to payment amounts of €2, 6, and 10, respectively, and let a denote the contextual cost of information acquisition, with $a = 1, 2, 3$ for the short, medium, and long lists of calculations, respectively. Let d be an indicator that equals 1 for the three information cost treatments and 0 for the fixed information treatment. Let $t = (m, a, d)$ be a treatment indicator. Finally, let X be a vector of control variables, which varies across specifications, but always includes session and order fixed effects. Specifically, the order j_i of treatment t for subject i counts the number of decisions the subject has made up to and including the current decision. The order therefore takes on an integer value between 1 and 18; thus we include 17 order fixed effects.

In columns 1 and 3 of Table 3, we run the following specification *on the sample of observations in which subjects take the gamble*:

$$\lambda_i = \beta_0 + \beta_1 m + \delta' X_i + \epsilon_i,$$

where λ_i is the percentile rank of an individual's mean reservation price across the four elicitations, and X_i includes session, order, and cost treatment fixed effects.

In column 2, we additionally control for risk preferences and behavior in the fixed information treatment, as follows:

$$\lambda_i = \alpha_0 + \alpha_1 m + d \cdot \left[\beta_0 + \beta_1 m\right] + r \cdot \left[\gamma_0 + \gamma_1 m\right] + \delta' X_i + \epsilon_i.$$

The main parameter of interest is again β_1, which measures the extent to which the composition of the sample of participants changes because of the endogenous information acquisition.

In columns 1 and 3 of Table 5, we further let the effect of the payment m depend on the contextual information acquisition cost parameter a. We estimate

$$\lambda_i = \beta_0 + \beta_1 m + \beta_2 a + \beta_3 \cdot m \cdot a + \delta' X_i + \epsilon_i.$$

The parameter of interest here is β_3, which measures the difference in how quickly the composition of participants changes with the payment as we vary the contextual information cost parameter. In column 2 of Table 5, we also control for risk aversion and behavior in the fixed information treatment, as follows:

$$\lambda_i = \alpha_0 + \alpha_1 m + d \cdot \left[\beta_0 + \beta_1 m + \beta_2 a + \beta_3 \cdot m \cdot a\right] + r \cdot \left[\gamma_0 + \gamma_1 m + \gamma_2 a + \gamma_3 \cdot m \cdot a\right] + \delta' X_i + \epsilon_i.$$

In this specification, we do not include cost fixed effects in X_i.

In columns 1, 2, 5, and 6 of Table 6, we estimate the above four models, but replace λ_i with educational demographic and cognitive ability variables, respectively. Whenever the dependent variable is cognitive ability, we also include a control variable for the amount of time a subject takes to answer the Raven's matrix questions.

Supply curves In columns 3 and 4 of Table 2, we run the following specification:

$$y_{i,t} = \beta_0 + \beta_1 a + \beta_2 m + \beta_3 \cdot a \cdot m + \delta' X + \epsilon_{i,t},$$

where $y_{i,t} = 1$ if subject i accepts the bet in treatment t, and 0 otherwise.

Table 3 employs a similar specification. For the individual-level measure of information costs, we set $\lambda_i = 1$ if subject i's mean reservation price across the four price lists is above the median, and $\lambda_i = 0$ otherwise. In this table, the vector of controls, X, includes cost-treatment fixed effects (i.e. a binary indicator for all but one level of a).

In columns 4 and 6, we estimate

$$y_{i,t} = \beta_0 + \beta_1 \lambda + \beta_2 m + \beta_3 \cdot \lambda \cdot m + \delta' X + \epsilon_{i,t}. \tag{17}$$

In column 5, we additionally control for risk aversion and for behavior in the fixed information treatment, as follows:

$$\begin{aligned} y_{i,t} = \alpha_0 + \alpha_1 \lambda_i + \alpha_2 m + \alpha_3 \cdot \lambda_i \cdot m + d \cdot \left[\beta_0 + \beta_1 \lambda_i + \beta_2 m + \beta_3 \cdot \lambda_i \cdot m\right] \\ + r \cdot \left[\gamma_0 + \gamma_1 \lambda_i + \gamma_2 m + \gamma_3 \cdot \lambda_i \cdot m\right] + \delta' X_i + \epsilon_{i,t}, \end{aligned} \tag{18}$$

where r_i denotes individual i's percentile rank of his or her mean certainty equivalent across the nine risk preference elicitation tasks. The parameter of interest is again β_3. With this specification, β_3 isolates the effect of information costs and incentive payments in addition to what is due to differential conclusions drawn from a given, costless piece of information alone, and in addition to what can be explained by correlation between risk preferences and individual-specific information costs λ_i.

Table 5 uses a similar specification, but instead of including fixed effects for contextual information costs, we interact the predictors in model (17) with contextual information costs a. In columns 4 and 6, we estimate

$$y_{i,t} = \alpha_0 + \alpha_1 \lambda_i + \alpha_2 m + \alpha_3 \cdot \lambda_i \cdot m + a \cdot d \cdot \left[\beta_0 + \beta_1 \lambda_i + \beta_2 m + \beta_3 \cdot \lambda_i \cdot m\right] + \gamma' X + \epsilon_{i,t},$$

and in column 5, we estimate

$$
\begin{aligned}
y_{i,t} = \alpha_0 + \alpha_1\lambda_i + \alpha_2 m + \alpha_3 \cdot \lambda_i \cdot m + a \cdot d \cdot \big[\beta_0 + \beta_1\lambda_i + \beta_2 m + \beta_3 \cdot \lambda_i \cdot m\big] \\
+ r \cdot \big[\gamma_0 + \gamma_1\lambda_i + \gamma_2 m + \gamma_3 \cdot \lambda_i \cdot m\big] + \delta' X_i + \epsilon_{i,t}.
\end{aligned}
$$

The main parameter of interest is again β_3.

D Experiment: Additional Materials

D.1 Laboratory sessions

Table D.7 presents details regarding each session. After analyzing the data from the sessions in May, we decided to replicate the results, with a treatment in which performance on the IQ test was incentivized. Additionally, in the first 4 sessions, we altered the number of correct/incorrect calculations in the low-cost condition. Session dates and times reflect lab availability, convenience, and the fact that subjects appeared to provide lower-quality data in late afternoon and Friday sessions.

In sessions 18 and 19, the IQ treatment that subjects performed was different from the one announced in the instructions that were read aloud. Since responses to incentives can depend significantly on expectations (Abeler, Falk, Goette, and Huffman, 2011), we discard these data.

Session	Date	Weekday	Time	#Subjects	Low-cost condition		IQ incentives
					# correct if $s = G$	# incorrect if $s = G$	
1	4/27/17	Mon	10 AM	19	12	8	No
2	5/3/17	Wed	10 AM	32	18	12	No
3	5/3/17	Wed	1 PM	29	18	12	No
4	5/3/17	Wed	4:30 PM	31	18	12	No
5	5/10/17	Wed	10 AM	32	15	10	No
6	5/10/17	Wed	1 PM	31	15	10	No
7	5/11/17	Thur	10 AM	30	15	10	No
8	5/11/17	Thur	1 PM	32	15	10	No
9	5/12/17	Fri	10 AM	32	15	10	No
10	5/12/17	Fri	1 PM	32	15	10	No
11	7/7/17	Fri	1 PM	29	15	10	Yes
12	7/10/17	Mon	10 AM	32	15	10	Yes
13	7/10/17	Mon	1 PM	32	15	10	Yes
14	7/17/17	Mon	10 AM	32	15	10	Yes
15	7/17/17	Mon	1 PM	32	15	10	Yes
16	7/18/17	Tue	10 AM	32	15	10	Yes
17	7/18/17	Tue	1 PM	32	15	10	Yes
18	7/24/17	Mon	10 AM	32	15	10	N.A.
19	7/24/17	Mon	1 PM	31	15	10	N.A.

Table D.7: Laboratory Sessions.

D.2 Order effects

There are pronounced order effects regarding the time subjects take to complete each decision. On average, they examine the first picture for over 2.7 minutes, whereas they examine the last one for just 1.2 minutes (with standard deviations in the population test subjects of 2 and 1.3 minutes, respectively). The fraction of betting-decisions that are aligned with the state is 79.5% for the first round, and 71.2% for the last round. Regressing the fraction of decisions that align with the state on the decision order yields a slope coefficient of 0.38 percentage points (SE 0.10). While this change is statistically significant, it is less pronounced than one might expect from a 60% drop in examination time. We therefore conclude that the drop in examination time includes a substantial learning component and is not merely an expression of sloppier decision making.

D.3 Experiment instructions

Note: Horizontal lines represent screen breaks. The instructions reproduced here concern the unincentivized IQ condition. In the incentivized IQ condition, subjects were told that there are three parts, that they could earn money in each of them, and that the chance of each of the parts counting for payment was 80%, 10% and 10%, respectively.

Welcome to this experiment!

Study structure and time involvement

This study has 3 parts:

1. Decision making part A
2. Logical puzzles
3. Decision making part B

Parts 1 and 2 will take you between 30 and 40 minutes to complete, and part 3 will take you approximately 10 minutes.

Payments

At the end of this study, you will be paid cash for your participation.

You start this experiment with a **budget** of **€15.** Depending on your decisions, and on luck, you can win or lose money. Money that you win will be added to your budget of €15. Money that you lose will be subtracted from your budget. The final sum of money will be paid to you in cash.

Whether you win or lose money depends on **a single decision** that you will make in one of the two decision making parts. The computer will **randomly** select the decision for which you will be paid.

Hence, you **should make every decision as if it was the one that counts – it could be the one!**

You will probably be paid for a decision from **decision making part A**. The exact probability of being paid according to that part is **80%**. The probability of being paid for a decision made in **decision making part B** is **20%**.

<< >>

ABOUT CHANCE

Some of your decision, as well as your payout, may be partially determined by chance.

We guarantee, that when we tell you that something will happen with some chance out of 100, it will happen with exactly that chance.

Rules

This is a study about individual decision making. This means that you must not talk during this study. If you have questions, raise your hand. We will come to you and answer your questions privately.

Please do not use cell phones or other electronic devices until the study is finished. Do not surf the internet and do not check your emails. Should we ascertain that you do one of these things, the rules of this study prescribe that we deduct €10 from your payout.

(Sometimes, the continue button will appear only after a few seconds.)

To start the study, please enter the password given to you by the administrator of this experiment.

losgehts

<< >>

Instructions for part 1

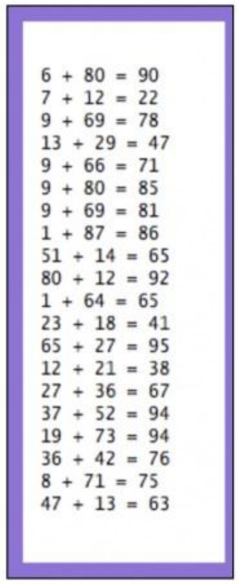
You will be able to continue with the study only if you correctly answer multiple test questions about these instructions. Therefore, it is in your best interest to pay close attention.

Part 1 one of this study has 18 rounds. Each round has two steps, the betting decision and the probability assessment.

<< >>

In each round, you will see a NEW picture like this, consisting of multiple calculations. Some calculations are CORRECT, others are WRONG.

For example, in this picture, the first two calculations are wrong and the third one is correct.



A picture can be GOOD or BAD. A picture is Good if it has more correct calculations than it has wrong calculations. Otherwise, it is Bad.

In each round, you will decide whether to bet on the picture or not. If you bet on the picture and the picture is Good, you win money. If you bet and the picture is Bad, you will lose money.

Important: A picture can be Good, even if it has many wrong calculations, as long as it has more correct calculations than wrong ones. Similarly, a picture can be Bad if it has some correct calculations, as long as it has more wrong calculations that correct ones.

In each round, it is **exactly equally likely** that you see a Good picture or a Bad picture. Each round, the computer will randomly decide which is the case.

The calculations in a picture appear in a **completely random order**!

None of this depends at all on what happened in previous rounds.

<< >>

Betting decision

This is how your betting decision works:

If you **bet** on on a picture and the **picture is Good** (i.e. it has more correct calculations than wrong ones), you **win** money.

If you **bet** on a picture and the **picture is Bad** (i.e. it has more wrong calculations than correct ones), you **lose** money.

If you do **not bet** on the picture, you **neither** win **nor** lose money.

Before deciding whether to bet on the picture or not, you may inspect the picture as long **as you wish** to get an idea of whether the picture is Good or Bad.

In a table like this one you will be able to see how much money you can win or lose if you bet on the picture in the current round. You can also see exactly how many correct and wrong calculations a Good or Bad picture contains.

	Good picture (probability 50%)	Bad picture (probability 50%)
Correct calculations	12	8
Wrong calculations	8	12
If you bet on the picture	***WIN €5***	***LOSE €5***

In each round, you will see a different picture.

How much money to win or lose by betting can differ from round to round!

If you are getting paid for a round of this part, the probability is 80% that your payment is determined by a betting decision. The remaining 20% are the probability of being paid for your answer in a probability assessment, which we will explain now.

<< >>

Probability assessment

After inspecting the picture and deciding whether to bet on the picture or not, we will ask you in each round how certain you are that the picture that you just saw was Good or Bad by showing you a question like this:

definitely good	most likely good	very likely good	quite likely good	fairly likely good	slightly likely good	slightly likely bad	fairly likely bad	quite likely bad	very likely bad	most likely bad	definitely bad
100%	90-99%	80-90%	70-80%	60-70%	50-60%	40-50%	30-40%	20-30%	10-20%	1-10%	0%
○	○	○	○	○	○	○	○	○	○	○	○

The number below each possible choice is the probability in percent with which you believe that the picture was Good.

There is a 1 in 5 chance that your earnings from this study are determined by your answer to such a question. Depending on your answer, the picture you have seen, and luck, your bonus may rise by €3 or fall by €3.

The payment procedure is designed such that it in your best interest to give your best, genuine answer to this question.

If you believe, for example, that it is about 75% likely that you have seen a good picture, it is in your best interest to select “quite likely good (70 - 80%)”. If you believe, for example that it is about 25% likely that you have seen a good picture (that is, you believe it is about 75% likely that you have seen a bad picture), then it is in your best interest to select “quite likely bad (20 - 30%)".

One of your decisions from this study will be randomly selected for payment. Thus, you will be paid for EITHER for your bet on a picture (with a 4 in 5 chance), OR for the answer you give to the questions explained above, but never for both.

Read this if you would like to know more about the payment mechanism and about WHY it is in your best interest to answer this questions according to you true beliefs.

(We will ***not*** ask you test questions about the remaining content on this page.)

The payment procedure works like this.

For most choices you can select, there is a range of chances (for example 50 - 60%). Your payment is determined by the number in middle of the range you select (for example 55%, if you select the range 50 - 60 %).

Suppose you select a choice for which the middle of the range is some number X. The computer will randomly and secretly draw another number Y between 0 and 100. If the number the computer randomly draws is the larger one, that is if Y > X, then you will win €3 with chance Y in 100 (and lose €3 if you don't win). If the number you stated is the larger one, that is, if X > Y, then you will win if the picture you have seen is good. So if X is your genuine belief that the picture you have seen was good, you will win with chance X or with chance Y, whichever of the two is larger.

WHY is it in my best interest to answer this question according to my genuine beliefs?

Simply, the reason is that you lower your chance of winning if you state a chance that is lower than you genuinely believe, and you also lower your chance of winning if you state something that is higher than you genuinely believe. So the best you can do is state what you genuinely believe.

To see why, it's best to go through an example.

Here's why you lose from stating a chance that is higher than you genuinely think is true. For example, suppose you genuinely believe the chance that the picture is good 60%, but in the survey question you select a higher chance, say 90%. Suppose the number Y that the computer draws is between 60% and 90%, let's say it is 80%. This is lower than what you've told us (you've told us 90%), so you will not play the computers' bet. Instead, you will win if the picture is good, which you genuinely think only occurs with 60% chance. The computers' bet would have given you a higher, 80%, chance instead. Hence, you hurt your chance of winning by stating the picture was more likely good than you genuinely think.

And here's why you lose from stating a lower chance than you genuinely think is true. For example, suppose again you genuinely believe the chance that the picture is good 60%, but in the survey question you select a lower chance, say 10%. Suppose the number Y that the computer draws is between 10% and 60%, let's say it is 30%. That is higher than what you told us (which is 10%), so you will play the computers' bet and win with chance 30%. That is lower than if you had instead received the bet on the picture, which, according to your genuine belief, has a 60% chance. Hence, you hurt your chance of winning by stating the picture was less likely good than you genuinely think.

Therefore, the best you can possibly do is to select exactly the answer that corresponds to your genuine beliefs.

If you have any questions about this payment mechanism, please raise your hand.

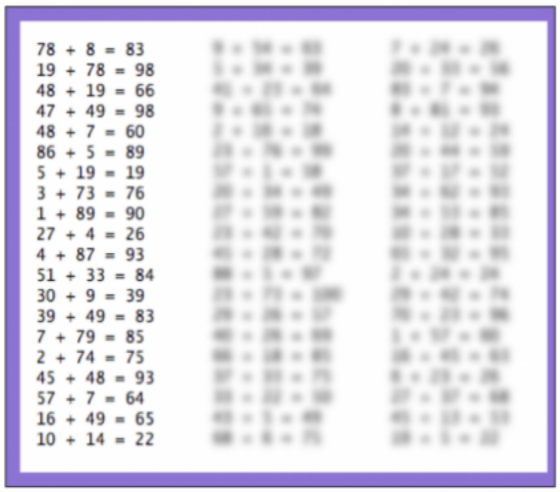
<< >>

Blurry Calculations

In some rounds, a part of the picture will be blurred, such as in the one below.

Amongst the area that is *not* blurred, the computer will automatically count for you how many correct and incorrect calculations there are, as below.

Number of CORRECT calculations in the visible part: 7
Number of WRONG calculations in the visible part: 13



You cannot see the other calculations, but they are just as relevant for winning or losing should you decide to bet on the picture. This means that winning or losing the bet depends on how many correct and wrong calculations are in the *whole* picture, counting those calculations that you cannot see.

Apart from what you can see and are told about the picture, these rounds are like all others.

<< >>

To make sure you got all of this, check all statements below that are true. You can only continue if you tick all boxes correctly.

Use the back button on the bottom if you would like to revisit the instructions.

(Do not try random combinations, there are far too many possible combinations. If you feel you understand the instructions, but still cannot continue, or have some other question, please raise your hand.)

- [ ] A picture is bad ONLY if ALL the calculations in that picture are incorrect.
- [x] A good picture has both correct and incorrect calculations (but more correct ones).
- [x] A bad picture has both correct and incorrect calculations (but more incorrect ones).
- [ ] A picture is good ONLY if ALL the calculations in that picture are correct.
- [x] At the end of the study, the computer will randomly select one decision I made. I will be paid for that and only that decision.
- [ ] When I am asked about how certain I am about a picture, I will earn most from this study if I state something a little higher than I truly think.
- [ ] When I am asked about how certain I am about a picture, I will earn most from this study if I state something a little lower than I truly think.
- [x] f a part of the picture is blurred, whether I will win or lose from taking the lottery depends on all calculations in the picture, even those that are blurry.
- [x] I can examine the picture FOR AS LONG AS I LIKE before I make a decision.
- [x] When I am asked about how certain I am about a picture, I will earn most from this study if I state exactly what I truly think.
- [ ] If a part of the picture is blurred, whether I will win or lose from taking the lottery ONLY depends on those calculations that I can see, not on those that are blurred.
- [x] If I am paid for a part with a picture, I will be paid EITHER for the bet I take on that picture, OR for the my answer to the question how certain I am about the picture I have seen, but NOT for BOTH.

<< >>

Before we begin the study, please conform your participation, if you still want to participate.

Protocol officers: Axel Ockenfels, Professor, Department of Economics, University of Cologne and Sandro Ambuehl, Assistant Professor for Management, Department of Management UTSC, Rotman School of Management, University of Toronto

PAYMENT: On average, you will receive **€10 per hour** as payment for your participation, depending on luck and the decisions that you and other participants make. All participants will receive a payment. The minimum payment is €4 show-up fee. (To understand the next paragraph, please keep in mind that the payment is *not* to be understood to be utility. You will definitely receive a payment as described in the next paragraph.)

RISKS AND BENEFITS: We do not promise that you derive any kind of benefit from this study. There are no other risks relating to this study.

SCHEDULE: Your participation in this study will take between **60 and 120 minutes**.

YOUR RIGHTS: If, after reading this form and deciding to participate in this study, know that <u>your participation is voluntary</u> and that <u>you can abort the study without any consequences or loss of due payments at all times</u>. You have the right to not give an answer to individual questions. Your privacy will be protected in all published or written works that result from this study. If you have questions about your rights as a participant, you can contact the Research Oversight and Compliance Office - Human Research Ethics Program at ethics.review@utoronto.ca, 416-946-3273.

ACCESS TO INFORMATION, PRIVACY AND PUBLICATION OF RESULTS: At the end of the experiment, you have the opportunity to enter your email address should you want to receive information about the hypotheses and results of this study as soon as it is finished. This email address is the sole identifiable information that we elicit from you and it is your right to withhold this information. All of your information will be stored on an encrypted and password protected computer for up to five years. An anonymized version of the data will be shared with coauthors and published on the Harvard dataverse platform indefinitely. The results of this study will be published in academic seminars and journals. The study in which you take part may be subjected to a quality control to ensure that applicable laws and regulations are fulfilled. Should it be selected, (one) representative(s) of the Human Research Ethics Program (HREP) may review data related to this study and/or declarations of consent as part of the audit. All informations accessed by HREP are governed by the same level of privacy as the level indicated by the research team.

CONFLICTS OF INTEREST:

None of the researchers that are involved with this study have any conflict of interest. This study is financed by the University of Cologne (Chair Prof. Dr. Axel Ockenfels).

CONTACT INFORMATION:

***If you believe that you were injured by participating in this study, please contact Sandro Ambuehl, University of Toronto, Rotman School of Management, 105 St. George Street, Toronto, M5S 3E6, sandro.ambuehl@utoronto.ca, (647) 981 63 84.

***Questions, concerns or complaints: If you have any kind of questions, concerns or complaints, the procedure, risks or benefits regarding this **research study**, please ask protocol officer Sandro Ambuehl, University of Toronto, Rotman School of Management, 105 St. George Street, Toronto, M5S 3E6, sandro.ambuehl@utoronto.ca, (647) 981 63 84.

***Independent contact: If you disagree with how this study was conducted or if you have any concerns, complaints or general questions about this research or your rights, please contact the University of Toronto Research Ethics Board, McMurrich Building, 2nd Floor, 12 Queen's Park Crescent W, Toronto, ON, M5S 1S8, ethics.review@utoronto.ca, 416-946-3273, to talk to someone who is independent of the research team.

I do **NOT** agree to participate in this study and would like to withdraw from my participation now.	I agree to participate in this experiment and would like to continue.
○	●

<< >>

The study starts now.

Your decisions are about real money.

<< >>

	Good picture (probability 50%)	Bad picture (probability 50%)
Correct calculations	55	45
Wrong calculations	45	55
If you bet on the picture	***WIN €2***	***LOSE €10***

3 + 47 = 55	62 + 11 = 73	49 + 44 = 93	23 + 31 = 54	5 + 37 = 41
23 + 50 = 73	60 + 31 = 91	31 + 64 = 95	33 + 42 = 72	19 + 71 = 90
83 + 7 = 90	77 + 5 = 82	20 + 63 = 83	40 + 2 = 40	18 + 49 = 63
66 + 8 = 78	4 + 74 = 82	3 + 77 = 77	17 + 5 = 22	20 + 43 = 64
2 + 58 = 60	8 + 49 = 55	54 + 25 = 79	35 + 39 = 72	18 + 73 = 91
31 + 61 = 92	53 + 23 = 71	5 + 39 = 48	67 + 12 = 79	32 + 52 = 89
53 + 17 = 70	11 + 5 = 14	51 + 47 = 103	38 + 12 = 49	17 + 14 = 31
18 + 38 = 60	33 + 51 = 82	21 + 69 = 85	29 + 16 = 45	33 + 7 = 44
1 + 96 = 97	34 + 59 = 92	42 + 53 = 96	62 + 15 = 77	18 + 9 = 27
13 + 73 = 86	9 + 67 = 76	7 + 40 = 47	11 + 27 = 38	5 + 22 = 27
3 + 36 = 43	11 + 23 = 34	8 + 30 = 38	15 + 74 = 92	32 + 58 = 91
7 + 68 = 70	46 + 6 = 52	5 + 33 = 38	21 + 19 = 40	62 + 29 = 91
2 + 80 = 84	38 + 49 = 87	64 + 25 = 89	51 + 30 = 81	49 + 47 = 99
25 + 33 = 58	9 + 76 = 82	32 + 16 = 43	12 + 15 = 22	15 + 28 = 38
27 + 43 = 70	53 + 16 = 69	79 + 10 = 85	57 + 28 = 87	58 + 26 = 84
34 + 7 = 36	53 + 40 = 93	29 + 4 = 33	41 + 43 = 84	71 + 18 = 84
12 + 79 = 87	68 + 27 = 95	47 + 16 = 63	8 + 17 = 30	17 + 65 = 82
48 + 41 = 89	21 + 44 = 68	3 + 74 = 77	55 + 19 = 74	18 + 75 = 96
75 + 18 = 93	17 + 81 = 95	93 + 2 = 95	64 + 23 = 86	11 + 86 = 97
28 + 50 = 83	21 + 30 = 51	80 + 17 = 97	32 + 11 = 43	35 + 22 = 56

Look at the purple image as long as you like to see if you want to bet on the image or not.

Click CONTINUE to make your decision.

(You can NOT return to this page once you have clicked CONTINUE.)

<< >>

	Good picture (probability 50%)	Bad picture (probability 50%)
Correct calculations	55	45
Wrong calculations	45	55
If you bet on the picture	***WIN €2***	***LOSE €10***

Make a decision.

- ● I bet on the picture. I WIN €2 if the purple picture is Good, and LOSE €10 if it is Bad.
- ○ I do not bet on the purple picture.

<< >>

How certain are you that the purple picture is Good?

It is...

definitely good	most likely good	very likely good	quite likely good	fairly likely good	slightly likely good	slightly likely bad	fairly likely bad	quite likely bad	very likely bad	most likely bad	definitely bad
100%	90-99%	80-90%	70-80%	60-70%	50-60%	40-50%	30-40%	20-30%	10-20%	1-10%	0%
○	○	○	●	○	○	○	○	○	○	○	○

(The number below each option is the probability in percent that you think the purpurrote image is good.)

If you like, you can go back now to change your decision whether you want to bet on the picture.

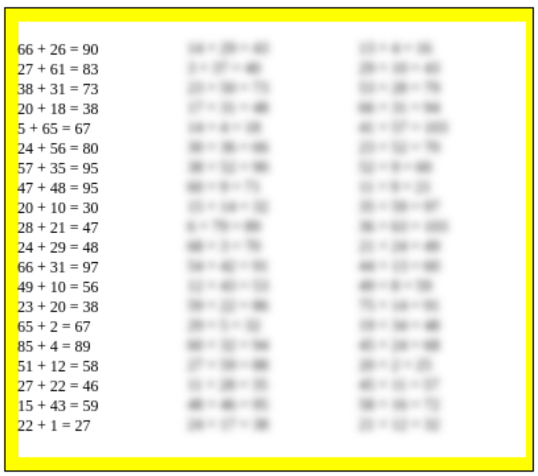
<< >>

Round 6 out of 18

	Good picture (probability 50%)	Bad picture (probability 50%)
Correct calculations	35	25
Wrong calculations	25	35
If you bet on the picture	***WIN €10***	***LOSE €2***

This picture also has 60 calculations, but you can only see 20 of them as the remainder is blurred.

Number of CORRECT calculations in the visible area: 7
Number of WRONG calculations in the visible area: 13



Look at the yellow image for as long as you like to see if you want to bet on the picture or not.

Click CONTINUE to make your decision.

(You can NOT return to this page once you have clicked CONTINUE.)

<< >>

Additional calculation tasks for bonus payment

You now have the option of solving additional calculations to earn a bonus.

You will receive this bonus payment *in addition* to your other payouts from this study.

To determine the amount of your bonus payment, you will complete four decision lists like the one below.

Are you willing to solve

60 calculations

in exchange for the given amount of money?

(One of your decisions will be randomly selected and executed!)

Solve calculations, receive €0	O	O	Don't solve, receive no bonus
Solve calculations, receive €0.25	O	O	Don't solve, receive no bonus
Solve calculations, receive €0.50	O	O	Don't solve, receive no bonus
Solve calculations, receive €0.75	O	O	Don't solve, receive no bonus
Solve calculations, receive €1	O	O	Don't solve, receive no bonus
Solve calculations, receive €1.50	O	O	Don't solve, receive no bonus
Solve calculations, receive €2	O	O	Don't solve, receive no bonus
Solve calculations, receive €2.50	O	O	Don't solve, receive no bonus
Solve calculations, receive €3	O	O	Don't solve, receive no bonus
Solve calculations, receive €3.50	O	O	Don't solve, receive no bonus
Solve calculations, receive €4	O	O	Don't solve, receive no bonus

When we speak of "60 calculations", each of these tasks is like the ones you saw in the picture before. This means that each task has a calculation like "45 + 37 = 67" and it is your task to indicate if the calculation is right or wrong. (In this example, the calculation is wrong.)

<< >>

Once you have completed all four decision lists, the computer will **randomly pick one of these lists and one of the decisions** you made. This decision will then be executed at the very end of the experiment, after you finish the part with the logic puzzle. (Your decisions have absolutely no effect on the choice that the computer makes!)

This means that if you have decided not to solve the calculations for the amount of money offered, you will end the study without having to perform further calculations. If you have agreed to solve the specified amount of calculations for the amount of money offered, you will complete the tasks and receive the corresponding additional bonus.

You will **only receive the estimated amount for the additional tasks if you solve the tasks well enough**. This means that you are allowed to solve 1 out of 10 calculations incorrectly. (For example, if you solve 30 tasks, that means you can answer three of them incorrectly.) **If you solve more than 1 in 10 tasks incorrectly**, you will not only lose the extra bonus you would have got if you had completed the task correctly but there are an additional **€** deducted from your bonus.

Example: Suppose that in the randomly chosen task you decided to solve 30 calculations for €2. If you solve at least 90% of them correctly, €2 will be added to your bonus. But if you solve less than 90% correctly, you will (i) *not* receive the 2€ you would have received had you solved at least 90% correctly and (ii) €10 will be deducted from your bonus.

A second example: Suppose that in the randomly chosen task you decided to solve 30 calculations for €8. If they solve at least 90% of them correctly, €8 will be added to your bonus. But if you solve less than 90% correctly, you will (i) *not* receive the €8 you would have received had you solved at least 90% correctly and (ii) €10 will be deducted from your bonus.

Therefore, it is in your best interests to agree to solving the additional tasks only if you are genuinely willing to solve them well, and otherwise refuse to solve the additional tasks.

Please complete the following four lists according to your true preferences.

<< >>

Are you willing to solve

30 calculations

in exchange for the given amount of money?

(One of your decisions will be randomly selected and executed!)

Solve calculations, receive €0	○	●	Don't solve, receive no bonus
Solve calculations, receive €0.25	○	●	Don't solve, receive no bonus
Solve calculations, receive €0.50	○	●	Don't solve, receive no bonus
Solve calculations, receive €0.75	○	●	Don't solve, receive no bonus
Solve calculations, receive €1	○	●	Don't solve, receive no bonus
Solve calculations, receive €1.50	○	●	Don't solve, receive no bonus
Solve calculations, receive €2	○	●	Don't solve, receive no bonus
Solve calculations, receive €2.50	●	○	Don't solve, receive no bonus
Solve calculations, receive €3	●	○	Don't solve, receive no bonus
Solve calculations, receive €3.50	●	○	Don't solve, receive no bonus
Solve calculations, receive €4	●	○	Don't solve, receive no bonus
Solve calculations, receive €4.50	●	○	Don't solve, receive no bonus
Solve calculations, receive €5	●	○	Don't solve, receive no bonus
Solve calculations, receive €5.50	●	○	Don't solve, receive no bonus
Solve calculations, receive €6	●	○	Don't solve, receive no bonus
Solve calculations, receive €6.50	●	○	Don't solve, receive no bonus
Solve calculations, receive €7	●	○	Don't solve, receive no bonus
Solve calculations, receive €7.50	●	○	Don't solve, receive no bonus
Solve calculations, receive €8	●	○	Don't solve, receive no bonus
Solve calculations, receive €8.50	●	○	Don't solve, receive no bonus
Solve calculations, receive €9	●	○	Don't solve, receive no bonus
Solve calculations, receive €9.50	●	○	Don't solve, receive no bonus
Solve calculations, receive €10	●	○	Don't solve, receive no bonus

<< >>

(Note: The selection of options in the above list is illustration purposes only. For all subjects, no options were selected until the subject made a selection. Subjects had to make an active choice on each line, but could only switch from the option on the right to the option on the left once, or never, and never in the opposite direction. Subjects also saw corresponding lists for totals of 60, 100, and 200 calculations)

Part 2

of this study will now start. You can take as much time as you like.

<<   >>

Logic Puzzles

In this task, please select the answer that best suits each of the 32 questions on the following pages.

(Your answers to these questions will not affect your payout in this experiment.)

>>

Note: At this stage, subjects solve the Raven's matrices (not reproduced here for copyright reasons).

Part 3

Note: There is a 20% chance that your payout in this study will be determined solely by a decision in this section. (With the remaining probability of 80%, it is determined by a decision you made in Part 1).

In this part of the experiment, you will complete 9 decision lists. Here is an example of a decision list.

What exactly "Lottery X" is will vary from round to round.

Would you rather participate in the following lottery or receive / lose the certain amount on the right-hand side?

Lottery X

In each line, choose the option that you prefer.

Participate in the lottery	○ ○	Lose €10 with certainty
Participate in the lottery	○ ○	Lose €9 with certainty
Participate in the lottery	○ ○	Lose €8 with certainty
Participate in the lottery	○ ○	Lose €7 with certainty
Participate in the lottery	○ ○	Lose €6 with certainty
....	○ ○	....
....	○ ○	....
Participate in the lottery	○ ○	Win €6 with certainty
Participate in the lottery	○ ○	Win €7 with certainty
Participate in the lottery	○ ○	Win €8 with certainty
Participate in the lottery	○ ○	Win €9 with certainty
Participate in the lottery	○ ○	Win €10 with certainty

Each line is a separate decision.

In each line, you can either select the option on the right or left side.

If, at the end, the computer randomly decides that your payout in this experiment is determined by a decision list, the following will happen:

The computer will randomly pick a line from the decision list. Your payout will then match the decision you made in this line. Your decision has absolutely no influence on which line the computer selects.

So it's in your best interest to pick the option that fits your true preferences in each line.

There are no correct or wrong decisions!

>>

For example, suppose you completed the decision list as follows:

Would you rather participate in the following lottery or receive / lose the certain amount on the right-hand side?

Win €6 with a probability of **50%**,
lose €6 with a probability of **50%**.

In each line, choose the option that you prefer.

Participate in the lottery	○	○	Lose €10 with certainty
Participate in the lottery	○	○	Lose €9 with certainty
Participate in the lottery	○	○	Lose €8 with certainty
Participate in the lottery	○	○	Lose €7 with certainty
Participate in the lottery	○	○	Lose €6 with certainty
....	○	○	....
....	○	○	....
Participate in the lottery	○	○	Win €6 with certainty
Participate in the lottery	○	○	Win €7 with certainty
Participate in the lottery	○	○	Win €8 with certainty
Participate in the lottery	○	○	Win €9 with certainty
Participate in the lottery	○	○	Win €10 with certainty

Suppose that the computer randomly selects the decision on the third line. In this line you have chosen the possibility on the left side. Therefore, your payout for this experiment will depend on Lottery X (described in more detail later) and whether you win or lose it.

Instead, assume that the computer randomly selects the decision in the third-bottom line. In this line you have chosen the possibility on the right side. That's why you will win €8.

Most people start such a decision-making list by making a choice on the left side and eventually switching to the right side (as in the example above).

<< >>

WHY is it in my best interest to answer that question with my true valuation?

Simply put, the reason is that you will receive a worse payout if you choose anything other than what you actually prefer.

For example, suppose you would rather lose €2 in a certain round than participate in the lottery. Also, suppose that you state that you would prefer the lottery to the safe loss of €2 (for example, because you can lose a lot of money in the lottery). If, by chance, the computer chooses this decision to determine your payout, you will play the lottery, although you would have preferred to lose €2 with certainty.

Simply put, if at the end of the experiment a particular decision is chosen from this part of the experiment to determine your payout, you will receive exactly what you have selected. But you have no influence on which decision will be selected.

Therefore, the best thing you can do is to pick exactly the option on each line of each decision list that you would rather be paid for.

If you have questions about this payout mechanism, please raise your hand.

<< >>

Part 3 of the experiment starts now.

Your decisions are about real money.

<< >>

List 1 out of 9

Would you rather participate in the following lottery or receive / lose the certain amount on the right-hand side?

Win €6 with a probability of **50%**,
lose €6 with a probability of **50%**.

In each line, choose the option that you prefer.

Participate in the lottery	Lose €10 with certainty
Participate in the lottery	Lose €9 with certainty
Participate in the lottery	Lose €8 with certainty
Participate in the lottery	Lose €7 with certainty
Participate in the lottery	Lose €6 with certainty
Participate in the lottery	Lose €5 with certainty
Participate in the lottery	Lose €4 with certainty
Participate in the lottery	Lose €3 with certainty
Participate in the lottery	Lose €2 with certainty
Participate in the lottery	Lose €1 with certainty
Participate in the lottery	Win €0 with certainty
Participate in the lottery	Win €1 with certainty
Participate in the lottery	Win €2 with certainty
Participate in the lottery	Win €3 with certainty
Participate in the lottery	Win €4 with certainty
Participate in the lottery	Win €5 with certainty
Participate in the lottery	Win €6 with certainty
Participate in the lottery	Win €7 with certainty
Participate in the lottery	Win €8 with certainty
Participate in the lottery	Win €9 with certainty
Participate in the lottery	Win €10 with certainty

<< >>

(Note: The selection of options in the above list is illustration purposes only. For all subjects, no options were selected until the subject made a selection. Subjects had to make an active choice on each line, but could only switch from the option on the right to the option on the left once, or never, and never in the opposite direction. Subjects decided for 8 further lotteries, in random order.

We would like to ask you some questions about yourself.

Please answer truthfully.

What is your gender?
[male; female; other (e.g. genderqueer)]

How old are you?

At which faculty do you study?
[Faculty of Economics, Management and Social Science; Faculty of Law; Faculty of Medicine; Faculty of Philosophy; Faculty of Mathematics and Natural Sciences; Faculty of the Humanities; I am not a student]

Which state conferred your Abitur (university entrance diploma)?
[Baden-Württemberg; Bayern; Berlin; Brandenburg; Bremen; Hamburg; Hesse; Mecklenburg-Vorpommern; Niedersaxen; Nordrhein-Westfalen; Rheinland-Pfalz; Saarland; Sachsen; Sachsen-Anhalt; Schleswig-Holstein; Thüringen; I received the International Baccalaureate; I do not have an Abitur; I prefer not to say]

What was your Grade Point Average in the Abitur?
[1.0, 1.1, 1.2, . . . , 3.9, 4.0; I do not have an Abitur; I do not remember; I prefer not to say]

What was your Abitur grade in Mathematics?
[15 points (1+), 14 points (1), 13 points (1-), 12 points (2+), 11 points (2), 10 points, (2-), . . . , 3 points (5+), 2 points (2), 1 point (2-), 0 points; I do not have an Abitur; I do not remember; I prefer not to say]

What was your Abitur grade in German?
[15 points (1+), 14 points (1), 13 points (1-), 12 points (2+), 11 points (2), 10 points, (2-), . . . , 3 points (5+), 2 points (2), 1 point (2-), 0 points; I do not have an Abitur; I do not remember; I prefer not to say]

Have you taken an honors class in Mathematics in high school (Leistungskurs im Abitur)?
[Yes; No; I do not have an Abitur]

Have you taken an honors class in German in high school (Leistungskurs im Abitur)?
[Yes; No; I do not have an Abitur]

How much money do you spend on average each month (incl. rent, food, transportation, etc.)

[€ 0 - € 150; € 150 - € 300; € 300 - € 450; 450 - € 600; € 600 - € 750; 750 - € 900; € 900 - € 1050; € 1050 - € 1200; € 1200 - € 1350; € 1350 - € 1500; € 1500 - € 2000; € 2000 - € 2500; € 2500 - € 3000; more than € 3000; I prefer not to say]

How much money do you earn each month through your own labor?
[€ 0 - € 50; € 50 - € 100; € 100 - € 150; 150 - € 200; € 200 - € 250; € 250 - € 300; € 300 - € 350; € 350 - € 400; € 400 - € 450; € 450 - € 500; € 500 - € 600; 600 - € 700; € 700 - € 800; € 800 - € 900; € 900 - € 1000; € 1000 - € 1250; € 1250 - € 1500; € 1500 - € 1750; € 1750 - € 2000; € 2000 - € 2500; € 2500 - € 3000; more than € 3000; I prefer not to say]

How much money do you receive from your parents each month?
[€ 0 - € 50; € 50 - € 100; € 100 - € 150; € 150 - € 200; € 200 - € 250; € 250 - € 300; € 300 - € 350; € 350 - € 400; € 400 - € 450; € 450 - € 500; € 500 - € 600; € 600 - € 700; € 700 - € 800; € 800 - € 900; € 900 - € 1000; € 1000 - € 1250; € 1250 - € 1500; € 1500 - € 1750; € 1750 - € 2000; € 2000 - € 2500; € 2500 - € 3000; more than € 3000; I prefer not to say]

What is the net wealth of your parents (incl. real estate)?
[€ 0k - € 25k; € 25k - € 50k; € 50k - € 75k; € 75k - € 100k; € 100k - € 125k; € 125k - € 150k; € 150k - € 175k; € 175k - € 200k; € 200k - € 250k; € 250k - € 300k; € 300k - € 350k; € 350k - € 400k; € 400k - € 450k; € 450k - € 500k; € 500k - € 600k; € 600k - € 700k; € 700k - € 800k; € 800k - € 900k; € 900k - € 1 mio.; € 1 mio. - € 1.5 mio.; € 1.5 mio. - € 2 mio.; € 2 mio. - € 2.5 mio.; € 2.5 mio. - € 3 mio.; € 3 mio. - € 3.5 mio.; € 3.5 mio. - € 4 mio.; more than € 4 mio.; I prefer not to say]

Would you like to be informed by e-mail about the results and hypotheses of this study?

If yes, please enter your email address here.
(You are also allowed to leave this field blank.)

<< >>

As a reminder, you have made various decisions about whether you want to solve extra calculations for additional bonuses in decision lists.

The computer has randomly selected a list and one of your decisions, which is now being executed.

The decision that was randomly chosen is the following:

Solve 200 calculations for €2.5.

You decided to REJECT this transaction.

<< >>

We're done!

Thank you for your participation in this study!

Again, on behalf of the University of Cologne and the University of Toronto, we guarantee that your payout is exactly as we described it to you. Specifically, this means that if we told you something would happen with a certain probability out of 100, then it was with that very probability.

As explained in the introduction, you will receive €15 for your participation in this experiment, plus any winnings you made in this experiment and less any losses you have suffered. Your total payout is calculated as follows.

Your payout is determined by your probability assessment in part 1, round 11 so that you receive €3.

You do not receive a bonus payment for solving additional calculations.

Now, therefore, your final payout is €18.

You have completed the experiment now. Please go quietly to the experimenter room to receive your payout. Bring the completed receipt with you.

<<

References

Aumann, R. J., M. Maschler, and R. E. Stearns (1995). *Repeated Games with Incomplete Information*. MIT Press.

Caplin, A. and M. Dean (2013). Rational inattention, entropy, and choice: The posterior-based approach. Working paper.

Caplin, A., M. Dean, and J. Leahy (2017). Rationally inattentive behavior: Characterizing and generalizing Shannon entropy. NBER Working Paper.

Gentzkow, M. and E. Kamenica (2011). Bayesian persuasion. *American Economic Review 101*(6), 2590–2615.

Matějka, F. and A. McKay (2015). Rational inattention to discrete choices: A new foundation for the multinomial logit model. *American Economic Review 105*(1), 272–98.

Morris, S. and P. Strack (2017). The Wald problem and the equivalence of sequential sampling and static information costs. Working paper.

Wald, A. (1947). *Sequential Analysis*. New York: Wiley.