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Keywords:

dispersed information, public information, beauty contest, coordination, experiment

JEL codes:

D84, C92, E12

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

Kene Boun My[†] Camille Cornand[‡] Rodolphe Dos Santos Ferreira[§]

January 31, 2018

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.

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[†]BETA - University of Strasbourg, 61 avenue de la Forêt Noire - 67085 Strasbourg Cedex, France; email: bounmy@unistra.fr.

[‡]Corresponding author: Univ Lyon, CNRS, GATE L-SE UMR 5824, F-69130 Ecully, France; email: cornand@gate.cnrs.fr.

[§]BETA - University of Strasbourg, 61 avenue de la Forêt Noire - 67085 Strasbourg Cedex, France; email: rdsf@unistra.fr; and Católica Lisbon School of Business and Economics.

1 Introduction

Equity generates two types of return: cash dividends and possibly capital gains from its sale, the latter depending on valuation by potential buyers, according to their expectations of future equity prices. The prospect of capital gains thus leads to a mutual dependence on each other's beliefs which is a source of strategic uncertainty adding to the fundamental uncertainty about future dividends. Strategic uncertainty opens the door to price deviations from the fundamental value of the equity, as determined by the sum of the discounted present value of expected future dividends. Such mispricing, which is the more likely to appear the more interested investors become in capital gains rather than in cash dividends, has been dealt with in experimental sequential asset markets, first designed by Smith *et al.* (1988). While the standard finance theory orienting this experimental literature predicts no deviation of prices from fundamentals, the experiments typically exhibit bubbles and crashes (for a survey see Palan, 2013). Such an issue might be the result of irrational behavior, but one of the major reasons for the experimentally observed deviation of prices from fundamentals, which is compatible with full rationality, is information heterogeneity, breaking away from the representative investor model of standard finance theory. By focusing on the mutual dependence of market participants' beliefs about other market participants' beliefs in a context of dispersed information, beauty contest games show the possibility of mispricing at equilibrium, providing some rationale for the experimental observations (Allen, Morris and Shin, 2006, Hirota and Sunder, 2007). Building on the literature devoted to static beauty contest games, we propose a simple theoretical framework and its experimental test that allows to capture under which informational circumstances subjects may endogenously be more interested in (short-term) capital gains than in (long-term) rewards from dividends, or, in other words, they may prefer to speculate rather than invest.

Our starting point is the beauty contest metaphor used by Keynes to characterise the working of financial markets that displays the existence of a dual motivation in agents' decision making: there is a fundamental motive making agents strive to foresee 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. Moreover, in Keynes' view, the working of stock markets, rather than imposing a balance between the two motives, tends to favour the coordination motive relative to the fundamental motive or, in Keynes' words, to foster 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 While inherent to Keynes' view, the fact

¹The tendency of speculation to predominate over enterprise is explicitly 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 [...] is enormous. [...] When 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 [...] a speculator" (Keynes, 1936, pp.

that speculation (associated with the coordination motive) dominates enterprise (associated with the fundamental motive) has not been considered in the literature that formalises the beauty contest.

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 aeconomici*. 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 crowding out 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.

We consider a valuation game 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, they 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, participants in financial markets may choose the shares of their portfolios they allocate to 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. Instead, making this relative weight the outcome of a strategic choice allows to capture in a simple manner² the idea that investors, caring about the short-run valuation of their capital, may become so concerned with matching prospective market values that they are ready to pay too much attention to any information helping to predict others' behaviour, thus disregarding fundamental relevant information. To this aim, we extend the MS model to a two-stage game, in which agents first choose the weights they attribute to the coordination and fundamental motives, before choosing the value that best matches the selected combination of the two motives. We then show that there is an incentive for agents

158-159).

²We will abstract from the analysis of risk diversification in portfolio choice, putting the emphasis on the conformity in behaviour and its relation to informational issues. The simplicity we aim at is a prerequisite to conduct an experiment designed to test these effects.

to favour the coordination over the fundamental motive, hence to prefer, in Keynes' words, speculation to enterprise. This is the consequence of coordination on a public signal entailing a lower information cost than the prediction of 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 a conventional value. The strategic choice to privilege 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. By varying the precision and the nature (public or private) of information, the experiment captures the impact of different informational contexts on the choice to speculate and thus on the consequent disconnection from fundamentals of the valuation activity. 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 since subjects always put more weight on the coordination than on the fundamental motive at the first stage, correspondingly putting a large weight on the public signal at the second stage, and 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. An exception to this observation, which contradicts theoretical predictions, is the case where private information is very precise while public information is not, making fundamental uncertainty be quite low relative to strategic uncertainty. The choice of the coordination game appears then too costly, as it typically entails a coordination problem at the second stage, requiring that the others match indeed the public signal so as to maximise payoffs. However, reducing the precision of public information (as is sometimes advocated in the literature in the vein of MS) may not be suitable, as subjects still choose to play the coordination game unless the precision of the private signal is very high.

Our paper contributes to the theoretical literature on beauty contest games initiated by Morris and Shin (2002)³ and to its experimental counterparts (Dale and Morgan, 2012,

³This literature largely expanded after Morris and Shin (2002) seminal contribution. See *e.g.* Angeletos and Pavan (2007). Recent extensions include endogenous information, in the form of either observation of aggregate outcomes (see *e.g.* Bayona, 2017), or information acquisition (see *e.g.* Hellwig and Veldkamp, 2009, Myatt and Wallace, 2012, Colombo *et al.*, 2014). In contrast to this literature, the aim of the present paper is to focus on the strategic weights that agents put on the two motives of the beauty contest, affecting the payoff structure and not the weights players may strategically attribute to public and private information for a given payoff. In addition, while the literature in the vein of Morris and Shin has concentrated on the social value of public information, a discussion of welfare issues is beyond the scope of the present paper.

Cornand and Heinemann, 2014, Baeriswyl and Cornand, 2014, 2016, Shapiro *et al.*, 2014, and Fehr *et al.*, 2017).⁴ 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 question of trading on fundamental value versus coordinating on some other elements has usually been dealt with in the experimental asset markets literature, following the pioneer work of Smith *et al.* (1988), exhibiting substantial price deviations from fundamental values. A considerable subsequent literature has consistently observed price bubbles in such markets, which makes the Smith *et al.* framework popular to understand and study how bubbles emerge. The beauty contest game approach tackles the same kind of question from a different perspective in a much simpler experimental framework, precisely because it does not consider the issue of trading on experimental asset markets. Instead, and importantly for our purpose, it allows to emphasize the role of fundamental versus strategic uncertainty in speculative behaviour.

The issue of strategic uncertainty – which is at the core of our paper – has been originally pointed out by Smith *et al.* (“uncertainty about the behaviour of others” as they call it on p. 1148) as an explanation for observed price deviations from fundamentals. According to the speculative hypothesis, expecting to resell the asset to potentially irrational subjects at a higher price, traders buy the asset above its fundamental price. Lei *et al.* (2001) tested whether speculation is responsible for experimental bubbles and suggest that their occurrence is rather associated with an experimenter demand effect, i.e. the lack of alternative activity in the lab.

However, the role of common expectations among subjects has been emphasised in various experiments. In order to analyse this role, Porter and Smith (1995) and Noussair and Tucker (2006) have introduced a futures market, which reduces bubble amplitude, suggesting that uncertainty about others’ behaviour is responsible for bubble formation. Just like futures markets, benign messages can help traders form common expectations. Corgnet *et al.* (2010) analyse the role of public information and show that qualitative announcements may reduce the development of experimental bubbles, notably depending on their content.⁵ Subjects’ inexperience is particularly important in the formation of experimental bubbles because common expectations among subjects may only be reached with experience (Dufwenberg *et al.*, 2005). Hirota and Sunder (2007) underline that common information about the dividend probabilistic structure does not lead to common expectations. They explain that deviations of observed prices from fundamentals in experimental asset

⁴Alternative specifications of coordination games under dispersed information, such as the global game approach, have been experimentally tested. See e.g. Cabrales *et al.* (2007) and Heinemann *et al.* (2004, 2009).

⁵See also Ackert *et al.* (2004) for experimental evidence on the role of disclosure in experimental asset markets. For recent experimental evidence on the role of information acquisition on market efficiency see e.g. Alfaro *et al.* (2011) and Middeldorp and Rozenkranz (2011).

markets come from the difficulty for investors to backward induct the fundamental value of the asset. It is the absence of a dividend anchor that generates price bubbles. Akiyama *et al.* (2013) analyse to what extent bubbles are caused by strategic uncertainty. By comparing markets with computer traders and a single human subject to markets with only human subjects, they are able to isolate the effect of strategic uncertainty and quantify its impact. They attribute the source of mis-pricing in experimental markets to both strategic uncertainty⁶ and confusion.

A common feature shared by the experimental asset markets literature dealing with the lack of common expectations is that strategic uncertainty is (at least partly) responsible for the development of bubbles. Moreover, except in some specific design (e.g. Akiyama *et al.* (2013)), bubbles experiments do not separate fundamental from strategic uncertainty.⁷ By contrast, our simple set-up allows to disentangle the role of fundamental versus strategic uncertainty, by varying the nature (public or private) and precision of information and offering to subjects the possibility to choose between playing a fundamental or a coordination game. It provides clear theoretical benchmark predictions conditional on the structure of uncertainty and their experimental counterparts. Making use of the distinction between fundamental and strategic uncertainty, our conclusions appear to stand in contrast to that of the experimental asset market literature: speculation positively depends on fundamental rather than strategic uncertainty. Typically, by reducing strategic uncertainty, noisy public information may increase speculation. However, when fundamental uncertainty is dramatically reduced by very precise private signals, the subjects may find it easier to guess the fundamental than to anticipate others' behavior, making strategic uncertainty appear large in comparison to fundamental uncertainty.

The remaining of the paper is structured as follows. Section 2 presents the theoretical framework. Section 3 develops the experimental design and Section 4 presents and comments 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.

⁶In the same vein, Bosch-Rosa *et al.* (2017) observe no bubble when sophisticated subjects play with each other.

⁷There may be more strategic uncertainty when the fundamental is stochastic (or declining) because it creates more confusion (Huber and Kirchler (2012) and Hubert *et al.* (2012)).

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 θ 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

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

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.⁸

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 θ 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, $(\mathbf{r}, (\theta, \dots, \theta))$ 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 $\mathbf{a} = (y, \dots, y)$), 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 $\mathbf{r} = (1, \dots, 1)$.

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 θ . 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 ε_i 's being identically and independently distributed across agents and independently distributed with respect to η . 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 sig-

⁸MS 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.

nals, with weights proportional to the corresponding precisions α and β : $\mathbb{E}(\theta \mid x_i, y) = (\alpha y + \beta x_i)/(\alpha + \beta)$.

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: $\mathbb{E}(\theta \mid x_i, y_i) = (\alpha y_i + \beta x_i)/(\alpha + \beta) = \mathbb{E}(a_j \mid x_i, y_i)$. 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 $\max_{a_i} \mathbb{E}(u(\mathbf{a}, \theta; r_i) \mid x_i, y)$, is given by

$$a_i^* = \underbrace{\frac{\alpha}{\alpha + \beta} \left(1 + \frac{\beta R_i}{\alpha + \beta(1 - R)}\right)}_{\kappa_i} y + \underbrace{\frac{\beta}{\alpha + \beta} \left(1 - \frac{\alpha R_i}{\alpha + \beta(1 - R)}\right)}_{1 - \kappa_i} x_i, \quad (2)$$

with $R_i \equiv \frac{(1 - 1/n)r_i}{1 - (1/n)r_i}$ and $R \equiv \frac{1}{n} \sum_j R_j$

(see Appendix 7.1 for details about the derivation).⁹ Thus, the second stage equilibrium is an action profile $\mathbf{a}^*(\mathbf{r})$ 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 $a_i^*(r_i, \mathbf{r}_{-i})$ 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 κ_i put on the public signal increases with the relative precision α/β 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

⁹Notice that we are excluding a second stage equilibrium with sunspots, since we want to focus on the role of public information providing some content on the fundamental. Allowing for sunspots, we would obtain a continuum of equilibria for $r = 1$ at the first stage, with any sunspot yielding a different second stage equilibrium. As we shall see, the third game of our experimental design rules out the possibility of this kind of sunspots as subjects are not provided with any other information than the signals on the fundamental.

utility of agent i , namely

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

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 $U(r_i, R_{-i}^1, R_{-i}^2, n, \beta/\alpha)$, 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 κ and R cease to depend upon the choice of r_i , and $R_i = r_i$. The expected utility then becomes

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

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

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

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

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

so that the function $\mathcal{U}(\cdot, \kappa_i(\cdot))$ 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 $((1, \dots, 1), (y, \dots, y))$, 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 ($\mathbf{r} = (1, \dots, 1)$), 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 ($\mathbf{a} = (y, \dots, y)$).
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 aeconomici*. Recurring to a laboratory experiment represents a natural way to test these assumptions, as real data may be difficult to collect and analyse.¹⁰ 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 $n \rightarrow \infty$. As a higher mean square R_{-i}^2 together with an unchanged mean R_{-i}^1 , 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_{-i}^2 compatible with a given value of the mean R_{-i}^1).

¹⁰This 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.

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_{-i}^1 and R_{-i}^2 . 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 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 $[50, 950]$.¹¹ 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.¹² 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.¹³

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

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

¹¹Note that participants were not told about the support of the distribution to avoid the skewness of the posterior distribution.

¹²The 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.

¹³While both actions and estimations were incentivized, our analysis is not subject to problems of hedging if participants are risk neutral. Indeed, incentives for estimations were designed much lower than those of actions on purpose. Moreover, as becomes clear from Appendix 7.10, there is a strong coherence between individual estimations and decisions.

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

$$200 - (E_1 - Z)^2, \quad (8)$$

and that with participant i 's estimation E_2 :

$$200 - (E_2 - \bar{D}_2)^2. \quad (9)$$

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 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 2.¹⁴ 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 $\sigma_\eta^2 = 16$.

¹⁴More 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.

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.

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	$x_{i1} \sim N(Z, 8), x_{i2} \sim N(Z, 8)$	$\frac{x_{i1} + x_{i2}}{2}$	$\frac{x_{i1} + x_{i2}}{2}$	$\frac{x_{i1} + x_{i2}}{2}$	$\{0, \dots, 1\}$
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

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 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.¹⁵ 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.¹⁶ The experiment started after all subjects had given the correct answers to these questions.

¹⁵ORSEE 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).

¹⁶The understanding questionnaire is available from the authors upon request.

After each period, subjects received some feedback about realised values and choices.¹⁷ 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.

4 Experimental results

The results of the experiment concerning game 3 are presented in the following manner.¹⁸ 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 7.7.¹⁹

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.²⁰

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 1.²¹ However, subjects played in line with theoretical predictions in the sense that they more

¹⁷In 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.

¹⁸Descriptive 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.

¹⁹We 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.

²⁰The relative frequency of weights on the coordination motive per group is reported in Appendix 7.8.

²¹When 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.

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	$\{0, \dots, 1\}$	1

Table 2: Average weight on the coordination motive

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 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.*

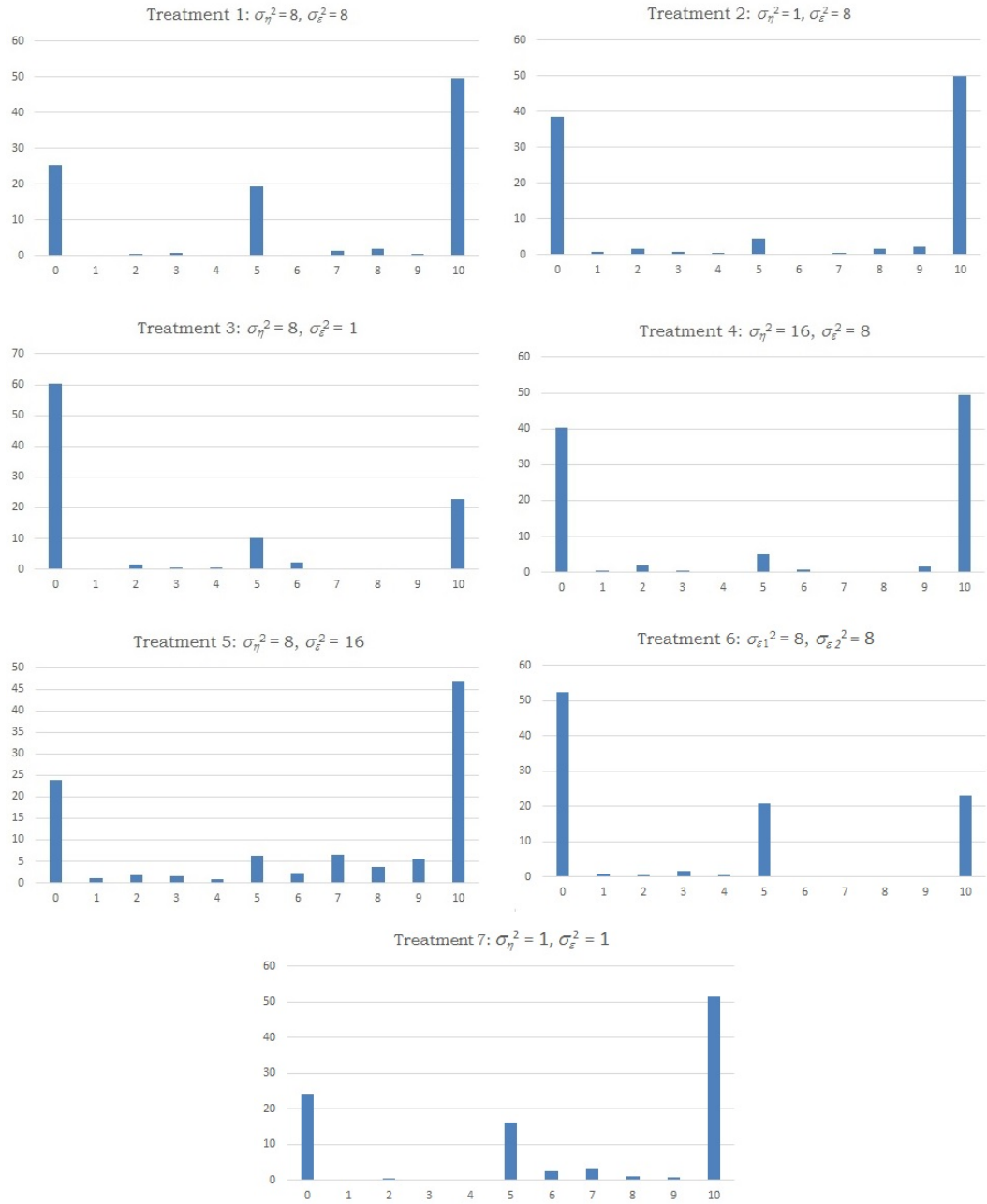


Figure 1: Relative frequency of weights D_1 on the coordination motive per treatment

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.²²

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. $\text{Theoretical}|_{th.stage1}$ 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 .²³ 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.

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
$\text{Theoretical} _{th.stage1}$	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 ($\text{Theoretical}|_{th.stage1}$) can be rejected.²⁴ 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 fre-

²²In 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.

²³The 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.

²⁴There 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.

quency 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.²⁵ 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 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.

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 treatment²⁶ and for all the treatments taken together²⁷:

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

²⁵Appendix 7.9 depicts the relative frequency of weight on Y in D_2 for each group.

²⁶Clusters were used for groups.

²⁷Clusters were used for both treatments and groups.



Figure 2: Relative frequency of weights on Y in D_2

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

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

	All	Tr1	Tr2	Tr3	Tr4	Tr5	Tr6	Tr7
Constant	0.5477*** (0.0226)	0.5224*** (0.0205)	0.6933*** (0.0443)	0.3688*** (0.0431)	0.4970*** (0.0634)	0.6892*** (0.0478)	0.4963*** (0.0068)	0.5697*** (0.0444)
$\frac{D_{1it}}{10}$	0.0990*** (0.0184)	0.1718*** (0.0449)	0.0856*** (0.0240)	0.1495* (0.0873)	0.1480*** (0.0174)	0.0622* (0.0333)	0.0021 (0.0096)	0.0535 (0.0556)
ν_i	0.1965	0.1607	0.1793	0.2024	0.2213	0.1975	0.0000	0.1824
ϵ_{it}	0.1753	0.1628	0.1580	0.2262	0.1735	0.1732	0.1458	0.1738
δ	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
χ^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)

Results²⁸ 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.*

²⁸Note that δ corresponds to the proportion of variation due to the individual specific term. If δ 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.

5 Conclusion

While standard asset market experiments in the tradition of Smith *et al.* (1988) usually exhibit price deviations from fundamentals, their complexity does not simply allow to disentangle the role of fundamental versus strategic uncertainty in bubble formation. To analyse the relative impact of each type of uncertainty, we resort to a simple beauty contest model that we bring to the lab. Our model intends to capture the common interest financial agents may have in coordinating on the same price. Indeed, although inherent to Keynes' beauty contest metaphor, we believe that 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 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 $\max_{a_i} \mathbb{E}(u(\mathbf{a}, \theta; r_i) | x_i, y)$, where $\mathbb{E}(\cdot | x_i, y)$ is the expectation operator conditional on the two signals received, and where the utility function u is given by

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

The first order condition yields

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

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 $a_i = \mathbb{E}(a_i | x_i, y)$, we have

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

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

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

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

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

By identification of the coefficient κ_i , we obtain

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

Plugging the expression of κ into (14) yields:

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

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

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

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 :

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

Using equations (16) and (18), we can see that κ and κ_i are functions of $(r_i, R_{-i}^1, n, \beta/\alpha)$, where $R_{-i}^1 = \frac{1}{n-1} \sum_{j \neq i} R_j$ 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 $R_{-i}^2 = \frac{1}{n-1} \sum_{j \neq i} R_j^2$, the mean square (the second moment about the origin) of the same R_j 's. Thus,

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

7.3 Derivation of the subgame perfect equilibrium

Referring to the expression of $U(r_i, R_{-i}^1, R_{-i}^2, n, \beta/\alpha)$ in equation (19), it is clear that dispersion of other agents' decisions (a high value of R_{-i}^2 , given R_{-i}^1), hence dispersion of the κ_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_{-i}^2 compatible with a given value of the mean R_{-i}^1 . This case results from taking the R_j 's symmetrically disposed at a maximum distance about the mean R_{-i}^1 . 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_{-i}^1 , denoted as ρ for brevity, and the ratio of precisions β/α .

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

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

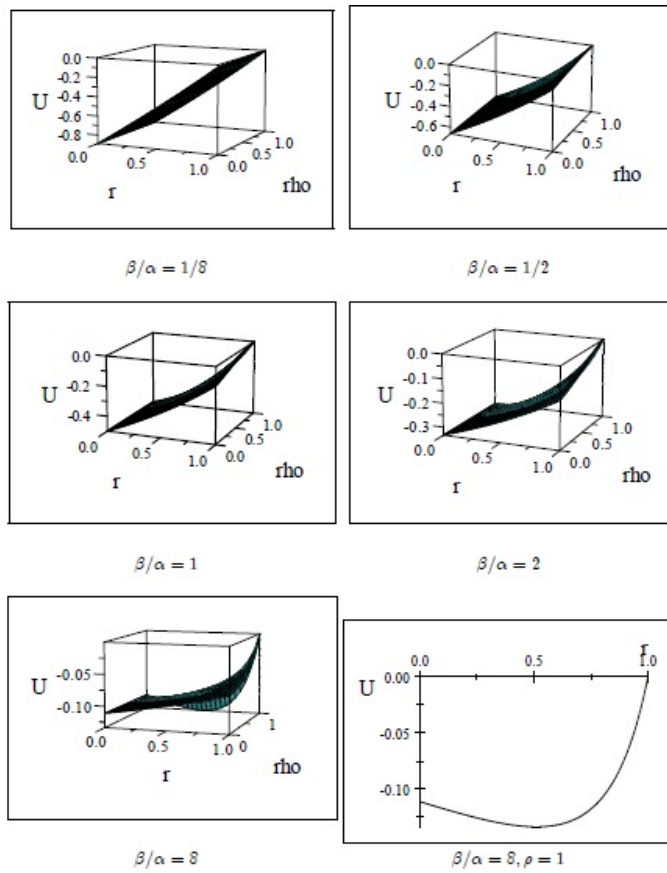
that is, $R_{-i}^2 = 1.8\rho^2 - 0.8 \max(2\rho - 1, 0)$.

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

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

These simulations, represented in the following figure, show that $\widehat{U}(\cdot, \rho, \beta/\alpha)$ 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 ρ put by other agents on the coordination motive. However, $\widehat{U}(\cdot, \rho, \beta/\alpha)$ is then strictly convex, with $\widehat{U}(1, \rho, \beta/\alpha) > \widehat{U}(0, \rho, \beta/\alpha)$. We conclude that $\widehat{U}(\cdot, \rho, \beta/\alpha)$ is always maximised at $r_i = 1$ in the the cases relevant for our experiment.



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**

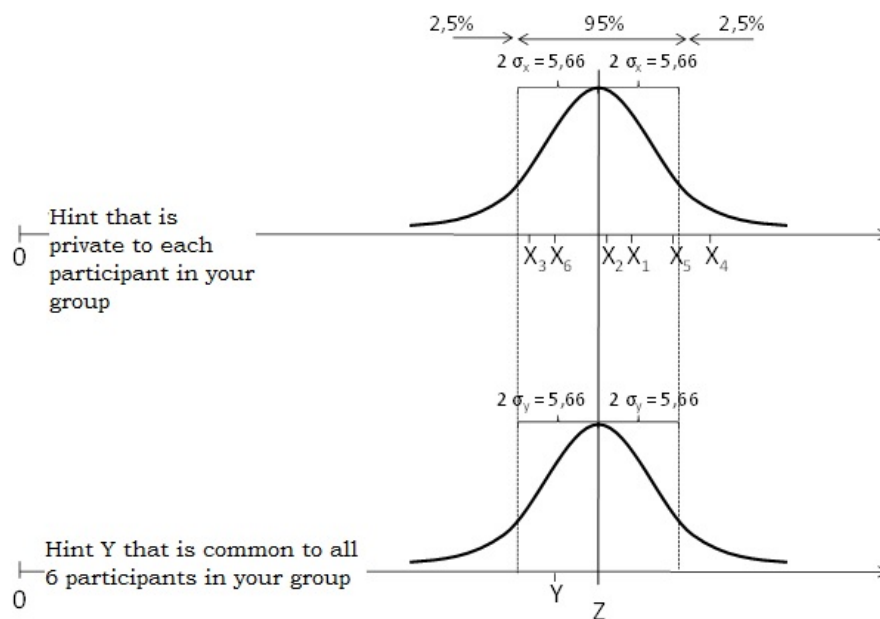
Each participant receives at each second sub-period a private hint X on the unknown number Z . Each private hint is centered on Z and contains an error that is randomly

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.

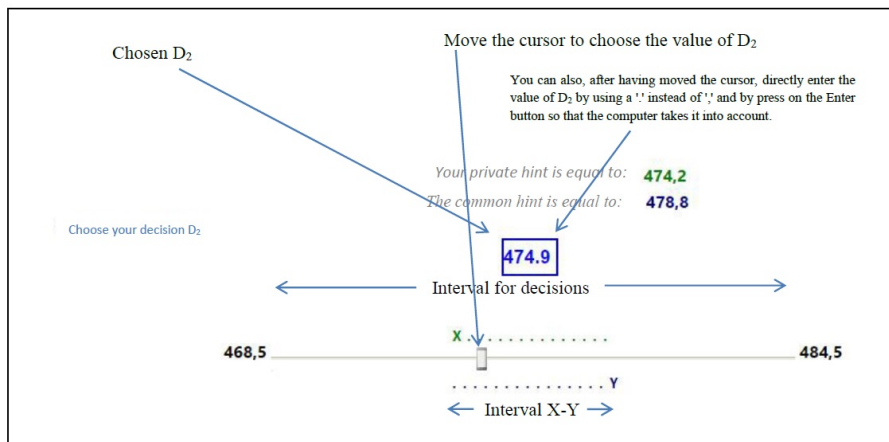
How to make your estimations E_1 and E_2 ?

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

econ *play*
Bienvenue lees_1 [Se déconnecter](#)

Vous êtes le joueur 1
Vous êtes dans le jeu 3
Période n° : 4 / 25

Vous êtes à la sous-période 1

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.

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

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

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

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.

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

econ *Flas* Bienvenue lees_1 Se déconnecter

Vous êtes le joueur 1 Vous êtes dans le jeu 3 Période n° : 4 / 25
 Vous êtes à la sous-période 2

Résultat de la sous-période 1 : - Votre choix D1 : 5. Vous choisissez ainsi d'être rémunéré à la sous-période 2 par:
 $400 - 5 \times (D_2 - Z)^2 - 5 \times (D_2 - \text{Moyenne des décisions D}_2 \text{ du groupe})^2$
 - Choix de D1 des 5 autres participants: 0, 10, 10, 0 et 5.

Estimation de E1, E2 et choix de D2
 Veuillez choisir vos E1, E2 et D2 en déplaçant le curseur ci-dessous.
 Vous pouvez également, après avoir déplacé les curseurs, entrer directement les valeurs de E1, E2 et D2 en utilisant un "." à la place de la "," puis appuyez sur la touche Entrée pour chacune de ces 3 valeurs pour qu'elle soit prise en compte.

Votre valeur indicative privée X est égale à : **719,8**
 La valeur indicative commune Y est égale à : **717,7**

E1: Estimez la valeur de Z: X
 712,24 723,56
 Y

E2: Estimez la décision D2 moyenne du groupe: X
 712,24 723,56
 Y

Choisissez votre décision D2: X
 712,24 723,56
 Y

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 it²⁹) 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 $D_2 = \text{Sunspot}$ (or values close to the sunspot in a range of 5% around it³⁰) is played most of the time in game 2.

²⁹This 5% tolerance interval intends to correct for mistakes due to an uneasy choice made owing to a slider on the interval [0; 2000].

³⁰This 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 $D_2 = \text{Sunspot} \pm 5\%$ 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

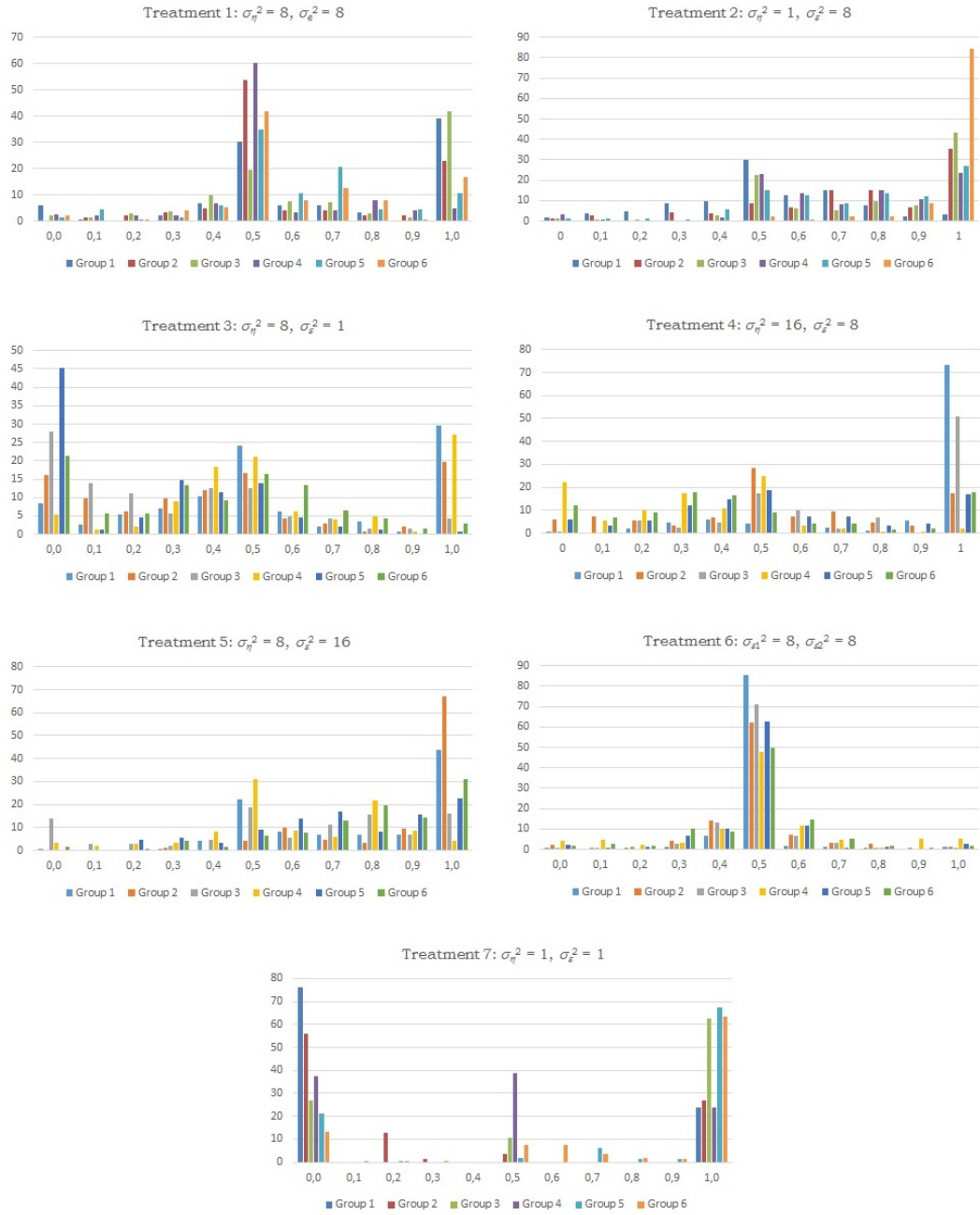


Figure 4: Relative frequency of weights on Y in D_2 per treatment and group

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.³¹ 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,³² but larger than the equal weight of 0.5.

³¹We accounted for the missing data by performing a bootstrap analysis in Appendix 7.11.

³²In 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 $\mathbb{E}(D_2)$, 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:

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

where C_o is the constant, α 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*** (0.0490)	0.3332*** (0.0106)	0.1698*** (0.0631)	0.3476*** (0.0443)	0.3048*** (0.0328)	0.7256*** (0.0492)	0.3945*** (0.0403)	0.2109*** (0.0540)
$OptD_2cond$	0.1488** (0.0736)	0.4470*** (0.0270)	0.7750*** (0.0799)	0.1636* (0.0855)	0.4679*** (0.0617)	0.0052 (0.0285)	0.1999** (0.0825)	0.6085*** (0.0933)
ν_i	0.1134	0.0986	0.0215	0.1417	0.1468	0.1323	0.0000	0.1220
ϵ_{it}	0.1705	0.1466	0.1225	0.2138	0.1510	0.1742	0.1383	0.1436
δ	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
χ^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.³³ The coefficient for D_{1it} measures the effect of decision 1 on the dependent variable while all the other explanatory variables are constant.

³³Note 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*** (0.0239)	0.5460*** (0.0748)
TR2	0.1181** (0.0517)	0.1033* (0.0542)
TR3	-0.1825*** (0.0601)	-0.1834*** (0.0688)
TR4	-0.0442 (0.0799)	-0.0425 (0.0737)
TR5	0.0968* (0.0518)	0.0856* (0.0450)
TR6	-0.1048*** (0.0250)	-0.1147*** (0.0271)
$\frac{D_{1it}}{10}$	0.1038*** (0.0192)	
$OptD_2cond$		0.1272 (0.1086)
ν_i	0.1824	0.1117
ϵ_{it}	0.1755	0.1717
δ	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
χ^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*** (0.0462)	0.3275*** (0.0492)
TR4	-0.1621* (0.0866)	-0.0753 (0.0509)
TR7	-0.1405** (0.0592)	-0.0846** (0.0348)
$\frac{D_{1it}}{10}$	0.0947*** (0.0235)	
$OptD_2cond$		0.5583*** (0.0561)
ν_i	0.1975	0.1100
ϵ_{it}	0.1692	0.1408
δ	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
χ^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*** (0.0519)	0.3814*** (0.0706)
TR5	0.2870*** (0.0748)	0.2855*** (0.0811)
TR7	0.1651** (0.0690)	0.1652** (0.0770)
$\frac{D_{1it}}{10}$	0.0810** (0.0324)	
$OptD_2cond$		0.0815 (0.1291)
ν_i	0.1948	0.1341
ϵ_{it}	0.1925	0.1898
δ	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
χ^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