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# Speculation rather than enterprise? Keynes' beauty contest revisited in theory and experiment*

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**Abstract**

In Keynes' beauty contest, agents make evaluations reflecting both an expected fundamental value and the conventional value expected to be set by the market. They thus respond to fundamental and coordination motives, respectively, the prevalence of either being set exogenously. Our contribution is twofold. First, we propose a valuation game in which agents strategically choose how to weight each motive. This game emphasises how public information leads agents to favour the coordination motive. Second, we test the game through a laboratory experiment. Subjects tend to conform to theoretical predictions, except when fundamental uncertainty is low relative to strategic uncertainty.

**Keywords:** dispersed information, public information, beauty contest, coordination, experiment.

**JEL codes:** D84 - C92 - E12.

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# 1 Introduction

The beauty contest metaphor used by Keynes to characterise the working of financial markets displays the existence of a dual motivation in agents' decision making: there is a fundamental motive making agents strive to predict the fundamental value of some financial asset and there is a coordination motive making them seek to predict the conventional value eventually set by the market. There is no reason for the two values to coincide and, in Keynes' view, the working of stock markets, rather than imposing a balance between the two motives, tends to favour the coordination relative to the fundamental motive or, in Keynes' words, to favour speculation rather than enterprise. Indeed, professional investors and speculators are not so much concerned with forecasting fundamentals as with "anticipating what average opinion expects the average opinion to be" (Keynes, 1936, ch.12, p.156).[1]

The aim of our paper is to revisit Keynes' beauty contest in a setup that captures the choice to play a pure coordination game, and then to question whether the coordination motive dominates the fundamental motive when *homines sapientes* are involved instead of *homines œconomici*. Therefore, our contribution is twofold. First, from a theoretical point of view, we consider the trade-off between the coordination and the fundamental motives not as structural but as resulting from strategic decisions. Those decisions may then end up in the full eviction of the fundamental motive. Second, a natural way to test whether this theoretical disconnection of economic activities from fundamentals also emerges in practice is to bring the model to the lab. Our second contribution thus consists in running an experiment on our extended version of the beauty contest.

The valuation game is directly based on Morris and Shin (2002) (henceforth MS). In this famous representation of the Keynesian beauty contest, agents' actions consist in choosing a value which is a compromise between the anticipated fundamental value and the anticipated conventional value (the average of all the agents' actions). Under perfect information, agents can easily coordinate on the fundamental value, so that the fundamental and coordination motives coincide. By contrast, under imperfect information, agents receive public and private signals about the unknown fundamental value. Information being imperfect, agents have to form expectations on the fundamental, and information being dispersed, agents may find it difficult to coordinate. Dispersed information generates a conflict between matching the fundamental value and matching the conventional value, which expresses itself in an information cost. While the terms of the trade-off between the fundamental and the coordination motives are exogenously given in MS, we argue that players may be interested in manipulating the weights put on the fundamental and the coordination motives, for instance by choosing the shares of their portfolios they allocate to

[1]The prevalence of speculation over enterprise is implicitly stated by Keynes: "as the organisation of investment markets improves, the risk of the predominance of speculation does [...] increase. In one of the greatest investment markets in the world, namely, New York, the influence of speculation (in the above sense) is enormous. [...W]hen he purchases an investment, the American is attaching his hopes, not so much to its prospective yield, as to a favourable change in the conventional basis of valuation, i.e. [...] he is, in the above sense, a speculator."

investment and speculation, respectively. The exogeneity of the relative weight put on the coordination motive leaves open the issue of the potential disconnection of actions from the fundamental value. We extend MS model to a two-stage game, in which agents first choose the weight they attribute to the coordination and fundamental motives before making the choice of the value that best matches the preferred combination of fundamental and coordination motives. We show that there is an incentive for agents to favour the coordination over the fundamental motive. More precisely, we show that the coordination activity (speculation) prevails in the valuation game as coordination on a public signal entails a lower information cost than predicting an unknown fundamental. Information is the driving force for the coordination loss to be weaker than the fundamental loss: as agents put more weight on the coordination motive, they rely more on public information to estimate the average action, making it easier to coordinate on the convention. The strategic choice to privilege the convention results in the limit in a total disconnection between the valuation activity and the fundamental.

We test the theoretical predictions of the extended valuation game through an experiment. More precisely, we test whether – under dispersed information – human subjects prefer to choose a fundamental or a coordination motive and how much weight they put on the public signal depending on the game they chose to play. The experiment captures the impact of different informational contexts – by varying the precision and the nature (public or private) of information – on the choice to speculate and thus on the consequent disconnection of activities from fundamentals.

Overall, our experiment shows that subjects play in line with theoretical predictions in the sense that they more often choose to play the coordination game and put more weight on the coordination motive when they receive both public and private signals (than when they receive two private signals). Variations in the relative precision of public and private signals do not affect such a conclusion as subjects always put more weight on the coordination than on the fundamental motive at the first stage – except in the specific case where private information is very precise while public information is not – and they put a large weight on the public signal at the second stage, the more so the higher the (relative) precision of this signal. Because the fundamental is unknown, by choosing to ignore it (almost) entirely in their payoffs, subjects are able to eliminate (almost) all uncertainty by coordinating on the public signal, thereby maximizing their payoff. However, when private information is very precise, while public information is not, subjects prefer to choose the fundamental game, which contradicts theoretical predictions. While theory predicts that subjects should always choose the coordination game whatever the level of uncertainty,[2] fundamental uncertainty is so weak compared to strategic uncertainty that choosing the coordination game appears too costly, as it typically entails a coordination problem at the second stage and thus requires that the others play the public signal as well to maximise payoffs. Reducing the precision of public information (as is sometimes advocated in the

[2] In the limit case where fundamentals are perfectly known, multiple equilibria arise at the first stage of the game.

literature in the vein of MS) may not be suitable, as subjects still do choose to play the coordination game.[3]

Our paper contributes to the theoretical literature on beauty contest games initiated by Morris and Shin (2002)[4] and its experimental counterparts (Dale and Morgan (2012), Cornand and Heinemann (2014), Baeriswyl and Cornand (2014, 2016), and Shapiro *et al*. (2014)).[5] Our analysis mainly differs from theirs in that we treat the relative weights put on the coordination vs. fundamental motives not as exogenous, but instead as strategic variables. In this respect, our work relates to Cornand and Dos Santos Ferreira (2016) who treat the weights put on the fundamental and strategic motives as pertaining to the structure of a differentiated duopoly but allow firm owners to manipulate these weights.

The remaining of the paper is structured as follows. Section 2 presents the theoretical framework. Section 3 develops the experimental design and Section 4 the results. Finally, Section 5 concludes the paper.

# 2 Theoretical framework

MS introduce a valuation game in which agents' decisions have to meet both a fundamental and a coordination motive. Their actions consist in choosing a value as close as possible to the fundamental value and to the conventional value set by the market, according to a trade-off between the two motives. However, while the relative weight agents put on each motive of MS's valuation game is fully exogenous, we take it as a strategic variable. We therefore extend MS framework to consider a two-stage game in which agents first choose the weight they attribute to the coordination (and fundamental) motive(s) before making a decision. This model accounts for the potential disconnection between speculation and enterprise in a very simple manner.

## 2.1 A two-stage valuation game under different informational structures

There is a finite number $n$ of agents. The utility function for individual $i$ has two components. The first component is a standard quadratic loss in the distance between the underlying fundamental value $\theta$ and $i$'s chosen value (action) $a_i$. The second component is the 'beauty contest' term: the loss is increasing in the distance between $i$'s chosen value (action) $a_i$ and the average action $\frac{1}{n}\sum_j a_j$. Formally, the utility of agent $i$ is given by

[3]Except when the precision of the signal is zero (in the private signals treatments, subjects more often play the fundamental game).

[4]This literature largely expanded following Morris and Shin (2002) seminal contribution. See *e.g.* Angeletos and Pavan (2007).

[5]Alternative specifications of coordination games under dispersed information, such as the global game approach, have been experimentally tested. See *e.g.* Cabrales *et al.* (2007), Heinemann *et al.* (2004, 2009). Neither the global game approach nor the beauty contest games consider the issue of trading. For experimental evidence on the role of public and private information on market efficiency when private information aggregates into prices, see *e.g.* Ackert *et al.* (2204), Alfarano *et al.* (2011), and Middeldorp and Rosenkranz (2011).

$$u(\mathbf{a}, \theta; r_i) = -(1 - r_i) \underbrace{(a_i - \theta)^2}_{\text{fundamental motive}} - r_i \underbrace{\left(a_i - \frac{1}{n}\sum_j a_j\right)^2}_{\text{coordination motive}}, \tag{1}$$

where $\mathbf{a}$ is the action profile over all the agents and $r_i$ is the weight agent $i$ decides to put on the beauty contest term.[6]

The timing of the game is as follows. First, each agent $i$ chooses $r_i$: he evaluates which motive he favours to maximise his utility (he somehow chooses 'the game he wants to play'). Second, each agent $i$ chooses $a_i$: he evaluates how to exploit his information to decide on the value that matches the combination of motives he favoured.

Under *perfect information*, any agent $i$ would exactly know the fundamental value $\theta$ and choose at the second stage $a_i^* = \theta$, so that there would be no conflict between the fundamental and the coordination motives. As a consequence, $(\mathbf{r}, (\theta, ..., \theta))$ would be a subgame perfect equilibrium for any profile $\mathbf{r}$.

Under *imperfect* but *homogeneous information*, diffused for instance by a noisy public signal $y$ received by all agents between the two stages of the game, there would typically be some fundamental loss, but no coordination loss. Subgame perfection would then impose the choice $r_i = 1$ at the first stage, allowing to get a zero loss with certainty at the second stage (with $\mathbf{a} = (y, ..., y)$), and potentially disconnecting the equilibrium actions from the fundamental. In this context, the public signal might well be biased (and not only noisy) without changing the equilibrium payoffs. In other words, any *sunspot* would indifferently perform its well-known coordinating role as soon as $\mathbf{r} = (1, ..., 1)$.

Following the literature in the vein of MS, we shall however assume *imperfect* and *heterogeneous* (or *dispersed*) *information*. Between the two stages of the game, each agent $i$ receives two signals on the unknown fundamental value $\theta$. All agents receive an unbiased public signal with a normally distributed error term: $y = \theta + \eta$, with $\eta \sim \mathcal{N}(0, 1/\alpha)$. Each agent $i$ receives in addition an unbiased private signal: $x_i = \theta + \varepsilon_i$, with $\varepsilon_i \sim \mathcal{N}(0, 1/\beta)$, the $\varepsilon_i$'s being identically and independently distributed across agents and independently distributed with respect to $\eta$. Thus, conditionally on the two signals $y$ and $x_i$ received by agent $i$, his expected value of the fundamental is a weighted arithmetic mean of those signals, with weights proportional to the corresponding precisions $\alpha$ and $\beta$: $\mathbb{E}(\theta \mid x_i, y) = (\alpha y + \beta x_i)/(\alpha + \beta)$.

As the signal $y$ is public, it conveys information not only on the fundamental, but also on other agents' actions. Should the two signals, say $y_i$ and $x_i$ be both private, agent $i$ would have no information on others' actions, about which he would be doomed to form the *same* expectation as about the fundamental: $\mathbb{E}(\theta \mid x_i, y_i) = (\alpha y_i + \beta x_i)/(\alpha + \beta) = \mathbb{E}(a_j \mid x_i, y_i)$.

[6] MS take a third motive into account: each agent wants to choose an action close to the average action, but would also like to succeed better than the others. In MS framework, in which the set of agents is a continuum, this *competition motive* appears as an externality: it influences the agents' welfare, not their decisions. This is not the case in our context, as we consider a finite number of agents and, in addition, a two-stage game. However, we have preferred to ignore this motive in order to simplify the task of the participants to the experiment.

As in the perfect information case, there would be no conflict between the fundamental and the coordination motives (any profile $\mathbf{r}$ decided at the first stage would do), even if the two losses would now be positive.

So, let us keep one signal public and the other private. We further assume that $\alpha > 0$ and $\beta < \infty$, so that the public signal never ceases to be informative and the private signal never becomes fully informative on the fundamental. These assumptions insure that the public signal is always relevant (on the fundamental).

## 2.2 Subgame perfect equilibrium under dispersed information

We solve the model backwards, starting by the second stage and taking the $r_i$'s as given. The solution to the maximization problem, conditional on the two signals received by any agent $i$, namely $\max_{a_i} \mathbb{E}\left(u\left(\mathbf{a}, \theta; r_i\right) \mid x_i, y\right)$, is given by

$$
\begin{aligned}
a_i^* &= \underbrace{\frac{\alpha}{\alpha+\beta}\left(1+\frac{\beta R_i}{\alpha+\beta\left(1-R\right)}\right)}_{\kappa_i} y + \underbrace{\frac{\beta}{\alpha+\beta}\left(1-\frac{\alpha R_i}{\alpha+\beta\left(1-R\right)}\right)}_{1-\kappa_i} x_i, \qquad (2)\\
\text{with } R_i &\equiv \frac{\left(1-1/n\right) r_i}{1-\left(1/n\right) r_i} \text{ and } R \equiv \frac{1}{n}\sum_j R_j
\end{aligned}
$$

(see Appendix 7.1 for details about the derivation). Thus, the second stage equilibrium is an action profile $\mathbf{a}^*\left(\mathbf{r}\right)$ depending on the profile of the weights $r_i$'s chosen by each agent $i$ at the first stage. For any agent $i$, the equilibrium value $a_i^*\left(r_i, \mathbf{r}_{-i}\right)$ is an arithmetic mean of the two signals $y$ and $x_i$ received by that agent, with relative weights depending on $r_i$, directly through a monotonic transformation $R_i$ of $r_i$ and indirectly through the mean $R$ of the $R_j$'s. Naturally, the relative weight $\kappa_i$ put on the public signal increases with the relative precision $\alpha/\beta$ of the public signal and with the relative weight $r_i$ attributed to the coordination motive.

To derive the subgame perfect equilibrium, we maximise with respect to $r_i$ the expected utility of agent $i$, namely

$$
\mathbb{E}\left(u\left(\mathbf{a}^*\left(\mathbf{r}\right), \theta; r_i\right)\right) = \frac{1}{\alpha}\left[\begin{array}{c} -\left(1-r_i\right)\left(\kappa_i^2+\frac{\left(1-\kappa_i\right)^2}{\beta/\alpha}\right) \\ -r_i\left(\left(\kappa_i-\kappa\right)^2+\frac{1-1/n}{\beta/\alpha}\left(\left(1-\frac{1}{n}\right)\left(1-\kappa_i\right)^2+\frac{1}{n}\frac{1}{n-1}\sum_{j\neq i}\left(1-\kappa_j\right)^2\right)\right)\end{array}\right], \qquad (3)
$$

with $\kappa = \frac{1}{n}\sum_j \kappa_j$ (see Appendix 7.2 for details about the derivation). By referring to equation (2) and as shown in this appendix, we see that the expression between square brackets is a function $U\left(r_i, R_{-i}^1, R_{-i}^2, n, \beta/\alpha\right)$, where $R_{-i}^1 = \frac{1}{n-1}\sum_{j\neq i} R_j$ and $R_{-i}^2 = \frac{1}{n-1}\sum_{j\neq i} R_j^2$ are the first and second moments (about the origin) of the $R_j$'s of all agents other than $i$.

The limit case of an infinite number of agents allows a simple derivation of the subgame perfect equilibrium. Indeed, in this case the function $U$ no more depends upon $R_{-i}^2$, the means $\kappa$ and $R$ cease to depend upon the choice of $r_i$, and $R_i = r_i$. The expected utility then becomes

$$\mathcal{U}\left(r_i, \kappa_i\left(r_i\right)\right) = \frac{1}{a}\left[-\frac{\left(1-\kappa_i\right)^2}{\beta/\alpha} - \kappa_i^2 + r_i\kappa(2\kappa_i - \kappa)\right], \tag{4}$$

and its derivative with respect to $r_i$ (using $\kappa = 1/\left(1+(\beta/\alpha)(1-r)\right)$, with $r = \frac{1}{n}\sum_j r_j$)

$$\frac{d\mathcal{U}\left(r_i, \kappa_i\left(r_i\right)\right)}{dr_i} = \frac{\kappa^2}{\alpha}\left[\frac{2}{1+\beta/\alpha}\left(\frac{1}{\kappa} + (\beta/\alpha)\, r_i\right) - 1\right], \tag{5}$$

an increasing function of $r_i$. The function $\mathcal{U}\left(\cdot, \kappa_i(\cdot)\right)$ is consequently strictly convex, and can only be maximised at $r_i = 0$ or at $r_i = 1$. Using $\kappa_i(1) = \left(1 + (\beta/\alpha)\,\kappa\right)/\left(1+\beta/\alpha\right)$ and $\kappa_i(0) = 1/\left(1+\beta/\alpha\right)$, we may establish that

$$\mathcal{U}\left(1, \kappa_i(1)\right) - \mathcal{U}\left(0, \kappa_i(0)\right) = \frac{\kappa\,(2-\kappa)}{a\left(1+\beta/\alpha\right)} > 0, \tag{6}$$

so that the function $\mathcal{U}\left(\cdot, \kappa_i(\cdot)\right)$ is always maximised at $r_i = 1$, whatever the profile $\mathbf{r}_{-i}$ of the weights chosen by the other agents. We can accordingly formulate the following proposition.

**Proposition** *In the limit case of an infinite number of agents, there is a unique subgame perfect equilibrium in dominant strategies* $\left((1, \ldots, 1), (y, \ldots, y)\right)$, *such that all the agents choose at the first stage to play the coordination game and at the second stage to coordinate their actions on the public signal.*

Notice that this result, stemming from the fact that the fundamental loss is always heavier than the coordination loss, which is an incentive to put all the weight on the latter, is not trivial since the two losses depend themselves on the weight $r_i$, through the decisions this weight induces at the second stage of the game. In Appendix 7.3, we show that this result is carried over to a game with a finite number of agents, at least for the parameter values that have been selected for the experiment.

To conclude, we obtain the following predictions to be tested by our experiment.

**Theoretical predictions**

1. *As to the second stage of the game, the higher the relative precision of the public signal and the higher the relative weight on the coordination motive, the more weight agents put on the public signal (see equation (2)).*

2. *As to the first stage, agents put all the weight on the coordination motive* ($\mathbf{r} = (1, \ldots, 1)$)*, which is a dominant strategy, whatever the relative precisions of the public and private signals. This implies that agents all choose to coordinate their actions on the public signal at the second stage* ($\mathbf{a} = (y, \ldots, y)$)*.*

3. *By contrast, the profile of the relative weights put on the two motives at the first stage is arbitrary in the case where both signals are private. At the second stage, the weight on the*

*public signal when agents receive public and private signals is higher than the weight put on any of the two private signals when agents receive two private signals.*

# 3 The experiment

One may question whether the theoretical predictions derived in Section 2 hold in practice, when *homines sapientes* are involved in the valuation game instead of *homines œconomici*. Recurring to a laboratory experiment represents a natural way to test these assumptions, as real data may be difficult to collect and analyse.[7] The theoretical model in Section 2 is adjusted to an experimental framework. We discuss in this section the chosen parameter values for each treatment, the corresponding theoretical prediction, and the general procedure of the experiment.

## 3.1 Treatment parameters and equilibrium values

We conducted 14 sessions (2 per treatment) with a total of 252 participants. In each session, 18 participants were separated into 3 independent groups of 6 participants (in order to get 6 independent observations per treatment).

### 3.1.1 Adjusted theoretical predictions to a finite number of participants

We focus on the parameter values $n = 6$ and $\beta/\alpha \in \{1/8, 1/2, 1, 2, 8\}$, which correspond to the cases we deal with in the different treatments of our experiment, as explained in Section 3.1.3. These parameter values ensure that we obtain the same theoretical predictions as in the theoretical framework of Section 2 when $n \to \infty$. As a higher mean square $R^2_{-i}$ together with an unchanged mean $R^1_{-i}$, reflecting more dispersion in other agents' decisions, is unfavourable to coordination, discouraging the choice of a high $r_i$, we always consider the worst case for $r_i$ (the highest value of $R^2_{-i}$ compatible with a given value of the mean $R^1_{-i}$). In spite of that, the simulations presented in Appendix 7.3 show that the expected utility is always maximised at $r_i = 1$ for the selected parameter values and whatever the values of $R^1_{-i}$ and $R^2_{-i}$. We thus obtain a unique subgame perfect equilibrium in dominant strategies, such that all agents choose to play the coordination game rather than the fundamental one or any mixture of the two.

### 3.1.2 Description of sessions

Each session consisted of 3 games, which amounted to a total of 35 periods. The first two games (5 periods each) were intended to familiarise subjects with the experiment and are considered as an incentivised training. Participants played within the same group during

[7] This is especially the case because precisely knowing what a fundamental value is may represent a difficult task and because private information is by definition not available in practice.

the whole length of the experiment and did not know the identity of the other participants of their group.

In every period, and for each group, a fundamental state $Z$ was drawn randomly using a uniform distribution from the interval $[50, 950]$.[8] Each period was divided into two sub-periods. In the first sub-period, subjects had to choose an integer between $0$ and $10$ in order to decide how much weight they wanted to attribute to the coordination motive of their utility function (decision $D_1$). Then first sub-period outcomes were revealed and the second sub-period started. In the second sub-period, each participant had to decide on a decision $D_2$ by moving a cursor only inside the interval, whose bounds were the minimum of the two signals received on the fundamental minus two standard deviations of this signal and the maximum of the signals plus two standard deviations of this signal.[9] Indeed, to make their decision $D_2$, in game 3 participants would receive 2 signals, that depend on treatments, as explained below. Participants also had to form estimations depending on the game of the experiment. Indeed, participants had to provide their best estimation $E_1$ of the fundamental and their best estimation $E_2$ of the average decision $\bar{D_2}$ of all participants of the same group.

The payoff in ECU (Experimental Currency Units) associated with participant $i$'s decisions $D_1$ and $D_2$ is given by the formula:

$$400 - (10 - D_1)(D_2 - Z)^2 - D_1(D_2 - \overline{D}_2)^2. \tag{7}$$

The payoff in ECU associated with participant $i$'s estimation $E_1$ is given by:

$$200 - (E_1 - Z)^2, \tag{8}$$

and that with participant $i$'s estimation $E_2$:

$$200 - (E_2 - \bar{D_2})^2. \tag{9}$$

In game 1 (5 periods), after the first sub-period, the realised value of $Z$ is commonly revealed. In this game, no estimation is asked for and subjects are simply rewarded according to (7). The interval for decisions is $[0, 2000]$. In game 2 (5 periods), after the first sub-period, a common value $s \in [0, 2000]$ independent from the unknown number $Z$ is sent to all participants. It corresponds to a sunspot. In this game, an estimation $E_2$ is asked for

[8]Note that participants were not told about the support of the distribution to avoid the skewness of the posterior distribution.

[9]The second sub-period was very similar to the experiments by Cornand and Heinemann (2014) and Baeriswyl and Cornand (2014, 2016), which aimed at testing variations of the beauty contest game of MS. The design was slightly modified as, contrary to Baeriswyl and Cornand, we allowed subjects to make choices outside the interval defined by the two signals participants received on the fundamental. Instead, and differently from Cornand and Heinemann, who made a restriction of possible choices on the interval defined by the public signal minus or plus 20 and observed many decisions outside the interval defined by the signals, we proposed a screen design that emphasised the position of signals on the interval of possible choices. See the example of screens provided in Appendix 7.5. This ensured that subjects mostly played inside the range defined by the signals, without too much constraining their choices. As will be underlined later on in the paper, we indeed observed only few decisions outside the range defined by the two signals.

so that participants are rewarded according to both (7) and (9). The interval for decisions is also $[0, 2000]$. In half of the sessions, for each treatment, we reversed the order of games 1 and 2.[10] Game 3 took place afterwards. In this game, $Z$ was unknown but after the first sub-period, subjects received signals on $Z$, whose nature (public or private) and precision depended on the treatment. In this game, $D_1$, $D_2$, $E_1$, and $E_2$ were rewarded according to (7), (8), and (9).

#### 3.1.3 Treatments

In the third game, we considered the 7 following treatments:

**Treatments 1 and 7 - Public vs. private signals, same precision** Each participant receives a private and a public signal. The private signal received by each participant is distributed as $x_i = Z + \varepsilon_i$ with $\varepsilon_i \sim N(0, \sigma_\varepsilon^2)$. The public signal received by every participant of each group is distributed as $y = Z + \eta$ with $\eta \sim N(0, \sigma_\eta^2)$. Whereas each participant may receive a different private signal $x_i$, the public signal $y$ is the same for all participants. In Treatment 1, $\sigma_\varepsilon^2 = \sigma_\eta^2 = 8$. In Treatment 7, $\sigma_\varepsilon^2 = \sigma_\eta^2 = 1$.

**Treatments 2 and 5 - Public vs. private signals, the public signal being more precise than the private one** These treatments are the same as Treatment 1, except that the public signal is more precise than the private one. In Treatment 2, $\sigma_\varepsilon^2 = 8$ and $\sigma_\eta^2 = 1$. In Treatment 5, $\sigma_\varepsilon^2 = 16$ and $\sigma_\eta^2 = 8$.

**Treatments 3 and 4 - Public vs. private signals, the private signal being more precise than the public signal** These treatments are the symmetric of Treatments 2 and 5. In Treatment 3, $\sigma_\varepsilon^2 = 1$ and $\sigma_\eta^2 = 8$. In Treatment 4, $\sigma_\varepsilon^2 = 8$ and $\sigma_\eta^2 = 16$.

**Treatment 6 - Private vs. private signals, same precision** Each participant receives 2 private signals on $Z$. Each of the 2 private signals may have a different distribution: $x_{i1} = Z + \varepsilon_{i1}$ with $\varepsilon_{i1} \sim N(0, \sigma_{\varepsilon 1}^2)$ and $x_{i2} = Z + \varepsilon_{i2}$ with $\varepsilon_{i2} \sim N(0, \sigma_{\varepsilon 2}^2)$. The private signals may thus be different from one participant to the next. In Treatment 6, $\sigma_{\varepsilon_1}^2 = \sigma_{\varepsilon_2}^2 = 8$.

#### 3.1.4 Summary

The choice of parameters for the experiment is summarised in Table 1 that also presents the corresponding theoretical predictions.

We will proceed to comparisons between observations and theoretical values as well as treatment comparisons. Comparing Treatments 1 and 6 directly allows to account for the role of the public signal. Comparing Treatment 1 and any of the Treatments 2 to 5 and

[10]More precisely, the motivation behind the first two games was to raise participants' awareness about the fundamental value $Z$ (game 1) and about common information (game 2), while keeping these games sufficiently different from game 3.

| Tr. | Game 3: Signals distributions | $\mathbb{E}_i(\theta)$ | $\mathbb{E}_i(\bar{a})$ | $a_i^*$ | $r_i^*$ |
|---|---|---|---|---|---|
| 1 | $y \sim N(Z,8)$, $x_i \sim N(Z,8)$ | $\frac{x_i+y}{2}$ | $y$ | $y$ | 1 |
| 2 | $y \sim N(Z,1)$, $x_i \sim N(Z,8)$ | $\frac{8y+x_i}{9}$ | $y$ | $y$ | 1 |
| 3 | $y \sim N(Z,8)$, $x_i \sim N(Z,1)$ | $\frac{y+8x_i}{9}$ | $y$ | $y$ | 1 |
| 4 | $y \sim N(Z,16)$, $x_i \sim N(Z,8)$ | $\frac{y+2x_i}{3}$ | $y$ | $y$ | 1 |
| 5 | $y \sim N(Z,8)$, $x_i \sim N(Z,16)$ | $\frac{2y+x_i}{3}$ | $y$ | $y$ | 1 |
| 6 | $x_{i1} \sim N(Z,8)$, $x_{i2} \sim N(Z,8)$ | $\frac{x_{i1}+x_{i2}}{2}$ | $\frac{x_{i1}+x_{i2}}{2}$ | $\frac{x_{i1}+x_{i2}}{2}$ | $\{0,...1\}$ |
| 7 | $y \sim N(Z,1)$, $x_i \sim N(Z,1)$ | $\frac{x_i+y}{2}$ | $y$ | $y$ | 1 |

Table 1: Experiment parameters and theoretical predictions

Treatment 7 to Treatments 2 and 3 allows to evaluate the role of increasing/decreasing the precision of either public or private signals.

### 3.2 Procedure

Sessions were run between June and November 2016 at the LEES (Laboratoire d'Economie Expérimentale de Strasbourg). Each session had 18 participants who were mainly students from the University of Strasbourg (most were students in economics and sciences) and were recruited through ORSEE.[11] Subjects were seated in random order at PCs. Instructions were then read aloud and questions answered in private. An example of instructions is given in Appendix 7.4. Throughout the sessions, students were not allowed to communicate with one another and could not see each others' screens. Each subject could only participate in one session. Before starting the experiment, subjects were required to answer a few questions to ascertain their understanding of the rules.[12] The experiment started after all subjects had given the correct answers to these questions.

After each period, subjects received some feedback about realised values and choices.[13] Information about past periods from the same game was displayed during the decision phase on the lower part of the screen. At the end of each session, the ECU earned were summed up and converted into euros. A single period for each of games 1 and 2 was randomly selected to be paid; five periods for game 3 were randomly selected. 1000 ECU were converted to 6 euros. The average payoff was about 25 euros. Sessions lasted for around 2 hours and 15 minutes.

[11]ORSEE is a web-based Online Recruitment System for Economic Experiments developed by Greiner (2015). The program of this experiment was designed with the web platform EconPlay (www.econplay.fr).

[12]The understanding questionnaire is available from the authors upon request.

[13]In game 3, they were informed about their own choice for $D_1$, the choice for $D_1$ of all other participants in their group, their own private hint $X_i$, the common hint $Y$, the true value of $Z$, their own estimation $E_1$ on $Z$, their own estimation $E_2$ on the average decision $D_2$ in the group, their own decision $D_2$, the average decision $D_2$ in the group, the distance between the average $D_2$ in the group and $Y$, the distance between the average $D_2$ in the group and $X$, their payoff associated with $E_1$, their payoff associated with $E_2$, their payoff associated with $D_1$ and $D_2$, and the overall payoff for the period.

# 4 Experimental results

The results of the experiment concerning game 3 are presented in the following manner.[14] First, we analyse the first stage decision before focusing on the second stage decision. Third, we check the coherence between first and second stage decisions. Statistical tests are based on Mann-Whitney tests for between treatment comparisons and Wilcoxon rank test when comparing observed data to theoretical predictions. All test results ($p$-values) are reported in Appendix 7.7.[15]

## 4.1 Playing the fundamental or the coordination game? An analysis of $D_1$

The first question we address is whether participants chose to play the fundamental or the coordination game. Table 2 presents the average weight $\frac{D_1}{10}$ participants attributed to the coordination motive in the experiment in each group for each treatment. In order to get a full picture of first stage decisions, Figure 1 depicts the relative frequency of weights put by each participant on the coordination motive for each treatment.[16]

| Tr. | 1 ($\sigma_\eta^2 = \sigma_\varepsilon^2 = 8$) | 2 ($\sigma_\eta^2 = 1, \sigma_\varepsilon^2 = 8$) | 3 ($\sigma_\eta^2 = 8, \sigma_\varepsilon^2 = 1$) | 4 ($\sigma_\eta^2 = 16, \sigma_\varepsilon^2 = 8$) | 5 ($\sigma_\eta^2 = 8, \sigma_\varepsilon^2 = 16$) | 6 ($\sigma_{\varepsilon_1}^2 = 8, \sigma_{\varepsilon_2}^2 = 8$) | 7 ($\sigma_\eta^2 = \sigma_\varepsilon^2 = 1$) |
|---|---|---|---|---|---|---|---|
| Gr. 1 | 0.66 | 0.01 | 0.54 | 0.88 | 0.86 | 0.73 | 0.24 |
| Gr. 2 | 0.71 | 0.55 | 0.06 | 0.58 | 0.85 | 0.10 | 0.31 |
| Gr. 3 | 0.80 | 0.82 | 0.20 | 0.79 | 0.72 | 0.50 | 0.68 |
| Gr. 4 | 0.65 | 0.68 | 0.79 | 0.26 | 0.46 | 0.37 | 0.43 |
| Gr. 5 | 0.56 | 0.32 | 0.26 | 0.54 | 0.51 | 0.34 | 0.75 |
| Gr. 6 | 0.40 | 0.99 | 0.00 | 0.22 | 0.50 | 0.04 | 0.77 |
| Av. | 0.63 | 0.56 | 0.31 | 0.54 | 0.65 | 0.35 | 0.53 |
| Th. | 1 | 1 | 1 | 1 | 1 | $\{0, ...1\}$ | 1 |

Table 2: Average weight on the coordination motive

Obviously, the average weight put on the coordination motive is different from theoretically predicted. Indeed, when participants received both public and private signals, they attributed a lower weight to the coordination motive than the full theoretical weight of 1.[17] However, subjects played in line with theoretical predictions in the sense that they more often chose to play the coordination game (Figure 1) and put more weight on the coordination motive (Table 2) when they received both public and private signals than when they

[14]Descriptive statistics for games 1 and 2 are reported in Appendix 7.6. Outcomes for games 1 and 2 show that participants properly understood the instructions and the games. In game 1, participants mostly chose $D_1 = 0$ and in 93% of cases over the 7 treatments, they played the fundamental as decision $D_2$. In game 2, participants mostly played $D_1 = 10$ and in 56% of cases over the 7 treatments they played the sunspot.

[15]We also performed the same analysis and tests by considering only the last 10 periods of the experiment (game 3). Results are unchanged. This robustness analysis is available from the authors upon request.

[16]The relative frequency of weights on the coordination motive per group is reported in Appendix 7.8.

[17]When participants received two private signals, the theoretical weight on the coordination motive is indeterminate. The weight selected by participants is generally relatively low, although the variance from one group to the next is high.

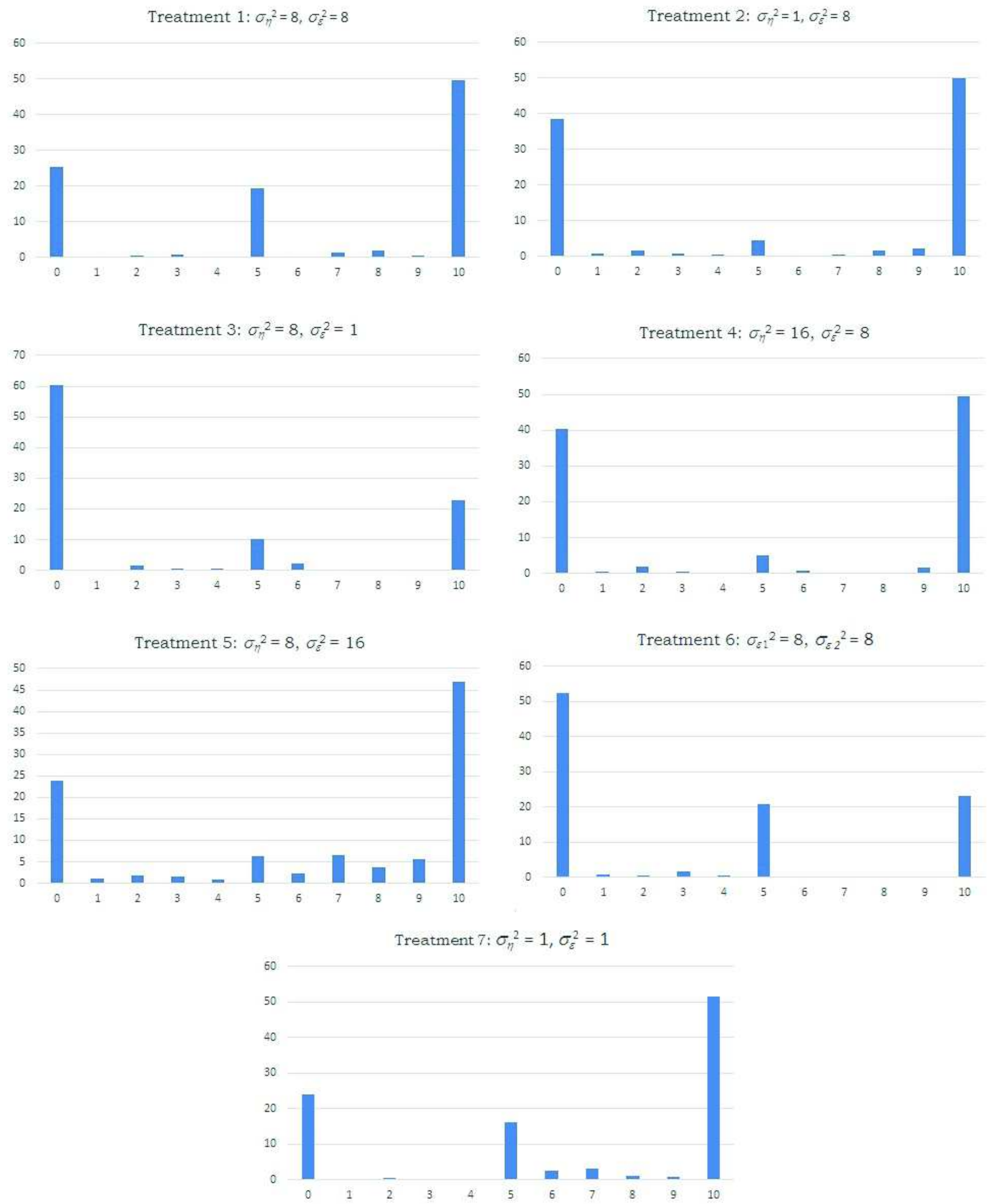


Figure 1: Relative frequency of weights $D_1$ on the coordination motive per treatment

received two private signals. As shown in Table 25 in Appendix 7.7, there is a significant difference between Treatments 1 and 6.

How do variations in the relative precision of public and private signals affect such a conclusion? As shown in Table 25 in Appendix 7.7, the only significant differences between treatments concern Treatments 1 vs. 3 and Treatments 3 vs. 5. The main driver for variations in the choice of $D_1$ is therefore fundamental vs. strategic uncertainty.

When private information is not very precise, so that fundamental uncertainty is rather large in comparison to strategic uncertainty (Treatments 1, 2, 4, and 5), variations in the relative precision of public and private information do not mitigate the fact that subjects put more weight on the coordination rather than the fundamental motive at the first stage of the game. Because the fundamental is unknown, they chose to ignore it in order to be rewarded more in accordance with the pure coordination game.

By contrast, when private information is very precise, so that fundamental uncertainty is weak in comparison to strategic uncertainty, subjects rather chose the fundamental game (Treatment 3). This observation contradicts theoretical predictions. Indeed, according to the theory, subjects should choose the coordination game whatever the level of uncertainty (as long as there is uncertainty). In the lab, fundamental uncertainty seemed so low to participants in comparison to strategic uncertainty, that choosing the coordination game appeared too costly, as it typically entails a coordination problem at the second stage.

**Result 1** *When fundamental uncertainty is high in comparison to strategic uncertainty, as theoretically predicted, subjects tend to put more weight on the coordination than on the fundamental motive at the first stage of the game. When fundamental uncertainty is low in comparison to strategic uncertainty, contrary to theoretical predictions, subjects tend to favour the fundamental rather than the coordination motive at the first stage of the game.*

## 4.2 Is the public signal a focal point? An analysis of $D_2$

The second question we address is whether subjects focus on the public signal. In Appendix 7.10, we check subjects' rationality by considering whether subjects played inside the interval defined by the signals they received. As only few decisions were outside this interval, and because we want to define a weight on the public signal, we keep only decisions inside this interval for our analysis.[18]

The average weight assigned in the experiment to the public signal in $D_2$ is reported in Table 3 for each treatment and each group. Theoretical$|_{th.stage1}$ denotes the theoretical weight on the public signal conditional on the theoretical weight in the first stage decision $r$. Theoretical 1st order denotes the theoretical weight on the public signal in the theoretical first order expectation on the fundamental, while Av. Observed 1st order denotes the average weight on the public signal in the observed first order expectation on the fundamental $E_1$.[19] We start our analysis by comparing the observed weight on the public signal to the theoretical weight on the public signal, before proceeding to a treatment comparison.

[18] In Appendix 7.10, we also show the optimality of decisions $D_2$ by checking that observed estimations are close to theoretical values of estimations and that observed $D_2$ is appropriate conditional on the estimations $E_1$ and $E_2$ made on an individual basis.

[19] The weights on the public signal in the observed first order expectation $E_1$ on the fundamental per group are reported in Table 28 in Appendix 7.10.

| Treatment | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
|---|---|---|---|---|---|---|---|
| Group 1 | 0.68 | 0.54 | 0.59 | 0.87 | 0.78 | 0.50 | 0.53 |
| Group 2 | 0.62 | 0.79 | 0.49 | 0.55 | 0.90 | 0.50 | 0.46 |
| Group 3 | 0.71 | 0.78 | 0.28 | 0.76 | 0.59 | 0.49 | 0.55 |
| Group 4 | 0.54 | 0.71 | 0.59 | 0.31 | 0.59 | 0.51 | 0.68 |
| Group 5 | 0.60 | 0.74 | 0.23 | 0.53 | 0.72 | 0.49 | 0.76 |
| Group 6 | 0.62 | 0.97 | 0.37 | 0.45 | 0.79 | 0.49 | 0.61 |
| Average | 0.63 | 0.76 | 0.43 | 0.58 | 0.73 | 0.50 | 0.60 |
| Theoretical 1st order | 0.50 | 0.89 | 0.11 | 0.33 | 0.67 | 0.50 | 0.50 |
| Av. Observed 1st order | 0.52 | 0.69 | 0.30 | 0.36 | 0.66 | 0.50 | 0.57 |
| Theoretical$\|_{th.stage1}$ | 1 | 1 | 1 | 1 | 1 | - | 1 |

Table 3: Weight on the public signal $Y$ in $D_2$

Obviously, when participants received both public and private signals, the theoretical benchmark conditional on the theoretical first stage decision (Theoretical$|_{th.stage1}$) can be rejected.[20] Note however that when participants got two private signals, they put equal weight on each, in line with theoretical predictions.

Nevertheless, Table 3 shows that the weight participants attributed to the public signal – when they received both public and private signals – is higher than the weight they put on any of the signals when they received two private signals. As shown in Table 26 in Appendix 7.7, there is a significant difference between Treatments 1 and 6. To get a more complete picture of the focal role of the public signal, Figure 2 presents the relative frequency of weights on the public signal per treatment. The higher the relative precision of the public signal, the more subjects played the public signal itself at the second stage.[21] Table 3 also shows that the higher the relative precision of the public signal, the higher the weight on the public signal. Indeed, as shown in Table 26 in Appendix 7.7, there is a significant difference between Treatments 1 and 2, 1 and 3, Treatments 3 and 5, and Treatments 2 and 7 and 3 and 7. The difference in the relative precision of the public signal needs to be sufficiently strong though to generate significant differences between treatments (there is no significant difference between Treatments 1 and 4, and 1 and 5). These effects go in the sense of the theory, as when $r \neq 1$, an increase in $r$ implies a larger weight on the public signal. All these results are confirmed on individual data (see Appendix 7.11).

**Result 2** *The public signal plays a focal role. First participants attribute a large weight to the public signal when they receive private and public signals in comparison to the weight they attribute*

[20] There is overreaction to the public signal in the sense that participants to the experiment attributed a larger weight to the public signal in their decision $D_2$ than in their stated first order expectation on the fundamental (Observed 1st order) (see Table 23 in Appendix 7.7 for Treatments 1 and 4). Indeed, following Baeriswyl and Cornand (2016), overreaction is observed when comparing the observed weight on the public signal to the weight in the stated first order expectation and not necessarily to the theoretical weight in the first order expectation (Theoretical 1st order) (see Table 22 in Appendix 7.7). Experimental overreaction to public information has been largely documented in Cornand and Heinemann (2014) and Baeriswyl and Cornand (2014, 2016). We therefore do not comment much upon this issue, which is not the main focus of the current paper.

[21] Appendix 7.9 depicts the relative frequency of weight on $Y$ in $D_2$ for each group.


Figure 2: Relative frequency of weights on $Y$ in $D_2$

*to a private signal in a treatment where they receive two private signals. Second, in line with theory at the second stage conditional on $r \neq 1$, the higher the relative precision of the public signal, the more weight participants put on the public signal.*

### 4.3 Is there coherence between decisions $D_1$ and $D_2$?

The third question we address is whether there is a coherence between observed first stage and second stage decisions. To answer this question, we proceed in two steps. First, we analyse whether the weight put on the public signal in decision $D_2$ better coincides with the theoretical weight on the public signal once accounting for the stated first stage decision. Second, we look at whether subjects put a larger weight on the public signal in their second stage decision $D_2$ when they choose to be rewarded more by the coordination motive at the first stage.

Table 4 proposes an alternative theoretical benchmark for assessing the weight put on the public signal in decision $D_2$, conditional on stated decisions $D_1$. The comparison between observed weights on the public signal and the second stage theoretical value conditional on the observed first stage decision $D_1$ reveals a better fit than the unconditional second stage theoretical value (as analysed in section 4.2.). Indeed, as shown in Table 24 in Appendix 7.7, the theoretical benchmark cannot be rejected in Treatments 4 and 7.

| Tr. | 1 ($\sigma_\eta^2 = \sigma_\varepsilon^2 = 8$) | 2 ($\sigma_\eta^2 = 1, \sigma_\varepsilon^2 = 8$) | 3 ($\sigma_\eta^2 = 8, \sigma_\varepsilon^2 = 1$) | 4 ($\sigma_\eta^2 = 16, \sigma_\varepsilon^2 = 8$) | 5 ($\sigma_\eta^2 = 8, \sigma_\varepsilon^2 = 16$) | 7 ($\sigma_\eta^2 = \sigma_\varepsilon^2 = 1$) |
|---|---|---|---|---|---|---|
| Gr. 1 | 0.73 | 0.89 | 0.20 | 0.78 | 0.92 | 0.56 |
| Gr. 2 | 0.75 | 0.94 | 0.12 | 0.52 | 0.92 | 0.58 |
| Gr. 3 | 0.81 | 0.97 | 0.13 | 0.67 | 0.86 | 0.73 |
| Gr. 4 | 0.72 | 0.96 | 0.34 | 0.39 | 0.77 | 0.62 |
| Gr. 5 | 0.67 | 0.92 | 0.14 | 0.50 | 0.79 | 0.78 |
| Gr. 6 | 0.61 | 1.00 | 0.11 | 0.38 | 0.79 | 0.79 |
| Av. | 0.71 | 0.95 | 0.17 | 0.54 | 0.84 | 0.68 |

Table 4: Weight on the public signal $Y$ in $D_2$ conditional on observed $D_1$

To address the question whether subjects put a larger weight on the public signal in their second stage decision $D_2$ when they choose to be rewarded more by the coordination motive at the first stage, we resort to an analysis on individual data and estimate the following equation, both for each treatment[22] and for all the treatments taken together[23]:

$$\left| \frac{D_{2it} - X_{it}}{Y_t - X_{it}} \right| = Co + \alpha \frac{D_{1it}}{10} + (\epsilon_{it} + \nu_i), \tag{10}$$

where $Co$ is the constant, $D_{1it}$ is the decision 1 of individual $i$ at period $t$ and $\alpha$ the estimated coefficient; $\nu_i + \epsilon_{it}$ is the error term ($\nu_i$ is the individual specific error term and $\epsilon_{it}$ is

[22]Clusters were used for groups.
[23]Clusters were used for both treatments and groups.

the idiosyncratic error term).

| | All | Tr1 | Tr2 | Tr3 | Tr4 | Tr5 | Tr6 | Tr7 |
|---|---|---|---|---|---|---|---|---|
| Constant | 0.5477*** | 0.5224*** | 0.6933*** | 0.3688*** | 0.4970*** | 0.6892*** | 0.4963*** | 0.5697*** |
| | (0.0226) | (0.0205) | (0.0443) | (0.0431) | (0.0634) | (0.0478) | (0.0068) | (0.0444) |
| $\frac{D_{1it}}{10}$ | 0.0990*** | 0.1718*** | 0.0856*** | 0.1495* | 0.1480*** | 0.0622* | 0.0021 | 0.0535 |
| | (0.0184) | (0.0449) | (0.0240) | (0.0873) | (0.0174) | (0.0333) | (0.0096) | (0.0556) |
| $\nu_i$ | 0.1965 | 0.1607 | 0.1793 | 0.2024 | 0.2213 | 0.1975 | 0.0000 | 0.1824 |
| $\epsilon_{it}$ | 0.1753 | 0.1628 | 0.1580 | 0.2262 | 0.1735 | 0.1732 | 0.1458 | 0.1738 |
| $\delta$ | 0.5569 | 0.4935 | 0.5628 | 0.4446 | 0.6195 | 0.5651 | 0.0000 | 0.5240 |
| N | 6014 | 860 | 795 | 848 | 884 | 873 | 883 | 871 |
| $R^2_{within}$ | 0.0151 | 0.0624 | 0.0133 | 0.0183 | 0.0355 | 0.0110 | 0.0000 | 0.0032 |
| $R^2_{between}$ | 0.2673 | 0.2468 | 0.2096 | 0.2462 | 0.4243 | 0.0268 | 0.0018 | 0.2887 |
| $R^2_{overall}$ | 0.1508 | 0.1633 | 0.1287 | 0.1261 | 0.2750 | 0.0192 | 0.0000 | 0.1300 |
| $\chi^2$ | 28.8211 | 14.6090 | 12.7734 | 2.9279 | 72.0911 | 3.4918 | 0.0466 | 0.9269 |

*Cluster robust standard errors are reported in the first column to control for individual and group specific heterogeneity among the treatments*
*For the remaining models, cluster robust standard errors to control for group specific heterogeneity are given in parentheses.*

Table 5: Random effects model - Equation (10)

Results[24] presented in Table 5 show that the choice of $D_1$ always exerts a positive and significant impact on the weight put on $Y$ in $D_2$, except in Treatment 6 (in which subjects received two private signals) and in Treatment 7 (where uncertainty was very low), which means that there is a coherence between choices $D_1$ and $D_2$, in line with the theoretical prediction.

**Result 3** *In line with theoretical predictions, a higher average weight on the coordination motive implies a larger weight on the public signal.*

## 5 Conclusion

Although inherent to Keynes' beauty contest metaphor, the idea that participants to financial markets exhibit a common interest in coordination *per se* has not yet received sufficient attention. The main contribution of this paper is to approach as strategic decisions the weights put by those participants on the coordination (rather than the fundamental) motive. These strategic decisions end up in the complete dominance of the coordination motive, evicting the fundamental motive and hence resulting in a disconnection of market activity from fundamentals. While this disconnection between fundamentals and agents' actions is trivial in the case where the weight on the two motives is given exogenously, it becomes crucial in a context where agents may manipulate the weights on each motive.

We have developed a valuation game focusing on how the information cost due to imperfect information may be the source of the disconnection between fundamentals and

[24]Note that $\delta$ corresponds to the proportion of variation due to the individual specific term. If $\delta$ is large (i.e. 80%), the main proportion is explained by the individual specific term and the rest due to idiosyncratic error term. ***, ** and * respectively indicate significance at 1%, 5% and 10% conventional levels.

activity and proposed an experiment aiming at testing this theoretical prediction. While participants in the lab tend to favour the coordination motive over the fundamental motive, our experiment qualifies theoretical predictions when fundamental uncertainty is low compared to strategic uncertainty: low fundamental uncertainty renders coordination costly and avoids a disconnection from fundamentals.

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# 7 Appendix

## 7.1 Derivation of the second stage equilibrium

Recall that the maximization problem of agent $i$ is $\max_{a_i} \mathbb{E}\left(u\left(\mathbf{a}, \theta; r_i\right) \mid x_i, y\right)$, where $\mathbb{E}(. \mid x_i, y)$ is the expectation operator conditional on the two signals received, and where the utility function $u$ is given by

$$u(\mathbf{a}, \theta; r_i) = -(1-r_i)\left(a_i - \theta\right)^2 - r_i \left(a_i - \frac{1}{n}\sum_j a_j\right)^2. \tag{11}$$

The first order condition yields

$$a_i = \frac{(1-r_i)\left(\mathbb{E}\left(\theta \mid x_i, y\right)\right) + r_i\left(1-\frac{1}{n}\right)^2 \frac{1}{n-1}\sum_{j\neq i}\mathbb{E}\left(a_j \mid x_i, y\right)}{1 - r_i + r_i\left(1-\frac{1}{n}\right)^2}. \tag{12}$$

Thus, the action $a_i$ is a weighted arithmetic mean of the expected fundamental value and of the expected average action of the other agents. At equilibrium, taking $a_i = \mathbb{E}\left(a_i \mid x_i, y\right)$, we have

$$a_i = \frac{(1-r_i)\left(\mathbb{E}\left(\theta \mid x_i, y\right)\right) + r_i\left(1-\frac{1}{n}\right)\frac{1}{n}\sum_j \mathbb{E}\left(a_j \mid x_i, y\right)}{1 - r_i + r_i\left(1-\frac{1}{n}\right)}. \tag{13}$$

To derive $\mathbb{E}\left(a_j \mid x_i, y\right)$ we assume, following MS, that any other agent $j$ follows the same linear strategy: $a_j = \kappa_j y + \left(1-\kappa_j\right) x_j$. We denote $\kappa = \frac{1}{n}\sum_j \kappa_j$, so that (using $\mathbb{E}(\theta \mid x_i, y) = (\alpha y + \beta x_i)/(\alpha+\beta)$),

$$\begin{aligned}\frac{1}{n}\sum_j \mathbb{E}\left(a_j \mid x_i, y\right) &= \kappa y + (1-\kappa)\mathbb{E}(\theta \mid x_i, y) \\ &= \frac{\alpha + \kappa\beta}{\alpha+\beta} y + \frac{(1-\kappa)\beta}{\alpha+\beta} x_i.\end{aligned} \tag{14}$$

Inserting (14) in (13), the optimal action of agent $i$ writes:

$$\begin{aligned} a_i &= \frac{(1-r_i)\frac{\alpha y+\beta x_i}{\alpha+\beta} + r_i\left(1-\frac{1}{n}\right)\left(\frac{\alpha+\kappa\beta}{\alpha+\beta}y + \frac{(1-\kappa)\beta}{\alpha+\beta}x_i\right)}{1-r_i+r_i\left(1-\frac{1}{n}\right)} \\ &= \left(\frac{\alpha}{\alpha+\beta} + \frac{\beta}{\alpha+\beta}\frac{(1-1/n)\,r_i}{1-(1/n)\,r_i}\kappa\right)y + \left(\frac{\beta}{\alpha+\beta} - \frac{\beta}{\alpha+\beta}\frac{(1-1/n)\,r_i}{1-(1/n)\,r_i}\kappa\right)x_i. \end{aligned} \tag{15}$$

By identification of the coefficient $\kappa_i$, we obtain

$$\kappa = \frac{\alpha}{\alpha+\beta\,(1-R)}, \text{ with } R \equiv \frac{1}{n}\sum_j \frac{(1-1/n)\,r_j}{1-(1/n)\,r_j}. \tag{16}$$

Plugging the expression of $\kappa$ into (14) yields:

$$\frac{1}{n}\mathbb{E}\left(\sum_j a_j \mid x_i, y\right) = \frac{\alpha}{\alpha+\beta}\left(1+\frac{\beta}{\alpha+\beta\,(1-R)}\right)y + \frac{\beta}{\alpha+\beta}\left(1-\frac{\alpha}{\alpha+\beta\,(1-R)}\right)x_i. \tag{17}$$

Using (17) to re-write (13), we finally obtain:

$$a_i = \underbrace{\frac{\alpha}{\alpha+\beta}\left(1+\frac{\beta R_i}{\alpha+\beta\,(1-R)}\right)}_{\kappa_i}y + \underbrace{\frac{\beta}{\alpha+\beta}\left(1-\frac{\alpha R_i}{\alpha+\beta\,(1-R)}\right)}_{1-\kappa_i}x_i, \text{ with } R_i \equiv \frac{(1-1/n)\,r_i}{1-(1/n)\,r_i}. \tag{18}$$

## 7.2 The first stage payoff

To derive the subgame perfect equilibrium, we have to determine the first stage payoff, that is, the expected utility to be maximised with respect to the decision variable $r_i$:

$$\begin{aligned} &\mathbb{E}\left(u\left(\mathbf{a}^*(\mathbf{r}),\theta;r_i\right)\right) = -(1-r_i)\,\mathbb{E}\left(a_i^*\left(r_i,\mathbf{r}_{-i}\right)-\theta\right)^2 - r_i\mathbb{E}\left(a_i^*\left(r_i,\mathbf{r}_{-i}\right) - \frac{1}{n}\sum_j a_j^*(r_j,\mathbf{r}_{-j})\right)^2 \\ &= -(1-r_i)\,\mathbb{E}\left(\kappa_i\eta+(1-\kappa_i)\,\varepsilon_i\right)^2 - r_i\mathbb{E}\left(\kappa_i\eta+(1-\kappa_i)\,\varepsilon_i - \frac{1}{n}\sum\nolimits_j\left(\kappa_j\eta+(1-\kappa_j)\,\varepsilon_j\right)\right)^2 \\ &= -(1-r_i)\,\mathbb{E}\left(\kappa_i\eta+(1-\kappa_i)\,\varepsilon_i\right)^2 - r_i\mathbb{E}\left((\kappa_i-\kappa)\,\eta + \left(1-\frac{1}{n}\right)(1-\kappa_i)\,\varepsilon_i - \frac{1}{n}\sum\nolimits_{j\neq i}(1-\kappa_j)\,\varepsilon_j\right)^2 \\ &= \frac{1}{\alpha}\left[\begin{array}{c} -(1-r_i)\left(\kappa_i^2+\frac{(1-\kappa_i)^2}{\beta/\alpha}\right) \\ -r_i\left((\kappa_i-\kappa)^2+\frac{1-1/n}{\beta/\alpha}\left(\left(1-\frac{1}{n}\right)(1-\kappa_i)^2+\frac{1}{n}\frac{1}{n-1}\sum_{j\neq i}(1-\kappa_j)^2\right)\right)\end{array}\right]. \end{aligned} \tag{19}$$

Using equations (16) and (18), we can see that $\kappa$ and $\kappa_i$ are functions of $\left(r_i, R^1_{-i}, n, \beta/\alpha\right)$, where $R^1_{-i} = \frac{1}{n-1}\sum_{j\neq i} R_j$ is the mean (the first moment about the origin) of the $R_j$'s of all agents other than $i$. Because of the term $\frac{1}{n-1}\sum_{j\neq i}(1-\kappa_j)^2$, the expression between square brackets will in addition depend upon $R^2_{-i} = \frac{1}{n-1}\sum_{j\neq i} R_j^2$, the mean square (the second moment about the origin) of the same $R_j$'s. Thus,

$$\mathbb{E}\left(u\left(\mathbf{a}^*\left(\mathbf{r}\right),\theta;r_i\right)\right) = \frac{1}{\alpha}U\left(r_i, R^1_{-i}, R^2_{-i}, n, \beta/\alpha\right). \tag{20}$$

### 7.3 Derivation of the subgame perfect equilibrium

Referring to the expression of $U\left(r_i, R^1_{-i}, R^2_{-i}, n, \beta/\alpha\right)$ in equation (19), it is clear that dispersion of other agents' decisions (a high value of $R^2_{-i}$, given $R^1_{-i}$), hence dispersion of the $\kappa_j$'s, can only discourage coordination (the choice of a high $r_i$). In order to show that agent $i$ has on the contrary an incentive to choose a high $r_i$, we shall consider the worst case for such choice, namely the highest value of $R^2_{-i}$ compatible with a given value of the mean $R^1_{-i}$. This case results from taking the $R_j$'s symmetrically disposed at a maximum distance about the mean $R^1_{-i}$. Given the number of agents, such procedure allows to reduce the arguments of the function $U$ to the decision variable $r_i$ plus only two parameters, the mean $R^1_{-i}$, denoted as $\rho$ for brevity, and the ratio of precisions $\beta/\alpha$.

For $n = 6$, the number of participants in each session of our experiment, this procedure leads us to consider four $R_j$'s symetrically disposed, at a maximum distance, about the mean $\rho$, plus a residual $R_j$ coinciding with the mean:

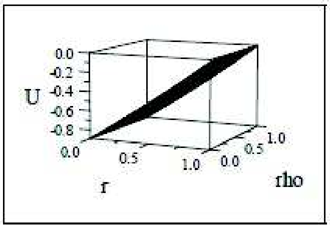
$$\begin{aligned}
\rho &\in [0, 0.5] \Longrightarrow R^2_{-i} = \frac{2\left(2\rho\right)^2 + 2\left(0\right)^2 + \rho^2}{5} = 1.8\rho^2 \\
\rho &\in [0.5, 1] \Longrightarrow R^2_{-i} = \frac{2\left(1\right)^2 + 2\left(2\rho-1\right)^2 + \rho^2}{5} = 1.8\rho^2 - 0.8\left(2\rho - 1\right),
\end{aligned} \tag{21}$$

that is, $R^2_{-i} = 1.8\rho^2 - 0.8\max\left(2\rho-1, 0\right)$.

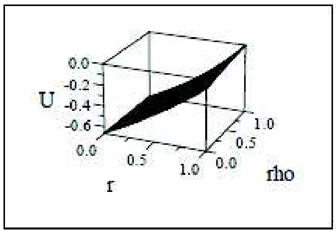
In order to determine the value of $r_i$ which maximises the expected utility, we thus perform, for the parameter values $\beta/\alpha \in \{1/8, 1/2, 1, 2, 8\}$ used in our experiment, simulations with the function

$$U\left(r_i, \rho, 1.8\rho^2 - 0.8\max\left(2\rho-1,0\right), 6, \beta/\alpha\right) \equiv \widehat{U}\left(r_i, \rho, \beta/\alpha\right). \tag{22}$$

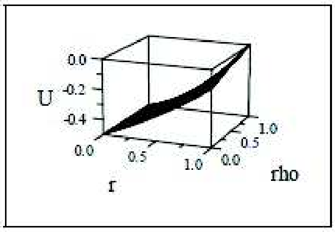
These simulations, represented in the following figure, show that $\widehat{U}\left(\cdot, \rho, \beta/\alpha\right)$ is increasing for most parameter values. Monotonicity is lost only for low relative precision of the public signal ($\beta/\alpha = 8$) and a very large weight $\rho$ put by other agents on the coordination motive. However, $\widehat{U}\left(\cdot, \rho, \beta/\alpha\right)$ is then stricly convex, with $\widehat{U}\left(1, \rho, \beta/\alpha\right) > \widehat{U}\left(0, \rho, \beta/\alpha\right)$. We conclude that $\widehat{U}\left(\cdot, \rho, \beta/\alpha\right)$ is always maximised at $r_i = 1$ in the the cases relevant for our experiment.



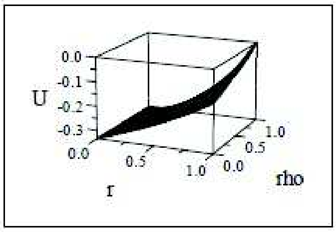

$\beta/\alpha = 1/8$



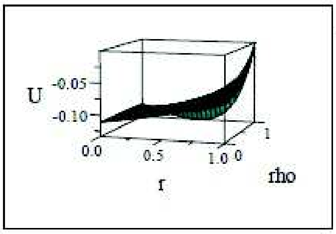

$\beta/\alpha = 1/2$



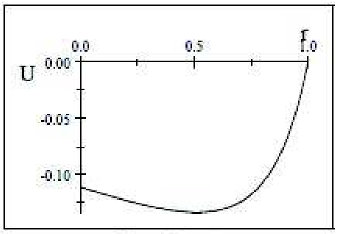

$\beta/\alpha = 1$




$\beta/\alpha = 2$




$\beta/\alpha = 8$




$\beta/\alpha = 8, \rho = 1$

### 7.4 Example of instructions

We present a translation from French to English of the instructions for Treatment 1. Instructions for other treatments are available from the authors upon request.

#### Instructions

**Hello and welcome to our laboratory**

*You will participate to an experiment on decision making. If you carefully follow the instructions, your decisions will allow you to earn a considerable amount of money. To this aim, do not hesitate to ask questions.*

*The money you will earn during this experiment will partly depend on your decisions, those of the other participants and randomness. All decisions are treated anonymously and you will never have to enter your name on the computer. The amount of money you will earn during the experiment is paid individually at the end of the experiment.*

*You are* 18 *people participating in this experiment. Three groups of* 6 *people are formed. These three groups are completely independent and do not interact with each other for the whole length of the experiment. Each participant interacts only with the other participants of his group. The present* *instructions describe the rule of the game for a group of* 6 *participants and all participants have the same instructions.*

**Framework of the experiment**

The experiment consists of 3 games. You may receive some payoffs for each of these three games. The overall payoff earned during the experiment is equal to the sum of payoffs obtained in each of the three games. Note that each game is paid independently, in the sense that if the payoff of a single game is negative, it will be set to zero. The three games do not compensate each other in terms of payoffs. The exchange rate is 1000 **ECU =** 6 **euros**.

***First game of the experiment:***

***Running of the game:***

This game lasts for 5 periods and each period consists in 2 sub-periods. At the beginning of each period, the computer randomly selects a positive number $Z$. This positive number is different in each period, but is ***identical*** for all the participants of a same group. You will ***know the true value of*** $Z$ after the first sub-period, and before making your decision for the second sub-period.

Each period is divided into two sub-periods to which two decisions are associated: $D_1$ and $D_2$, where $D_1$ is your decision in the first sub-period, and $D_2$ your decision in the second sub-period.

At each period, your payoff in ECUs associated with your decisions is given by the following formula:

$$400 - (10 - D_1)(D_2 - Z)^2 - D_1(D_2 - \text{ AVERAGE OF DECISIONS } D_2 \text{ IN THE GROUP})^2$$

***Running of sub-period 1 :***

During the first sub-period, you have to make a decision $D_1$ by choosing an integer number between 0 and 10. The following formula indicates that, ***owing to your choice of*** $D_1$ ***between*** 0 ***and*** 10, you choose how to be paid:

- By choosing $D_1 = 10$, you choose to be paid only according to the distance between your decision $D_2$ and the average of decisions $D_2$ in your group.

  Your payoff is given by the formula:

  $$400 - 10(D_2 - \text{ AVERAGE OF DECISIONS } D_2 \text{ IN THE GROUP})^2.$$

- By choosing $D_1 = 0$, you choose to be paid only according to the distance between your decision $D_2$ and the value of the number $Z$ that you will know.

Your payoff is given by the following formula: $400 - 10(D_2 - Z)^2$.

- By choosing $D_1$ strictly between $0$ and $10$, you choose to be paid according to these two distances, that is:

  i) both according to the distance between your decision $D_2$ and the average of decisions $D_2$ in your group,

  ii) and according to the distance between your decision $D_2$ and the value of the number $Z$, that you will know at the beginning of sub-period $2$, that is before making your decision $D_2$.

You can choose to put more or less weight on one or the other distance. The closer your $D_1$ to $0$, the more you will be rewarded according to the distance between your decision $D_2$ and the value of $Z$.

Conversely, the closer your $D_1$ to $10$, the more you will be rewarded according to the distance between your decision $D_2$ and the average of decisions $D_2$ in your group.

Therefore, if it seems easier for you to estimate $Z$, you will certainly prefer to be rewarded according to the distance between your decision $D_2$ and the number $Z$. To the contrary, if it seems easier for you to estimate the average of decisions $D_2$ in your group, you will certainly prefer to be rewarded according to the distance between your decision $D_2$ and the average of decisions $D_2$ in your group.

***Running of sub-period 2 :***
During the second sub-period, you have to make a decision $D_2$ by choosing a number between $0$ and $2000$. The preceding formula indicates that, owing to **your choice** $D_2$, your payoff gets higher the closer your decision $D_2$ to:

- either the known number $Z$;
- or the average of decisions $D_2$ in your group;
- or both.

Note that owing to your decision $D_1$ (that you will previously have made in sub-period 1), you will have chosen the relative importance of the proximity between your $D_2$ and the known number $Z$ on the one hand, and between your $D_2$ and the average of decisions $D_2$ in your group on the other.

To maximise your payoff associated to your choice of $D_2$, you have to account for the choice of $D_1$ that you will have previously made: the fact to be close to the average of decisions $D_2$ in your group will matter for the choice of your decision $D_2$, the more so the higher decision $D_1$.

Conversely, the fact to be close to $Z$ will matter for the choice of your decision $D_2$, the more so the closer decision $D_1$ to $0$.

By your choice of $D_1$, you can even choose to be paid only according to a single of these two distances.

Before making decision $D_2$, you will be informed about the decisions $D_1$ of all other participants in your group.

***A single period*** of this game will be randomly selected to be paid at the end of the experiment.

***Second game of the experiment:***

***Running of the game:***
The second game also lasts for $5$ periods, and is identical to the first game except that this time, **you will not know the true value of** $Z$ before making your decision for the second sub-period.

At each period, your payoff in ECUs associated with your decisions is again given by the following formula:

$$400 - (10 - D_1)(D_2 - Z)^2 - D_1(D_2 - \text{ AVERAGE OF DECISIONS } D_2 \text{ IN THE GROUP})^2,$$

where $D_1$ is your decision in sub-period 1 and $D_2$ your decision in sub-period 2.

The running of sub-period $1$ for this game is **strictly identical** to the sub-period of the first game of the experiment.

During the second sub-period, to ease your choice of $D_2$, we ask you, on top of making decision $D_2$, to also form an estimation $E_2$ on the average of decisions $D_2$ in your group, which payoff will be:

$$200 - (E_2 - \text{ AVERAGE OF DECISIONS } D_2 \text{ IN THE GROUP})^2.$$

Before making your decision $D_2$, you will be informed about the decision $D_1$ of all the other participants.

However, contrary to the first game of the experiment, none of the participants will know the true value of $Z$ when making his decisions $E_2$ and $D_2$.

Nevertheless, at the second sub-period, each participant observes the same *common* value $S$: it is **identical to all participants** in your group. This common value $S$ contains **no information on the unknown number** $Z$. This *common* value $S$ is not centered on $Z$ and is not distributed on the same support as $Z$. It has therefore no link with $Z$.

***A single period*** of this game will be randomly selected to be paid at the end of the experiment.

***Third game of the experiment:***

***Running of the game:***
The third game lasts for $25$ periods and is identical to the first game, except that this time, **you will not know the true value of** $Z$ before making your decisions of sub-period $2$.

At each period, your payoff in ECUs associated with your decisions is again given by the following formula:

$$400 - (10 - D_1)(D_2 - Z)^2 - D_1(D_2 - \text{ AVERAGE OF DECISIONS } D_2 \text{ IN THE GROUP})^2,$$

where $D_1$ is your decision in the sub-period 1 and $D_2$ your decision in sub-period 2.

The running of sub-period 1 for this game is **strictly identical** to the first sub-period of the first game of the experiment.

During the second sub-period, to ease your choice of $D_2$, we ask you, on top of making your decision $D_2$, to form two estimations $E_1$ and $E_2$:

- an estimation of the true value of $Z$, which payoff will be: $200 - (E_1 - Z)^2$. The closer your estimation $E_1$ to $Z$, the higher your payoff from $E_1$;
- an estimation $E_2$ of the average of decisions $D_2$ in the group, which payoff will be: $200 - (E_2 - \text{ AVERAGE OF DECISIONS } D_2 \text{ IN THE GROUP})^2$. The closer your estimation $E_2$ to the average of decisions $D_2$ in the group, the higher your payoff from $E_2$.

Before making your decision $D_2$, you will be informed about the decision $D_1$ of all other participants. As previously, none of the participants will know the true value of $Z$ before making decisions $D_1$ and $D_2$.

However, at the second sub-period, each participant receives two hints, $X$ and $Y$ on the unknown number $Z$, as explained below.

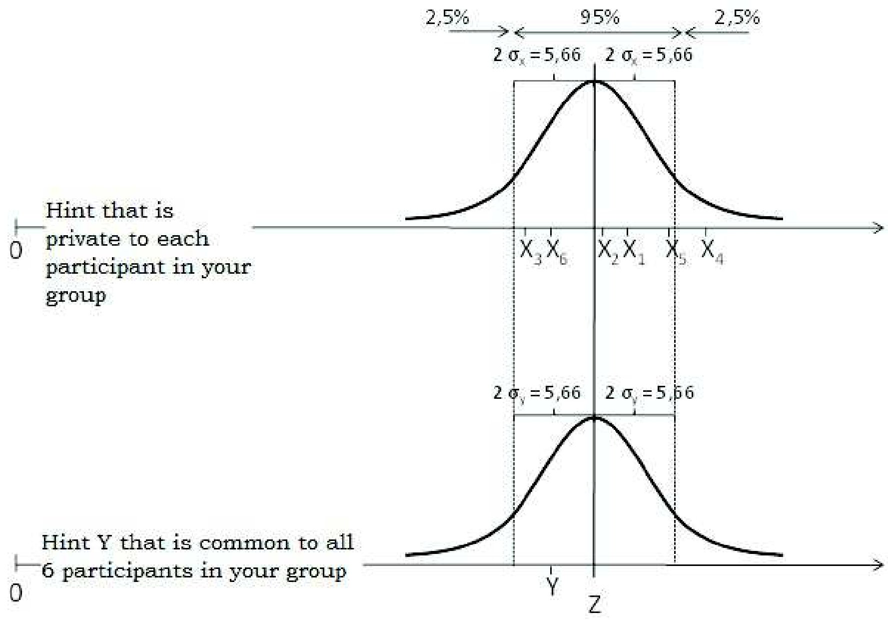
- **Private hint** $X$

  Each participant receives at each second sub-period a private hint $X$ on the unknown number $Z$. Each private hint is centered on $Z$ and contains an error that is randomly selected from a normal distribution with mean $0$ and standard deviation $\sigma_x = 2.83$. Given the properties of the normal distribution, this means that *in 95% of cases*, your private hint $X$ is inside the interval $[Z - 5.66; Z + 5.66]$. Your private hint and the private hints of the other participants are selected independently from each other, so that ***each participant will receive a private hint that can be different from those of the other participants***.

- **Common hint** $Y$

  On top of your private hint $X$, you, as well as the other participants in your group, will receive at each second sub-period, a common hint $Y$ on the unknown number $Z$. This common hint is also centered on $Z$ and contains an error that is randomly selected from a normal distribution with mean $0$ and standard deviation $\sigma_y = 2.83$. Given the properties of the normal distribution, this means that *in 95% of cases*, the common hint $Y$ is inside the interval $[Z - 5.66; Z + 5.66]$. This common hint $Y$ is ***the same for all participants in your group***.

**Graphical example:**




**Distinction between private hint $X$ and common hint $Y$**

The private hint $X$ and the common hint $Y$ have the same precision (same standard deviation): both hints are equally informative **on the unknown number** $Z$. The sole distinction between both hints is that each participant observes a private hint $X$ that is different from those of the other participants, while all participants observe the same common hint $Y$.

**Interval for decisions $E_1$, $E_2$ and $D_2$** In order to limit the spread between your decisions and the true value of $Z$, the interval for decisions $E_1$, $E_2$ and $D_2$ will be set to $[X - 5.66; Y + 5.66]$ if the common hint $Y$ is above the private hint $X$, and to $[Y - 5.66; X + 5.66]$ in the opposite case.

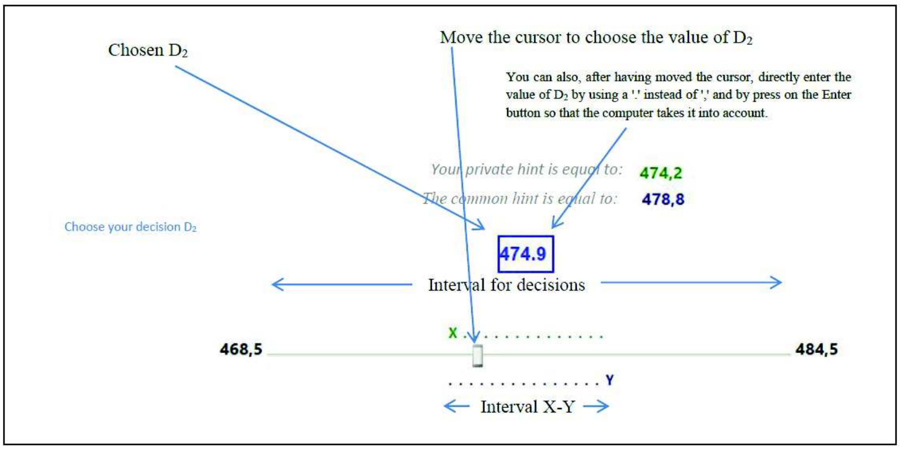
**How to make your estimations $E_1$ and $E_2$?**

To make your estimations $E_1$ and $E_2$, we ask you to select a number inside the interval of decisions owing to a cursor. Nevertheless, as you do not know the errors in your hints, it is natural to choose for your estimations numbers inside the interval defined by your private hint $X$ and the common hint $Y$. You thus have to combine your two hints in order to maximise the payoffs associated with these two estimations. These estimations intend to ease your choice for decisions $D_2$.

**How to make decision $D_2$?**

Similarly, we ask you to select a number inside the interval of decisions owing to a cursor. Nevertheless, as you do not know the errors in your hints, it is natural to choose for your decision $D_2$ a number inside the interval defined by your private hint $X$ and the common hint $Y$. You thus have to combine your two hints in order to maximise your payoff associated with your decisions $D_1$ and $D_2$.

The graph below presents an example explaining how to make a decision $D_2$:

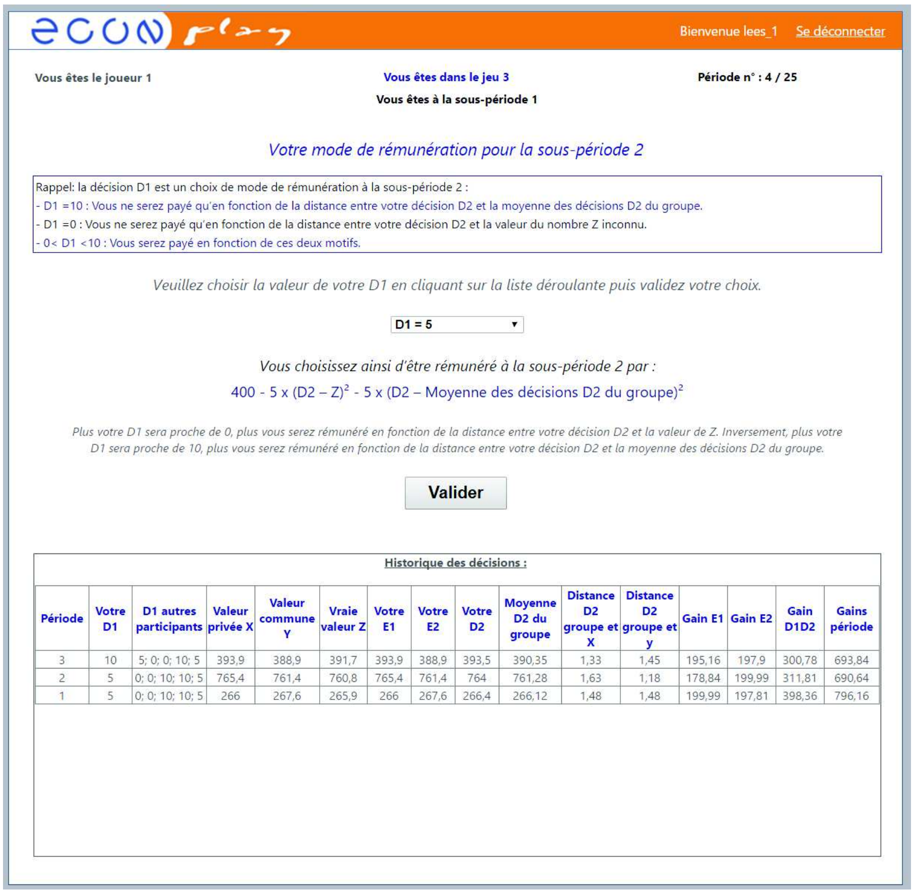



**Five periods** of this game will be randomly selected to be paid at the end of the experiment.

**Payoffs:** At the end of this third game, one of the participants to the experiment will be randomly selected and will loudly announce to the other participants the periods that will be selected for the payoffs of the three games. Your total payoff for the experiment will consist in the sum of the payoffs obtained in the first, second and third games. In case of negative payoff at one of these games, this payoff will be set to zero.

*Before the beginning of the experiment, you will be asked questions in order to make sure you understood the instructions.*

## 7.5 Example of screens



Bienvenue lees_1 Se déconnecter

Vous êtes le joueur 1

Vous êtes dans le jeu 3

Vous êtes à la sous-période 1

Période n° : 4 / 25

*Votre mode de rémunération pour la sous-période 2*

Rappel: la décision D1 est un choix de mode de rémunération à la sous-période 2 :
- D1 =10 : Vous ne serez payé qu'en fonction de la distance entre votre décision D2 et la moyenne des décisions D2 du groupe.
- D1 =0 : Vous ne serez payé qu'en fonction de la distance entre votre décision D2 et la valeur du nombre Z inconnu.
- 0< D1 <10 : Vous serez payé en fonction de ces deux motifs.

*Veuillez choisir la valeur de votre D1 en cliquant sur la liste déroulante puis validez votre choix.*

D1 = 5

*Vous choisissez ainsi d'être rémunéré à la sous-période 2 par :*

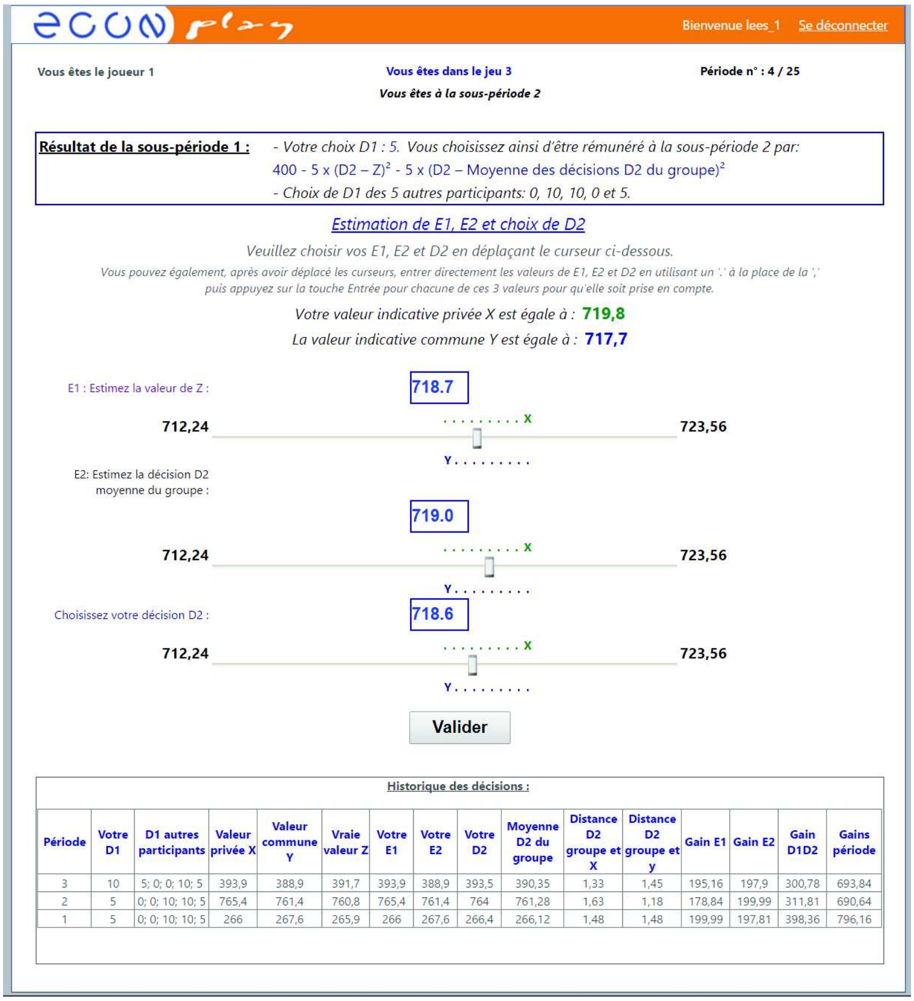
400 - 5 x $(D2 – Z)^2$ - 5 x $(D2 – \text{Moyenne des décisions D2 du groupe})^2$

*Plus votre D1 sera proche de 0, plus vous serez rémunéré en fonction de la distance entre votre décision D2 et la valeur de Z. Inversement, plus votre D1 sera proche de 10, plus vous serez rémunéré en fonction de la distance entre votre décision D2 et la moyenne des décisions D2 du groupe.*

Valider

Historique des décisions :

| Période | Votre D1 | D1 autres participants | Valeur privée X | Valeur commune Y | Vraie valeur Z | Votre E1 | Votre E2 | Votre D2 | Moyenne D2 du groupe | Distance D2 groupe et X | Distance D2 groupe et y | Gain E1 | Gain E2 | Gain D1D2 | Gains période |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 3 | 10 | 5; 0; 0; 10; 5 | 393,9 | 388,9 | 391,7 | 393,9 | 388,9 | 393,5 | 390,35 | 1,33 | 1,45 | 195,16 | 197,9 | 300,78 | 693,84 |
| 2 | 5 | 0; 0; 10; 10; 5 | 765,4 | 761,4 | 760,8 | 765,4 | 761,4 | 764 | 761,28 | 1,63 | 1,18 | 178,84 | 199,99 | 311,81 | 690,64 |
| 1 | 5 | 0; 0; 10; 10; 5 | 266 | 267,6 | 265,9 | 266 | 267,6 | 266,4 | 266,12 | 1,48 | 1,48 | 199,99 | 197,81 | 398,36 | 796,16 |




## 7.6 Descriptive statistics for games 1 and 2

**Game 1: $Z$ known** As shown by Tables 6, 7, 8, 9, 10, 11, and 12, in game 1, $D_1 = 0$ is the most played strategy.

Table 13 shows that $D_2 = Z$ (or values close to $Z$ in a range of 5% around it[25]) is played most of the time in game 1.

**Game 2: Sunspot** As shown by Tables 14, 15, 16, 17, 18, 19, and 20, in game 2, $D_1 = 10$ is the most played strategy.

Table 21 shows that $D_2 = Sunspot$ (or values close to the sunspot in a range of 5% around it[26]) is played most of the time in game 2.

[25]This 5% tolerance interval intends to correct for mistakes due to an uneasy choice made owing to a slider on the interval [0; 2000].

[26]This 5% tolerance interval intends to correct for mistakes due to an uneasy choice made owing to a slider on the interval [0; 2000].

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 27 | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 |
| Group 2 | 27 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 1 |
| Group 3 | 28 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 |
| Group 4 | 24 | 1 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 3 |
| Group 5 | 27 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| Group 6 | 29 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Total | 162 | 2 | 2 | 0 | 0 | 5 | 0 | 1 | 0 | 0 | 8 |

Table 6: Treatment 1 - Frequency of $D_1$ in Game 1 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 25 | 1 | 0 | 0 | 0 | 2 | 1 | 0 | 0 | 0 | 1 |
| Group 2 | 25 | 0 | 1 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 2 |
| Group 3 | 27 | 0 | 2 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 |
| Group 4 | 22 | 0 | 0 | 2 | 0 | 2 | 0 | 0 | 0 | 0 | 4 |
| Group 5 | 30 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Group 6 | 30 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Total | 159 | 1 | 3 | 2 | 0 | 7 | 1 | 0 | 0 | 0 | 7 |

Table 7: Treatment 2 - Frequency of $D_1$ in Game 1 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 29 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Group 2 | 25 | 0 | 0 | 0 | 0 | 3 | 0 | 0 | 0 | 0 | 2 |
| Group 3 | 25 | 1 | 0 | 1 | 0 | 0 | 0 | 1 | 1 | 0 | 1 |
| Group 4 | 27 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 3 |
| Group 5 | 30 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Group 6 | 27 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 3 |
| Total | 163 | 2 | 0 | 1 | 0 | 3 | 0 | 1 | 1 | 0 | 9 |

Table 8: Treatment 3 - Frequency of $D_1$ in Game 1 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 30 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Group 2 | 27 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 0 | 0 |
| Group 3 | 30 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Group 4 | 29 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Group 5 | 27 | 2 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 |
| Group 6 | 29 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Total | 172 | 2 | 0 | 0 | 0 | 2 | 1 | 0 | 1 | 0 | 2 |

Table 9: Treatment 4 - Frequency of $D_1$ in Game 1 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 29 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 |
| Group 2 | 24 | 0 | 1 | 0 | 0 | 2 | 0 | 1 | 0 | 0 | 2 |
| Group 3 | 24 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 1 | 0 | 3 |
| Group 4 | 26 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 2 |
| Group 5 | 25 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 3 |
| Group 6 | 29 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Total | 157 | 0 | 2 | 0 | 1 | 5 | 0 | 3 | 1 | 0 | 11 |

Table 10: Treatment 5 - Frequency of $D_1$ in Game 1 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 18 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 11 |
| Group 2 | 27 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 2 |
| Group 3 | 28 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 |
| Group 4 | 25 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 5 |
| Group 5 | 29 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Group 6 | 27 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 3 |
| Total | 154 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 0 | 0 | 23 |

Table 11: Treatment 6 - Frequency of $D_1$ in Game 1 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 29 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 |
| Group 2 | 27 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| Group 3 | 28 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Group 4 | 24 | 1 | 0 | 0 | 0 | 2 | 0 | 1 | 0 | 0 | 2 |
| Group 5 | 30 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Group 6 | 29 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 |
| Total | 167 | 2 | 0 | 2 | 0 | 2 | 1 | 1 | 0 | 1 | 4 |

Table 12: Treatment 7 - Frequency of $D_1$ in Game 1 per group

| Treatment | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
|---|---|---|---|---|---|---|---|
| Group 1 | 30 | 29 | 30 | 30 | 29 | 29 | 29 |
| Group 2 | 29 | 25 | 26 | 28 | 29 | 23 | 29 |
| Group 3 | 27 | 28 | 27 | 30 | 23 | 28 | 24 |
| Group 4 | 24 | 23 | 30 | 30 | 29 | 29 | 26 |
| Group 5 | 30 | 30 | 30 | 26 | 28 | 27 | 29 |
| Group 6 | 28 | 30 | 30 | 29 | 30 | 28 | 29 |
| Total | 168 | 165 | 173 | 173 | 168 | 164 | 166 |

Table 13: Frequency of $D_2 = Z \pm 5\%$ in Game 1 per treatment and group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 8 | 0 | 0 | 0 | 0 | 2 | 0 | 2 | 0 | 0 | 18 |
| Group 2 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 27 |
| Group 3 | 2 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 26 |
| Group 4 | 1 | 0 | 0 | 1 | 1 | 2 | 1 | 2 | 2 | 1 | 19 |
| Group 5 | 9 | 0 | 0 | 2 | 0 | 2 | 0 | 0 | 0 | 0 | 17 |
| Group 6 | 3 | 0 | 0 | 0 | 0 | 3 | 1 | 0 | 1 | 0 | 22 |
| Total | 24 | 0 | 0 | 3 | 1 | 11 | 2 | 5 | 4 | 1 | 129 |

Table 14: Treatment 1 - Frequency of $D_1$ in Game 2 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 3 | 0 | 2 | 0 | 1 | 1 | 0 | 0 | 1 | 1 | 21 |
| Group 2 | 8 | 0 | 3 | 0 | 0 | 3 | 1 | 0 | 1 | 0 | 14 |
| Group 3 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 28 |
| Group 4 | 3 | 0 | 0 | 0 | 0 | 1 | 0 | 2 | 1 | 2 | 21 |
| Group 5 | 2 | 0 | 0 | 0 | 0 | 1 | 1 | 3 | 2 | 3 | 18 |
| Group 6 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 1 | 2 | 25 |
| Total | 17 | 0 | 5 | 0 | 1 | 7 | 3 | 6 | 6 | 8 | 127 |

Table 15: Treatment 2 - Frequency of $D_1$ in Game 2 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 29 |
| Group 2 | 2 | 0 | 0 | 0 | 0 | 3 | 0 | 1 | 0 | 0 | 24 |
| Group 3 | 6 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 22 |
| Group 4 | 6 | 0 | 0 | 0 | 2 | 0 | 1 | 2 | 1 | 0 | 18 |
| Group 5 | 5 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 23 |
| Group 6 | 3 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 1 | 0 | 24 |
| Total | 23 | 0 | 0 | 0 | 2 | 7 | 1 | 4 | 3 | 0 | 140 |

Table 16: Treatment 3 - Frequency of $D_1$ in Game 2 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 29 |
| Group 2 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 28 |
| Group 3 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 27 |
| Group 4 | 5 | 0 | 0 | 0 | 0 | 4 | 0 | 2 | 1 | 0 | 18 |
| Group 5 | 3 | 2 | 0 | 1 | 0 | 5 | 2 | 2 | 3 | 0 | 12 |
| Group 6 | 8 | 0 | 0 | 0 | 0 | 4 | 0 | 1 | 0 | 0 | 17 |
| Total | 18 | 2 | 0 | 1 | 0 | 14 | 2 | 6 | 5 | 1 | 131 |

Table 17: Treatment 4 - Frequency of $D_1$ in Game 2 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 1 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 27 |
| Group 2 | 4 | 0 | 0 | 0 | 0 | 4 | 0 | 0 | 0 | 0 | 22 |
| Group 3 | 3 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 1 | 0 | 24 |
| Group 4 | 3 | 0 | 0 | 0 | 1 | 5 | 0 | 1 | 3 | 3 | 14 |
| Group 5 | 6 | 0 | 0 | 0 | 0 | 3 | 0 | 0 | 7 | 4 | 10 |
| Group 6 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 3 | 25 |
| Total | 18 | 0 | 0 | 0 | 1 | 16 | 0 | 1 | 12 | 10 | 122 |

Table 18: Treatment 5 - Frequency of $D_1$ in Game 2 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 1 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 27 |
| Group 2 | 5 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 23 |
| Group 3 | 2 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 27 |
| Group 4 | 2 | 0 | 0 | 1 | 1 | 0 | 2 | 0 | 1 | 1 | 22 |
| Group 5 | 3 | 0 | 0 | 1 | 0 | 7 | 1 | 0 | 0 | 0 | 18 |
| Group 6 | 6 | 0 | 3 | 0 | 1 | 2 | 2 | 3 | 0 | 0 | 13 |
| Total | 19 | 0 | 3 | 2 | 2 | 14 | 5 | 3 | 1 | 1 | 130 |

Table 19: Treatment 6 - Frequency of $D_1$ in Game 2 per group

| $D_1$ | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Group 1 | 6 | 0 | 0 | 0 | 0 | 1 | 0 | 2 | 0 | 0 | 21 |
| Group 2 | 11 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 18 |
| Group 3 | 10 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 18 |
| Group 4 | 4 | 0 | 0 | 1 | 2 | 4 | 1 | 0 | 0 | 1 | 17 |
| Group 5 | 1 | 0 | 0 | 0 | 1 | 2 | 0 | 0 | 1 | 1 | 24 |
| Group 6 | 2 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 27 |
| Total | 34 | 0 | 0 | 1 | 3 | 10 | 1 | 3 | 1 | 2 | 125 |

Table 20: Treatment 7 - Frequency of $D_1$ in Game 2 per group

| Treatment | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
|---|---|---|---|---|---|---|---|
| Group 1 | 18 | 11 | 30 | 28 | 25 | 22 | 8 |
| Group 2 | 19 | 12 | 24 | 25 | 9 | 26 | 12 |
| Group 3 | 17 | 28 | 15 | 26 | 18 | 29 | 12 |
| Group 4 | 18 | 13 | 14 | 18 | 8 | 14 | 6 |
| Group 5 | 19 | 9 | 20 | 4 | 7 | 13 | 19 |
| Group 6 | 17 | 25 | 16 | 9 | 11 | 12 | 23 |
| Total | 108 | 98 | 119 | 110 | 78 | 116 | 80 |

Table 21: Frequency of $D_2 = Sunspot \pm 5\%$ in Game 2 per treatment and group

## 7.7 Results of tests

Table 22 presents the results of Wilcoxon rank tests to compare observed data to theoretical values, while Tables 23, 24, 25 and 26 present the results of Mann-Whitney tests to deal with treatment comparisons.

| Tr. 1 — Th. 1st or. | Tr. 2 — Th. 1st or. | Tr. 3 — Th. 1st or. | Tr. 4 — Th. 1st or. | Tr. 5 — Th. 1st or. | Tr. 6 — Th. 1st or. | Tr. 7 — Th. 1st or. |
|---|---|---|---|---|---|---|
| 0.0277 | 0.0464 | 0.0273 | 0.0464 | 0.2476 | 0.3173 | 0.0277 |

Table 22: Weight on $Y$ in game 3: comparison between observed weight on $Y$ and theoretical weight in the first order expectation, $p$-values for the Wilcoxon rank test

| Tr. 1 — Obs. 1st or. | Tr. 2 — Obs. 1st or. | Tr. 3 — Obs. 1st or. | Tr. 4 — Obs. 1st or. | Tr. 5 — Obs. 1st or. | Tr. 6 — Obs. 1st or. | Tr. 7 — Obs. 1st or. |
|---|---|---|---|---|---|---|
| 0.0064 | 0.4233 | 0.1994 | 0.0250 | 0.4225 | 0.7466 | 0.6310 |

Table 23: Weight on $Y$ in game 3: comparison between observed weight on $Y$ and observed weight in the first order expectation, $p$-values for the Mann-Whitney test

| Tr. 1 $- Th.\|_{D_1}$ | Tr. 2 $- Th.\|_{D_1}$ | Tr. 3 $- Th.\|_{D_1}$ | Tr. 4 $- Th.\|_{D_1}$ | Tr. 5 $- Th.\|_{D_1}$ | Tr. 7 $- Th.\|_{D_1}$ |
|---|---|---|---|---|---|
| 0.0542 | 0.0250 | 0.0103 | 0.6310 | 0.0776 | 0.1495 |

Table 24: Weight on $Y$ in game 3: comparison between observed weight on $Y$ and theoretical weight conditional on observed first stage decision, $p$-values for the Mann-Whitney test

| Tr. 1 — Tr. 2 | Tr. 1 — Tr. 3 | Tr. 1 — Tr. 4 | Tr. 1 — Tr. 5 | Tr. 1 — Tr. 6 | Tr. 2 — Tr. 4 | Tr. 2 — Tr. 7 | Tr. 3 — Tr. 7 | Tr. 3 — Tr. 5 |
|---|---|---|---|---|---|---|---|---|
| 0.8728 | 0.0547 | 0.5218 | 0.7488 | 0.0547 | 0.7488 | 0.6884 | 0.2002 | 0.0782 |

Table 25: Average $D_1$: treatment comparisons, $p$-values for the Mann-Whitney test

| Tr. 1 — Tr. 2 | Tr. 1 — Tr. 3 | Tr. 1 — Tr. 4 | Tr. 1 — Tr. 5 | Tr. 1 — Tr. 6 | Tr. 2 — Tr. 4 | Tr. 2 — Tr. 7 | Tr. 3 — Tr. 7 | Tr. 3 — Tr. 5 |
|---|---|---|---|---|---|---|---|---|
| 0.0782 | 0.0103 | 0.4233 | 0.1994 | 0.0036 | 0.1495 | 0.0547 | 0.0776 | 0.0091 |

Table 26: Weight on $Y$ in game 3: treatment comparisons, $p$-values for the Mann-Whitney test

## 7.8 Relative frequency of weights on the coordination motive per group


Figure 3: Relative frequency of weights on the coordination motive per treatment and group

## 7.9 Relative frequency of weights on the public signal in Decision 2

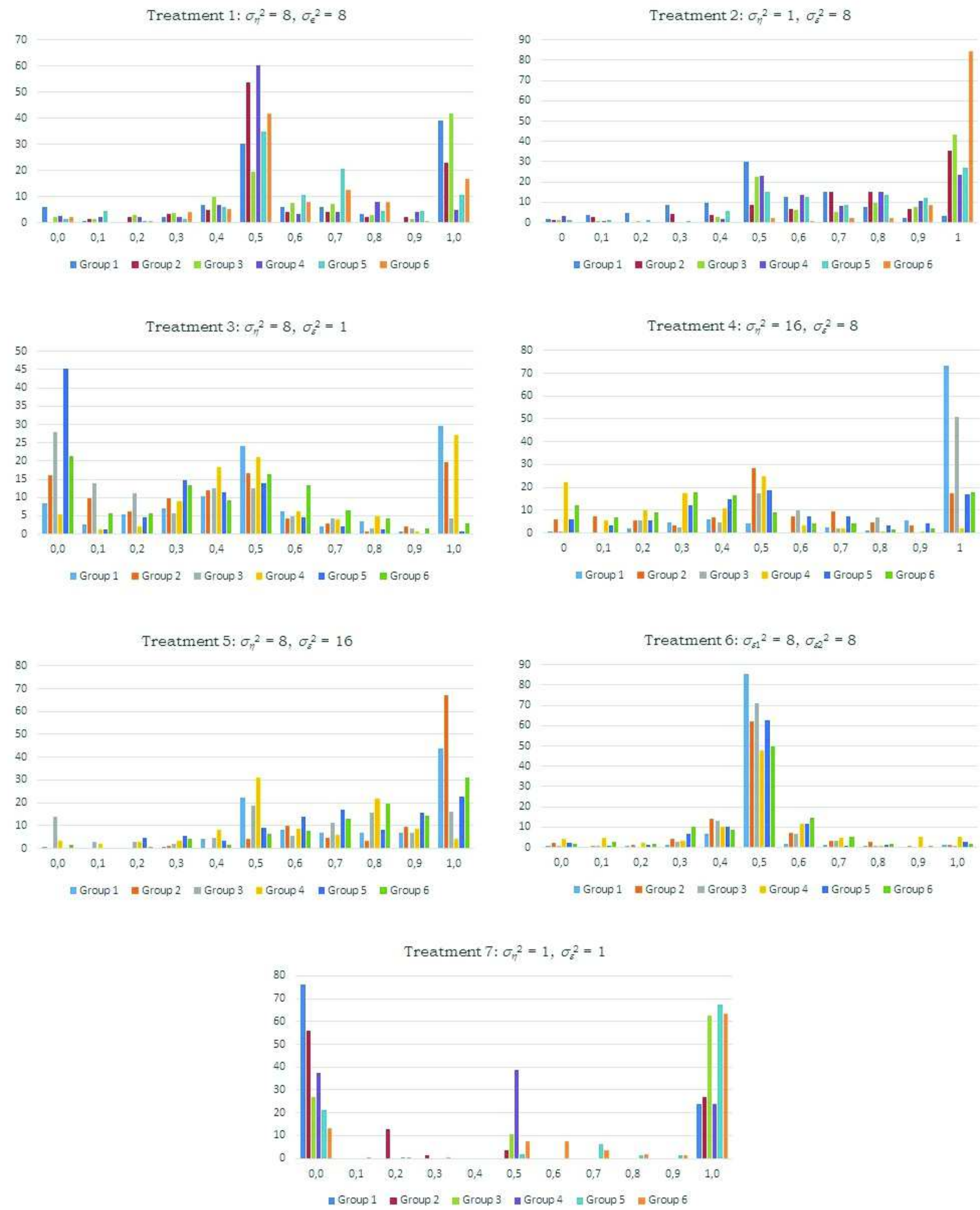



Figure 4: Relative frequency of weights on $Y$ in $D_2$ per treatment and group

## 7.10 Rationality: optimality of decisions $D_2$?

**Playing inside the interval** In most cases, participants played inside the interval defined by the two signals although this was not made compulsory by the design (contrary to Baeriswyl and Cornand (2014, 2016)). As shown in Table 27, only 285 decisions over 6300 were outside this interval, which represents 4.5% of decisions.[27] This proportion contrasts with that obtained in Cornand and Heinemann (2014), owing to our design that showed the positions of signals on the interval of possible decisions.

| Tr. | 1 ($\sigma_\eta^2 = \sigma_\varepsilon^2 = 8$) | 2 ($\sigma_\eta^2 = 1, \sigma_\varepsilon^2 = 8$) | 3 ($\sigma_\eta^2 = 8, \sigma_\varepsilon^2 = 1$) | 4 ($\sigma_\eta^2 = 16, \sigma_\varepsilon^2 = 8$) | 5 ($\sigma_\eta^2 = 8, \sigma_\varepsilon^2 = 16$) | 6 ($\sigma_{\varepsilon_1}^2 = 8, \sigma_{\varepsilon_2}^2 = 8$) | 7 ($\sigma_\eta^2 = \sigma_\varepsilon^2 = 1$) |
|---|---|---|---|---|---|---|---|
| Gr. 1 | 2 | 39 | 11 | 0 | 3 | 1 | 2 |
| Gr. 2 | 1 | 28 | 10 | 2 | 1 | 2 | 11 |
| Gr. 3 | 11 | 4 | 19 | 2 | 3 | 1 | 9 |
| Gr. 4 | 2 | 21 | 3 | 4 | 2 | 9 | 2 |
| Gr. 5 | 23 | 11 | 0 | 3 | 5 | 4 | 5 |
| Gr. 6 | 0 | 2 | 9 | 5 | 13 | 0 | 0 |
| Sum | 39 | 105 | 52 | 16 | 27 | 17 | 29 |

Table 27: Number of decisions outside the interval defined by the two signals per group and treatment

**Coherence between estimations and decisions** As shown in Tables 28 and 30, estimations $E_1$ are relatively close to theoretical values. For Treatments 1, 4, 5, and 6, statistical equality cannot be rejected. There is however a significant difference for Treatments 2 and 3 (which exhibit extreme theoretical weights on the public signal).

| Tr. | 1 ($\sigma_\eta^2 = \sigma_\varepsilon^2 = 8$) | 2 ($\sigma_\eta^2 = 1, \sigma_\varepsilon^2 = 8$) | 3 ($\sigma_\eta^2 = 8, \sigma_\varepsilon^2 = 1$) | 4 ($\sigma_\eta^2 = 16, \sigma_\varepsilon^2 = 8$) | 5 ($\sigma_\eta^2 = 8, \sigma_\varepsilon^2 = 16$) | 6 ($\sigma_{\varepsilon_1}^2 = 8, \sigma_{\varepsilon_2}^2 = 8$) | 7 ($\sigma_\eta^2 = \sigma_\varepsilon^2 = 1$) |
|---|---|---|---|---|---|---|---|
| Gr. 1 | 0.59 | 0.55 | 0.27 | 0.39 | 0.69 | 0.51 | 0.52 |
| Gr. 2 | 0.53 | 0.70 | 0.30 | 0.45 | 0.79 | 0.49 | 0.52 |
| Gr. 3 | 0.49 | 0.58 | 0.28 | 0.40 | 0.48 | 0.50 | 0.50 |
| Gr. 4 | 0.48 | 0.72 | 0.38 | 0.29 | 0.55 | 0.50 | 0.62 |
| Gr. 5 | 0.52 | 0.75 | 0.19 | 0.30 | 0.67 | 0.48 | 0.62 |
| Gr. 6 | 0.52 | 0.84 | 0.36 | 0.35 | 0.79 | 0.50 | 0.64 |
| Av. | 0.52 | 0.69 | 0.30 | 0.36 | 0.66 | 0.50 | 0.57 |
| Th. | 0.50 | 0.94 | 0.06 | 0.33 | 0.67 | 0.50 | 0.50 |

Table 28: Weight on $Y$ in $E_1$

As shown by Tables 29 and 31, estimations $E_2$ are always below the extreme theoretical weight of $1$ on the public signal,[28] but larger than the equal weight of $0.5$.

[27] We accounted for the missing data by performing a bootstrap analysis in Appendix 7.11.
[28] In Treatment 6 where subjects received two private signals, however, estimations are in line with theory.

| Tr. | 1 ($\sigma_\eta^2 = \sigma_\varepsilon^2 = 8$) | 2 ($\sigma_\eta^2 = 1, \sigma_\varepsilon^2 = 8$) | 3 ($\sigma_\eta^2 = 8, \sigma_\varepsilon^2 = 1$) | 4 ($\sigma_\eta^2 = 16, \sigma_\varepsilon^2 = 8$) | 5 ($\sigma_\eta^2 = 8, \sigma_\varepsilon^2 = 16$) | 6 ($\sigma_{\varepsilon_1}^2 = 8, \sigma_{\varepsilon_2}^2 = 8$) | 7 ($\sigma_\eta^2 = \sigma_\varepsilon^2 = 1$) |
|---|---|---|---|---|---|---|---|
| Gr. 1 | 0.79 | 0.55 | 0.73 | 0.92 | 0.83 | 0.50 | 0.58 |
| Gr. 2 | 0.73 | 0.75 | 0.44 | 0.55 | 0.90 | 0.49 | 0.63 |
| Gr. 3 | 0.80 | 0.79 | 0.39 | 0.84 | 0.63 | 0.51 | 0.59 |
| Gr. 4 | 0.66 | 0.70 | 0.61 | 0.60 | 0.62 | 0.51 | 0.71 |
| Gr. 5 | 0.59 | 0.72 | 0.29 | 0.73 | 0.73 | 0.49 | 0.85 |
| Gr. 6 | 0.65 | 0.97 | 0.38 | 0.60 | 0.79 | 0.47 | 0.63 |
| Av. | 0.70 | 0.75 | 0.47 | 0.71 | 0.75 | 0.49 | 0.66 |
| Th. | 1 | 1 | 1 | 1 | 1 | 0.50 | 1 |

Table 29: Weight on $Y$ in the estimation $E_2$

| Tr. 1 − Th. | Tr. 2 − Th. | Tr. 3 − Th. | Tr. 4 − Th. | Tr. 5 − Th. | Tr. 6 − Th. | Tr. 7 − Th. |
|---|---|---|---|---|---|---|
| 0.7532 | 0.0277 | 0.0277 | 0.2489 | 0.9165 | 0.9165 | 0.0277 |

Table 30: Weight on $Y$ in $E_1$ in game 3: comparison between observed weight on $Y$ in $E_1$ and theoretical weight in $\mathbb{E}(\theta)$, $p$-values for the Wilcoxon rank test

| Tr. 1 − Th. | Tr. 2 − Th. | Tr. 3 − Th. | Tr. 4 − Th. | Tr. 5 − Th. | Tr. 6 − Th. | Tr. 7 − Th. |
|---|---|---|---|---|---|---|
| 0.0277 | 0.0277 | 0.0277 | 0.0277 | 0.0277 | 0.4631 | 0.0277 |

Table 31: Weight on $Y$ in $E_2$ in game 3: comparison between observed weight on $Y$ in $E_2$ and theoretical weight in $\mathbb{E}(\bar{D}_2)$, $p$-values for the Wilcoxon rank test

To obtain a more detailed picture of coherence between estimations and decisions, we estimate whether the weight put on the public signal in $D_2$ is highly dependent the optimal weight on the public signal conditional on estimations $E_1$ and $E_2$ ($OptD_2cond$), by regressing the following equation, both for each treatment and for all the treatments taken together:

$$\left|\frac{D_{2it} - X_{it}}{Y_t - X_{it}}\right| = Co + \alpha \underbrace{\left|\frac{(10 - D_1)}{10} E_{1it} + \frac{D_1}{10} E_{2it}\right|}_{OptD_2cond} + (\epsilon_{it} + \nu_i), \tag{23}$$

where $Co$ is the constant, $\alpha$ is the estimated coefficient of the optimal decision conditional on stated expectations; $\epsilon_{it} + \nu_i$ is the error term.

Overall, Table 32 shows that there is a significant and positive relation between estimations $E_1$ and $E_2$ and the decision $D_2$ at the individual level and for all treatments, except Treatment 5.

| | All | Tr1 | Tr2 | Tr3 | Tr4 | Tr5 | Tr6 | Tr7 |
|---|---|---|---|---|---|---|---|---|
| Const | 0.5066*** | 0.3332*** | 0.1698*** | 0.3476*** | 0.3048*** | 0.7256*** | 0.3945*** | 0.2109*** |
| | (0.0490) | (0.0106) | (0.0631) | (0.0443) | (0.0328) | (0.0492) | (0.0403) | (0.0540) |
| $OptD_2cond$ | 0.1488** | 0.4470*** | 0.7750*** | 0.1636* | 0.4679*** | 0.0052 | 0.1999** | 0.6085*** |
| | (0.0736) | (0.0270) | (0.0799) | (0.0855) | (0.0617) | (0.0285) | (0.0825) | (0.0933) |
| $\nu_i$ | 0.1134 | 0.0986 | 0.0215 | 0.1417 | 0.1468 | 0.1323 | 0.0000 | 0.1220 |
| $\epsilon_{it}$ | 0.1705 | 0.1466 | 0.1225 | 0.2138 | 0.1510 | 0.1742 | 0.1383 | 0.1436 |
| $\delta$ | 0.3064 | 0.3111 | 0.0298 | 0.3050 | 0.4862 | 0.3659 | 0.0000 | 0.4191 |
| N | 6014 | 860 | 795 | 848 | 884 | 873 | 883 | 871 |
| $R^2_{within}$ | 0.0681 | 0.2392 | 0.4070 | 0.1226 | 0.2696 | 0.0000 | 0.1002 | 0.3193 |
| $R^2_{between}$ | 0.7415 | 0.7029 | 0.9715 | 0.6152 | 0.7422 | 0.5460 | 0.0072 | 0.6756 |
| $R^2_{overall}$ | 0.2616 | 0.4527 | 0.7016 | 0.2234 | 0.5604 | 0.0382 | 0.0936 | 0.5144 |
| $\chi^2$ | 4.0909 | 274.4484 | 94.1295 | 3.6562 | 57.5190 | 0.0338 | 5.8643 | 42.5019 |

*Cluster robust standard errors are reported in the first column to control for individual and group specific heterogeneity among the treatments*

*For the remaining models, cluster robust standard errors to control for group specific heterogeneity are given in parentheses.*

Table 32: Random effects model - Equation (23)

### 7.11 Treatment effect: an analysis on individual data

We perform a treatment comparison for the individual weight put on the public signal in $D_2$. We observe similar patterns in terms of treatment comparisons as in the analysis of aggregate data relying on non-parametric tests.

The interpretation of Tables 33, 34, and 35 is the following. Consider for example the first column of Table 33. Each treatment should be compared to the baseline, which is in the present case, Treatment 1 (the value is that of the constant). Treatment 2 affects the dependent variable (Weight on $Y$ in $D_2$) positively and significantly compared to the baseline. In other words, TR2 compared to TR1 increases significantly the dependent variable by an effective size of 0.1181. Similarly, Treatment 6 significantly and negatively affects the dependent variable compared to the baseline.[29] The coefficient for $D_{1it}$ measures the effect of decision 1 on the dependent variable while all the other explanatory variables are constant.

[29]Note that the baseline (TR1) is the reference and is similar to the constant in the regression analysis of Tables 5 and 32, where all the other treatments (TR2, TR3, TR4, TR5, TR6) are the slopes for each treatment respectively.

| | Weight on $Y$ in $D_2$ | |
|---|---|---|
| | Eq. (10) | Eq. (23) |
| Baseline (TR1) | 0.5653*** | 0.5460*** |
| | (0.0239) | (0.0748) |
| TR2 | 0.1181** | 0.1033* |
| | (0.0517) | (0.0542) |
| TR3 | -0.1825*** | -0.1834*** |
| | (0.0601) | (0.0688) |
| TR4 | -0.0442 | -0.0425 |
| | (0.0799) | (0.0737) |
| TR5 | 0.0968* | 0.0856* |
| | (0.0518) | (0.0450) |
| TR6 | -0.1048*** | -0.1147*** |
| | (0.0250) | (0.0271) |
| $\frac{D_{1it}}{10}$ | 0.1038*** | |
| | (0.0192) | |
| $OptD_2cond$ | | 0.1272 |
| | | (0.1086) |
| $\nu_i$ | 0.1824 | 0.1117 |
| $\epsilon_{it}$ | 0.1755 | 0.1717 |
| $\delta$ | 0.5193 | 0.2975 |
| N | 5143 | 5143 |
| $R^2_{within}$ | 0.0182 | 0.0606 |
| $R^2_{between}$ | 0.3492 | 0.4187 |
| $R^2_{overall}$ | 0.2393 | 0.2949 |
| $\chi^2$ | 104.9290 | 95.0009 |

*Cluster robust and bootstrap standard errors are given in parentheses*
*Bootstrap: 3000 replication*

Table 33: Random effects model - Comparisons to Treatment 1

| | Weight on $Y$ in $D_2$ | |
|---|---|---|
| | Eq. (10) | Eq. (23) |
| Baseline (TR2) | 0.6881*** | 0.3275*** |
| | (0.0462) | (0.0492) |
| TR4 | -0.1621* | -0.0753 |
| | (0.0866) | (0.0509) |
| TR7 | -0.1405** | -0.0846** |
| | (0.0592) | (0.0348) |
| $\frac{D_{1it}}{10}$ | 0.0947*** | |
| | (0.0235) | |
| $OptD_2cond$ | | 0.5583*** |
| | | (0.0561) |
| $\nu_i$ | 0.1975 | 0.1100 |
| $\epsilon_{it}$ | 0.1692 | 0.1408 |
| $\delta$ | 0.5767 | 0.3792 |
| N | 2550 | 2550 |
| $R^2_{within}$ | 0.0144 | 0.3176 |
| $R^2_{between}$ | 0.2514 | 0.7642 |
| $R^2_{overall}$ | 0.1970 | 0.6019 |
| $\chi^2$ | 24.1014 | 158.0893 |

*Cluster robust and bootstrap standard errors are given in parentheses*
*Bootstrap: 3000 replication*

Table 34: Random effects model - Comparisons to Treatment 2

| | Weight on $Y$ in $D_2$ | |
|---|---|---|
| | Eq. (10) | Eq. (23) |
| Baseline (TR3) | 0.3898*** | 0.3814*** |
| | (0.0519) | (0.0706) |
| TR5 | 0.2870*** | 0.2855*** |
| | (0.0748) | (0.0811) |
| TR7 | 0.1651** | 0.1652** |
| | (0.0690) | (0.0770) |
| $\frac{D_{1it}}{10}$ | 0.0810** | |
| | (0.0324) | |
| $OptD_2cond$ | | 0.0815 |
| | | (0.1291) |
| $\nu_i$ | 0.1948 | 0.1341 |
| $\epsilon_{it}$ | 0.1925 | 0.1898 |
| $\delta$ | 0.5060 | 0.3329 |
| Obs | 2592 | 2592 |
| $R^2_{within}$ | 0.0096 | 0.0368 |
| $R^2_{between}$ | 0.3349 | 0.3677 |
| $R^2_{overall}$ | 0.2105 | 0.2404 |
| $\chi^2$ | 36.4934 | 20.3457 |

*Cluster robust and bootstrap standard errors are given in parentheses*
*Bootstrap: 3000 replication*

Table 35: Random effects model - Comparisons to Treatment 3