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Does free information provision crowd out costly information acquisition? It's a matter of timing[1]

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Abstract

Conventional wisdom suggests that promising an agent free information would crowd out costly information acquisition. We theoretically demonstrate that this intuition only holds as a knife-edge case where priors are symmetric. For asymmetric priors, agents are predicted to increase their information acquisition when promised free information in the future. We test in the lab whether such crowding out occurs for both symmetric and asymmetric priors. We find theoretical support for the predictions: when priors are asymmetric, the promise of future "free" information induces subjects to acquire costly information which they would not be acquiring otherwise.

JEL Classifications: C72, C90, D44, D80.
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1 Introduction

The impact of information provision is a major theme in the literature on consumer choice: Bollinger et al. (2011) on calories for Starbucks purchases, Chaloupka et al. (2015) on smoking warning labels, Bhargava et al. (2017) on improvements in the presentation of health insurance plans, and Allcott and Taubinsky (2015) on comparisons of light bulb technologies.[1] Information provision is also considered an important tool to improve political choices. León (2017) studied the impact of information on monetary penalties for abstention on turnout in Peruvian municipal elections. Larreguy et al. (2020) gives evidence of the impact of information on government performance on voting outcomes in Mexico. The role of information provision in "nudging" behavior appears by now well-established. Yet, the interaction between free information provision and individuals' acquisition effort is underexplored. In this paper we study how crowding out of information acquisition relates to the timing of the free information provision.

It may appear that the promise of free information in the future might merely crowd-out information acquisition by an agent. Yet, it turns out it can actually incentivize information acquisition efforts. The intuition is that additional information that does not make you change your choice has no (instrumental) value. Unless one is ex-ante indifferent, a small amount of information will do little to change posterior beliefs, and will, therefore, not affect choices.[2] That is, weak signals do not affect optimal behavior and are payoff-irrelevant ex-post, no matter their realization. Thus, the Radner and Stiglitz (1984) observation that the marginal value of small amounts of information is usually zero. This implies that individual demand for information would normally exhibit discontinuities at zero: there is a minimum scale at which information should be acquired. Unless reaching this threshold is sufficiently cheap, people may want to stop paying attention and act based on their prior beliefs. This is the well known Radner-Stiglitz non-concavity in the value of information.[3]

A promise of delivering free additional information, after an individual completes her costly acquisition, may be used to "smooth out" this non-concavity. Indeed, if an agent knows that additional information will be provided, and has to decide on her own effort before observing whatever may be told to her later, anything she learns on her own would be valuable with positive probability. As long as future help is expected, the marginal value of small amounts of information is no longer zero. Rather than substituting for individual effort, promised free information may complement it. Hence, such promise may indeed stimulate acquiring small amounts of information, which otherwise should never be acquired. We provide a model of promising free information in the future as an incentive and test it in the lab.

Note that substitution intuition is not completely wrong. As previously mentioned, if the agent is indifferent between actions, arbitrarily small amounts of information become valuable. In this rare knife-edge case, she may hope to free ride on any promised future help, thus, crowding out her effort. As this relies on initial exact indifference, it would not be something one could expect to observe often in real life. Yet, this allows us to introduce a complementary subtle test for the theory in the lab.

This straightforward and intuitive point has not, to the best of our knowledge, been made or tested. It is, however, a common implication of costly information acquisition environments. In this respect, our work relates to the recent theoretical and experimental work on rational inattention (Caplin and Martin,

[1]For a recent review, see Bernheim and Taubinsky (2018).

[2]If without additional information the agent is indifferent between actions, arbitrarily small amounts of information should be sufficient to break this indifference. This, however, is readily seen to be a rare knife-edge case.

[3]Chade and Schlee (2002) have shown that the Radner-Stiglitz non-concavity is an extremely robust feature of costly information acquisition environments.

2015; Caplin and Dean, 2015; Caplin and Martin, 2018; Dean and Neligh, 2017). In common with these studies, we explore the consequences of agents rationally deciding how much attention they would like to pay in a costly information environment. Promising additional information in the future allows us to directly vary the set of available informational strategies. In this sense, the interaction between individual information-acquisition effort and timing of free information provision we explore here may be seen as a novel implication of rational inattention.[4]

Though a promise of free information may create incentives for costly information acquisition that would not exist otherwise, it would not improve the quality of individual decision, nor the welfare of the decision-maker. This result follows straightforwardly from Blackwell (1962).[5] In some settings, however, we may expect greater individual learning to have spillovers on others. In fact, this reasoning may be behind the typical prohibition for jurors to talk about the evidence they listen to during the course of a trial. Jury discussion is supposed to occur only after they listen to every bit of evidence presented. In Hannaford et al. (2000), for example, the authors show intriguing evidence of increased drop out by jurors in a field experiment in which these instructions were relaxed, which lends support to the hypothesis we have just laid out.

The experimental test follows the approach of recent laboratory studies on costly information acquisition (Bhattacharya et al., 2017; Elbittar et al., 2016; Grosser and Seebauer, 2018).[6] Our experimental design allows us to explore the predictions of the model using both between- and within- subjects data. Results are consistent with the predictions of the model: offering future information encourages greater investments in information acquisition. Furthermore, when we explore the rare knife-edge case of indifference, we find, as predicted, that information acquisition reverses with the promise of future information.

In section 2 we provide a theoretical model of attentional response to variation in the timing of free information provision. Section 3 introduces the experimental design and section 4 describes our data and empirical analysis. To encourage you to read along, we promise to report the results of the experiment and conclusions in the last two sections.

2 Theory

We consider a standard information acquisition setting with two states and two actions.[7] Specifically, the two possible states of the world are $\Omega = \{R, B\}$. The agent assigns a prior probability $P(R) = \beta \in (0,1)$ to $\omega = R$, chooses $a \in \{r, b\}$, and her utility, U, depends only on a and ω.

We assume that the agent is indifferent between the two states, as long as her decision is correct ($U(R,r) = U(B,b) = 0$), while her attitude towards the two possible errors may differ, so that $U(B,r) = -q$ and $U(R,b) = -(1-q)$. Indeed, with these parameters the agent would be willing to choose $a = r$ if

[4]A somewhat similar effect has been noted by Caplin and Martin (2018) who explored, from a rational inattention standpoint, how varying default options presented to subjects may nudge them to either acquire information or to "drop out".

[5]This is striking, and is an important aspect of our analysis, since policy-makers often seek to induce individual informedness under the presumption that this is a social good.

[6]These studies in juries establish a framework in which potential informational spillovers from increased study generated through delayed communication between jury members may be explored. It is of interest that, in the last two studies referenced, the subjects consistently acquired less information than predicted for the experimental setting. Elbittar et al. (2016) propose that this may arise from biased priors, which effectively lead to the drop out from information acquisition of the sort we study in this paper. Offer of future information, which in this setting may be interpreted as arising from jury deliberation, would be expected to help resolve this problem.

[7]This simplified setting corresponds to our experimental environment, but the results presented below are fairly general, as follows from Radner and Stiglitz (1984) and Chade and Schlee (2002).

and only if $P(R) \geq q$. Thus $q \in (0,1)$ may be interpreted as the degree of certainty necessary for the agent to choose $a = r$.

The agent observe a stream of signals. Each signal $S_i \in \{\hat{r}, \hat{b}\}$ that agents will observe is binary and is correlated with the true state of the world, so that $P(S_i = \hat{r}|\omega = R) = P(S_i = \hat{b}|\omega = B) = p \in \left(\frac{1}{2}, 1\right)$. Conditional on the true state, signals are *i.i.d.* Before choosing $a \in \{r, b\}$, the agent decides how many signals she would like to purchase with a constant per signal cost $c > 0$. In addition, a fixed number of free signals will be observed before the agent chooses how many signals she would like to purchase, v, and the agent knows this. We concentrate on the interaction between the timing at which the free signals are observed by the agent and her decision to purchase additional signals.

The posterior belief of a Bayesian agent who observes m signals $S_i = \hat{r}$ and $k < m$ signals $S_i = \hat{b}$ is

$$P\left(R|m \text{ signals } \hat{r} \text{ and } k < m \text{ signals } \hat{b}\right) = \frac{\beta p^{m-k}}{\beta p^{m-k} + (1-\beta)(1-p)^{m-k}}$$

Since utility U depends only on a and ω, information only has an instrumental value. It is only *ex-ante* valuable if it has the potential to change an agents action, a. The agent changes an action when $E[U(\omega, r)|\text{signals}] > E[U(\omega, b)|\text{signals}]$.[8] Thus, the smallest number of signals such that observing them has *ex-ante* positive value is

$$n^* = \min\left\{k \in \mathbb{N} : \frac{\beta p^n}{\beta p^n + (1-\beta)(1-p)^k} > q > \frac{\beta (1-p)^n}{\beta (1-p)^n + (1-\beta) p^n}\right\}.$$

As long as $\beta \neq q$ and p is close enough to $\frac{1}{2}$ (i.e., information comes in small increments), $n^* > 1$: small amounts of information are useless. This is the manifestation of the Radner-Stiglitz non-concavity in our setting. On the other hand, as long as we are promising to show the agent (right before she has to choose her action) $N \geq n^* - 1$ signals *in addition* to whatever information she acquires, there will always be positive value of acquiring even a single signal. Since without that promise acquiring a single signal is useless, additional information we offer cannot displace the agent's own information acquisition effort: it can only be complementary to it.

There is, however, one special case in which any small amount of information is *ex-ante* valuable. This happens if $\beta = q$, so that without any information she is indifferent between the two actions. In this case standard Bayesian updating implies that she should choose $a = r$ whenever $m \equiv \#\{i : S_i = \hat{r}\}$ is bigger than $k \equiv \#\{i : S_i = \hat{b}\}$ and should prefer to declare $a = b$ whenever $k > m$ (the agent is indifferent whenever $k = m$). If $\beta = q$, therefore, standard Bayesian updating coincides with the simple "count-the-signals" rule of thumb, greatly simplifying the decision the agent faces. Notably, however, this is also the case in which even minute amount of information would break the indifference, so marginal value of information at zero is, in fact, positive. This, in turn, makes possible a reversal of the expected impact of free information: there is no longer any non-concavity for the promise of future information to smooth, but the agent may be tempted to free-ride on this promise. The particular case of $\beta = q$ is, of course, non-generic, but we shall be making an extensive use of this environment in our design.

As discussed above, except in the knife-edge indifference case, we may be able to induce greater information acquisition by promising that more information will be given after the agent purchases information; however, the additional learning will not improve the expected quality of the verdict. To see this, suppose

[8] This is the same as $-(1-P)q > -(1-q)P$, or $\pi > q$ (where π is the posterior probability of the first equation).

a total of n signals, $S_1^f, ...S_n^f$ may be provided to the agent for free. If we choose to provide them at the beginning, so that their realizations may be observed before the agent decides how many signals to purchase at cost, she would be able to condition her decision on the observed realizations. Defining the difference between the number of signals that indicate R and B as $X^f = \#\left\{i : S_i^f = \hat{r}\right\} - \#\left\{i : S_i^f = \hat{b}\right\}$, we observe that, in order for the additional information to have any potential impact on the agent's choice of v, she would have to buy more signals than the observed realization of X^f. Indeed, suppose the number of costly signals she purchases, $s \leq |X^f|$. Then, even if all of the signal realizations are identical, the sign of $m - k$ is equal to the sign of X^f, implying the same choice of v. This implies that the expected value of purchasing $s \leq |X^f|$ signals is equal to zero: she should either buy a lot of information, or none – Radner and Stiglitz (1984) in action. Of course, even a single signal would be valuable if the free signals "tie", $X^f = 0$. This tie can only occur if the number of these signals n is even, in which case, irrespective of the true state of the world it would happen with probability $P\left(X^f = 0\right) = \frac{n!}{\left(\frac{n}{2}!\right)^2} p^{\frac{n}{2}} (1-p)^{\frac{n}{2}} > 0$.

If we instead promise to show the agent the same signals after she decides how many signals to purchase, this will be her prior probability that $X^f = 0$. Hence, with positive probability even a single signal would turn out to be valuable.

3 Experimental Design

Our experimental task follows a standard information acquisition environment (Elbittar et al., 2016; Guarnaschelli et al., 2018; Battaglini et al., 2010). Participants earn money if they correctly guess the true binary state of the world, $\Omega = \{R, B\}$, framed as guessing the color of a jar.[9] Subjects received four free binary signals, S_i, corresponding to the two possible states, and could acquire additional signals at a cost. Our treatment variable is the timing of free signals relative to the acquisition decision.

Participants know that the true color of a jar is equally likely to be Red or Blue. Jars contain 100 balls: 60 corresponding to the jar's color and 40 of the other color. For each of 24 periods, participants guess the true color of the jar and decide how many (up to 5) balls to acquire at a known cost of c each.[10] Those who guess correctly earn E\$1,000 minus the cost of purchased balls; otherwise, they earn E\$300 minus the cost of purchased balls.

We deliberately chose to induce payoffs and initial priors so that without any information subjects were exactly indifferent between the jars. As discussed in the previous section, this made the jar choice particularly simple: subjects needed merely to choose the jar corresponding to the color of the majority of the observed balls. As we demonstrate in the next section, the subjects used overwhelmingly that heuristic. This allowed us to concentrate on the information acquisition decision, which was our primary interest.

Before guessing the color of the jar, they know they will see, besides the number of balls acquired, the color of 4 balls (at no cost). Treatments vary the number of free balls participants get to see *after* deciding how many balls to buy. In treatment *0B*, no (0) free balls are observed after their decision. Participants see

[9]In addition to the main jar-guessing task, there were three additional tasks that followed: a risk elicitation task using a multiple price list, and two additional jar-guessing tasks (for 12 periods each). Participants knew that one of the four tasks was to be randomly selected at the end, and their choice in a single (randomly selected) period for the task was to determine their earnings from the experiment. The experimental earnings was the sum of the randomly selected choice plus the show up fee and a starting balance of E\$120.

[10]The cost of balls purchased is drawn in each period from a uniform distribution between 0 and 100. Participants observe the realized cost before deciding how many balls to purchase each period. In the first twelve periods, they do not learn the true state. In the last twelve periods, they do obtain feedback from their guess. Instructions are available in Appendix D. Appendix C provides a detailed description of the experimental protocols.

all 4 free balls before the decision to acquire information. In treatment *2B*, participants observe the color of 2 free balls before and 2 after the decision to acquire additional information. Thus, they are promised information (2 balls) in the future. Finally, in treatment *4B*, participants know they will see all 4 free balls after the decision to acquire information.

In addition, we have the exogenous variation in the realization of free signals prior to information acquisition. This is a feature of our design which gives rise to different informational situations (updated priors) with different predictions. Table 1 presents the predicted marginal value of each additional signal across every informational situation in which a risk-neutral participant may find herself.

Due to the non-concavity in the value of information, the main prediction of the model is that, promising future information -as in *2B* relative to *0B*- should induce greater information acquisition when the updated prior is asymmetric (i.e., $\beta_\omega = 9/13$). The opposite is true in the rare knife-edge case where the updated prior leads to indifference ($\beta_\omega = 1/2$). This is reflected in the marginal value of additional signals across the different treatments and induced updated priors. The intuition is the following: Assume that in *0B*, where a participant observes four free balls before she decides how many to acquire at cost, she is shown at least 3 balls of the same color ($\beta_\omega = 9/13$). Acquiring two balls or less does not provide evidence sufficient to change beliefs. Thus, she would have to acquire at least 3 balls at cost for this additional information to have any value. Given the cost of this investment, such a participant is likely to "drop out" by purchasing no costly information at all. Contrast this with *2B* where a participant observed both balls of the same color ($\beta_\omega = 9/13$). In this case, purchasing a single additional ball would make sense as the future information (2 additional balls) can smooth out the non-concavity. Thus, whether she is in treatment *0B* and observed 3 Red balls or in treatment *2B* and observed 2 Red balls, a Bayesian subject should have identical prior about the likelihood of a Red jar at the time of the information acquisition decision. However, purchasing a single additional ball would only make sense in the latter case. Hence, in this setting ($\beta = 9/13$), we should observe fewer drop out decisions in *2B* as compared to *0B*.

An additional advantage of this set-up is the effective oversampling of the otherwise rare knife-edge case ($\beta_\omega = 1/2$). In this rare case even small amounts of information matter and, hence, the effect of the timing of free information provision is reversed.

4 Data and Empirical Analysis

We conducted two waves of sessions. In the first wave, 72 undergraduates (mainly) from Universidad Francisco Marroquín participated in the experiment.[11] Within each session, a third of the subjects were randomly assigned to one of the three treatments; that is, we implemented all treatments within the same session (to different subjects). We used the same set of (predefined random) draws across treatments. In the second wave, 84 undergraduates from Chapman University took part.[12] In each of these sessions, we used $i.i.d.$ draws for each individual decision. All subjects within a session participated in the same treatment for the first 24 periods. In addition, for tasks 3 and 4, we repeat the main jar-guessing task for the other 2 treatments.[13] Thus, in these sessions we have between-subject variation (in the first 24-period) task that is comparable with the first wave, and within-subject variation from tasks 1, 3 and 4.

[11]Payments were converted to local currency at a rate of $E\$6 = Q1$ ($E\$46.2 = 1$USD) and participants received a show-up fee of $Q20$ (2.6USD).

[12]Payments were converted to local currency at a rate $E\$40$ per 1USD, and subjects received a show-up fee 7 USD.

[13]We have perfect balance regarding the order in which the within-subjects treatments were implemented across the 6 sessions.

We exploit both the between- and within- subject variation in our data. We pool together data from the main task in the 24 periods across the two waves for our between-subjects (BSs) data. In addition, we take from the second wave the last 12 periods of the main task and the 12 periods of each of tasks 3 and 4. This is our within-subjects (WSs) data. We report data from 156 subjects who took part in 12 sessions: six 12-subject sessions from the first wave of data collection, and six 14-subject sessions for the second wave.

Given our design, there are some ancillary predictions that we can rely on to check whether participants understood the environment and behave consistently with a "rational" choice model –beyond the precise main predictions of the model. First, Bayesian subjects should make their final decision by simply counting the number of balls of each color they observe in total, and declaring the majority color as the color of the jar (they would be indifferent in case of a tie). They do so 94.4% (96.5%) of the time in the BSs (WSs) data. Second subjects react to information acquisition prices.[14] Finally, as Table 1 illustrates, an idiosyncratic feature of our experimental design is that purchasing a positive even number of balls is dominated. Since the last of these would never make the subject strictly prefer to change her decision, it would never have a positive value in expectation. We see that conditional on acquiring information, subjects tend to acquire an odd number of signals 73.2% of the time (77.6% during the second half).[15]

We test our main hypotheses using both non-parametric tests and reduced-form regressions on treatment effects. For our non-parametric tests, we take the average value of the variable of interest for each individual across all 24 periods (and use that to make comparisons across treatments (BSs). For the WSs data, we compare the average value of the variable of interest for each individual across all periods of each treatment (24 for the first treatment, and the 12 periods for each of the other treatments). Thus, we have 52 independent observations per treatment in the BSs data and 84 matched-paired observations for the WSs data.[16] For BSs we use the robust rank order test (Fligner and Policello, 1981) for pairwise comparisons, and the Kruskal-Wallis test for comparisons of more than two categories (i.e., for $\beta_\omega = 1/2$, *4B* = *2B* = *0B*). For WSs data, we use the Wilcoxon sign-rank test for pairwise comparisons, and the Friedman test for comparisons across all three treatments for the knife-edge case. Except for the Kruskal-Wallis and Friedman tests, we report p-values from one-sided tests since our model provides clear predictions regarding the direction of treatment effects.

For the regressions, we separately estimate on the pooled BSs data and for the WSs data, indexing subjects by i and periods by t, the following regression model:

$$Y_{it} = \alpha_0 + \alpha_1 \beta_t^{\omega=\frac{1}{2}} 2B_i + \alpha_2 \beta_t^{\omega=\frac{1}{2}} 0B_i + \alpha_3 \beta_t^{\omega=\frac{9}{13}} 2B_i + \alpha_4 \beta_t^{\omega=\frac{9}{13}} 0B_i + \gamma Cost_{it} + X'\delta + \epsilon_{it} \tag{1}$$

where our dependent variable is information acquisition, Y_{it}, either at the intensive margin (a dummy on whether any balls were acquired), or extensive margin (a discrete variable $[0, 5]$ on the number of balls acquired).[17] Our independent variables are treatment-prior dummies, and we control for the cost of acquiring information. X is a vector of controls that includes period (linear and squared), a dummy for the wave of the data (task order controls) for BSs (WSs), and, in some WSs specifications also individual fixed effects.

[14] Although on average they acquire more information than predicted by the risk-neutral model, we observe that prices influence their information acquisition decisions.

[15] We also estimate the probability of acquiring signals using a linear probability model. Our dependent variable is a dummy on whether an even number of balls were acquired. Our independent variables are treatment dummies, cost of acquiring information, period, period^2, and wave.

[16] Tables 3 and 4 presents the summary of results by treatment and situation, separately for each wave.

[17] We also use equation (1) to examine the predictive accuracy of guesses as dependent variable.

Our main hypothesis is that the offer of free information in the future increases information acquisition, $(1)Ha : \alpha_3 > \alpha_4$. However, in the rare knife-edge cases, the effect reverses as free information in the future substitutes for information acquisition, $(2)Ha : \alpha_0 < \alpha_1 < \alpha_2$

5 Results

Table 4 provides summary statistics of observed and predicted instances of "dropping out" (no information acquisition), amount of information acquired, and correct guesses broken down by informational situation. Figure 1 presents, separately for BSs and WSs, histograms of predicted and observed information acquisition by treatment and informational situation.[18] As the table and figures also illustrate, the share of observations that involve no purchase of information is lower than predicted across all informational situations. Indeed, the average number of balls purchased exceeds predictions in all situations.[19] The table, as well as figures, illustrates the main predictions of the model: under asymmetric priors (i.e., $\beta_\omega = 9/13$ in top row) promising future information should induce greater information acquisition. This is especially noteworthy at the extensive margin through the reduction in the mass at zero instances in the figures, and in the table, the increase in the share of no information acquisition. However, the opposite is true in the rare knife-edge case ($\beta_\omega = 1/2$): promises of future information induce lower information acquisition.

(1.1) Ha (extensive margin): For given (non-symmetric) priors ($\beta_\omega = 9/13$), subjects are more likely to acquire information when free information is promised in the future (2B, $2 : 0$) than when no future information is offered (0B, $3 : 1$).

Results: We find that the rate of information acquisition when future information is promised is .535 (.408) for BSs (WSs), compared to .345 (0.313) when no future information is promised. Using non-parametric tests we reject the null hypothesis that the probability of acquiring information in $2B \leq 0B$ using either the BSs ($U = 2.702$, $p = 0.003$) or the WSs data $2B \leq 0B$ ($z = 2.475$, $p = 0.006$).

Our reduced-form estimates confirm the results. The top panel of figure 2 plots the coefficients from a linear probability model. Appendix Table 5 presents the full results from the regressions exploring the extensive margin.[20] We reject the null hypothesis using either a linear probability model (BSs: $p = 0.004$, WSs: $p = 0.015$) or a random effects Logit model (BSs: $p = 0.005$, WSs: $p = 0.009$). Using the linear probability model, we find that the probability of acquiring information increases by $8 - 17$ percentage points (depending on whether we use BSs or WSs) when future information is promised.

(1.2) Ha (intensive margin): For given (non-symmetric) priors ($\beta_\omega = 9/13$), when free information is promised in the future (2B, $2 : 0$) subjects acquire more information compared to when no future information is promised (0B, $3 : 1$).

Results: With BSs (WSs) data, we find that subjects acquire on average 0.315 (0.125) more balls when future information is promised. That is an increase of 37% (16%) compared to when no future information is promised. Using non-parametric tests, we reject the null hypothesis that the amount of information acquired $2B \leq 0B$ with the BSs data ($U = 1.680$, $p = 0.046$). For the WSs data the result is only marginally significant ($z = 1.542$, $p = 0.062$).

[18] Appendix figures 4 and 5 present CDF's of predicted and observed information acquisition decisions for BSs and WSs.

[19] Appendix table 3 contains both predicted and observed summary statistics by treatment, conditional on the realized draws observed by subjects when they made their information acquisition decisions.

[20] For sake of brevity, in this section we report the $p-$values for a (one-sided) test of the alternative hypothesis that $\alpha_3 > \alpha_4$, as described in equation (1).

Reduced form results are only marginally significant. Using the random effects Poisson model (BSs: $p = 0.065$, WSs: $p = 0.051$) or a random effects Tobit model (BSs: $p = 0.058$, WSs: $p = 0.001$).[21] The top panel of figure 3 presents coefficient plots for the random effects Poisson model. As the figure illustrates, although our results are only marginally significant, they have the predicted sign and are consistent across the sub-samples.

A further confirmation of the model is that a simple change in the parameters that modify the updated prior to a knife-edge case ($\beta_\omega = 1/2$) reverses the effects of promising future information.

(2.1) Ha (extensive margin): For symmetric knife-edge priors ($\beta_\omega = 1/2$), subjects are less likely to acquire information in 4B or 2B ($1:1$) than in 0B ($2:2$).

Results: We reject the null hypothesis that the probability of acquiring any information is equal across treatments ($\chi^2 = 13.862$, $p < 0.001$ for BSs; $Friedman = 195.2$, $p < 0.0001$; for WSs). For pairwise comparisons, we reject that $4B \geq 0B$ (BSs data: $U = -3.813$, $p < 0.0001$; WSs data: $z = -5.605$, $p < 0.0001$) , that $2B \geq 0B$ (BSs data: $U = -1.680$, $p = 0.046$; WSs data: $z = -3.953$, $p < 0.0001$), and that $4B \geq 2B$ (BSs data: $U = -2.2253$, $p = 0.013$; WSs data: $z = -3.341$, $p < 0.001$). Our reduced form models (reported in Table 5 and illustrated in the bottom panel of figure 2) also allow us to test these hypotheses and overall we find strong support for them.[22]

(2.2) Ha (intensive margin): For symmetric knife-edge priors ($\beta_\omega = 1/2$), subjects acquire less information in 4B or 2B ($1:1$) than in 0B ($2:2$).

Results: In the knife-edge cases for BSs we marginally reject ($\chi^2 = 4.752$, $p = 0.093$) and for WSs we strongly reject ($Friedman = 194.96$, $p < 0.0001$) the null hypothesis of equal information acquisition across treatments. For pairwise comparisons that information acquisition is lower when information is promised in the future, we reject using any test/data for the most extreme case ($4B$ vs. $0B$): $4B \geq 0B$ (BSs data: $U = -2.260$, $p = 0.0119$; WSs data: -2.870, $p = 0.002$). For our other hypotheses, although results have the predicted sign in all cases, they are only statistically significant using the (more powerful) within-subjects data: $2B \geq 0B$ (BSs data: $U = -1.194$, $p = 0.116$; WSs data: $z = -0.898$, $p = 0.019$); for $4B \geq 2B$, results (BSs data: $U = -0.909$, $p = 0.182$; WSs data: $z = -2.431$, $p < 0.007$). Our reduced form results can be seen in the bottom panel of figure 3. Using the reduced form results we see again that we consistently reject the null for the most extreme hypotheses $4B \geq 0B$ using random effects Poisson model and a random effects Tobit model. We only reject the other hypotheses ($2B \geq 0B$ and $4B \geq 2B$) using the within-subject data.[23]

In addition to the main hypotheses, we find support for additional propositions of secondary importance. As predicted by theory, we observe no difference in the decision quality across treatments with asymmetric priors. Despite differences in information acquisition by treatment, the proportion of time the subjects choose the correct jar color is no higher in *2B* than in *0B* (NPT: $p = 0.368$ for BSs, $p = 0.6146$ for WSs; reduced-form regressions: $p > 0.307$ for BSs and $p > 0.104$ for WSs). However, in the rare knife-edge situations where even small amounts of information are valuable, providing free information in the future is expected to slightly increase the predictive accuracy of guesses. We find support for it when comparing the

[21] Appendix Table 6 presents the full results for the regression estimates.

[22] The only exception is $2B \geq 0B$, where the differences are not statistically significant with the pooled BSs data, although they are significant at the $p < 0.001$ with the WSs data.

[23] Appendix Table 6 presents the full results for the regression estimates.

most extreme treatments: *0B* $(2:2) <$ *4B* (BSs: $U = 2.136$, $p = 0.016$, WSs: $z = 3.607$, $p < 0.001$).[24] This lends further support to the theory.

6 Conclusions and Further Research

We present results of a laboratory experiment on the impact a promise of future information may exert on individual information acquisition effort. We observe that offering future information encourages greater costly information acquisition. Furthermore, when we explore the rare knife-edge case of indifference, as predicted we observe that information acquisition reverses with the promise of future information. The differences across treatments are more pronounced on the extensive than on the intensive margin: promise of delayed information makes the agents less likely to choose not to acquire any information at all. As predicted by the model, information acquired in this manner is, ex-post, useless: the quality of the overall decision-making is unaffected.

Our ability to induce information acquisition through a promise of free information is, however, suggestive of possible information spillovers in group information acquisition environments with communication, such as juries: the possibility that we believe should be explored in future research. The effect we identify appears to be an important previously unobserved feature of costly attention environments and may, therefore, be used to identify rational inattention in the field. IRB

In terms of institutional design, delay in information provision may be sufficient to discourage "informational drop out": situations in which agents choose to forgo attentional effort and make decisions based entirely on their prior beliefs (this appears to be a plausible explanation for certain typical features of the common law jury system). It may also be a useful tool in experimental design, as it could be used to avoid excessive drop-out by subjects that has been observed in some previous experimental studies (Elbittar et al., 2016). It might also be a feature of the registered reports –the peer-review of pre-results submissions– to encourage greater attention from reviewers.

Finally, this study poses some questions to the important literature that evaluates the impact of free information provision and begs to raise the bar. Since the information provided in most studies cannot be anticipated at the moment of making information acquisition decisions, why do so many studies expect to find a positive effect? What is a good model that takes into account the non-concavity in the value of information that predicts such effects? What are the underlying assumptions behind the baseline priors? What role do biases (i.e. base rate neglect) play in expecting such findings? Applied research evaluating the effects of free information provision should aim to answer these questions to improve our understanding of the mechanisms through which they work.

[24] We also estimate the probability of guessing correctly the state using equation (1) with a linear probability model and a random-effects Logit model. Our dependent variable is a dummy on whether they correctly guessed the color of the jar. Results are presented in Appendix Table 7. Again we find support, rejecting the one-sided hypothesis that *0B* $(2:2) \geq$ *4B*, with $p < 0.05$ for all specifications.

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7 Tables

Table 1: Marginal value of information acquisition

Updated prior	Situation	Ball 1	Ball 2	Ball 3	Ball 4	Ball 5
	B_4	3.46	0	2.76	0	2.32
$\beta_\omega = 1/2$	B_2 (1 : 1)	4.8	0	3.46	0	2.76
	B_0 (2 : 2)	10	0	4.8	0	3.46
$\beta_\omega = 9/13$	B_2 (2 : 0)	2.22	0	2.13	0	1.92
	B_0 (3 : 1)	0	0	2.22	0	2.13
$\beta_\omega = 81/97$	B_0 (4 : 0)	0	0	0	0	0.43

Notes: Marginal value of purchasing a ball, in percentages of the prize value at the moment of information acquisition. To understand the numbers in this table, consider the marginal value of going from 1 to 3 balls having previously observed two Red balls. Purchasing the extra 2 balls could ex post change choice only if either two or all the three of the balls she is already set to observe (the two free balls and the one the agent has already decided to purchase) would turn out to be blue. In the former case, the agent would change her behavior if she observes two blue balls, in the latter case she would change her behavior if she observes two red balls. Having observed two red balls the agent's current prior belief that the jar is Red is $\beta = 9/13$. Combining the ex ante value of observing 2 blue balls after 1 red and 2 blues, and 2 red balls after 3 blues and substituting the signal strength $p = 0.6$ we obtain $3\left(1-\beta\right)\left(1-p\right)p^4 - 3\beta p\left(1-p\right)^4 + \beta\left(1-p\right)^3 p^2 - \left(1-\beta\right)p^3\left(1-p\right)^2 = 2.13$

Table 2: Summary statistics by situation

Updated Prior	Situation	Obs.	Share No Info		Avg. Balls Purchased		Avg. Correct	
			Predicted	Observed	*Predicted*	Observed	*Predicted*	Observed
Between-subjects data								
	B_4	1,248	*0.749*	0.453	*0.675*	1.345	*0.695*	0.684
$\beta_\omega = 1/2$	B_2 (1 : 1)	581	*0.651*	0.339	*0.921*	1.551	*0.620*	0.645
	B_0 (2 : 2)	423	*0.319*	0.253	*1.324*	1.730	*0.521*	0.617
$\beta_\omega = 9/13$	B_2 (2 : 0)	667	*0.849*	0.465	*0.436*	1.168	*0.740*	0.729
	B_0 (3 : 1)	614	*0.957*	0.655	*0.212*	0.853	*0.725*	0.712
$\beta_\omega = 81/97$	B_0 (4 : 0)	211	*0.972*	0.787	*0.142*	0.559	*0.896*	0.877
Within-subjects data								
	B_4	1,008	*0.736*	0.546	*0.641*	1.14	*0.683*	0.676
$\beta_\omega = 1/2$	B_2 (1 : 1)	479	*0.647*	0.438	*0.766*	1.25	*0.612*	0.620
	B_0 (2 : 2)	361	*0.307*	0.343	*1.30*	1.50	*0.589*	0.554
$\beta_\omega = 9/13$	B_2 (2 : 0)	529	*0.853*	0.588	*0.393*	0.907	*0.704*	0.709
	B_0 (3 : 1)	490	*0.951*	0.694	*0.245*	0.782	*0.702*	0.682
$\beta_\omega = 81/97$	B_0 (4 : 0)	157	*0.987*	0.828	*0.064*	0.433	*0.847*	0.815

Notes: Summary statistics of predicted and observed actions for between- and within-subjects data, according to the updated priors generated by the informational case (situation). "Obs." denotes the number of observations collected in each informational case. "Share No Info" is the relative number of instances where no information was acquired. "Avg. Balls Purchased" denotes the unconditional number of balls purchased. "Avg. Correct" is the share of instances of the correct state of the world predictions (color of jar guesses).

8 Figures

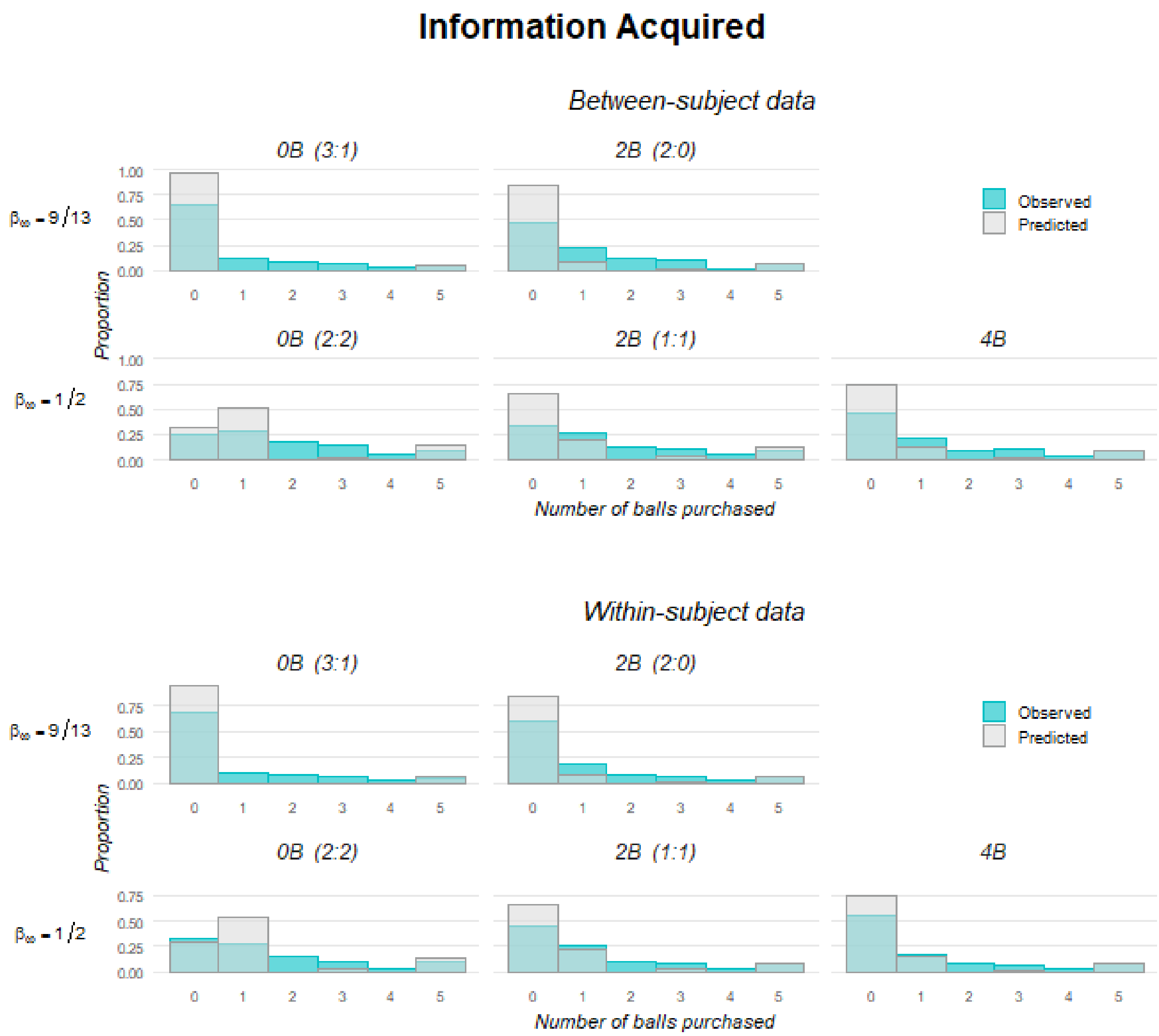


Figure 1: Histogram of predicted and observed balls purchased by treatment and informational case. Top panel for between-subject (BSs) data, and bottom panel for within-subjects (WSs) data. Top row or each panel for $\beta_\omega = 9/13$; bottom row of each panel for the rare knife-edge case of indifference ($\beta_\omega = 1/2$).

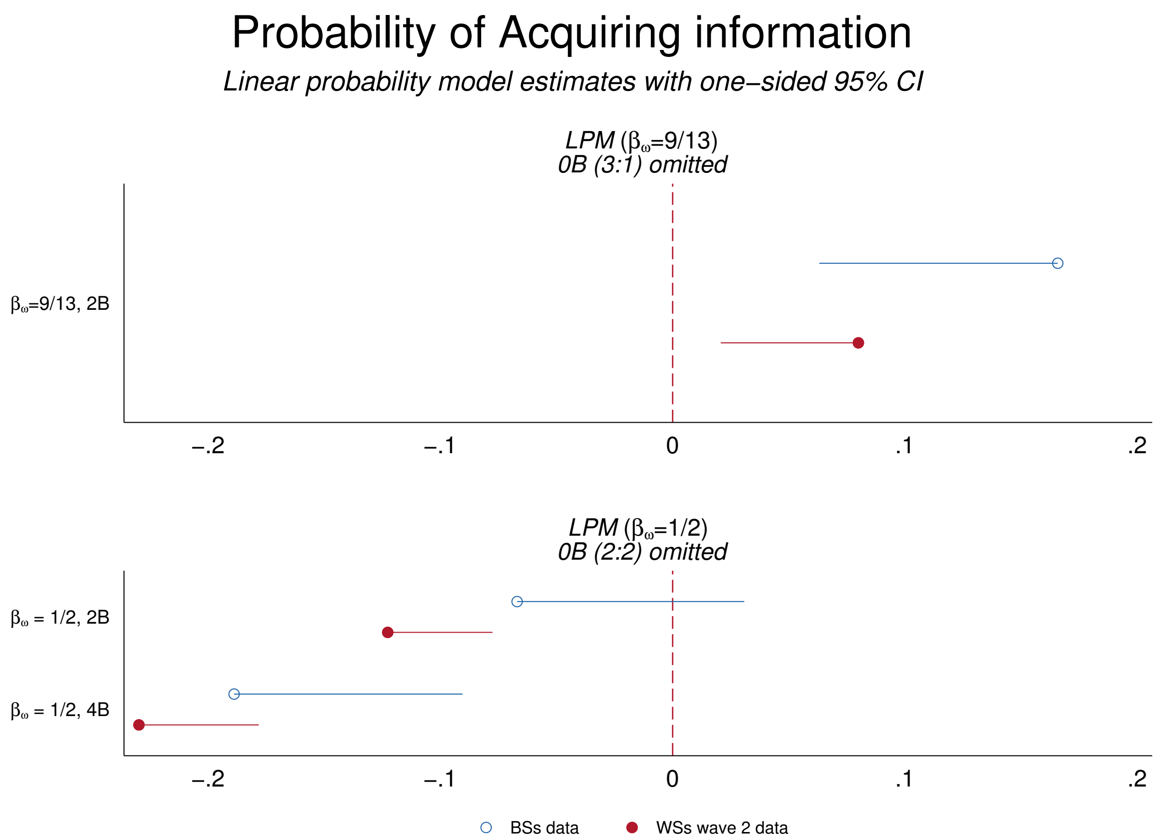


Figure 2: Coefficients plots for linear probability estimates of the effects of promising future information on the decision to acquire information (extensive margin). Results from joint estimates using all informational cases. Top panel ($\beta_\omega = 9/13$) presents estimates of promising two balls ($2B$) after information acquisition relative to the omitted category of no information ($0B$) in the future. Bottom panel presents estimates of promising two ($2B$) and four ($4B$) balls after information acquisition relative to the omitted category of no information ($0B$) in the future for the rare knife-edge case of indifference ($\beta_\omega = 1/2$).

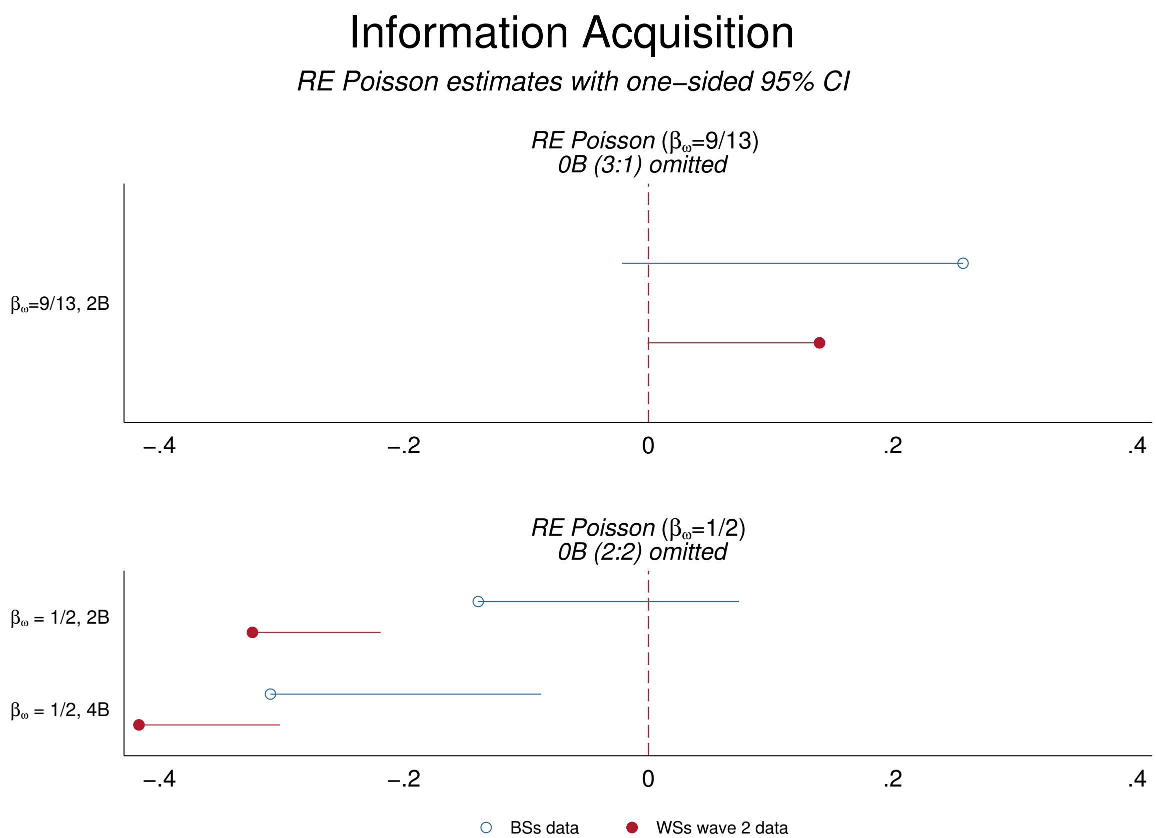


Figure 3: Coefficients plots for random effects Poisson model estimates of the effects of promising future information on information acquisition decisions (intensive margin). Results from joint estimates using all informational cases. Top panel ($\beta_\omega = 9/13$) presents estimates of promising two balls ($2B$) after information acquisition relative to the omitted category of no information ($0B$) in the future. Bottom panel presents estimates of promising two ($2B$) and four ($4B$) balls after information acquisition relative to the omitted category of no information ($0B$) in the future for the rare knife-edge case of indifference ($\beta_\omega = 1/2$).

A Additional Tables

Table 3: Summary statistics by treatment

Treatment	Subjects	Share No Info		Avg. Balls Purchased		Avg. Correct	
		Predicted	Observed	*Predicted*	Observed	*Predicted*	Observed
Between-subjects data							
B_4	52	*0.749*	0.453	*0.675*	1.35	*0.695*	0.684
B_2	52	*0.756*	0.406	*0.662*	1.35	*0.684*	0.690
B_0	52	*0.744*	0.541	*0.577*	1.101	*0.685*	0.708
Within-subjects data							
B_4	28	*0.736*	.546	*0.641*	1.14	*0.683*	.676
B_2	28	*0.755*	.517	*0.570*	1.07	*0.660*	.667
B_0	28	*0.726*	.589	*0.595*	.984	*0.684*	.657

Table 4: Summary statistics for main task (BSs data) by wave

Updated Prior	Situation	Obs.	Share No Info		Avg. Balls Purchased		Avg. Correct	
			Predicted	Observed	*Predicted*	Observed	*Predicted*	Observed
UFM								
	B_4	576	*0.743*	0.340	*0.753*	1.679	*0.733*	0.724
$\beta_\omega = 1/2$	B_2 (1 : 1)	249	*0.606*	0.213	*1.18*	1.928	*0.622*	0.711
	B_0 (2 : 2)	200	*0.345*	0.200	*1.3*	1.935	*0.502*	0.660
$\beta_\omega = 9/13$	B_0 (2 : 0)	327	*0.865*	0.339	*0.44*	1.327	*0.775*	0.734
	B_0 (3 : 1)	279	*0.971*	0.577	*0.143*	0.993	*0.742*	0.767
$\beta_\omega = 81/97$	B_0 (4 : 0)	97	*0.969*	0.742	*0.155*	0.588	*0.907*	0.866
ESI								
	B_4	672	*0.754*	0.549	*0.609*	1.058	*0.663*	0.650
$\beta_\omega = 1/2$	B_2 (1 : 1)	332	*0.684*	0.434	*0.726*	1.268	*0.617*	0.596
	B_0 (2 : 2)	223	*0.296*	0.300	*1.34*	1.547	*0.538*	0.578
$\beta_\omega = 9/13$	B_0 (2 : 0)	340	*0.832*	0.585	*0.432*	1.015	*0.706*	0.724
	B_0 (3 : 1)	335	*0.946*	0.719	*0.269*	0.737	*0.71*	0.666
$\beta_\omega = 81/97$	B_0 (4 : 0)	114	*0.974*	0.825	*0.132*	0.535	*0.886*	0.886

Notes: Summary statistics by treatment and information scenario for the main task (24 periods) of each wave, according to the updated priors generated by the informational case (situation). "Obs." denotes the number of observations collected in each informational case. "Share No Info" is the rlative number of instances where no information was acquired. "Avg. Balls Purchased" denotes the unconditional number of balls purchased. "Avg. Correct" is the share of instances of correct state of the world predictions (color of jar guesses).

Table 5: Information acquisition: extensive margin

	Dependent variable: Purchased any balls?			
	(1)	(2)	(3)	(4)
$\beta_\omega = \frac{1}{2}$, *2B*	0.122	1.325	0.107	0.970
	[0.054]	[0.542]	[0.025]	[0.209]
$\beta_\omega = \frac{1}{2}$, *0B*	0.189	1.675	0.230	1.932
	[0.060]	[0.577]	[0.031]	[0.277]
$\beta_\omega = \frac{9}{13}$, *2B*	-0.021	-0.169	-0.044	-0.415
	[0.061]	[0.551]	[0.032]	[0.277]
$\beta_\omega = \frac{9}{13}$, *0B*	-0.187	-1.535	-0.124	-1.179
	[0.055]	[0.466]	[0.035]	[0.327]
$\beta_\omega = \frac{81}{97}$, *0B*	-0.371	-2.977	-0.266	-2.470
	[0.059]	[0.577]	[0.046]	[0.522]
Cost	-0.006	-0.054	-0.007	-0.059
	[0.000]	[0.004]	[0.000]	[0.005]
Period	0.010	0.080	0.001	0.007
	[0.004]	[0.035]	[0.002]	[0.015]
Period^2	-0.000	-0.002	-0.000	-0.002
	[0.000]	[0.001]	[0.000]	[0.001]
Constant	0.892	3.449	0.726	2.677
	[0.050]	[0.488]	[0.029]	[0.404]
σ_u^2		1.741		1.767
		[0.187]		[0.221]
Individual fixed effects?	No	No	Yes	No
Wave / Task Order?	Yes	Yes	Yes	Yes
Mean of dependent variable	0.533	0.533	0.446	0.446
P-values for one-tailed Wald tests				
$\beta_\omega = \frac{9}{13}$: *2B* (2:0) = *0B* (3:1)	0.004	0.005	0.015	0.009
$\beta_\omega = \frac{1}{2}$: *2B* (1:1) = *0B* (2:2)	0.131	0.283	0.000	0.000
0B: 2:2 = 3:1	0.000	0.000	0.000	0.000
2B: 1:1 = 2:0	0.000	0.000	0.000	0.000
Number of Observations	3744	3744	4032	4032
Number of clusters	156	156	84	84
Log Likelihood	-2198.3	-1490.1	-1459.3	-1513.1
BIC	4478.8	3070.6	3001.5	3125.9
AIC	4416.6	3002.1	2938.5	3050.2

Linear probability model (1 and 3) and random effects logit model (2 and 4) estimates of the probability of information acquisition. Estimates using between- (within-) subjects data reported in columns 1 and 2 (3 and 4).Robust standard errors clustered at the individual level in brackets. Omitted treatment variable is baseline treatment *4B* ($\beta_\omega = \frac{1}{2}$): no free information before decision to acquire information.

Table 6: Information acquisition: intensive margin

	Dependent variable: Number of balls purchased			
	(1)	(2)	(3)	(4)
$\beta_\omega = \frac{1}{2}$, *2B*	0.170	0.551	0.093	0.469
	[0.123]	[0.361]	[0.054]	[0.112]
$\beta_\omega = \frac{1}{2}$, *0B*	0.309**	1.065	0.417	1.229
	[0.135]	[0.363]	[0.070]	[0.120]
$\beta_\omega = \frac{9}{13}$, *2B*	-0.112	-0.371	-0.166	-0.446
	[0.141]	[0.361]	[0.068]	[0.113]
$\beta_\omega = \frac{9}{13}$, *0B*	-0.370	-0.950	-0.306	-0.876
	[0.157]	[0.363]	[0.086]	[0.120]
$\beta_\omega = \frac{81}{97}$, *0B*	-0.889	-2.096	-0.892	-1.953
	[0.226]	[0.392]	[0.218]	[0.215]
Cost	-0.024	-0.055	-0.030	-0.066
	[0.001]	[0.001]	[0.002]	[0.002]
Period	0.029	0.069	0.005	0.002
	[0.010]	[0.020]	[0.005]	[0.007]
Period^2	-0.001	-0.002	-0.000	-0.001
	[0.000]	[0.001]	[0.000]	[0.001]
Constant	1.210*	3.191	0.919	3.031
	[0.118]	[0.318]	[0.076]	[0.247]
$\ln(\alpha)$	-0.645		-18.850	
	[4.044]		[4.108]	
$\ln(\sigma_u)$		1.757		2.048
		[0.115]		[0.172]
$\ln(\sigma_e)$		1.694		1.823
		[0.033]		[0.037]
Individual fixed effects?	No	No	Yes	No
Wave / Task Order?	Yes	Yes	Yes	Yes
Mean of dependent variable	1.264	1.264	1.066	1.066
P-values for one-tailed Wald tests				
$\beta_\omega = \frac{9}{13}$: *2B* (2:0) = *0B* (3:1)	0.065	0.058	0.051	0.001
$\beta_\omega = \frac{1}{2}$: *2B* (1:1) = *0B* (2:2)	0.141	0.080	0.000	0.000
0B: 2:2 = 3:1	0.000	0.000	0.000	0.000
2B: 1:1 = 2:0	0.000	0.000	0.000	0.000
Number of Observations	3744	3744	4032	4032
Number of clusters	156		84	
Log Likelihood	-4469.7	-4523.1	-3977.3	-4208.1
BIC	9029.9	9145.0	8104.0	8524.2
AIC	8961.4	9070.3	7990.6	8442.2

Random effects Poisson model (1 and 3) and random effects Tobit model (2 and 4) estimates of the amount of information acquired (number of balls purchased). Estimates using between- (within-) subjects data reported in columns 1 and 2 (3 and 4).Standard errors (clustered at the individual level for specifications 1 and 3) in brackets. Omitted treatment variable is baseline treatment *4B* ($\beta_\omega = \frac{1}{2}$): no free information before decision to acquire information.

Table 7: Correct choice

	Dependent variable: Choose correct color of jar			
	(1)	(2)	(3)	(4)
$\beta_\omega = \frac{1}{2}$, *2B*	-0.035	-0.157	-0.067	-0.257
	[0.027]	[0.126]	[0.022]	[0.096]
$\beta_\omega = \frac{1}{2}$, *0B*	-0.067	-0.312	-0.122	-0.493
	[0.028]	[0.125]	[0.033]	[0.138]
$\beta_\omega = \frac{9}{13}$, *2B*	0.041	0.199	0.038	0.201
	[0.024]	[0.120]	[0.024]	[0.111]
$\beta_\omega = \frac{9}{13}$, *0B*	0.029	0.148	0.007	0.050
	[0.023]	[0.114]	[0.021]	[0.097]
$\beta_\omega = \frac{81}{97}$, *0B*	0.188	1.184	0.166	0.992
	[0.030]	[0.250]	[0.030]	[0.204]
Cost	-0.000	-0.002	-0.001	-0.004
	[0.000]	[0.001]	[0.000]	[0.001]
Period	-0.009	-0.048	-0.001	-0.002
	[0.004]	[0.021]	[0.001]	[0.006]
Period^2	0.000	0.002	0.000	0.001
	[0.000]	[0.001]	[0.000]	[0.001]
Constant	0.773	1.250	0.712	0.849
	[0.031]	[0.161]	[0.018]	[0.102]
σ_u^2		-2.430		-3.725
		[0.378]		[0.852]
Individual fixed effects?	No	No	Yes	No
Wave/Task order?	Yes	Yes	Yes	Yes
Mean of dependent variable	0.694	0.694	0.666	0.666
P-values for one-tailed Wald tests				
$\beta_\omega = \frac{9}{13}$: *2B* (2:0) = *0B* (3:1)	0.307	0.343	0.112	0.104
$\beta_\omega = \frac{1}{2}$: *2B* (1:1) = *0B* (2:2)	0.161	0.140	0.058	0.047
0B: 2:2 = 3:1	0.000	0.000	0.000	0.000
2B: 1:1 = 2:0	0.002	0.002	0.000	0.000
Number of Observations	3744	3744	4032	4032
Number of clusters	156	156	84	84
Log Likelihood	-2367.9	-2253.7	-2595.1	-2523.0
BIC	4818.2	4597.9	5273.2	5145.6
AIC	4755.9	4529.4	5210.2	5070.0

Linear probability model (1 and 3) and random effects logit model (2 and 4) estimates of the probability of correctly predicting the state of world (guessing the color fo the jar). Estimates using between- (within-) subjects data reported in columns 1 and 2 (3 and 4).Robust standard errors clustered at the individual level in brackets. Omitted treatment variable is baseline treatment *4B* ($\beta_\omega = \frac{1}{2}$): no free information before decision to acquire information.

B Additional Figures

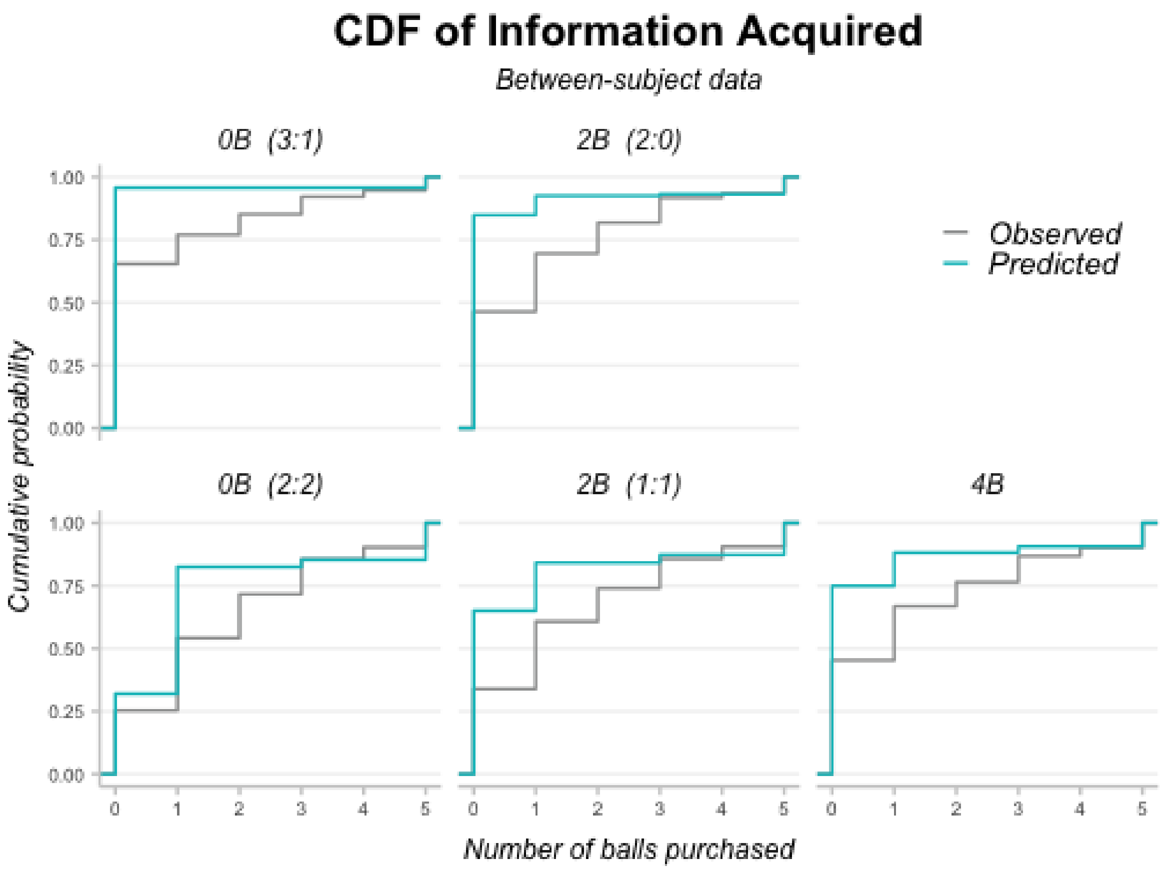


Figure 4: CDF of information purchased by individuals during last 12 periods, by treatment and information scenario for BSs

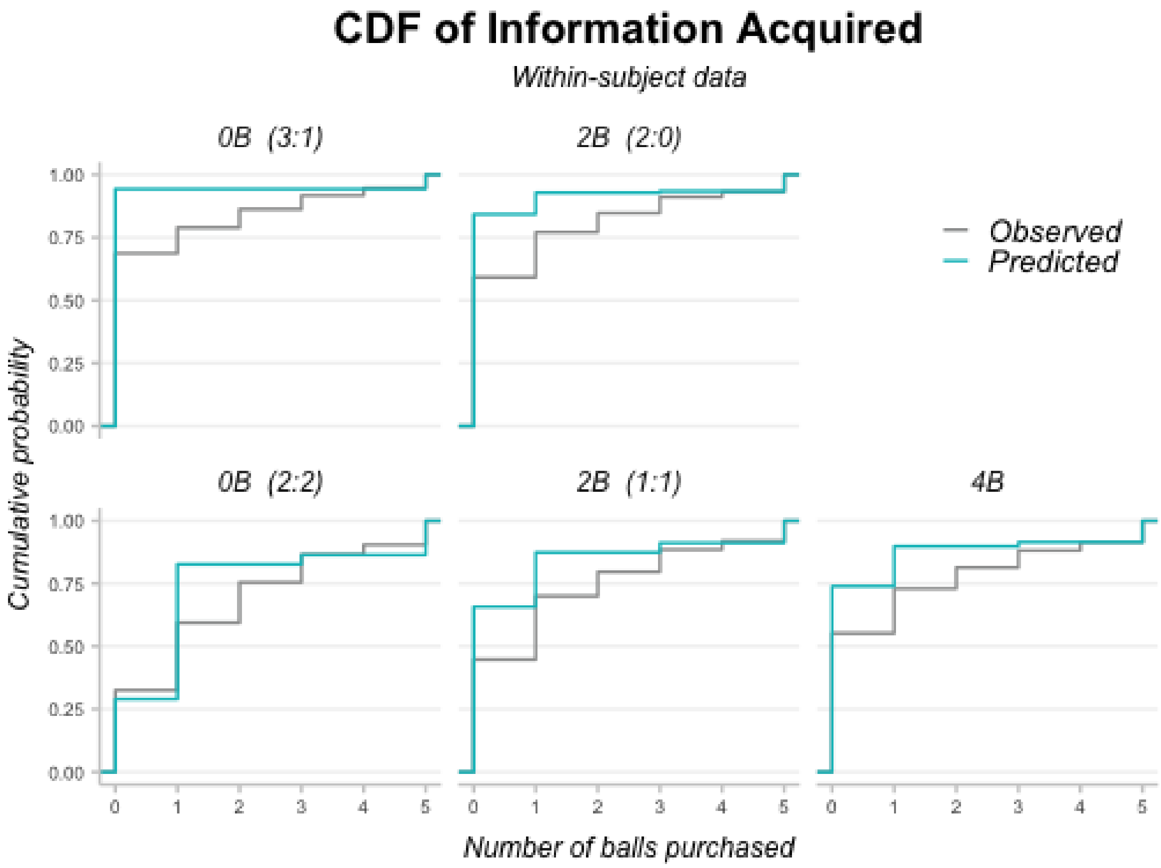


Figure 5: CDF of information purchased by individuals during last 12 periods, by treatment and information scenario for WSs

C Experimental Protocols

Upon arrival, subjects checked-in and assigned to a computer through which they interacted. Paper copies of the instructions were distributed and video of instructions with audio were played at the beginning of each experimental task. After instructions for each part and before subjects made decisions, they had to pass a test to control for comprehension of instructions. Subjects had to correctly answer all multiple-choice questions before moving on to the main task.[25] They were allowed one incorrect attempt per question before the screen locked and an experimenter was prompted. After each attempt (correct or incorrect), feedback was given to subjects to reinforce learning. For our main task, the test questions controlled understanding for: baseline probability of each state of the color of the jar (.5), conditional probability of balls being different from the true color of the jar (.4), probability of independent draws from the same color (.6), estimating earnings for guessing correctly after purchasing information, and independence of draws for the color of jars.

Across both waves, the first task consisted of 24 periods of our maininformation acquisition experimental task (for BSs data). The second task was a risk elicitation task using a multiple price list framed in the context of jars as state of the world. The third and fourth tasks were different across waves 1 and 2. In wave 1 our third (fourth) task was 12 periods of a task intended to capture independence neglect (base-rate neglect). In wave 2 sessions, we dropped the exploratory independence and base-rate neglect tasks. Instead, tasks 3 and 4 were each 12 periods of our main information acquisition task, changing treatments. In sessions 3 and 4 participants received different amounts of information after their information acquisition decision with respect to previous task(s). These tasks give rise to our within-subjects data.

Participants knew that one of the four tasks was to be randomly selected at the end, and their choice in a single (randomly selected) period for the task was to determine their earnings from the experiment. The experimental earnings was the sum of the randomly selected choice plus the show-up fee and a starting balance of E$120. This was determined upon completion of the four tasks.

After completing the four tasks, participants learned their payments and took part in a post experimental survey. In the post-experimental survey we collected general demographic data (gender, age, number of siblings), unincentivized cognitive reflection test (Frederick, 2005), major and school, self reported GPA, familiarity with Bayes Theorem, number of math and stat courses taken and previous participation in research experiments.

Each session lasted about 90 min, including survey and private payment. The experiment interface was programmed using zTree (Fischbacher, 2007). Software and supporting materials are available here.

Wave 1 had between subject treatment variation, within the same session. That is, subjects within each session were randomly assigned to one of the three treatments and received video (with audio delivered via headsets) for the corresponding treatment. After pilot sessions, we discarded data from 1 session from wave 1 due to problems with the display of instruction videos in several computers that was not mentioned/revealed until the end of the session. We report data from six 12-subject sessions in wave 1.

Wave 2 had between subject treatment variation (assigned at the session level). In addition, we have within-subject data by comparing behavior of an individual in tasks 1, 3 and four. We conducted six 14-subject sessions perfectly balanced within subject treatment order.

[25]Number of questions for each task were between 3-5. Appendix D.2 shows the questions for the main task.

D Instructions

This section presents the script used to run the wave 2 sessions and the slides to present instructions.[26] Appendix section D.1 presents the instructions slides for the main task *4B* *2B* *0B* treatment. Text in black is common across all treatments. Instructions were presented through a video with audio that narrated the slides. Full video of instructions available here.

Appendix sub-section D.2 presents the control questions that followed the instructions. Appendix sub-section D.3 presents the script used to conduct wave 2 sessions.

D.1 Instruction Slides

Overview

This is an experiment about economic decision making. Various agencies have provided funds for this research. If you understand the instructions (and depending on your decisions), you can earn a considerable sum of money. At the end of today's session you will receive your earnings in cash, in private. In this experiment, the sums of money are expressed in Experimental Dollars ($E\$$). At the end of today's session, we will convert your earnings into US Dollars, at an exchange rate $E\$40 = \$1$. For today's session, you will receive an initial payment of $E\$120$.

It is important that you remain silent and not look at other people's work. If you have any questions, or need help of any kind, please raise your hand and an experimenter will come to you. If you speak aloud, you will be asked to leave the experiment. We expect and appreciate your cooperation.

Now we will describe the session in more detail.

Overview

Today's experiment consists of four parts. In each part you will make some decisions. The other participants will face similar decision-making tasks. However, their decisions will not affect your earnings and you decisions will not affect their earnings.

When all four parts have been completed, one of those parts will be randomly chosen. In the selected part one of your decisions will be chosen randomly and the result of said decision will determine your earnings.

Your final earnings for today's session will be the sum of your initial payment of $E\$120$ and your earnings in the decision which is randomly selected.

Introduction: Part 1

In the first part of the experiment there are twenty-four periods. In each period there are two jars, one red and one blue. Each jar contains 60 balls which are the same color as the jar, and 40 which are the color of the other jar.

The computer will randomly select one of the two jars. You must predict the color of the jar selected for that period. If you predict the color of the jar, you can earn up to E $\$1,000$.

To help you predict, you can observe the color of some balls from the jar. The computer will show you 4 balls for free and you will have the option to buy up to 5 additional balls (at a COST).

[26] We choose instructions for wave 2 since they are in English. Instructions for wave 1 are available from the authors upon request.

Next, we will explain this part of the experiment in m ore detail.

Jars and balls

At the beginning of each period the computer randomly selects one of two virtual jars: red or blue. The probability that the color of the jar is red is 50% and the probability that it is blue is 50%.

The color of the jar in one period does not affect the color of the jar in another period or of another participant. That is, the true color of jar is determined independently of the color of jar in another period and for other participants.

Each jar is filled with 100 virtual balls; 60 correspond to the true color of the jar and 40 are the color of the other jar. That is, the red jar contains 60 red balls and 40 blue balls. The blue jar contains 60 blue balls and 40 red balls.

In this way, the color of a ball corresponds to the true color of jar with a probability of 60%. That is, if the true color of jar is red, when drawing a ball, it will be red with 60% probability and will be blue with 40% probability. If the true color of the jar is blue, when drawing a ball, it will be blue with 60% probability and will be red with 40% probability.

Obtaining Information

Before you make your prediction about the color of the jar, you will get information by observing the colors of several balls drawn from the jar which has been selected for that period. This will work in the following way:

The computer draws a ball (chosen at random) from the jar for that period and records its color. It then deposits the ball back into the jar and all the balls are mixed. Then, another ball is randomly drawn and the color is recorded. The ball is again deposited in the jar and again the balls are mixed. This process continues until all the balls shown in the period have been drawn and recorded.

That is, each time the computer draws a ball from the jar, 60% of balls in the jar are the same color as the jar, and 40% of the balls are the other color.

Obtaining Information

Each period the computer will show you the colors of 4 balls taken from the jar at no cost. In addition, you can buy additional balls.

[You will be able to see the colors of the 4 balls that the computer will show you at no cost AFTER you decide how many balls to buy.]

[Out of the 4 balls that the computer will show you at no cost, you will be able to see the colors of 2 of these balls BEFORE you decide how many balls to buy. The colors of the other 2 balls will be revealed AFTER you decide how many you want to buy.]

[You will be able to see the colors of the 4 balls that the computer will show you at no cost BEFORE you decide how many balls to buy.]

Each additional ball you buy will have a COST. In each period, the COST of the balls will be determined randomly and will be a number between 0 and 100, all being equally probable. (The COST in one period does not affect that of other periods or other participants.)

After having seen [the colors of the 4 [2] free balls and] the COST of the additional balls, you will decide how many you want to buy, if you want to buy any.

After you decide the number of balls you want to buy, you will see the colors of the free balls and of the additional balls purchased.

Earnings from the prediction

After observing the colors of all the balls, you make your prediction regarding the color of the jar for that period. If your prediction is correct, you will earn $E\$1,000$. If you do not correctly predict the color, you will earn $E\$300$. Regardless of whether you are correct or not, you will have to pay the COST of the balls you bought that period.

That is, if your prediction about the color of the jar is correct, your earnings for the period will be given by:

$$E\$1,000 - \text{COST} \cdot (\text{\# of balls purchased})$$

If your prediction is not correct, your earnings for the period will be given by:

$$E\$300 - \text{COST} \cdot (\text{\# of balls purchased})$$

Feedback

During the first twelve periods, you will not be able to observe the true color of the jar at the end of each period. In the second twelve periods, you will be able to observe the true color of the jar at the end of each period.

Summary

1. In each period there are two jars and the computer will select one of these at random: the RED jar with 50% probability or the BLUE jar with 50% probability.
2. You will observe for free the colors of 4 balls. In addition, you will have the option to buy 0 to 5 additional balls, at a COST (selected randomly from between 0 to 100 for each period).
3. The color of each ball corresponds to the true color of the jar with 60% probability.
4. You must predict the color of the jar selected for that period. If your prediction is correct, you earn:
$$E\$1,000 - \text{COST} \cdot (\text{\# of balls purchased})$$
5. If you do not correctly predict the color of the jar, then your earnings for the period are:
$$E\$300 - \text{COST} \cdot (\text{\# of balls purchased})$$

D.2 Control Questions

After the instructions video ended, participants had to complete an Instructions Comprehension Test which consisted of the following (multiple choice) questions:

1. What is the probability that at the beginning of the period the computer selects the color RED jar?

2. If at the beginning of the period the computer randomly selects the BLUE jar, what is the probability that when drawing a ball from the jar it is a RED ball?

3. Assume that the jar selected at the beginning of the period is RED. Assume also that the computer has already drawn the 4 balls that it will show, and they are all red. What is the probability that if you draw an additional ball, it is RED?

4. Assume you decide to purchase three balls, at a cost of E$50 each. Also, assume you correctly predict that the jar color is BLUE. If this decision is chosen at random for your payment, what would be your profit?

5. Assume that for the last period, the true color of the jar selected by the computer was BLUE. What is the probability that for the next period the computer randomly selects the BLUE jar?

Participants had to answer all questions correctly in order to proceed to the experiment. If participants selected the correct answer for a question, they received feedback and reinforced the explanation for the correct response.

If participants selected the wrong answer, they received feedback and had a chance to answer again. If they selected an incorrect answer for a second time, the screen was locked (and requested a code that the experimental monitors had). It asked them to raise their hand so that the experimental monitor could clarify any questions or misunderstandings the subject could have.

After all participants had correctly answered all questions, they proceeded with the experimental task.

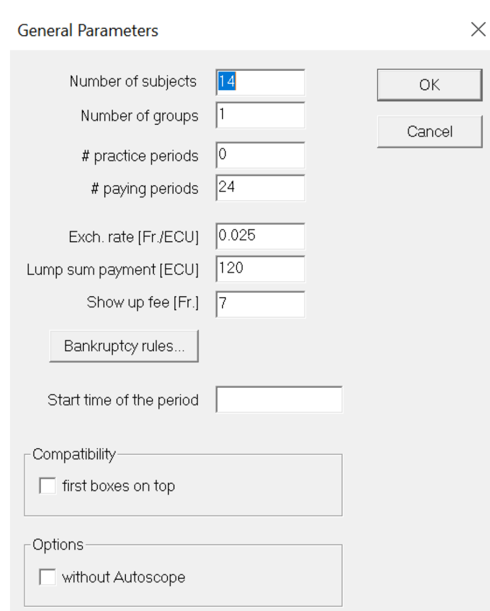
D.3 Script

Script VOI

<u>30 minutes before the session begins</u>:

1. Restart the monitor computer and the computers in the subjects' room
2. Print VOI Script, *InstrutionsComprehensionTest-Solution&Codes.pdf,* and materials for subjects:
   - Instructions VOI T#__part-1.pdf
   - Instructions _ VOI_Risk__part-2.pdf
   - RiskTask.pdf
   - VOI T# Handout part 3.pdf
   - VOI T# Handout part 4.pdf
3. Open the session # folder in the computer in the monitor room
4. Leave printed instructions <u>for part 1</u> in each subjects' computers
5. Turn on TV monitors and prepare to project the ppt "…Instructions – part 1" (Note: make sure the zTree screen is never projected; you can project from a different PC or use extend instead of duplicate mode).
   - Test to make sure volume is on at an appropriate level.
6. In the monitor computer, open zTree and open the following zTree treatment files (.ztt):
   - 1-VOI Task_english.ztt
   - 2-Risk Task_english.ztt
   - 3-VOI Task_english.ztt
   - 4-VOI Task_english.ztt
   - 5-Survey-VOI-english.ztq
7. In addition to the *client´s table*, also open the *subjects table*, *session table* and *parameters table*.
8. Change language to English in ztree treatments: *Treatment >> Language >> English* and in the zTree questionnaire (5-Survey-VOI-english.ztq) *Questionnairre >> Language >> English*

Note that *Number of subjects* need not be adjusted, unless fewer than the number of subjects recruited (14) show-up for the experiment. In that case, only the *Number of subjects* should be adjusted (and it should be adjusted in all four (4) .ztt files. No need to adjust *Number of groups.* Matching should not matter, but if need to adjust, do: *Treatment >> matching >> partner*.



Do not open the zleafs  yet in the subjects' computers.

Please make sure that these 3 JPG files are in all of the subjects' computers in *C:\Experiments\VOI*

- image_risk_options.jpg
- image_risk_JA.jpg
- image_risk_JB.jpg

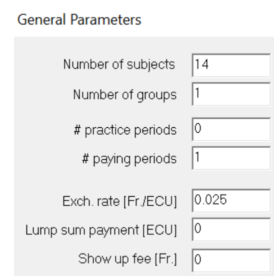
Once subjects are seated:

Read aloud to subjects: *"Welcome. Today's experiment consists of four parts. The instructions for each part will be explained through videos. Videos in the screens in front will go over the instructions. You may follow the instructions on the instruction sheets provided. At the end of the instructions, you will participate in an instructions comprehension test to ensure that everyone understands the instructions."*

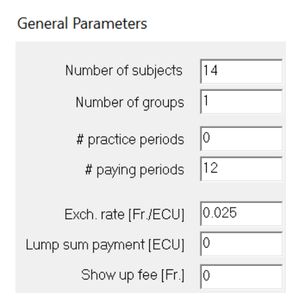
1. Start the ppt "*VOI…Instructions – part 1*"
   - When the video ends, launch the z-leafs.

2. Run ***1-VOI Task_english.ztt*** , once all subjects are connected
   - Be mindful if subjects raise their hand. If someone does raise their hand, go out (take *InstrutionsComprehensionTest-Solution&Codes.pdf*) and enter the code in their screen, make sure they understand the question they got wrong.

3. Once everyone finishes, distribute instructions for part 2 (including RiskTask.pdf handout). Start the ppt "*VOI_Risk – part 2*"
   - When the video ends, run ***2-Risk Task_english.ztt***
   - (Note: same number of subjects as in previous task, 1 group, 0 practice periods, 1 paying period, Exchange rate 0.025, lump sum payment 0, show up fee 0)
   - Be mindful if subjects raise their hand. If someone does raise their hand, go out (take *InstrutionsComprehensionTest-Solution&Codes.pdf*) and enter the code in their screen, make sure they understand the question they got wrong.



4. Distribute instructions handout for part 3. Start the ppt "*VOI…Handout – part 3*"
   - When the video ends, run ***3-VOI Task_english.ztt***
   - (Note: same number of subjects as in previous task, 1 group, 0 practice periods, 12 paying period, Exchange rate 0.025, lump sum payment 0, show up fee 0)
   - [Note that there is no instructions comprehension test for this part]



5. Distribute instructions handout for part 4. Start the ppt "*VOI…Handout – part 4*"
   - When the video ends, run ***4-VOI Task_english.ztt***
   - (Note: same number of subjects as in previous task, 1 group, 0 practice periods, 12 paying period, Exchange rate 0.025, lump sum payment 0, show up fee 0)
   - [Note that there is no instructions comprehension test for this part]

   Read aloud to subjects: *"The experiment is over. Thank you for your participation. While we prepare your payments, we ask that you complete a short questionnaire."*

6. Run ***5-Survey-VOI-english.ztq*** (while subjects complete the questionnaire, prepare to pay subjects.)
   - The amount to pay each subject is in the $MoneyToPay$ variable, in the *session table*.
   - The zTree generated pay file ($*.pay$) will have the corresponding payment to each subject, with the subject name.

7. Pay subjects

Once subjects have left:

1. Close zleafs. Close (and save) zTree .ztt files.
    - Put all data and zTree files in the *data* folder in the corresponding session.

2. Record log file for the session.