Peer betting to elicit unverifiable information*

Aurélien Baillon[1], Cem Peker[2], and Sophie van der Zee[3]

[1]baillon@em-lyon.com, Emlyon Business School & GATE Lyon-Saint-Etienne UMR 5824

[2]cem.peker@nyu.edu, Division of Social Science, NYU Abu Dhabi

[3]vanderzee@ese.eur.nl, Erasmus School of Economics, Erasmus University Rotterdam

February 17, 2025

Abstract

We introduce a transparent incentive mechanism to elicit answers to binary questions that cannot be verified for accuracy. Agents choose whether to receive a costly private signal, which leads them to endorse "yes" or "no" as an answer. Then, they either bet that the rate of "yes" answers is higher or lower than prior expectations. We obtain a separating equilibrium, where agents want signals and they bet as a function of their signal. Two experimental studies test the theoretical results. The first shows that the mechanism motivates costly information acquisition, the second that it motivates signal revelation when answers are mildly stigmatizing. No alternatives so far combined transparency and unbiasedness in a single question.

1 Introduction

"Have you stood less than 6 feet apart from another person in a queue yesterday?" "Did you have a good stay in hotel H?" Health surveys and customer reviews regularly require respondents to recollect past experiences. These experiences can be

*The research was made possible by ERC Starting Grant 638408. The experiments reported in this paper received IRB approvals from Erasmus University Rotterdam, ERIM Internal Review Board, Section Experiments. The approvals were registered under number 2020/04/22-65868ape (Study 1) and number 2020/10/16-65868ape (Study 2).

seen as private signals that respondents acquire by exerting effort (recalling, to their mind, what they did a day earlier, or whether they had a good stay a week earlier). But how can we ensure that the respondents will, first, provide such effort and then, answer accurately if there is no way to compare their answer to some ground truth?

Without observing the ground truth (what actually happened), rewarding accuracy to motivate respondents to acquire and reveal private signals is impossible. Scoring rules or contracting on the state space are not feasible. The Bayesian mechanism design literature offers alternatives (e.g. Crémer and McLean, 1988; Miller, Resnick, and Zeckhauser, 2005). However, these alternatives have been mostly unexploited in surveys and experiments so far because they tend to be too complex to explain in laypeople terms.

In this paper, we borrow an old idea from the literature originating with Crémer and McLean (1988): proposing a side bet on others' signals to extract information. In our case, however, the bet is the central piece. The novelty is twofold: (i) we develop a simple version of this mechanism, and (ii) this simplicity allows us to transparently implement it in online experiments and surveys. Papers developing similar mechanisms have mostly shied away from implementing them, and implementations often resorted to the "intimidation method", i.e., telling people it is in their interest to tell the truth.[1] Transparency and simplicity may help make mechanisms be not only incentive compatible, but also behaviorally so (Danz, Vesterlund, and Wilson, 2022).

The mechanism introduced in this paper is called *peer betting*. When asked a yes-no question, yes-respondents are rewarded with the formula "the rate of yes answer minus common prior expectation of the rate of yes answer". Those who answer no get the opposite reward. This formula makes use of the fact that a Bayesian respondent whose own private signal is yes will *increase* their expectation about the proportion of other people answering yes. They will thus expect a positive payoff if they reveal their yes signal. Those with private signal no will *decrease* their posterior expectations of yes answer rate with respect to the prior, and therefore also expect a positive payoff by revealing their no signal.

Formally, the changes in expectations are direct implications of Bayesian updating when respondents draw a private signal (yes/no), with unknown probability p of yes

[1] See for instance the implementation of Bayesian truth serum in John, Loewenstein, and Prelec (2012),Frank, Cebrian, Pickard, and Rahwan (2017) and Baillon, Bleichrodt, and Granic (2022). Subjects were not given the details of scoring. Instead, they were only told that the incentive mechanism is based on a paper published in *Science* and it rewards truth-telling.

signals: a yes (no) signal makes higher (lower) values of p more likely than initially believed.[2] Intuitively, a yes (no) signal to the 6-feet-apart question can suggest that others also had (no) difficulty complying with social distancing guidelines. A bad hotel stay also makes it more likely that others will have bad stays as well. Signals bring information about the unobserved state of nature.

First, we show that signal acquisition and revelation is a Bayesian Nash equilibrium, providing a partial-implementation solution. The solution is minimal, in the sense that it does not ask respondents to provide more than their answer. It does not require the surveyor to share more than prior expectations with the respondents. We then extend our analysis to incorporate psychological costs, capturing the possible discomfort of reporting a mildly stigmatizing answer and lying aversion or preference for truth-telling (Abeler, Nosenzo, and Raymond, 2019).

Second, we test peer betting in an online experiment closely following the theoretical model and show that it incentivizes costly signal acquisition: respondents may exert an effort (i.e., complete a real-effort task borrowed from the experimental economics literature, Abeler, Falk, Goette, and Huffman (2011)) to obtain a signal and report the beliefs they derive from it; or they may simply answer randomly. We compare peer betting with two benchmarks: flat fee (no incentives) and accuracy incentives (incentives that reward ex-post accuracy when ground truth is observable). The former is commonly used when signals are unverifiable, the latter when signals are verifiable. Accuracy incentives are not applicable in most surveys, where the signals or states of nature are typically unobservable, but it provides a gauge for the effect of peer betting. In our experiment, accuracy incentives increase the effort rate by about 23 percentage points with respect to a flat fee. Peer betting allows us to achieve nearly two-third of this increase without relying on observing the signals or the states of nature.

Third, we demonstrate the feasibility of peer betting in a natural setting, where accuracy incentives are not possible, and show that it incentivizes signal revelation. We implement it in the context of a health survey, involving questions of the 6-feet-apart type during a pandemic period. Respondents bet whether non-compliance is higher than prior expectations, which are set to the previous week non-compliance rate. We hypothesize that people not exerting recollection efforts or feeling some slight discomfort for not complying with health guidelines are likely to deny having experienced

[2] We assume here that signals are conditionally independent, i.e. independent given the probability of getting a yes signal. The probability of yes signals is assumed to be itself drawn from a non-degenerate distribution over $(0, 1)$.

such situation, and therefore that peer betting will elicit higher non-compliance rates than a flat fee. Hence, even though ground truth cannot be verified, this second study can assess whether peer betting influences answers in the expected direction. We indeed find that more people admit experiencing situations in contradiction with health guidelines in the peer betting treatment than in the flat fee treatment. We rule out the alternative explanation that the mere mention of prior expectations in the bets influences answers. This second study shows that peer betting can be applied to socially relevant questions with unverifiable answers and when psychological costs of reporting non-compliance may be present.

When ground truth is unobservable and rewarding accuracy is impossible, peer betting offers a simple solution. It is based on a transparent payment rule and our two studies establish that it motivates signal acquisition and revelation, even when answers are (mildly) stigmatizing. The literature review below shows that no alternative combines transparency and unbiasedness in a single question.

Related literature - Since Myerson (1986) and Crémer and McLean (1988), the mechanism design literature has demonstrated the possibility to make people reveal their private information and extract the surplus they obtain from it. More recent papers have added information acquisition to the problem setting (e.g. Bikhchandani, 2010; Bikhchandani and Obara, 2017). This literature builds signal revelation mechanisms exploiting between-agent signal correlation to construct side bets on private signals of others. In that sense, the idea behind peer betting is quite old. However, we deviate from this literature in that in our case, the signal is not payoff-relevant. Agents do not derive any direct utility from their signal.[3]

The setting of the present paper originates from Miller, Resnick, and Zeckhauser (2005) and follow-ups (Witkowski and Parkes, 2012a; Waggoner and Chen, 2013; Witkowski and Parkes, 2013; Liu and Chen, 2017a). These papers have proposed solutions exploiting the informativeness of a respondent's answer in predicting their peers' answers. As common in this literature, signal revelation in our paper is not the only equilibrium, which is known as partial implementation. However, peer betting is more transparent than mechanisms from the peer prediction literature, which used scoring rules instead of simple bets. As a consequence, these methods have never been implemented in surveys. Our health survey in Section 4 illustrates the practical usability of peer betting. A mechanism close in spirit, using answer correlation to in-

[3] Our setting also differs from the (Bayesian) information design literature, where the payoff structure is fixed (Kamenica, 2017, 2019).

centivize truth-telling and implementable in survey, has been developed by Toussaert (2018) but it elicits beliefs, not signals.

The present paper is also the first of this stream of literature to include both cost of efforts and psychological costs in the model. It follows similar approaches proposed in the Bayesian persuasion literature (Gentzkow and Kamenica, 2014; Nguyen and Tan, 2021).

Peer betting relaxes the typical common prior assumption made, for instance, by Miller, Resnick, and Zeckhauser (2005), by requiring agents to share their prior *expectation*, instead of the full prior. Weakening assumptions on beliefs is central in the literature on (partial or full) implementation (Bergemann and Morris, 2005, 2009a,b). A mechanism is more robust if it provides incentive compatibility for a larger set of beliefs (Ollár and Penta, 2017, 2019).

Simple output-agreement mechanisms have been implemented to crowdsourcing problems, such as peer grading, content classification etc. Witkowski, Bachrach, Key, and Parkes (2013) study output agreement mechanisms, in which agents receive positive payment if their reports agree with their peers'. By creating a 'beauty contest', output agreement mechanisms do not achieve signal revelation when an agent believes to hold a minority signal, which may also affect effort decision. Peer betting do not have this limitations because it does not reward agreeing with the majority per se.

Methods to elicit private signals face the trade-off between *minimality* (Witkowski and Parkes, 2012a), i.e. asking only one question as we do, and being *detail-free*, i.e. not requiring specific knowledge from the center, to follow the desiderata of the Wilson doctrine (Wilson, 1987). The peer prediction literature and peer betting choose minimality. By contrast, the Bayesian truth serum (Prelec, 2004) and its variants (Witkowski and Parkes, 2012b; Radanovic and Faltings, 2013, 2014; Baillon, 2017) are detail-free. They do not require any knowledge of the prior. However, respondents are asked to provide some information about it on top of their answers. Cvitanić, Prelec, Riley, and Tereick (2019) proposes the most general form, even replacing the additional information about prior by another verifiable question. All these mechanisms are however not minimal and therefore more demanding to respondents than peer betting. They double the number of questions, which can be costly and penalize data quality.

Settings with multiple, correlated questions allow for minimal and detail-free methods. (Dasgupta and Ghosh, 2013; Shnayder, Agarwal, Frongillo, and Parkes, 2016; Baillon and Xu, 2021). These mechanisms use multiple questions and require specific assumptions about correlations across questions or shared signal technology, which peer betting do not require. The peer truth-serum for crowdsourcing is another mechanism which uses agents' responses to multiple questions (Radanovic, Faltings, and Jurca, 2016). Liu and Chen (2017b) develop sequential peer prediction, in which agents submit answers sequentially and the mechanism learns the optimal reward for effort elicitation over time. Sequential peer prediction is minimal, but unlike peer betting, requires a dynamic setup.

In binary elicitation problems, peer betting offers a simple minimal solution to incentivize signal acquisition and revelation. It is unbiased (unlike output agreement mechanisms) and transparent (unlike existing peer prediction mechanisms). It works in one-shot problems (unlike mechanisms using cross-questions correlations) and does not make surveys longer (unlike Bayesian truth-serums and follow-ups). For all these reasons, it can easily and successfully be implemented in surveys, as demonstrated below.

2 Theory

2.1 Agents and their information

A *center* (a researcher, a survey company) is interested in eliciting N *agents*' informed answers to a question Q, with possible answers $\{0, 1\}$. Agents can answer randomly at no cost but they may also decide to provide an effort (thinking, remembering, looking for information, etc.) to obtain their informed answer. Formally, agent $i \in \{1, \ldots, N\}$ can obtain a *signal* $s_i \in \{0, 1\}$ by providing *effort* $e_i = 1$ at a cost $c_i > 0$ (expressed in monetary terms). The cost of no effort ($e_i = 0$) is 0. There are two possible interpretations for s_i. It is either directly the informed answer to the question (agent i remembers what happened) or a signal that unequivocally influences the agent's opinion about the correct answer, i.e., signal 1 leads the agent to believe that answer 1 is correct and signal 0 induces the opposite belief. To keep notation minimal, we do not formally differentiate between signals and signal-induced beliefs. As usual in this literature (e.g., Prelec, 2004; Miller, Resnick, and Zeckhauser, 2005), we assume that the probability of getting signal 1 is the same for all agents (hence, it is independent of the effort cost) but is unknown. We model it as a random variable ω over $[0, 1]$. Denoting $s = (s_1, \ldots, s_N)$, a *state of nature* is thus a realization of ω and s, with the *state space* being $\Omega = [0, 1] \times \{0, 1\}^N$.The probability space is (Ω, Σ, P), with Σ the Borel $\sigma-$algebra of Ω and we assume that P is countably additive. The next assumption describes the full signal technology.

Assumption 1 (Signal technology)**.** *The signal technology is such that for all* $i, j \in \{1 \ldots, N\}$*,* $i \neq j$*, and* $o \in [0, 1]$*:*

1. $P(s_i = 1|\omega = o) = o$*;*

2. $P(s_i = 1|s_j, \omega = o) = o$*;*

3. *and* $P(\omega)$ *is continuous over* $[0, 1]$*.*

Part 1 of Assumption 1 states that the signal technology is anonymous, part 2 that it satisfies *conditional independence*, and part 3 that no value of ω has a probability mass. The latter excludes degenerate cases in which all agents could get the same signal for sure or in which ω would be known.

Let P_i represent the belief of agent i about the signal technology, and P_0 that of the center. It is common to assume $P_i = P_0 = P$ in peer prediction mechanisms.[4]. We allow agents to have different opinions on how likely various values of ω are but the following assumption restrict their belief in two ways.

Assumption 2 (Unbiased prior expectations)**.** *For all* $i \in \{0, \ldots, N\}$*,* P_i *satisfies properties 1-3 of Assumption 1 and* $E_i(\omega) = E(\omega)$*.*

Assumption 2 states that all agents and the center agree on the main properties of the signal technology and share the same prior expectation. It is a strong assumption, despite relaxing the often-used common prior assumption. Assumption 2 is plausible if (i) question Q is new and people have no reason to believe that answer 1 is more likely than answer 0, i.e., $E(\omega) = 0.5$; or (ii) signals of another group of agents have been publicly revealed (possibly with another mechanism); or (iii) the agents have no clue about ω but the center shares its prior expectation. In case (i), we do not need to assume uniform P_i over the possible values of ω; e.g., it can be bell-shaped for some agents. Case (ii) can correspond to situations in which question Q was asked in the past (to other agents) but the center and the (new) agents do not know whether the signal distribution will be exactly the same. For instance, imagine that, a month ago, it was published that 73% of people reported complying with a guideline. There are

[4]Or $P_i = P$ with no assumption on P_0 in the Bayesian truth-serum (Prelec, 2004) or Bayesian markets (Baillon, 2017)

many reasons why this proportion might change but before agents try to remember their own experience, 73% is a good average guess about what others will answer. Case (iii) may occur when the center has the means to study the signal technology; for instance, a review website where people report their (binary) experience with hotels or movies can study signal distribution and display prior average expectation. Let us denote $\bar{\omega} \equiv E(\omega)$, $\bar{\omega}_i^0 \equiv E_i(\omega|s_i = 0)$ and $\bar{\omega}_i^1 \equiv E_i(\omega|s_i = 1)$.

Lemma 1. *Under Assumptions 1 and 2, for all $i \in \{1, \ldots, N\}$, $0 < \bar{\omega}_i^0 < \bar{\omega} < \bar{\omega}_i^1 < 1$.*

All proofs are relegated to Appendix A. Lemma 1 shows that under our assumptions, all agents receiving signal 1 have higher expectations about ω than they had ex ante (and than the center) whereas agents with signal 0 decrease their expectations. Finally, we make the following assumption on agents' risk preferences:

Assumption 3 (Risk neutrality)**.** *Agents are risk neutral.*

Assumption 3 implies that agents maximize their expected payoffs. Section 2.2 introduces a betting mechanism to exploit the difference in expectations established in Lemma 1. Assumption 3 implies that agents' optimal strategy will not depend on risk attitude.

2.2 Peer betting

The center implements peer betting for Q. Payoff size is given by π, a scaling constant. If the currency is the dollar, $\pi = 10$ means that agents may earn up to \$10.

Definition 1. *The peer betting rules are:*

1. *The center announces the bet price $\bar{\omega}\pi$.*
2. *Agents simultaneously choose a report $r_i \in \{0, 1\}$. Those who report 1 become buyers of the bet and those who report 0 become sellers.*
3. *The center computes the bet final value $\bar{r}\pi = \frac{\pi}{N} \sum_{i=1}^{n} r_i$.*
4. *If $\bar{r} = 0$ or $\bar{r} = 1$, all bets are canceled; no payment occurs.*
5. *Otherwise, buyers pay $\bar{\omega}\pi$ to the center in exchange of $\bar{r}\pi$ and sellers receive $\bar{\omega}\pi$ from the center in exchange of $\bar{r}\pi$.*

Reporting a 1 answer ($r_i = 1$) means betting that the proportion of 1 answers will be higher than $\bar{\omega}$. Symmetrically, reporting a 0 answer is a bet on a proportion of 1 answers lower than $\bar{\omega}$. Step 5 specifies that all bets are made with the center, and not directly between agents. Betting between agents would lead to complications such as the no-trade theorem (Milgrom and Stokey, 1982): knowing that someone wants to bet that the value will be lower than $\bar{\omega}$ informs the buyer that someone received a 0 signal, and conversely. Ultimately, agents who report 1 get $(\bar{r} - \bar{\omega})\pi$ and those who report 0 get $(\bar{\omega} - \bar{r})\pi$. The agents subtract c_i from their earnings if they provided an effort.

2.3 Strategies and Equilibria

The agents' strategies in peer betting involve first deciding whether to exert an effort, and then what to report. We will consider mixed strategies only in reports, so agent i's strategy is given by (e_i, R_i, R_i^0, R_i^1) with R_i, R_i^0, and R_i^1 the probabilities of $r_i = 1$ if $e_i = 0$, if $e_i = 1$ and $s_i = 0$, and if $e_i = 1$ and $s_i = 1$ respectively. The strategy space is thus $\{0,1\} \times [0,1]^3$. The center is interested in situations in which agent i exerts an effort and reveals s_i, i.e., $e_i = 1$, $R_i^0 = 0$, and $R_i^1 = 1$. We need to make one final assumption, about what agents know about each others.

Assumption 4 (Common knowledge)**.** *The peer betting rules, the strategy space, the signal technology, the beliefs P_i, the costs c_i and agents' risk neutrality are common knowledge.*

Assumption 4 ensures that we have specified all the elements of a *Bayesian game*, as defined by Osborne and Rubinstein (1994, Definition 25.1). If beliefs and costs were not common knowledge, we would have to define higher-order beliefs, which would complicate the proofs. As we will see below the crucial part is not so much that agents know the exact beliefs of everyone, but rather that all agents know that Lemma 1 holds. Again for convenience, we let $N \to \infty$. It allows us to relate signal probability to signal proportion. It also allows us to neglect the impact of a single agent on the final bet value.

Proposition 1. *Under Assumptions 1 to 4 and with N infinite, if $c_i > \pi$ for all $i \in \{1, \ldots, N\}$, then Nash equilibria are characterized by $e_i = 0$ and $R_i \in \{0, \bar{\omega}, 1\}$. Expected payoffs are 0.*

Proposition 1 establishes that when the cost of acquiring a signal is too high or the reward is too low ($c_i > \pi$), agents will refrain from exerting effort. Multiple equilibria arise under this condition. In two of them, all agents coordinate on reporting either 0 or 1. In the third equilibrium, agents report 1 with probability equal to the prior probability $\bar{\omega}$. Study 1 will examine agents' behavior when they choose not to acquire a signal.

Proposition 2. *Under Assumptions 1 to 4 and with N infinite, if $\frac{c_i}{\pi} < \bar{\omega} \times (\bar{\omega}_i^1 - \bar{\omega}) + (1 - \bar{\omega})\,(\bar{\omega} - \bar{\omega}_i^0)$ for all $i \in \{1, \ldots, N\}$, acquiring and revealing signals ($e_i = 1$, $R_i^0 = 0$, and $R_i^1 = 1$) is a Nash equilibrium, and it strictly dominates the no-effort equilibria.*

Proposition 2 is the key result. When the reward is sufficiently high for all agents, acquiring and truthfully reporting signals becomes an equilibrium. This equilibrium is achieved when the reward structure ensures that the expected gain from obtaining and revealing a signal outweighs the cost of effort for every agent. The next propositions explore cases where some agents exert effort while others do not.

Proposition 3. *Under Assumptions 1 to 4 and with N infinite, if for $T \times 100\%$ of the agents $\frac{c_i}{\pi} > \bar{\omega} \times (T\bar{\omega} + (1 - T)\bar{\omega}_i^1 - \bar{\omega}) + (1 - \bar{\omega})\,(\bar{\omega} - T\bar{\omega} - (1 - T)\bar{\omega}_i^0)$ and the inequality is reversed for the remaining agents, then there is a Nash equilibrium in which these $T \times 100\%$ will exert no effort and report 1 with probability $\bar{\omega}$ and where the other agents acquire and reveal their signals.*

In the equilibrium described by Proposition 3, the fraction T of agents who choose not to exert effort create negative externalities for the others. Their inaction reduces the degree to which the final reported value can deviate from the prior expectation, thereby diminishing the overall incentive to acquire and reveal signals.

2.4 Psychological costs

So far, we have only considered effort costs. In this subsection, two additional costs are considered:

- *Asymmetric reporting cost:* Sometimes, one answer may be slightly stigmatizing, regardless of the truth, for instance admitting non-compliance with guidelines. We model this as a cost $a_i \geq 0$ borne by agent i when reporting $r_i = 1$ per se, no matter whether the agent receives a signal and what this signal may be. We choose 1 arbitrarily, and without loss of generality. This cost can reflect a stigma associated with answer 1. As we will see in the theoretical results and later in the experimental applications, a_i should not be too high, thereby excluding major incentives to lie. Cost a_i can arise from social desirability bias (Tourangeau and Yan, 2007), including descriptive (what behaviours are common) and injunctive norms (what behaviours are acceptable).

- *Lying cost:* The cost $d_i \geq 0$ of reporting $r_i = 0$ after receiving signal $s_i = 1$ or reporting $r_i = 1$ after receiving signal $s_i = 0$. This cost captures people's preference to tell the truth, as shown by Abeler, Nosenzo, and Raymond (2019) and also known in psychology as the Truth-Default Theory (Levine, Kim, and Hamel, 2010; Levine, 2014). People are averse towards lying about private information (Lundquist, Ellingsen, Gribbe, and Johannesson, 2009). Moreover, lying tends to be more cognitively demanding, leading to increased reaction times (Suchotzki, Verschuere, Van Bockstaele, Ben-Shakhar, and Crombez, 2017) and negatively affecting people's self-concept (Mazar, Amir, and Ariely, 2008). We assume that such costs can only occur when a signal has been received because cost for reporting an answer in spite of having no signal would be equivalent to decreasing the effort costs.

Assumption 5. *Agents bear asymmetric reporting costs $a_i \geq 0$ and lying costs $d_i \geq 0$ and these costs are common knowledge.*

Proposition 4. *Under Assumptions 1 to 5 and with N infinite, if for all $i \in \{1, \dots, N\}$ $\frac{c_i}{\pi} < \bar{\omega} \times \left(\bar{\omega}_i^1 - \bar{\omega} - \frac{a_i}{\pi}\right) + (1 - \bar{\omega})\left(\bar{\omega} - \bar{\omega}_i^0\right)$ and $\frac{a_i}{\pi} < \frac{d_i}{\pi} + 2\left(\bar{\omega}_i^1 - \bar{\omega}\right)$, signal acquisition and revelation ($e_i = 1$, $R_i^0 = 0$, and $R_i^1 = 1$) is a Nash equilibrium, and it strictly dominates the no-effort equilibrium.*

Proposition 4 establishes two sufficient conditions for the existence of an equilibrium in which agents acquire and reveal signals. The first condition, similar to Proposition 2, ensures that the expected payoff from exerting effort exceeds that of abstaining. The second condition guarantees that the cost of reporting a stigmatizing answer does not outweigh the benefit of truthfully revealing one's signal. This benefit is twofold: the agent avoids lying, thereby incurring no lying cost d_i, and prefers to buy the bet rather than sell it.

These conditions lead to three observations. First, the cost of reporting a stigmatizing answer is moderated by the cost of lying. Second, when the inequality $\frac{a_i}{\pi} > \frac{d_i}{\pi} + 2\left(\bar{\omega}_i^1 - \bar{\omega}\right)$, holds, the agent anticipates never reporting 1, regardless of the acquired signal. As a result, they have no incentive to exert effort. In our model, conscious lying does not occur; instead, agents prefer to avoid acquiring a signal altogether and report the more socially acceptable answer. Third, increasing the reward π both encourages effort and reduces incentives to lie, reinforcing truthful information revelation.

3 Experimental Evidence

Section 2 established the existence of an equilibrium where agents in peer betting seek costly information and make informed bets. Incentives in betting are based on peer behavior, as the final value of the bet is determined by other agents' reports. Are such peer betting incentives effective in eliciting effort in practice? This section presents evidence from two experimental studies. Section 3.1 provides a brief overview of the two studies and the findings. Sections 3.2 and 3.3 provide detailed information on the two studies and present the results in full detail.

3.1 Overview

We run two experimental studies to test if peer betting elicits effort in judgment formation. Study 1 aims to test peer betting in a controlled setting. We recruit participants for an online experiment where they are presented with pairs of virtual boxes, containing yellow and blue balls of unknown proportions. In each pair, one of the boxes is the "actual box" with equal probability. Participants are asked to pick a box within each pair. Before making a pick, participants could independently draw a single ball from the actual box by completing a real effort task, which involves counting the number of zeroes in a binary matrix. In this design the actual box is known to the experimenter, implying that the information is verifiable. Testing peer betting in a verifiable task allows us to implement rewards for accuracy of the reported information as a benchmark. Study 1 runs three treatments in which participants complete the same tasks. The baseline treatment offers a fixed reward (a flat participation fee), while the other two treatments implement peer betting incentives and incentives for accuracy. Results suggest that peer betting elicits significantly more effort than fixed rewards, while the effort is highest under incentives for accuracy. The results of Study 1 suggest that peer betting is an effective alternative to stimulate effort when rewarding accuracy is not feasible.

Study 2 explores the feasibility of peer betting in a practical problem of elicitation of unverifiable information. In response to the Covid-19 pandemic in 2020, governments around the world issued guidelines for social distancing and other safe practices. Policy makers would like to know if such guidance is followed by the public. When asked to self-report if they were following a safe practice, people may not recall instances where they failed to do so. Futhermore, as discussed in Section 2.4 people may be reluctant to admit unsafe practices due to the social stigma associated with such anti-social behavior. Hence, even though the ground truth is unverifiable, one would expect that peer betting will increase the self-reported rate of non-compliance. We implement peer betting in an online survey aimed at the residents of the UK. Participants are asked 8 questions, each involving an unsafe practice according to the Covid-19 guidance issued by the UK government in October-November 2020. Study 2 allows us to test peer betting in a setup where psychological costs are relevant. We find that with peer betting incentives, participants are more likely to admit not following the safety guidance.

3.2 Study 1 - Peer betting in a simple prediction task

3.2.1 Design and procedures

Tasks. Participants complete 10 *prediction tasks*. Each prediction task displays a pair of boxes as shown in Figure 1 below. There are 10 such pairs and each pair appears in a single prediction task only. One of the boxes in each pair is set as the actual box via a virtual coin flip prior to the experiment. Participants are informed that one of the boxes is the actual box, but they do not know which. In each task, participants are asked to pick one of the boxes, which may affect their rewards depending on the experimental treatment.

In Figure 1, there are 120 yellow and 80 blue balls in total. Box Q contains more than 60 yellow balls while Box I contains more than 40 blue balls. The exact number of balls of each color are determined randomly according to the specifications. Hence, the number of yellow balls in Box Q is within $(60, 100]$. For example, if Box Q contains 80 yellow and 20 blue balls, Box I contains 40 yellow and 60 blue balls. In the experiment, pairs of boxes are presented as shown in Figure 1. Thus, participants do not know the exact number of yellow and blue balls in a box. The boxes are constructed such that the left box (Box Q in Figure 1) always contains more than half of the total number of yellow balls. Table B1 in Online Appendix B provides the

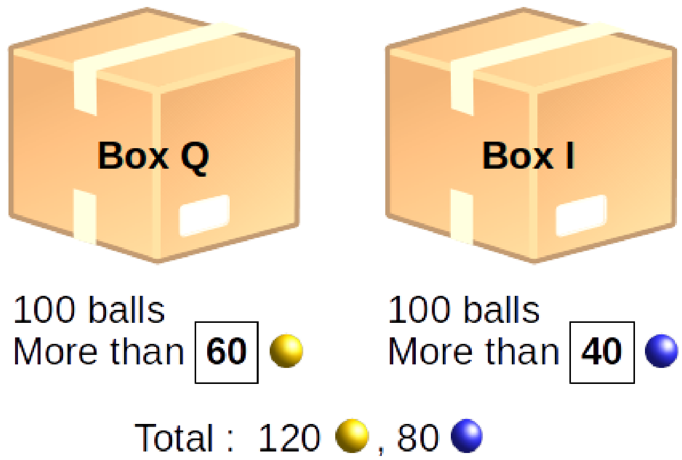


Figure 1: An example pair of boxes

composition of all 10 pairs.

Before picking a box, each participant is offered a choice to observe a single draw from the actual box with replacement. Participants have to complete a *real effort task* to observe their draw. The effort task is counting the number of 0s in a matrix (Abeler, Falk, Goette, and Huffman, 2011). Figure 2 shows one such matrix. There is a unique matrix for each effort task and there is a single effort task associated with each prediction task. The number of 0s in each matrix varies between 8 and 16. Figure B1 in Appendix B shows the matrices in all effort tasks.

0	0	1	1	0	1
1	0	0	1	0	0
0	0	1	1	1	1
0	0	1	1	0	1

Figure 2: An example binary matrix

The sequence of events in each prediction task is as follows: First, participants are shown a pair of boxes and asked if they want to complete the effort task. Participants skipping the effort task are immediately asked to pick a box. Otherwise, they are presented the associated binary matrix and asked to report the number of 0s. They are required to report an accurate count to proceed and are allowed an unlimited number of retries to do so. Upon reporting the accurate count, the participants observes a personal random draw, which is either a blue or a yellow ball, and proceed to picking a box.

Design & Rewards. We set up three experimental treatments which differ only in reward structure. In the Flat treatment, participants receive a fixed reward of £3.25 for completing the experiment. In the Accuracy treatment, participants receive a basis reward of £3.25. In addition, they earn £0.20 per accurate pick and lose £0.20 per inaccurate pick, where the accurate pick in a pair is picking the actual box. Thus, a participant's total reward is within [£1.25, £5.25]. Finally, the Peer Betting treatment implements our new incentive mechanism. Similar to the Accuracy treatment, basis reward is £3.25. In addition, participants may earn a bonus from each pick, which is determined by their peers' picks in the same pair and composition of the boxes. To illustrate, consider a participant who is asked to pick a box in the pair shown in Figure 1. Suppose, among all other participants, 82% picked Box Q and 18% picked Box I. Then, the participant earns $82 - 60 = 22$p when picking Box Q, loses $40 - 18 = 22$p if Box I. The final value of the bet for a given box is simply the percentage of people who pick that box. The number within the square below each box corresponds to the bet price. We set $\pi = 1$, so the bonus per task is simply the difference between the final value of the bet and its price. A negative total reward in the Peer Betting treatment is possible but extremely unlikely. Table C1 in Appendix C shows that the minimum realized reward was £2.05.

Participants in Flat have no direct financial incentives to complete the effort tasks as their reward does not depend on prediction accuracy. In contrast, bonus in Accuracy depends on prediction accuracy, which could be improved by observing the draw. Thus, we expect participants in the Accuracy treatment to complete effort tasks more frequently to maximize their accuracy. Peer betting also provides incentives to complete effort tasks if, as predicted by the theory, participants consider their signal informative on others' picks. Consider a truthful equilibrium outcome for the example in Figure 1. If the actual box is Q, then more than 60% of others are expected to draw a yellow ball and pick Q. The percentage of blue draws (and I picks) will be less than 40%. In that case, picking Box Q gives a positive expected payoff while picking Box I leads to a loss. The opposite is true when Box I is the actual box. Participants have an incentive to complete the effort task because their draw provides information on the actual box, which in turn suggests which box is more likely to be picked more often than the prior (60 and 40 for Boxes Q and I in Figure 1).

Note that the exact expected payoff of a participant depends on her beliefs on the composition of the boxes, which are not restricted by the experiment to allow the heterogeneity of posterior expectations in the theory. Suppose a participant has a uniform belief over all possible compositions of Boxes Q and I given that Box Q contains more than 60 yellow and Box I contains more than 40 blue. In that case, the participant expects 80 yellow in Box Q and 60 blue in Box I, implying that 80% (60%) are expected to pick Box Q (I) if the actual box is Box Q (I). Since the priors 60 and 40 respectively, the participants expect 20p from picking the actual box and -20p from a wrong pick. In the absence of a draw, Q and I are equally likely to be the actual box and the expected payoff is zero. If a participant completes the effort task and draws yellow, the expected payoff from picking Box Q is Pr(actual box is Q | yellow) 20 + Pr(actual box is I | yellow)(-20). Observe that, in this example, the expected payoff conditional on the draw is identical in Accuracy and Peer Betting because win/loss per task in Accuracy is also 20p. This need not hold for all participants and tasks. The expected payoffs in Peer Betting depend on the participants' beliefs on the composition of the boxes. So, the expected bonus from an accurate pick may differ from 20p. Table B2 in Appendix B shows the range of anticipated bonuses from an accurate pick in each prediction task. Consider uniform beliefs over the possible yellow/blue ratios, given participants' information on the pairs. Then, the expected bonus from a truthful pick ranges between 15p and 25p across the tasks, with an average of 20p. In order to make Peer Betting and Accuracy payoff-equivalent, we set the bonus per pick in Accuracy at 20p. Appendix B provides further information on how expected bonuses were kept comparable between the Accuracy and Peer Betting treatments.

Link with the theory. The prediction task is a representation of the binary question Q, where the two boxes in any pair correspond to the possible answers. Picking the left (right) box represents reporting $r_i = 1$ ($r_i = 0$). The effort task corresponds to the costly signal c_i in the theoretical framework. Participants are allowed to skip the effort task, in which case they make a pick without observing a draw. Let $s_i = 1$ represent drawing a yellow ball. In any given pair, the total number of yellow (and blue) balls are known and boxes are a priori equally likely to be the actual box, which induces a common prior expectation on the number of yellow and blue balls in the actual box. For example, the common prior expectation of getting a yellow ball (i.e. getting signal 1) in Figure 1 is 0.6. Let $r_i = 1$ ($r_i = 0$) correspond to picking the left (right) box. Participants who draw a yellow (blue) ball increase their probability of the left (right) box being the actual box. Hence, signals unequivocally influence belief and revealing signals coincides with $r_i = s_i$. To illustrate the incentives, consider again the example in Figure 1 and suppose $r_j = s_j$ for all $j \neq i$. Following $s_i = 1$, participant i puts a higher probability on more than 60% of others drawing yellow and picking the left box. Then, $r_i = 1$ at price 0.6 leads to a positive expected payoff. Similarly, for $s_i = 0$, $r_i = 0$ gives a positive expected payoff.

Participants. We recruited 210 participants from Prolific, an online platform for conducting surveys. We restricted our participant pool to U.S. citizens who are students at the time of the experiment. Average duration of the experiment is around 9 minutes. Table C1 in Appendix C includes further information on the participants and provides summary statistics. Figure C1 shows the distribution of completion times.

Procedure. The experiment was published on Prolific in May 2020 and implemented via Qualtrics. Participants are randomly selected into one of the experimental treatments. They are first presented with instructions, which differ across the treatments in rewards only. Then, the participants respond to a quiz question about the rewards in their treatment. Depending on the answer, the experiment provides feedback with an example illustration of the rewards. The quiz marks the end of instructions and the beginning of the main body of the experiment. Participants complete the 10 prediction tasks. The order of the prediction tasks is randomized. Finally, participants complete a short survey on demographics. The survey also elicits participants' opinions on the clarity of the experimental instructions and their self-reported training in statistics. The latter could be relevant for participants' ability to process their signal properly. Figure C2 in Appendix C provides the frequency distribution of responses on the clarity of instructions. Figure C3 depicts the levels of training in statistics across the treatments. Participants also respond to a quiz question about incentives to verify their understanding. The replication material at the end of this document provides the full text of the instructions, quiz questions (before and after the main tasks), and the final survey.

3.2.2 Results

The primary question of interest is whether participants are more likely to seek costly information under peer betting incentives than fixed rewards. The effort task completion in Flat and Peer Betting allows us to test the effect of peer betting. Furthermore, in our prediction task, the ground truth (the actual box in any pair) is known to the experimenter. Accuracy implements rewards for ex-post accuracy, which are not feasible in practical problems of information elicitation without verification. We compare effort task completion in the Accuracy and Peer Betting treatments to test if peer betting can elicit as much effort as rewarding accuracy. Figure 3 depicts the percentage of instances per prediction task and treatment where participants completed the associated effort task.

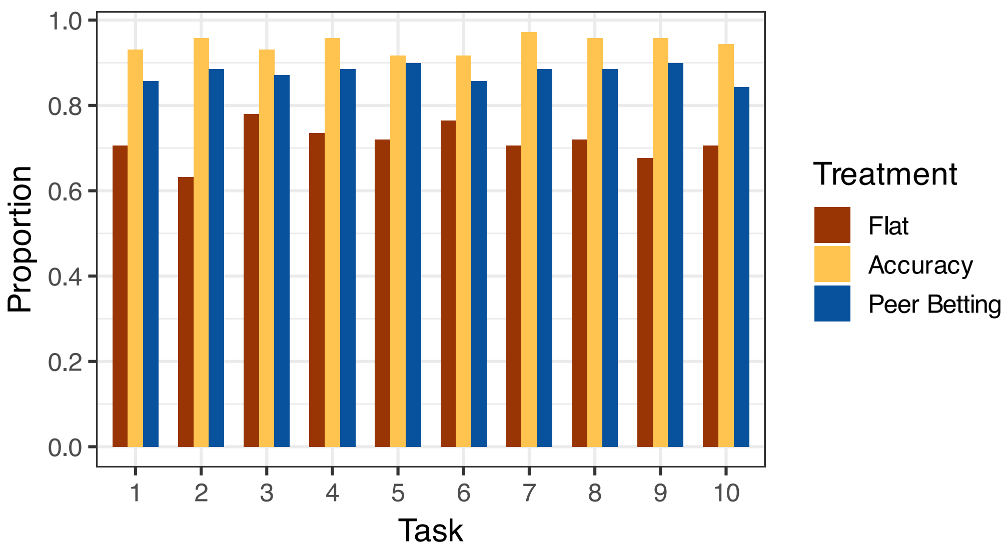


Figure 3: Proportion of times participants completed the effort task associated with the prediction task.

The effort level is substantial, even in the Flat treatment. Effort task completion is higher in Peer Betting and the highest in Accuracy. Figure 3 suggests that incentives provided by peer betting is effective in eliciting a higher proportion of informed judgments compared to a fixed reward. Incentives for accuracy are the most effective in eliciting effort. Figure 3 also indicates that the effort level in Peer Betting is similar across tasks. Section 3.2.1 discussed that the expected bonus from an accurate pick may differ according the composition of the boxes, which vary across tasks. Figure D1 in Appendix D shows that the effort rate does not differ significantly across the levels of expected bonuses provided in Table B2.

For a statistical analysis on effort task completion, we estimate logistic regressions where probability of effort task completion is the dependent variable. Table 1 below shows the marginal effects. The corresponding logistic regression estimates are included in Table D2. The pooled data includes 2100 decisions about whether to complete the effort task. We include binary indicators for the treatments as dependent variables. The coefficient of Peer Betting in Table 1 measures the average marginal effect of implementing peer betting incentives (instead of a flat fee) on the likelihood of effort task completion. The coefficient of Accuracy measures the same for rewarding participants for accuracy.

Specifications (2),(3),(5) and (6) include various controls. The variables "US citizen" and "Female" are binary indicators for US residents and gender respectively while "Age" is a numeric variable. As discussed in the experimental design, prior expectation on yellow varies across the prediction tasks, which affects the information value of a draw. The variable "|Prior-50|" measures the distance between the prior expectation and 50, and allows us to check if having a more extreme prior has an impact on effort task completion. Since the experiment consists of 10 predictions tasks, participants might be less likely to complete the effort tasks in later tasks, which we can study because the order of tasks is randomized. "Task order" is a numerical variable (1 to 10) that represents the rank of the effort task for the participant. We divide numeric variables by 10 to obtain more informative point estimates at two decimal values. Thus, coefficient estimates of Age, |Prior-50| and Task order measure the effect of being 10 years older, increasing the prior on yellow by 10 and completing the task last (in 10th place) instead of first respectively. Table 1 evaluates the marginal effects for |Prior-50| and Task order in each treatment level to investigate if these effects differ across treatments. For all other variables, reported estimates are average marginal effects. In all models, standard errors are clustered at participant level. Models (1) to (3) show the marginal effects using the whole sample of participants, while (4),(5) and (6) presents the marginal effects when participants who gave an incorrect answer in the post-experimental quiz are excluded to construct a filtered sample. Standard error and 95% confidence interval are included underneath each estimated effect.

	Dep. var.: P(effort task completed)					
		(whole sample)			*(filtered sample)*	
	(1)	(2)	(3)	(4)	(5)	(6)
Accuracy	0.23	0.23	0.23	0.23	0.23	0.23
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
	[0.13; 0.33]	[0.14; 0.32]	[0.14; 0.32]	[0.13; 0.33]	[0.14; 0.32]	[0.14; 0.32]
Peer Betting	0.16	0.14	0.14	0.16	0.14	0.14
	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
	[0.05; 0.27]	[0.04; 0.25]	[0.04; 0.25]	[0.05; 0.26]	[0.03; 0.25]	[0.03; 0.25]
Age		−0.04	−0.04		−0.04	−0.04
		(0.03)	(0.03)		(0.03)	(0.03)
		[−0.10; 0.02]	[−0.10; 0.02]		[−0.10; 0.01]	[−0.10; 0.01]
Female?		0.04	0.04		0.04	0.04
		(0.04)	(0.04)		(0.04)	(0.04)
		[−0.03; 0.11]	[−0.03; 0.11]		[−0.04; 0.11]	[−0.04; 0.11]
US resident?		−0.03	−0.03		−0.02	−0.02
		(0.07)	(0.07)		(0.07)	(0.07)
		[−0.17; 0.12]	[−0.17; 0.12]		[−0.17; 0.12]	[−0.17; 0.12]
|Prior-50| (Flat)			−0.03			−0.03
			(0.02)			(0.02)
			[−0.06; 0.00]			[−0.06; 0.00]
|Prior-50| (Accuracy)			0.01			0.01
			(0.01)			(0.01)
			[−0.02; 0.03]			[−0.02; 0.03]
|Prior-50| (Peer Betting)			−0.01			−0.01
			(0.01)			(0.02)
			[−0.04; 0.02]			[−0.04; 0.02]
Task order (Flat)			0.05			0.05
			(0.03)			(0.03)
			[−0.01; 0.11]			[−0.01; 0.11]
Task order (Accuracy)			0.01			0.01
			(0.02)			(0.02)
			[−0.03; 0.04]			[−0.03; 0.04]
Task order (Peer Betting)			0.04			0.04
			(0.03)			(0.03)
			[−0.03; 0.10]			[−0.03; 0.10]
Num. obs.	2100	2070	2070	2060	2030	2030
Likl. Ratio.	148.93	175.79	179.37	146.39	173.35	176.94
LR test p-val	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001
AIC	1649.70	1549.38	1557.80	1638.88	1539.16	1547.57

Table 1: Marginal effects, logistic regression (baseline category: Flat). Standard error (in brackets) and 95% confidence interval (in square brackets) are included underneath the estimated effects.

In all specifications, the marginal effects for the Peer Betting and Accuracy treatments are significantly positive. Participants are 14 to 16 percentage points (ppt) more likely to complete the effort task under peer betting incentives compared to a fixed payment. Table 1 also suggests that incentives for accuracy is 23 ppt more likely to elicit effort than a flat fee. Prior expectation and the order in which a participant completes prediction tasks have no significant effect on effort task completion. Table D3 estimates the same logistic regression with Peer Betting as the baseline category, and Table D4 provides the corresponding marginal effects. As suggested by Figure 3, participants are more likely to complete effort tasks when they are incentivized for the accuracy of their picks. We can infer that incentives for accuracy is the most effective in effort elicitation, followed by peer betting and flat payments. In the absence of verifiability, peer betting provides an alternative for incentivizing effort.

We now investigate if participants revealed their signals, which means picking the left (right) box when a yellow (blue) ball is drawn. Given the simplicity of the predictions task, participants do not have any external motives to misreport their signals. However, deviations from signal revelation may occur due to confusion or errors, or due to beliefs that others will deviate. Figure 4 shows participants' picks given their draw. The 3x3 grid depicts the three treatments as well as the three possible situations after the effort task. Participants receive a yellow or blue draw if they complete the effort task. Alternatively, they do not receive a draw if they skip the effort task. The bars show the number of left and right box picks in the subsequent prediction task. Since picking the left (right) box when the draw is yellow (blue) is the signal-revelation strategy, the number of left (right) picks are represented by yellow (blue) colored bars. The black dots show participants' prior expectation on the number of yellow balls in the actual box, given that left and right boxes are equally likely to be the actual box. Table B2 in Appendix B provides the prior expectations on the number of yellow balls in each task. Figure 4 strongly suggests that the picks typically reveal true signals. Participants who observe a yellow (blue) draw typically pick the left (right) box. The distribution of picks in Peer Betting and Accuracy are very similar, so we can argue that peer betting reveals true signals as well as incentives for accuracy do. The same is true for the Flat treatment. Conditional on drawing a costly signal, picks often reveal true signals under fixed payment as well.

The rightmost panel in Figure 4 illustrates the strategy participants use if they do not draw a ball. Interestingly, participants in Peer Betting (and in Flat) appear to follow a mixed strategy (at the aggregate level), picking left with a probability

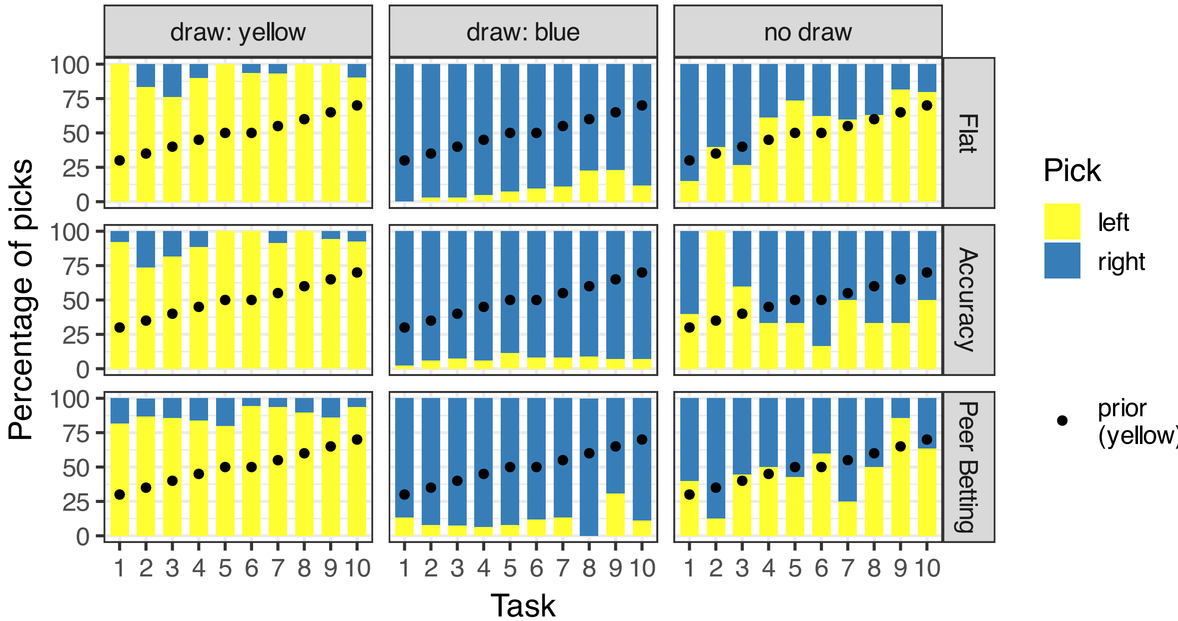


Figure 4: Participants' picks

equal to the prior, as described in the equilibrium of Proposition 3. The proportion of left picks and the prior expectation on yellow are not significantly different for Peer Betting participants who do not draw a ball (t-test $t = -0.34$ $p = 0.739$). As indicated in Figure 4, participants who draw a yellow ball in Peer Betting pick left box at a significantly higher rate than the prior ($t = 8.56, p < 0.0001$). The opposite is true for participants who draw a blue ball. Table D1 in Appendix D provides further comparisons of the prior and left picks for each treatment and draw.

3.3 Study 2 - Eliciting Covid-19 experiences using peer betting

Study 2 implements peer betting in measuring if residents of the UK followed safety guidance during the Covid-19 pandemic. For most of the safe practices in the guidance, it is not feasible to monitor all individual behavior. Self-reported behavior is practically unverifiable and therefore, unlike in Study 1, rewards based on accuracy are not possible. In an unincentivized or a flat-fee survey, participants may not make the mental effort to recall (signal acquisition) and report their behavior truthfully (signal revelation). Furthermore, reporting costs can be asymmetric. Unsafe behavior is typically stigmatized and likely to be under-reported (Tourangeau and Yan, 2007).

We investigate if peer betting motivates participants to spend more time in answering questions and report their unsafe practices at a higher rate.

3.3.1 Design and procedures

Tasks. Participants are presented a survey consisting of 8 statements. Each statement describes a situation that was considered unsafe and inadvisable (if not prohibited) by the UK Covid-19 guidance at the time of this survey. All situations involve others' actions, thereby mitigating one's own responsibility and lowering the stigma (in the terms of our model, to keep cost a_i reasonably low). For each statement, participants pick True or False to self-report if they have been in the described situation. Table 2 provides the list of statements.

1.	I have been in an elevator with another person in it at least once in the last 7 days
2.	I may have stood less than 2 metres away from the person in front in a queue at least once in the last 7 days
3.	I was seated less than 2 metres away from someone who is not part of my household in a restaurant/cafe/bar at least once in the last 7 days
4.	I have been in a social gathering with more than 6 people who are not part of my household at least once in the last 7 days
5.	I have been in a busy shop/market with no restrictions on number of customers at least once in the last 7 days
6.	I participated in an indoor activity with more than 6 people who are not part of my household at least once in the last 7 days
7.	I have been in a shop/market where one or more of the staff did not wear a mask at least once in the last 7 days
8.	I had an interaction with someone experiencing high body temperature, persistent cough or loss of taste/smell at least once in the last 7 days

Table 2: Covid-19 survey statements

We ran this survey for two weeks with a new sample of participants every week. The two iterations of the survey are referred to as week 1 and week 2 surveys respectively. As we will introduce below, week 1 and week 2 surveys include treatments that implement peer betting. We also run a week 0 survey to elicit information necessary to initialize peer betting. The week 0 survey uses the same questions, but they are presented in a slightly different way to elicit more information on the number of instances participants engaged in the described behavior. [5] Based on the results of the

[5]For example, question 1 in Table 2 is presented as "In the last 7 days, I have been in an elevator

week 0 survey, we decided to implement two versions of each survey in weeks 1 and 2. Both versions ask the questions in Table 2, but in the second version "at least once" is replaced with "at least twice" in each question. We provide more information on how week 0 survey is used in the design below.

Design. In week 0 survey, participants receive a flat fee only. In week 1 and 2 surveys, we manipulate incentives to create control and peer betting surveys. As ground truth (guideline compliance) is not observable, an accuracy treatment as in Study 1 is unfeasible. In the controls, participants are rewarded with a flat fee for completing the survey, while the Peer Betting treatment implements the peer betting incentives. Figure 5 shows the experiment interface in Peer Betting.

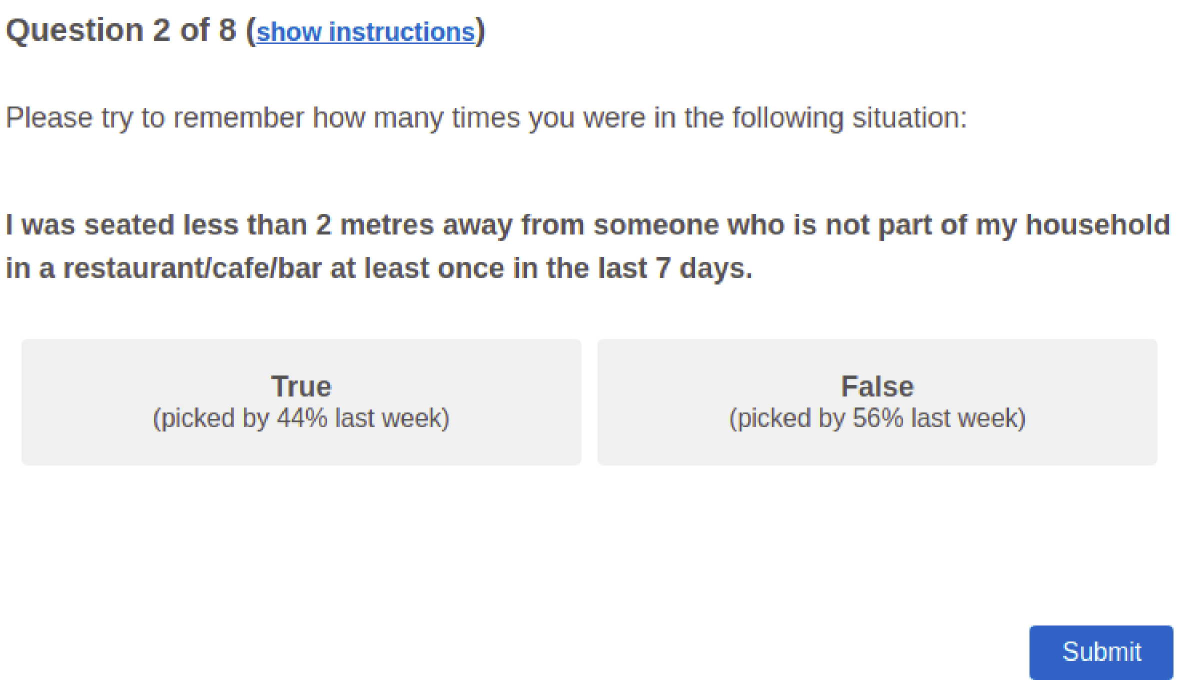


Figure 5: A screenshot from the Peer Betting treatment

The interface displays the statement and requires participants to pick True or False. The text below each alternative indicates the percentage of participants who endorsed that alternative in the previous week's survey. Recall that in our Bayesian setup, agents have a common prior expectation $\bar{\omega}$ on the distribution of responses. To implement Assumption 2 in practice, we provide the participants with the latest realization of ω. Participants' bonus depends on the previous and current endorse-

with another person in it ..." and the participant picks one of the following answers: "once or more", "twice or more", "3 times or more", "4 times or more", "5 times or more".

ment rates. In Figure 5, the endorsement rate of True in the last iteration is 44%. A participant who picks True in this iteration wins a positive (negative) bonus from this question if the realized endorsement rate in this iteration exceeds (falls below) 44%. The same holds for False, except that the threshold is 56%. Thus, the Peer Betting treatment implements the mechanism in weeks 1 and 2 such that last week's realization of % True(False) determines the price for the current bet on True(False). We provide more information on the rewards below. Peer betting is expected to incentivize mental effort and/or overcome the psychological costs of reporting one's actual behavior. If peer betting incentivizes signal revelation under the psychological costs of reporting True, we may expect endorsement rates for True to be higher in the Peer Betting treatment. Furthermore, if peer betting incentivizes signal acquisition, we may expect decision times–a proxy for mental effort–to be longer.

The control surveys are similar to the Peer Betting treatment except that participants are rewarded with a flat fee. We implement two different types of control surveys: Flat and Flat-PastRate. In the Flat treatment, the survey interface does not present any information on previous iterations' endorsement rates. The Flat treatment mimics how such questions would be implemented in a regular survey. The Flat-PastRate treatment shows the same screen as the Peer Betting treatment by displaying previous week's endorsement rates, as in Figure 5. The rewards are fixed in both Flat and Flat-PastRate, thus the previous endorsement rates are irrelevant. Nevertheless, we included the Flat-PastRate treatment to check if merely presenting that information affects participants decision time and reports. First, processing additional information (previous endorsement rates) could, per se, increase decision times even if there is no additional effort to acquire signals. Second, it could influence endorsement rates by social proof (Cialdini, 2008) or conformity desire (Morgan and Laland, 2012).

Week 0 survey is used to determine the previous endorsement rates presented in the Flat-PastRate and Peer Betting treatments of week 1. In week 2, we use the realized endorsement rates of the Peer Betting treatment in week 1 as last-week data in both Flat-PastRate and Peer Betting. Recall that the theory predicts signal revelation under peer betting incentives, which leads to a more accurate measurement of actual percentage of true-types in week 1. The week 0 survey also motivates our choice to run two versions where the statements include "at least once" and "at least twice" respectively. [6] In each week $i \in \{1, 2\}$, we implement 6 surveys in a 3 (Flat,

[6]Table C2 in Appendix C provides the percentage of participants who pick True in each question

Flat-PastRate, Peer Betting) × 2 (at least once, at least twice) design. Table C4 in Appendix C provides the priors (previous endorsement rates) for both "at least once" and "at least twice" surveys in weeks 1 and 2.

Rewards. Flat and Flat-PastRate pay a fixed reward of £1.75. In the Peer Betting treatment, participants earn £0.75 for participation. In addition, they start with an endowment of £1, which represents the initial level of bonus. In each question, the bonus changes according to the difference between the endorsement rate in the current survey versus the previous iteration. To illustrate, suppose a participant picked True in a question in week 2 survey and endorsement rate of True was 50% in week 1. If the realized endorsement rate of True in week 2 at the same question is 70%, the participant wins $70-50=20$p. In contrast, if the endorsement rate in week 2 is 30%, the participant loses $50-30=20$p. The previous week's endorsement rate serves as the price of the bet in peer betting while the current week's endorsement rate, unknown to the participant at the decision time, is analogous to realized value of the bet. Similar to Study 1, we set $\pi = 1$ and the bonus is simply the difference between value and price. For each participant in Peer Betting, we sum the gains and losses over all questions to determine the net bonus. As in Study 1, the total reward can theoretically be negative in the Peer Betting treatment. However, this is extremely unlikely and Table C3 in Appendix C shows that the minimum reward was £1.18.

Link with the theory. In Study 2, the binary question Q corresponds to endorsing, or not, a health related statement. Let $r_i = 1$ represent endorsing True for a given statement. Remembering whether the situation described in the statement occurred corresponds to signal acquisition cost c_i in the theoretical framework. This cost may be purely cognitive (recollection effort) but also due to the discomfort to think about it (no matter what the signal is). Clicking on an answer without thinking allows respondents to avoid the discomfort. The stigma to answer True corresponds to a_i and giving an answer whilst remembering the opposite corresponds to d_i. The previous-week endorsement rate of True mentioned beneath the choice corresponds to $\bar{\omega}$, while the final value $\bar{r}$ is the resulting endorsement rate in the current survey. Signal s_i represents participant i's correct answer in a given statement, where $s_i = 1$

in the week 0 survey. For "3 times or more" and higher thresholds, the percentage of True picks are close to 0. Then, participants in week 1 iteration of an "at least 3 times" version may report True simply because the threshold is very low and a few True picks could easily bring the week 1 endorsement rates above the threshold. To avoid such cases, we only run two versions with "at least once" and "at least twice" respectively. The week 0 survey included a ninth statement: "I had physical contact with someone who came from abroad in the last 10 days". Only 2% picked True for once or more and we decided to exclude it in weeks 1 and 2.

represents True and $r_i = s_i$ corresponds to revealing the signal. For $s_i = 1$, participant i's posterior prediction on the endorsement True(False) is higher(lower) than the previous-week endorsement rate, which provides incentives to report $r_i = s_i = 1$. A similar reasoning holds for $s_i = 0$.

Participants. As in Study 1, participants are recruited from Prolific. However, for Study 2, we restrict our participant pool to students who currently reside in the UK. We chose the UK because it had uniform national social-distancing guidelines and sufficient Prolific participants at the time of the study. We restricted the study to students because we needed a homogeneous group such that Assumption 1 (signal technology) may plausibly hold. In total, 692 participants completed our survey, 50 of which participated in week 0 survey while the remaining 642 participated in either week 1 or 2 (but not both). Participants in a given week $i \in \{1, 2\}$ are assigned randomly in one of the 6 treatments explained above. One participant is excluded for being in a non-student status at the time of data collection. All surveys are implemented via Qualtrics. Participants spent around 3 minutes to complete the experiment. Table C3 in Appendix C provides further information on the participants. Figure C4 provides the distribution of completion times.

Procedure. The experiment was conducted over three consecutive weeks (week 0: October 19; week 1: October 26; week 2: November 2, 2020). We initially planned to run Study 2 over four weeks, but we had to stop earlier when the pandemic amplified in the UK (second wave) and more strict measures are put in place, making our questions less applicable. The week 0 iteration was a single survey while in weeks 1 and 2, participants were randomly assigned to the different treatments. In each survey of each iteration, participants are first presented with instructions. Then they are asked to respond to the questions, which are presented in randomized order. Finally, participants complete a short survey on demographics and the clarity of the instructions. The replication material at the end of this document provides the full text of the instructions and the final survey. Figure C5 in Appendix C shows the distribution of self-reported clarity of instructions for week 1 and 2 surveys (pooled across "at least once" and "at least twice" versions).

3.3.2 Results

Figure 6 shows the percentage of True picks for each treatment and version in the week 1 and week 2 surveys. Responses are pooled across questions and participants. Twelve observations have response times longer than 60 seconds, which suggests outliers as showed by Figure D2 in Appendix D. Table D5 provides the outliers. The statistical analyses below using the "filtered sample" exclude the outlier responses.

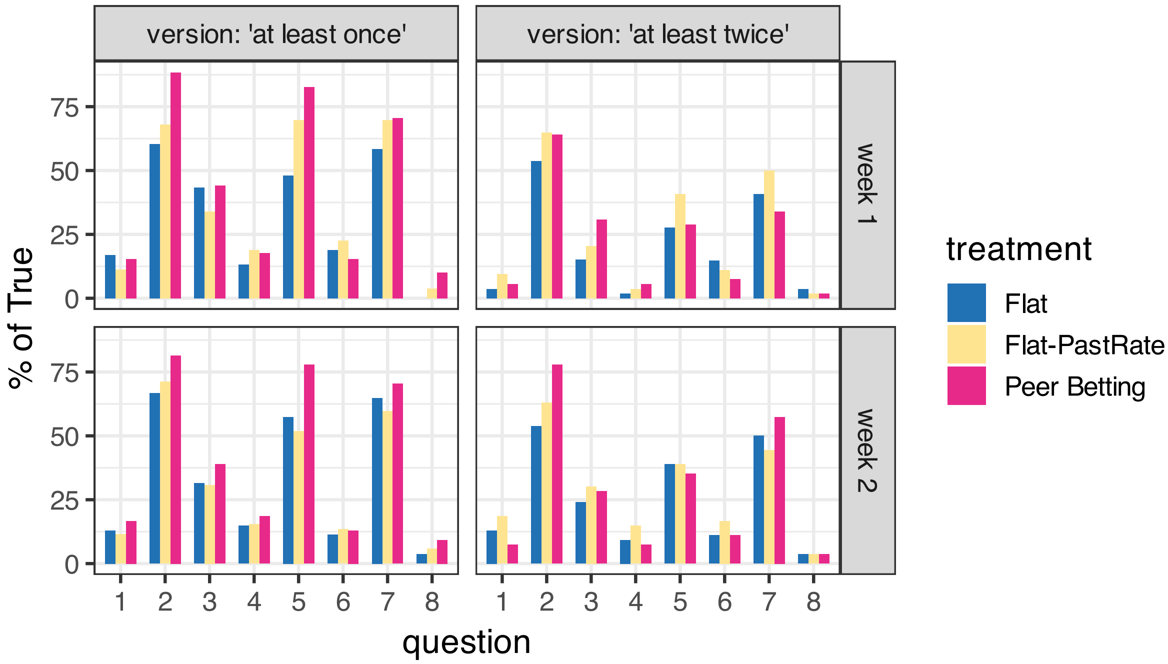


Figure 6: Percentage of True picks in week 1 and 2 surveys.

Peer betting elicits True at a higher rate in some of the questions, particularly in the "at least once" version. Recall that week 1 surveys are initialized with the unincentivized week 0 survey (of a slightly different format) while week 2 surveys use data from week 1 survey of the Peer Betting treatment. Since the prior has an effect on peer betting, we will analyze the response data from weeks 1 and 2 separately.

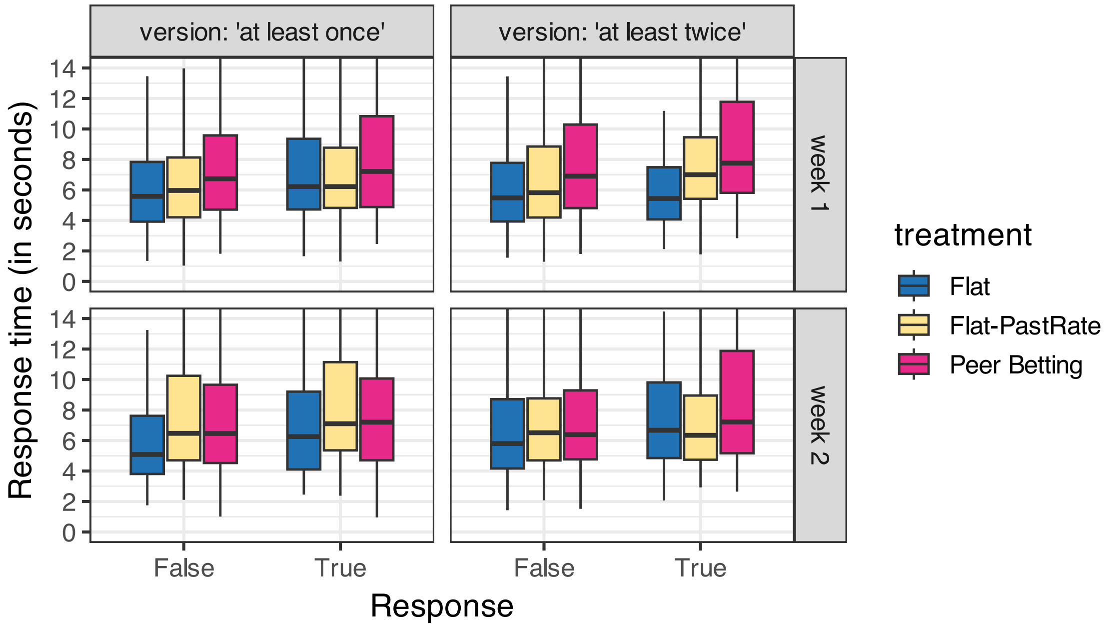


Figure 7: Response time of participants. The data points above 14 are included in calculations but not shown on the figure.

Figure 7 depicts the response times for each version and week, and by response type. Figure 7 suggests that the median response time in Peer Betting is higher than Flat in all iterations. The same is true for the Flat-PastRate treatment in week 1. However, response times in Flat-PastRate and Peer Betting are comparable in week 2 surveys. To test for significance, we estimate two classes of regression models. Firstly, we estimate a logistic regression for participants' likelihood of picking True in any given question. Secondly, we estimate a linear regression model where response time is the dependent variable. In both models, Flat is the baseline category and binary indicators for Flat-PastRate and Peer Betting are variables of interest. We also include various demographic controls representing the age, gender, and citizenship of the participants. We focus here on the "at least once" versions of all iterations as Figure 6 suggested a possible difference for these versions only. Section D.2.2 in Appendix D performs the same analysis for the "at least twice" surveys.

Table 3 presents the average marginal effects from the logistic regressions. "Flat-PastRate" and "Peer Betting" are binary indicators for the treatment. "Female?" and "UK citizen?" are also binary variables that represent gender and citizenship. Similar to the analysis on Study 1, numeric variables are divided by 10. Thus, coefficient estimates of "Age" and "Response Time" measure the effect of being 10 years older and increasing the response by 10 respectively. Models (1,2) and (4,5) show the results with outliers excluded, while (3) and (6) include all responses. Models (1) and (4) do not include control variables, while (2,3,5,6) include question fixed effects as well as demographic controls. Table D6 in Appendix D provides the corresponding parameter estimates. In all models, standard errors are clustered at the participant level.

	P(response = 'true'), marginal effects					
	(week 1)			*(week 2)*		
	(filtered sample)		*(all)*	*(filtered sample)*		*(all)*
	(1)	(2)	(3)	(4)	(5)	(6)
Flat-PastRate	0.05	0.04	0.04	−0.00	−0.00	−0.00
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
	[−0.02; 0.12]	[−0.03; 0.11]	[−0.03; 0.12]	[−0.07; 0.06]	[−0.07; 0.07]	[−0.07; 0.06]
Peer Betting	0.11	0.10	0.10	0.08	0.09	0.08
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
	[0.05; 0.17]	[0.04; 0.16]	[0.04; 0.16]	[0.01; 0.15]	[0.01; 0.16]	[0.01; 0.15]
Response Time		0.00	0.01		−0.01	0.00
		(0.02)	(0.02)		(0.02)	(0.03)
		[−0.03; 0.04]	[−0.03; 0.04]		[−0.05; 0.04]	[−0.05; 0.05]
Age		−0.04	−0.04		−0.02	−0.02
		(0.03)	(0.03)		(0.02)	(0.02)
		[−0.10; 0.01]	[−0.10; 0.01]		[−0.05; 0.01]	[−0.05; 0.01]
Female?		0.02	0.02		−0.02	−0.02
		(0.03)	(0.03)		(0.03)	(0.03)
		[−0.04; 0.08]	[−0.04; 0.08]		[−0.08; 0.04]	[−0.08; 0.04]
UK citizen?		−0.00	−0.00		0.03	0.03
		(0.03)	(0.03)		(0.04)	(0.04)
		[−0.06; 0.05]	[−0.06; 0.06]		[−0.04; 0.10]	[−0.04; 0.10]
Question FE		✓	✓		✓	✓
Num. obs.	1259	1259	1264	1279	1279	1280
Likl. Ratio.	10.44	428.84	428.83	8.03	408.73	406.81
LR test p-val	0.0054	< 0.0001	< 0.0001	0.0180	< 0.0001	< 0.0001
AIC	1662.27	1293.87	1300.62	1660.66	1309.96	1313.96

Table 3: Logistic regression, average marginal effects. Standard error (in brackets) and 95% confidence interval (in square brackets) are included underneath the estimated effects.

The average marginal effects in Table 3 show that the peer betting survey elicits a higher frequency of True picks. A participant in the Peer Betting treatment of week 1 survey is around 10 ppt more likely to report True for a given statement compared to a participant in the Flat treatment. In contrast, Flat-PastRate has no effect. A similar result holds for the week 2 survey where the marginal effect of the peer betting incentives is estimated to be 8-9 ppt. Results support the equilibrium characterized in Proposition 4. Peer betting motivates participants to reveal unsafe practices at a higher rate, which suggest that such practices are under-reported in basic surveys.

We consider two possible mechanisms through which peer betting could lead to a higher percentage of True responses. Peer betting incentives may dominate potential reporting costs associated with the stigmatized response (which is True in our surveys), and/or peer betting may encourage participants to exert more mental effort and recall their unsafe practice accurately. The next paragraph analyzes response time, as a proxy for mental effort.

Table 4 presents OLS estimates where the dependent variable is response time in seconds. Similar to Table 3, standard errors are clustered at the participant level. In addition, we include a binary "Response" indicator which is 1 if the response is True, and 0 otherwise. Response and its interactions with treatment variables aim to measure if response times differ across responses.

The response time regressions show mixed results. In models (1)-(3), participants in the Peer Betting treatment spend significantly more time in their responses than the Flat treatment. However, week 2 results suggest otherwise. Models (4)-(6) do not indicate a strong difference in response times between the Peer Betting and Flat treatments. The test of the two parameters (Peer Betting vs Flat-PastRate) in (2) results in a significant difference (mean difference $= 1.871$, $t = 2.363$, $p = 0.018$), while the same test in (5) suggests no difference (mean difference $= -0.5274$, $t = -0.7923$ $p = 0.4283$). Hence, we cannot rule out that higher response times relative to the Flat survey could partly be the result of the presentation of more information in both Flat-PastRate and Peer Betting treatments. In all specifications except (1), Response has no significant effect, which implies that response times do not differ across True and False responses.

	OLS, Dep.Var.: Response time					
	|	*(week 1)*	|	|	*(week 2)*	|
	(filtered sample)		*(all)*	*(filtered sample)*		*(all)*
	(1)	(2)	(3)	(4)	(5)	(6)
(Intercept)	6.38	5.24	5.58	6.82	6.33	6.43
	(0.27)	(1.15)	(1.30)	(0.46)	(0.97)	(0.99)
	[5.85; 6.91]	[2.97; 7.52]	[3.02; 8.14]	[5.92; 7.73]	[4.42; 8.25]	[4.47; 8.38]
Flat-PastRate	0.87	0.84	0.63	1.60	1.56	1.57
	(0.57)	(0.58)	(0.60)	(0.66)	(0.63)	(0.63)
	[−0.25; 1.99]	[−0.30; 1.99]	[−0.55; 1.81]	[0.29; 2.91]	[0.31; 2.81]	[0.32; 2.82]
Peer Betting	2.64	2.71	3.06	1.14	1.03	1.04
	(0.66)	(0.65)	(0.81)	(0.69)	(0.69)	(0.69)
	[1.33; 3.95]	[1.42; 4.00]	[1.45; 4.66]	[−0.22; 2.50]	[−0.33; 2.39]	[−0.32; 2.40]
Response	1.14	0.75	0.65	0.39	−0.26	0.26
	(0.52)	(0.56)	(0.65)	(0.53)	(0.62)	(0.88)
	[0.11; 2.17]	[−0.36; 1.85]	[−0.64; 1.93]	[−0.65; 1.43]	[−1.49; 0.97]	[−1.47; 2.00]
Flat-PastRate x Response	−0.84	−0.99	−0.83	0.19	0.24	−0.18
	(0.74)	(0.74)	(0.76)	(0.87)	(0.88)	(1.01)
	[−2.30; 0.62]	[−2.45; 0.47]	[−2.33; 0.67]	[−1.53; 1.92]	[−1.49; 1.97]	[−2.17; 1.82]
Peer Betting x Response	−0.91	−1.05	−0.76	−0.07	−0.06	−0.46
	(0.81)	(0.80)	(0.96)	(0.83)	(0.84)	(0.98)
	[−2.51; 0.69]	[−2.62; 0.52]	[−2.66; 1.14]	[−1.72; 1.57]	[−1.71; 1.59]	[−2.39; 1.46]
Age		−0.07	−0.18		0.02	−0.02
		(0.39)	(0.43)		(0.24)	(0.24)
		[−0.85; 0.71]	[−1.03; 0.67]		[−0.45; 0.48]	[−0.49; 0.46]
Female?		0.26	0.01		0.40	0.29
		(0.50)	(0.57)		(0.51)	(0.53)
		[−0.73; 1.26]	[−1.11; 1.14]		[−0.60; 1.41]	[−0.77; 1.34]
UK citizen?		−0.81	−0.76		−1.64	−1.61
		(0.52)	(0.54)		(0.64)	(0.65)
		[−1.83; 0.21]	[−1.82; 0.30]		[−2.89; −0.38]	[−2.89; −0.34]
Question FE		✓	✓		✓	✓
R^2	0.03	0.06	0.05	0.02	0.06	0.05
Adj. R^2	0.03	0.05	0.04	0.01	0.05	0.04
Num. obs.	1259	1259	1264	1279	1279	1280
RMSE	5.89	5.82	7.13	5.82	5.72	5.95

Table 4: Response time regressions. Standard error (in brackets) and 95% confidence interval (in square brackets) are included underneath the estimates.

To sum up, the peer betting incentives increased the probability to report deviations from Covid-19 guidelines. However, this effect does not necessarily arise from additional mental effort as approximated by response time. We should note that, unlike choice data analysis, response time regressions have low explanatory power as indicated by small R^2 values. Response time data could be too noisy to draw strong conclusions. We can exclude that the effect on self-reported True answers is a byproduct of mentioning the answer rates of the previous week, which may serve as an anchor and induce some social norms. Flat-PastRate treatment provided the same information as Peer Betting. The two treatments differ only in incentives. Hence, higher rate of self-reported unsafe practice in the Peer Betting treatment indicate that the peer betting incentives dominate potential reporting costs associated with the stigmatized response.

4 Discussion

4.1 Theoretical limitations

The signal technology assumption includes anonymity, i.e, that the probability to obtain signal 1 is the same for all agents. This assumption, even though common in the theoretical literature, limits possible applications. It can be easily implemented in artificial studies but for relevant topics, it requires implementing peer betting on homogeneous groups of respondents.

Peer betting, like similar mechanisms, assume risk neutrality. Risk aversion could decrease the perceived incentives provided by the mechanism. When participation is compulsory however, the no effort strategy is also risky. In the presence of high risk aversion, a degenerate equilibrium with no-one providing effort and everyone reporting the same answer would dominate equilibria with efforts. Loss aversion could also distort the results as some outcomes implied losses but it is unlikely to be substantial for the type of amounts used in surveys and in the presence of an initial endowment as in our studies. So far, the only mechanism to elicit unverifiable signals explicitly handling risk attitudes and even non-expected utility has been proposed by Baillon and Xu (2021). It requires, however, multiple questions with the exact same signal technology.

As illustrated by Propositions 1 to 3, there are several types of equilibria. To those should be added equilibria in which signal 1 agents report 0 and conversely. These latter equilibria did not occur in Study 1. Interestingly, at the aggregate level, participants seemed to play the strategies of Proposition 3, and those who did not draw a signal played a mixed strategy (at the aggregate level) where the randomization probability was equal to the prior.

We considered a very simple model, binary in all dimensions. Effort could be continuous, signal informativeness could be a function of effort, and answers could be non-binary. We leave these refinements for future research. Similarly, we limited our analysis to some types of psychological costs. Others would be possible but are unlikely to substantially change the results. For instance, symmetric reporting costs would not bring new insights but only require higher payoffs (by rescaling π).

The asymmetric reporting cost, a_i, is exogenous. However, setting up peer betting (or any incentive mechanism) may necessitate to break anonymity to process payment. The lack of anonymity may then increase a_i further. There are practical solutions to this problem. For instance, as we did in Study 2, one can erect a 'China wall' between the payment provider (Prolific, who knows identity but not people's answer) and the center (the researchers who know the answers but not the respondents' identities).

4.2 Empirical limitations

Study 1 borrowed tasks from the experimental literature, which allowed us to observe effort and signal acquisition. The main drawback is that those tasks were artificial, and may have been seen as quite unnatural. Furthermore, there was hardly any reason not to reveal the acquired signal. Study 2 was conducted to test whether peer betting elicits signal acquisition and revelation in a more realistic context. Results of Study 2 give credence to the real-world validity of peer betting, but signal acquisition can only be proxied by decision time and ground truth is not observable.

Both studies were conducted online with participants from the Prolific platform. Participants from online platforms take part in experiments in an uncontrolled setting such as their home. This lack of experimental control has elicited concerns amongst researchers. However, experimental research has shown that this concerns is largely unfounded. Hauser and Schwarz (2016) demonstrated that participants from an online platform are more attentive than college students. Peer, Rothschild, Gordon, Evernden, and Damer (2022) demonstrated that Prolific outperformed other participant platforms regarding data quality. To ensure high data quality in the current research, post-experimental quiz questions were included in Study 1, allowing to remove inattentive participants. In Study 2, the instructions in the Peer Betting treatment emphasize that the bonuses depend on others' responses.

In Study 2, participants were asked about their violations of COVID guidelines. The discrepancy between the prevalence of self-reported lies (Debey, De Schryver, Logan, Suchotzki, and Verschuere, 2015) and lies told during experimental research (Feldman, Forrest, and Happ, 2002) demonstrates that people are reluctant to admit anti-social behavior. Since violations of COVID guidelines could negatively affect the health of both oneself and others, a violation of COVID guidelines can be seen as immoral behavior. However, the questions we use limited this effect. In most statements, non-compliance could have been due to behavior of others. Results of Study 2 demonstrate that participants in the Peer Betting treatment admitted more violations of COVID guidelines than in both control surveys. Peer betting may have helped overcome the discomfort of reporting non-compliance with health guidelines (a_i in the theory). However, peer betting has no effect though when we replace "at least once" by "at least twice" in the statements. In the latter case, it is more difficult to minimize one's responsibility and the asymmetric cost is therefore likely to be higher.

Effort was directly observable in Study 1, which is the main reason why we used artificial tasks. However, it was not observable in Study 2 and we used response times as a proxy. We could not exclude that participants took more time to answer partly due to the presence of past endorsement rates. In a comparable setting, using the Bayesian truth-serum to study health-related questions, Baillon, Bleichrodt, and Granic (2022) also used answer time as a proxy for effort and found that incentives increased response time. We may expect response times to be more noisy in online experiments where participants could be subject to more distractions. Approximating effort by response time is imperfect and a different operationalization of effort might have shown a more solid effect of peer betting on effort, as found in Study 1.

In Study 2, there is no ground truth that allows a verification of the self-reported information. We chose such a setting because it corresponds to a practical case in which peer betting can be valuable. Alternative settings, in which ground truth is observable, are not ideal to test signal revelation. Respondents may expect their answers to be checked and that mere expectation may influence their behavior. Such settings (as in Study 1) are more useful to study signal acquisition. Hence, we decided to test peer betting in its natural setting. Even without ground truth, the directional effect of peer betting could be hypothesized. In Study 2, we predicted that participants would be more likely to report True under peer betting, because people may have motives to not reveal their anti-social behavior in a regular survey. Results indicate that peer betting affected the responses in the direction predicted by our theory. Moreover, the Flat-PastRate treatment allowed us to rule out the alternative explanation that merely mentioning prior expectations could create social norms and influence answers.

Incentives for unverifiable truths have been implemented in experiments and surveys before (e.g;, John, Loewenstein, and Prelec, 2012; Weaver and Prelec, 2013; Frank, Cebrian, Pickard, and Rahwan, 2017; Baillon, Bleichrodt, and Granic, 2022) but these studies had two major drawbacks. First, the participants had to report both an endorsement and a prediction of others' endorsements, making the task more cumbersome. Second, the payoff rule was not transparent. Participants were told truth-telling were in their interest with a reference to Prelec (2004). By contrast, our peer betting incentives require only an endorsement (no prediction task) and the payment rule is simple and transparent.

5 Conclusion

When responses to questions cannot be independently verified, researchers and practitioners often rely on simple surveys with fixed rewards. However, such surveys fail to incentivize individuals to acquire costly information and disclose it truthfully. Since Crémer and McLean (1988), the economic literature has proposed various mechanisms to elicit private signals, but their real-world application has been limited due to their complexity.

This paper introduces peer betting, a simple and transparent mechanism designed to encourage individuals to acquire and reveal private signals in binary-choice settings. We tested peer betting in two experimental studies. The first study demonstrates that the mechanism successfully motivates participants to exert costly effort to obtain information. In the second study, we applied peer betting to a practical case: eliciting unverifiable information about compliance with Covid-19 safety guidelines. Because participants' actual compliance was unobservable to the surveyor, this setting provided a real-world test of the mechanism. Our results suggest that peer betting can be effectively implemented to elicit more truthful responses to mildly stigmatizing questions.

A Appendix - Proofs

A.1 Lemma 1

Proof. First part 3 of Assumption 1 excludes $\bar{\omega} \in \{0, 1\}$.

Second, $P_i(s_i = 1) = \int_0^1 P_i(s_i = 1|\omega = o) \times P_i(\omega = o)do = \int_0^1 o \times P_i(\omega = o)do = E_i(\omega) = \bar{\omega}$. $\bar{\omega}_i^1 = \int_0^1 \frac{P_i(s_i=1|\omega=o)\times P_i(\omega=o)\times o}{P_i(s_i=1)}do = \int_0^1 \frac{o^2 \times P_i(\omega=o)}{\bar{\omega}}do > \bar{\omega}$ because $\int_0^1 o^2 \times P_i(\omega = o) >$

$\left(\int_0^1 o \times P_i(\omega = o)\right)^2 = \bar{\omega}^2$ by Jensen's inequality applied to the convex squared function and the inequality is strict because degenerate cases were excluded by Part 3 of Assumption 1, which also excludes a posterior expectation of 1. The proof of $0 < \bar{\omega}_i^0 < \bar{\omega}$ is symmetric. □

A.2 Proposition 1

Proof. Possible earnings $(\bar{r} - \bar{\omega})\pi$ and $(\bar{\omega} - \bar{r})\pi$ are both strictly lower than π, and therefore than c_i if $c_i > \pi$. There are no incentives to provide efforts; hence, $e_i = 0$. Consider agent i and assume all other agents $j \neq i$ have the same probability to report 1 ($R_j = R$ for some $R \in [0, 1]$). Hence, with N infinite, the final bet value $\bar{r}$ is R. Agent i hence expects to earn $[R_i \times (R - \bar{\omega}) + (1 - R_i) \times (\bar{\omega} - R)] \times \pi$. If $R \in (\bar{\omega}, 1]$, then $R_i = 1$ is optimal. If $R \in [0, \bar{\omega})$, then $R_i = 0$ is optimal. Finally, if $R = \bar{\omega}$, then any $R_i \in [0, 1]$ is optimal. Nash equilibria require $R_i = R$ such that no one has incentives to deviate. Hence, we must have either $R_i = 1$ for all i, or $R_i = 0$ for all i, or $R_i = \bar{\omega}$ for all i. In all these cases, earnings are 0 (remember that if $\bar{r} = 0$ or 1, no payoffs occur as specified in step 4 of Definition 1. □

A.3 Proposition 2

Proof. Let us consider agent i's view point and assume $e_j = 1$, $R_j^0 = 0$, and $R_j^1 = 1$ for all $j \neq i$. Without any signal, agent i's expected earnings are

$$[R_i \left(E_i(\omega) - \bar{\omega}\right) + (1 - R_i) \left(\bar{\omega} - E_i(\omega)\right)] \times \pi = 0$$

by Assumption 2.

With signal 1, agent i's expected earnings are

$$\left[R_i^1 \left(\bar{\omega}_i^1 - \bar{\omega}\right) + (1 - R_i^1) \left(\bar{\omega} - \bar{\omega}_i^1\right)\right] \times \pi$$

. By Lemma 1, this is maximum for $R_i^1 = 1$, yielding $(\bar{\omega}_i^1 - \bar{\omega}) \times \pi > 0$.

With signal 0, agent i's expected earnings are

$$\left[R_i^0 \left(\bar{\omega}_i^0 - \bar{\omega}\right) + (1 - R_i^0) \left(\bar{\omega} - \bar{\omega}_i^0\right)\right] \times \pi$$

. By Lemma 1 again, this is maximum for $R_i^0 = 0$, yielding $(\bar{\omega} - \bar{\omega}_i^0) \times \pi > 0$.

Before getting a signal, the expected gain is therefore,

$$\left[P_i(s_i = 1) \times \left(\bar{\omega}_i^1 - \bar{\omega}\right) + P_i(s_i = 0)\left(\bar{\omega} - \bar{\omega}_i^0\right)\right] \times \pi = \left[\bar{\omega} \times \left(\bar{\omega}_i^1 - \bar{\omega}\right) + (1 - \bar{\omega})\left(\bar{\omega} - \bar{\omega}_i^0\right)\right] \times \pi.$$

This is strictly positive by construction and strictly more than c_i by assumption. Hence, the net earnings (once the costs are subtracted) are strictly positive and providing an effort is worth it. As a consequence, $e_i = 1$, $R_i^0 = 0$, and $R_i^1 = 1$ is a Nash equilibrium.

Finally, let us consider the case in which all agents but i provide no efforts and report 1 with probability R. The expected earnings are

$$\begin{cases} [R_i^1 \times (R - \bar{\omega}) + (1 - R_i^1) \times (\bar{\omega} - R)] \times \pi & \text{with signal 1} \\ [R_i^0 \times (R - \bar{\omega}) + (1 - R_i^0) \times (\bar{\omega} - R)] \times \pi & \text{with signal 0} \\ [R_i \times (R - \bar{\omega}) + (1 - R_i) \times (\bar{\omega} - R)] \times \pi & \text{with no signal.} \end{cases}$$

As in Proposition 1, the only equilibria must be of the form $R_i = R \in \{0, \omega, 1\}$, and by similar arguments $R_i^1 = R_i^0 = R \in \{0, \omega, 1\}$. The earnings are always 0 and the net earnings with effort are even strictly negative. Hence, $e_i = 0$, $R_i \in \{0, \omega, 1\}$ is also a Nash equilibrium (with $R_i^1 = R_i^0 = R_i$) but it is dominated by the equilibrium with signal acquisition and revelation ($e_i = 1$, $R_i^0 = 0$, and $R_i^1 = 1$). □

A.4 Proposition 3

Proof. First, let us assume that all agents but i play the strategy described in the proposition. With signal 1, agent i expects the final bet value to be $T\bar{\omega} + (1-T)\omega_i^1$, and with signal 0 $T\bar{\omega} + (1-T)\omega_i^0$. By Lemma 1, $T\bar{\omega} + (1-T)\omega_i^0 < \bar{\omega} < T\bar{\omega} + (1-T)\omega_i^1$, and with the same argument as in the proof of Proposition 2, it is best to reveal signals, $R_i^0 = 0$ and $R_i^1 = 1$. Ex ante, the expected benefit of exerting an effort is therefore
$[\bar{\omega} \times (T\bar{\omega} + (1-T)\bar{\omega}_i^1 - \bar{\omega}) + (1 - \bar{\omega})(\bar{\omega} - T\bar{\omega} - (1-T)\bar{\omega}_i^0))]\pi - c_i$.

If $\frac{c_i}{\pi} \leq \bar{\omega} \times (T\bar{\omega} + (1-T)\bar{\omega}_i^1 - \bar{\omega}) + (1 - \bar{\omega})(\bar{\omega} - T\bar{\omega} - (1-T)\bar{\omega}_i^0)$ then $e_i = 1$ is optimal.

If $\frac{c_i}{\pi} > \bar{\omega} \times (T\bar{\omega} + (1-T)\bar{\omega}_i^1 - \bar{\omega}) + (1 - \bar{\omega})(\bar{\omega} - T\bar{\omega} - (1-T)\bar{\omega}_i^0)$, an effort leads to negative net earnings, whereas exerting no efforts gives
$[R_i \times (T\bar{\omega} + (1-T)E_i(\omega) - \bar{\omega}) + (1 - R_i)(\bar{\omega} - T\bar{\omega} - (1-T)E_i(\omega))]\,\pi \;=\; 0$ because of the common prior expectations assumption. Hence, $e_i = 0$ and $R_i = \bar{\omega}$ is a best

response in this case. □

A.5 Proposition 4

Proof. Let us consider agent i's view point and assume $e_j = 1$, $R_j^0 = 0$, and $R_j^1 = 1$ for all $j \neq i$. Without any signal, agent i's expected earnings are

$$\left[R_i\left(E_i(\omega) - \bar{\omega} - \frac{a_i}{\pi}\right) + (1 - R_i)\left(\bar{\omega} - E_i(\omega)\right)\right] \times \pi \leq 0.$$

With signal 1, agent i's expected earnings are

$$\left[R_i^1\left(\bar{\omega}_i^1 - \bar{\omega} - \frac{a_i}{\pi}\right) + (1 - R_i^1)\left(\bar{\omega} - \bar{\omega}_i^1 - \frac{d_i}{\pi}\right)\right] \times \pi - c_i.$$

This is maximum for $R_i^1 = 1$, because $\frac{a_i}{\pi} < \frac{d_i}{\pi} + 2\left(\bar{\omega}_i^1 - \bar{\omega}\right)$. With signal 0, agent i's expected earnings are

$$\left[R_i^0\left(\bar{\omega}_i^0 - \bar{\omega} - \frac{a_i}{\pi} - \frac{d_i}{\pi}\right) + (1 - R_i^0)\left(\bar{\omega} - \bar{\omega}_i^0\right)\right] \times \pi - c_i.$$

This is maximum for $R_i^0 = 0$. Before getting a signal, the expected payoff is therefore, $\left[\bar{\omega} \times \left(\bar{\omega}_i^1 - \bar{\omega} - \frac{a_i}{\pi}\right) + (1 - \bar{\omega})\left(\bar{\omega} - \bar{\omega}_i^0\right)\right] \times \pi - c_i$. This is strictly positive by assumption. Hence, providing an effort is worth it. As a consequence, $e_i = 1$, $R_i^0 = 0$, and $R_i^1 = 1$ is a Nash equilibrium.

Finally, let us consider the case in which all agents but i provide no efforts and report 0 (as in Proposition 1). The best agent i can do is to provide no effort and report 0 as well, yielding expected earnings 0, which is dominated by signal acquisition and revelation. □

References

Abeler, J., A. Falk, L. Goette, and D. Huffman (2011): "Reference points and effort provision," *American Economic Review*, 101(2), 470–492.

Abeler, J., D. Nosenzo, and C. Raymond (2019): "Preferences for truth-telling," *Econometrica*, 87(4), 1115–1153.

Baillon, A. (2017): "Bayesian markets to elicit private information," *Proceedings of the National Academy of Sciences*, 114(30), 7958–7962.

Baillon, A., H. Bleichrodt, and G. D. Granic (2022): "Incentives in surveys," *Journal of Economic Psychology*, 93, 102552.

Baillon, A., and Y. Xu (2021): "Simple bets to elicit private signals," *Theoretical Economics*, 16(3), 777–797.

Bergemann, D., and S. Morris (2005): "Robust Mechanism Design," *Econometrica*, 73(6), 1771–1813.

——— (2009a): "Robust implementation in direct mechanisms," *The Review of Economic Studies*, 76(4), 1175–1204.

——— (2009b): "Robust Virtual Implementation," *Theoretical Economics*, 4(1), 45–88.

Bikhchandani, S. (2010): "Information acquisition and full surplus extraction," *Journal of Economic Theory*, 145(6), 2282–2308.

Bikhchandani, S., and I. Obara (2017): "Mechanism design with information acquisition," *Economic Theory*, 63(3), 783–812.

Cialdini, R. B. (2008): *Influence: Science and Practice*. Pearson Education, Boston, 5th edn.

Crémer, J., and R. P. McLean (1988): "Full Extraction of the Surplus in Bayesian and Dominant Strategy Auctions," *Econometrica*, 56(6), 1247–1257.

Cvitanić, J., D. Prelec, B. Riley, and B. Tereick (2019): "Honesty via choice-matching," *American Economic Review: Insights*, 1(2), 179–92.

Danz, D., L. Vesterlund, and A. J. Wilson (2022): "Belief elicitation and behavioral incentive compatibility," *American Economic Review*, 112(9), 2851–2883.

Dasgupta, A., and A. Ghosh (2013): "Crowdsourced judgement elicitation with endogenous proficiency," in *Proceedings of the 22nd International Conference on World Wide Web*, WWW '13, p. 319–330, New York, NY, USA. Association for Computing Machinery.

Debey, E., M. De Schryver, G. D. Logan, K. Suchotzki, and B. Verschuere (2015): "From junior to senior Pinocchio: A cross-sectional lifespan investigation of deception," *Acta Psychologica*, 160, 58–68.

Feldman, R. S., J. A. Forrest, and B. R. Happ (2002): "Self-presentation and verbal deception: Do self-presenters lie more?," *Basic and applied social psychology*, 24(2), 163–170.

Frank, M. R., M. Cebrian, G. Pickard, and I. Rahwan (2017): "Validating Bayesian truth serum in large-scale online human experiments," *PLOS ONE*, 12(5), 1–13.

Gentzkow, M., and E. Kamenica (2014): "Costly persuasion," *American Economic Review*, 104(5), 457–462.

Hauser, D. J., and N. Schwarz (2016): "Attentive Turkers: MTurk participants perform better on online attention checks than do subject pool participants," *Behavior research methods*, 48(1), 400–407.

John, L. K., G. Loewenstein, and D. Prelec (2012): "Measuring the Prevalence of Questionable Research Practices With Incentives for Truth Telling," *Psychological Science*, 23(5), 524–532, PMID: 22508865.

Kamenica, E. (2017): "Information economics," *Journal of Political Economy*, 125(6), 1885–1890.

——— (2019): "Bayesian persuasion and information design," *Annual Review of Economics*, 11, 249–272.

Levine, T. R. (2014): "Truth-default theory (TDT) a theory of human deception and deception detection," *Journal of Language and Social Psychology*, 33(4), 378–392.

Levine, T. R., R. K. Kim, and L. M. Hamel (2010): "People lie for a reason: Three experiments documenting the principle of veracity," *Communication Research Reports*, 27(4), 271–285.

Liu, Y., and Y. Chen (2017a): "Machine-Learning Aided Peer Prediction," in *Proceedings of the 2017 ACM Conference on Economics and Computation*, EC '17, pp. 63–80, New York, NY, USA. ACM.

——— (2017b): "Sequential peer prediction: Learning to elicit effort using posted prices," in *Thirty-First AAAI Conference on Artificial Intelligence*.

Lundquist, T., T. Ellingsen, E. Gribbe, and M. Johannesson (2009): "The aversion to lying," *Journal of Economic Behavior & Organization*, 70(1-2), 81–92.

Mazar, N., O. Amir, and D. Ariely (2008): "The dishonesty of honest people: A theory of self-concept maintenance," *Journal of marketing research*, 45(6), 633–644.

Milgrom, P., and N. Stokey (1982): "Information, trade and common knowledge," *Journal of economic theory*, 26(1), 17–27.

Miller, N., P. Resnick, and R. Zeckhauser (2005): "Eliciting informative feedback: The peer-prediction method," *Management Science*, 51(9), 1359–1373.

Morgan, T. J., and K. N. Laland (2012): "The Biological Bases of Conformity," *Frontiers in Neuroscience*, 6.

Myerson, R. B. (1986): "Multistage Games with Communication," *Econometrica*, 54(2), 323–358.

Nguyen, A., and T. Y. Tan (2021): "Bayesian persuasion with costly messages," *Journal of Economic Theory*, 193, 105212.

Ollár, M., and A. Penta (2017): "Full implementation and belief restrictions," *American Economic Review*, 107(8), 2243–77.

——— (2019): "Implementation via transfers with identical but unknown distributions," *Barcelona GSE working paper*, (1126).

Osborne, M. J., and A. Rubinstein (1994): *A course in game theory*. MIT press.

Peer, E., D. Rothschild, A. Gordon, Z. Evernden, and E. Damer (2022): "Data quality of platforms and panels for online behavioral research," *Behavior Research Methods*, 54(4), 1643–1662.

Prelec, D. (2004): "A Bayesian Truth Serum for Subjective Data," *Science*, 306(5695), 462–466.

Radanovic, G., and B. Faltings (2013): "A robust Bayesian truth serum for non-binary signals," in *Twenty-Seventh AAAI Conference on Artificial Intelligence*.

——— (2014): "Incentives for Truthful Information Elicitation of Continuous Signals," in *AAAI Conference on Artificial Intelligence*.

Radanovic, G., B. Faltings, and R. Jurca (2016): "Incentives for effort in crowdsourcing using the peer truth serum," *ACM Transactions on Intelligent Systems and Technology (TIST)*, 7(4), 48.

Shnayder, V., A. Agarwal, R. Frongillo, and D. C. Parkes (2016): "Informed Truthfulness in Multi-Task Peer Prediction," in *Proceedings of the 2016 ACM Conference on Economics and Computation*, EC '16, p. 179–196, New York, NY, USA. Association for Computing Machinery.

Suchotzki, K., B. Verschuere, B. Van Bockstaele, G. Ben-Shakhar, and G. Crombez (2017): "Lying takes time: A meta-analysis on reaction time measures of deception.," *Psychological Bulletin*, 143(4), 428.

Tourangeau, R., and T. Yan (2007): "Sensitive questions in surveys.," *Psychological bulletin*, 133(5), 859.

Toussaert, S. (2018): "Eliciting temptation and self-control through menu choices: A lab experiment," *Econometrica*, 86(3), 859–889.

Waggoner, B., and Y. Chen (2013): "Information Elicitation Sans Verification," in *Proceedings of the 3rd Workshop on Social Computing and User Generated Content, in conjunction with ACM EC'13*, vol. 16.

Weaver, R., and D. Prelec (2013): "Creating Truth-Telling Incentives with the Bayesian Truth Serum," *Journal of Marketing Research*, 50(3), 289–302.

Wilson, R. (1987): *Game-theoretic analyses of trading processes*p. 33–70, Econometric Society Monographs. Cambridge University Press.

Witkowski, J., Y. Bachrach, P. Key, and D. Parkes (2013): "Dwelling on the Negative: Incentivizing Effort in Peer Prediction," *Proceedings of the AAAI Conference on Human Computation and Crowdsourcing*, 1(1), 190–197.

Witkowski, J., and D. C. Parkes (2012a): "Peer prediction without a common prior," in *Proceedings of the 13th ACM Conference on Electronic Commerce*, EC '12, p. 964–981, New York, NY, USA. Association for Computing Machinery.

Witkowski, J., and D. C. Parkes (2012b): "A robust Bayesian truth serum for small populations," in *Twenty-Sixth AAAI Conference on Artificial Intelligence*.

——— (2013): "Learning the prior in minimal peer prediction," in *Proceedings of the 3rd Workshop on Social Computing and User Generated Content at the ACM Conference on Electronic Commerce*, vol. 14.

Online Appendices

B Experimental materials for Study 1

Table B1 provides detailed information on the pairs of boxes in each prediction task. The exact composition of Yellow/Blue is unknown to the participants.

		Participants' information		Exact Yellow/Blue	
Pair	Total Yellow/Blue	Left box	Right box	Left box	Right box
1.	60Y 140B	More than 30Y	More than 70B	40Y 60B	20Y 80B
2.	70Y 130B	More than 35Y	More than 65B	40Y 60B	30Y 70B
3.	80Y 120B	More than 40Y	More than 60B	48Y 52B	32Y 68B
4.	90Y 110B	More than 45Y	More than 55B	56Y 44B	34Y 66B
5.	100Y 100B	More than 50Y	More than 50B	62Y 38B	38Y 62B
6.	100Y 100B	More than 50Y	More than 50B	57Y 43B	43Y 57B
7.	110Y 90B	More than 55Y	More than 45B	69Y 31B	41Y 59B
8.	120Y 80B	More than 60Y	More than 40B	69Y 31B	51Y 49B
9.	130Y 70B	More than 65Y	More than 35B	78Y 22B	52Y 48B
10.	140Y 60B	More than 70Y	More than 30B	77Y 23B	63Y 37B

Table B1: The content of boxes and participants' information in each pair

Table B2 shows the theoretical prior and posterior beliefs of a participant in each pair. Consider pair 1 where there are 60 yellow and 140 blue balls in total. The left (right) box includes more (less) than 30 yellow. Prior to observing the draw, each box is equally likely to be the actual box. Thus, the common prior expectation on yellow (blue) is 30 (70). If the draw is yellow, the left box will be considered more likely. Then, the posterior expectation on yellow will be within $(30, 60]$, while the posterior on blue is simply 100 minus the posterior on yellow. Note that the exact posterior expectation of a participant depends on the prior belief on the composition of the boxes, which is not restricted by the experiment, in accordance with the theoretical framework. Participants with a yellow (blue) draw expect left (right) box to be more likely for the actual box. Under the equilibrium in Proposition 2, participants with a yellow (blue) draw would pick the left (right) box. The last column in Table B2 gives the range of expected bonus in the Peer Betting treatment if the participant's pick (left if yellow draw, right if blue draw) corresponds to the actual box. Note that

E[bonus | pick = actual] = 20p for all pairs in the Accuracy treatment. This constant value is set to achieve a payoff equivalence between the Peer Betting and Accuracy treatments. To illustrate, consider pair 1 and suppose a participant with a yellow draw has a uniform belief over all possible Yellow/Blue compositions in the left box. Then, the exact E[bonus | pick = actual] is 15p. Under the uniformity assumption, the expected bonus ranges from 15p to 25p across all pairs, with an average of 20p.

	Priors		Posterior on Yellow		Range of E[bonus | pick = actual]
Pair	Yellow	Blue	Yellow draw	Blue draw	Posterior (draw) - Prior (draw)
1.	30	70	(30,60]	[0,30)	(0p,30p]
2.	35	65	(35,70]	[0,35)	(0p,35p]
3.	40	60	(40,80]	[0,40)	(0p,40p]
4.	45	55	(45,90]	[0,45)	(0p,45p]
5.	50	50	(50,100]	[0,50)	(0p,50p]
6.	50	50	(50,100]	[0,50)	(0p,50p]
7.	55	45	(55,100]	[0,55)	(0p,45p]
8.	60	40	(60,100]	[0,60)	(0p,40p]
9.	65	35	(65,100]	[0,65)	(0p,35p]
10.	70	30	(70,100]	[0,70)	(0p,30p]

Table B2: Priors, posteriors and expected bonus conditional on an accurate pick.

Figure B1 show the matrices used in effort tasks. Each prediction task $i \in \{1, 2, \ldots, 10\}$ uses pair i in Table B1, and the corresponding effort task uses matrix i in Figure B1.

0	0	1	1	1	1
0	0	1	1	0	0
1	1	1	1	1	0
0	1	0	1	1	1

(1)

0	0	1	0	1	0
1	0	1	1	1	0
0	0	1	0	0	0
0	1	0	0	0	1

(2)

1	0	0	1	1	1
1	0	1	1	0	1
0	1	0	1	1	1
1	1	1	0	0	1

(3)

1	0	0	1	1	1
1	0	1	1	0	1
0	1	0	1	1	1
1	1	1	0	0	1

(4)

0	0	1	1	1	1
0	0	1	1	0	0
1	1	1	1	1	0
0	1	0	1	1	1

(5)

0	0	1	0	1	0
1	0	1	1	1	0
0	0	1	0	0	0
0	1	0	0	0	1

(6)

1	0	0	1	1	1
1	0	1	1	0	1
0	1	0	1	1	1
1	1	1	0	0	1

(7)

1	0	0	1	1	1
1	0	1	1	0	1
0	1	0	1	1	1
1	1	1	0	0	1

(8)

0	0	1	1	1	1
0	0	1	1	0	0
1	1	1	1	1	0
0	1	0	1	1	1

(9)

0	0	1	0	1	0
1	0	1	1	1	0
0	0	1	0	0	0
0	1	0	0	0	1

(10)

Figure B1: Binary matrices used real effort tasks.

Complete instructions for all treatments in both Study 1 and Study 2 are available in Appendix D.2.2.

C Summary statistics

Table C1: Summary statistics, Study 1

	Experimental Treatment		
	Flat	Accuracy	Peer Betting
Number of participants	68	72	70
Female/Male	29/39	36/36	34/36
Average age	23.09	23.76	22.64
US resident	63	65	62
Average duration	8 min 59 sec	9 min 31 sec	9 min 8 sec
Min/Average/Max reward (£)	3.25/3.25/3.25	2.05/3.50/4.85	2.65/3.34/3.94
Correct answer in pre-experimental quiz	54	67	57
Correct answer in post-experimental quiz	68	72	66

Table C2: Study 2, Week 0 answers

	Percentage of 'true' picks				
Question	once or more	twice or more	3 times or more	4 times or more	5 times or more
1	18	12	6	4	4
2	76	50	20	6	2
3	58	22	8	4	2
4	16	8	0	0	0
5	70	34	14	4	2
6	24	10	8	4	2
7	54	24	8	2	2
8	12	4	2	2	2

Table C3: Summary statistics, Study 2

	Exp. Treatment / version					
Week 1						
	Flat / 'once'	Flat-PastRate / 'once'	Peer Betting / 'once'	Flat / 'twice'	Flat-PastRate / 'twice'	Peer Betting / 'twice'
Number of participants	53	53	52	54	54	53
Female/Male	36/17	36/17	33/19	36/18	25/29	33/20
Average age	24.85	23.53	22.73	23.11	23.57	25.17
UK/Non-UK citizen	42/11	36/17	40/12	44/10	45/9	37/16
Average duration	2 min 10 sec	2 min 38 sec	3 min 34 sec	2 min 14 sec	2 min 30 sec	3 min 38 sec
Min/Average/ Max reward (£)	1.75/1.75/ 1.75	1.75/1.75/ 1.75	1.49/2.03/ 2.39	1.75/1.75/ 1.75	1.75/1.75/ 1.75	1.43/1.81/ 2.23
Week 2						
Number of participants	54	52	54	54	54	54
Female/Male	31/23	31/21	39/15	37/17	39/15	38/16
Average age	24.39	25.65	24.98	25.13	24.25	25.09
UK/Non-UK citizen	46/8	44/8	43/11	43/11	46/8	48/6
Average duration	2 min 14 sec	2 min 52 sec	3 min 44 sec	2 min 45 sec	2 min 25 sec	4 min 12 sec
Min/Average/ Max reward (£)	1.75/1.75/ 1.75	1.75/1.75/ 1.75	1.47/1.66/ 1.88	1.75/1.75/ 1.75	1.75/1.75/ 1.75	1.18/1.73/ 2.16

Table C4: Prior on True, Study 2. Priors on False are given by 100-Prior on True

		Question							
Week	Survey version	1	2	3	4	5	6	7	8
week 1	at least once	18	76	58	16	70	24	54	12
week 1	at least once	12	50	22	8	34	10	24	4
week 2	at least twice	15	88	44	17	83	15	71	12
week 2	at least twice	6	64	32	6	28	8	34	2

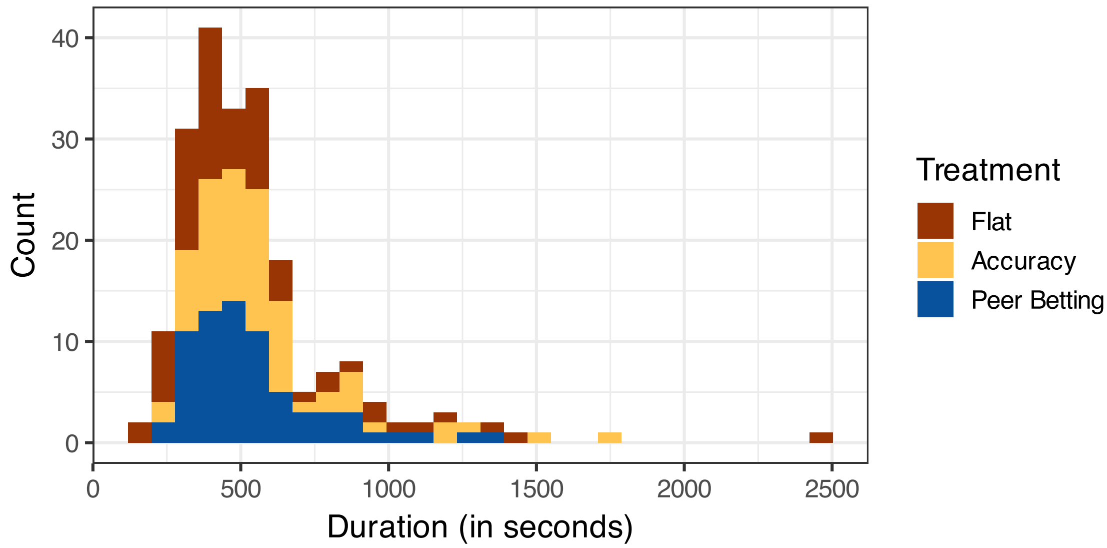


Figure C1: The distribution of completion times in seconds, Study 1.

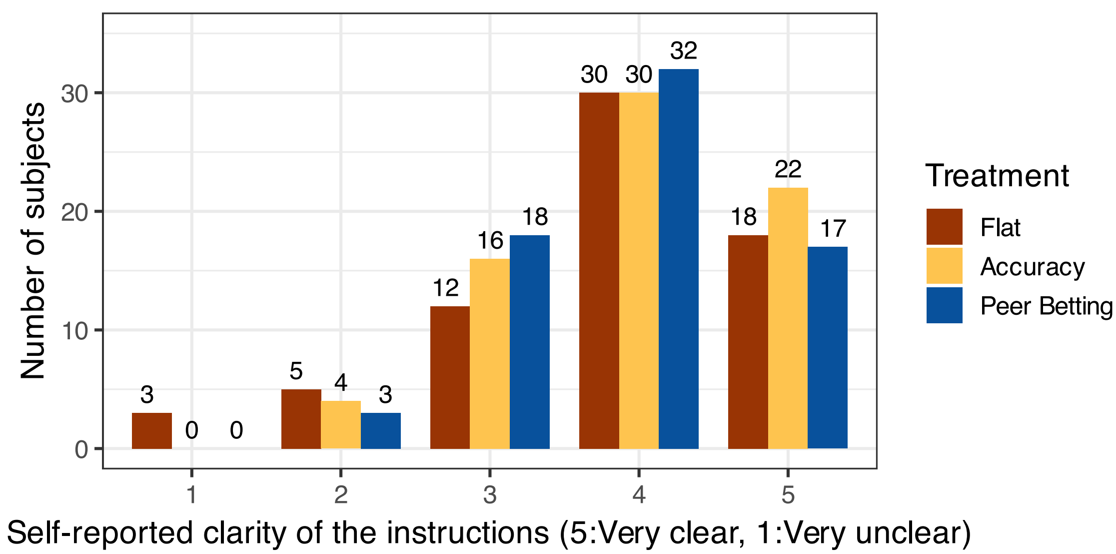


Figure C2: The distribution of participants' responses to the question "How clear were the instructions in this experiment?" in Study 1, coded on a scale 1 to 5.

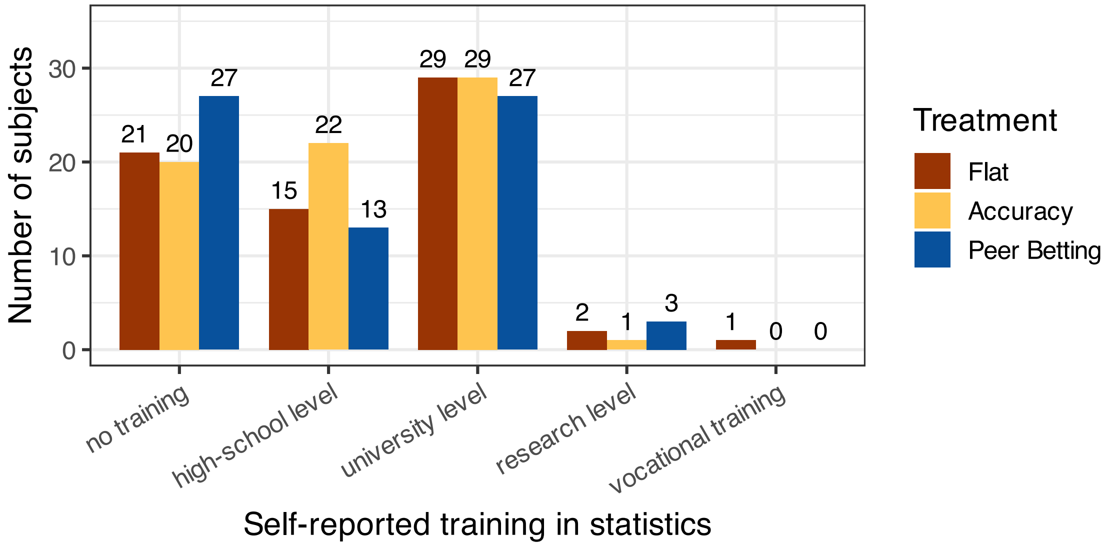


Figure C3: The distribution of participants' responses to the question "Did you receive a training in statistics? If yes, on which level?" in Study 1.

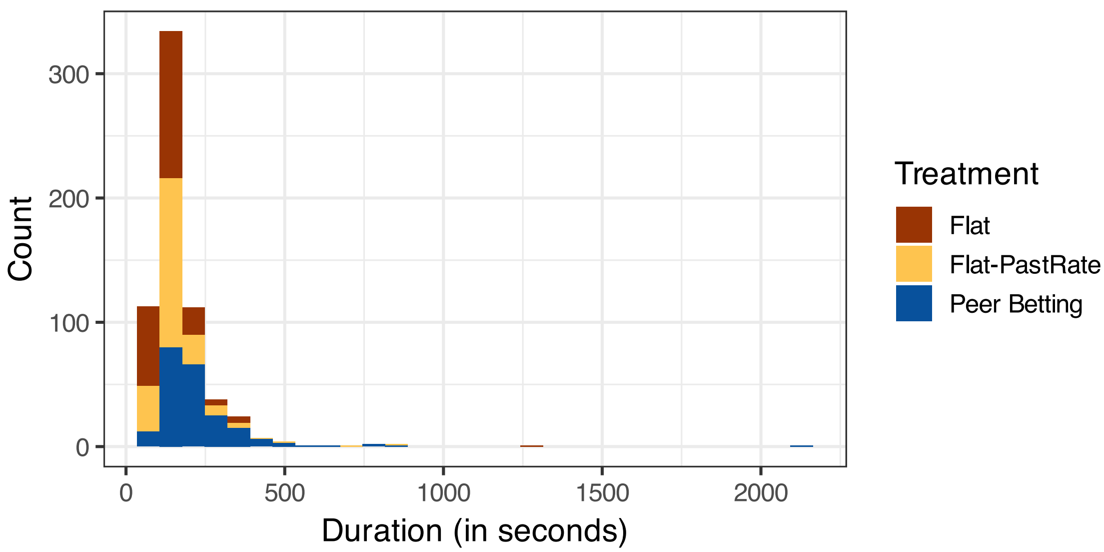


Figure C4: The distribution of completion times in seconds, Study 2.

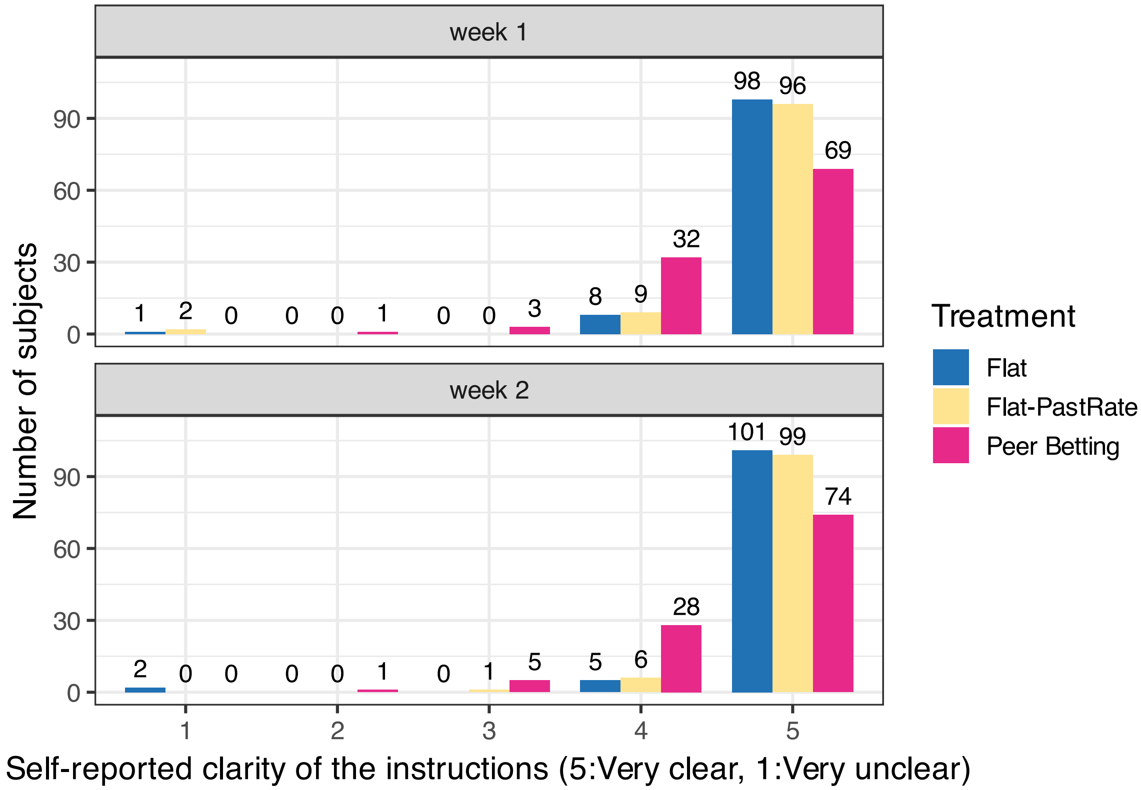


Figure C5: The distribution of participants' responses to the question "How clear were the instructions in this experiment?" in Study 2, coded on a scale 1 to 5.

D Additional results

D.1 Study 1

(a) Correlation tests

Treatment	Draw	Pearson's C.C.	Spearman's C.C.
Flat	yellow	r = 0.33, p = 0.349	rho = 0.3, p = 0.393
Flat	blue	r = 0.83, p = 0.003	rho = 0.95, p< 0.001
Flat	no draw	r = 0.88, p = 0.001	rho = 0.87, p = 0.001
Accuracy	yellow	r = 0.53, p = 0.118	rho = 0.55, p = 0.101
Accuracy	blue	r = 0.5, p = 0.138	rho = 0.45, p = 0.192
Accuracy	no draw	r = -0.37, p = 0.291	rho = -0.3, p = 0.402
Peer Betting	yellow	r = 0.53, p = 0.118	rho = 0.52, p = 0.121
Peer Betting	blue	r = 0.28, p = 0.425	rho = 0.21, p = 0.555
Peer Betting	no draw	r = 0.64, p = 0.048	rho = 0.68, p = 0.032

(b) Two-sided t-test and Wilcoxon test

Treatment	Draw	T-test	Wilcoxon test
Flat	yellow	t = 8.86, p< 0.001	W = 100, p< 0.001
Flat	blue	t = -8.42, p< 0.001	W = 0, p< 0.001
Flat	no draw	t = 0.78, p = 0.446	W = 64, p = 0.307
Accuracy	yellow	t = 8.47, p< 0.001	W = 100, p< 0.001
Accuracy	blue	t = -10.27, p< 0.001	W = 0, p< 0.001
Accuracy	no draw	t = -0.6, p = 0.555	W = 34, p = 0.237
Peer Betting	yellow	t = 8.56, p< 0.001	W = 100, p< 0.001
Peer Betting	blue	t = -8.12, p< 0.001	W = 1, p< 0.001
Peer Betting	no draw	t = -0.34, p = 0.739	W = 44, p = 0.676

Table D1: Proportion of left picks vs prior expectation on the number of yellow balls in the actual box.

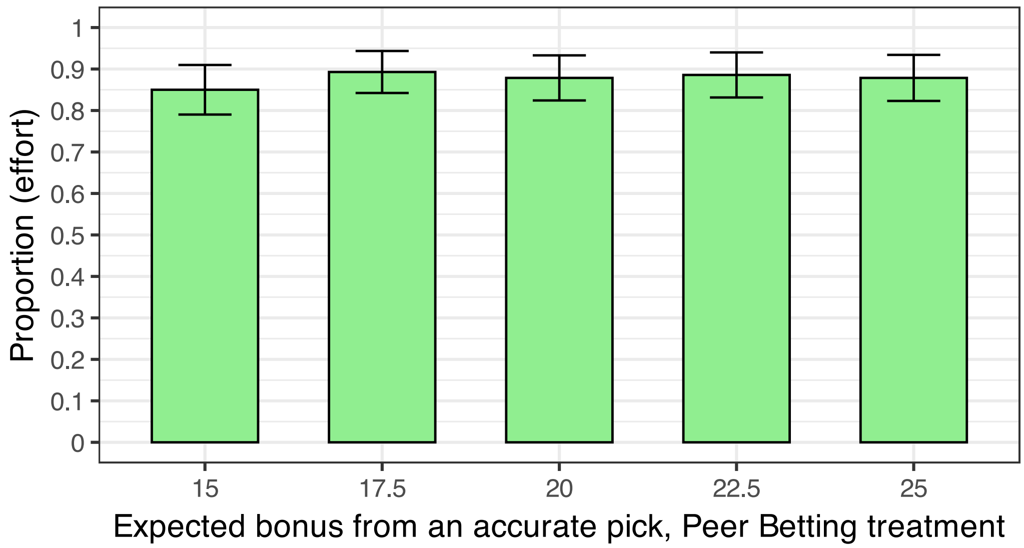


Figure D1: Effort levels in the Peer Betting treatment for different levels of the expected bonus from an accurate pick. Error bars show 95% bootstrap CI. See Table B2 for the derivation of expected bonuses.

	Dep. var.: P(effort task completed)					
	(whole sample)			*(filtered sample)*		
	(1)	(2)	(3)	(4)	(5)	(6)
(Intercept)	0.92	1.91	1.92	0.92	1.91	1.93
	(0.22)	(0.86)	(0.86)	(0.22)	(0.87)	(0.87)
	[0.48; 1.36]	[0.22; 3.60]	[0.24; 3.61]	[0.48; 1.36]	[0.20; 3.62]	[0.22; 3.64]
Accuracy	1.91	2.15	1.88	1.91	2.15	1.89
	(0.43)	(0.41)	(0.54)	(0.43)	(0.41)	(0.54)
	[1.08; 2.75]	[1.35; 2.95]	[0.83; 2.94]	[1.08; 2.75]	[1.35; 2.95]	[0.84; 2.94]
Peer Betting	1.05	0.96	0.84	0.98	0.89	0.78
	(0.36)	(0.37)	(0.42)	(0.36)	(0.37)	(0.42)
	[0.34; 1.75]	[0.23; 1.69]	[0.01; 1.67]	[0.27; 1.69]	[0.17; 1.62]	[−0.05; 1.60]
Age		−0.37	−0.37		−0.39	−0.39
		(0.26)	(0.26)		(0.26)	(0.26)
		[−0.89; 0.14]	[−0.89; 0.14]		[−0.90; 0.13]	[−0.91; 0.13]
Female?		0.37	0.37		0.33	0.33
		(0.33)	(0.33)		(0.33)	(0.33)
		[−0.29; 1.02]	[−0.29; 1.02]		[−0.32; 0.98]	[−0.32; 0.98]
US resident?		−0.24	−0.24		−0.19	−0.19
		(0.65)	(0.65)		(0.65)	(0.65)
		[−1.51; 1.03]	[−1.51; 1.04]		[−1.46; 1.08]	[−1.46; 1.08]
|Prior-50|			−0.15			−0.15
			(0.08)			(0.08)
			[−0.31; 0.01]			[−0.31; 0.01]
Task order			0.27			0.27
			(0.15)			(0.15)
			[−0.03; 0.56]			[−0.03; 0.56]
|Prior-50| x Accuracy			0.34			0.34
			(0.33)			(0.33)
			[−0.31; 0.99]			[−0.31; 0.99]
|Prior-50| x Peer Betting			0.08			0.08
			(0.16)			(0.16)
			[−0.22; 0.39]			[−0.22; 0.39]
Task order x Accuracy			−0.13			−0.13
			(0.50)			(0.50)
			[−1.11; 0.85]			[−1.11; 0.85]
Task order x Peer Betting			0.06			0.06
			(0.33)			(0.33)
			[−0.58; 0.70]			[−0.58; 0.70]
Num. obs.	2100	2070	2070	2060	2030	2030
Likl. Ratio.	148.93	175.79	179.37	146.39	173.35	176.94
LR test p-val	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001
AIC	1649.70	1549.38	1557.80	1638.88	1539.16	1547.57

Table D2: Logistic regression estimates (baseline: Flat)

	Dep. var.: P(effort task completed)					
	(whole sample)			*(filtered sample)*		
	(1)	(2)	(3)	(4)	(5)	(6)
(Intercept)	1.97	2.87	2.77	1.90	2.81	2.70
	(0.28)	(0.83)	(0.85)	(0.28)	(0.84)	(0.86)
	[1.41; 2.52]	[1.23; 4.50]	[1.09; 4.44]	[1.34; 2.45]	[1.15; 4.46]	[1.01; 4.40]
Flat	−1.05	−0.96	−0.84	−0.98	−0.89	−0.78
	(0.36)	(0.37)	(0.42)	(0.36)	(0.37)	(0.42)
	[−1.75; −0.34]	[−1.69; −0.23]	[−1.67; −0.01]	[−1.69; −0.27]	[−1.62; −0.17]	[−1.60; 0.05]
Accuracy	0.87	1.19	1.04	0.93	1.26	1.11
	(0.46)	(0.44)	(0.58)	(0.46)	(0.44)	(0.58)
	[−0.04; 1.77]	[0.32; 2.06]	[−0.09; 2.17]	[0.03; 1.84]	[0.39; 2.12]	[−0.02; 2.24]
Age		−0.37	−0.37		−0.39	−0.39
		(0.26)	(0.26)		(0.26)	(0.26)
		[−0.89; 0.14]	[−0.89; 0.14]		[−0.90; 0.13]	[−0.91; 0.13]
Female?		0.37	0.37		0.33	0.33
		(0.33)	(0.33)		(0.33)	(0.33)
		[−0.29; 1.02]	[−0.29; 1.02]		[−0.32; 0.98]	[−0.32; 0.98]
US resident?		−0.24	−0.24		−0.19	−0.19
		(0.65)	(0.65)		(0.65)	(0.65)
		[−1.51; 1.03]	[−1.51; 1.04]		[−1.46; 1.08]	[−1.46; 1.08]
|Prior-50|			−0.07			−0.07
			(0.13)			(0.13)
			[−0.33; 0.19]			[−0.33; 0.19]
Task order			0.32			0.33
			(0.29)			(0.29)
			[−0.24; 0.89]			[−0.24; 0.90]
|Prior-50| x Flat			−0.08			−0.08
			(0.16)			(0.16)
			[−0.39; 0.22]			[−0.39; 0.22]
|Prior-50| x Accuracy			0.25			0.26
			(0.35)			(0.35)
			[−0.43; 0.94]			[−0.43; 0.94]
Task order x Flat			−0.06			−0.06
			(0.33)			(0.33)
			[−0.70; 0.58]			[−0.70; 0.58]
Task order x Accuracy			−0.19			−0.19
			(0.56)			(0.56)
			[−1.28; 0.91]			[−1.29; 0.91]
Num. obs.	2100	2070	2070	2060	2030	2030
Likl. Ratio.	148.93	175.79	179.37	146.39	173.35	176.94
LR test p-val	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001
AIC	1649.70	1549.38	1557.80	1638.88	1539.16	1547.57

Table D3: Logistic regression estimates (baseline: Peer Betting)

Dep. var.: P(effort task completed)						
	(whole sample)			*(filtered sample)*		
	(1)	(2)	(3)	(4)	(5)	(6)
Flat	−0.16	−0.14	−0.14	−0.16	−0.14	−0.14
	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
	[−0.27; −0.05]	[−0.25; −0.04]	[−0.25; −0.04]	[−0.26; −0.05]	[−0.25; −0.03]	[−0.25; −0.03]
Accuracy	0.07	0.08	0.08	0.07	0.09	0.09
	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
	[−0.00; 0.14]	[0.02; 0.15]	[0.02; 0.15]	[0.00; 0.15]	[0.02; 0.16]	[0.02; 0.16]
Age		−0.04	−0.04		−0.04	−0.04
		(0.03)	(0.03)		(0.03)	(0.03)
		[−0.10; 0.02]	[−0.10; 0.02]		[−0.10; 0.01]	[−0.10; 0.01]
Female?		0.04	0.04		0.04	0.04
		(0.04)	(0.04)		(0.04)	(0.04)
		[−0.03; 0.11]	[−0.03; 0.11]		[−0.04; 0.11]	[−0.04; 0.11]
US resident?		−0.03	−0.03		−0.02	−0.02
		(0.07)	(0.07)		(0.07)	(0.07)
		[−0.17; 0.12]	[−0.17; 0.12]		[−0.17; 0.12]	[−0.17; 0.12]
|Prior-50| (Flat)			0.01			0.01
			(0.01)			(0.01)
			[−0.02; 0.03]			[−0.02; 0.03]
|Prior-50| (Accuracy)			−0.01			−0.01
			(0.01)			(0.02)
			[−0.04; 0.02]			[−0.04; 0.02]
|Prior-50| (Peer Betting)			−0.03			−0.03
			(0.02)			(0.02)
			[−0.06; 0.00]			[−0.06; 0.00]
Task order (Flat)			0.01			0.01
			(0.02)			(0.02)
			[−0.03; 0.04]			[−0.03; 0.04]
Task order (Accuracy)			0.04			0.04
			(0.03)			(0.03)
			[−0.03; 0.10]			[−0.03; 0.10]
Task order (Peer Betting)			0.05			0.05
			(0.03)			(0.03)
			[−0.01; 0.11]			[−0.01; 0.11]
Num. obs.	2100	2070	2070	2060	2030	2030
Likl. Ratio.	148.93	175.79	179.37	146.39	173.35	176.94
LR test p-val	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001	< 0.0001
AIC	1649.70	1549.38	1557.80	1638.88	1539.16	1547.57

Table D4: Marginal effects, logistic regression (baseline category: Peer Betting)

D.2 Study 2

D.2.1 Additional figures and tables

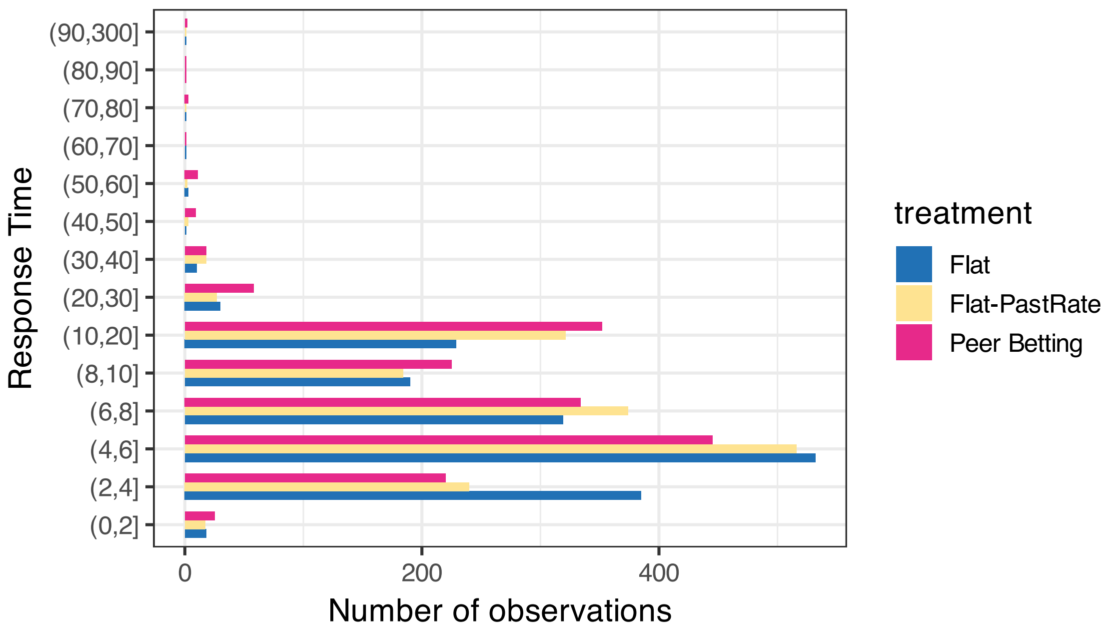


Figure D2: Response times

	week	version	cond.	resp. time	response		week	version	cond.	resp. time	response
1	1	"once"	Flat	71.074	"False"	10	2	"once"	Flat	67.074	"True"
2	1	"once"	Peer Betting	78.342	"True"	11	2	"twice"	Flat-PR	73.208	"False"
3	1	"once"	Peer Betting	80.594	"False"	12	2	"twice"	Peer Betting	70.845	"True"
4	1	"once"	Peer Betting	74.812	"False"						
5	1	"once"	Peer Betting	65.680	"True"						
6	1	"twice"	Flat	287.396	"False"						
7	1	"twice"	Flat-PR	99.080	"True"						
8	1	"twice"	Peer Betting	185.663	"False"						
9	1	"twice"	Peer Betting	104.542	"True"						

Table D5: Study 2, outlier responses based on response time > 60 seconds

	P(response = 'true'), Logit estimates					
	(week 1)			(week 2)		
	(filtered sample)		(all)	(filtered sample)		(all)
	(1)	(2)	(3)	(4)	(5)	(6)
(Intercept)	−0.74	−1.52	−1.54	−0.71	−1.77	−1.80
	(0.10)	(0.49)	(0.49)	(0.11)	(0.44)	(0.44)
	[−0.94; −0.54]	[−2.49; −0.56]	[−2.51; −0.57]	[−0.92; −0.50]	[−2.63; −0.91]	[−2.66; −0.94]
Flat-PastRate	0.22	0.26	0.27	−0.02	0.00	−0.02
	(0.16)	(0.22)	(0.22)	(0.16)	(0.21)	(0.22)
	[−0.10; 0.53]	[−0.18; 0.69]	[−0.17; 0.70]	[−0.33; 0.28]	[−0.41; 0.42]	[−0.45; 0.40]
Peer Betting	0.46	0.58	0.60	0.34	0.52	0.50
	(0.13)	(0.18)	(0.18)	(0.16)	(0.22)	(0.22)
	[0.21; 0.71]	[0.22; 0.94]	[0.24; 0.96]	[0.03; 0.64]	[0.09; 0.96]	[0.07; 0.94]
Respone Time		0.02	0.03		−0.06	0.02
		(0.11)	(0.11)		(0.14)	(0.16)
		[−0.20; 0.25]	[−0.18; 0.24]		[−0.34; 0.22]	[−0.29; 0.32]
Age		−0.26	−0.27		−0.13	−0.13
		(0.16)	(0.16)		(0.10)	(0.10)
		[−0.58; 0.05]	[−0.59; 0.05]		[−0.33; 0.07]	[−0.33; 0.07]
Female?		0.12	0.12		−0.12	−0.14
		(0.18)	(0.18)		(0.19)	(0.19)
		[−0.24; 0.48]	[−0.23; 0.47]		[−0.49; 0.25]	[−0.51; 0.24]
UK citizen?		−0.03	−0.01		0.19	0.21
		(0.18)	(0.18)		(0.22)	(0.22)
		[−0.38; 0.33]	[−0.36; 0.34]		[−0.25; 0.63]	[−0.23; 0.65]
Question 2		2.77	2.77		2.89	2.88
		(0.29)	(0.29)		(0.27)	(0.27)
		[2.20; 3.35]	[2.19; 3.35]		[2.37; 3.42]	[2.36; 3.40]
Question 3		1.40	1.40		1.19	1.17
		(0.28)	(0.28)		(0.25)	(0.25)
		[0.84; 1.96]	[0.84; 1.96]		[0.70; 1.69]	[0.68; 1.66]
Question 4		0.15	0.14		0.21	0.20
		(0.31)	(0.31)		(0.28)	(0.28)
		[−0.45; 0.75]	[−0.46; 0.74]		[−0.35; 0.76]	[−0.36; 0.75]
Question 5		2.51	2.49		2.40	2.38
		(0.30)	(0.30)		(0.28)	(0.28)
		[1.92; 3.10]	[1.91; 3.07]		[1.85; 2.95]	[1.83; 2.93]
Question 6		0.32	0.32		−0.09	−0.06
		(0.31)	(0.31)		(0.30)	(0.29)
		[−0.29; 0.92]	[−0.29; 0.92]		[−0.67; 0.49]	[−0.63; 0.51]
Question 7		2.49	2.50		2.51	2.49
		(0.28)	(0.28)		(0.28)	(0.28)
		[1.94; 3.03]	[1.95; 3.04]		[1.96; 3.05]	[1.95; 3.03]
Question 8		−1.29	−1.18		−0.88	−0.88
		(0.45)	(0.43)		(0.39)	(0.39)
		[−2.18; −0.41]	[−2.02; −0.34]		[−1.65; −0.10]	[−1.65; −0.11]
Num. obs.	1259	1259	1264	1279	1279	1280
Likl. Ratio.	10.44	402.56	401.01	8.03	403.32	401.05
LR test p-val	0.0054	< .0001	< .0001	0.0180	< .0001	< .0001
AIC	1662.27	1292.15	1300.44	1660.66	1287.37	1291.72

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{+}p < 0.1$

Table D6: Logistic regression estimates

D.2.2 Analyses on the 'at least twice' survey data

	P(response = 'true'), Logit estimates					
	(week 1)			*(week 2)*		
	(filtered sample)		*(all)*	*(filtered sample)*		*(all)*
	(1)	(2)	(3)	(4)	(5)	(6)
(Intercept)	−1.37	−2.69	−2.66	−1.07	−1.33	−1.30
	(0.12)	(0.45)	(0.43)	(0.13)	(0.49)	(0.48)
	[−1.62; −1.13]	[−3.57; −1.81]	[−3.50; −1.82]	[−1.33; −0.82]	[−2.29; −0.37]	[−2.24; −0.35]
Flat-PastRate	0.29	0.38	0.40	0.17	0.22	0.22
	(0.17)	(0.22)	(0.21)	(0.18)	(0.23)	(0.23)
	[−0.03; 0.62]	[−0.04; 0.80]	[−0.01; 0.81]	[−0.19; 0.53]	[−0.23; 0.67]	[−0.24; 0.67]
Peer Betting	0.13	0.29	0.30	0.16	0.20	0.21
	(0.18)	(0.22)	(0.21)	(0.17)	(0.21)	(0.21)
	[−0.22; 0.47]	[−0.13; 0.72]	[−0.12; 0.72]	[−0.18; 0.49]	[−0.21; 0.62]	[−0.21; 0.62]
Respone Time		−0.01	0.02		0.19	0.19
		(0.13)	(0.05)		(0.12)	(0.10)
		[−0.26; 0.25]	[−0.08; 0.12]		[−0.05; 0.42]	[−0.02; 0.39]
Age		−0.30	−0.30		−0.24	−0.25
		(0.12)	(0.12)		(0.12)	(0.12)
		[−0.54; −0.06]	[−0.54; −0.06]		[−0.48; 0.00]	[−0.49; −0.01]
Female?		0.00	0.01		−0.11	−0.12
		(0.18)	(0.17)		(0.19)	(0.19)
		[−0.34; 0.35]	[−0.33; 0.35]		[−0.49; 0.27]	[−0.50; 0.26]
UK citizen?		0.57	0.59		−0.20	−0.20
		(0.23)	(0.23)		(0.25)	(0.25)
		[0.11; 1.02]	[0.14; 1.04]		[−0.70; 0.29]	[−0.70; 0.29]
Question 2		3.19	3.10		2.51	2.51
		(0.36)	(0.35)		(0.29)	(0.28)
		[2.49; 3.90]	[2.41; 3.79]		[1.95; 3.07]	[1.95; 3.07]
Question 3		1.46	1.38		0.88	0.88
		(0.35)	(0.33)		(0.28)	(0.28)
		[0.78; 2.14]	[0.73; 2.03]		[0.34; 1.43]	[0.34; 1.43]
Question 4		−0.55	−0.64		−0.28	−0.28
		(0.51)	(0.51)		(0.34)	(0.34)
		[−1.56; 0.46]	[−1.64; 0.35]		[−0.95; 0.39]	[−0.95; 0.39]
Question 5		2.01	1.90		1.35	1.36
		(0.38)	(0.37)		(0.27)	(0.27)
		[1.25; 2.76]	[1.17; 2.62]		[0.82; 1.89]	[0.82; 1.89]
Question 6		0.64	0.54		−0.09	−0.09
		(0.42)	(0.41)		(0.31)	(0.31)
		[−0.18; 1.46]	[−0.26; 1.34]		[−0.71; 0.52]	[−0.71; 0.52]
Question 7		2.41	2.32		1.90	1.90
		(0.36)	(0.35)		(0.26)	(0.26)
		[1.71; 3.12]	[1.63; 3.00]		[1.38; 2.41]	[1.39; 2.41]
Question 8		−0.97	−1.06		−1.38	−1.38
		(0.62)	(0.61)		(0.48)	(0.48)
		[−2.18; 0.24]	[−2.26; 0.13]		[−2.32; −0.45]	[−2.32; −0.45]
Num. obs.	1284	1276	1280	1294	1286	1288
Likl. Ratio.	3.24	309.88	308.09	1.49	291.56	292.69
LR test p-val	0.1983	< .0001	< .0001	0.4759	< .0001	< .0001
AIC	1374.64	1083.44	1092.19	1528.92	1253.75	1255.83

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{+}p < 0.1$

Table D7: Logistic regression estimates, 'at least twice' survey

	P(response = 'true'), marginal effects					
	|	*(week 1)*	|	|	*(week 2)*	|
	(filtered sample)		*(all)*	*(filtered sample)*		*(all)*
	(1)	(2)	(3)	(4)	(5)	(6)
Flat-PastRate	0.05	0.05	0.05	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
	[−0.01; 0.11]	[−0.01; 0.11]	[−0.00; 0.11]	[−0.04; 0.10]	[−0.04; 0.10]	[−0.04; 0.10]
Peer Betting	0.02	0.04	0.04	0.03	0.03	0.03
	[−0.04; 0.08]	[−0.02; 0.09]	[−0.02; 0.09]	[−0.04; 0.10]	[−0.03; 0.10]	[−0.03; 0.10]
Response Time		−0.00	0.00		0.03	0.03
		[−0.04; 0.03]	[−0.01; 0.02]		[−0.01; 0.07]	[−0.00; 0.06]
Age		−0.04	−0.04		−0.04	−0.04
		(0.02)	(0.02)		(0.02)	(0.02)
		[−0.07; −0.01]	[−0.07; −0.01]		[−0.07; −0.00]	[−0.08; −0.00]
Female?		0.00	0.00		−0.02	−0.02
		(0.02)	(0.02)		(0.03)	(0.03)
		[−0.05; 0.05]	[−0.04; 0.05]		[−0.08; 0.04]	[−0.08; 0.04]
UK citizen?		0.08	0.08		−0.03	−0.03
		(0.03)	(0.03)		(0.04)	(0.04)
		[0.02; 0.14]	[0.02; 0.14]		[−0.11; 0.05]	[−0.11; 0.05]
Question FE		✓	✓		✓	✓
Num. obs.	1284	1276	1280	1294	1286	1288
Likl. Ratio.	3.24	309.88	308.09	1.49	291.56	292.69
LR test p-val	0.1983	< .0001	< .0001	0.4759	< .0001	< .0001
AIC	1374.64	1083.44	1092.19	1528.92	1253.75	1255.83

Table D8: Logistic regression, average marginal effects, 'at least twice' survey

	OLS, Dep.Var.: Response time					
	(week 1)			(week 2)		
	(filtered sample)		(all)	(filtered sample)		(all)
	(1)	(2)	(3)	(4)	(5)	(6)
(Intercept)	6.38	5.24	5.58	6.82	6.33	6.43
	(0.27)	(1.15)	(1.30)	(0.46)	(0.97)	(0.99)
	[5.85; 6.91]	[2.97; 7.52]	[3.02; 8.14]	[5.92; 7.73]	[4.42; 8.25]	[4.47; 8.38]
Flat-PastRate	0.87	0.84	0.63	1.60	1.56	1.57
	(0.57)	(0.58)	(0.60)	(0.66)	(0.63)	(0.63)
	[−0.25; 1.99]	[−0.30; 1.99]	[−0.55; 1.81]	[0.29; 2.91]	[0.31; 2.81]	[0.32; 2.82]
Peer Betting	2.64	2.71	3.06	1.14	1.03	1.04
	(0.66)	(0.65)	(0.81)	(0.69)	(0.69)	(0.69)
	[1.33; 3.95]	[1.42; 4.00]	[1.45; 4.66]	[−0.22; 2.50]	[−0.33; 2.39]	[−0.32; 2.40]
Response	1.14	0.75	0.65	0.39	−0.26	0.26
	(0.52)	(0.56)	(0.65)	(0.53)	(0.62)	(0.88)
	[0.11; 2.17]	[−0.36; 1.85]	[−0.64; 1.93]	[−0.65; 1.43]	[−1.49; 0.97]	[−1.47; 2.00]
Flat-PastRate x Response	−0.84	−0.99	−0.83	0.19	0.24	−0.18
	(0.74)	(0.74)	(0.76)	(0.87)	(0.88)	(1.01)
	[−2.30; 0.62]	[−2.45; 0.47]	[−2.33; 0.67]	[−1.53; 1.92]	[−1.49; 1.97]	[−2.17; 1.82]
Peer Betting x Response	−0.91	−1.05	−0.76	−0.07	−0.06	−0.46
	(0.81)	(0.80)	(0.96)	(0.83)	(0.84)	(0.98)
	[−2.51; 0.69]	[−2.62; 0.52]	[−2.66; 1.14]	[−1.72; 1.57]	[−1.71; 1.59]	[−2.39; 1.46]
Age		−0.07	−0.18		0.02	−0.02
		(0.39)	(0.43)		(0.24)	(0.24)
		[−0.85; 0.71]	[−1.03; 0.67]		[−0.45; 0.48]	[−0.49; 0.46]
Female?		0.26	0.01		0.40	0.29
		(0.50)	(0.57)		(0.51)	(0.53)
		[−0.73; 1.26]	[−1.11; 1.14]		[−0.60; 1.41]	[−0.77; 1.34]
UK citizen?		−0.81	−0.76		−1.64	−1.61
		(0.52)	(0.54)		(0.64)	(0.65)
		[−1.83; 0.21]	[−1.82; 0.30]		[−2.89; −0.38]	[−2.89; −0.34]
Question 2		1.36	1.32		1.82	1.68
		(0.61)	(0.65)		(0.65)	(0.66)
		[0.16; 2.57]	[0.04; 2.60]		[0.53; 3.11]	[0.38; 2.98]
Question 3		2.94	2.93		2.46	2.41
		(0.62)	(0.62)		(0.50)	(0.50)
		[1.73; 4.16]	[1.70; 4.16]		[1.47; 3.46]	[1.42; 3.41]
Question 4		2.33	2.74		1.64	1.64
		(0.67)	(0.80)		(0.54)	(0.54)
		[1.00; 3.66]	[1.16; 4.32]		[0.58; 2.71]	[0.58; 2.70]
Question 5		3.44	3.79		3.12	3.00
		(0.65)	(0.81)		(0.67)	(0.68)
		[2.15; 4.73]	[2.18; 5.40]		[1.80; 4.43]	[1.65; 4.34]
Question 6		2.16	2.16		2.55	2.93
		(0.64)	(0.64)		(0.56)	(0.67)
		[0.91; 3.42]	[0.91; 3.42]		[1.44; 3.66]	[1.61; 4.24]
Question 7		1.98	2.39		2.61	2.48
		(0.56)	(0.71)		(0.73)	(0.74)
		[0.87; 3.09]	[0.98; 3.80]		[1.17; 4.05]	[1.02; 3.94]
Question 8		1.04	1.86		0.45	0.47
		(0.57)	(0.85)		(0.44)	(0.44)
		[−0.10; 2.17]	[0.18; 3.54]		[−0.41; 1.31]	[−0.39; 1.33]
R^2	0.03	0.06	0.05	0.02	0.06	0.05
Adj. R^2	0.03	0.05	0.04	0.01	0.05	0.04
Num. obs.	1259	1259	1264	1279	1279	1280
RMSE	5.89	5.82	7.13	5.82	5.72	5.95

Table D9: Response time regressions, ‘at least once’ survey

	OLS, Dep.Var.: Response time					
	(week 1)			*(week 2)*		
	(filtered sample)		*(all)*	*(filtered sample)*		*(all)*
	(1)	(2)	(3)	(4)	(5)	(6)
(Intercept)	6.68	7.02	8.91	7.39	5.20	4.70
	(0.38)	(1.00)	(1.63)	(0.42)	(1.31)	(1.38)
	[5.94; 7.42]	[5.05; 8.99]	[5.70; 12.12]	[6.56; 8.22]	[2.61; 7.79]	[1.96; 7.43]
Flat-PastRate	0.97	1.24	0.21	0.34	0.41	0.63
	(0.60)	(0.56)	(1.06)	(0.56)	(0.58)	(0.60)
	[−0.21; 2.15]	[0.13; 2.36]	[−1.88; 2.31]	[−0.76; 1.44]	[−0.73; 1.56]	[−0.55; 1.81]
Peer Betting	2.49	2.56	2.31	0.58	0.70	0.69
	(0.71)	(0.71)	(1.14)	(0.57)	(0.56)	(0.56)
	[1.09; 3.89]	[1.16; 3.97]	[0.06; 4.55]	[−0.54; 1.70]	[−0.41; 1.81]	[−0.43; 1.80]
Response	0.40	0.05	−0.70	1.64	1.08	1.13
	(0.63)	(0.70)	(1.13)	(0.73)	(0.76)	(0.75)
	[−0.84; 1.64]	[−1.34; 1.43]	[−2.92; 1.53]	[0.20; 3.09]	[−0.42; 2.57]	[−0.34; 2.61]
Flat-PastRate x Response	−0.19	−0.30	1.40	−1.78	−1.51	−1.73
	(0.88)	(0.87)	(1.39)	(0.89)	(0.90)	(0.91)
	[−1.92; 1.53]	[−2.03; 1.43]	[−1.35; 4.14]	[−3.55; −0.02]	[−3.30; 0.27]	[−3.52; 0.06]
Peer Betting x Response	0.26	−0.03	1.13	0.37	0.38	0.87
	(0.94)	(0.96)	(1.87)	(1.10)	(1.11)	(1.17)
	[−1.59; 2.11]	[−1.93; 1.86]	[−2.56; 4.82]	[−1.80; 2.53]	[−1.80; 2.57]	[−1.45; 3.18]
Age		−0.68	−0.80		0.55	0.70
		(0.26)	(0.40)		(0.35)	(0.38)
		[−1.19; −0.16]	[−1.59; −0.01]		[−0.15; 1.25]	[−0.06; 1.45]
Female?		0.83	−0.20		−0.42	−0.52
		(0.55)	(0.96)		(0.51)	(0.54)
		[−0.26; 1.92]	[−2.10; 1.71]		[−1.43; 0.59]	[−1.58; 0.54]
UK citizen?		−1.65	−1.67		−1.00	−0.85
		(0.72)	(0.98)		(0.78)	(0.78)
		[−3.08; −0.23]	[−3.61; 0.28]		[−2.54; 0.54]	[−2.40; 0.69]
Question 2		2.01	1.31		2.06	1.95
		(0.61)	(1.00)		(0.50)	(0.52)
		[0.81; 3.22]	[−0.66; 3.27]		[1.07; 3.05]	[0.91; 2.98]
Question 3		2.68	4.41		3.06	3.79
		(0.62)	(2.03)		(0.59)	(0.80)
		[1.45; 3.91]	[0.41; 8.41]		[1.90; 4.22]	[2.22; 5.36]
Question 4		2.14	1.58		1.95	1.95
		(0.54)	(0.79)		(0.53)	(0.53)
		[1.07; 3.21]	[0.01; 3.15]		[0.90; 3.01]	[0.90; 3.01]
Question 5		3.58	4.01		3.11	3.07
		(0.63)	(1.46)		(0.60)	(0.60)
		[2.32; 4.83]	[1.14; 6.89]		[1.93; 4.30]	[1.89; 4.25]
Question 6		2.30	1.74		1.92	1.91
		(0.59)	(0.85)		(0.49)	(0.49)
		[1.14; 3.46]	[0.06; 3.42]		[0.96; 2.88]	[0.95; 2.87]
Question 7		2.67	2.02		2.81	2.73
		(0.51)	(0.85)		(0.52)	(0.53)
		[1.67; 3.67]	[0.34; 3.69]		[1.78; 3.85]	[1.68; 3.78]
Question 8		1.32	0.77		1.42	1.42
		(0.52)	(0.72)		(0.41)	(0.41)
R^2	0.03	0.07	0.03	0.02	0.05	0.06
Adj. R^2	0.03	0.06	0.02	0.01	0.04	0.05
Num. obs.	1284	1276	1280	1294	1286	1288
RMSE	6.06	5.96	11.60	5.84	5.77	6.25

Table D10: Response time regressions, ‘at least twice’ survey

Replication material

Complete instructions

Study 1

1144

Instructions - Peer Betting

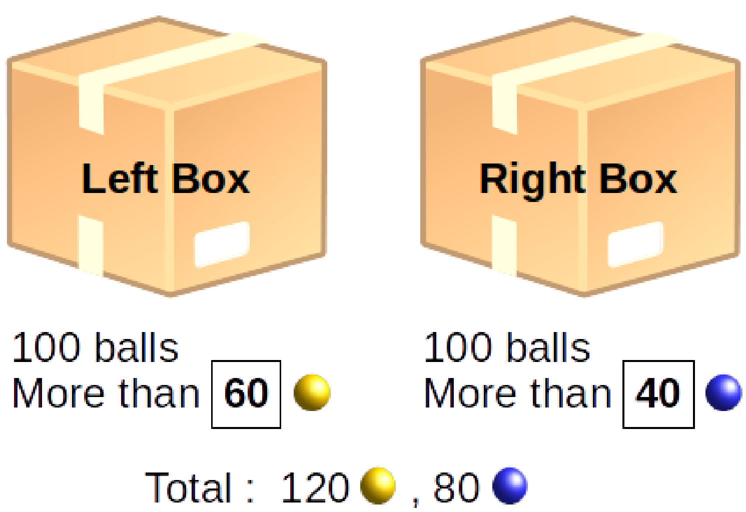
Instructions

(page 1 out of 5)

In this experiment, you will answer 10 questions in total.

In each question, there are two new boxes, which contain yellow ( ) and blue ( ) balls in different proportions.

A picture like the one below will give you information on the boxes:



Numbers may change in each question. But, following is always true:

...Left box always contains more than half of all

...Right box always contains more than half of all
...Both boxes always contain 100 balls each.

1145 In the example above, if left box contains 68 and 32 , right box contains 52 and 48

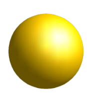
Instructions

(page 2 out of 5)

In each question, one of the boxes is the 'actual box'.

The actual box is predetermined by an unbiased coin flip. It is same for all participants, including you.

A ball will be drawn randomly from the actual box for you. Following is an example draw:



Note that...
...if you draw , Left box is more likely.
...if you draw , Right box is more likely.

The color of your draw helps you guess the actual box.

Instructions

To see the color of your draw, you need to complete an **effort**

task.

You will first see the following question:

Would you like to work on the effort task?

Yes | No

If you select 'Yes', you will be presented a table as below:

0	0	0	1	0	0
0	0	0	0	1	1
1	0	0	1	1	0
1	0	1	0	1	0

Your task is to count the number of 0s.

There is no time limit. You can try multiple times.

Once you submit the correct answer, you observe your draw.

You may skip the effort task by selecting 'No'. Then, you will not see the color of your draw.

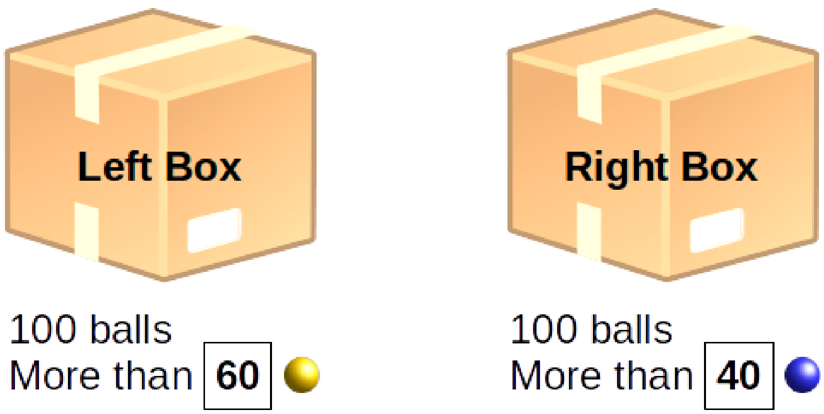
Instructions

(page 4 out of 5)

1147

Finally, you will pick one of the boxes. The question will appear as below:

Which box do you pick?



You may click on...

Left box if you pick Left box

Right box if you pick Right box

Your pick will be submitted when you click Submit

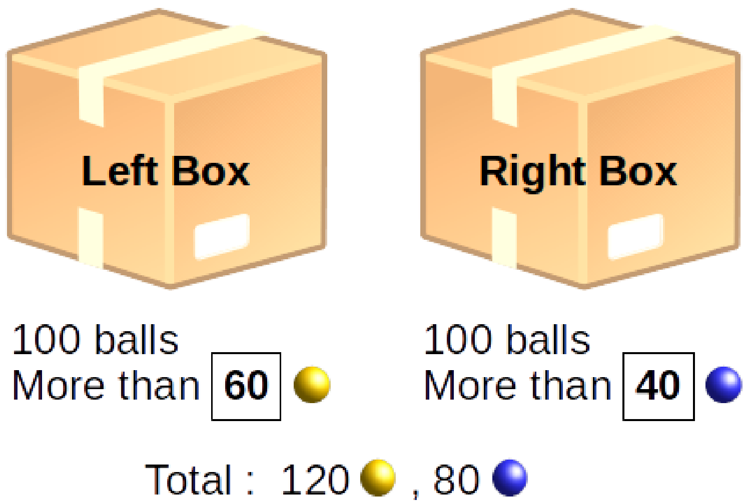
Instructions

(page 5 out of 5)

You will earn £2 bonus, on top of £1.25, for completing the experiment.

In addition, you may earn bonus from each question.

Let's see how it works with the example boxes:
1148



There will be at least 50 other participants in the experiment.

After the experiment, we calculate the percentage of participants other than you who pick each box.

We compare those percentages to the numbers in ☐.

Suppose 79% picked Left, 21% picked Right. Then,...
...you win 79 - 60 = 19p if you picked Left
...you lose 40 - 21 = 19p if you picked Right

So, **you win money if you pick the box that others will pick more often than indicated in** ☐.

The color of your draw helps you guess others' draws, which may affect their picks.

The maximum total gain from your picks is +£2 and the maximum total loss is -£2.

So, your total reward at the end of the experiment is between £1.25 and £5.25.

1149

NEW YORK UNIVERSITY

1150

Instructions - Flat

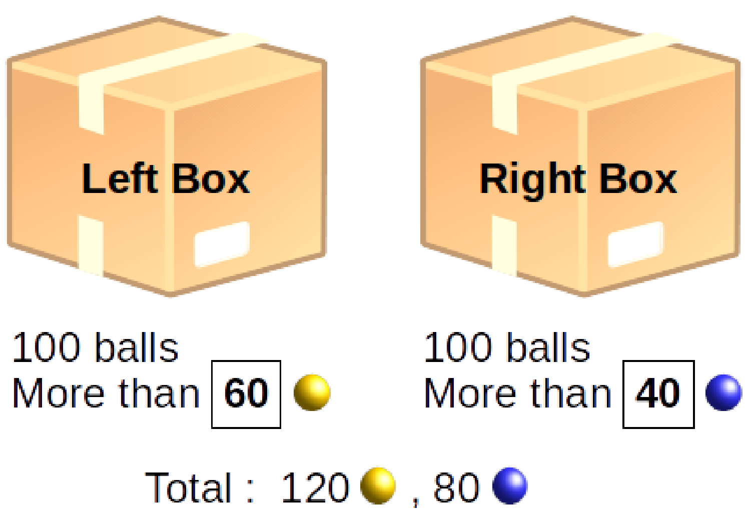
Instructions

(page 1 out of 5)

In this experiment, you will answer 10 questions in total.

In each question, there are two new boxes, which contain yellow ( ) and blue ( ) balls in different proportions.

A picture like the one below will give you information on the boxes:



Numbers may change in each question. But, following is always true:

...Left box always contains more than half of all

...Right box always contains more than half of all 
...Both boxes always contain 100 balls each.

1151 In the example above, if left box contains 68  and 32 , right box contains 52  and 48 

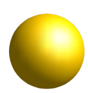
Instructions

(page 2 out of 5)

In each question, one of the boxes is the 'actual box'

The actual box is predetermined by an unbiased coin flip. It is same for all participants, including you.

A ball will be drawn randomly from the actual box for you. Following is an example draw:



Note that...
...if you draw , Left box is more likely.
...if you draw , Right box is more likely.

The color of your draw helps you guess the actual box.

Instructions

To see the color of your draw, you need to complete an **effort** 1152 **task**.

You will first see the following question:

Would you like to work on the effort task?

Yes | No

If you select 'Yes', you will be presented a table as below:

0	0	0	1	0	0
0	0	0	0	1	1
1	0	0	1	1	0
1	0	1	0	1	0

Your task is to count the number of 0s.

There is no time limit. You can try multiple times.

Once you submit the correct answer, you observe your draw.

You may skip the effort task by selecting 'No'. Then, you will not see the color of your draw.

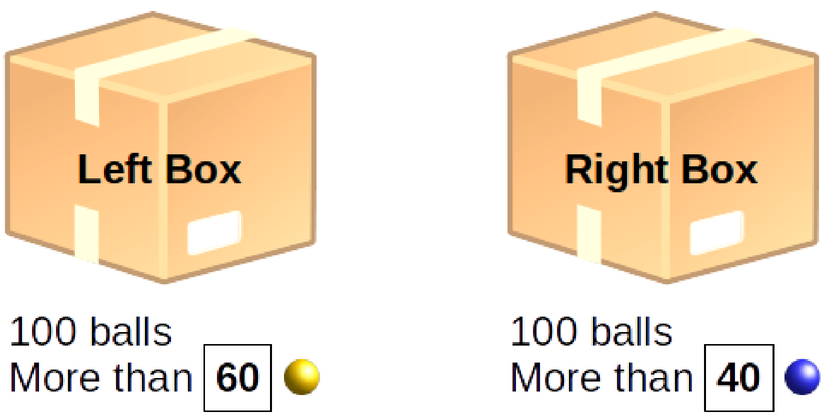
Instructions

(page 4 out of 5)

1153

Finally, you will pick one of the boxes. The question will appear as below:

Which box do you pick?



You may click on...

Left box if you pick Left box

Right box if you pick Right box

Your pick will be submitted when you click Submit

Instructions

(page 5 out of 5)

You will earn a fixed £2 bonus, on top of £1.25, for completing the experiment.

Your total reward will be £3.25.

1154

1155

Instructions - Accuracy

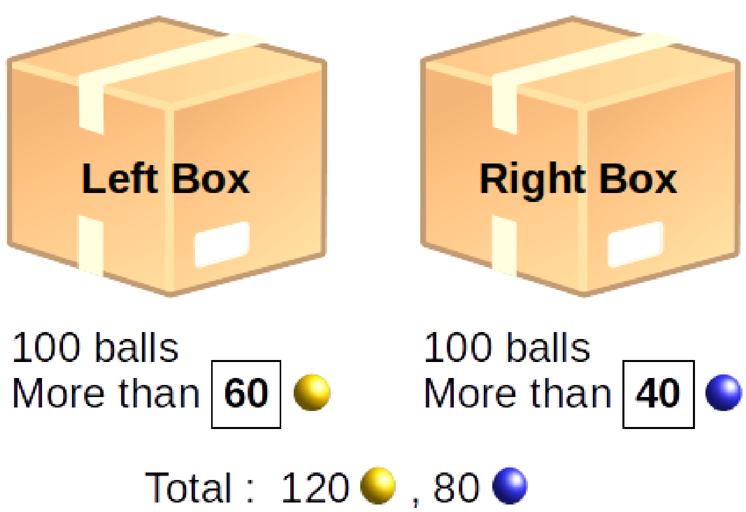
Instructions

(page 1 out of 5)

In this experiment, you will answer 10 questions in total.

In each question, there are two new boxes, which contain yellow ( ) and blue ( ) balls in different proportions.

A picture like the one below will give you information on the boxes:



Numbers may change in each question. But, following is always true:

...Left box always contains more than half of all

...Right box always contains more than half of all 
...Both boxes always contain 100 balls each.

1156
In the example above, if left box contains 68  and 32 , right box contains 52  and 48 

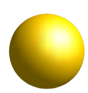
Instructions

(page 2 out of 5)

In each question, one of the boxes is the 'actual box'

The actual box is predetermined by an unbiased coin flip. It is same for all participants, including you.

A ball will be drawn randomly from the actual box for you. Following is an example draw:



Note that...
...if you draw , Left box is more likely.
...if you draw , Right box is more likely.

The color of your draw helps you guess the actual box.

Instructions

To see the color of your draw, you need to complete an **effort**
1157
task.

You will first see the following question:

Would you like to work on the effort task?

Yes	No

If you select 'Yes', you will be presented a table as below:

0	0	0	1	0	0
0	0	0	0	1	1
1	0	0	1	1	0
1	0	1	0	1	0

Your task is to count the number of 0s.

There is no time limit. You can try multiple times.

Once you submit the correct answer, you observe your draw.

You may skip the effort task by selecting 'No'. Then, you will not see the color of your draw.

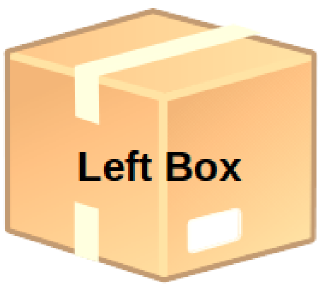
Instructions

(page 4 out of 5)

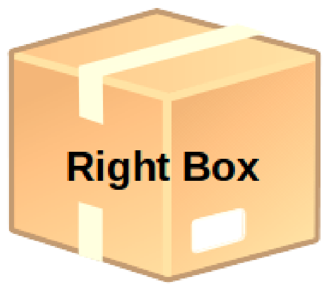
1158

Finally, you will pick one of the boxes. The question will appear as below:

Which box do you pick?



100 balls
More than 60



100 balls
More than 40

You may click on...

Left box if you pick Left box

Right box if you pick Right box

Your pick will be submitted when you click Submit

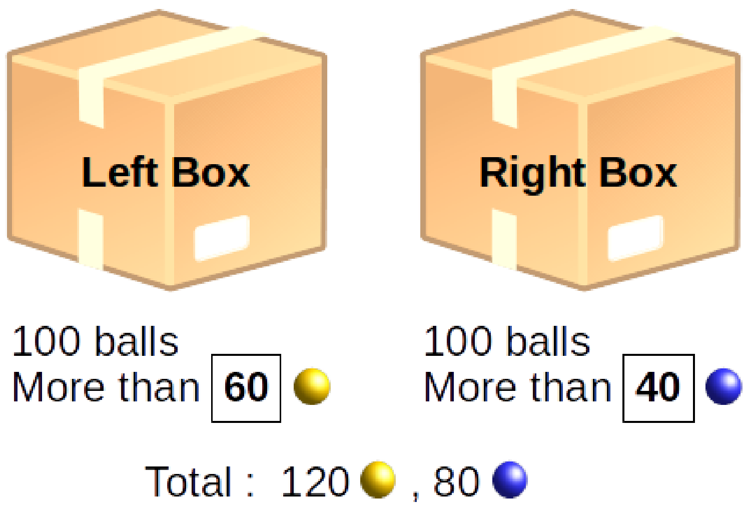
Instructions

(page 5 out of 5)

You earn £2 bonus, on top of £1.25, for completing the experiment.

In addition, you earn a bonus from each question if you guess the actual box accurately.

1159 Let's see how it works with the example boxes:



Suppose Left is the actual box. Then,...
...you **win 20p** if you picked Left.
...you **lose 20p** if you picked Right.

Suppose instead Right is the actual box. Then,...
...you **lose 20p** if you picked Left.
...you **win 20p** if you picked Right.

The maximum total gain from your picks is +£2 and the maximum total loss is -£2.

So, your total reward at the end of the experiment is between £1.25 and £5.25.

Quiz for attention check

Quiz question is the same in all experimental conditions and provided below. The order of choices is randomized.

Quiz

Here's a small quiz on rewards!

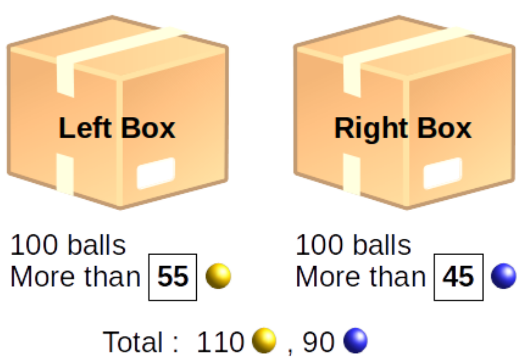
Which of the three statements is most accurate?

- My bonus is fixed, regardless of the box I pick.
- My bonus depends on the actual box and the box I pick.
- My bonus depends on the box I pick and what other participants pick.

Participants receive feedback according to their answer. In the PPM condition, the correct answer is "My bonus depends on the box I pick and what other participants." If the correct answer is reported, the following is displayed:

TRUE! Your bonus depends on the box you picked and what other participants picked.

Here's an example. Suppose you have the following pair of boxes:



Suppose, of all other participants, 65% picked Right, 35% picked Left

Let's say your draw was and you picked Right.

Then, you win **65**-45 = 20p.

If you had picked Left instead, you would have lost 55-**35** = 20p.

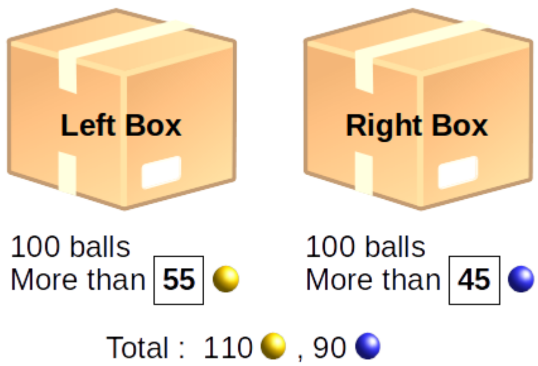
So, your reward depends on your pick AND other participants' picks.

The color of your draw helps you guess others' draws, which may affect their picks.

If a participant picks one of the wrong answers, the following is displayed:

1161 **FALSE!** Your bonus depends on the box you picked and what other participants picked.

Here's an example. Suppose you have the following pair of boxes:



Suppose, of all other participants, 65% picked Right, 35% picked Left

Let's say your draw was [blue ball] and you picked Right.

Then, you win **65**-45 = 20p.

If you had picked Left instead, you would have lost 55-**35** = 20p.

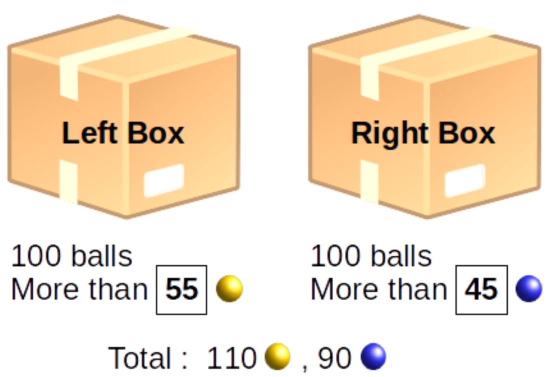
So, your reward depends on your pick AND other participants' picks.

The color of your draw helps you guess others' draws, which may affect their picks.

In the Flat condition, the correct answer is "My bonus is fixed, regardless of the box I pick." If the correct answer is reported, the following is displayed:

TRUE! Your bonus is fixed, regardless of the box you pick.

Here's an example. Suppose you have the following pair of boxes:



It does not matter if your pick is the actual box or not.

Other participants' picks are also irrelevant.

You will earn £2 bonus for completing the experiment. Your total reward will be £3.25.

There is no bonus for working on the effort tasks.

1162

If a participant picks one of the wrong answers, the following is displayed:

FALSE! Your bonus is fixed, regardless of the box you pick.

Here's an example. Suppose you have the following pair of boxes:

100 balls
More than **55**

100 balls
More than **45**

Total : 110 , 90

It does not matter if your pick is the actual box or not.

Other participants' picks are also irrelevant.

You will earn £2 bonus for completing the experiment. Your total reward will be £3.25.

There is no bonus for working on the effort tasks.

In the Accuracy condition, the correct answer is "My bonus depends on the actual box and the box I picked." If the correct answer is reported, the following is displayed:

TRUE! Your bonus depends on the actual box and the box you picked.

Here's an example. Suppose you have the following pair of boxes:

100 balls
More than **55**

100 balls
More than **45**

Total : 110 , 90

Suppose Right box is the actual box.

Let's say your draw was and you picked Right.

Then, you win 20p because you guessed the actual box accurately.

If you had picked Left instead, you would have lost 20p.

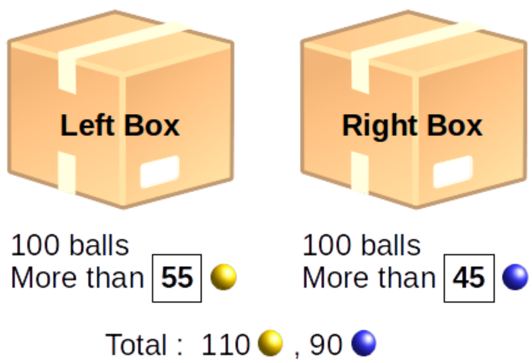
So, your reward depends on your accuracy only.

The color of your draw helps you make an accurate guess.

If a participant picks one of the wrong answers, the following is displayed:

FALSE! Your bonus depends on the actual box and the box you picked.

Here's an example. Suppose you have the following pair of boxes:



Suppose Right box is the actual box.

Let's say your draw was and you picked Right.

Then, you win 20p because you guessed the actual box accurately.

If you picked Left instead, you would have lost 20p.

So, your reward depends on your accuracy only.

The color of your draw helps you make an accurate guess.

Closing Survey

1164 ### Thank you for your answers!

To conclude, we would like you to answer some questions about your personal background and your experience in this experiment

How old are you?

What is your gender?

- Male.
- Female.
- Other / Prefer not to disclose.

What is your education level?

Did you receive a training in statistics? If yes, on which level?

When did you receive this training?

How clear were the instructions in this experiment?

Very clear.	Mostly clear.	Understandable, but not very clear.	Mostly unclear.	Very unclear.

1165 **Which of the three statements is most accurate?**

- My bonus depends on the actual boxes and the boxes I picked.
- My bonus depends on the boxes I picked and what other participants picked.
- My bonus is fixed, regardless of the boxes I picked.

Do you have any other comments or suggestions?

Click Finish to complete the experiment. You will be redirected to Prolific.

Finish

Study 2

NEW YORK UNIVERSITY

1167

Instructions - Peer Betting

Instructions

(page 1 out of 5)

Welcome! In this survey, you will answer 8 questions on the COVID-19 pandemic.

The UK government issues COVID-19 guidance and passes regulations to control the pandemic.

This survey aims to collect data on people's behaviour to assess whether such guidelines are helpful.

In each question, we will ask you about your experience for certain situations related to the pandemic.

Instructions

(page 2 out of 5)

Here's an example on how questions will appear:

I may have stood less than 2 metres away from the person in front in a queue at least once in the last 7 days.

1168

True ○　　　False ○

You may pick True or False depending on whether you have been in the situation described in the question.

Your pick will be submitted when you click Submit

Instructions

(page 3 out of 5)

We ask the same questions every 7 days to a new group of at least 50 participants.

All participants are students who currently reside in the UK. The survey can be taken only once.

In all questions, you will see the percentage of people who picked each answer in the last survey, 7 days ago.

For example, if 65% of participants picked True and 35% picked False, the choices will appear as follows:

True
(picked by 65% last week)

False
(picked by 35% last week)

1169

The following page will explain rewards.

Instructions

(page 4 out of 5)

You will earn £0.75 for completing the survey.

In addition, you may earn bonus from each question.

Let's see how it works in the example question. Suppose you picked True, as shown below:

True
(picked by 65% last week)

False
(picked by 35% last week)

At the end of this survey, we calculate the percentage of participants other than you who picked each answer.

You start with £1 bonus. Your bonus increases if the answer you picked is more popular among others in this survey, compared to last week.

Suppose 80% of others picked True this week. Then, you win 80 - 65 = 15 pence from this question.

Suppose 55% of others picked True this week instead. Then, you lose 65 - 55 = 10 pence.
1170

We sum your gains/losses over all questions. Your bonus is never negative and it can increase up to £2.

Your total reward is therefore between £0.75 and £2.75.

Instructions
(page 5 out of 5)

Note that your bonus depends on others' responses.

You earn a higher bonus if you picked answers that became more popular compared to the last survey, which covered the previous 7-day period.

Your own experience may help you guess how others respond.

In the example, say you recall staying too close in a queue at least once.

If keeping distance was more difficult in the last 7 days due to busier streets and shops, it is likely that other people experience the same.

Then, you might expect a higher percentage of True picks among others. In that case, picking True increases your bonus.

1171

Remembering your own experiences more accurately can improve your bonus.

1172

Instructions - Flat

Instructions

(page 1 out of 4)

Welcome! In this survey, you will answer 8 questions on the COVID-19 pandemic.

The UK government issues COVID-19 guidance and passes regulations to control the pandemic.

This survey aims to collect data on people's behaviour to assess whether such guidelines are helpful.

In each question, we will ask you about your experience for certain situations related to the pandemic.

Instructions

(page 2 out of 4)

Here's an example on how questions will appear:

I may have stood less than 2 metres away from the person in front in a queue at least once in the last 7 days.

1173

True **False**

You may pick True or False depending on whether you have been in the situation described in the question.

Your pick will be submitted when you click Submit

Instructions

(page 3 out of 4)

We ask the same questions every 7 days to a new group of at least 50 participants.

All participants are students who currently reside in the UK.
The survey can be taken only once.

The following page will explain rewards.

Instructions

(page 4 out of 4)

You will earn a fixed £1 bonus, on top of £0.75, for completing the survey.

1174 Your total reward will be £1.75.

1175

Instructions - Flat-PastRate

Instructions

(page 1 out of 4)

Welcome! In this survey, you will answer 8 questions on the COVID-19 pandemic.

The UK government issues COVID-19 guidance and passes regulations to control the pandemic.

This survey aims to collect data on people's behaviour to assess whether such guidelines are helpful.

In each question, we will ask you about your experience for certain situations related to the pandemic.

Instructions

(page 2 out of 4)

Here's an example on how questions will appear:

I may have stood less than 2 metres away from the person in front in a queue at least once in the last 7 days.

1176

True ◯ False ◯

You may pick True or False depending on whether you have been in the situation described in the question.

Your pick will be submitted when you click Submit

Instructions

(page 3 out of 4)

We ask the same questions every 7 days to a new group of at least 50 participants.

All participants are students who currently reside in the UK.
The survey can be taken only once.

In all questions, you will see the percentage of people who picked each answer in the last survey, 7 days ago.

For example, if 65% of participants picked True and 35% picked False, the choices will appear as follows:

True
(picked by 65% last week)

False
(picked by 35% last week)

1177

The following page will explain rewards.

Instructions

(page 4 out of 4)

You will earn a fixed £1 bonus, on top of £0.75, for completing the survey.

Your total reward will be £1.75.

Closing Survey

1178 **Thank you for your answers!**

To conclude, we would like you to answer some questions about your personal background and your experience in this experiment

How old are you?

What is your gender?

Male.

Female.

Other / Prefer not to disclose.

What is your education level?

How clear were the instructions in this survey?

Very clear.	Mostly clear.	Understandable, but not very clear.	Mostly unclear.	Very unclear.

Do you have any other comments or suggestions?

1179 Click Finish to complete the survey

Finish

NEW YORK UNIVERSITY

1180

Instructions - Week 0 survey

Instructions
(page 1 out of 4)

Welcome! In this survey, you will answer 9 questions on the COVID-19 pandemic.

The UK government issues COVID-19 guidance and passes regulations to control the pandemic.

This survey aims to collect data on people's behaviour to assess whether such guidelines are helpful.

In each question, we will ask you about your experience for certain situations related to the pandemic.

Instructions
(page 2 out of 4)

Here's an example on how questions will appear:

In the last 7 days, I may have stood less than 2 metres away from the person in front in a queue [ ]

1181	True	False
once or more	○	○
twice or more	○	○
3 times or more	○	○
4 times or more	○	○
5 times or more	○	○

In each question, there is a statement with a [ ] in it.

There are 5 alternatives for [ ]. You will be asked if the statement becomes True or False for you under each alternative.

Note that the alternatives are related. If you pick True for "3 times or more", the interface auto-selects True for "once or more" and "twice or more" as well. Try it!

Instructions

(page 3 out of 4)

We run the same survey once every 7 days with a new group of at least 50 participants.

1182 All participants are students who currently reside in the UK. The survey can be taken only once.

The following page will explain rewards.

Instructions
(page 4 out of 4)

You will earn a fixed £2 bonus, on top of £1, for completing the survey.

Your total reward will be £3.

End of Instructions

You are ready to begin the survey!

You can view the instructions in a new tab at any point.