The predominant role of signal precision in experimental beauty contest

Camille Cornand, Romain Baeriswyl

The predominant role of signal precision
in experimental beauty contest[*]

Romain Baeriswyl[†]

Swiss National Bank

Camille Cornand[‡]

CNRS-GATE

July 2014

Abstract

The weight assigned to public information in Keynesian beauty contest depends on the signal precision and on the degree of strategic complementarities. This experimental study shows that the response of subjects to changes in the signal precision and in the degree of strategic complementarities is qualitatively consistent with theoretical predictions, though quantitatively weaker. The weaker subjects' response to changes in the signal precision, however, mainly drives the weight observed in the experiment, making strategic complementarities and overreaction an issue of second order.

JEL classification: C92, D82, D84, E58.

Keywords: heterogeneous information, beauty contest, experiment, public information.

1   Introduction

In coordination games under heterogeneous information, public information plays a double role. While public information conveys information about economic fundamentals, it also conveys strategic information because it is common knowledge among all market participants. Fundamental uncertainty is driven by the signal precision, strategic uncertainty by the degree of strategic complementarities. The weight assigned to public information in equilibrium, thus, depends both on the signal precision and on the degree of strategic complementarities. As shown by Morris and Shin (2002), the coordination motive leads

---

[†]Swiss National Bank, Boersenstrasse 15, 8022 Zurich, Switzerland; email: Romain.Baeriswyl@snb.ch.

[‡]Université de Lyon, Lyon, F-69007, France ; CNRS, GATE Lyon Saint-Etienne, Ecully, F-69130, France; email: cornand@gate.cnrs.fr.

agents to overreact to public information in the sense that the equilibrium weight assigned to it is larger than what can be justified by its information about fundamentals.

Several studies have identified the occurrence of overreaction in laboratory experiments. For instance, the experimental analysis by Cornand and Heinemann (2014) measures subjects' overreaction to public information when varying the degree of strategic complementarities. However, while the role of the precision of public signal is as essential as strategic complementarities for the determination of equilibrium weight, it has hardly been studied. This paper fills this gap.

We run an experiment built on the beauty contest game of Morris and Shin (2002). In this game, agents have to choose actions that are as close as possible to a fundamental and as close as possible to the actions of others. To decide on their actions, agents receive a public and a private signal on the fundamental. The equilibrium weight assigned to public information increases with both its precision and the degree of strategic complementarities. We test predictions of this beauty contest game when varying the precision of public information and the degree of strategic complementarities.

In line with theory, the weight assigned to public information increases with its precision both in the first-order expectation of the fundamental and in the beauty contest action. However, compared to theoretical predictions, subjects underweight precise public signals and overweight imprecise public signals both in the first-order expectation of the fundamental and in the beauty contest action. In other words, subjects are less sensitive to signal precision than theory predicts, as already highlighted by Ackert et al. (2004).[1] For a given precision, the weight on the public signal in the first-order expectation of the fundamental is always lower than the weight in the beauty contest action, indicating that strategic complementarities induce subjects to overreact to public information. Compared to the weight on the public signal in the first-order expectation, the overreaction in the beauty contest is weaker than theoretically predicted, which is consistent with Nagel (1995) or Cornand and Heinemann (2014). In short, whereas the response of subjects to changes in the signal precision and in the degree of strategic complementarities is qualitatively consistent with theoretical predictions, it is quantitatively weaker.

Interestingly, the weaker subjects' response to changes in the signal precision makes strategic complementarities and overreaction an issue of second order. Because subjects underweight precise public information, the weight assigned to it both in the first-order expectation of the fundamental and in the beauty contest action is lower than the theoretical weight in the first-order expectation. Respectively, because subjects overweight imprecise public information, the weight assigned to it both in the first-order expectation and in the beauty contest action is higher than the theoretical weight in the beauty contest action. This suggests that the issue of overweighting/underweighting imprecise/precise information matters more than overreaction to common knowledge information.

This paper contributes to a growing experimental literature related to the effects of

---

[1] Ackert et al. (2004), however, analyse an experimental financial market with trading and do not account for overreaction issues due to strategic complementarities, which are typical in beauty contest games.

public vs. private information in games with strategic complementarities.[2] Cornand and Heinemann (2014) are the first to render account for overreaction to public signals. Baeriswyl and Cornand (2014) test the effectiveness of two communication strategies – partial publicity consisting in disclosing transparent information as a semi-public signal to a fraction of market participants only and partial transparency consisting in disclosing ambiguous public information to all market participants – to reduce market overreaction. Shapiro et al. (2014) show that the predictive power of the level-k reasoning approach is related to the strength of the coordination motive and the symmetry of information. None of these papers focuses on the role of information precision. To our knowledge, Dale and Morgan (2012) are the first to analyze the impact of signals' precision. They show that the quality of decisions increases with the addition of a lower quality private signal. However, subjects place inefficiently high weight on poorly accurate public signal, which is welfare damaging.[3] Our design is different as we always have public and private signals and only vary the precision of public information. This characteristic allows us to assess how subjects respond to pure changes in the precision of public information.

The remaining of the paper is structured as follows. Section 2 presents the theoretical framework and section 3 the experiment. Results are stated in section 4. Section 5 discusses the results in terms of policy implications and concludes.

2   The theoretical model

The spirit of the Keynesian beauty contest is characterized by strategic complementarities in agents' decision rule: each agent takes its decision not only according to its expectation of economic fundamentals but also according to its expectation of other agents' decision. The utility function for agent i is given by:

$$u_i(a_i, \theta) \equiv -(1-r)(a_i - \theta)^2 - r(a_i - a_{-i})^2, \tag{1}$$

where θ is the fundamental, a_i is the action taken by agent i, and a_{-i} is the average action taken by other agents $-i$, and r is a constant. Maximizing utility yields the optimal action of agent i:

$$a_i = (1-r)\mathbb{E}_i(\theta) + r\mathbb{E}_i(a_{-i}).$$

The parameter r is the weight assigned to the strategic component which drives the strength of the coordination motive in the decision rule. Assuming $0 \leq r \leq 1$ implies

---

[2]Among coordination games with heterogeneous information, the theory of global games has been experimentally tested by Cabrales et al. (2007) who show that the behavior of subjects converges to the theoretical prediction and explain this behavior by learning and risk dominance. Heinemann et al. (2004) and Heinemann et al. (2009) analyze the role of public and private information in the speculative attack game. They show that the global game equilibrium selection device is useful to predict behavior. Here, we focus on tests of the Morris and Shin (2002) model.

[3]While the aforementioned literature does not consider trading, there is some experimental evidence about the role of public and private signals on market efficiency in aggregating private information into prices (see e.g. Ackert et al. (2004), Alfarano et al. (2011) and Middeldorp and Rosenkranz (2011)).

that decisions are strategic complements: agents tend to align their decision with those of others.[4] Following the insight of Morris and Shin (2002) (henceforth MS), the optimal action in the Keynesian beauty contest is derived under imperfect, heterogenous, information.

2.1 Information structure

Each agent i receives a private signal x_i and a public signal y. These signals deviate from the fundamental θ by some error terms with uniform distribution. Whereas the private signal $x_i = \theta + \epsilon_i$ with $\epsilon_i \sim U[-\mu_\epsilon, +\mu_\epsilon]$ is different for each agent i, the public signal $y = \theta + \eta$ with $\eta \sim U[-\mu_\eta, +\mu_\eta]$ is the same for all agents and common knowledge among them. Noise terms ϵ_i of distinct agents and the noise η of the public signal are independent and their distribution is treated as exogenously given.

2.2 Equilibrium

To derive the perfect Bayesian equilibrium action of agents, we express the first-order expectation of agent i about the fundamental θ conditional on its private and public information. With error terms uniformly distributed, this corresponds to the middle of the intersection of the intervals $[x_i - \mu_\epsilon, x_i + \mu_\epsilon]$ and $[y - \mu_\eta, y + \mu_\eta]$, and yields

$$
\begin{aligned}
\mathbb{E}(\theta|x_i, y) &= \frac{\max\{x_i - \mu_\epsilon; y - \mu_\eta\} + \min\{x_i + \mu_\epsilon; y + \mu_\eta\}}{2} \\
&= (1 - f_i) \cdot x_i + f_i \cdot y.
\end{aligned}
\tag{2}
$$

The weight assigned to the public signal y in the first-order expectation, f_i, is dependent on the signal draw. The average first-order conditional expectation of agents about the fundamental θ is written as

$$
\bar{\mathbb{E}}(\theta) = (1 - f) \cdot \bar{x} + f \cdot y.
\tag{3}
$$

f represents the average weight assigned to y when each agent estimates θ according to (2). Values of f will be numerically calculated for a large number of draws.

We express the average optimal action as a linear combination of the average private signal and the public signal:

$$
\begin{aligned}
\bar{a} &= (1 - w)\bar{x} + wy = (1 - r)\bar{\mathbb{E}}(\theta) + r\bar{\mathbb{E}}(\bar{a}) \\
&= (1 - r)\bar{\mathbb{E}}(\theta) + r(1 - w)\bar{\mathbb{E}}(\theta) + rwy = (1 - rw)\bar{\mathbb{E}}(\theta) + rwy \\
&= (1 - rw)((1 - f) \cdot \bar{x} + f \cdot y) + rw \cdot y = \underbrace{\frac{(1 - r)(1 - f)}{1 - r(1 - f)}}_{1-w} \bar{x} + \underbrace{\frac{f}{1 - r(1 - f)}}_{w} y.
\end{aligned}
\tag{4}
$$

---

[4]Such an optimal decision rule could be derived from various economic contexts. For example, Amato et al. (2002), Hellwig and Veldkamp (2009), and Baeriswyl and Cornand (2010) interpret the beauty contest as the price-setting rule of monopolistically competitive firms; Angeletos and Pavan (2004) as the investment decision rule of competing firms.

The overreaction in the sense of MS is characterized by the fact that the weight assigned to the public signal y in the optimal action is larger than in the first-order expectation: $w = f/(1 - r(1 - f)) > f$. Relying on the average optimal action (4), the optimal action of agents i yields

$$
\begin{aligned}
a_i &= (1-r)\mathbb{E}(\theta|x_i, y) + r\mathbb{E}(\bar{a}|x_i, y) = ((1-r) + r(1-w))\mathbb{E}(\theta|x_i, y) + rw \cdot y \\
&= (1 - rw)\mathbb{E}(\theta|x_i, y) + rw \cdot y = \underbrace{(1 - rw)(1 - f_i)}_{1 - w_i} x_i + \underbrace{((1 - rw)f_i + rw)}_{w_i} y, \quad (5)
\end{aligned}
$$

where $\mathbb{E}(\theta|x_i, y)$ and f_i are given by (2). It can be shown that the average individual weight on y, $\bar{w}_i$, is equal to w in (4).

3   The experiment

To analyze the role of the precision of signals in the coordination game, we run an experiment with three treatments, each corresponding to a different degree of relative precision of the private and public signals.

3.1   Experimental procedure

We conducted 6 sessions with a total of 108 participants. Sessions were run at the RE-GATE lab in Lyon. Participants were mainly students from Lyon University, the engineering school Ecole Centrale Lyon and EM Lyon business school. In each session, the 18 participants were separated into three independent groups of 6 participants. Each participant could only participate in one session. Each session consisted of three stages, composed of 10 periods each (thus a total of 30 periods per session). Each stage corresponded to a different treatment. Participants played within the same group of participants during the whole length of the experiment and did not know the identity of the other participants of their group. Subjects were seated in random order at PCs. Instructions were then read aloud and questions answered in private. Throughout the sessions, participants were not allowed to communicate with one another and could not see each others' screens. Before starting the experiment, participants were required to answer a few questions to ascertain their understanding of the rules. The experiment started after all participants had given the correct answers to these questions. Examples of instructions, questionnaire and screens are given in the Appendix.

The program was written using z-Tree experimental software (Fischbacher (2007)).

3.2   Treatment parameters and theoretical predictions

In every period and for each group, a fundamental state θ is drawn randomly using a uniform distribution from the interval $[50, 950]$.[5] In every period of the experiment, each

---

[5]This interval was not told to participants.

subject has to make two decisions:

1. Each subject forms its best expectation e_i about the fundamental θ (decision 1). The payoff function in ECU (experimental currency units) for subject i related to his estimation is given by

$$u(e_i, \theta) = 200 - (e_i - \theta)^2.$$

2. Each subject decides on an action a_i in the beauty contest (decision 2). The payoff function in ECU for subject i is given by

$$u(a_i, a_{-i}, \theta) = 400 - 1.5(a_i - \theta)^2 - 8.5(a_i - a_{-i})^2,$$

in sessions 1 to 4 and by

$$u(a_i, a_{-i}, \theta) = 350 - (a_i - \theta)^2 - (a_i - a_{-i})^2,$$

in sessions 5 and 6, where a_{-i} is the average action of other subjects of the same group.

To make their decisions, subjects receive some signals on the fundamental θ and are forced to choose as decisions a weighted average of the signals they get.[6]

After each period, subjects were informed about the true state, their partner's decision and their payoff. Information about past periods from the same stage (including signals and own decisions) 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. 1000 ECU were converted to 2 euros.[7]

The parameters choice for the experiment is summarized in Table 1. Column tf shows the average optimal weight assigned to the public signal in decision 1 (the first-order expectation of the fundamental θ) and column tw shows the average optimal equilibrium weight in decision 2 (the beauty contest).

Each subject receives both a public and a private signal as described in Section 2.1. The private signal received by each subject is distributed as $x_i \sim U[\theta - 10, \theta + 10]$. The distribution of the public signal differs depending on the treatment. As reported in Table 1, the order of play is different in sessions 1, 2, 5, and 6 from that in sessions 3 and 4. In sessions 1, 2, 5, and 6, the common (public) signal is drawn from $y \sim U[\theta - 5, \theta + 5]$ in stage 2 and from $y \sim U[\theta - 20, \theta + 20]$ in stage 3. In sessions 3 and 4, the common (public) signal is drawn from $y \in [\theta - 20, \theta + 20]$ in stage 2 and from $y \sim U[\theta - 5, \theta + 5]$ in stage 3.

---

[6]Concretely, subjects click on values inside the interval defined by their signals to determine their chosen action. By doing so, we restrain subjects from choosing actions outside of their signals interval.

[7]In all stages, it was possible to earn negative points. Realized losses were of a size that could be counterbalanced by positive payoffs within a few periods. In total, no subject earned a negative payoff in any session.

Sessions	Groups	Players	Stage	Periods	r	μ_ϵ	μ_η	tf	tw
1-2	1-6	6	1	10	0.85	10	10	0.50	0.87
			2	10	0.85	10	5	0.91	0.98
			3	10	0.85	10	20	0.09	0.39
3-4	7-12	6	1	10	0.85	10	10	0.50	0.87
			2	10	0.85	10	20	0.09	0.39
			3	10	0.85	10	5	0.91	0.98
5-6	13-18	6	1	10	0.5	10	10	0.50	0.67
			2	10	0.5	10	5	0.91	0.95
			3	10	0.5	10	20	0.09	0.16

Table 1: Experiment parameters, average optimal weight on y in decisions 1 (tf) and 2 (tw)

4 The results

The observed average weight assigned in the experiment to the public signal in decision 1 (of_{obs}) is reported in Table 2, while the observed average weight assigned to the public signal in decision 2 (ow_{obs}) is reported in Table 3. tf_{cond} and tw_{cond} denote the theoretical weight conditional on the realization of signals in the experiment. tf_{uncond} and tw_{uncond} denote the theoretical weight unconditional on the realization of signals. Figure 1 compares the observed average weight assigned to the public signal in decisions 1 (of) and 2 (ow) to their optimal unconditional value for each treatment over 10 periods. Figure 2 compares the observed average weight assigned to the public signal in decision 1 (of) to the observed average weight assigned to the public signal in decision 2 (ow), and the observed average weight in decision 2 with $r = 0.85$ to that with $r = 0.5$.

To study the effect of the precision of public information, we analyze experimental data by looking at stated first-order expectations, overreaction effects, decision in the beauty contest, and change in the degree of strategic complementarities. Statistical tests are based on Mann-Whitney U-tests when comparing observed data to theoretical predictions and on Mann-Whitney two-samples statistic for between treatment tests. The fact that we cannot detect order effect[8] allows us to pool data from groups 1 to 12 together for the remaining analysis.[9]

4.1 Effect of information precision on the first-order expectation

We analyze the formation of the first-order expectation when beliefs are stated directly as in decision 1, the first-order expectation on the fundamental. We compare the observed weight (of) in each treatment to its theoretical prediction (tf_{cond} in Table 2) before analyzing the impact of a change in the precision of the public signal.

---

[8]The observed weight assigned to the public signal by groups 1-6 in decision 1 and decision 2 is not different from that assigned by groups 7-12 when $\mu_\eta = 10$ ($p = 0.6310$ and $p = 0.6741$), when $\mu_\eta = 5$ ($p = 0.2002$ and $p = 0.2489$) and when $\mu_\eta = 20$ ($p = 0.3367$ and $p = 0.1730$).

[9]As Figures 1 and 2 show, there do not seem to be any convergence effects (as is standard in laboratory experiments on games with strategic complementarities).
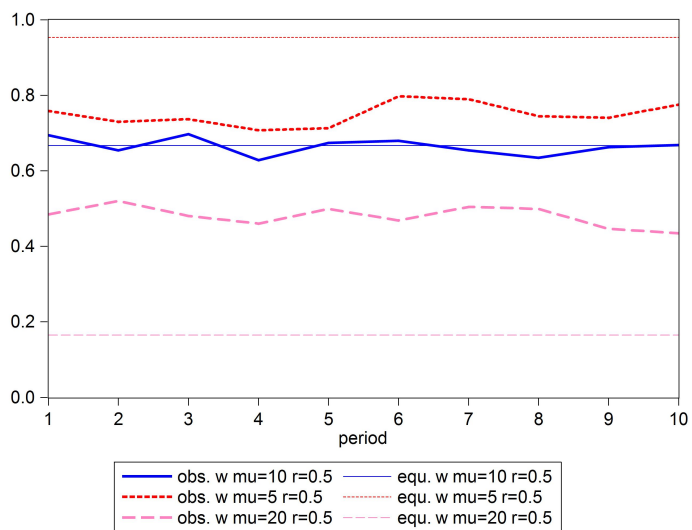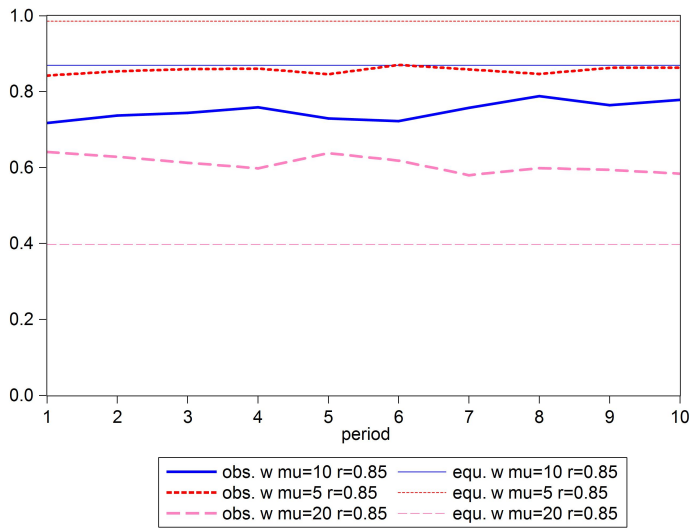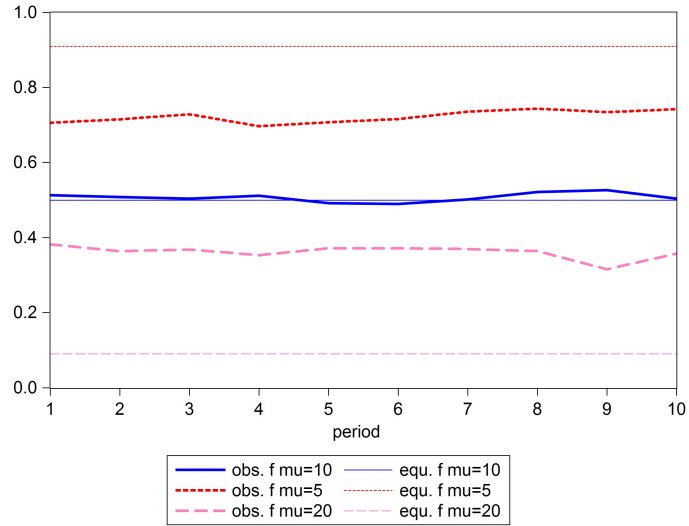
Figure 1: Average weight assigned to the public signal in decision 1 f (first-order expectation) and in decision 2 w (beauty contest)7
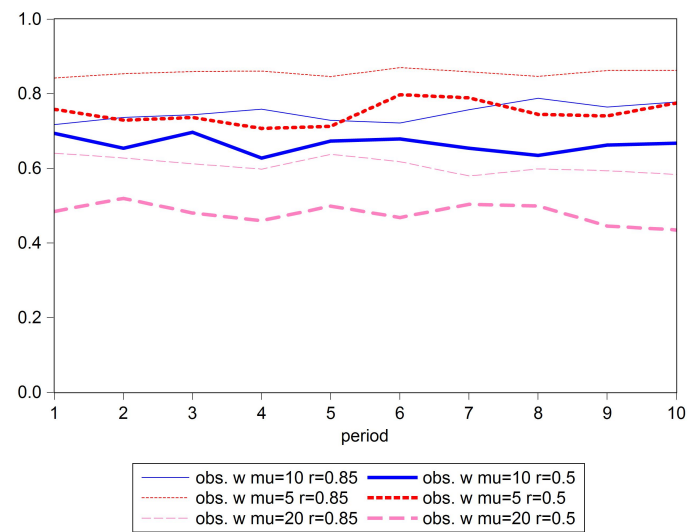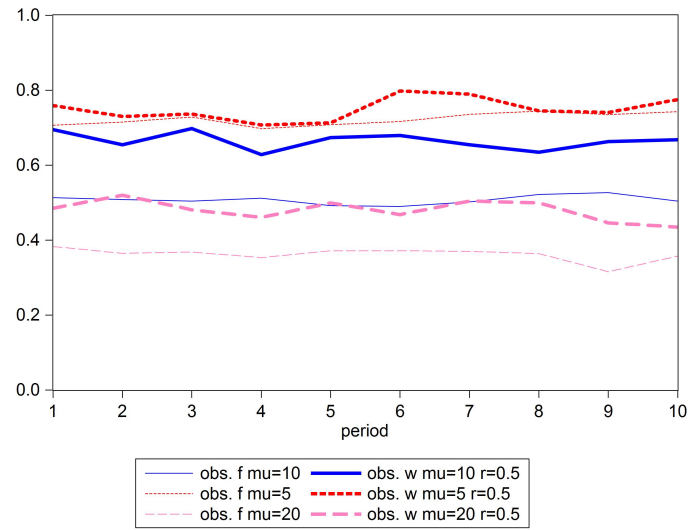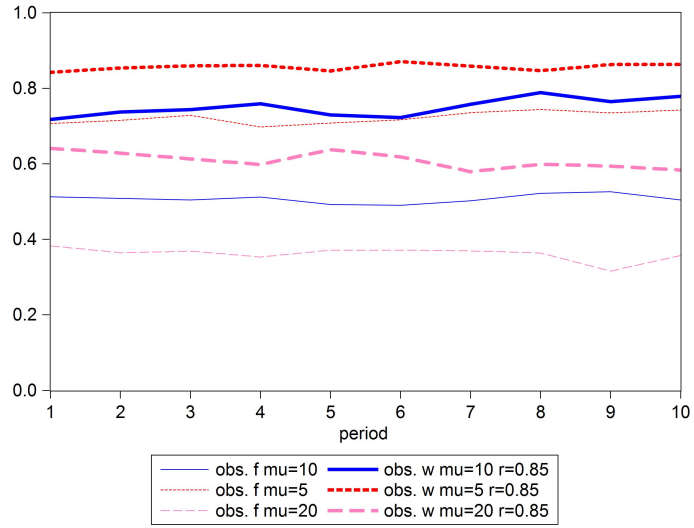
Figure 2: Average weight assigned to the public signal in decision 1 f (first-order expectation) and in decision 2 w (beauty contest)8

Sessions	Groups	$\mu_\eta = 10$		$\mu_\eta = 5$		$\mu_\eta = 20$	
		of_{obs}	tf_{cond}	of_{obs}	tf_{cond}	of_{obs}	tf_{cond}
1	1	.54	.50	.77	.90	.33	.12
1	2	.46	.50	.65	.89	.31	.09
1	3	.48	.50	.76	.89	.30	.11
2	4	.48	.50	.70	.90	.41	.07
2	5	.53	.50	.73	.89	.44	.07
2	6	.53	.50	.71	.87	.30	.07
3	7	.52	.50	.64	.89	.43	.09
3	8	.51	.50	.81	.90	.35	.18
3	9	.51	.50	.71	.90	.36	.08
4	10	.45	.50	.83	.90	.32	.12
4	11	.50	.50	.78	.90	.36	.08
4	12	.51	.50	.81	.89	.37	.14
5	13	.52	.50	.77	.91	.34	.11
5	14	.54	.50	.72	.90	.37	.08
5	15	.56	.50	.59	.91	.39	.12
6	16	.49	.50	.77	.89	.41	.11
6	17	.48	.50	.61	.91	.32	.08
6	18	.50	.50	.62	.88	.39	.10
Average	**1-18**	**.51**	**.50**	**.72**	**.90**	**.36**	**.10**
tf_{uncond}	13-18	.50		.91		.09	

Table 2: Average weight on y in the expectation of the fundamental (decision 1)

4.1.1 Observed behavior vs. theory

The weight of put by subjects on the public signal does not significantly differ from the theoretical prediction (0.5) when $\mu_\eta = 10$ ($p = 0.3125$ for groups 1-12 and $p = 0.6892$ for groups 13-18). By contrast, when the public signal and the private signal are not of equal precision, the observed weight significantly differs from theoretical predictions, although subjects tend to put a higher weight on the signal that is more precise and a lower weight on the signal that is less precise. In other words, the effect of the precision of the public signal is less pronounced in the first-order expectation than theoretically predicted. This is consistent with the findings of Ackert et al. (2004).

The observed weight assigned to the public signal of is significantly below its theoretical value in treatment $\mu_\eta = 5$ ($p = 0.0000$ for groups 1-12 and $p = 0.0051$ for groups 13-18) and significantly above its theoretical value in treatment $\mu_\eta = 20$ ($p = 0.0000$ for groups 1-12 and $p = 0.0051$ for groups 13-18). Nevertheless, the weight of put on the public signal increases with its precision: one can reject the equality of weights between treatments $\mu_\eta = 10$ and $\mu_\eta = 5$ ($p = 0.0000$ for groups 1-12 and $p = 0.0039$ for groups 13-18), between treatments $\mu_\eta = 10$ and $\mu_\eta = 20$ ($p = 0.0000$ for groups 1-12 and $p = 0.0039$ for groups 13-18), and between treatments $\mu_\eta = 5$ and $\mu_\eta = 20$ ($p = 0.0000$ for groups 1-12 and $p = 0.0039$ for groups 13-18).

Sessions	Groups	$\mu_\eta = 10$		$\mu_\eta = 5$		$\mu_\eta = 20$	
		ow_{obs}	tw_{cond}	ow_{obs}	tw_{cond}	ow_{obs}	tw_{cond}
1	1	.73	.87	.82	.98	.53	.42
1	2	.70	.87	.83	.98	.61	.40
1	3	.88	.87	.96	.98	.67	.41
2	4	.63	.87	.81	.98	.58	.38
2	5	.81	.87	.96	.98	.80	.38
2	6	.79	.87	.87	.98	.78	.38
3	7	.70	.87	.73	.98	.61	.40
3	8	.82	.87	.86	.98	.58	.46
3	9	.76	.87	.84	.98	.70	.39
4	10	.74	.87	.91	.98	.47	.42
4	11	.78	.87	.85	.98	.51	.39
4	12	.65	.87	.83	.98	.45	.43
Average	**1-12**	**.75**	**.87**	**.86**	**.98**	**.61**	**.41**
tw_{uncond}	1-12	.87		.99		.39	
5	13	.67	.67	.79	.95	.44	.18
5	14	.72	.67	.81	.95	.50	.15
5	15	.57	.67	.66	.95	.43	.19
6	16	.65	.67	.76	.94	.45	.18
6	17	.67	.67	.72	.95	.50	.16
6	18	.70	.67	.75	.94	.54	.17
Average	**13-18**	**.66**	**.67**	**.75**	**.95**	**.48**	**.17**
tw_{uncond}	13-18	.67		.95		.16	

Table 3: Average weight on y in the beauty contest (decision 2)

4.1.2 Asymmetric response

The theoretical weights assigned to the public signal in the treatments $\mu_\eta = 5$ (0.91) and $\mu_\eta = 20$ (0.09) are symmetrically spread around the weight in treatment $\mu_\eta = 10$ (0.5), as reported in Table 2. However, subjects do not symmetrically respond to an increase and to a decrease in the precision of the public signal in forming their expectation on the fundamental. The difference between treatments $\mu_\eta = 10$ and $\mu_\eta = 5$ is significantly larger in absolute value than the difference between treatments $\mu_\eta = 10$ and $\mu_\eta = 20$. The effect of less precise public signal is weaker than that of more precise public signal.

Table 4 (columns 2 and 3) computes the ratios between the difference of the observed weights in two treatments ($of_{\mu_\eta=10} - of_{\mu_\eta=5}$, for instance) and the difference of the theoretical weights in the same treatments ($tf_{\mu_\eta=10} - tf_{\mu_\eta=5}$). A ratio lower than 1 indicates that the effect of increasing or decreasing the precision of the public signal is weaker than predicted by theory. The ratio for the treatments $\mu_\eta = 5$ vs. $\mu_\eta = 10$ is significantly larger than for the treatments $\mu_\eta = 20$ vs. $\mu_\eta = 10$ ($p = 0.6312$), meaning that subjects underweight less the public signal when it is more precise than they overweight it when it is less precise than the private signal.

Gr.	$\dfrac{of_{(10)}-of_{(5)}}{tf_{(10)}-tf_{(5)}}$	$\dfrac{of_{(10)}-of_{(20)}}{tf_{(10)}-tf_{(20)}}$	$\dfrac{ow_{(10)}-ow_{(5)}}{tw_{(10)}-tw_{(5)}}$	$\dfrac{ow_{(10)}-ow_{(20)}}{tw_{(10)}-tw_{(20)}}$	$\dfrac{ow_{(10)}-ow_{(5)}}{tw|of_{(10)}-tw|of_{(5)}}$	$\dfrac{ow_{(10)}-ow_{(20)}}{tw|of_{(10)}-tw|of_{(20)}}$
1	0.58	0.55	0.80	0.45	1.26	1.66
2	0.49	0.37	1.10	0.19	1.67	0.87
3	0.71	0.47	0.73	0.44	0.87	1.65
4	0.53	0.17	1.59	0.10	2.37	1.22
5	0.52	0.21	1.33	0.02	2.28	0.19
6	0.47	0.54	0.70	0.03	1.32	0.11
7	0.30	0.22	0.30	0.19	0.78	2.03
8	0.73	0.50	0.32	0.57	0.41	2.58
9	0.48	0.37	0.68	0.13	1.19	0.75
10	0.95	0.36	1.50	0.59	1.38	2.88
11	0.70	0.36	0.69	0.57	0.88	3.16
12	0.76	0.38	1.54	0.45	1.90	2.64
13	0.62	0.44	0.40	0.47	0.61	1.36
14	0.46	0.40	0.32	0.43	0.65	1.35
15	0.08	0.43	0.30	0.30	3.14	0.95
16	0.73	0.21	0.39	0.41	0.50	2.55
17	0.33	0.38	0.19	0.32	0.47	1.01
18	0.32	0.27	0.20	0.31	0.54	1.48
Av.	**0.54**	**0.37**	**0.73**	**0.33**	**1.24**	**1.58**

Table 4: Differences between observed weight for treatments $\mu_\eta = 10$ and $\mu_\eta = 5$ and for treatments $\mu_\eta = 10$ and $\mu_\eta = 20$ normalized by their respective theoretical values tf and tw

4.1.3 Payoff incentives

Can payoff incentives be responsible for the fact that the difference between the observed weight of and the theoretical weight tf is larger in treatment $\mu_\eta = 20$ than in treatment $\mu_\eta = 5$? The answer is no. Figure 3 represents the expected payoff as a function of deviation from the optimal weight conditional on signals. It shows that deviating from the optimal weight is more costly when $\mu_\eta = 20$ than when $\mu_\eta = 5$. According to payoff incentives, one would expect subjects to deviate less from optimal weight with a less precise signal than with a more precise one.

An alternative explanation for the asymmetric response may be associated with the stronger level of uncertainty that prevails when $\mu_\eta = 20$ compared to $\mu_\eta = 5$. Under stronger uncertainty, it seems that subjects prefer to avoid playing extreme values. The relative frequency of weights depicted on Figure 4 shows that subjects more often play the middle of the signals' interval when $\mu_\eta = 20$ than when $\mu_\eta = 5$. While uncertainty should provide incentives for subjects to play extreme values, it rather seems to prevent them from doing so.

Result 1 - When subjects form expectation on some fundamental value, they put more weight on more precise signals. The effect of precision is, however, less pronounced than theoretically predicted. Moreover, subjects underweight
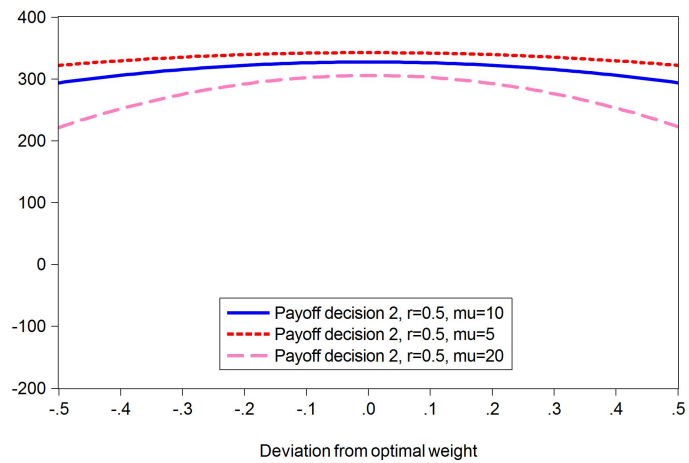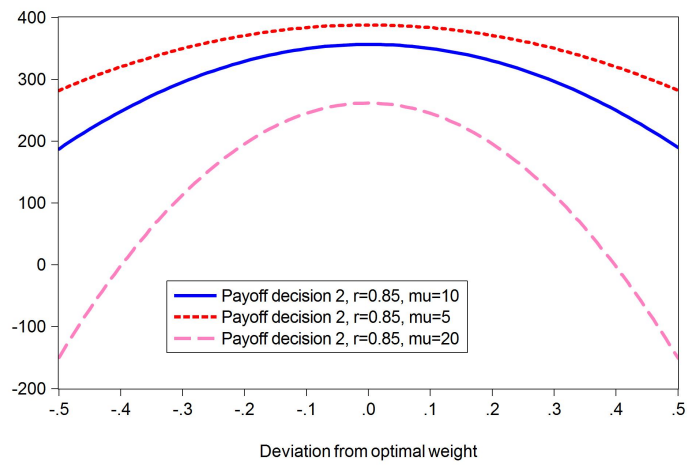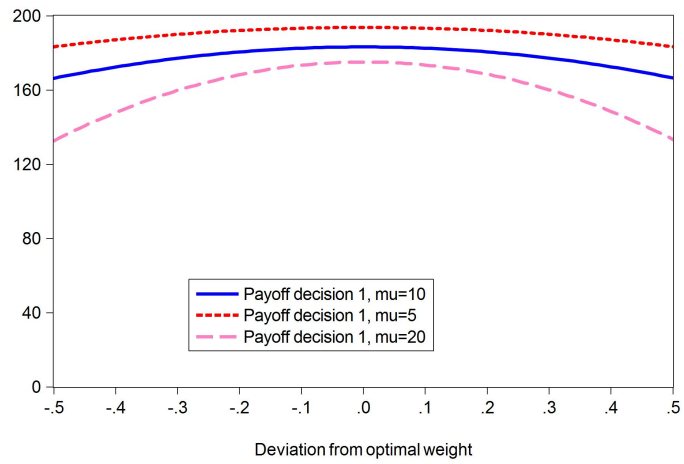
Figure 3: Expected payoff as function of deviation from optimal weight

precise information less than they overweight imprecise information.
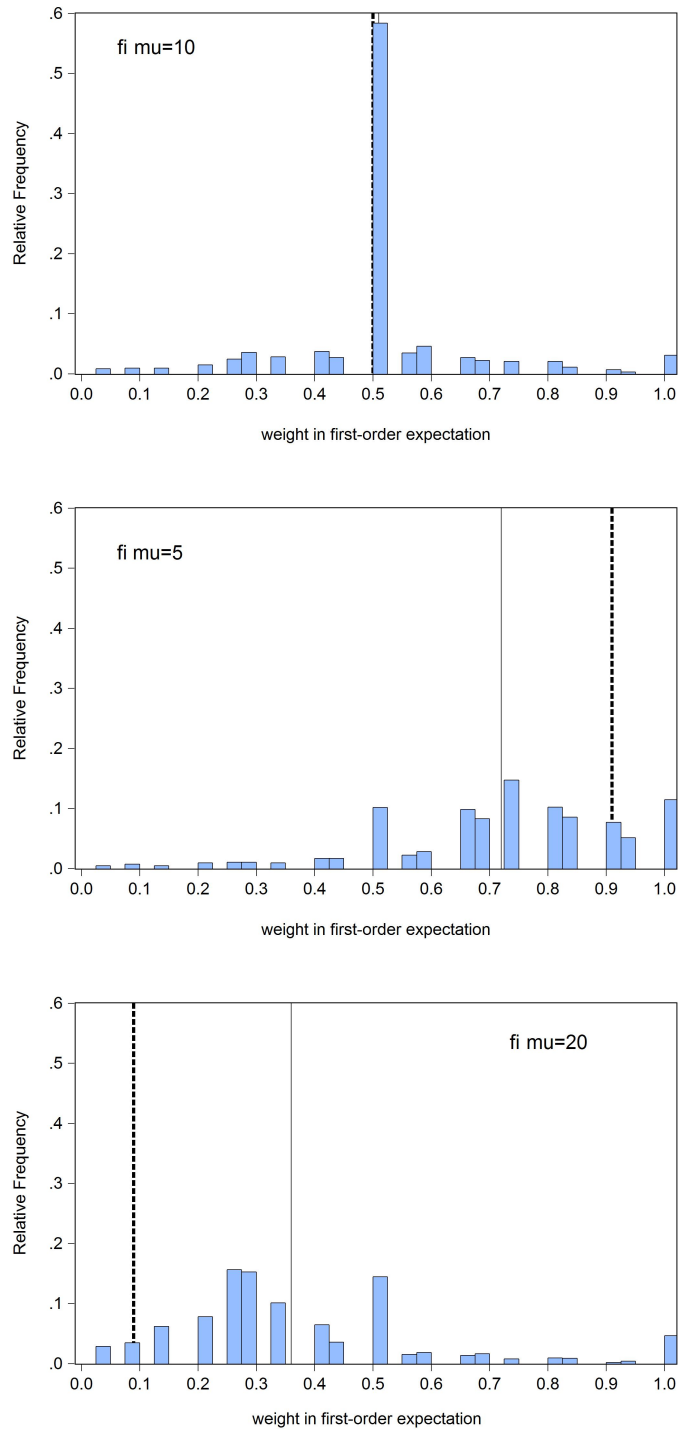

Figure 4: Relative frequency of weight assigned to the public signal in decision 1 (first-order expectation); dashed line: uncond. optimal weight; solid line: observed average

4.2 Overreaction

Following the literature in the vein of Morris and Shin (2002), overreaction consists in the fact that the equilibrium weight assigned to the public signal in the beauty contest (w) is larger than in the first-order expectation of the fundamental (f). In an experiment, overreaction can be assessed either against the theoretical weight or against the observed weight assigned to the public signal in the first-order expectation. We first discuss overreaction in ow against the theoretical weight tw and then against the observed weight of.

4.2.1 Overreaction to theoretical weight in the first-order expectation

The observed weight assigned to the public signal in the beauty contest is significantly higher than the theoretical weight in the first-order expectation of the fundamental in treatments $\mu_\eta = 10$ ($p = 0.0000$ for groups 1-12, $p = 0.0051$ for groups 13-18) and $\mu_\eta = 20$ ($p = 0.0000$ for groups 1-12, $p = 0.0051$ for groups 13-18). Subjects clearly overreact in treatments $\mu_\eta = 10$ and $\mu_\eta = 20$. By contrast, they underreact in treatment $\mu_\eta = 5$, in the sense that the weight on y in the beauty contest is systematically below the theoretical weight in the first-order expectation of the fundamental ($p = 0.0434$ for groups 1-12, $p = 0.0051$ for groups 13-18). In short, subjects overreact when the public signal is rather imprecise, while they underreact to it when it is rather precise.

When overreaction is observed, it is weaker than equilibrium theory predicts – as already observed in Baeriswyl and Cornand (2014) and Cornand and Heinemann (2014) – except in treatment $\mu_\eta = 10$ for groups 13-18 where equality cannot be rejected.[10] Overreaction can be assessed in terms of limited levels of reasoning. Starting from the definition of level-1, actions for higher levels of reasoning can be calculated as follows. Suppose that the players $-i$ (all players except i) attach weight ρ_k to the public signal. The best response of player i to such behaviour is

$$
\begin{aligned}
a_i^{k+1} &= (1 - r\rho_k)\mathbb{E}_i(\theta) + r\rho_k y \\
&= (1 - r\rho_k)[(1 - f_i)x_i + f_i y] + r\rho_k y \\
&= x_i[(1 - r\rho_k)(1 - f_i)] + y\underbrace{[(1 - r\rho_k)f_i + r\rho_k]}_{\rho_{k+1}}.
\end{aligned}
\tag{6}
$$

Hence the weight on the public signal for the next level of reasoning is

$$
\rho_{k+1} = f_i + \rho_k r(1 - f_i).
\tag{7}
$$

While level-1 reasoning corresponds to the first-order expectation of the fundamental (tf), the infinite level of reasoning corresponds to the equilibrium action in the beauty contest (tw). Table 5 presents the theoretical weights for limited levels of reasoning when level-1

---

[10]$\mu_\eta = 10$: $p = 0.0006$ for groups 1-12, $p = 0.9362$ for groups 13-18; $\mu_\eta = 5$: $p = 0.0000$ for groups 1-12, $p = 0.0051$ for groups 13-18; $\mu_\eta = 20$: $p = 0.0000$ for groups 1-12, $p = 0.0051$ for groups 13-18.

Treatment Groups	$\mu_\eta = 5$		$\mu_\eta = 10$		$\mu_\eta = 20$	
	1-12	13-18	1-12	13-18	1-12	13-18
Theoretical level-1	.91	.91	.50	.50	.09	.09
	(.0434)	(.0051)	(.0000)	(.0051)	(.0000)	(.0051)
	rejected	rejected	rejected	rejected	rejected	rejected
Level-2	.98	.95	.71	.62	.16	.13
(p-value)	(.0000)	(.0051)	(.1387)	(.0403)	(.0000)	(.0051)
	rejected	rejected	**accepted**	rejected	rejected	rejected
Level-3	.98	.95	.80	.66	.21	.15
(p-value)	(.0000)	(.0051)	(.0263)	(.3051)	(.0000)	(.0051)
	rejected	rejected	rejected	**accepted**	rejected	rejected
Level-4	.98	.95	.84	.66	.25	.16
(p-value)	(.0000)	(.0051)	(.0002)	(.3051)	(.0000)	(.0051)
	rejected	rejected	rejected	**accepted**	rejected	rejected
Level-5	.98	.95	.86	.67	.29	.16
(p-value)	(.0000)	(.0051)	(.0002)	(.3051)	(.0000)	(.0051)
	rejected	rejected	rejected	**accepted**	rejected	rejected
Level-6	.98	.95	.86	.67	.31	.16
(p-value)	(.0000)	(.0051)	(.0002)	(.3051)	(.0000)	(.0051)
	rejected	rejected	rejected	**accepted**	rejected	rejected
Level-∞	.98	.95	.87	.67	.39	.16
(p-value)	(.0000)	(.0051)	(.0002)	(.3051)	(.0000)	(.0051)
	rejected	rejected	rejected	**accepted**	rejected	rejected
Obs. weight	.8562	.7489	.7497	.6646	.6090	.4794

Table 5: Theoretical values of weights put on y for different levels of reasoning and starting with level-1 as the theoretical weight on y in the first-order expectation of the fundamental

is the theoretical weight in the first-order expectation.

In treatment $\mu_\eta = 10$, the observed weight corresponds to level-2 reasoning when $r = 0.85$ and to level-∞ when $r = 0.5$. The level of reasoning observed when $r = 0.85$ is consistent with the result of Nagel (1995), Baeriswyl and Cornand (2014) and Cornand and Heinemann (2014). In treatment $\mu_\eta = 5$, the observed weight does not correspond to any level of reasoning because it is lower than the theoretical weight in level-1 reasoning. In treatment $\mu_\eta = 20$, the observed weight does not correspond to any level of reasoning because it is higher than the theoretical weight in level-∞ reasoning.

4.2.2 Overreaction to observed weight in the first-order expectation

Compared to the observed weight in the first-order expectation, however, there is overreaction for each level of precision: the observed weight ow is always larger than the observed weight of.[11]

Table 6 presents the values for limited level of reasoning when level-1 is defined as

---

[11]In treatment $\mu_\eta = 10$: $p = 0.0000$ for groups 1-12, $p = 0.0051$ for groups 13-18; in treatment $\mu_\eta = 5$: $p = 0.0006$ for groups 1-12, $p = 0.0658$ for groups 13-18; $\mu_\eta = 20$: $p = 0.0000$ for groups 1-12, $p = 0.0051$ for groups 13-18.

| Treatment | $\mu_\eta = 5$ | | $\mu_\eta = 10$ | | $\mu_\eta = 20$ | |
Groups	1-12	13-18	1-12	13-18	1-12	13-18
Observed	.74	.68	.50	.51	.36	.37
level-1	(.0006)	(.0658)	(.0000)	(.0051)	(.0000)	(.0051)
	rejected	**accepted**	rejected	rejected	rejected	rejected
Level-2	.90	.79	.71	.63	.56	.49
(p-value)	(.0404)	(.1738)	(.1738)	(.0658)	(.1738)	(.9362)
	rejected	**accepted**	**accepted**	**accepted**	**accepted**	**accepted**
Level-3	.94	.81	.80	.66	.66	.52
(p-value)	(.0061)	(.0203)	(.0404)	(.3789)	(.1738)	(.0658)
	rejected	rejected	rejected	**accepted**	**accepted**	**accepted**
Level-4	.95	.81	.84	.67	.72	.53
(p-value)	(.0061)	(.0203)	(.0006)	(.9362)	(.0061)	(.0658)
	rejected	rejected	rejected	**accepted**	rejected	**accepted**
Level-5	.95	.81	.86	.67	.75	.54
(p-value)	(.0061)	(.0203)	(.0006)	(.9362)	(.0061)	(.0203)
	rejected	rejected	rejected	**accepted**	rejected	rejected
Level-6	.95	.81	.86	.67	.77	.54
(p-value)	(.0061)	(.0203)	(.0006)	(.9362)	(.0061)	(.0203)
	rejected	rejected	rejected	**accepted**	rejected	rejected
Level-∞	.95	.81	.87	.67	.79	.54
(p-value)	(.0061)	(.0203)	(.0006)	(.9362)	(.0006)	(.0203)
	rejected	rejected	rejected	**accepted**	rejected	rejected
Obs. weight	.86	.75	.75	.66	.61	.48

Table 6: Theoretical values of weights put on y for different levels of reasoning and starting with level-1 as the observed weight on y in decision 1

the observed weight of rather than the theoretical weight tf. The overreaction is not as strong as predicted by the infinite level of reasoning, when the observed weight in the first-order expectation is taken as level-1 reasoning, except for $\mu_\eta = 10$ groups 13-18 where equality cannot be rejected (see also Figure 1). Subjects generally reach a level of reasoning between 1 and 4, depending on the treatment.

Result 2 - Compared to the weight observed in the first-order expectation, subjects overreact to public information in the beauty contest decision. The overreaction is however weaker than theoretically predicted. Compared to theory, there is no systematic overreaction because the overreaction relative to the observed first-order expectation may be dominated by the fact that subjects underweight precise information.

4.3 Effect of information precision on the beauty contest

We compare the observed weight in the beauty contest (ow) in each treatment to its theoretical prediction (tw) before analyzing the impact of a change in the precision of the public signal.

4.3.1 Observed behavior vs. theory

As in the first-order expectation, the precision of the public signal has a clear effect on the weight assigned to it in the beauty contest. The weight assigned to the public signal increases with its precision. The observed weight assigned to the public signal by groups 1-12 (respectively 13-18) in treatment $\mu_\eta = 10$ is significantly below the observed weight in treatment $\mu_\eta = 5$ ($p = 0.0015$, respectively $p = 0.0304$) and significantly above the observed weight in treatment $\mu_\eta = 20$ ($p = 0.0039$, respectively $p = 0.0039$).[12]

4.3.2 Asymmetric response

As in the first-order expectation, subjects asymmetrically respond to an increase in the precision of the public signal and a decrease in the precision of the public signal in their beauty contest decision (ow). This can be shown owing to the ratio between the difference of the observed weights in two treatments ($ow_{\mu_\eta=10} - ow_{\mu_\eta=5}$, for instance) and the difference of the theoretical weights in the same treatments ($tw_{\mu_\eta=10} - tw_{\mu_\eta=5}$), as shown on Table 4 (columns 4 and 5). A ratio lower than 1 indicates that the effect of increasing or decreasing the precision of the public signal is weaker than predicted by theory.

The ratio for the treatments $\mu_\eta = 10$ vs. $\mu_\eta = 5$ is significantly larger than for the treatments $\mu_\eta = 10$ vs. $\mu_\eta = 20$ ($p = 0.0164$), indicating that subjects less underweight the public signal when it is more precise than they overweight it when it is less precise than the private signal.

However, when we account for the asymmetry observed in the first-order expectation (as stated in Result 1), there is no asymmetry in the way the precision influences the weight in the beauty contest ow. This is shown by computing the ratio between the difference of the observed weights in two treatments ($ow_{\mu_\eta=10} - ow_{\mu_\eta=5}$, for instance) and the difference of the theoretical weight conditional on the observed first-order expectation ($tw|of_{\mu=10} - tw|of_{\mu=5}$), as done in Table 4 (columns 6 and 7). This corresponds exactly to what we observe in the analysis of limited level of reasoning.

Result 3 - Once accounting for the asymmetry in the observed weight put on the public signal in the first-order expectation, there is no asymmetric response in the weight put on the public signal in the beauty contest decision.

4.3.3 Payoff incentives

As for the analysis of the stated first-order expectations in section 4.1, payoff incentives as depicted in Figure 3 cannot rationalize the asymmetric response in the observed weight put on the public signal in treatment $\mu_\eta = 20$ vs. $\mu_\eta = 5$. However, as shown on Figures 5 and 6, the coordination motive in the beauty contest leads them to play focal points more

---

[12]We also check that the observed weight assigned to the public signal by groups 1-12 (respectively 13-18) in treatment $\mu_\eta = 5$ is significantly different from that in treatment $\mu_\eta = 20$ ($p = 0.0001$, respectively $p = 0.0039$).
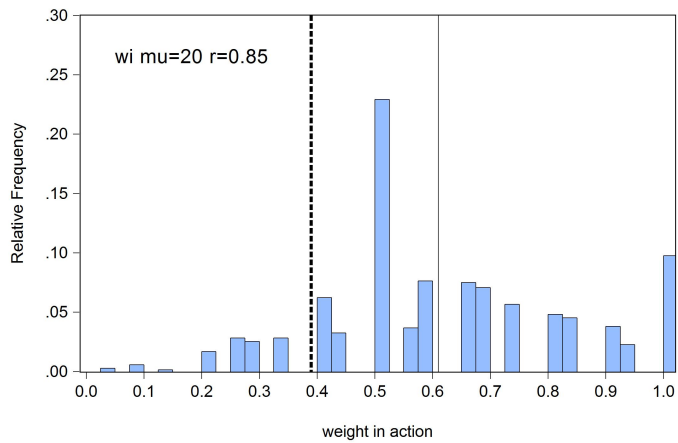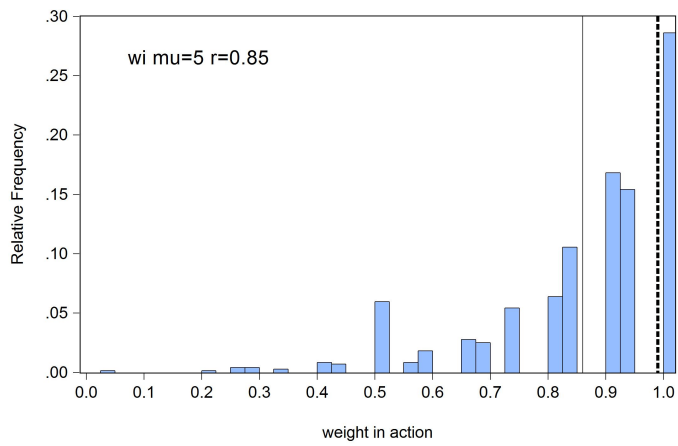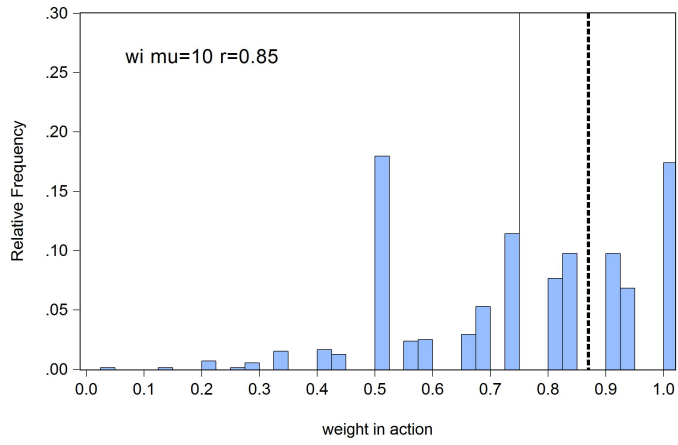
Figure 5: Relative frequency of weight assigned to the public signal in decision 2 (the action); dashed line: uncond. optimal weight; solid line: observed average
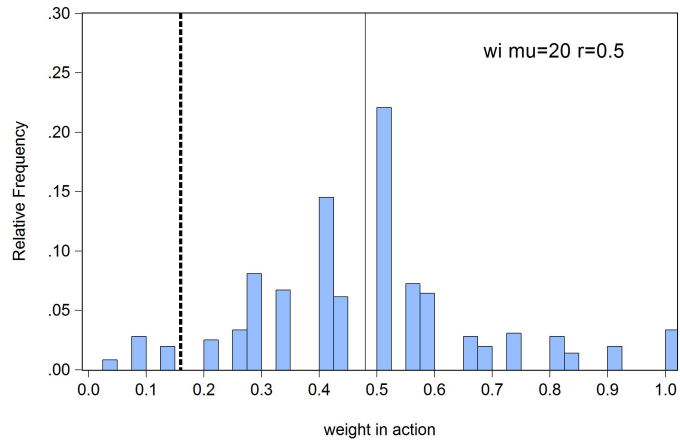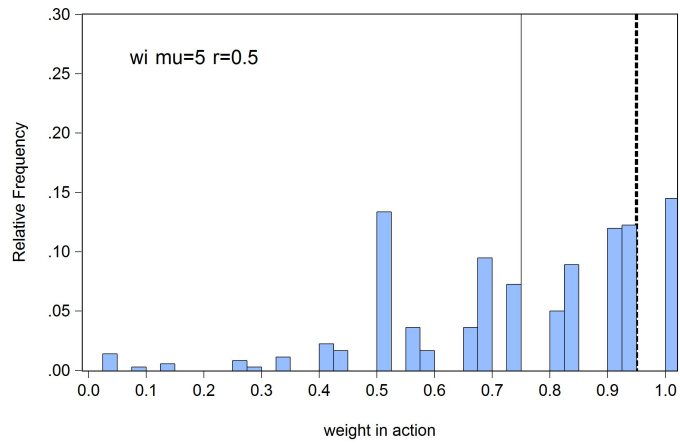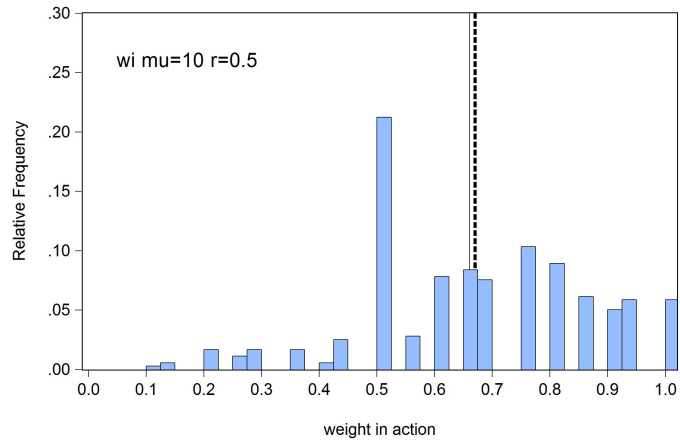
Figure 6: Relative frequency of weight assigned to the public signal in decision 2 (the action); dashed line: uncond. optimal weight; solid line: observed average

often. While Figure 4 exhibits no focal point for decision 1 (except in $\mu_\eta = 10$, where it coincides with the equilibrium value), we can identify clear focal points on Figures 5 and 6. In treatment $\mu_\eta = 5$, 1 represents a focal point for groups 1-12, while 1 and 0.5 are focal for groups 13-18. In treatment $\mu_\eta = 20$, 0.5 is a focal point. In treatment $\mu_\eta = 10$, both 1 and 0.5 are focal for groups 1-12, while only 0.5 is focal for groups 13-18. The combination of uncertainty and strategic complementarity (so strategic uncertainty) seems to induce subjects to play focal points.

4.4 Change in the degree of strategic complementarities

In line with theory and with the findings of Cornand and Heinemann (2014), the reaction to public information is reinforced with a stronger degree of strategic complementarities. As can been seen on Figure 2 (panel 3), a change in r has a significant effect in the beauty contest: the observed weight assigned to the public signal in periods 1-10 for groups 1-12 is significantly higher than that observed for groups 13-18 for treatments $\mu_\eta = 5$, $\mu_\eta = 10$ and $\mu_\eta = 20$ ($p = 0.0002$ in all three cases).[13] The comparison between Figures 5 and 6 shows that as the degree of strategic complementarities r increases, subjects more often play focal points.

Result 4 - In line with theory, as the degree of strategic complementarities increases, the weight put on the public signal in the beauty contest decision increases.

5 Discussion and conclusion

Whereas the market response to public disclosure depends on both the signal precision and the degree of strategic complementarities, the literature has focused on the role of strategic complementarities, assuming economic agents to correctly take the signal precision into account.

Theoretical analysis shows that strategic complementarities induce agents to overreact to public information, calling into question the desirability of disclosing public information, especially when it is not very accurate. Experimental studies have confirmed the theoretical prediction, although subjects typically overreact to public information to a smaller extent.

Focusing on fundamental uncertainty, our experiment shows that the response to public information is mainly driven by the signal precision. This indicates that the response may deviate from social optimum not solely because of the overreaction due to strategic complementarities, but also because of the overweighting/underweighting of imprecise/precise information.

Both effects have to be accounted for in combination. When public information is precise, overreaction can be beneficial as it may help reducing the underweighting associated

---

[13]Tests are conducted across periods instead of groups because the number of groups is different between sessions with $r = 0.85$ and $r = 0.5$.

with the error in stating the first-order expectation. However, when public information is poorly accurate, overreaction can be detrimental as it may exacerbate the overweighting already made in the stated first-order expectation.

While a welfare analysis is beyond the scope of this paper, the fact that subjects imperfectly take into account the signal precision could lead to reconsider the policy prescription derived from coordination games with heterogeneous information. Reducing the precision of public signals in order to mitigate overreaction due to strategic complementarities may not help aligning agents' response on the social optimum as they may overweight poorly accurate signals. By contrast, when public information is inaccurate, it may be better not to disclose it at all since agents overweight inaccurate signals, even in the absence of strategic complementarities.

References

Ackert, L., Church, B., and Gillette, A. (2004). Immediate disclosure or secrecy? the release of information in experimental asset markets. *Financial Markets, Institutions and Instruments*, 13(5):219–243.

Alfarano, S., Morone, A., and Camacho, E. (2011). The role of public and private information in a laboratory financial market. *Working Papers Serie AD 2011-06, Instituto Valenciano de Investigaciones Econmicas, S.A. (Ivie)*.

Amato, J., Morris, S., and Shin, H. S. (2002). Communication and monetary policy. *Oxford Review of Economic Policy*, 18(4):495–503.

Angeletos, G.-M. and Pavan, A. (2004). Transparency of Information and Coordination in Economies with Investment Complementarities. *American Economic Review (Papers and Proceedings)*, 94(2):91–98.

Baeriswyl, R. and Cornand, C. (2010). The signaling role of policy actions. *Journal of Monetary Economics*, 57(6):682–695.

Baeriswyl, R. and Cornand, C. (2014). Reducing overreaction to central banks disclosure: theory and experiment. *Journal of the European Economic Association*, forthcoming.

Cabrales, A., Nagel, R., and Armenter, R. (2007). Equilibrium selection through incomplete information in coordination games: An experimental study. *Experimental Economics*, 10(3):221–234.

Cornand, C. and Heinemann, F. (2014). Measuring agents' reaction to private and public information in games with strategic complementarities. *Experimental Economics*, 17(1):61–77.

Dale, D. J. and Morgan, J. (2012). Experiments on the social value of public information. *mimeo*.

Fischbacher, U. (2007). z-tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics*, 10(2):171–178.

Heinemann, F., Nagel, R., and Ockenfels, P. (2004). The theory of global games on test: Experimental analysis of coordination games with public and private information. *Econometrica*, 72(5):1583–1599.

Heinemann, F., Nagel, R., and Ockenfels, P. (2009). Measuring strategic uncertainty in coordination games. *Review of Economic Studies*, 76:181–221.

Hellwig, C. and Veldkamp, L. (2009). Knowing what others know: Coordination motives in information acquisition. *Review of Economic Studies*, 76:223–251.

Middeldorp, M. and Rosenkranz, S. (2011). Central bank transparency and the crowding out of private information in an experimental asset market. *Federal Reserve Bank of New York Staff Reports*, (487).

Morris, S. and Shin, H. S. (2002). Social value of public information. *American Economic Review*, 92(5):1521–1534.

Nagel, R. (1995). Unraveling in guessing games: An experimental study. *American Economic Review*, 85:1313–1326.

Shapiro, D., Shi, X., and Zillante, A. (2014). Level-k reasoning in a generalized beauty contest. *Games and Economic Behavior*, forthcoming.

A   Instructions

Instructions to participants varied according to the treatments. We present the instructions for a treatment with $\mu_\eta = 10$ in stage 1, $\mu_\eta = 5$ in stage 2 and $\mu_\eta = 20$ in stage 3. For the other treatments, instructions were adapted accordingly and are available upon request.[14]

General information

Thank you for participating in an experiment in which you can earn money. These earnings will be paid to you in cash at the end of the experiment.

We ask you not to communicate from now on. If you have a question, then raise your hand and the instructor will come to you.

You are a group of 18 persons in total participating in this experiment and you are allocated into three groups of 6 persons. These three groups are totally independent and do not interact one with another during the whole length of the experiment. Each participant interacts only with other participants in his group and not with the participants of the other group. The current instructions describe the rules of the game for a group of 6 participants.

The rules are the same for all the participants. The experiment consists of 3 stages, each including 10 periods. At each of the 30 periods, you are asked to make two decisions. Your payoff depends on the decisions you make all along the experiment. The stages differ from one another by the hints (indicative values) that will be given to you to make your decisions.

Section A describes how your payoff is calculated at each stage. Sections B, C and D describe the indicative values you have at stages 1, 2 and 3 respectively.

A - Rule that determines your payoff at each of the 30 periods (3 stages of 10 periods)

Z is an unknown positive number. This unknown positive number is different at each period but identical for all the participants (of the same group).

At each period, you are asked to make two decisions. Your payoff in ECU (Experimental Currency Unit) at each period is composed of the payoff associated with your decision 1 and the payoff associated with your decision 2.

Decision 1   On the one hand, your payoff in ECU (Experimental Currency Unit) associated with your decision 1 is given by the formula:

$200 - (yourdecision1 - Z)^2$**.**

This formula indicates that your payoff gets higher the closer your decision 1 to the unknown number **Z**.

---

[14]What follows is a translation (from French to English) of the instructions given to the participants.

Decision 2 On the other hand, your payoff in ECU associated with your decision 2 is given by the formula:

$400 - 1, 5(your decision 2 - Z)^2 - 8, 5(your decision 2 - average decision 2 of other participants)^2$.

This formula indicates that your payoff gets higher the closer your decision 2 to on the one hand the unknown number **Z** and on the other hand the average decision of the other participants.

To maximize your payoff you have to make a decision 2 that is

- as close as possible to the unknown number **Z** and

- to the decision 2 of the other participants.

Note however that it is more important to be close to the average decision 2 of the other participants than to the unknown number **Z**.

No participant knows the true value of Z when making his decisions. However, each participant receives some hints on the unknown number **Z** as explained in sections B, C and D.

B - Your hints on Z during stage 1 (10 periods)

At each period of the first stage, you receive two hints (numbers) on the unknown number **Z** to make your decision. These hints contain unknown errors.

- **Private hint X drawn from the interval $[Z - 10, Z + 10]$** Each participant receives at each period a private hint X on the unknown number Z. The private hints are selected randomly over the error interval $[Z - 10, Z + 10]$. All the numbers of this interval have the same probability to be drawn. Your private hint and the private hint of any of the other participants are selected independently from one another over the same interval, so that in general **each participant receives a private hint that is different from that of the other participants.**

- **Common hint Y drawn from the interval $[Z - 10, Z + 10]$** On top of this private hint X, you, as well as the other members of your group, receive at each period, a common hint Y on the unknown number Z. This common hint is also randomly selected over the interval $[Z - 10, Z + 10]$. All the numbers of this interval have the same probability to be selected. This common hint Y is **the same for all participants.**
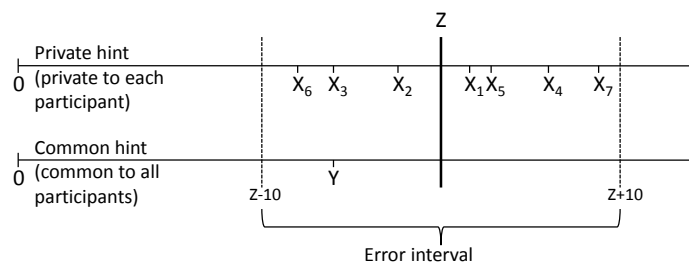
Distinction between private hint X and common hint Y Note that at the first stage, your private hint **X** and the common hint **Y** have the same precision: each is drawn from the same error interval. The sole distinction between the two hints is that each participant observes a private hint **X** that is different from that of the other participants whereas all the participants observe the same common hint **Y**.

How to make a decision? As you do not know the errors associated with your hints, it is natural to choose, as a decision, a number that is between your private hint **X** and the common hint **Y**. To make your decisions, you are asked to select two numbers, by clicking on a scale that is defined between your private hint **X** and the common hint **Y**. You thus have to choose how to combine your two hints in order to maximize the payoff associated with your decision 1 and your decision 2.

Once you have chosen each of your decisions 1 and 2, click on the Validate button. Once all the participants have done the same, a period ends and you are told about the result of the period. Then a new period starts.

As soon as the 10 periods of the first stage are over, the second stage of the experiment starts.

C - Your hints on Z during stage 2 (10 periods)

The second stage is different from the first in that the precision of **the common hint** increases: it is **twice more informative** than the private hint on the unknown number **Z**.

– **Private hint X drawn from the interval [Z-10, Z+10]** Each participant receives at each period a private hint **X** on the unknown number **Z**. The private hints are selected randomly over the error interval **[Z-10, Z+10]**. All the numbers of this interval have the same probability to be selected. Your private hint and the private hint of each other participant are selected independently from one another over the same interval, so that in general **each participant receives a private hint that is different from that of the other participants.**

– **Common hint Y drawn from the interval [Z-5, Z+5]** On top of this private hint **X**, at each period, you receive a common hint **Y** on the unknown number **Z**. This common hint is randomly selected on the interval **[Z-5, Z+5]**. All the numbers of this interval have the same probability to be selected. This common hint is **the same for all the participants.**

In accordance with stage 1, at each period, you have to make two decisions. The only difference compared to the first stage is that the common hint is twice more precise than the private hint.

As soon as the 10 periods of the second stage are over, the third stage of the experiment starts.

D - Your hints on Z during stage 3 (10 periods)

At each period of the third stage, you receive two hints on **Z** to make your decisions. This time, **the common hint** is less precise: it is **twice less precise** than your private hint on the unknown number **Z**.

- Private hint X drawn from the interval [Z-10, Z+10] Each participant receives at each period a private hint **X** on the unknown number **Z**. The private hints are selected randomly over the error interval **[Z-10, Z+10]**. All the numbers of this interval have the same probability to be selected. Your private hint and the private hint of each other participant are selected independently from one another over the same interval, so that in general **each participant receives a private hint that is different from that of the other participants.**

- **Common hint Y drawn from the interval [Z-20, Z+20]** On top of this private hint **X**, at each period, you receive a common hint **Y** on the unknown number **Z**. This common hint is randomly selected on the interval **[Z-20, Z+20]**. All the numbers of this interval have the same probability to be selected. This common hint is **the same for all the participants.**

In accordance with stage 1, at each period, you have to make two decisions. The only difference compared to the first stage is that the common hint is twice less precise than the private hint.

As soon as the 10 periods of the third stage are over, the experiment ends.

You will be told about each change in stage.

Questionnaires:

At the beginning of the experiment, you are asked to fill in an understanding questionnaire on the computer; when all the participants have responded properly to this questionnaire, the experiment starts. At the end of the experiment, you are asked to fill on a personal questionnaire on the computer. All information will remain secret.

Payoffs: At the end of the experiment, the ECUs you have obtained are converted into Euros and paid in cash. 700 ECUs correspond to 1 Euros.

If you have any question, please ask them at this time.

Thanks for participating in the experiment!

B   Understanding questionnaire

The training questionnaire varied according to the treatments. Each of the 10 following questions had to be answered by right or wrong, yes or no or multiple choices.

1. During each period of the three stages of the experiment, you always interact with the same participants.

2. At each period of the three stages, all the participants of the same group receive the same private hint X.

3. At each period of the three stages, all the participants receive the same common hint Y.

4. Is it rational to make a decision outside the interval defined by your two hints?

5. Your payoff associated with decision 1 does not depend on the average decision 1 of the other members of your group.

6. To maximize your payoff associated with your decision 2, it is more important that your decision 2 is closer to the unknown number Z than the average decision 2 of the other members of your group.

7. Suppose the value of Z is equal to 143 and the average decision 2 of the other participants of your group is equal to 133, what will be your payoff in ECU if your decision 2 is equal to 138: 150? 300? 350?

8. Generally the private hint X is as informative on the average decision 2 of the other participants as the common hint Y.

9. The difference between stages 1 and 2 is that the common hint Y is more precise than the private hint X on the unknown number Z.

10. The difference between decision 1 and decision 2 is that the payoff associated with decision 1 is independent from the decision 1 of the other participants whereas the payoff associated with decision 2 depends on the decision 2 of the other participants.

C Example of decision and feedback screens

VALEURS INDICATIVES

Vous recevez deux valeurs indicatives sur le nombre inconnu Z.

La valeur indicative privée X que vous recevez en propre, tirée de l'intervalle [Z-10, Z+10], est : 649.7

La valeur indicative Y commune à tous les participants, tirée de l'intervalle [Z-10, Z+10], est : 650.7

DECISION 1

Prenez votre décision 1 afin de maximiser votre gain :
200 - (votre décision 1 - Z) ²

- ○ Valeur commune Y ○ 650.70
- ○ 95% ○ 650.66
- ○ 90% ○ 650.61
- ○ 85% ○ 650.55
- ○ 80% ○ 650.50
- ○ 75% ○ 650.45
- ○ 70% ○ 650.40
- ○ 65% ○ 650.35
- ○ 60% ○ 650.30
- ○ 55% ○ 650.24
- ○ moyenne ○ 650.19
- ○ 45% ○ 650.14
- ○ 40% ○ 650.09
- ○ 35% ○ 650.04
- ○ 30% ○ 649.99
- ○ 25% ○ 649.93
- ○ 20% ○ 649.88
- ○ 15% ○ 649.83
- ○ 10% ○ 649.78
- ○ 5% ○ 649.73
- ○ Valeur privée X ○ 649.70

DECISION 2

Prenez votre décision 2 afin de maximiser votre gain:
400 - 1.5*(votre décision 2 - Z)² - 8.5*(votre décision 2 - décision 2 moyenne des autres) ²

- ○ Valeur commune Y ○ 650.70
- ○ 95% ○ 650.66
- ○ 90% ○ 650.61
- ○ 85% ○ 650.55
- ○ 80% ○ 650.50
- ○ 75% ○ 650.45
- ○ 70% ○ 650.40
- ○ 65% ○ 650.35
- ○ 60% ○ 650.30
- ○ 55% ○ 650.24
- ○ moyenne ○ 650.19
- ○ 45% ○ 650.14
- ○ 40% ○ 650.09
- ○ 35% ○ 650.04
- ○ 30% ○ 649.99
- ○ 25% ○ 649.93
- ○ 20% ○ 649.88
- ○ 15% ○ 649.83
- ○ 10% ○ 649.78
- ○ 5% ○ 649.73
- ○ Valeur privée X ○ 649.70

Veuillez prendre vos décisions en cliquant entre les deux valeurs indicatives, puis cliquez sur Valider.

[Valider]

VALEURS

Votre valeur indicative privée X, est	649.7
La valeur indicative commune Y, est	650.7
La valeur de Z	658.4

DECISION 1

Votre décision 1	650.19
Votre gain associé à la décision 1	132

DECISION 2

La décision 2 moyenne des autres	0.00
Votre décision 2	650.19
Votre gain associé à la décision 2	-3593075
Ecart entre votre valeur indicative privée X et la décision 2 moyenne des autres	649.7
Ecart entre la valeur indicative commune Y et la décision 2 moyenne des autres	650.7

GAINS TOTAUX

Votre gain pour cette période	-3592942
Vos gains cumulés	-3592942

[Suite]