Public information and the concern for coordination

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Keywords:

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

JEL codes:
D84, C92, E12

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1 Introduction

Human beings often seek to conform to social norms. This applies both to the behavioural and cognitive levels. Rather than trying to get by themselves an accurate perception of reality, individuals find it easier to follow others. Public information – as long as it reduces strategic uncertainty – also helps to achieve better than private information the level of conformity to which individuals aspire. Instances of such social conformity can be easily found in many contexts, in particular in many economic contexts. Let us just evoke those of financial markets and industrial organization.

- First, take Keynes's metaphor of the *beauty contest*, the winner of which is the competitor whose choice is closest to the average choice of all the participants. Applied to stock markets, this means that investors who care about the short run valuation of their capital more than about its expected yield in the long run, may become so concerned with matching prospective market values that they are ready to disregard relevant information on fundamentals. They rather look for any information allowing to anticipate "what average opinion expects the average opinion to be" (Keynes 1936, p. 156).
- Second, take price competition between suppliers of close substitutes. As substitutability increases, information about the competitors' price policy may become more important than information about customers' behavior: a firm may easily be thrown out of the market if it fails to match the best price, be it in itself high or low from the customers' viewpoint. In this context, a public signal, which is also informative of others' decisions, responds to the competitors' concern for coordination, which may allow it to dominate any private signal, although more precise about fundamental characteristics of demand.

Because public information can drastically reduce strategic uncertainty, it may induce a preference for coordination as such, at the expense of fundamental motives colliding with an irreducible fundamental uncertainty. In the just mentioned contexts, this means that speculation may look more attractive for capitalists than investment (which is the intended lesson of Keynes's parable) or that managers' concern for coordination may be encouraged by firm owners in the framework of a delegation game (Cornand and Dos Santos Ferreira, 2020). Should rational agents all have access to a public signal received without noise and have perfect control over the weight of the fundamental and coordination components of their payoffs, they would make coordination all important, thus eliminating uncertainty. Of course, this is true under full rationality. As soon as there is a doubt about others' rationality, strategic uncertainty gets in and that conclusion may break down.

The aim of our paper is to conceptualize the choice to play a pure coordination game by revisiting Keynes's beauty contest and then to question whether the coordination motive dominates the fundamental motive when *homines sapientes* are involved instead of *homines* *œconomici*. We propose a simple theoretical framework and its experimental test that allows to understand under which informational circumstances subjects may be more interested in coordinating their activities than in guessing the fundamental state of nature.

In the famous representation of Keynes's beauty contest by Morris and Shin (2002, henceforth MS), 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 able to manipulate the weights that will be put on the fundamental and the coordination motives.

The contribution of the present paper 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 former strategic decisions. Those decisions may then end up in the full crowding out of the fundamental motive. 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 to favor the coordination over the fundamental motive. 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.

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 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 the fundamental or the 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 coordinate and thus on the consequent disconnection of the actions from fundamentals. Overall, our experiment shows that subjects play in line with theoretical predictions in the sense that they more often choose to play the coordination game and put more weight on the coordination motive when they receive both public and private signals than when they receive two private signals. Variations in the relative precision of public and private signals do not affect such a conclusion, since subjects always put more weight on the coordination than on the fundamental motive at the first stage, correspondingly putting a larger 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 under full rationality, is the case where public and private information precisions are very asymmetric. A simple model of bounded rationality can rationalize this finding by taking into account strategic uncertainty. Agents may consider that others play randomly. If an agent has good reasons to believe that others' actions will not be too different from her own (because the precisions of public and private signals are not too asymmetric), she will naturally favor the coordination motive. Otherwise, high asymmetry implying high precision of one of the two signals, she will favor the fundamental motive. So, 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 precisions of public and private signals are very asymmetric.

Relation to the literature

Our paper contributes to the theoretical literature on beauty contest games initiated by Morris and Shin (2002)[1] and to its experimental counterparts (Cornand and Heinemann, 2014, Baeriswyl and Cornand, 2014, 2016, and Fehr *et al.*, 2019).[2] In contrast to this literature, the aim of the present paper is to focus on the strategic choice of the weights put on the two motives of the beauty contest affecting the payoff structure, and not on the strategic choice of the weights attributed 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. This paper relates to Cornand and Dos Santos Ferreira (2020), where the weights affecting the payoff structure in a differentiated duopoly can be manipulated by firm owners at the first stage of a delegation game.

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.

[1]This 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).

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

2 Theoretical framework

MS introduce a beauty contest 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 average action in the population, according to a trade-off between the two motives. However, while the relative weight agents put on each motive of MS's beauty contest game is fully exogenous, we take it as a strategic variable to be chosen at a preliminary stage. In other words, MS's utility will be viewed as the second stage payoff of a two-stage game, in which agents first choose the relative weights they attribute to the coordination and fundamental motives before making the final decision. This model accounts for the potential disconnection between the actions and the fundamental in a very simple manner.

2.1 A two-stage valuation game under different kinds of information

There is a finite number n of agents. The utility function of 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 a_i. The second component is the 'beauty contest' term: the loss is increasing in the distance between i's chosen action a_i and the average action $\frac{1}{n}\sum_j a_j$. Formally, the utility of agent i is given by

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

where $\mathbf{a}$ is the action profile over all the agents and r_i is the weight agent i has decided to put on the beauty contest term.[3]

The timing of the game is as follows. First, each agent i chooses r_i: he evaluates which motive he favors to maximize 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 favored.

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, $(\mathbf{r}, (\theta, ..., \theta))$ 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

[3]MS take a third motive into account: each agent wants to choose an action close to the average action, but would also like to succeed better than the others. In the MS framework, in which the set of agents is a continuum, this *competition motive* appears as an externality: it influences the agents' welfare, offsetting the influence of the coordination motive, 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. The competition motive may then play a significant role, which is highlighted in Cornand and Dos Santos Ferreira (2019, 2020). Here, however, we have preferred to ignore this motive in order to simplify the task of the participants to the experiment.

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

Following the literature in the vein of MS, we shall however assume *imperfect* and *heterogeneous* (or *dispersed*) *information*. Between the two stages of the game, each agent i receives two signals on the unknown fundamental value 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 signals, 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: $\mathbb{E}(\theta \mid x_i, y_i) = (\alpha y_i + \beta x_i)/(\alpha + \beta) = \mathbb{E}(a_j \mid x_i, y_i)$, for $j \neq i$. Here, with independently distributed signals, the agent's response to each signal is entirely determined by the relative precision of that signal. Of course, should the signals be correlated, they would convey some additional information on others' actions, and the response to each signal would have to be modulated according to the weight put on the coordination motive. With uncorrelated signals, however, we would be back to the perfect information case, in the sense that 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 to be noisy about the fundamental. These assumptions insure that the public signal is always relevant (as concerns 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. To derive agent i's expectation conditional on his information $\mathbb{E}(a_j \mid x_i, y)$ of any other agent j's action, we assume, following MS, that any agent $j \in \{1, ..., n\}$ responds to the signals by following the same linear strategy $a_j = \kappa_j y + (1 - \kappa_j) x_j$. The solution to the problem

$\max_{a_i} \mathbb{E}(u(\mathbf{a}, \theta; r_i) \mid x_i, y)$ is given by

$$a_i = \underbrace{\frac{\alpha}{\alpha+\beta} + \frac{\beta}{\alpha+\beta} R_i \kappa_{-i}}_{\kappa_i} y + \underbrace{\frac{\beta}{\alpha+\beta} - \frac{\beta}{\alpha+\beta} R_i \kappa_{-i}}_{1-\kappa_i} x_i, \tag{2}$$

$$\text{with } R_i \equiv \frac{r_i\left(1-\frac{1}{n}\right)^2}{1-r_i+r_i\left(1-\frac{1}{n}\right)^2} \text{ and } \kappa_{-i} = \frac{1}{n-1}\sum_{j \neq i} \kappa_j$$

(see Appendix A.1 for details about this derivation).[4] For any agent i, the best reply a_i is an arithmetic mean of the two signals y and x_i received by that agent. The relative weight κ_i (resp. $1-\kappa_i$) put on the public (resp. private) signal is equal to the relative precision $\alpha/(\alpha+\beta)$ (resp. $\beta/(\alpha+\beta)$) of this signal plus (resp. minus) an index of the *concern for coordination* of agent i. This concern for coordination increases with the relative precision $\beta/(\alpha+\beta)$ of the private signal (deterring the agents from the public signal), with the relative weight r_i put on the coordination motive together with the number n of agents (R_i is monotonically increasing in r_i and in n) and with the average relative weight κ_{-i} put on the public signal by the other agents.

An alternative expression for κ_i, referring to the average $\kappa = \frac{1}{n}\sum_j \kappa_j = \frac{1}{n}((n-1)\kappa_{-i} + \kappa_i)$, instead of κ_{-i}, is

$$\kappa_i = \frac{\alpha/\beta + \frac{R_i}{n-1} n\kappa}{1 + \alpha/\beta + \frac{R_i}{n-1}}. \tag{3}$$

Here κ_i appears as an increasing function of α/β and, as $n\kappa > \alpha/(\alpha+\beta)$, of R_i, hence of r_i.

To derive the subgame perfect equilibrium, we maximize with respect to r_i the expected utility of agent i at second stage equilibrium $\mathbf{a}^*(\mathbf{r})$:

$$\begin{aligned} & \mathbb{E}(u(\mathbf{a}^*(\mathbf{r}), \theta; r_i)) \\ = \; & \underbrace{-\frac{1}{\alpha}\left[\kappa_i^2 + (\alpha/\beta)(1-\kappa_i)^2 + r_i \left(\begin{array}{c} \kappa(\kappa - 2\kappa_i) \\ +\frac{\alpha/\beta}{n}\left(\frac{1}{n}\sum_j (1-\kappa_j)^2 - 2(1-\kappa_i)^2\right) \end{array} \right)\right]}_{l(r_i, \kappa_i, \kappa_{-i}, n, \alpha/\beta)}, \end{aligned} \tag{4}$$

with $\kappa = \frac{1}{n}\sum_j \kappa_j$ (see Appendix A.2 for details about this derivation).

The limit case of an infinite number of agents allows a simple derivation of the subgame perfect equilibrium. Indeed, in this case the loss function becomes

$$l\left(r_i, \kappa_i^*, \kappa_{-i}^*, n, \alpha/\beta\right) = \kappa_i^{*2} + (\alpha/\beta)(1-\kappa_i^*)^2 + r_i \kappa^*(\kappa^* - 2\kappa_i^*). \tag{5}$$

[4]Notice 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.

By the envelope theorem, we do not have to consider the impact of the variations of r_i through κ_i^*, as $\partial l\left(r_i, \kappa_i^*, \kappa_{-i}^*, n, \alpha/\beta\right)/\partial\kappa_i = 0$. Also, since by (2) and (3), with $n \to \infty$, $\kappa_i^* = (\alpha/\beta + r_i\kappa^*)/(1+\alpha/\beta)$, hence $\kappa^* = (\alpha/\beta)/(\alpha/\beta + 1 - r)$, with $r = \frac{1}{n}\sum_j r_j$, we see that changes in r_i cease to have a significant impact on κ^* when n becomes indefinitely large. As a consequence, it suffices to determine the signal of

$$\kappa^* - 2\kappa_i^* = \frac{\kappa^*(1+\alpha/\beta - 2r_i) - 2\alpha/\beta}{1+\alpha/\beta} = -\frac{\alpha/\beta}{1+\alpha/\beta}\frac{\alpha/\beta + 1 + 2(r_i - r)}{\alpha/\beta + 1 - r}. \quad (6)$$

This expression is decreasing in r_i and negative if $r_i = 1$. So, either it is always negative and the loss function is minimized at $r_i = 1$, or it is positive at $r_i = 0$, and we must compare $l\left(\cdot, \kappa_i^*, \kappa_{-i}^*, \infty, \alpha/\beta\right)$ at the two extremities of its domain:

$$l\left(1, \kappa_i^*, \kappa_{-i}^*, \infty, \alpha/\beta\right) = \frac{\alpha/\beta}{1+\alpha/\beta}(1-\kappa^*)^2 < \frac{\alpha/\beta}{1+\alpha/\beta} = l\left(0, \kappa_i^*, \kappa_{-i}^*, \infty, \alpha/\beta\right), \quad (7)$$

which leads to the same conclusion. We can accordingly formulate the following proposition.

Proposition *In the limit case of an infinite number of agents, the unique subgame perfect equilibrium (a dominant strategy equilibrium)* is $((1,...,1),(y,...,y))$, *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 on the weight r_i through the decisions this weight induces at the second stage of the game. In Appendix A.2, 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 ($\mathbf{r} = (1,...,1)$), 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 ($\mathbf{a} = (y,...,y)$).*
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 (and uncorrelated). At the second stage,*

because of the coordination motive, the weight on the public signal when agents receive public and private signals is higher than the weight put on any of the two private signals when agents receive two private signals.

3 The experiment

One may question whether the theoretical predictions derived in Section 2 hold in practice, when *homines sapientes* are involved in the valuation game instead of *homines œconomici*. Recurring to a laboratory experiment represents a natural way to test these assumptions, as real data may be difficult to collect and analyze.[5] The theoretical model in Section 2 is adjusted to an experimental framework. We discuss in this section the chosen parameter values for each treatment, the corresponding theoretical prediction, and the general procedure of the experiment.

3.1 Treatment parameters and equilibrium values

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

3.1.1 Adjusted theoretical predictions to a finite number of participants

We focus on the parameter values $n = 6$ and $\beta/\alpha \in \{1/8, 1/2, 1, 2, 8\}$, which correspond to the cases we deal with in the different treatments of our experiment, as explained in Section 3.1.3. These parameter values ensure that we obtain the same theoretical predictions as in the theoretical framework of Section 2 when $n \to \infty$. The simulations presented in Appendix A.2 show that we 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 familiarize subjects with the experiment and are considered as an incentivized 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 $[50, 950]$.[6] Each period was divided into two sub-

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

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

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.[7] 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.[8]

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

$$400 - (10 - D_1)(D_2 - Z)^2 - D_1(D_2 - \overline{D}_2)^2. \tag{8}$$

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

$$200 - (E_1 - Z)^2, \tag{9}$$

and that with participant i's estimation E_2:

$$200 - (E_2 - \bar{D_2})^2. \tag{10}$$

In game 1 (5 periods), after the first sub-period, the realized value of Z is commonly revealed. In this game, no estimation is asked for and subjects are simply rewarded according to (8). 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 (8) and (10). The interval for decisions is also $[0, 2000]$. In half of the sessions, for each treatment, we reversed the order of games

[7]The second sub-period was very similar to the experiments by Cornand and Heinemann (2014) and Baeriswyl and Cornand (2014, 2016), which aimed at testing variations of the beauty contest game of MS. The design was slightly modified as, contrary to Baeriswyl and Cornand, we allowed subjects to make choices outside the interval defined by the two signals participants received on the fundamental. Instead, and differently from Cornand and Heinemann, who made a restriction of possible choices on the interval defined by the public signal minus or plus 20 and observed many decisions outside the interval defined by the signals, we proposed a screen design that emphasized the position of signals on the interval of possible choices. See the example of screens provided in Appendix B.2. 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.

[8]While 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 B.3, there is a strong coherence between individual estimations and decisions.

1 and 2.[9] 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 (8), (9), and (10).

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 $\varepsilon_i \sim N(0, 1/\beta)$. The public signal received by every participant of each group is distributed as $y = Z + \eta$ with $\eta \sim N(0, 1/\alpha)$. Whereas each participant may receive a different private signal x_i, the public signal y is the same for all participants. In Treatment 1, $\alpha = \beta = 1/8$. In Treatment 7, $\alpha = \beta = 1$.

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, $\beta = 1/8$ and $\alpha = 1$. In Treatment 5, $\beta = 1/16$ and $\alpha = 1/8$.

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, $\beta = 1$ and $\alpha = 1/8$. In Treatment 4, $\beta = 1/8$ and $\alpha = 1/16$.

Treatment 6 - Private vs. private signals, same precision Each participant receives 2 private signals on Z. Each of the 2 private signals may have a different distribution: $x_{i1} = Z + \varepsilon_{i1}$ with $\varepsilon_{i1} \sim N(0, 1/\beta_1)$ and $x_{i2} = Z + \varepsilon_{i2}$ with $\varepsilon_{i2} \sim N(0, 1/\beta_2)$. The private signals may thus be different from one participant to the next. In Treatment 6, $\beta_1 = \beta_2 = 1/8$.

3.1.4 Summary

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

We will proceed to comparisons between observations and theoretical values as well as treatment comparisons. Comparing Treatments 1 and 6 directly allows to account for the role of the public signal. Comparing Treatment 1 and any of the Treatments 2 to 5 and Treatment 7 to Treatments 2 and 3 allows to evaluate the role of increasing/decreasing the precision of either public or private signals.

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

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

Table 1: Experiment parameters and theoretical predictions

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.[10] 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 B.1. 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.[11] The experiment started after all subjects had given the correct answers to these questions.

After each period, subjects received some feedback about realized values and choices.[12] 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.

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

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

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

4 Experimental results

The results of the experiment concerning game 3 are presented in the following manner.[13] First, we analyze 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.[14] Finally, we analyze payoff incentives and convergence issues.

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, the left panels of Figure 1 depict the relative frequency of weights put by each participant on the coordination motive for each treatment.

Tr.	1 ($\alpha = \beta = 1/8$)	2 ($\alpha = 1, \beta = 1/8$)	3 ($\alpha = 1/8, \beta = 1$)	4 ($\alpha = 1/16, \beta = 1/8$)	5 ($\alpha = 1/8, \beta = 1/16$)	6 ($\beta_1 = \beta_2 = 1/8$)	7 ($\alpha = \beta = 1$)
Gr. 1	0.66	0.01	0.54	0.88	0.86	0.73	0.24
Gr. 2	0.71	0.55	0.06	0.58	0.85	0.10	0.31
Gr. 3	0.80	0.82	0.20	0.79	0.72	0.50	0.68
Gr. 4	0.65	0.68	0.79	0.26	0.46	0.37	0.43
Gr. 5	0.56	0.32	0.26	0.54	0.51	0.34	0.75
Gr. 6	0.40	0.99	0.00	0.22	0.50	0.04	0.77
Av.	0.63	0.56	0.31	0.54	0.65	0.35	0.53
Th.	1	1	1	1	1	$\{0, ...1\}$	1

Table 2: Average weight on the coordination motive

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.[15] However, subjects played in line with theoretical predictions in the sense that they more often chose to play the coordination game (Figure 1) and put more weight on the coordination motive (Table 2) when they received both public and private signals than when they received two private signals. There is a significant difference between Treatments 1 and 6 ($p = 0.0547$).

[13] 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. Descriptive statistics for games 1 and 2 are available from the authors upon request.

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

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

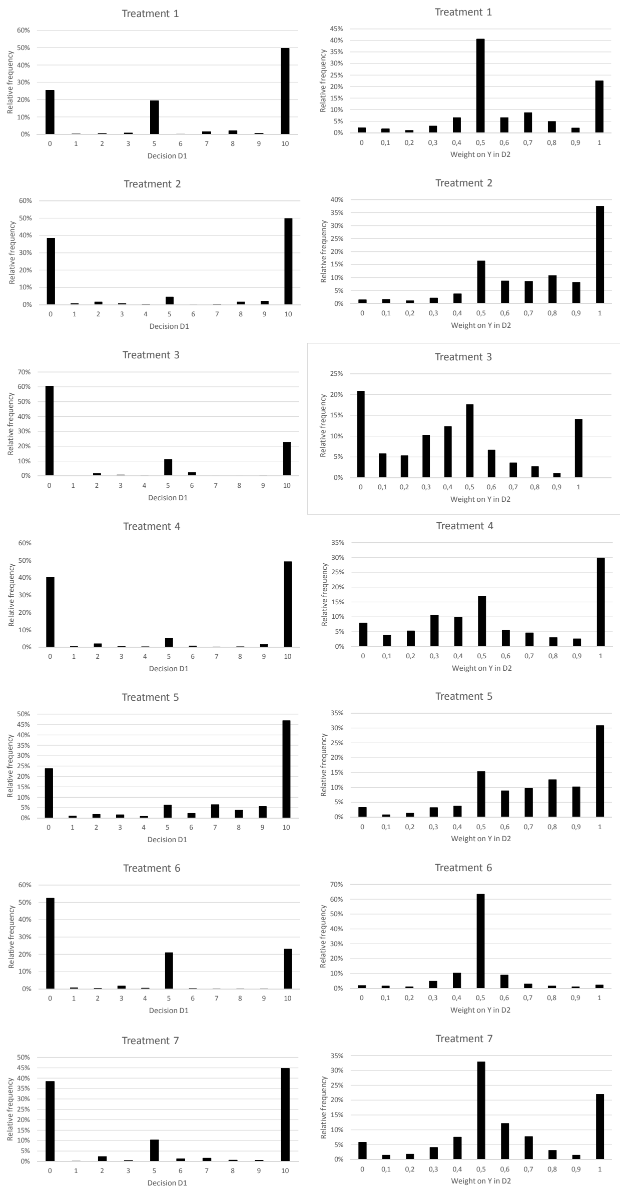

Figure 1: Relative frequency of decisions D_1 on the coordination motive (left panels) and of weights on Y in decision D_2 (right panels) per treatment

Result 1 *In line with theoretical predictions, subjects put more weight on the coordination motive when they receive both public and private signals than when they receive two private signals.*

According to theoretical predictions, variations in the relative precision of public and private signals should not affect this conclusion. However, there are significant differences between Treatments 1 vs. 3 ($p = 0.0547$) and Treatments 3 vs. 5 ($p = 0.0782$). As we shall see, this finding can be rationalized thanks to a simple model of bounded rationality.

Let us reconsider the model of Section 2, now abandoning full rationality and subgame perfection (which implied taking κ as a function $\kappa^*(\mathbf{r})$) and including instead *strategic uncertainty* due to bounded rationality. Suppose that agent i behaves non-strategically, setting at the second stage the weight $\kappa_i = \alpha/(\alpha+\beta)$, with zero concern for coordination, and taking other agents' second stage strategies κ_{-i} as constant, not as functions of $\mathbf{r}$ and so ignoring in particular the influence of his own first stage strategy r_i. This strategy is then chosen by simply minimizing the loss function $l(\cdot, \alpha/(\alpha+\beta), \kappa_{-i}, n, \alpha/\beta)$ defined by (4), at given κ_{-i}. Since this function is linear affine, we just have to look at the sign of the coefficient of r_i. Should agent i assume that the other agents intend to choose the same value $\alpha/(\alpha+\beta)$, so that $\kappa_j = \kappa_i$ for any j, the coefficient of r_i would be clearly negative, leading to the solution $r_i = 1$. However, if agent i is fully uncertain about the intended choice of the other agents, he may conjecture that others play randomly:[16] he assumes a random choice of κ_j for $j \neq i$. Assuming the uniform distribution over $[0,1]$, the first moments are $m_1 = 1/2$ and $m_2 = 1/3$, so that the coefficient of r_i becomes:

$$\left(\frac{1-1/n}{2} + \frac{(1/n)\,\alpha/\beta}{1+\alpha/\beta}\right)\left(\frac{1-1/n}{2} - \frac{(2-1/n)\,\alpha/\beta}{1+\alpha/\beta}\right) + (1/n)\,\alpha/\beta\left(\frac{1-1/n}{3} - \frac{2-1/n}{(1+\alpha/\beta)^2}\right). \tag{11}$$

If $\alpha/\beta = 1$, making again κ_i coincide with the mathematical expectation $1/2$ of κ_j for $j \neq i$, this coefficient is negative and we obtain the same result. However, as α/β becomes large or small enough, making κ_i differ substantively from $1/2$, this coefficient becomes positive, leading to the solution $r_i = 0$. Under the parameter configuration of our experiment, with $n = 6$, the coefficient of r_i is negative iff $0.226 < \alpha/\beta < 16.62$, so that the sole treatment where it becomes positive is Treatment 3, with $\alpha/\beta = 0.125$.

So in the case of limited reasoning, each agent neglecting the impact on other agents' decisions of his own actions and conjecturing a pure random behavior of the other agents, the preference for the coordination motive is restricted to situations that are not too asymmetric regarding the precisions of the public and private signals. A strong asymmetry will translate into a preference for the fundamental motive, as in Treatment 3.

When public and private information precisions are not too asymmetric (Treatments 1,

[16]This approach is close to that in terms of limited level of reasoning: it is similar to considering that an agent considers that others have a lower level of reasoning. Limited level of reasoning is a standard assumption in the beauty contest literature. See *e.g.* Nagel (1995), Hanaki *et al.* (2019).

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 public and private information precisions are asymmetric, subjects rather chose the fundamental game (Treatment 3). This observation contradicts theoretical predictions under full rationality. Indeed, according to the theory, subjects should choose the coordination game whatever the level of relative precision of public and private signals. In the lab, choosing the coordination game appeared too costly, as it typically entails a coordination problem at the second stage. This behavior goes in the direction of the theoretical predictions of our simple model of bounded rationality.

Result 2 *When public and private information precisions are not too asymmetric, subjects tend to put more weight on the coordination than on the fundamental motive at the first stage of the game. Otherwise, for instance private information being relatively very precise, subjects tend to favor the fundamental rather than the coordination motive at the first stage of the game. This behavior stands in contrast with theoretical predictions under full rationality but can be rationalized by a simple model of bounded rationality that captures strategic uncertainty.*

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 B.3, 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.[17]

The average weight assigned in the experiment to the public signal in D_2 is reported in Table 3 for each treatment and each group. Theoretical$|_{th.stage1}$ denotes the theoretical weight on the public signal conditional on the theoretical weight in the first stage decision r. Theoretical 1st order denotes the theoretical weight on the public signal in the theoretical first order expectation on the fundamental, while Av. Observed 1st order denotes the average weight on the public signal in the observed first order expectation on the fundamental E_1.[18] 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.

When participants received both public and private signals, the theoretical benchmark

[17] In Appendix B.3, 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.

[18] The weights on the public signal in the observed first order expectation E_1 on the fundamental per group are reported in Table 7 in Appendix B.3.

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

Table 3: Weight on the public signal Y in D_2

conditional on the theoretical first stage decision (Theoretical$|_{th.stage1}$) can be rejected.[19] 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. There is a significant difference between Treatments 1 and 6 ($p = 0.0036$). To get a more complete picture of the focal role of the public signal, the right panels of Figure 1 present the relative frequency of weights on the public signal per treatment. The higher the relative precision of the public signal, the more subjects played the public signal itself at the second stage. Table 3 also shows that the higher the relative precision of the public signal, the higher the weight on the public signal. Indeed, there is a significant difference between Treatments 1 and 2 ($p = 0.0782$), 1 and 3 ($p = 0.0103$), Treatments 3 and 5 ($p = 0.0091$), and Treatments 2 and 7 $p = 0.0547$) and 3 and 7 ($p = 0.0776$). 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 ($p = 0.4233$), and 1 and 5 ($p = 0.1994$)). 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 B.4).

Result 3 *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*

[19]There is overreaction to the public signal in the sense that participants to the experiment attributed a larger weight to the public signal in their decision D_2 than in their stated first order expectation on the fundamental (Observed 1st order) (results of Mann-Whitney tests for Treatments 1 and 4 are respectively $p = 0.0064$ and $p = 0.0250$). 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). 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.

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.

Recall that in the case of Treatment 3, our simple model of bounded rationality predicted that agents should choose $r_i = 1$ at the first stage, implying that the weight on the public signal at the second stage is given by $1/9$ (theoretical first order expectation of the fundamental). This can rationalize why the observed average weight on the public signal in Treatment 3 is the lowest. The next section analyses in more details the relation between decisions in the first and second stage of the game.

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, the theoretical benchmark cannot be rejected in Treatments 4 ($p = 0.6310$) and 7 ($p = 0.1495$).

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

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

To address the question whether subjects put a larger weight on the public signal in their second stage decision D_2 when they choose to be rewarded more by the coordination motive at the first stage, we resort to an analysis on individual data and estimate the following equation, both for each treatment[20] and for all the treatments taken together[21]:

$$\left|\frac{D_{2it} - X_{it}}{Y_t - X_{it}}\right| = Co + \gamma \frac{D_{1it}}{10} + (\epsilon_{it} + \nu_i), \tag{12}$$

where Co is the constant, D_{1it} is the decision D_1 of individual i at period t and γ 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***	0.5224***	0.6933***	0.3688***	0.4970***	0.6892***	0.4963***	0.5697***
	(0.0226)	(0.0205)	(0.0443)	(0.0431)	(0.0634)	(0.0478)	(0.0068)	(0.0444)
$\frac{D_{1it}}{10}$	0.0990***	0.1718***	0.0856***	0.1495*	0.1480***	0.0622*	0.0021	0.0535
	(0.0184)	(0.0449)	(0.0240)	(0.0873)	(0.0174)	(0.0333)	(0.0096)	(0.0556)
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 (12)

Results[22] 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 4 *In line with theoretical predictions, a higher average weight on the coordination motive implies a larger weight on the public signal.*

4.4 Payoff incentives and convergence

While previously stated results mainly go in the direction of the theory, an important deviation from theory is observed when the precisions of public and private information are asymmetric (Treatment 3). How can payoff incentives account for this deviation?

[20]Clusters were used for groups.

[21]Clusters were used for both treatments and groups.

[22]Note 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.

Figure 2 illustrates how payoffs are related to the weight assigned to the coordination motive in the first stage (left panels) and to the weight attributed to the public signal in the second stage (right panels). The dashed lines represent the realized payoffs according to the weights played in the experiment (on the x-axis). The solid line exhibits the theoretical payoffs for an agent deviating (on the x-axis) on the left panels from the equilibrium strategy in the first stage, given that all agents play the optimal behaviour at the second stage[23] and on the right panels from the equilibrium strategy in the second stage, given that all agents play the optimal behaviour at the first stage.

Because the payoff function is quadratic, payoffs are very flat around the maximum. Looking at the various measures of realized and expected payoffs, one can note nevertheless that deviations from first stage equilibrium are less costly in Treatments 2 and 7 and more costly in Treatment 4. They are equally intermediary costly in the remaining Treatments 1, 3 and 5. Despite similar incentives in the latter treatments, average weight on the coordination motive are quite different in Treatment 3 in comparison to the other treatments. This can be rationalized by the fact that playing the second stage optimal weight (or close to it) was very costly in Treatment 3 because other subjects deviated from the first stage optimum, in accordance with their low second stage weight (as analysed in 4.3). This translated in losses in realized payoffs from playing weight 1 or close to 1 on the public signal at the second stage (third left panel).

The average decisions D_1 and weights on the public signal in D_2 over periods are represented by the thick black lines on respectively the right and left panels of Figure 3. Some convergence is observed especially in Treatments 1, 2, 5 and 7. Although deviations were costly in Treatment 4, no convergence is observed. In conformity with the analysis of payoff incentives, no convergence is observed in Treatment 3. Finally, no convergence is observed in Treatment 6 as there is a relatively small initial deviation of the average weight on the public signal in decisions D_2 from the equilibrium.

Figure 3 also plots the relative frequency of decisions D_1 (left panels) and weights assigned to the public signal (right panels) per period for each treatment. It shows how the role of focal points evolves over time. Whereas the focal point 10 for D_1 becomes more important (and the focal point 0 less important) over periods in Treatments 1, 2, 4, 5 and 7, it does not become more important in Treatment 3. Accordingly, the focal point 1 for the weight on Y in D_2 also tends to become more important, especially in Treatments 1, 2 and 5. This indicates that subjects learn over periods that playing 10 in D_1 and Y for D_2 is associated with a rather good payoff in the Treatments 1, 2, 4, 5 and 7, but with a bad observed payoff in Treatment 3.

Result 4 *Convergence in the direction of theory is observed in all treatments with public and private information, except when the precisions of public and private are asymmetric.*

[23]In Treatment 6, such theoretical prediction cannot be derived since there are multiple equilibria in the first stage of the game.

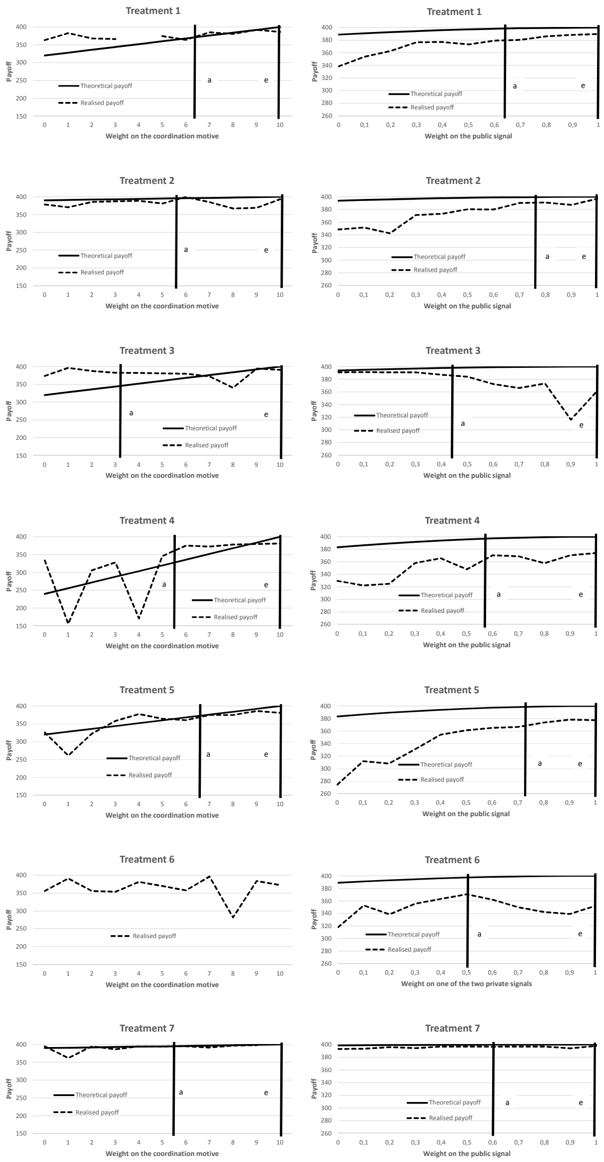

Figure 2: Realized payoffs and payoff incentives; e: theoretical equilibrium weight D_1, a: realized average weight

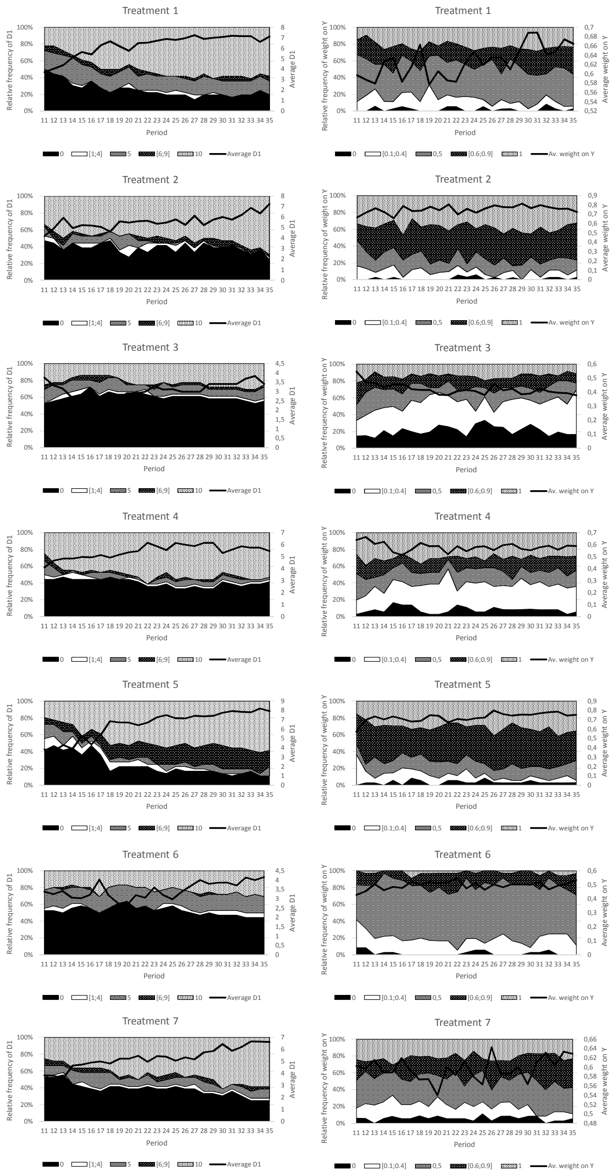

Figure 3: Relative frequency of decisions D_1 and weights assigned to the public signal per period for each treatment

5 Conclusion

Our model intends to capture the common interest agents may have in coordinating on the same action. Indeed, although inherent to Keynes' beauty contest metaphor, we believe that the idea that agents may 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 the participants in a beauty contest on the coordination and fundamental motives compounding their payoffs. These strategic decisions end up in the complete dominance of the coordination motive, crowding out the fundamental motive and hence resulting in a disconnection of participants' actions from fundamentals. While this disconnection between actions and fundamentals is trivial in the case where all the weight is exogenously put on the coordination motive, it becomes a crucial result in a context where agents may manipulate the weights on each motive.

We have developed a 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 indeed to favor the coordination motive over the fundamental motive, our experiment qualifies theoretical predictions when the precisions of public and private information are strongly asymmetric, one of them becoming very high. This makes coordination relatively costly, avoiding the disconnection from fundamentals. A simple model of bounded rationality can account for this observation.

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A Theory

A.1 Derivation of the second stage equilibrium

Recall that the problem of agent i at stage 2 is to maximize in a_i the expected utility value $\mathbb{E}\left(u\left(\mathbf{a},\theta;r_i\right)\mid x_i,y\right)$, where $\mathbb{E}(.\mid x_i,y)$ is the expectation operator conditional on the two signals received, and where the utility function u is given by

$$u(\mathbf{a},\theta;r_i) = -(1-r_i)\left(a_i-\theta\right)^2 - r_i\left(a_i - \frac{1}{n}\sum_j a_j\right)^2. \tag{13}$$

The first order condition yields

$$a_i = \frac{(1-r_i)\,\mathbb{E}\left(\theta\mid x_i,y\right) + r_i\left(1-\frac{1}{n}\right)^2\frac{1}{n-1}\sum_{j\neq i}\mathbb{E}\left(a_j\mid x_i,y\right)}{1-r_i+r_i\left(1-\frac{1}{n}\right)^2}. \tag{14}$$

Thus, the optimal action a_i is a weighted arithmetic mean of the expected fundamental value and of the expected average action of the other agents.

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

$$\begin{aligned}\frac{1}{n-1}\sum_{j\neq i}\mathbb{E}\left(a_j\mid x_i,y\right) &= \kappa_{-i}y+(1-\kappa_{-i})\frac{\alpha y+\beta x_i}{\alpha+\beta} \\ &= \frac{\alpha+\kappa_{-i}\beta}{\alpha+\beta}y+\frac{\left(1-\kappa_{-i}\right)\beta}{\alpha+\beta}x_i.\end{aligned} \tag{15}$$

Notice that, in the case of a second private signal y_i instead of the public signal y, we would simply have $\frac{1}{n-1}\sum_{j\neq i}\mathbb{E}\left(a_j\mid x_i,y_i\right) = \mathbb{E}(\theta\mid x_i,y_i)$, hence $a_i=\mathbb{E}(\theta\mid x_i,y_i)$ and $\kappa_i=\alpha/\left(\alpha+\beta\right)$, independently from r_i. In the case we are examining, of a public and a private signal, by inserting (15) in (14), we obtain for the optimal action of agent i:

$$\begin{aligned} a_i &= \frac{\alpha}{\alpha+\beta}y + \frac{\beta}{\alpha+\beta}x_i + \frac{r_i\left(1-\frac{1}{n}\right)^2 \frac{\kappa_{-i}\beta}{\alpha+\beta}\left(y-x_i\right)}{1-r_i+r_i\left(1-\frac{1}{n}\right)^2} \\ &= \underbrace{\left(\frac{\alpha}{\alpha+\beta} + \frac{\beta}{\alpha+\beta}R_i\kappa_{-i}\right)}_{\kappa_i} y + \underbrace{\left(\frac{\beta}{\alpha+\beta} - \frac{\beta}{\alpha+\beta}R_i\kappa_{-i}\right)}_{1-\kappa_i} x_i, \end{aligned} \tag{16}$$

where

$$R_i \equiv \frac{r_i\left(1-\frac{1}{n}\right)^2}{1-r_i+r_i\left(1-\frac{1}{n}\right)^2}. \tag{17}$$

A.2 Derivation of the subgame perfect equilibrium

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

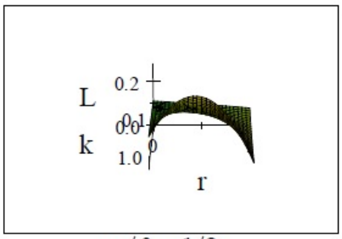
$$\begin{aligned} & \mathbb{E}\left(u\left(\mathbf{a}^*\left(\mathbf{r}\right),\theta;r_i\right)\right) \qquad (18) \\ = \quad & -\left(1-r_i\right)\mathbb{E}\left(a_i^*\left(r_i,\mathbf{r}_{-i}\right)-\theta\right)^2 - r_i\mathbb{E}\left(a_i^*\left(r_i,\mathbf{r}_{-i}\right)-\frac{1}{n}\sum_j a_j^*(r_j,\mathbf{r}_{-j})\right)^2 \\ = \quad & -\left(1-r_i\right)\mathbb{E}\left(\kappa_i^*\eta+\left(1-\kappa_i^*\right)\varepsilon_i\right)^2 - r_i\mathbb{E}\left(\begin{array}{c}\left(\kappa_i^*-\kappa^*\right)\eta+\left(1-1/n\right)\left(1-\kappa_i^*\right)\varepsilon_i \\ -\frac{1}{n}\sum_{j\neq i}\left(1-\kappa_j^*\right)\varepsilon_j\end{array}\right)^2 \\ = \quad & -\frac{1}{\alpha}\underbrace{\left[\kappa_i^{*2}+\frac{\alpha}{\beta}\left(1-\kappa_i^*\right)^2+r_i\left(\begin{array}{c}\kappa^*\left(\kappa^*-2\kappa_i^*\right) \\ +\frac{\alpha/\beta}{n}\left(\frac{1}{n}\sum_j\left(1-\kappa_j^*\right)^2-2\left(1-\kappa_i^*\right)^2\right)\end{array}\right)\right]}_{l\left(r_i,\kappa_i^*,\kappa_{-i}^*,n,\alpha/\beta\right)} \end{aligned}$$

By the envelope theorem, since κ_i^* is a second stage equilibrium value ($a_i^*\left(\mathbf{r}\right)=\kappa_i^*y+\left(1-\kappa_i^*\right)x_i$), we do not have to care about the impact on the first stage payoff of a variation in r_i through κ_i^*. Furthermore, we can replace the vector κ_{-i}^* by the mean value κ^* in order to reduce the arguments of the loss function which depend on r_i. It is clear that a higher dispersion of agents' decisions (a larger value of $\frac{1}{n}\sum_j\left(1-\kappa_j^*\right)^2$, given κ^*), by augmenting the coefficient of r_i in $l\left(r_i,\kappa_i^*,\kappa_{-i}^*,n,\alpha/\beta\right)$, can only discourage coordination (the choice of a high r_i). In order to show that agent i has still an incentive to choose a high r_i when confronted to a large dispersion of other agents' actions, we shall consider the worst case, that of the highest value of $\frac{1}{n-1}\sum_{j\neq i}\left(1-\kappa_j\right)^2$ compatible with a given value of the mean κ^* (hence of $\kappa_{-i}^*=\frac{n}{n-1}\left(\kappa^*-\frac{1}{n}\kappa_i^*\right)$). This case results from taking the κ_j's symetrically disposed at a maximum distance about the mean κ^*. Given the number n of agents and referring to the optimal value of κ_i^* (as given by (3)), such procedure allows to reduce the arguments of the re-defined loss function to the decision variable r_i plus only two parameters, the mean κ and the ratio of precisions α/β: $L\left(r_i,\kappa,\alpha/\beta\right)$. Finally, the admissible values of κ (those compatible with a second stage equilibrium) must belong to the interval $[\alpha/(\alpha+\beta), 1]$, since the concern for coordination (an increasing function of r_i) can only augment its fundamental value $\alpha/(\alpha+\beta)$.

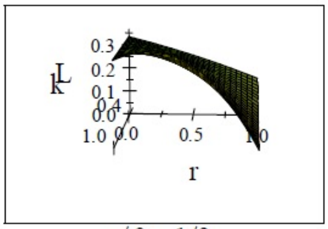
For $n = 6$, the number of participants in each session of our experiment, this procedure leads us to consider four κ_j's symetrically disposed, at a maximum distance, about the mean κ, plus a residual κ_j coinciding with the mean. Thus, $\frac{1}{n-1}\sum_{j\neq i}(1-\kappa_j)^2$ takes the value

$$\begin{aligned}\frac{2(1)^2 + 2(1-2\kappa)^2 + (1-\kappa)^2}{5} &= 1.8\kappa^2 - 2\kappa + 1 \text{ if } 0 \leq \kappa \leq 0.5 \text{ and} \\ \frac{2(0)^2 + 2(2-2\kappa)^2 + (1-\kappa)^2}{5} &= 1.8(1-\kappa)^2 \text{ if } 0.5 \leq \kappa \leq 1. \end{aligned} \tag{19}$$

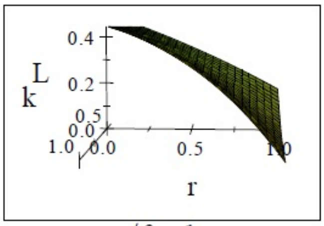
In order to determine the value of r_i which minimizes the loss function $L(\cdot, \kappa, \alpha/\beta)$, we thus perform simulations with this function for the parameter values $\alpha/\beta \in \{1/8, 1/2, 1, 2, 8\}$ used in our experiment. These simulations, represented in the following figure, show that the loss function is decreasing in r_i for most parameter values. Monotonicity is lost only for low relative precision of the public signal ($\alpha/\beta = 1/8$ and $\alpha/\beta = 1/2$) together with a large weight κ put on the public signal (because of a high concern for coordination). However, $L(\cdot, \kappa, \alpha/\beta)$ is then stricly concave, with $L(1, \kappa, \alpha/\beta) < L(0, \kappa, \alpha/\beta)$. We conclude that the loss function $L(\cdot, \kappa, \alpha/\beta)$ is always minimized at $r_i = 1$ in the cases relevant for our experiment.



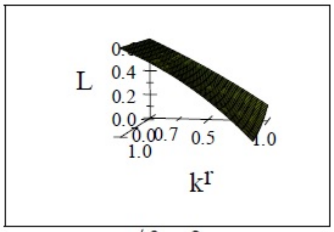
$\alpha/\beta = 1/8$



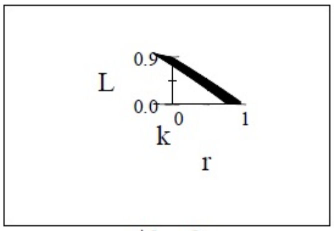
$\alpha/\beta = 1/2$



$\alpha/\beta = 1$



$\alpha/\beta = 2$



$\alpha/\beta = 8$

B Experiment

B.1 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:

$$400 - (10 - D_1)(D_2 - Z)^2 - D_1(D_2 - sc\ averageofdecisionsD_2sc\ inthegroup)^2$$

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:

  $$400 - 10(D_2 - sc\ averageofdecisionsD_2sc\ inthegroup)^2.$$

- 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 maximize 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.

<u>***Second game of the experiment:***</u>

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:

$$400 - (10 - D_1)(D_2 - Z)^2 - D_1(D_2 - sc\, average of decisions D_2 sc\, in the group)^2,$$

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:

$$200 - (E_2 - sc\, average of decisions D_2 sc\, in the group)^2.$$

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.

<u>***Third game of the experiment:***</u>

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:

$$400 - (10 - D_1)(D_2 - Z)^2 - D_1(D_2 - sc\, averageofdecisions D_2 sc\, inthegroup)^2,$$

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: $200 - (E_2 - sc\, averageofdecisions D_2 sc\, inthegroup)^2$. 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.

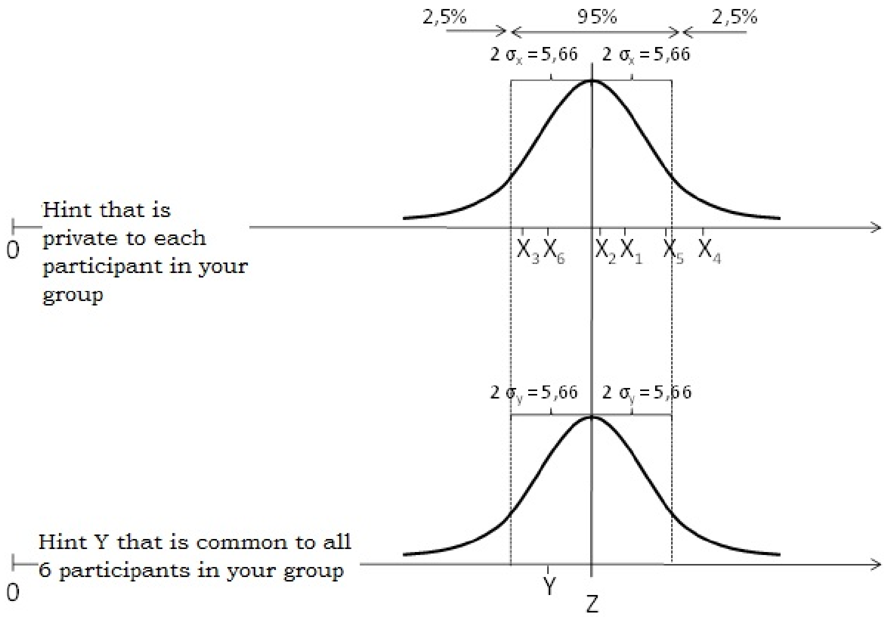
- **Private hint** X

  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.

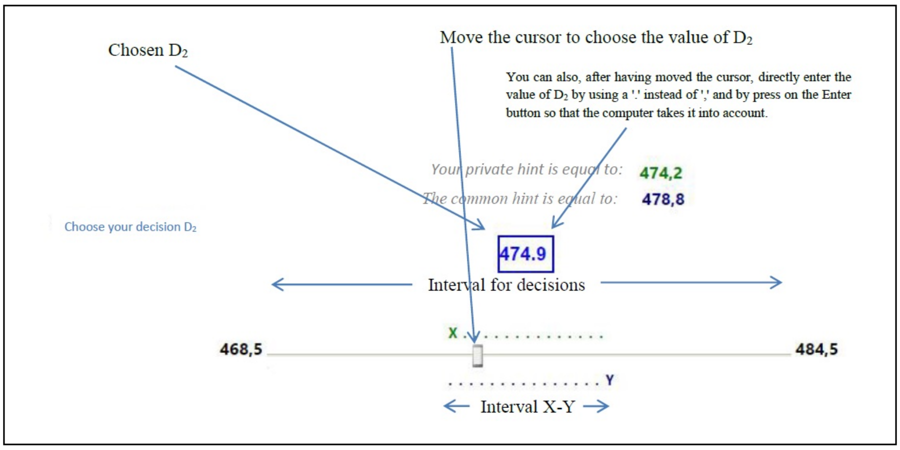
How to make your estimations E_1 and E_2?

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 maximize 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 maximize 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:

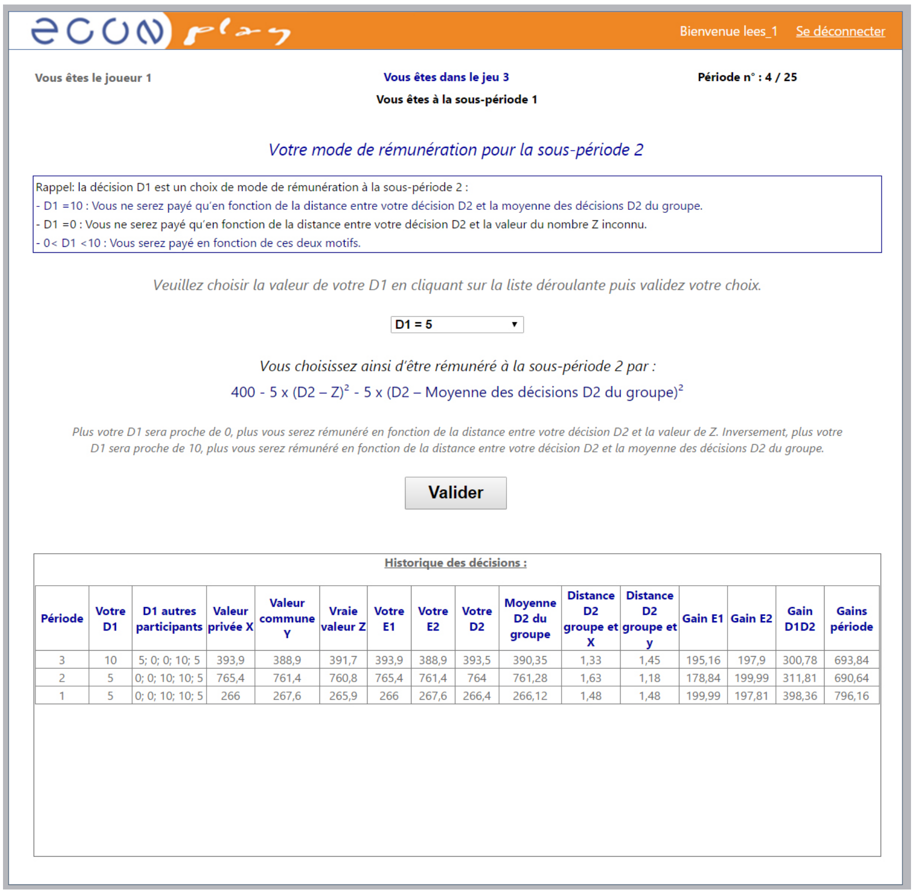


Five periods of this game will be randomly selected to be paid at the end of the experiment.

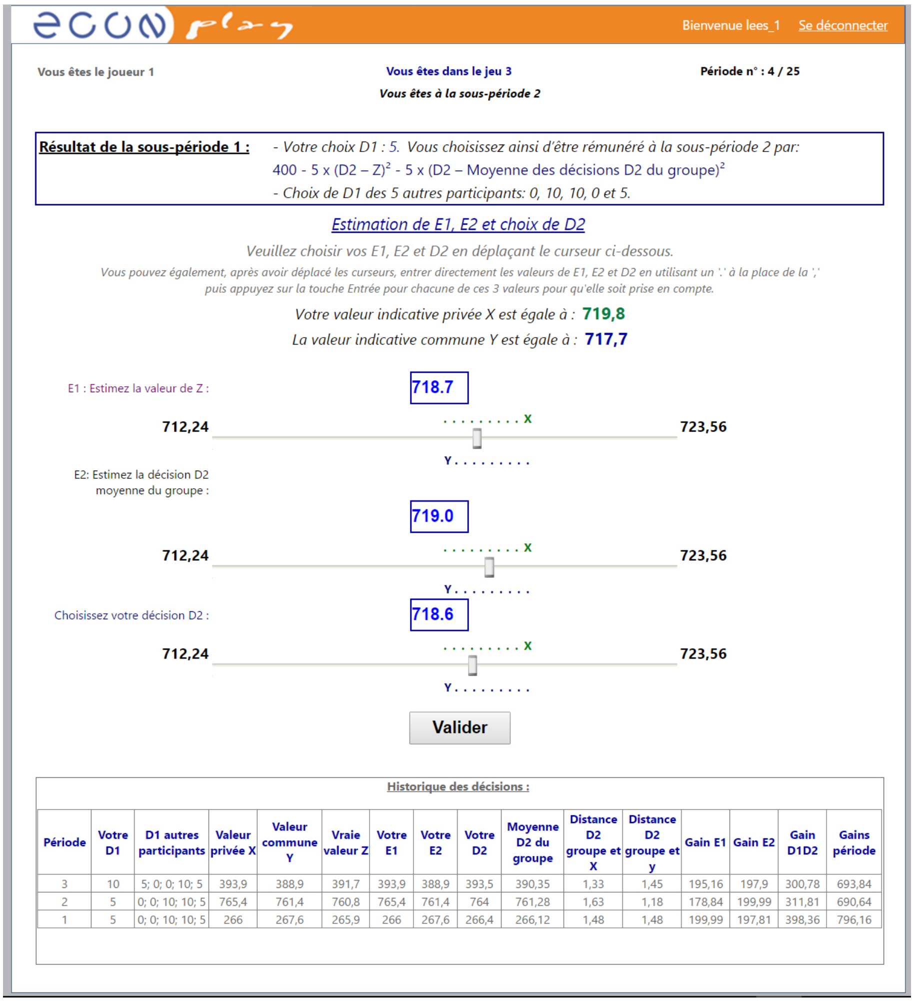
Payoffs: At the end of this third game, one of the participants to the experiment will be randomly selected and will loudly announce to the other participants the periods that will be selected for the payoffs of the three games. Your total payoff for the experiment will consist in the sum of the payoffs 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.

B.2 Example of screens



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



B.3 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 6, only 285 decisions over 6300 were outside this interval, which represents 4.5% of decisions.[24] 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.

Coherence between estimations and decisions As shown in Tables 7 and 9, 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).

[24] We accounted for the missing data by performing a bootstrap analysis in Appendix B.4.

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

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 7: Weight on Y in E_1

As shown by Tables 8 and 10, estimations E_2 are always below the extreme theoretical weight of 1 on the public signal,[25] but larger than the equal weight of 0.5.

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 8: Weight on Y in the estimation E_2

[25]In Treatment 6 where subjects received two private signals, however, estimations are in line with theory.

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 9: 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 10: Weight on Y in E_2 in game 3: comparison between observed weight on Y in E_2 and theoretical weight in $\mathbb{E}(\bar{D}_2)$, p-values for the Wilcoxon rank test

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

$$\left|\frac{D_{2it} - X_{it}}{Y_t - X_{it}}\right| = Co + \alpha \underbrace{\left|\frac{(10 - D_{1it})}{10} E_{1it} + \frac{D_{1it}}{10} E_{2it}\right|}_{OptD_2cond} + (\epsilon_{it} + \nu_i), \tag{20}$$

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

	All	Tr1	Tr2	Tr3	Tr4	Tr5	Tr6	Tr7
Const	0.5066***	0.3332***	0.1698***	0.3476***	0.3048***	0.7256***	0.3945***	0.2109***
	(0.0490)	(0.0106)	(0.0631)	(0.0443)	(0.0328)	(0.0492)	(0.0403)	(0.0540)
$OptD_2cond$	0.1488**	0.4470***	0.7750***	0.1636*	0.4679***	0.0052	0.1999**	0.6085***
	(0.0736)	(0.0270)	(0.0799)	(0.0855)	(0.0617)	(0.0285)	(0.0825)	(0.0933)
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 11: Random effects model - Equation (20)

Overall, Table 11 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.

B.4 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.

	Weight on Y in D_2	
	Eq. (12)	Eq. (20)
Baseline (TR1)	0.5653***	0.5460***
	(0.0239)	(0.0748)
TR2	0.1181**	0.1033*
	(0.0517)	(0.0542)
TR3	-0.1825***	-0.1834***
	(0.0601)	(0.0688)
TR4	-0.0442	-0.0425
	(0.0799)	(0.0737)
TR5	0.0968*	0.0856*
	(0.0518)	(0.0450)
TR6	-0.1048***	-0.1147***
	(0.0250)	(0.0271)
$\frac{D_{1it}}{10}$	0.1038***	
	(0.0192)	
$OptD_2cond$		0.1272
		(0.1086)
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 12: Random effects model - Comparisons to Treatment 1

The interpretation of Tables 12, 13, and 14 is the following. Consider for example the first column of Table 12. 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.[26] The coefficient for D_{1it} measures the effect of decision 1 on the dependent variable while all the other explanatory variables are constant.

[26]Note that the baseline (TR1) is the reference and is similar to the constant in the regression analysis of Tables 5 and 11, where all the other treatments (TR2, TR3, TR4, TR5, TR6) are the slopes for each treatment respectively.

	Weight on Y in D_2	
	Eq. (12)	Eq. (20)
Baseline (TR2)	0.6881***	0.3275***
	(0.0462)	(0.0492)
TR4	-0.1621*	-0.0753
	(0.0866)	(0.0509)
TR7	-0.1405**	-0.0846**
	(0.0592)	(0.0348)
$\frac{D_{1it}}{10}$	0.0947***	
	(0.0235)	
$OptD_2cond$		0.5583***
		(0.0561)
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 13: Random effects model - Comparisons to Treatment 2

	Weight on Y in D_2	
	Eq. (12)	Eq. (20)
Baseline (TR3)	0.3898***	0.3814***
	(0.0519)	(0.0706)
TR5	0.2870***	0.2855***
	(0.0748)	(0.0811)
TR7	0.1651**	0.1652**
	(0.0690)	(0.0770)
$\frac{D_{1it}}{10}$	0.0810**	
	(0.0324)	
$OptD_2cond$		0.0815
		(0.1291)
$AvD_{1it}others$		
0.0069**		
(0.0032)		
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 14: Random effects model - Comparisons to Treatment 3