Measuring agents' overreaction to public information in games with strategic complementarities

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Abstract:
Some recent theoretical papers following Morris and Shin (2002) have emphasised the role of agents' overreaction to public information in beauty contest games, which leads to reconsidering the benefits from transparency. We present an experiment on a coordination game that is characterised by both fundamental and strategic uncertainty: agents receive public and private information about a fundamental state. They have an incentive to choose actions that are close to the fundamental state but they also have an incentive to coordinate their actions. We find that, in line with theoretical predictions, subjects put a larger weight on the public signal. However, the weight is smaller than theoretically predicted. These weights can be explained by limited levels of reasoning. Stated second order beliefs indicate that subjects underestimate the information contained in public signals about other players' beliefs. In the extreme case of a pure co-ordination game (without incentive to meet fundamentals) subjects still use their private signals, preventing full co-ordination. These results indicate that (i) public information is less detrimental to welfare than predicted by theory, (ii) providing private information matters and is eventually reducing welfare.

JEL Classification: C72, C92, D84.

Keywords: co-ordination games, strategic uncertainty, private information, public information.

1 – Introduction

Some recent theoretical papers following Morris and Shin (2002)[1] have emphasised the role of agents' overreaction to public information in beauty contest games, which leads to reconsider the benefits from transparency. Indeed, when theoretically considering financial markets and macroeconomic environments characterised by positive externalities, transparency may be welfare detrimental as public announcements serve as focal points for higher-order beliefs and affect agents' behaviour more than justified by their informational content. Svensson (2006) has argued that detrimental welfare effects of public information are unlikely, because they require that public information is of lower precision than private information, which is opposed to empirical findings about the quality of various

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[1] Among others, see Angeletos and Pavan (2004), Hellwig (2005), Angeletos and Pavan (2007a,b), Cornand and Heinemann (2008), and Myatt and Wallace (2008).

kinds of information.[2] Private information, on the other hand, is always welfare improving in the equilibrium of a beauty contest game.

Welfare effects of public and private information depend on two considerations requiring empirical investigation: the relative precision of the two signals and the relative weight that agents attach to these signals. These considerations interact: the higher the weight that agents attach to public signals, the more likely does an imprecise public signal reduce welfare. In rational expectations equilibrium, the relative weight is determined by the signals' relative precision and agents' payoff functions. However, we know from Nagel (1995), and many other experimental papers that equilibrium may be a very poor predictor for actual behaviour, when it requires infinite levels of reasoning as in the guessing game.

Empirically, the focal potential of public information cannot be neglected. In an experiment on a speculative attack game, Cornand (2006) shows that subjects put a larger weight on the public signal if they receive both a private and a public signal about the state of the economy. However, the crucial issue related to measuring the extent of agents' overreaction to public information does not yet have its empirical counterpart. This paper precisely aims at filling in this gap by measuring and analysing the actual multiplier effect of public signals in such environments owing to an experiment.

We present an experiment on a coordination game based on the theoretical framework by Morris and Shin (2002) that is characterised by both fundamental and strategic uncertainty: agents have to choose actions that are close to a fundamental state but also close to what the others believe. We test predictions of this approach by implementing a simultaneous-move game with different weights on fundamental and strategic uncertainty.

In a benchmark case, where subjects' payoffs depend only on how close their action is to an unknown fundamental, they use all information of the same precision with equal weights, regardless of whether information is private or public. Thereby, they follow the theoretical advice from Bayesian rationality. If subjects have an incentive to minimize a weighted average of the square distance of their own action from the fundamental and from the action of another player, they put, in line with theoretical predictions, larger weights on the public signal. The weights are significantly larger than ½, but smaller than theoretically predicted. Observed weights can be explained by limited levels of reasoning of order 2. Stated second order beliefs, however, indicate that subjects underestimate the weight of the public signal in a Bayesian update for the conditional distribution of other players' signals. This result provides an alternative explanation for the systematic deviation of behaviour from equilibrium.

In the limiting case, where fundamental uncertainty disappears, and subjects' payoffs depend only on the distance between their actions, theory does not yield a unique prediction. Any coordinated

[2] Romer and Romer (2000) have shown that errors in the Federal Reserve forecasts of inflation are smaller than errors of commercial inflation forecasts.

strategy is an equilibrium. The limit of equilibria in games with a decreasing weight of fundamental uncertainty selects a strategy in which all agents follow the public signal and ignore all private information. In the experiment, we observe that subjects indeed tend to follow the public signal and put a significantly larger weight on it than in games with both, fundamental and strategic uncertainty. However, they still put a positive weight on their private signals, which prevents full co-ordination. Here, the provision of private information leads to inefficient outcomes.

These results indicate that: (i) public information is less detrimental to welfare than predicted by theory, (ii) providing private information matters and is eventually reducing welfare, even when it is intrinsically irrelevant to choices.

The experiment seems to provide convincing data for subjects' overweighing of public information in environments with strategic complementarities. So if we take data seriously, public information can be detrimental to welfare although not as much as what the theory says. This strengthens Svensson's point that public information is usually too precise compared to private signals for transparency reducing welfare. Contrary to all theories, observed deviations from equilibrium give rise to potential welfare reducing effects of private information.

Since observed behaviour is consistent with second-order reasoning, we provide a theoretical analysis of welfare effects in the original model by Morris and Shin (2002) assuming that agents limit their reasoning to second-order beliefs. We show that for this form of limited rationality, public information is always welfare-enhancing, while private information may be welfare-detrimental if coordination is desirable from a welfare point of view. This partially turns around results that have been obtained for fully rational agents, where private information is always welfare-enhancing and public information may be detrimental.

Our work relates to Anctil *et al.* (2004) who propose an experiment that mitigates the benefits of transparency to help decentralised decision makers to co-ordinate efficiently, when participants face both strategic and fundamental uncertainties. However, they interpret transparency as the informativeness of the private signal the agent receives regarding the underlying state (see Walther (2004)). Moreover, they consider public information as a strict prior, while here, as in Heinemann, Nagel and Ockenfels (2004) and Cornand (2006), we give a public signal to agents that is distinct from the prior distribution of the fundamental state. One other difference is that the design of Anctil *et al.* is contextualised.[3]

[3] For other experiments on co-ordination games, see for example Van Huyck, Battaglio and Beil (1990, 1991); they show that with perfect information subjects coordinate rather quickly, efficiency depending on group size and experience. See also Nagel (1995), which presents an experiment on a beauty contest game and shows that although information is publicly disclosed, agents are unable to form high common beliefs. These studies however are under public information. See Heinemann, Nagel and Ockenfels (2004) for an experiment with differential information. For experiments dealing with public *versus* private information, see Forsythe, Palfrey, and Plott (1982), Plott and Sunder (1988), McKelvey and Ordeshook (1985), McKelvey and Page (1990) and

The next section presents the model, section 3 sets the experimental design and section 4 provides equilibrium predictions and hypotheses. Section 5 states the results. Finally section 6 concludes.

2 – The model

We describe a coordination game in which agents have to choose actions that are close to a fundamental state but also close to what the others believe. The framework is very close to Morris and Shin (2002).[4]

The fundamental state of nature is given by θ and has a uniform distribution on the reals. There is a continuum of agents, indexed by the unit interval [0,1]. Agent *i* chooses an action $a_i \in \Re$, and we write *a* for the action profile over all agents. The utility function for individual *i* has two components:

$$u_i(a,\theta) \equiv -(1-r)(a_i-\theta)^2 - r\left(a_i-\overline{a}\right)^2 .$$

The first component is a quadratic loss in the distance between the underlying state θ and his action a_i. The second component is the coordination term, a quadratic loss in the distance between the average action in the population $\overline{a} = \int_0^1 a_j dj$ and his action a_i. Finally, *r* is a constant, $0 \leq r \leq 1$, that indicates the weight attributed to the second component.

Agents face uncertainty concerning θ. However, to decide on an action, they potentially receive two kinds of signals that deviate from θ by some error terms with uniform distribution. All agents receive a public (common) signal $y \sim U[\theta \pm \varepsilon]$. In addition, each agent receives a private signal $x_i \sim U[\theta \pm \varepsilon]$. Noise terms $x_i - \theta$ of distinct individuals and the noise of the public signal $y - \theta$ are independent and their distribution is treated as exogenously given.

The optimal action of agent *i* is given by the first order condition:

$$a_i = (1-r)E_i(\theta) + r\,E_i\left(\overline{a}\right),$$

where $E_i(\cdot)$ is the expectation operator of player *i*. In equilibrium, agent *i*'s action is given by

$$a_i^* = \frac{y + (1-r)\,x_i}{2-r} .$$

The equilibrium weight on public information is $1/(2-r)$ and exceeds its weight of ½ in the Bayesian expectation of the fundamental,

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Hanson (1996). They investigate how individuals use public information to augment their original private information, and whether in doing so, a rational expectations equilibrium is attained.

[4] The main changes concern the distributions of the signals (that are chosen uniform instead of normal) and the form of the utility function (we chose a pure quadratic form because it is easier to understand for participants to the experiment).

$$E(\theta \mid y, x_i) = \frac{y + x_i}{2}.$$

In equilibrium, actions are distorted away from θ towards *y*. The distortion increases in the weight *r*. This mirrors the disproportionate impact of the public signal in co-ordinating agents' actions. This model emphasises the role of public information as a focal point for private actions. Strategic complementarities provide incentives to co-ordinate on the publicly announced state of the world and neglect private information. If public announcements are inaccurate, private actions are drawn away from the fundamental value. Overreaction to public information is costly insofar as it can lead agents to co-ordinate far away from what is fundamentally justified (if public information is not sufficiently precise). Public information is a double-edged instrument: it conveys valuable information, but the desire to co-ordinate leads agents to condition their actions stronger on public announcements than is optimal.

This analysis raises doubts about the benefits of transparency, i.e. the provision of fully public information. Financial markets and macroeconomic environments are often characterised by strategic complementarities. For example, during speculative episodes, it is rewarding for a trader to attack a currency if other traders decide to do so; monopolistic competition also implies that a firm changes its price in direction of the price changes by its competitors. While, transparency is supported by central banks and international institutions, the provision of public announcements can destabilise markets by generating some over-reaction. How much agents over-react to public information and therefore whether it is beneficial to provide fully public information in such macroeconomic contexts is an empirical question. If over-reaction to public information is negligible, providing more information to the market is suitable as it enables agents to make better informed decisions; by contrast, if empirical/ experimental evidence shows that over-reaction is strong, one may question the standard view about the welfare effects of transparency.

To answer this, we propose an experiment that measures the actual multiplier effect of public information. We present a 2–player version of the above mentioned game. Subjects repeatedly play the game with randomly selected partners.

3 – The experimental design

Sessions were run at the BETA (*Bureau d'Economie Théorique et Appliquée*) laboratory in Strasbourg (using software Regate (Zeiliger, 2000)) in January and February 2008. Each session had 16 participants who were mainly students from Strasbourg Louis Pasteur University (most of them were students in economics, mathematics, biology and psychology). Subjects were seated in a random order at PCs. Instructions were then read aloud and questions answered in private. 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. This aimed at having a proper training period to ensure participants understand the game. Instructions and questionnaires are given in Appendix 8-A. The experiment started after all subjects had given the correct answers to these questions.

Each session consists of four stages with a total of 50 periods. In each period, subjects were randomly matched to pairs. They did not know the identity of their partner and they knew that they would most likely not meet the same partner in the next period. For each pair, a fundamental state θ was drawn randomly with a uniform distribution on the interval [50, 450]. Each subject received a private signal $x_i \in [\theta - 10, \theta + 10]$. In addition each pair of subjects received a public signal $y \in [\theta - 10, \theta + 10]$. Signals were drawn independently from these intervals with a uniform distribution.[5] The random process was explained in the instructions.

In the first three stages, each subject had to decide for an action a_i, conditional on her signals. Subjects could choose any number within [*Y*-20, *Y*+20]. Signals and actions were decimals with one digit behind the point. In the first stage (Treatment A, 5 periods), the payoff function was given by $100 - (a_i - \theta)^2$. Here, subjects should choose an action as close as possible to the fundamental state, independent from their partners' choices. In the second stage (Treatment B, 10 periods), the payoff function was given by $100 - (a_i - a_j)^2$, where a_i denotes the partner's action. Here, subjects should coordinate their actions irrespective of the fundamental state. In the third stage (30 periods), the payoff function was $200 - (a_i - a_j)^2 - (a_i - \theta)^2$ in Treatment C and $400 - (a_i - a_j)^2 - 3(a_i - \theta)^2$ in Treatment D. Treatment C corresponds to the model by Morris and Shin (2002) with $r = 0.5$. Treatment D corresponds to $r = 0.25$. Treatments A and B are extreme cases with $r = 0$ and $r = 1$, respectively. In the fourth stage (Treatment E, 5 periods) each subject was asked to state an expectation for the state and for her partner's stated expectation of the state. Here, the payoff function was $100 - (e_i(\theta) - \theta)^2 - (e_i(e_j(\theta)) - e_j(\theta))^2$, where e_i denotes the stated expectation of subject *i*.

We conducted 12 sessions with a total of 192 subjects, 6 sessions had treatment C and 6 had treatment D in the third stage. Table 1 gives an overview sessions and treatments.

Sessions	*Stage 1* *5 periods*	*Stage 2* *10 periods*	*Stage 3* *30 periods*	*Stage 4* *5 periods*
1 to 6	*Treatment A* *r*=0	*Treatment B* *r=1*	*Treatment C* *r*=0.5	*Treatment E* *stating expectations*
7 to 12			*Treatment D* *r*=0.25	

Table 1 – Recapitulative table for the different sessions

[5] Having a sufficiently large support of the prior distribution compared to ε enables us to reduce the informational content conveyed by the prior mean. In addition, this reduces the set of signals, for which the conditional posterior distribution is skewed.

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 of the next period. Subjects knew from the start that they were playing the aforementioned treatments in this order. At the end of each session the earned points were summed up and converted into Euros (100 points were converted to 25 cents in sessions 1 – 6 and to 43 cents in sessions 7 – 12).

In all stages, it was possible to earn negative points. This actually occurred in about 3.4% of all decision situations. With one exception, realized losses were of a size that they could always be counterbalanced by positive payoffs within three periods.[6] In total, no subject earned a negative payoff in any session.

Payoffs ranged from 21 to 31 Euros. The average payoff was about € 27. Sessions lasted for around 90 minutes.

4 – Equilibrium predictions and hypotheses

Treatment A (r=0) is a crude test of Bayesian rationality. Since both signals have the same precision, the rational choice is action $a_i = (x_i + y)/2$ whenever both signals are in the interval [60, 440]. When signals are smaller than 60 or larger than 440, the posterior distribution of θ is skewed, because of the limited support of θ.

Treatment B (r=1) is a pure coordination game. Any strategy that maps the public signal into the reals is an equilibrium, provided that all subjects in a session coordinate on the same strategy. The public signal, however, provides a focal point, so that we expected subjects to coordinate on actions $a_i = y$.

Treatments C (r=0.5) and D (r=0.25) resemble the game by Morris and Shin (2002). Given that players choose linear combinations of both signals $a_j = \gamma x_j + (1-\gamma)y$, the equilibrium weight on the private signal in treatments C and D is

$$\gamma^* = \frac{1-r}{2-r}.$$

Note that the limit of γ^* for $r \to 1$ selects the equilibrium with $a_i = y$ for r = 1. In the next section, we test various hypotheses arising from equilibrium values and their comparative statics with respect to r.

[6] The exception is subject 7 in session7, who chose 200.9 in the second period of stage 1, when the state was 229.3 (signals were y=219.7 and x_i=235.5). It took him or her until period 8 of the second stage to compensate this loss. In total, there were 4 subjects earning a negative payoff in stage 1, one subject in stage 2, and 5 subjects in stage 4.

Due to the limited support of θ, the equilibrium deviates from this linear combination into the direction of the centre of the support. To see this, imagine that player i receives signals $x_i = 40$ and $Y = 50$. From her private signal, she can deduce that $\theta = 50$. The posterior distribution of the other player's signal is uniform in [40, 60]. Since the other player should never choose an action below 50, the expected action by the other player is above 50. Therefore, player i should also choose an action above 50. Obviously, it is too demanding to assume that players update their beliefs correctly at the edges of the distribution. We find systematic effects at the edges for signals up to 60 and above 440. Hence, we restrict data analysis to situations with $70 < \theta < 430$. Thereby, we exclude about 10 per cent of all situations.

Table 2 summarizes the equilibrium weights on the private signal γ^* for interior states.

r	0	0.25	0.5	1
γ^*	0.50	0.43	0.33	0

Table 2 – Theoretical values of γ^* in function of r

5 – The results

We use the following structure in analyzing data from treatments A to D in situations with $70 < \theta < 430$: In Subsection 5.1, we analyze rationality of choices, in 5.2 we give summary statistics for the weights that subjects attach to the private signal. In 5.3, we investigate (separately for each treatment), whether observed weights on the private signal are positive, smaller than 0.5, and whether they deviate systematically in one direction from the theoretical prediction. We also test for convergence of behaviour over time by distinguishing data from the first and second half of a treatment. Comparing observed weights, we test comparative static predictions arising from theory in subsection 5.4. Finally, we test whether data can be explained by limited levels of reasoning or by underestimating the weight of public information in a Bayesian update of expectations about other subjects' expectations.

5.1. Some considerations about rationality

Bayesian rationality requires choosing an action that is a linear combination of the two signals. Hence, actions should be contained in $I_3 = [\min(y, x_i), \max(y, x_i)]$. However, we observe that many subjects chose actions outside this interval. Another reference point for rational behaviour is given by the support of the conditional distribution of fundamental states. From her signals, subject i can deduce[7] that the true state of the world is contained in the interval $I_1 = [\max(y, x_i) - 10, \min(y, x_i) + 10]$.

[7] This deduction was actually advanced by the understanding questionnaire (presented in Appendix 8.A.2.).

We observe that the proportion of choices outside these intervals get large when the respective interval is small. Since both of these intervals have some appeal for reasonable choices, we check, how many choices were contained in the union of both sets, $I_2 = I_1 \cup I_3$.

Result 1: In all treatments, at least 97% of choices are contained in I_2. The proportion of choices inside I_3 is 85%

In Table 3, we display the percentage of choices within these intervals. Counting the number of choices that are closer to the public signal, closer to the private, or middle in between provides us a first crude impression of whether public signals are overweighed and how dispersed the distribution of relative weights is.

Choice	*r=0*	*r=0.25*	*r=0.5*	*r=1*
Inside $I_1 = [\max(Y,X)-10; \min(y,X)+10]$	*94%*	*97%*	*96%*	*88%*
Outside I_1	*6%*	*3%*	*4%*	*12%*
Inside $I_2 = I_1 \cup I_3$	*97%*	*99%*	*99%*	*98%*
Outside I_2	*3%*	*1%*	*1%*	*2%*
Inside $I_3 = [\min(y, x_i), \max(y, x_i)]$	*75%*	*85%*	*86%*	*87%*
Choice = y	*0%*	*1%*	*7%*	*35%*
Closer to y	*26%*	*30%*	*40%*	*23%*
Mid (+/-0.5, but not beyond I_3)	*21%*	*30%*	*20%*	*14%*
Closer to x_i	*27%*	*24%*	*19%*	*12%*
Choice = x_i	*1%*	*1%*	*1%*	*1%*
Outside I_3 beyond y	*13%*	*8%*	*8%*	*7%*
Outside I_3 beyond x_i	*11%*	*8%*	*6%*	*5%*

Table 3 – Crude classification of choices

When the size of interval I_3 is larger than or equal to 5, 94.3% of choices are inside this range. On average, over all sessions and treatments, more than 85% of choices were inside I_3.[8] The further analyses consider only actions that were inside I_3. We choose to exclude data from subjects whose choices were out of this range, because their weights for either signal are not in [0,1] and might distort average weights. We must exclude outer choices, because they can easily lead to weights of 10 and higher (or -10 and lower), e.g., when $X - Y = 0.1$, but the action is more than 1 away from these signals. These outliers can distort the average even towards a negative weight. See session 6 in Table A.1 in Appendix 8-B., r=0, for an example. These high values distort averages of otherwise small numbers in [0,1].

5.2. Group specific weights

The following table displays and compares average weights on the private signal for all sessions and treatments with theoretical weights.

[8] This proportion is not very different from one treatment to the next and neither decreases over time.

Sessions	Sessions 1 to 6			Sessions 7 to 12		
	r=0	*r*=1	*r*=0.5	*r*=0	*r*=1	*r*=0.25
1 / 7	0.500 (0.162)	0.188 (0.250)	0.380 (0.202)	0.525 (0.168)	0.266 (0.272)	0.496 (0.145)
2 / 8	0.478 (0.164)	0.276 (0.254)	0.462 (0.175)	0.476 (0.175)	0.283 (0.260)	0.474 (0.174)
3 / 9	0.509 (0.145)	0.376 (0.259)	0.438 (0.216)	0.543 (0.184)	0.367 (0.287)	0.510 (0.171)
4 / 10	0.507 (0.172)	0.306 (0.260)	0.449 (0.184)	0.485 (0.209)	0.254 (0.270)	0.489 (0.174)
5 / 11	0.476 (0.200)	0.189 (0.255)	0.338 (0.221)	0.514 (0.148)	0.215 (0.252)	0.484 (0.152)
6 / 12	0.526 (0.176)	0.302 (0.257)	0.431 (0.174)	0.482 (0.155)	0.277 (0.262)	0.485 (0.130)
Average	0.499 (0.170)	0.273 (0.256)	0.416 (0.195)	0.504 (0.173)	0.277 (0.267)	0.490 (0.158)
Theoretical weight	0.5	0	0.33	0.5	0	0.43

Table 4 – Average weights on the private signal and standard deviation within a session (data from all periods, choices in I_3 only).

In the following sections, we analyse participants' behaviour subject to different types of uncertainty (pure fundamental uncertainty (r=0), pure strategic uncertainty (r=1) and simultaneous fundamental and strategic uncertainties ($0<r<1$)) and especially focus on:

- theory matching: we ask whether results are significantly different from what theory predicts;
- over-reaction: we try to detect whether there is over-reaction to the public signal;
- convergence: we look at whether there is some convergence of participants' behaviours towards co-ordination. We test convergence by comparing whether there is a significant difference between the weight assigned to X in the first half of rounds and in the last half of rounds in all treatments that are kept for at least ten rounds.

The following tests are based on counting group averages as independent observations. This is a conservative measure that we employ, because during the course of a treatment, weights of different subjects may converge towards each other and are, therefore, not independent. Thus, we have only 12 independent observations for treatments A and B and 6 for treatments C and D.

5.2.1. Behaviour under (pure) fundamental uncertainty (r = 0, treatment A)

Result 2: For r = 0, subjects put an equal weight on both signals, which is consistent with theoretical results.

- *Theory matching*: In both groups of sessions, when *r*=0, the Wilcoxon matched pairs signed rank test[9] shows that there is no significant difference[10] between the weight assigned to the private signal *X* and the theoretical weight of 0.5 (see test in Appendix 8-C.1.).[11]

5.2.2. Behaviour under (pure) strategic uncertainty (r = 1, treatment B)

Result 3: For r = 1, subjects significantly over-react to public information (they tend to co-ordinate on the public signal), but less than what is theoretically predicted.

- *Theory matching*: In both groups of sessions, the Wilcoxon matched pairs signed rank test shows that there is a significant difference between the weight assigned to the private signal *X* and the theoretical weight (0) (see test in Appendix 8-C.2.).
- *Over-reaction*: In both groups of sessions, owing to the two samples permutation test[12], we can detect an over-reaction to the public signal that is stronger than the theoretical value for *r*=0.5 in sessions 1 to 6 and for *r*=0.25 in sessions 7 to 12. In other words: the observed weights assigned to the private signal *X* when *r*=1 are significantly smaller than 0.33 for sessions 1 to 6 and 0.43 for sessions 7 to 12 (see tests in Appendix 8-C.3.).
- *Convergence*: We compare the weight assigned to *X* in the first 5 rounds and in the last 5 rounds of stage 2 (when *r*=1) of both groups of sessions simultaneously and find a significantly smaller weight in the last rounds. So there is a significant convergence of behaviours towards lower weights on *X* (see Table A.2 and tests in Appendix 8-C.8.).

In the extreme case of a pure co-ordination game (without incentive to meet fundamentals), subjects still use their private signals, which prevents full co-ordination (although there is some convergence). So providing private information matters and is welfare damaging even though it is irrelevant to choices.

5.2.3. Behaviour under both fundamental and strategic uncertainties (0 < r < 1, treatments C and D)

Result 4: For r = 0.5 and r = 0.25, many subjects significantly over-react to public information but less than what theory predicts and less than if only strategic uncertainty mattered. For r = 0.5 *the weight on private information is significantly smaller than for r* = 0.25.

[9] We use the Wilcoxon signed ranked test because we can both tell the sign of the difference between any pair and rank the differences in order of absolute size (see Siegel and Castellan (1988)).
[10] All along the paper we take a threshold of significance of 5%.
[11] We do a single test with 12 observations as observations are independent.
[12] The permutation test for two independent samples tests the significance of the difference between the means of two independent samples when the sample sizes *m* and *n* are small. This tests enables us to determine the exact probability associated with our observations under the assumption that H_0 is true without making any special assumption about the underlying distributions in the population involved (see Siegel and Castellan (1988)).

- *Theory matching*: In both groups of sessions, there is a significant difference between the weight assigned to the private signal X when r=0.5 (respectively r=0.25) and the theoretical weight (see the Wilcoxon matched pairs signed rank test*s* in Appendix 8-C.4.).
- *Over-reaction*: In both groups of sessions, we can detect some over-reaction to the public signal as the weight assigned to the private signal X is significantly lower than 1/2. (see the two samples permutation tests in Appendix 8-C.5.).
Subjects over-react to public information, but less than what is theoretically predicted. Since weights on private signals in treatment B (r=1) were smaller than the theoretical values for r=0.5 and r=0.25, respectively, the over-reaction to public signals in treatments C and D is significantly smaller than in treatment B.
- *Convergence*: We compare the weight assigned to X in the first 15 rounds and in the last 15 rounds of stage 3 for sessions 1 to 6 (when r=0.5) and for sessions 7 to 12 (when r=0.25) independently and find no significant difference in either treatment. So there is no convergence process of behaviour towards lower weights on X (see Table A.1 and tests in Appendix 8.C.8.).

These results seem to be corroborated by subjects' written comments in the post-experimental questionnaire[13]. 38% of subjects explicitly wrote (without being directly asked for) that Y is more informative than X on the other participant's decision.

5.3. Monotony

Result 5: Over all sessions, the weight on X was decreasing in r (as predicted by theory).

In sessions 1 to 6, the monotony is clearly verified: there is a significant difference inside sessions 1 to 6 between each step. However, in sessions 7 to 12, while there is a significant difference between data from steps 1 and 3, there is no significant difference between weights attributed to X by the participants when r was equal to 0 and when r was equal to 0.25 (see Appendix 8-C.6.).[14] This means that the co-ordination motive when r=0.25 is not sufficiently strong for subjects to have a significantly different behaviour than when there is no co-ordination motive at all (*i.e.* when r=0). Surprisingly, we could nevertheless notice some over-reaction to public information even in the case where r=0.25 (see section 4.2.3.). Monotony across treatments is verified as the weight attributed to X is significantly lower when r=0.5 than when r=0.25 (see section 4.2.3.).

5.4. Why is over-reaction not as strong as theory predicts?

The reason why over-reaction is not as strong as theory predicts may be linked to agents having higher-order beliefs of a finite level only. However, it may also be a result of underestimating the importance of the public signal in predicting other agents' information.

[13] The post-experimental questionnaire is presented in Appendix 8.A.3.
[14] The negative test between observations for r=0.25 and r=0 but positive result comparing the weight for r=0.25 with ½ must be due to small deviations from ½ in treatment A.

5.4.1. Analyzing limited levels of reasoning

We can determine the level of higher order beliefs participants were close to. Suppose that a player *j* attaches weight γ_k to her private signal. The best response to this is

$$\begin{aligned} a_i &= (1-r)E_i(\theta) + rE_i(a_j) \\ &= (1-r)E_i(\theta) + r\gamma_k E_i(x_j) + r(1-\gamma_k)y. \end{aligned}$$

Since expected private signal of the other equals the expected state,

$$\begin{aligned} a_i &= \left[(1-r) + r\gamma_k\right]E_i(\theta) + r(1-\gamma_k)y \\ &= \frac{(1-r)+r\gamma_k}{2}x_i + \left[\frac{(1-r)+r\gamma_k}{2} + r(1-\gamma_k)\right]y. \end{aligned}$$

Hence, the next level of reasoning is

$$\gamma_{k+1} = \frac{(1-r)+r\gamma_k}{2}.$$

The following table sums up the results and gives the outcome depending on *r*=0, *r*=1, *r*=0.5 or *r*=0.25.

Values of r	*Level 1*	*Level 2*	*Level 3*	*Level 4*	*...*	*Theoretical value (infinite level of reasoning)*
0.5	0.50	0.375	0.344	0.336	…	0.333
0.25	0.50	0.437	0.429	0.429	…	0.429
0	0.50	0.50	0.50	0.50	…	0.50
1	0.50	0.25	0.125	0.062	…	0

Table 6 – Theoretical weight on *X* depending on *r* and on the level of higher-order beliefs

Average observations show that participants had a level of reasoning between higher order beliefs of degrees 1 and 2 (closer to 2 in most cases). Indeed, the difference between data and theoretical predictions related to the second level of reasoning is generally not significant (see Appendix 8-C.7.[15]).

Result 6: Subjects behaved with a level of higher order beliefs of degree 2.

It is known that in experimental economics, common information does not necessarily leads to a common knowledge situation (Smith, 1991). For the guessing game and for cascade games, Stahl and Wilson (1994), Nagel (1995), and Kübler and Weizsäcker (2004) have already shown that subjects' behaviour is more consistent with finite levels of beliefs over beliefs than with theoretical predictions from common knowledge. Our results extend this result to coordination games à la Morris and Shin (2002).

[15] The difference between the weight attributed to *X* when *r*=0 (both in treatments 1 and 2) and theoretical prediction for second (and any) level of reasoning (0.5) is not significant; this is what is shown in section 4.2.1. Appendix 8-C.7. provides Wilcoxon tests showing that: (i) the difference between the weight assigned to *X* when *r*=1 (both in treatments 1 and 2) and theoretical prediction for second level of reasoning (0.25) is not significant, (ii) the difference between the weight assigned to *X* when *r*=0.5 (sessions 1 to 6) and theoretical prediction for second level of reasoning (0.375) is not significant, (iii) the difference between the weight assigned to *X* when *r*=0.25 (sessions 7 to 12) and theoretical prediction for second level of reasoning (0.437) is not significant.

5.4.2. Belief elicitation

Result 7: Subjects attach a too high weight on the private signal when predicting other subjects' posterior beliefs about fundamentals.

In stage 4 (treatment E), we asked subjects to state their beliefs about the true state of the world and about their partner's stated belief about the state of the world. Here, we directly elicit first and second order beliefs. Theoretically, agents should put a weight of ½ on X in the estimation of θ and a weight of ¼ in estimating the other's guess on θ. This does not require any assumptions about behaviour of others in coordination games. It follows directly from Bayesian rationality and from the assumption that other subjects' do not deviate systematically towards or away from the public signal when forming their expectations about θ. To see this, suppose that the stated belief of subject j is $E_j(\theta) = \alpha_j x_j + (1-\alpha_j) y$. Subject i does not know α_j. However, there is no reason to believe that α_j deviates systematically from 0.5. Hence,

$$E_i(E_j(\theta)) = E_i(\alpha_j)E_i(x_j) + (1-E_i(\alpha_j))y = \frac{E_i(\alpha_j)}{2} x_i + \left(1 - \frac{E_i(\alpha_j)}{2}\right) y .$$

Subject i does not know α_j. However, there is no reason to believe that α_j deviates systematically from 0.5. If $E_i(\alpha_j) = 0.5$, then Bayesian rationality requires that $E_i(E_j(\theta)) = 0.25\, x_i + 0.75\, y$.

Data from stage 4, summed up in the following table, reveal, however, that subjects attach (on average) a weight lower than 0.5 on the private signal when estimating θ and a weight higher than 0.25 when estimating their partner's guess on θ.

Sessions	*Sessions 1 to 6*		*Sessions 7 to 12*	
	Weight on X in the estimation of θ	Weight on X in the estimation of the other participant's guess on θ	Weight on X in the estimation of θ	Weight on X in the estimation of the other participant's guess on θ
1 / 7	0.4096	0.1976	0.4507	0.2803
2 / 8	0.5053	0.3698	0.4647	0.3146
3 / 9	0.4731	0.3773	0.5096	0.4374
4 / 10	0.4718	0.2510	0.4423	0.3217
5 / 11	0.3923	0.1829	0.4664	0.3104
6 / 12	0.4982	0.3230	0.4834	0.3155
Average	0.4583	0.2836	0.4695	0.3299

Table 7 – Weight on X in stage 4

If we take stated beliefs in treatment E as a measure of beliefs in the treatments C and D, we find two opposing effects for the weight on the private signal: for the motive of meeting the fundamental state, private signals are underused, while for predicting the partner's beliefs (on which his choice may be assumed to depend) the private signal is given a higher weight than prescribed by Bayesian rationality. In total, the second effect seems to dominate the first, so that we still find a deviation of choices in treatments C and D from the equilibrium towards higher weights on private signals. Thereby, the mistake in the Bayesian update of beliefs about others' beliefs may be able to explain why actions in treatments C and D deviate from the equilibrium towards higher weights on the private signal.

It is surprising, though, that subjects deviate so strongly from the Bayesian expectation of θ. This is even more surprising in the light of results from treatment A, where subjects used (on average) a weight of 0.5. There may be an order-effect, such that after 40 periods in stages 2 and 3, in which the public signal was more important than the private, subjects underestimate the importance of the private signal in the Bayesian update in stage 4. This, however, should also hold for guessing expectations about others' expectations. Thus, without an order-effect, we should see an even larger weight on the private signal in forming higher-order beliefs.

For the Bayesian update of higher-order beliefs we assumed that $E_i(\alpha_j) = 0.5$. In fact, data reveal that on average $\alpha_i \approx 0.46$. If subjects would actually guess this correctly, the optimal weight on the private signal in higher-order beliefs is only 0.23. Thus, the observed higher weights cannot be explained by subjects correctly expecting that others are not Bayesian rational.

Ruling out order effects and best response to others' deviation from rationality as possible explanations for the high observed weight on the public signal in forming higher-order beliefs, leaves us with the impression that subjects simply underestimate how informative the public signal is for predicting others' expectations. This can be viewed at as a systematic error in Bayesian updating.

6 – Welfare analysis

There is a current debate about the welfare effects of public disclosures. In the Morris and Shin' model, increasing the precision of private information is always beneficial, while increasing the precision of public information may be detrimental to welfare if public information is not sufficiently precise and gains from coordination are a zero-sum game. The welfare function in Morris and Shin (2002) is such that there is a conflict between individual decisions that have to match both the fundamentals and the decisions of others and the welfare function in which the central bank only aims at bringing decisions as close as possible to the fundamental (the coordination motive is a zero-sum game at the social level).

Angeletos and Pavan (2007a, b) emphasize that the results of this literature are sensitive to the extent to which coordination is socially valuable. Hellwig (2005), Roca (2005), and Lorenzoni (2005) and Woodford (2005) provide models similar to Morris and Shin (2002), but where coordination is socially valuable. If gains from coordination enter the welfare function, as in Hellwig (2005), increasing precision of both signals enhances welfare.[16]

[16] Hellwig (2005) shows in a fully micro-founded model that more accurate public information about monetary shocks is always welfare increasing because it reduces price dispersion. Here, coordination is socially desirable.

In this section, we analyse the welfare effects of information depending on its nature, public or private, given that agents attach a higher weight on the public signal than predicted by equilibrium theory. We distinguish two welfare functions, the original one from Morris and Shin and the alternative formulation where coordination is socially desirable.

6.1. Would providing more precise public or private information be beneficial?

The experiment has shown that subjects weigh the two signals as if they employ at most two levels of reasoning. Following this result, we analyse welfare effects of increasing the precision of signals in an economy where agents have bounded rationality in the sense of limited levels of reasoning. For this theoretical exercise we follow the original set-up of Morris and Shin (2002) using normally distributed signals.

Result 8: In striking contrast to the Morris and Shin's welfare analysis, we find that – when agents have a level of reasoning of degree 2 – increasing the precision of public information is never welfare detrimental, while increasing the precision of private information may be.

6.1.1. Expected welfare loss in the Morris-Shin-model for limited levels of reasoning

In the model of Morris and Shin (2002), the payoff function for agent *i* is given by

$$u_i\left(a,\theta\right) \equiv -\left(1-r\right)\left(a_i-\theta\right)^2 - r\left(L_i-\overline{L}\right),$$

where $L_i \equiv \int_0^1 \left(a_j - a_i\right)^2 dj$ and $\overline{L} \equiv \int_0^1 L_j dj$. The utility function for individual *i* has two components: the first component is the same as in the game presented in section 2 (standard quadratic loss in the distance between the underlying state θ and his action a_i). The second component is the "beauty contest" term. The loss is increasing in the distance between *i*'s action and the average action of the whole population. However, the beauty-contest term is a zero-sum game. An agent gains from predicting the average action better than other players. Social welfare, as defined by the (normalised) average of individual utilities, is given by

$$W\left(a,\theta\right) \equiv \frac{1}{1-r}\int_0^1 u_i\left(a,\theta\right)di = -\int_0^1 \left(a_i-\theta\right)^2 di\ .$$

As a consequence, the social planner, who cares only about social welfare, seeks to keep all agents' actions close to state θ. The payoff structure is a reminiscence of Keynes' beauty contest. Speculators gain from predicting the average opinion better than others. The “beauty contest”-part of the payoff function describes a zero-sum game. Coordination affects individual payoffs but not social welfare.

As in our framework, agents face uncertainty concerning θ. In Morris and Shin (2002), agents receive two kinds of signals that deviate from θ by independent error terms with normal distribution. Each agent receives a private signal $x_i = \theta + \varepsilon_i$ with precision β. Signals of distinct individuals are independent and the distribution of private signals is treated as exogenously given. Agents have access to a public signal $y = \theta + \eta$ with precision α. The public signal is given to all agents.

Now let us solve the Morris and Shin model for a limited level of reasoning (degree 2). The first level of reasoning is given by the Bayesian expectation of θ:

$$E\left(\theta \mid y, x_i\right) = \frac{\beta x_i + \alpha y}{\alpha + \beta},$$

so that the weight on the private signal with a higher order belief of degree 1 is: $\gamma_1 = \dfrac{\beta}{\alpha + \beta}$ and the corresponding action is: $a_j = \gamma_1 x_j + (1 - \gamma_1) y$.

Suppose that a player j attaches weight γ_1 to her private signal. The best response to this is

$$\begin{aligned} a_i &= (1-r)E_i(\theta) + rE_i(a_j) \\ &= (1-r)E_i(\theta) + r\gamma_1 E_i(x_j) + r(1-\gamma_1)y \\ &= (1 - r(1-\gamma_1))E_i(\theta) + r(1-\gamma_1)y \end{aligned}$$

(since expected private signal of the other equals the expected state).

So,

$$a_i = \frac{\alpha\beta(1-r) + \beta^2}{(\alpha+\beta)^2} x_i + \frac{\alpha^2 + \alpha\beta(1+r)}{(\alpha+\beta)^2} y.$$

Hence, the weight on private signals for a second level of reasoning is

$$\gamma_2 = \frac{\alpha\beta(1-r) + \beta^2}{(\alpha+\beta)^2}.$$

In this framework, the expected welfare is given by:

$$E\left(W\left(a,\theta\right)\right) = -E\left[\int_{i\in(0,1)} \left(a_i - \theta\right)^2 di\right]$$
$$= -E\left[\int_{i\in(0,1)} \left(\gamma_2 x_i + (1-\gamma_2)y - \theta\right)^2 di\right]$$
$$= -\gamma_2{}^2 \frac{1}{\beta} - (1-\gamma_2)^2 \frac{1}{\alpha}$$
$$= -\frac{(\alpha+\beta)^2 + r^2\alpha\beta}{(\alpha+\beta)^3}.$$

How does welfare change with variations in the precision of public information on the one hand and of private information on the other hand?

We find that $\frac{\partial E\left(W\right)}{\partial \alpha} > 0$ and $\frac{\partial E\left(W\right)}{\partial \beta} > 0$, so the comparative statics exercise shows that increasing the precision of either type of information increases welfare for any value of relative precision. This result is in striking contrast to Morris and Shin, who show that increasing the precision of public signals may reduce welfare, because in equilibrium (with an infinite level of reasoning), the sign of $\partial EW / \partial \alpha$ is ambiguous.

When agents have higher order beliefs of degree 2, they do not expect the others to overreact to public announcements. Apparently, this is sufficient to rule out welfare detrimental effects of public announcements. By means of simulation, we checked whether this is still true if agents apply third level of reasoning. We detected parameter combinations with high r and a high relation β / α, for which an increase in α reduces expected welfare. Note, however, that Svensson's (2006) critique applies even more: The lower the levels of reasoning, the higher must be β / α in order to get a negative welfare effect of increasing α. For level 3, private information must be at least 5 times as precise as public information to get this effect.

6.1.2. Expected welfare loss for two levels of reasoning with coordination entering welfare

Now, let us compare the result from 6.1.1. to that when coordination enters the welfare function. In section 2, we have defined the individual utility function as:

$$u_i(a,\theta) = -(1-r)(a_i - \theta)^2 - r\int_0^1 (a_i - a_j)^2 dj .$$

Summing up individual utilities, we get the following social welfare:

$$W(a,\theta) \equiv \int_0^1 u_i(a,\theta)di = -(1-r)\int_0^1 (a_i - \theta)^2 di - r\int_0^1\int_0^1 (a_i - a_j)^2 djdi\,,$$

which yields the following expected welfare:

$$E(W(a,\theta)) = -(1-r)E\left[\int_{i\in(0,1)} \left(\gamma_2(x_i-\theta)+(1-\gamma_2)(y-\theta)\right)^2 di\right] - rE\left[\int_{i\in(0,1)}\int_{j\in(0,1)} \left(\gamma_2(x_i-x_j)\right)^2 djdi\right]$$

$$= -\gamma_2{}^2\frac{1+r}{\beta} - (1-\gamma_2)^2\frac{1-r}{\alpha}.$$

How does welfare change with variations in the precision of private or public information? By contrast to the welfare analysis by Hellwig (2005) and Angeletos and Pavan (2007a, b) who have agents with an infinite level of higher order beliefs (and who find that more information whether it be public or private is always beneficial), we find that the sign of $\partial E(W)/\partial\beta$ is ambiguous. More precisely, increasing the precision of private information can deteriorate welfare if this precision is relatively low (α/β is small) and the coordination motive matters a lot (*r* relatively close to 1). The reason for the difference with the Morris and Shin's type of analysis lies in that with such a welfare function, coordination matters at a social level and so a relatively imprecise private signal may impede agents to coordinate. In the extreme case where *r*=1, providing subjects with private information would always be detrimental, if they attach a positive weight to it. The reason for the difference with the Hellwig's analysis is linked to the fact that agents have a limited level of reasoning and they do not take into account the public signal in a sufficient manner to achieve coordination.

Increasing the precision of public information is always beneficial here, because it helps to achieve more coordination, which improves welfare.

7 – Concluding remarks

The literature in the vein of Morris and Shin (2002) has largely been interpreted in terms of central bank communication[19]. This literature especially highlights the potentially damaging effects of central banks announcements and the disturbances resulting from too much transparency[20]. The experiment seems to provide convincing data for subjects overweighing public signals if they have a motive to coordinate their actions. The observed weights are, however, better described by limited levels of reasoning than by equilibrium behaviour.

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[19] See for example, Amato, Morris and Shin (2002), Hellwig (2005), Svensson (2006), Morris, Shin and Tong (2006), and Cornand and Heinemann (2008) among others.

[20] Some authors like Angeletos and Pavan (2004), Hellwig (2005), Angeletos and Pavan (2007a,b) and Svensson (2006) question the result of Morris and Shin according to which transparency is welfare detrimental. Most of these authors criticize the welfare function they use to establish their results.

By striking contrast to previous welfare analyses, we find that – when agents have a level of reasoning of degree 2 – increasing the precision of public information is never welfare detrimental, while increasing the precision of private information may be.

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8 – Appendix

8-A.1. Instructions

Instructions to participants varied according to the treatments. We present the instructions for a treatment with r=0, r=1 and r=0.25. For the other treatment, instructions (with r=0, r=1 and r=0.5) were adapted accordingly and are available upon request.[21]

Instructions

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.

Cadre framework of the experiment

You are 16 persons participating in this experiment. The experiment consists of 4 stages, the first one including 5 situations, the second one 10, the third one 30 and the fourth one 5. In each situation you will be randomly matched with one of the other 15 participants. You will not get to know with whom you are matched. The rules are the same for all participants. Situations are independent and in each of them, you will have to take a decision.

RULES COMMON TO ALL STAGES

Decision situation

In each situation you will be randomly matched with one of the other participants.
For each situation a number called Z is drawn randomly from the interval 50 to 450. This number is the same for both of you. All numbers in the interval [50, 450] have the same probability to be drawn.
When you make your decision, you will **not** know the drawn number Z.
However, you will be receiving two hints (numbers) on Z:

- You and the person with whom you are matched, both receive a common hint number Y for the unknown number Z. This common hint number is randomly selected from the interval [Z-10, Z+10]. All numbers in this interval are equally likely. This common hint number Y is the same for both of you.

- In addition to the common hint number, each participant receives a private hint number X for the unknown number Z. The private hint numbers are also randomly selected from the interval [Z-10, Z+10]. All numbers in this interval have the same probability to be drawn. Your private hint number and the private hint number of the person whom you are matched are drawn independently from this interval, so that (in general) you will not get the same private numbers.

RULES OF THE 1ST STAGE (5 situations)

You will be asked to make a decision by choosing some number.
Your payoff positively depends on the proximity between your decision and the true value of the unknown number Z:

$$\text{Payoff in ECU} = 100 - (\text{your decision} - Z)^2 .$$

[21] Here is a translation (from French to English) of the instructions and the questionnaire given to the participants.

This means that your payoff only depends on how close is your decision to the true value *Z* and not on your partner's decision.
Once you have made a decision, click on the OK-button. Once all participants made their decision for the game, a situation is terminated.

RULES OF THE 2ND STAGE (10 situations)

Again, you will be asked to make a decision by choosing some number.
The rules are the same as in the first stage, but here your payoffs are given by:

$$\textit{Payoff in ECU} = 100 - (\textit{your decision} - \textit{the other participant's decision})^2 .$$

This means that your payoff only depends on how close is your action to the action of the other participant and not on the unknown number *Z*.

RULES OF THE 3RD STAGE (30 situations)

Again, you will be asked to make a decision by choosing some number.
The rules are unchanged, but here your payoffs depend positively on the one hand on the proximity between your decision and the unknown number *Z* and on the other hand on the proximity between your decision and the choice of your partner.

$$\textit{Payoff in ECU} =$$
$$400 - 3.(\textit{your decision} - Z)^2 - 1.(\textit{your decision} - \textit{the decision of the other participant})^2 .$$

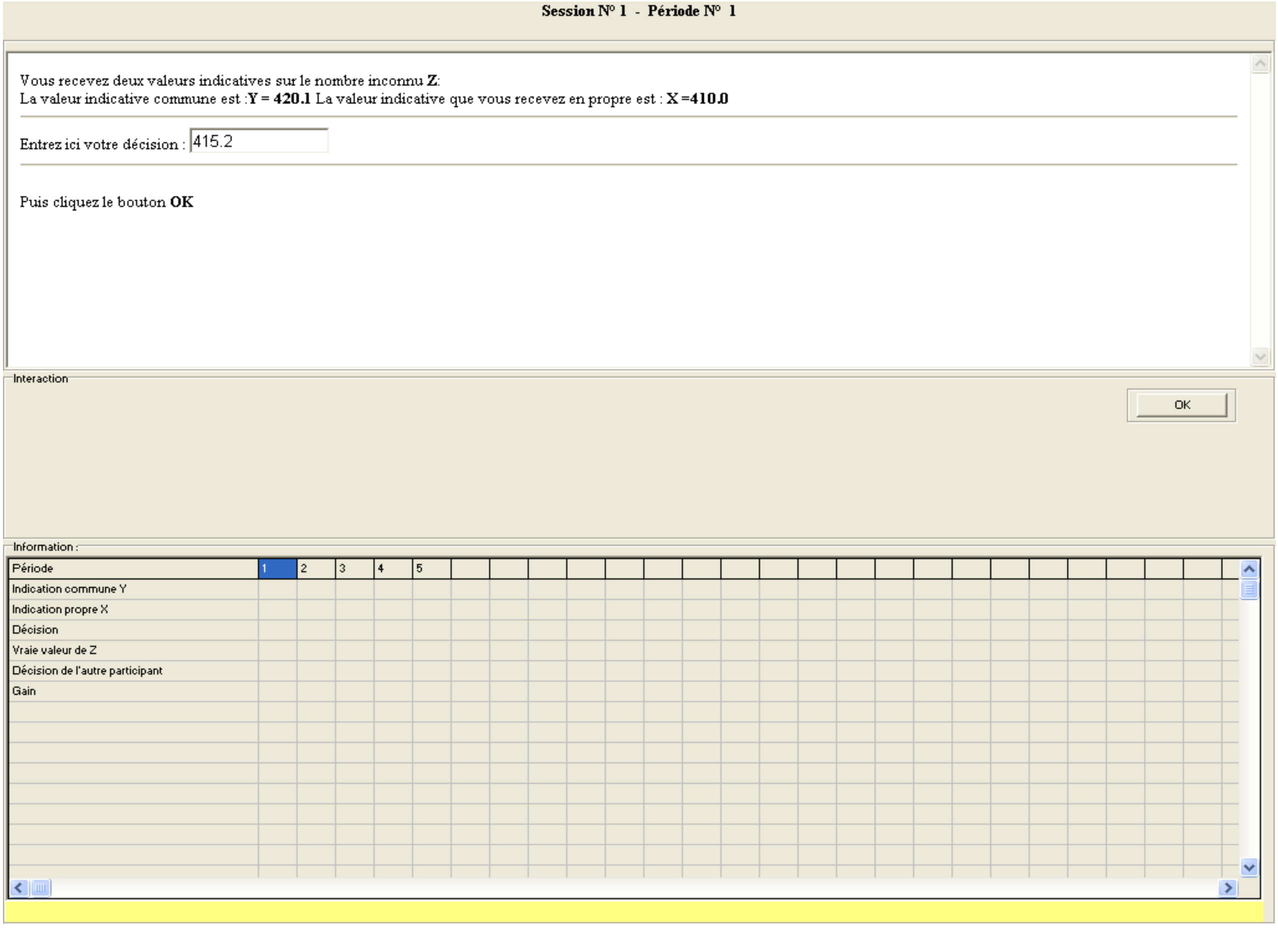
This formula says that your payoff in each situation is at most 400 ECU. It is reduced for deviations of your decisions from the unknown number *Z*, and it is also reduced for deviations between your and your partner's decision. The closer is your decision to both the other participant's choice and *Z*, the higher will be your payoff.

Note that your payoff depends more on the spread between your decision and *Z* than on the spread between your decision and the decision of the other participant.

Example of decision phase:

You receive two hint numbers: One hint is common for the two participants, the other is your private hint. Both hint numbers are drawn with uniform distribution from (Z+/-10)

The common hint number *Y* is: 420.1.
Your private hint number *X* is: 410.0.



You enter your decision, for example here: 415.2.

Information after each situation

Each participant will be informed after each situation on
(1) the common hint *Y* and his private hint *X*,
(2) the true number *Z*, the true choice of his pair-mate,
(3) his own choice and his related payoff.

Example of information phase:
In the previous example, your own choice was 415.2.
Suppose the true value of *Z* is 419.4. Your pair-mate's decision was 413.7.
Then, your payoff will be:
Payoff = $400 - 3.(415.2 - 419.4)^2 - 1.(415.2 - 413.7)^2 = 344.83$ ECU.

RULES OF THE 4TH STAGE (5 situations)

In this stage, rules are a bit different.
Again, *Z* is unknown and you receive two hint numbers, *X* (your private hint) and *Y* (the common hint).
Now, you are asked to choose two numbers:

(1) give what you think is *Z*,
(2) give what you think the other participant think about *Z*.

Your payoff positively depends on the one hand on the proximity between your estimation on *Z* and the true value of *Z* and on the other hand between your estimation of the estimation of the other participant on *Z* and the true estimation of the other participant on *Z*.
Your payoff is given by:

$$100 - (\text{your estimation sur } Z - Z)^2$$
$$-(\text{your estimation of the estimation of the other participant on } Z - \text{the estimation of the other participant on } Z)^2$$

The closer your estimations are to true values, the higher your payoff.

You will be told about each change in stage.

Questionnaires:
At the beginning of the experiment, you will be asked to fill in an understanding questionnaire on a paper. Afterward, the experiment will begin. At the end of the experiment you will fill in a "personal" questionnaire on the computer. All information will remain secret.

Payoffs:
Also at the end of the experiment the ECUs you have obtained are converted into Euros and paid in cash. 1 ECU corresponds to 0.25 Cents.

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

Thanks for your participation!

8-A.2. Understanding and training questionnaire

Fill in

- In each situation, you interact with ________ other participant(s).
- You receive in each situation ________ hints.
- The difference between the unknown number *Z* and any hint is at most _______.

Yes or no

- At stage 3, when a participant makes a decision, does his payoff depend on the decision of his pair-mate? ____
- Do pair-mates receive the same hints? _____
- Is there a hint that is more precise than another? ______
- Do you play with the same participant during the whole length of the experiment? _____

Practice

You are at the third stage of the experiment.
You receive *Y*=135 and *X*=141.

Among the next statements, choose the right one(s):

- o The true value of *Z* is between 125 and 151.
- o The true value of *Z* is 135.
- o The true value of *Z* is between 131 and 145.

Suppose that the true value of *Z* is 143 and that the true decision of your pare-mate is 133. What is your payoff (in ECU) if you choose 134?

Now, suppose again that the true value of *Z* is 143 and the true decision of your paire-mate is 133. What is your payoff (in ECU) if you choose138?

8-A.3. Post-experimental questionnaire

1. How did you make a decision? On which criteria?
2. During the first 3 stages, have you tried to guess the value of *Z*? And the value of the decision of the other participant?
3. Do you think that one of the two indicative hints (private *versus* common) was more informative than the other on *Z*? And on the decision of the other participant?
4. Did you take into account the two indicative hints in the same manner? Or more your private hint? Or more the common hint?

8-B. Weights on *X* for inner and outer choices

	r=0			*r=1*			*r=0.5*		
	Weight on X	For inner choices	For outer choices	Weight on X	For inner choices	For outer choices	Weight on X	For inner choices	For outer choices
Session 1	0.45	0.50	0.04	0.36	0.19	4.89	0.34	0.38	-0.07
Session 2	0.38	0.48	-0.28	0.28	0.27	0.35	0.25	0.46	-1.27
Session 3	0.61	0.51	0.78	-0.15	0.38	-2.89	0.47	0.44	0.60
Session 4	0.52	0.51	0.58	0.25	0.31	-0.13	0.56	0.45	1.18
Session 5	0.74	0.48	1.70	0.24	0.19	0.61	0.29	0.34	-0.14
Session 6	-0.95	0.53	-4.47	0.05	0.30	-1.12	0.49	0.43	0.73
average	0.29	0.50	-0.27	0.17	0.27	0.28	0.40	0.42	0.17
							r=0.25		
Session 7	0.15	0.52	-0.91	0.43	0.27	1.62	0.49	0.50	0.44
Session 8	0.01	0.48	-1.11	0.33	0.28	0.69	0.53	0.47	0.78
Session 9	1.33	0.54	2.80	-0.02	0.37	-1.93	0.60	0.51	0.98
Session 10	0.58	0.48	0.84	0.75	0.25	2.83	0.37	0.49	-0.33
Session 11	-0.21	0.51	-2.53	0.17	0.21	-0.25	0.39	0.48	-0.15
Session 12	0.20	0.48	-0.77	0.30	0.28	0.65	0.43	0.48	-0.03
average	0.34	0.50	-0.28	0.33	0.28	0.60	0.47	0.49	0.28

Table A.1 – Weights on *X* for inner and outer choices

8-C. Non-parametric tests

8-C.1. Inside both treatments - Difference between r=0 and its theoretical value: not significant

Treatments 1 and 2 - Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 12
Number of non-zero differences: 12
Rank sum of positive differences: 42.0000
Rank sum of negative differences: 36.0000
Wilcoxon Test (exact): upper tail prob = 0.4112; lower tail prob = 0.5888.
Wilcoxon Test (table): NO sig (at 2.5% or better, one-tailed).
Wilcoxon Test (asymptotic): z-value = -0.235; Why did you test this?

8-C.2. Inside both treatments - Difference between r=1 and its theoretical value: significant

Treatments 1 and 2 - Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 12
Number of non-zero differences: 12
Rank sum of positive differences: 78.0000

Rank sum of negative differences: 0.0000
Wilcoxon Test (exact): upper tail prob = 0.0002; lower tail prob = 0.9998.
Wilcoxon Test (table): sig at 0.5% level, one-tailed.
Wilcoxon Test (asymptotic): z-value = -3.059; sig at 1% level, one-tailed.

8-C.3. Detect over-reaction inside a treatment for r=1

Sessions 1 to 6 - Two Sample Permutation Test
Evaluating 924 permutations
Larger: 887
Equal: 1
Smaller: 36
Upper tail Significance: 0.9610390
Lower tail Significance: 0.0400433

Sessions 7 to 12 - Two Sample Permutation Test
Evaluating 924 permutations
Larger: 0
Equal: 1
Smaller: 923
Upper tail Significance: 0.0010823
Lower tail Significance: 1.0000000

8-C.4. Inside a treatment - Difference between r=0.5 or r=0.25 and its theoretical value: significant

Sessions 1 to 6 - Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 7
Number of non-zero differences: 7
Rank sum of positive differences: 0.0000
Rank sum of negative differences: 28.0000
Wilcoxon Test (exact): upper tail prob = 0.9922; lower tail prob = 0.0078.
Wilcoxon Test (table): sig at 1% level, one-tailed.
Wilcoxon Test (asymptotic): z-value = -2.366; sig at 1% level, one-tailed.

Sessions 7 to 12 – Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 6
Number of non-zero differences: 6
Rank sum of positive differences: 0.0000
Rank sum of negative differences: 21.0000
Wilcoxon Test (exact): upper tail prob = 0.9846; lower tail prob = 0.0154.
Wilcoxon Test (table): sig at 2.5% level, one-tailed.
Wilcoxon Test (asymptotic): z-value = -2.201; sig at 5% level, one-tailed.

8-C.5. Detect over-reaction inside a treatment for r=0.5 or r=0.25

Sessions 1 to 6 - Two Sample Permutation Test
Evaluating 924 permutations.
Larger: 923
Equal: 1
Smaller: 0
Upper tail Significance: 1.0000000
Lower tail Significance: 0.0010823

Sessions 7 to 12 - Two Sample Permutation Test
Evaluating 924 permutations
Larger: 902
Equal: 1
Smaller: 21

Upper tail Significance: 0.9772727
Lower tail Significance: 0.0238095

8-C.6. Differences in samples

Inside Sessions 1 to 6

Difference between r=0 and r=1: significant
Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 6
Number of non-zero differences: 6
Rank sum of positive differences: 21.0000
Rank sum of negative differences: 0.0000
Wilcoxon Test (exact): upper tail prob = 0.0154; lower tail prob = 0.9846.
Wilcoxon Test (table): sig at 2.5% level, one-tailed.
Wilcoxon Test (asymptotic): z-value = -2.201; sig at 5% level, one-tailed.

Difference between r=0 and r=0.5: significant
Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 6
Number of non-zero differences: 6
Rank sum of positive differences: 21.0000
Rank sum of negative differences: 0.0000
Wilcoxon Test (exact): upper tail prob = 0.0154; lower tail prob = 0.9846.
Wilcoxon Test (table): sig at 2.5% level, one-tailed.
Wilcoxon Test (asymptotic): z-value = -2.201; sig at 5% level, one-tailed.

Inside Sessions 7 to 12

Difference between r=0 and r=1: significant

Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 6
Number of non-zero differences: 6
Rank sum of positive differences: 21.0000
Rank sum of negative differences: 0.0000
Wilcoxon Test (exact): upper tail prob = 0.0154; lower tail prob = 0.9846.
Wilcoxon Test (table): sig at 2.5% level, one-tailed.
Wilcoxon Test (asymptotic): z-value = -2.201; sig at 5% level, one-tailed.

Difference between r=0 and r=0.25: not significant
Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 6
Number of non-zero differences: 6
Rank sum of positive differences: 5.0000
Rank sum of negative differences: 16.0000
Wilcoxon Test (exact): upper tail prob = 0.8507; lower tail prob = 0.1493.
Wilcoxon Test (table): NO sig (at 2.5% or better, one-tailed).
Wilcoxon Test (asymptotic): z-value = -1.153; NO significance.

8-C.7. Tests related to the level of reasoning (degree 2)

Difference between the weight assigned to X when r=1 (both in treatments 1 and 2) and theoretical prediction for second level of reasoning (0.25): not significant
Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 12
Number of non-zero differences: 12

Rank sum of positive differences: 25.0000
Rank sum of negative differences: 53.0000
Wilcoxon Test (exact): upper tail prob = 0.8520; lower tail prob = 0.1480.
Wilcoxon Test (table): NO sig (at 2.5% or better, one-tailed).
Wilcoxon Test (asymptotic): z-value = -1.098; NO significance.

Difference between r=0.5 (Sessions 1 to 6) and theoretical prediction for second level of reasoning (0.375): not significant
Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 6
Number of non-zero differences: 6
Rank sum of positive differences: 19.0000
Rank sum of negative differences: 2.0000
Wilcoxon Test (exact): upper tail prob = 0.0462; lower tail prob = 0.9538.
Wilcoxon Test (table): NO sig (at 2.5% or better, one-tailed).
Wilcoxon Test (asymptotic): z-value = -1.782; sig at 5% level, one-tailed.

Difference between r=0.25 (Sessions 7 to 12) and theoretical prediction for second level of reasoning (0.437): significant
Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 6
Number of non-zero differences: 6
Rank sum of positive differences: 0.0000
Rank sum of negative differences: 21.0000
Wilcoxon Test (exact): upper tail prob = 0.9846; lower tail prob = 0.0154.
Wilcoxon Test (table): sig at 2.5% level, one-tailed.
Wilcoxon Test (asymptotic): z-value = -2.201; sig at 5% level, one-tailed.

8-C.8. Table and tests related to convergence

	Sessions 1 to 6				*Sessions 7 to 12*			
	r=1		*r=0.5*		*r=1*		*r=0.25*	
Sessions	Weight on *X* over first 5 situations	Weight on *X* over last 5 situations	Weight on *X* over first 15 situations	Weight on *X* over last 15 situations	Weight on *X* over first 5 situations	Weight on *X* over last 5 situations	Weight on *X* over first 15 situations	Weight on *X* over last 15 situations
1	0.259	0.124	0.404	0.356	0.352	0.195	0.497	0.495
2	0.329	0.222	0.456	0.399	0.334	0.233	0.483	0.465
3	0.349	0.402	0.428	0.448	0.417	0.327	0.509	0.510
4	0.446	0.247	0.440	0.458	0.319	0.194	0.487	0.490
5	0.286	0.110	0.309	0.367	0.241	0.190	0.483	0.485
6	0.347	0.258	0.416	0.445	0.331	0.230	0.496	0.473

Table A.2 – Weight assigned by participants to the private signal depending on treatment and rounds

Difference between the weight assigned to X when r=1 (both in treatments 1 and 2) over the first 5 situations and the weight assigned to X when r=1 (in both treatments) over the last 5 situations: significant

Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 12
Number of non-zero differences: 12
Rank sum of positive differences: 76.0000
Rank sum of negative differences: 2.0000
Wilcoxon Test (exact): upper tail prob = 0.0007; lower tail prob = 0.9993.
Wilcoxon Test (table): sig at 0.5% level, one-tailed.
Wilcoxon Test (asymptotic): z-value = -2.903; sig at 1% level, one-tailed.

Difference between the weight assigned to X when r=0.5 (in sessions 1 to 6) over the first 15 situations and the weight assigned to X when r=0.5 (in sessions 1 to 6) over the last 15 situations: not significant

Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 6
Number of non-zero differences: 6
Rank sum of positive differences: 9.0000
Rank sum of negative differences: 12.0000
Wilcoxon Test (exact): upper tail prob = 0.6087; lower tail prob = 0.3913.
Wilcoxon Test (table): NO sig (at 2.5% or better, one-tailed).

Difference between the weight assigned to X when r=0.25 (in sessions 7 to 12) over the first 15 situations and the weight assigned to X when r=0.25 (in sessions 7 to 12) over the last 15 situations: not significant

Wilcoxon Matched Pairs Signed Rank Test
Total number of observations : 6
Number of non-zero differences: 6
Rank sum of positive differences: 13.5000
Rank sum of negative differences: 7.5000
Wilcoxon Test (exact): upper tail prob = 0.2941; lower tail prob = 0.7059.
Wilcoxon Test (table): NO sig (at 2.5% or better, one-tailed).