Peer prediction markets to elicit unverifiable information

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Abstract

Prediction markets reward ex-post accuracy to incentivize agents to seek and reveal information. Some private signals, such as individual experiences or very long-run predictions, do not concern verifiable outcomes. In such cases, outcome-based rewards are not feasible. This paper presents peer prediction markets to elicit subjective judgments in binary questions of unverifiable information. Agents choose whether they receive a costly signal, which lead them to endorse either 'yes' or 'no' as an answer. Then, they either buy or sell a single unit of an asset at a price whose price is determined by endorsement rate of 'yes'. The price of the asset is set at the prior expectation of the endorsement rate. We obtain a separating equilibrium, where agents buy or sell the asset as a function of their signal. Evidence from two experimental studies demonstrate that peer prediction markets motivate agents to seek costly information and reveal it.

1 Introduction

"Have you stood less than 6 feet apart from another person in a queue yesterday?" Health surveys often require respondents to recollect past experiences. This experience can be seen as a private signal that a respondent acquire by exerting effort (recalling, to their mind, what they did a day earlier.) But how can we ensure that the respondents will provide such effort and answer truthfully if there is no way to compare their answer to some truth?

Starting with Crémer and McLean (1988), the mechanism design literature has explored ways to reveal private signals. Miller et al. (2005), and more broadly the peer-prediction literature

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(Witkowski and Parkes, 2012b, 2013; Liu and Chen, 2017a), have proposed solutions exploiting the informativeness of a respondent's answer in predicting their peers' answers. For instance, imagine that we have some prior expectations about the rate of yes answers to the 6-feet-apart question. A respondent answering yes increases our expectations about the proportion of *other* people answering yes. Formally, this increase is a simple application of Bayesian updating when respondents draw a private signal (yes/no), with unknown probability p of yes signals: a yes signal makes higher values of p more likely than initially believed.[1] Intuitively, the yes answer to the 6-feet-apart question can suggest that others also had difficulty complying with a social distancing guidelines.

In this paper, we propose and implement a novel solution to incentivize private signals acquisition and revelation: a peer-prediction market (PPM). In a PPM, yes respondents are rewarded with the formula " yes answer rate - prior expectations of yes answer rate". Those who answer no get the opposite reward. If there are fewer yes answers than expected, yes respondents get a negative reward while no respondents get a positive one. Equivalently, a PPM can be presented as yes (no) respondents buying (selling) a single asset, the value of which is eventually determined by the proportion of yes answers. The price is set to the prior expectations. In a situation in which the yes-answer rate is expected to follow a random walk, a repeated PPM can be implemented in which the price at period t is the value of the asset at $t-1$.

First, we show that signal acquisition and truthful revelation is a Bayesian Nash equilibrium, providing a partial-implementation solution to the static problem. Our solution is minimal, in the sense that it does not ask respondents to provide more than their answer and it does not require the surveyor to share more than prior expectations with the respondents.

Second, we test the static PPM in an online experiment closely following the theoretical model: respondents may exert an effort (i.e., complete a real-effort task borrowed from the experimental economics literature) to obtain a signal and report it; or they may simply answer randomly. We compare PPM with two benchmarks: flat fee (no incentives) and accuracy incentives (incentives when the signal generation process is observable). The latter is not applicable in surveys, where

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

such process in unobservable but it provides a gauge for the effect of PPM. A flat fee decreased the effort rate by about 20 percentage points with respect to accuracy incentives. PPM allowed us to recover half of this difference.

Third, we implement the repeated PPM in the context of a health survey, involving questions of the 6-feet-apart type. The asset price was fixed to the previous week yes-rate. We hypothesized that people not exerting recollection efforts were likely to deny having experienced such situation, and therefore that PPM would trigger higher rate of yes answers than a flat fee. Two weeks in a raw, we indeed obtained that more people admitted experiencing situations in contradictions with health guidelines in the PPM treatment than in the flat fee treatment.

PPMs present a market-based solution to the problem of incentivizing effort in information elicitation without verification (Waggoner and Chen, 2013). Previous work introduced peer prediction mechanisms that consider the truthful elicitation problem only, and does not explicitly incorporate costly effort. The original peer prediction method (Miller et al., 2005) can be adjusted for costly effort via re-scaling of payments. However, the researcher has to know the full common prior belief of participants. Bayesian truth serum (Prelec, 2004) and its variants (Witkowski and Parkes, 2012a; Radanovic and Faltings, 2013, 2014) do not require any knowledge of priors. But, as a result, the researcher lacks information to scale rewards appropriately for costly effort. Recent work developed peer prediction mechanisms to incentivize effort in crowdsourcing tasks in which ground truth is unverifiable. Such mechanism rely on additional structure on agents' proficiency (Witkowski et al., 2013), multiple tasks (Dasgupta and Ghosh, 2013; Radanovic et al., 2016; Shnayder et al., 2016) or a dynamic framework (Liu and Chen, 2017b). Similar to the original peer prediction method, PPM is one-shot and 'minimal' (Witkowski and Parkes, 2013): agents complete a single task only. But, PPM requires less information on priors. Furthermore, PPM offers a simpler solution in binary problems compared to other peer prediction mechanisms with costly effort.

Closest to PPMs are Bayesian markets (Baillon, 2017), which provide a market solution to binary elicitation problem in a similar Bayesian setup to ours, except that information is not costly. Moreover, unlike PPM, an agent first reports her answer. She can later buy (sell) one unit of the asset only if she reported 'yes' ('no'). Price is determined randomly afterwards, so the agents decide on trade options before price is observed. In equilibrium, agents report their true judgments to be eligible for their desired trade. In the way they are set-up, PPMs aim to be closer to prediction markets than Bayesian markets are. Agents can trade freely, according to their private information, at a pre-specified price.

2 Theory

2.1 Agents and their information

There are N agents. A researcher is interested in eliciting agents' informed judgments on a question Q with possible answers $\{0, 1\}$. Each agent i has a private *type* $\tau_i \in \{0, 1\}$, which corresponds to her informed judgment on Q. There are strictly positive proportions of both types of agents. Agents are expected payoff maximizers. In our main result, we let $N \to \infty$ for convenience.

An agent's type is unknown to her at first. Each agent i chooses whether to incur a fixed cost c 'learn' her type and become informed. The variable c represents cost of seeking information to form informed judgments. It is assumed that c is common knowledge to all agents. Let $e_i \in \{0, 1\}$ denote agent i's choice in judgment formation, where $e_i = 1$ if agent i incurs c and learns her type τ_i, $e_i = 0$ otherwise. In the latter case, agent i is assumed to be indifferent between two alternative answers to Q.

Let $\omega = \sum_i \tau_i / N$ be the proportion of type-1 agents. Each agent i in the sample has a non-degenerate prior belief $f_i(\omega)$, representing how likely she would consider various proportions of ω to be, prior to learning her type. Agents may have heterogeneous prior beliefs, but it's assumed that the priors are anonymously known by all agents. To illustrate, consider a setup where $f_i \equiv Beta(\alpha_i, \beta_i)$ for all agents i. Parameters $\{\alpha_i, \beta_i\}$ are drawn from a commonly known distribution (to agents) such that $\frac{\alpha_i}{\alpha_i+\beta_i} = \omega^0$. Agents know the population distribution of $\{\alpha_i, \beta_i\}$. Since priors are completely characterized by $\{\alpha_i, \beta_i\}$, all distinct priors are common knowledge to agents.

Let $E_i(\omega)$ be agent i's prior expectation on ω. It is assumed that $E_i(\omega) = E_j(\omega) = \omega^0$ for any $i \neq j$. Agents share the same prior expectation ω^0, common knowledge to all agents and the researcher. To illustrate, consider the following framework: ω is the unknown proportion of type-1 agents at a given time, and ω^0 is the last realization before the current time. Given ω^0, $\omega = \omega^0 + \epsilon$, where ϵ is a zero-mean random shock. Intuitively, information on Q up to current time is incorporated in ω^0. The change from ω^0 to next realization ω can be attributed to recent evidence. The error term ϵ captures the impact of such evidence. It is common knowledge to agents that ω is determined as specified above and ϵ is a zero-mean random shock. Last realization ω^0 is public knowledge. The exact distribution of ϵ is unknown to both the agents and the researcher. Zero-mean shock implies that, on average, new evidence does not favor one side. In this framework, agent i's prior reflects her belief on the distribution of ϵ. Note that $E(\omega|\omega^0) = \omega^0$. Therefore, conditional on knowing ω^0, a rational agent's prior expectation must be ω^0. Thus, ω^0 is the common prior expectation.

Agent i updates her prior on ω based on τ_i if $e_i = 1$. Posterior belief of agent i with $e_i = 1$ on ω is given by $f_i(\omega|\tau_i)$, with associated posterior expectation $E_i(\omega|\tau_i)$. Agent i's type is a stochastically relevant signal from the distribution of opinions (Miller et al., 2005). If $\tau_i = 1$, then $f_i(\omega|\tau_i)$ (first order) stochastically dominates $f_i(\omega)$, i.e. agent i considers large proportions of ω more likely compared to her prior belief. In contrast, if $\tau_i = 0$, $f_i(\omega|\tau_i)$ is stochastically dominated by $f_i(\omega)$, i.e. agent i considers large proportions of ω less likely compared to her prior belief. Given $f_i(\omega)$, let $\bar{\omega}_{ik}$ be posterior expectation on ω for $\tau_i = k$. The posterior expectation of agent i is denoted by $\bar{\omega}_i$, where $\bar{\omega}_i = \bar{\omega}_{ik}$ if $\tau_i = k$. The relationship between prior and posterior beliefs suggest that $\bar{\omega}_{i1} > \omega_0 > \bar{\omega}_{i0}$. Thus, it is common knowledge that agent i's posterior expectation is higher than her prior expectation ω_0 if she is type-1, and vice versa if she is type-0. If $e_i = 0$, agent i's posterior expectation is the same as her prior expectation, i.e. $\bar{\omega}_i = \omega^0$.

2.2 The Market

A one-shot market is set up for Q. The market is operated by the researcher, also referred as the market maker. The market maker sets the price as $p = \omega^0$. Agents are first presented with Q and choose $e_i \in \{0, 1\}$. Then, they participate in the market and simultaneously buy or (short) sell a single unit of the asset at p. Agent i's trade decision is denoted by $x_i \in \{0, 1\}$, where $x_i = 1$ if agent i places a buy order and $x_i = 0$ if agent i places a sell order. Liquidation value of the asset, denoted by v, will eventually be determined by the proportion of buyers, i.e. $v = \sum_{i=1}^{N} x_i/N$. Because there are infinitely many agents, a single agent cannot unilaterally affect v. Let $E_i(v)$ and $E_i(v|\tau_i)$ be the prior and posterior expectations of agent i on v.

Agents do not trade with each other but with a market maker. The market maker collects trade orders simultaneously and executes all trades. The market terminates when v is revealed and transfers occur. If trade occurs, buyers receive the liquidation value from the market maker, sellers compensate the market maker. Agent i's net payoff is $v - p$ if he is a buyer, $p - v$ if he is a seller.

2.3 Truthful trading

An agent i's payoff maximization problem involves choosing (e_i, x_i) given (c, p). Agent i's trade is *truthful* if $e_i = 1$ and $x_i = \tau_i$, i.e. agent i incurs the cost to learn her type and becomes a buyer if she is type-1, a seller if she is type-0. *Truthful trading* refers to the situation where all trades are informed. Under informed trading, agents' trade reflect their informed judgments (types).

Theorem. *There exists $\bar{c} \in \mathrm{R}_+$ such that truthful trading is a Bayesian Nash equilibrium in a PPM for $c < \bar{c}$*

Appendix A provides the proof of the theorem. When cost of information is sufficiently low, all agents choose to receive costly signal. Furthermore in a coordination equilibrium, type-1 agents become buyers and type-0 agents become sellers. So, truthful judgments of agents can be inferred from their trades. Positive expected payoffs from trading based on posterior expectations motivate agents to seek costly information. When all other agents trade truthfully, an agent i's expectation on v is the same as her expectation on ω, i.e. $E_i(v|\tau_i) = \bar{\omega}_i$. Agent i's expected payoff is $\bar{\omega}_i - c - p$ if she is a buyer, $p - \bar{\omega}_i - c$ if she is a seller. For c sufficiently small, expected payoff from one of the trades become positive. Agent i can expect a positive payoff from learning her type and trading based on $\bar{\omega}_i$. In contrast, agent i expects zero payoff from trading if she does not learn

τ_i, as $E_i(v) = \omega^0 = p$. Thus, for c sufficiently small, agent i has an incentive to incur the cost and learn her type τ_i. Recall that $\bar{\omega}_{i1} > \omega^0 = p > \bar{\omega}_{i0}$. If $\tau_i = 1$, agent i could expect a positive payoff from buying only as $\bar{\omega}_i > p$. Vice versa for $\tau_i = 0$. Thus, $x_i = \tau_i$ is the expected payoff maximizing trade.

3 Experimental Evidence

Section 4 established the existence of an equilibrium agents in a PPM seek costly information and make informed trades. An agent's incentives in trading are based on her peers' behavior, as value of the asset is determined by other agents' trades. Are such peer-based 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.

3.1 Overview

We run two experimental studies to test if PPM elicit effort in judgment formation. Study 1 aims to test PPM 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, each participant could independently draw a single ball from the actual box if she completes 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 the PPM in a verifiable task allows us to implement incentives for ex-post accuracy as a benchmark. Study 1 consists of three experimental conditions in which participants complete the same task. The control condition offers a fixed participation fee while the two treatments implement PPM incentives and incentives for ex-post accuracy. Results suggest that the PPM elicit significantly more effort than fixed rewards while the effort is highest under incentives for ex-post accuracy. As discussed before, ex-post accuracy is not observable in practical elicitation problems of unverifiable information. The results of Study 1 suggest that the PPM are effective when ex-post rewards are not feasible.

Study 2 tests the PPM in a practical problem of elicitation of unverifiable information. In response to the Covid-19 pandemic in 2020, governments around the world issued guidance to encourage social distancing. Policy makers would like to know if such guidance is followed by the public. When asked to self-report if they were following a certain safe practice, people may not recall instances where they failed to do so. We implement the PPM in an online survey aimed at the residents of the UK. Participants are asked 8 questions, each involving an unsafe behavior according to the Covid-19 guidance issued by the UK government. We find that under the PPM incentives participants are more likely to admit not following the guidance and they took longer to respond on average. Study 2 illustrates how the PPM can be implemented in practice to encourage respondents to exert more mental effort and provide accurate responses when it is impossible to verify the accuracy ex-post.

3.2 Study 1 - PPM 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 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 condition.

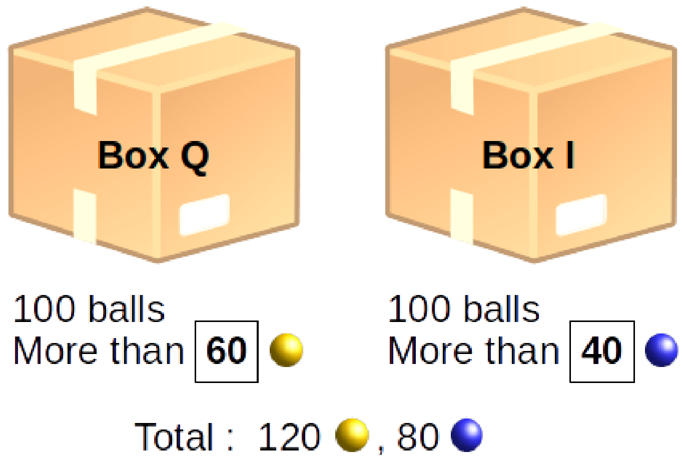

Figure 1: An example pair of boxes

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. So, 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 Z 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.

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. 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.

$$\begin{array}{cccccc} 0 & 0 & 1 & 1 & 0 & 1 \\ 1 & 0 & 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 1 & 1 & 1 \\ 0 & 0 & 1 & 1 & 0 & 1 \end{array}$$

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. If a participant skips the effort task, she is immediately asked to pick a box. Otherwise, she is presented the associated binary matrix and asked to report the number of 0s. The participant is required to report an accurate count to proceed. Upon reporting the accurate count, the participant observes her draw, which is either a blue or a yellow ball. Then, she proceeds to picking a box.

The prediction task is a representation of the binary question Q, where the two boxes in any pair correspond to the possible answers. The effort task corresponds to the costly signal in our framework. Participants are allowed to skip the effort task, in which case they make a pick without observing a draw. In any given pair, the boxes are a priori equally likely to be the actual box. If a participant draws a yellow (blue) ball, her posterior probability on left (right) box being the actual box is higher. An agent's best guess on the actual box corresponds to her type. Thus, a participant's draw is effectively the signal that fully determines her type.

Design. We set up three experimental conditions which differ only in reward structure. In the *flat* condition, 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 $[£1.25, £5.25]$. The *peer incentives* treatment implements the PPM. Similar to the accuracy treatment the basis reward is £3.25. In addition, participants may earn a bonus from each pick which is determined by her 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 if she picked Box Q, loses $40 - 18 = 22$p if she picked Box I. The number within the square below each box is serves as a threshold. The participant earns a positive bonus from her pick if the percentage of others who pick the same box in that pair exceeds the threshold of that box.

Rewards in the peer incentives treatment represent the incentives in a PPM. Consider the pair of urns given in Figure 1. The actual box is either Box Q or Box I with equal probability. Prior expectation of a participant on the number of yellow balls is 60. Suppose the participant chooses to complete the effort task and draws a yellow ball. Her posterior probability on Box Q being the actual box is higher, which has two implications: i) her best guess on the actual box is Box Q, and ii) her posterior expectation on the number of yellow balls in the actual box is greater than 60. Then, the participant expects more than 60% of her peers to draw yellow and consider Box Q more likely as well. In a situation where all others pick the box they consider more likely, the participant expects a positive bonus from picking Box Q. Vice versa holds for a participant who draws a blue ball. This setup is analogous to a PPM with $p = \omega_0 = 0.6$, where the differing best guesses of participants who draw different colors correspond to the types. Trades are represented by picks in the prediction task. Informed trading corresponds to the situation where, in a given pair, each participant completes the effort task and picks a box according to her draw. Participants who draw different colors (and hence, have a different best guess on the actual box), pick different boxes.

Participants in the flat condition have no incentive to complete the effort tasks as their reward does not depend on prediction accuracy. In contrast, rewards in the accuracy condition are determined by prediction accuracy. Thus, participants in the accuracy condition could be expected to complete effort tasks more frequently to maximize their accuracy. The peer incentives condition also provides incentives to complete effort tasks if, as predicted by the theory, participants consider their signal informative on others' picks. We could observe more effort task completion relative to the flat condition if the PPM incentives work in practice.

Participants. We recruit 210 subjects for an online experiment, implemented via Qualtrics. The subjects are recruited from Prolific, an online platform for conducting surveys. We restrict our subject pool to U.S. citizens and students. Table B1 in Appendix B provides further information on the participants.

Procedure. The experiment was published on Prolific in May 2020. Subjects are randomly selected into one of the experimental conditions. They are first presented with instructions, which differ across the experimental conditions in rewards only. Then, subjects complete the prediction tasks. The order of the prediction tasks is randomized. Finally, subjects complete a short survey on demographics and their experience in the experiment.

3.2.2 Results

The primary question of interest is whether participants are more likely to seek costly information under the incentives provided by a PPM compared to fixed rewards. The effort task completion in control and peer incentives treatments allows us to test the effect of PPM incentives. Furthermore in our prediction task, the ground truth (the actual box in any pair) is known to the experimenter. The accuracy treatment implements rewards for ex-post accuracy, which are not feasible in practice for elicitation without verification. We compare accuracy and peer incentives treatments to assess the effectiveness of PPM incentives relative to ex-post rewards.

We measure the frequency with which subjects completed the effort tasks across the experimental conditions. Figure 3 depicts the percentage of participants in each experimental condition who complete the associated effort task in each prediction task:

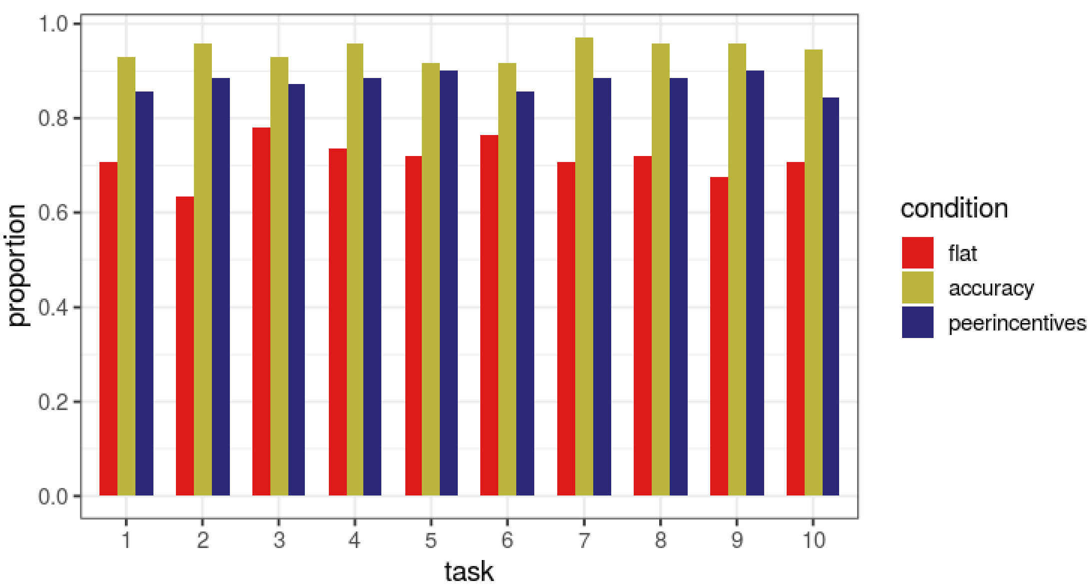

Figure 3: Proportion of participants who complete effort tasks in each prediction task.

The effort level is higher than zero, even in the control condition. Effort task completion is strictly higher in peer incentives and accuracy treatments, the latter achieves the highest proportions. Figure 3 suggests that incentives provided by a PPM is effective in eliciting a higher proportion of informed judgments compared to a fixed reward. Incentives in the accuracy treatment are the most effective.

Table 1 below shows the average marginal effects from logistic regressions where probability

Dep. var.: P(effort task completed)				
	(whole sample)		*(filtered sample)*	
	(1)	(2)	(3)	(4)
Peer incentives	0.10^{***}	0.09^{***}	0.10^{***}	0.08^{***}
	(0.03)	(0.03)	(0.03)	(0.03)
Accuracy	0.18^{***}	0.18^{***}	0.18^{***}	0.18^{***}
	(0.03)	(0.03)	(0.03)	(0.03)
Age		-0.00		-0.00
		(0.00)		(0.00)
Female?		0.04		0.04
		(0.03)		(0.03)
US resident?		-0.02		-0.02
		(0.06)		(0.06)
Num. obs.	2100	2070	2060	2030
Log Likelihood	-821.85	-768.69	-816.44	-763.58
Deviance	1643.70	1537.38	1632.88	1527.16
AIC	1649.70	1549.38	1638.88	1539.16
BIC	1666.65	1583.19	1655.77	1572.86

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

Table 1: Marginal effects, logistic regression (baseline category: flat)

of effort task completion is the dependent variable. The pooled data includes 2100 decisions to complete the effort task or not. We include binary indicators for the experimental conditions as dependent variables. The coefficient of 'Peer incentives' in Table 1 measures the estimated difference from implementing PPM incentives instead of a flat fee on the likelihood of effort task completion in any task. The coefficient of 'Accuracy' measures the same for rewarding participants for ex-post accuracy. Models (1) and (2) use the whole sample of subjects. In (3) and (4), participants who gave an incorrect answer in the post-experimental quiz are excluded to construct a filtered sample. Specifications (2) and (4) also include various controls. The variables 'US citizen?' and 'Female?' are binary indicators for US residents and gender respectively while 'Age' is a numeric variable. In all models, standard errors are clustered at participant level. Tables B4 and B5 in Appendix B present probit marginal effects and regression estimates from both logistic and probit models.

In all specifications, the marginal effects for peer incentive and accuracy treatments are positively significant. Based on model (1), we see that a participant in peer incentives treatment is 10%

more likely to complete the associated effort task in a given prediction task. Incentives provided by a PPM motivates agents to seek costly information. Table B4 suggests a similar conclusion. Furthermore, marginal effects for accuracy treatment are higher, suggesting that incentives for ex-post accuracy could be even more effective.

3.3 Study 2 - Eliciting Covid-19 experiences truthfully using PPM

Study 2 implements PPM incentives in measuring if the 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. In an unincentivized or a flat-fee survey, participants may not exert the mental effort to recall and report their behavior truthfully. The reports are practically unverifiable and we may expect unsafe behavior to be under-reported. We investigate if the PPM motivate 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. 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 questions:

	Statement
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 questions

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 different experimental conditions some of which implement the PPM. We also run a week 0 survey to elicit information necessary to initialize the PPM. 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. For example, question 1 in Table 2 is presented as 'In the last 7 days, I have been in an elevator with another person in it ...' and the participant is presented with 5 choices: 'once or more', 'twice or more', '3 times or more', '4 times or more', '5 times or more'. Based on the results of the 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 will provide more information on how week 0 survey is used in the design below.

Design. In the week 0 survey, all participants receive a flat fee. In week 1 and 2 surveys, we manipulate incentives to create the control and treatment conditions. In the control, participants are rewarded with a flat fee for completing the survey while the treatment implements the PPM incentives. Figure 4 shows the experiment interface in the treatment condition:

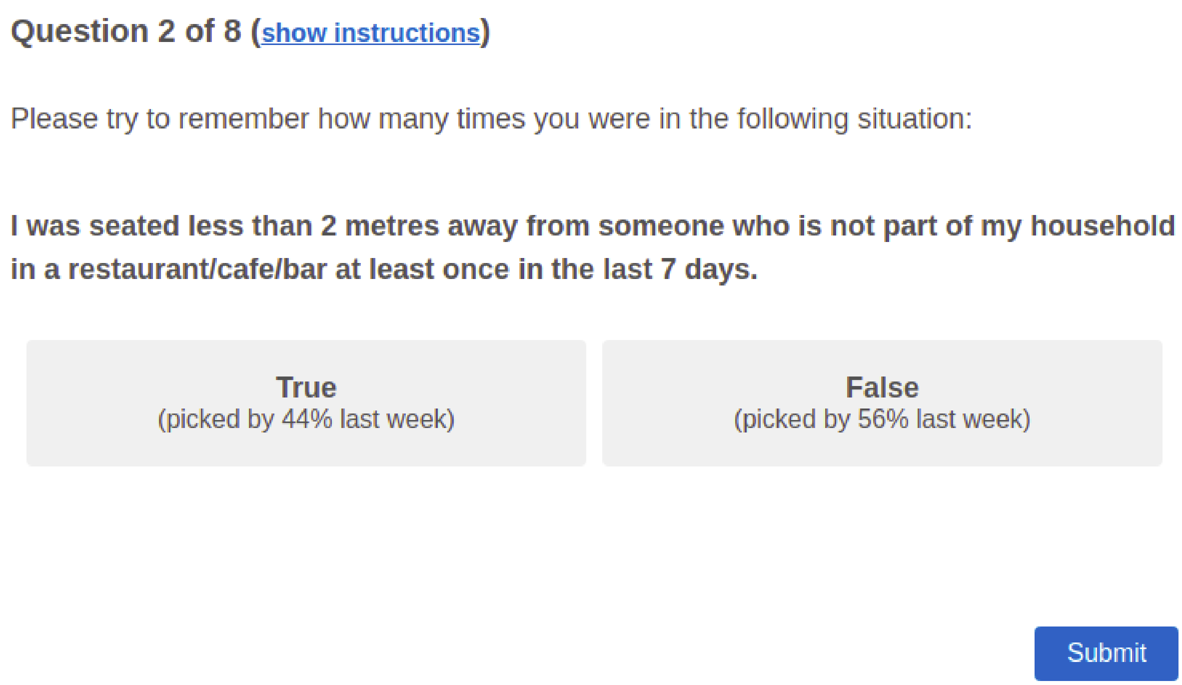

Figure 4: A screenshot from the treatment condition

The interface displays the statement and requires subjects to pick 'true' or 'false'. The text below each alternative shows 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 ω^0, which can be considered as the last realization of ω. The market maker sets $p = \omega^0$, which leads to the separating equilibrium. The endorsement rates of the previous iteration represents ω^0. Furthermore, participants' bonus depends on the endorsement rates. In Figure 4, 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 treatment survey essentially implements a repeated PPM where last iteration's realization determines the price for the current iteration. We will provide more information on the rewards below. If the PPM incentives are effective in incentivizing participants to exert mental effort and give more accurate answers, we might expect decision times to be longer and endorsement rates for 'true' to be higher.

The control surveys are similar to the treatment surveys except that participants are rewarded with a flat fee. We implement two different types of control surveys. In the control-1 condition, the survey interface does not present any information on previous iterations' endorsement rates. In contrast, the control-2 survey shows the same screen as the treatment condition, shown in Figure 4. The rewards are fixed in both control-1 and control-2 surveys, thus the previous endorsement rates are irrelevant. Nevertheless, we included control-2 condition to check if merely presenting that information affects participants reports. If a PPM is effective, we could expect to see higher endorsement for 'true' and longer response times in the treatment survey compared to control-1. However, participants process additional information (previous endorsement rates) in the treatment condition, which might particularly affect decision times. A significant difference between the treatment and the control-2 would further suggest that the effect on endorsements and decision times is not simply due to the availability of previous endorsement rates.

As discussed above, the control-2 and treatment surveys present information on endorsement rates in the previous iteration. Since week 1 is the first iteration, the week 0 survey is used to determine the previous endorsement rates presented in the control-2 and treatment surveys of week 1. Thus, week 0 data is used to initialize the control-2 survey and the PPM in the treatment survey. Furthermore, the week 0 survey motivates our choice to run two versions where the statements include 'at least once' and 'at least twice' respectively. Table B2 in Appendix B provides the percentage of participants who pick 'true' in each question 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.

To summarize, we implement 6 surveys in a 3 (control-1,control-2,treatment) $\times$ 2 ('at least once', 'at least twice') design in each iteration. The week 0 survey is used to initialize the control-2 and treatment surveys in week 1 while week 2 surveys are initialized using week 1 results endorsement rates from the same survey.

Participants. Participants are recruited from Prolific, an online platform that provides subject pools for online experiments. We restrict our subject pool to students who currently reside in the UK. In total 692 participants completed our survey, 50 of which participate in week 0 survey while the remaining 642 participated in a week 1 or week 2 survey, assigned randomly in one of the 6 conditions 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. Table B3 in Appendix B provides further information on the participants.

Rewards. Control-1 and control-2 surveys pay a fixed reward of £1.75. In the treatment surveys, participants earn £0.75 for participation. In addition, they start with a bonus of £1. In each question, a participant's bonus changes according to the difference between the endorsement rate in the current survey versus the endorsement rate in 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 subject wins $70 - 50 = 20$ pence. In contrast, if the endorsement rate in week is 30%, the subject loses $50 - 30 = 20$ pence. The previous week's endorsement rate serves as the price in a PPM while the current week's endorsement rate, unknown to the participant at the time of her decision, is analogous to realized value of the asset. For each participant in the treatment surveys, we sum the gains and losses over all question to determine the net bonus.

Procedure. The experiment is conducted over three weeks and consists of week 0, 1 and 2 surveys that take place 7 days apart. The week 0 iteration is a single survey while in weeks 1 and 2, participants are randomly assinged to the different conditions. 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 their experience in the experiment.

3.3.2 Results

Figure 5 shows the percentage of 'true' picks for each condition and version in the week 1 and week 2 surveys. Responses are pooled across questions and participants.

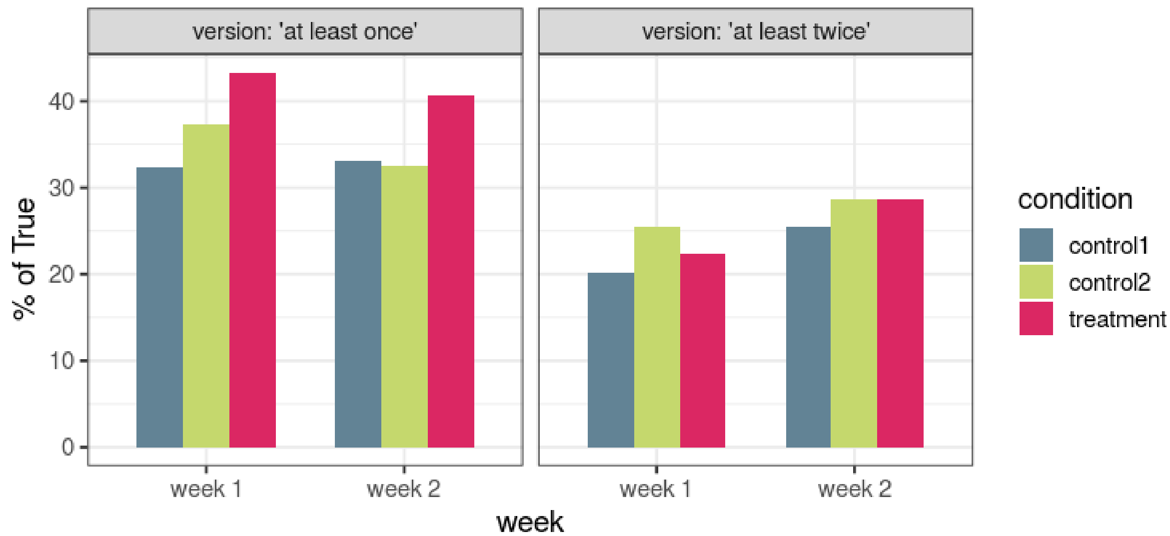

Figure 5: Proportion of participants who complete effort tasks in each prediction task.

In the 'at least once' surveys, the treatment elicits a higher percentage of 'true' responses compared to both controls. No such difference is observed in any iteration in the 'at least twice' version. Figure 6 depicts the response times:

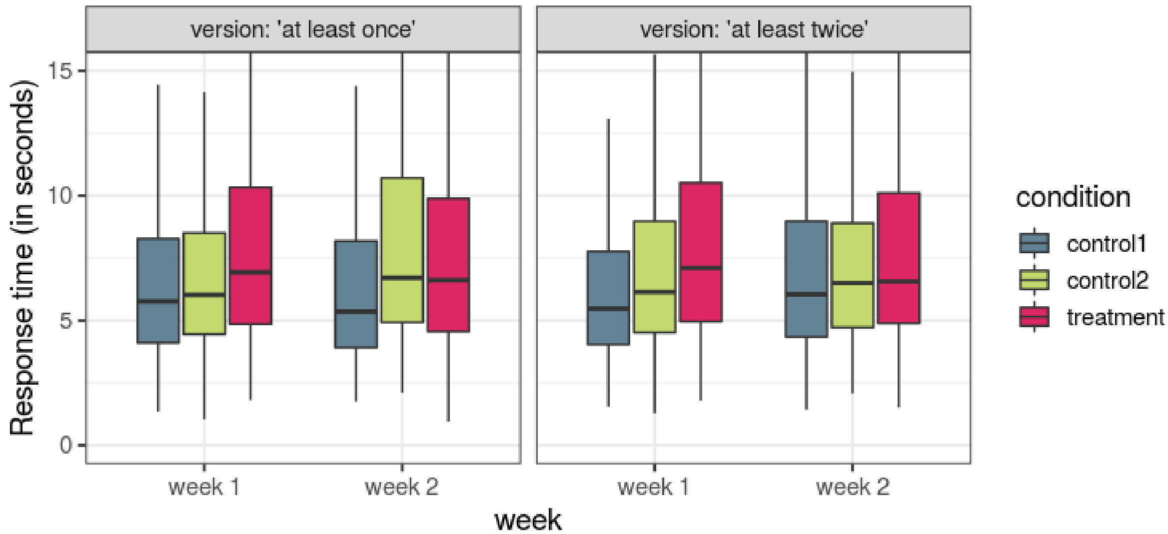

Figure 6: Proportion of participants who complete effort tasks in each prediction task.

The median response time in the treatment condition is higher than the median response time in the control-1 surveys in all iterations. The differences with the control-2 surveys are smaller or even reversed in week 2 ‘at least once’ survey.

Figure 5 suggests that participants in the treatment survey are more likely to recall engaging in an unsafe practice at least once in a 7-day period. In addition, we may expect longer response times in the treatment survey in most cases. These imply that the PPM incentives motivate participants to exert more mental effort before responding. For a formal analysis, we estimate two classes of regression models. Firstly, we estimate a logistic regression for participants’ likelihood picking ‘true’ in any given question. Secondly, we estimate a linear regression model where response time is the dependent variable. In both models, control-1 is the baseline category and binary indicators for control-2 and treatment are variables of interest. We also include various demographic controls representing the age, gender and citizenship of participants. We focus on the ‘at least once’ versions of all iterations as Figure 5 suggested a possible difference for these versions only. Furthermore, we estimate models separately for weeks 1 and 2 to investigate if the results are similar.

Table 3 presents the average marginal effects from the logistic regressions and the estimates from the response time regressions. Models (1) to (4) includes average marginal effects where {(1),(2)} and {(3),(4)} show the estimates from week 1 and week 2 surveys respectively. Similarly, {(5),(6)} and {(7),(8)} show the response time regression output from weeks 1 and 2. In models

(5) to (8), the intercept term represents the estimated response time in the control-1 condition. In all models, standard errors are clustered at the participant level.

	P(response = 'true'), marginal effects				*Response time*			
	(week 1)		*(week 2)*		*(week 1)*		*(week 2)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Intercept)					6.90^{***}	7.95^{***}	7.09^{***}	8.35^{***}
					(0.29)	(1.18)	(0.46)	(1.07)
Control2	0.05	0.05	−0.01	−0.00	0.46	0.35	1.52^{**}	1.51^{**}
	(0.04)	(0.04)	(0.04)	(0.04)	(0.49)	(0.50)	(0.63)	(0.62)
Treatment	0.11^{***}	0.10^{***}	0.08^{**}	0.08^{**}	2.85^{***}	2.80^{***}	1.00	0.87
	(0.03)	(0.03)	(0.04)	(0.04)	(0.72)	(0.73)	(0.65)	(0.65)
Age		-0.00^{*}		−0.00		−0.02		−0.00
		(0.00)		(0.00)		(0.04)		(0.02)
Female?		0.02		−0.02		0.02		0.29
		(0.03)		(0.03)		(0.57)		(0.54)
UK citizen?		−0.00		0.03		−0.76		-1.61^{**}
		(0.03)		(0.04)		(0.53)		(0.64)
Num. obs.	1264	1264	1280	1280	1264	1264	1280	1280
Log Likelihood	−831.33	−829.47	−828.44	−826.92				
Deviance	1662.67	1658.94	1656.88	1653.85				
AIC	1668.67	1670.94	1662.88	1665.85				
BIC	1684.09	1701.79	1678.34	1696.78				
R^2					0.03	0.03	0.01	0.02
Adj. R^2					0.03	0.03	0.01	0.02
RMSE					7.18	7.18	6.05	6.02
N Clusters					158	158	160	160

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

Table 3: Logistic regression and linear regression on response times

The average marginal effects in Table 3 show that the treatment survey elicits a higher frequency of 'true' picks. According to model (1), a participant in the treatment condition of week 1 survey is 11% more likely to report 'true' for a given statement compared to a participant in the control-1 condition. In contrast, control-2 condition has no effect. A similar result holds for the week 2 survey where the marginal effect of the treatment condition is estimated to be 8%. Tables

B6 and B7 in Appendix B show similar results in probit marginal effects and the logistic and probit regression estimates. The PPM incentives motivate participants to declare unsafe practices at a higher rate, which might indicate that such practices are under-reported in basic surveys. The PPM encourage participants to exert more mental effort and report more accurate responses. The results on the response time regressions partially support this interpretation. In the week 1 survey (models (5) and (6)), participants in the treatment condition spend significantly more time in their responses. The same effect is positive but not significant in the week 2 survey.

4 Related Literature

Elicitation problem we address in this paper have two dimensions. The first, referred to as *truthful elicitation*, is incentivizing agents to report their true judgments when ex-post verification is not possible. The researcher has no 'answer key' with which she can evaluate accuracy of an agent's report. The second, referred to as *effort elicitation*, involves incentivizing agents to exert effort in judgment formation, so that truthful reports produce high-quality subjective data. With verifiable information, outcome-based rewards as in regular prediction markets achieve both effort elicitation and truthful elicitation. The literature on peer prediction mechanisms propose alternative incentivization schemes for the unverifiable information case. Early works focus on ensuring truthful elicitation. Some recent work studies effort elicitation. PPM offer a market solution for binary information, where incentives in the market both motivate agents to exert effort and trade according to their truthful judgments. This section provides a comparative discussion of alternative solutions to both problems.

The original peer prediction method of Miller et al. (2005) asks agents to report an answer to a multiple choice question. It is assumed that the mechanism designer knows the common prior (possibly from previous data). An agent's report is used to update the prior. The resulting posterior is used to predict what another agent reported. Accuracy of the posterior determines initial agent's reward. Subsequent work extended peer prediction method to settings with weaker information requirements, at the cost of 'non-minimality' (Witkowski and Parkes, 2012b) or introducing a dynamic setup (Witkowski and Parkes, 2013; Zhang and Chen, 2014). In the Bayesian truth serum (Prelec, 2004, BTS), agents are assumed to have a common prior belief. But, the mechanism designer need not have access to that prior. So, BTS can be implemented without using previous data. In BTS, agents make two reports. In one, they respond to a multiple choice question. In the other, they predict the frequency of each possible response. Agents' prediction reports are scored based on accuracy. Private responses are scored according to actual vs predicted endorsement frequencies of responses, such that surprisingly common answers are scored higher. A Bayesian agent, who shares a common prior belief with others on population distribution of responses, expects her own response to be more common than average prediction of all agents. Thus, scoring incentivizes agents to report their true answer.

Both the original peer prediction method and BTS are solutions to the problem of truthful elicitation. They do not incorporate costly effort. The peer prediction method can be adapted to costly effort by re-scaling payoffs, using the knowledge on common prior belief. Similar to the peer prediction method, PPM is one-shot and minimal. However, PPM does not require common prior, nor the complete knowledge of prior beliefs. The market maker is assumed to know the common prior expectation only. In binary elicitation problems, PPM provides a simpler and less information demanding alternative to the peer prediction method.

Recent work developed peer prediction mechanisms for effort elicitation in crowdsourcing problems with unverifiable tasks, such as peer grading, content classification etc. Witkowski et al. (2013) study output agreement mechanisms, in which an agent receives positive payment if her report agrees with a peer agent. Simple output agreement mechanisms do not achieve truthful elicitation when an agent believes that she holds a minority opinion, which may also affect effort decision. Dasgupta and Ghosh (2013) use reports in multiple auxiliary questions to penalize agreement without effort in a binary question of interest. Given common prior expectation, PPM achieve the same binary elicitation while maintaining minimality (single task). Shnayder et al. (2016) generalize Dasgupta and Ghosh (2013) to obtain correlated agreement mechanism for non-binary questions. Correlated agreement uses multiple questions and requires knowledge of signs of individual correlations across questions. Peer truth serum for crowdsourcing is another peer agreement mechanism which uses agents' responses to multiple questions Radanovic et al.

(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 PPM, requires a dynamic setup. In binary elicitation problems, PPM offers a simpler minimal alternative to other peer prediction mechanisms for effort elicitation.

Bayesian markets (Baillon, 2017) offer a market-based solution for truthful elicitation in binary questions. In a Bayesian market, agents report an answer to a binary of question of interest. There is a single asset, whose value is determined by the proportion of agents who report 'yes'. Agents receive a costless binary signal, which fully determine their type. Agents share a common prior belief on population distribution of types. As in our setup, agents update their beliefs using their own types. Belief updating is 'impersonal', agents with the same type have the same posterior beliefs. A Bayesian type-1 ('yes') agent expects a higher value of asset compared to a Bayesian type-0 ('no') agent. Agents who report yes (no) are allowed to only buy (sell) the asset, at a price drawn randomly from unit interval later. The market maker executes trades only when majority of agents in both sides of the market (yes and no) are willing to trade, which occurs when price is within posterior expectations of the two types. In this setup, both types are incentivized to report their true beliefs. Since type-1 agents have a higher posterior expectation, they prefer to become buyers when trade occurs. Vice versa for type-0 agents.

In binary truthful elicitation problems, Bayesian markets have an appeal over scoring-based methods: prediction reports and scoring are replaced by simple betting decisions and market payoffs. PPM follow a similar approach, but the elicitation procedure is simplified further. Unlike Bayesian markets, participants in a PPM do not report an answer. They trade freely according to their private information. In equilibrium, participant's true judgments can be inferred from their trade. In a Bayesian market, trade is an auxiliary tool to incentivize truthful reports. If the randomly drawn price is not in the appropriate range, trade may not occur even in the truthful equilibrium. In a PPM, trade occurs at any price. PPM is more analogous to a prediction market as participants trade at a given price.

5 Conclusion

For events with ex-post verifiable outcomes, prediction markets are known to be effective in eliciting and aggregating informed judgments. However, prediction markets are not suitable for unverifiable judgments, as the outcome-based rewards are not feasible. Researchers and practitioners typically resort to simple surveys with fixed rewards, which do not provide incentives to acquire costly information. PPM provide a market mechanism that incentivize agents to seek information and trade truthfully on binary questions of unverifiable information. Experimental evidence suggests that incentives provided by a PPM motivates agents to seek costly information in judgment formation.

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Appendices

Appendix A: Proof of Theorem

Let $\{e_i^*, x_i^*\}$ denote expected payoff-maximizing effort and trade decisions. First, assume that all agents participate in the market. Consider the following *truthful strategy* for agent i:

$$e_i = 1 \qquad x_i = \begin{cases} 1 & \text{if } \bar{\omega}_i \geq p \\ 0 & \text{if } \bar{\omega}_i < p \end{cases}$$

This strategy is considered as truthful for two reasons. Firstly, agent i incurs the effort ($e_i = 1$) and learn her type. Secondly, her trade is determined by her posterior expectation $\bar{\omega}_i$. Recall that $\bar{\omega}_{i1} > p = \omega_0 > \bar{\omega}_{i0}$, i.e. posterior expectation of an agent i is higher (lower) than p if she is type-1 (type-0). Thus, the truthful strategy specified above leads to truthful trading. Agent i learns her type and becomes a buyer (seller) if she is type-1 (type-0), resulting in $x_i = \tau_i$. So, if there exists a $\bar{c}$ such that all agents following the truthful strategy is an equilibrium for $c < \bar{c}$, the proof is done.

Suppose all agents other than i follow the truthful strategy. Since $x_j = \tau_j$ for all $j \neq i$, $E[v|\tau_i] = E[\omega|\tau_i]$. Assume for the moment that $e_i = 1$. Expected payoff from $x_i = 1$ is given by $E_i(v - p - c) = E_i(v|\tau_i) - p - c = \bar{\omega}_i - p - c$. Similarly, expected payoff from $x_i = 0$ is given by $E_i(p - v - c) = p - E_i(v|\tau_i) - c = p - \bar{\omega}_i - c$. If $\tau_i = 1$, $\bar{\omega}_i = \bar{\omega}_{i1} > p$, implying that agent i expects a higher payoff from buying, which gives $x_i^* = 1$. In contrast, if $\tau_i = 0$, $\bar{\omega}_i = \bar{\omega}_i^0 < p$, agent i expects a higher payoff from selling, $x_i^* = 0$. Thus, $x_i^* = \tau_i$ if $e_i^* = 1$.

Consider now agent i's decision on e_i. If $e_i = 0$, $E_i(v) = \omega_0 = p$, implying that expected payoff from $x_i = 0$ and $x_i = 1$ are both zero. Thus, $e_i = 1$ if agent i expects a positive payoff from trading based on $E[v_i|\tau_i]$. Expected payoff prior to decision on e_i is given by

$$\omega_0(\bar{\omega}_i^1 - p - c) + (1 - \omega_0)(p - \bar{\omega}_i^0 - c)$$

which is strictly positive if $c < \omega_0(\bar{\omega}_i^1 - p) + (1 - \omega_0)(p - \bar{\omega}_i^0)$. Let $\bar{c}_i = \omega_0(\bar{\omega}_i^1 - p) + (1 - \omega_0)(p - \bar{\omega}_i^0)$.

Then, $e_i^* = 1$ for agent i if $c < \bar{c}_i$. Now let $\bar{c} = min\{\bar{c}_1, \bar{c}_2, \ldots, \bar{c}_N\}$. Since $\bar{c} \leq \bar{c}_i$ for all i, $e_i^* = 1$ for any agent i. An arbitrary agent i follows the truthful strategy if all other agents follow that strategy a well. Thus, all agents following the truthful strategy is an equilibrium when $c < \bar{c}$, which completes the proof.

Appendix B: Summary statistics

Table B1: Summary statistics, Study 1...

	Experimental Condition		
	Flat	Accuracy	Peer incentives
Number of subjects	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
Average reward	£3.25	£3.50	£3.342
Correct answer in pre-experimental quiz	54	67	57
Correct answer in post-experimental quiz	68	72	66

Table B2: 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 B3: Summary statistics, Study 2...

	Exp. Condition / version					
Week 1						
	Control-1 / 'once'	Control-2 / 'once'	Treatment / 'once'	Control-1 / 'twice'	Control-2 / 'twice'	Treatment / 'twice'
Number of subjects	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
Average reward	£1.75	£1.75	£2.03	£1.75	£1.75	£1.81
Week 2						
Number of subjects	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
Average bonus	£1.75	£1.75	£1.66	£1.75	£1.75	£1.73

Appendix C: Additional results

Study 1

Dep. var.: P(effort task completed)				
	(whole sample)		*(filtered sample)*	
	(1)	(2)	(3)	(4)
Peer incentives	0.11***	0.10***	0.11***	0.09***
	(0.04)	(0.03)	(0.04)	(0.03)
Accuracy	0.19***	0.19***	0.19***	0.19***
	(0.03)	(0.03)	(0.04)	(0.03)
Age		−0.00		−0.00
		(0.00)		(0.00)
Female?		0.04		0.03
		(0.03)		(0.04)
US resident		−0.03		−0.03
		(0.06)		(0.06)
Num. obs.	2100	2070	2060	2030
Log Likelihood	−821.85	−768.78	−816.44	−763.66
Deviance	1643.70	1537.56	1632.88	1527.33
AIC	1649.70	1549.56	1638.88	1539.33
BIC	1666.65	1583.37	1655.77	1573.02

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

Table B4: Marginal effects, probit regression (baseline category: flat)

Dep. var.: P(effort task completed)								
	(logit)		*(logit, filtered)*		*(probit)*		*(probit, filtered)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Intercept)	0.92***	1.91**	0.92***	1.91**	0.57***	1.17**	0.57***	1.18**
	(0.22)	(0.86)	(0.22)	(0.87)	(0.13)	(0.48)	(0.13)	(0.49)
Accuracy	1.91***	2.15***	1.91***	2.15***	1.03***	1.13***	1.03***	1.13***
	(0.43)	(0.41)	(0.43)	(0.41)	(0.22)	(0.20)	(0.22)	(0.20)
Peer incentives	1.05***	0.96**	0.98***	0.89**	0.59***	0.54***	0.56***	0.51**
	(0.36)	(0.37)	(0.36)	(0.37)	(0.20)	(0.21)	(0.20)	(0.21)
Age		−0.04		−0.04		−0.02		−0.02
		(0.03)		(0.03)		(0.02)		(0.02)
Female?		0.37		0.33		0.19		0.17
		(0.33)		(0.33)		(0.18)		(0.18)
US resident?		−0.24		−0.19		−0.17		−0.14
		(0.65)		(0.65)		(0.33)		(0.34)

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

Table B5: Regression estimates (baseline: flat)

Study 2

	(week 1)		*(week 2)*	
	(1)	(2)	(3)	(4)
Control-2	0.05	0.05	−0.01	−0.00
	(0.04)	(0.04)	(0.04)	(0.04)
Treatment	0.11***	0.10***	0.08**	0.08**
	(0.03)	(0.03)	(0.04)	(0.04)
Age		−0.00*		−0.00
		(0.00)		(0.00)
Female?		0.02		−0.02
		(0.03)		(0.03)
UK citizen?		−0.00		0.03
		(0.03)		(0.04)
Num. obs.	1264	1264	1280	1280
Log Likelihood	−831.33	−829.47	−828.44	−826.92
Deviance	1662.67	1658.94	1656.88	1653.85
AIC	1668.67	1670.94	1662.88	1665.85
BIC	1684.09	1701.79	1678.34	1696.78

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

Table B6: Probit marginal effects

	Logistic				*Probit*			
	(week 1)		*(week 2)*		*(week 1)*		*(week 2)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Intercept)	−0.74***	−0.31	−0.70***	−0.55*	−0.46***	−0.19	−0.44***	−0.34**
	(0.10)	(0.33)	(0.11)	(0.28)	(0.06)	(0.20)	(0.07)	(0.17)
Control-2	0.22	0.19	−0.03	−0.01	0.13	0.12	−0.02	−0.01
	(0.16)	(0.16)	(0.16)	(0.16)	(0.10)	(0.10)	(0.10)	(0.10)
Treatment	0.47***	0.43***	0.33**	0.36**	0.29***	0.27***	0.20**	0.22**
	(0.13)	(0.13)	(0.16)	(0.16)	(0.08)	(0.08)	(0.10)	(0.10)
Age		−0.02*		−0.01		−0.01*		−0.01
		(0.01)		(0.01)		(0.01)		(0.00)
Female?		0.09		−0.10		0.05		−0.06
		(0.13)		(0.13)		(0.08)		(0.08)
UK citizen?		−0.01		0.15		−0.01		0.09
		(0.13)		(0.16)		(0.08)		(0.10)

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

Table B7: Logistic and probit regression estimates (baseline: control-1)