Measuring Agents' Reaction to Private and Public Information in Games with Strategic Complementarities

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Abstract

In games with strategic complementarities, public information about the state of the world has a larger impact on equilibrium actions than private information of the same precision, because the former is more informative about the likely behavior of others. This may lead to welfare-reducing 'overreactions' to public signals. We present an experiment based on a game of Morris and Shin (2002), in which agents' optimal actions are a weighted average of the fundamental state and their expectations of other agents' actions. We measure the responses to public and private signals and find that, on average, subjects put a larger weight on the public signal. However, the weight is smaller than in equilibrium and closer to level-2 reasoning. Stated second order beliefs indicate that subjects underestimate the information contained in public signals about other players' beliefs, but this can account only for a part of the observed deviation of behavior from equilibrium. In the extreme case of a pure coordination game, subjects still use their private signals, preventing full coordination. Reconsidering the welfare effects of public and private information theoretically, we find for level-2 reasoning that increasing precision of public signals always raises expected welfare, while increasing precision of private signals may reduce expected welfare if coordination is socially desirable.

JEL-Code: C92, D82, D84.

Keywords: coordination games, strategic uncertainty, private information, public information, higher-order beliefs, levels of reasoning.

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1 – Introduction

Strategic complementarities are widespread in macroeconomics, financial crises, price competition, investment in network goods, and many other economic situations. In games with strategic complementarities, public and private information about an uncertain state of the world have distinct effects, because information that is shared by several agents is more informative about the likely beliefs and actions of others than private information. Since agents have an incentive to align their actions to the expected actions of others, equilibrium choices are affected more by public than by private signals of equal precision. This may lead markets to 'overreact' to public signals, raise price volatility, and a commitment to provide noisy public information may reduce expected welfare.[1] The aim of this paper is to evaluate experimentally the weights that subjects attach to private and public signals in games of strategic complementarities and the extent to which public information may be detrimental to welfare.

Our experiment builds upon Morris and Shin (2002), who present a stylized game with strategic complementarities for analyzing the welfare effects of public and private information. In their game, an agent's action is a weighted average of a public and a private signal. They emphasize the role of agents' overreaction to public information in games with strategic complementarities and show that public announcements affect agents' behavior more than justified by their informational content (what we call 'overreaction' to public information). In their welfare analysis, Morris and Shin concentrate on a case where those who predict the average opinion better gain at the expense of those whose prediction is worse. Here, coordination is a zero-sum game. Morris and Shin show that raising the precision of public signals may reduce expected welfare while raising the precision of private signals is always welfare improving. Svensson (2006) has argued that detrimental welfare effects of public information are unlikely, because they require that public information be of lower precision than private information, which conflicts with empirical findings.[2] Cornand and Heinemann (2008) argue that central banks should release information with the highest possible precision, but (under certain conditions) to just a fraction of all traders. They show that limiting the degree of publicity can be welfare improving even when public information is more precise than private information. Woodford (2005) has altered the game to consider socially desirable coordination, in which case raising the precision of public signals unambiguously raises expected welfare. Angeletos and Pavan (2004) and Hellwig (2005) have applied the game to contexts in which public information raises welfare while private information may have detrimental welfare effects. Myatt and Wallace (2008) extend the information structure to include correlated private signals. They show that in a Lucas-Phelps island economy all signals should be provided with the same correlation coefficient.

[1] The precise relations between strategic substitutability and the social value of information have been analyzed by Angeletos and Pavan (2007a,b).

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 it is that an imprecise public signal will reduce welfare. In the rational expectations equilibrium, the relative weight is determined by the signals' relative precision and agents' payoff functions. However, we know from Nagel (1995), Kübler and Weizsäcker (2004) and many other experimental papers that the equilibrium may be a poor predictor for actual behavior when it requires infinite levels of reasoning.

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 aims precisely at filling this gap by measuring and analyzing the actual multiplier effect of public signals – that is, how much public signals are taken into account compared to private ones – in a game with strategic complementarities.[3]

We present an experiment on a game that is characterized by both fundamental and strategic uncertainty. As in Morris and Shin (2002), agents have to choose actions that are close to a fundamental state but also close to each other. We test predictions of this approach by implementing two-player versions of this game with varying weights on fundamental and strategic uncertainty.[4]

In a benchmark case, subjects' payoffs depend only on how close their actions are to an unknown fundamental. Here, 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. Our empirical main result is that these weights are smaller than theoretically predicted. Observed weights can be explained by limited

[2] Romer and Romer (2000) have shown that errors in Federal Reserve forecasts of inflation are smaller than the errors in commercial inflation forecasts. Morris et al. (2006) show that for correlated signals, the result by Morris and Shin (2002) may hold even when public signals are more precise than private signals.

[3] Hertzberg, Liberti and Paravisini (2009) provide empirical evidence of a publicity multiplier among creditors to a common borrower. They show that, on average, making information public increases defaults, causes a permanent decline in debt, and results in firms' borrowing from fewer lenders. Ehrmann and Fratzscher (2007) provide an empirical test of three hypotheses arising from Morris and Shin (2002) using market responses to announcements of Federal Open Market Committee (FOMC) members. They argue that (in line with theory) public information regarding low precision reduces the predictability of subsequent FOMC decisions.

[4] Other recent experiments concerning the welfare effects of transparency have concentrated on games with multiple equilibria: Anctil *et al.* (2004) demonstrate that private signals with high precision are not sufficient to achieve coordination on an efficient equilibrium. Heinemann et al. (2004) compare perfect public and noisy private signals in coordination games and find small effects towards higher efficiency with perfect information. For other experiments dealing with public versus private information, see Forsythe et al. (1982), Plott and Sunder (1988), McKelvey and Ordeshook (1985), McKelvey and Page (1990) and Hanson (1996). They

levels of reasoning of order 2.[5] 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 may be viewed as an alternative explanation of behavior. However, non-Bayesian beliefs can explain only a part of the observed systematic deviation of behavior from equilibrium. Thus, we identify two sources of bounded rationality that should be combined to explain the observations.

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 strategy is an equilibrium. However, the limit of equilibria in games with a decreasing weight on fundamental uncertainty uniquely selects a strategy in which all agents follow the public signal and ignore all private information.[6] 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 coordination. Here, the provision of private information reduces efficiency.

Since observed behavior is consistent with level-2 reasoning, we reconsider the theoretical analysis of welfare effects, assuming that agents apply limited levels of reasoning. While in the original model of Morris and Shin (2002), a higher precision of public information may reduce expected welfare, we show, for level-2 reasoning, that public information is always welfare improving. In a variant of this model, put forth by Woodford (2005), coordination is socially valuable and both public and private information contribute to social welfare. Under level-2 reasoning, however, increasing the precision of private signals may reduce expected welfare. These findings establish our theoretical main result and partially turn around the results that have been obtained for fully rational agents. Finally, reconsidering the optimal degree of publicity, introduced by Cornand and Heinemann (2008), we find that this optimal degree is higher if agents apply level-2 reasoning than if they follow equilibrium strategies, but it may still be lower than full publicity. However, if public information is more precise than private information, for level-2 reasoning it is optimal to be fully transparent.

Overall, these results indicate that under strategic complementarities, (i) public information is less detrimental to welfare than predicted by equilibrium theory, (ii) private information can be welfare reducing and matters, even when it is intrinsically irrelevant for agents' choices.

The next section presents the model, sets the experimental design, and provides equilibrium predictions and hypotheses. Section 3 states the experimental results and provides measures for the relative weights that subjects put on private and common signals, relating them to theoretical predictions. In Section 4, we show that average behavior can be described by limited levels of

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

[5] We define level 1 as the optimal action of a player who neglects that public signals provide more information about other players' beliefs and level 2 as the best response to level 1.

[6] The same selection is made by applying the theory of focal points of Alós-Ferrer and Kuzmics (2008).

reasoning, but may be partially explained by a systematic mistake in the Bayesian update of higher-order beliefs. Section 5 reconsiders welfare effects of public and private information, accounting for limited levels of reasoning. Finally, section 6 concludes.

2 – The model, the experimental design, and theoretical predictions

We describe a 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 a two-player version of Morris and Shin (2002) adapted for conducting an experiment.[7]

2.1. The 2-players version of the game

The fundamental state of nature is given by θ and has a uniform distribution on the reals. Agent *i* chooses 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:

$$u_i(a,\theta) \equiv -(1-r)\,(a_i-\theta)^2 - r\,(a_i-a_j)^2\,,$$

where *j* is the partner of *i*. The first component is a quadratic loss in the distance between the underlying state θ and her/his action a_i. The second component is the coordination term, a quadratic loss in the distance between both players' actions. Finally, *r* is a constant, $0 \leq r \leq 1$, that indicates the relative weight attributed to the second component.

Agents face uncertainty concerning θ. They 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:

$$a_i = (1-r)E_i(\theta) + r\,E_i(a_j)\,,$$

where $E_i(\cdot)$ is the posterior expectation conditional on x_i and *y*. 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, which has been called multiplier effect of public information.

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

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 coordinating agents' actions. This model emphasizes the role of public information as a focal point for private actions. Strategic complementarities provide incentives to coordinate on the publicly announced state of the world and underemphasize 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 coordinate far away from what is fundamentally justified. Public information is a double-edged sword: it conveys valuable information, but the desire to coordinate may lead agents to condition their actions more strongly on public announcements than is optimal.

This raises doubts about the benefits of transparency, i.e. the provision of fully public information. Financial markets and macroeconomic environments are often characterized 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 destabilize markets by generating some overreaction. How much agents overreact to public information and, therefore, whether it is beneficial to provide fully public information in such macroeconomic contexts, is an empirical question. If the overreaction 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 overreaction is stronger than predicted by theory, transparency may be detrimental even if public signals are more precise than private information. To answer these questions, we propose an experiment that measures the actual multiplier effect of public information.

2.2.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 (standard sessions) and April 2009 (control sessions). Each session had 16 participants who were mainly students from Strasbourg Louis Pasteur University (most were students in economics, mathematics, biology and psychology). Subjects were seated in 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. Instructions and questionnaires are given in Appendix 8-A. The experiment started after all subjects had given the correct answers to these questions.

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[7] Besides requiring the number of players to be finite, we also changed the distributions of the signals from normal to uniform in order to have a simple distribution with bounded support for the experiment.

Each session consisted of four or five stages with a total of 50 periods. Each stage contained a different treatment. In each period, subjects were paired up randomly. 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 using a uniform distribution from 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 common (public) signal $y \in [\theta - 10, \theta + 10]$. Signals were drawn independently from these intervals using a uniform distribution.[8] The random process was explained in the instructions.

In Treatments A to D, each subject had to decide on an action a_i, conditional on her signals. Subjects could choose any number from [Y-20, Y+20]. Signals and actions were limited to one decimal 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 Treatment B, 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 Treatment C, the payoff function was $200 - (a_i - a_j)^2 - (a_i - \theta)^2$ while it was $400 - (a_i - a_j)^2 - 3(a_i - \theta)^2$ in Treatment D. Treatment C corresponds to the model of 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 Treatment E (5 periods) each subject was asked to state two expectations: one regarding the state and one regarding her partner's 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 $e_i(\cdot)$ denotes the stated expectation of subject i.

We conducted 12 standard sessions and 6 control sessions with a total of 288 subjects. In standard sessions, 6 sessions had Treatment C and 6 had Treatment D in the third stage; all standard sessions had Treatment A in the first stage, Treatment B in the second stage and Treatment E in the fourth stage. Treatment A aimed at familiarizing subjects with the game form and controlling for their Bayesian rationality. Treatment B is a pure coordination game from which subjects should learn that public information is more helpful in overcoming coordination problems than private information; Treatments C and D are the ones that we are most interested in. Treatment E aimed at eliciting higher-order beliefs directly. Table 1 gives an overview over sessions and treatments.

[8] Having a sufficiently large support of the prior distribution 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.

Sessions	*Stage 1*	*Stage 2*	*Stage 3*	*Stage 4*	*Stage 5*
1 to 6	*Treatment A* r=0 *(5 periods)*	*Treatment B* *r=1* *(10 periods)*	*Treatment C* r=0.5 (*30 periods*)	*Treatment E stating expectations (5 periods)*	–
7 to 12			*Treatment D* r=0.25 (*30 periods*)		
13 to 15		*Treatment E' stating expectations (5 periods)*	*Treatment B'* *r=1* *(10 periods)*	*Treatment C* r=0.5 (*20 periods*)	*Treatment B* *r=1* *(10 periods)*
16 to 18			*Treatment B''* *r=1* *(10 periods)*		

Table 1 – Treatment composition of the different sessions

Sessions 13 to 18 are control sessions, motivated by the results from standard sessions. Here, we first did the same Treatment A as in standard sessions. Then, we proceeded with treatment E', in which we asked for expectations as in Treatment E. The difference is that we used two separate payoff functions, $100-(E_i(\theta)-\theta)^2$ and $100-(E_i(E_j(\theta)-E_j(\theta))^2$ instead of one unified. We did these changes, because we were concerned that results for Treatment E in standard sessions were distorted by spillovers from the previous treatment and because the game might be easier to understand with separate payoff functions.[9] Treatments B' and B'' had the same payoff function as B, but subjects got only one signal. In Treatment B' they received just a common and no private signal, in Treatment B'' they received just private signals. Here, they could choose any number in $[x_i - 20, x_i + 20]$. We used these treatments to analyze subjects' ability to coordinate when they received only one signal and to compare the welfare effects of adding a second signal in Treatment B. In the control sessions, we divided the 16 participants into 2 matching groups of 8, thereby gathering two independent observations per session.[10]

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. Subjects knew from the start that they were playing the aforementioned treatments in this order. At the end of each session, the points earned were summed up and converted into euros. 100 points were converted to 25 cents in sessions 1 – 6, to 43 cents in sessions 7 – 12 and to 40 cents in sessions 13 – 18.

In all stages, it was possible to earn negative points. This actually occurred in about 3.4% of all decision situations in standard sessions. With one exception, realized losses were of a size that

[9] In addition, Treatments A and E' are both tests of Bayesian rationality, though with a different framing. Proceeding with these treatments one directly after another allows for a better comparison of their results.
[10] Subjects were randomly matched with other subjects from the same matching group only. Subjects were not informed about the size of matching groups.

could be counterbalanced by positive payoffs within three periods.[11] In total, no subject earned a negative payoff in any session. Payoffs ranged from € 16 to € 31. The average payoff was about € 24. Sessions lasted for around 90 minutes.

2.3. Equilibrium predictions

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 bounded 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 both subjects coordinate on the same strategy. The public signal, however, provides a focal point,[12] so that we expected subjects to coordinate on actions $a_i = y$. The same is true for treatment B'. In treatment B'', subjects had only private signals. Here, the unique equilibrium is to choose an action equal to the private signal, because actions are restricted to the interval [x_i – 20, x_i + 20].

Treatments C (r=0.5) and D (r=0.25) correspond to 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.

Due to the bounded support of θ, the equilibrium deviates from this linear combination into the direction of the center 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 θ = 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. In the data, we find systematic effects near the borders of the distribution for

[11] The exception is subject 7 in session 7, 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 who earned a negative payoff in stage 1, one subject in stage 2, and 5 subjects in stage 4 of standard sessions. In sessions 16-18, 15 of 48 subjects earned negative payoffs in treatment B''. Here, it took up to 8 periods to compensate these losses.

[12] A formalization of focal points is provided by Alós-Ferrer and Kuzmics (2008).

signals up to 60 and above 440. We checked that restricting data analysis to situations with $70 < \theta < 430$ does not alter our results.

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

r	0	0.25	0.5	$\to 1$
γ^*	0.50	0.43	0.33	$\to 0$

Table 2 – Theoretical values of γ^* as a function of r

3 – Experimental results

We use the following structure in analyzing data. We analyze rationality of choices. We give summary statistics for the weights that subjects attach to the private signal. We investigate (separately for each treatment) whether observed weights on the private signal are positive, smaller than 0.5, and whether they deviate systematically from the theoretical prediction. We also test for the convergence of behavior over time by splitting the data from the first and second halves of treatments. Comparing observed weights, we test comparative static predictions arising from theory.

3.1. Some considerations about rationality

Bayesian rationality requires choosing an action that is a weighted average of the two signals. Hence, actions should be contained in $I_1 = [\min(y, x_i), \max(y, x_i)]$. However, we observe that many subjects chose actions outside this interval. Another reference point for rational behavior is given by the support of the conditional distribution of fundamental states. From her signals, subject *i* can deduce[13] that the true state of the world is contained in the interval $I_2 = [\max(y, x_i) - 10, \min(y, x_i) + 10]$.

We observe that the proportion of choices outside these intervals becomes 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, $I_3 = I_1 \cup I_2$.

Result 1: In treatments A, B, C, and D, at least 96% of choices are contained in I_3. The proportion of choices inside I_1 is 86%.

In Tables 3a and b, we display the percentage of all choices within these intervals. Counting the number of choices that are closer to the common signal, closer to the private, or in the middle provides us a crude first impression of whether subjects put a larger weight on the common signal and how dispersed the distribution of relative weights is.[14]

[13] This deduction might actually have been hinted at in the comprehension questionnaire (presented in Appendix 8.A.2.).

[14] To break ties in counting: if both signals and the action coincide, we count the choice as "Middle". If $|y - x_i| = 0.1$, and $a_i = x_i$ $[a_i = y]$, we count the choice as "equal to x_i" ["equal to y"].

Treatment	A (r=0)	D (r=0.25)	C (r=0.5)	B (r=1)
Inside $I_1 = [\min(y,x_i), \max(y,x_i)]$	75%	85%	86%	87%
Choice = y	1%	1%	7%	35%
Closer to y	30%	34%	44%	27%
Middle (+/-0.05)	13%	18%	11%	8%
Closer to x_i	31%	31%	24%	17%
Choice = x_i	1%	1%	1%	1%
Outside I_1 beyond y	13%	8%	8%	7%
Outside I_1 beyond x_i	12%	8%	6%	6%
Inside $I_2 = [\max(Y,X)-10; \min(y,X)+10]$	94%	97%	96%	88%
Inside $I_3 = I_1 \cup I_2$	97%	99%	99%	98%

Table 3a – Crude classification of choices, sessions 1-12

Treatment	A (r=0)	C (r=0.5)	B (r=1)
Inside $I_1 = [\min(y,x_i), \max(y,x_i)]$	75%	86%	94%
Choice = y	1%	4%	61%
Closer to y	28%	35%	15%
Middle (+/-0.05)	15%	18%	6%
Closer to x_i	31%	28%	12%
Choice = x_i	0%	1%	1%
Outside I_1 beyond y	11%	7%	3%
Outside I_1 beyond x_i	14%	7%	3%
Inside $I_2 = [\max(Y,X)-10; \min(y,X)+10]$	93%	97%	84%
Inside $I_3 = I_1 \cup I_2$	96%	99%	99.6%

Table 3b – Crude classification of choices, sessions 13-18

As r is increased, the proportion of choices closer or equal to the common signal y increases. Comparing control sessions with standard sessions, the only remarkable difference is that in Treatment B of Sessions 13-18 there are many more choices equal to the public signal than in Sessions 1-12. We attribute this to the different order of treatments. Experience from other treatments seems to make it easier for subjects to coordinate on the public signal when coordination is the only motive for action.

3.2. Estimated weights on the private signal

We estimate the relative weights of the two signals on subjects' decisions by fitting linear regressions to data. We do this (i) for each subject and treatment separately and (ii) joining the data from all subjects within one matching group[15]. Treatments with 10 or more rounds are split up in two half treatments, in order to detect strategy changes. The regression equation for (i) is

$$a_{it} - y_t = c_i + \gamma_i (x_{it} - y_t) + u_{it} ,$$

where γ_i is the subject-specific relative weight on private information. The constant c_i stands for an individual's bias towards higher numbers. It should be zero, but in many cases we can reject the hypothesis that $c_i = 0$. Therefore, we did not impose this as a restriction. Implicitly, our regression imposes a restriction that the absolute coefficients on private and common signals add up to 1. In regressions without this restriction, we checked that violations of this restriction are not significant.

[15] For Sessions 1 – 12, we use the term "matching group" meaning all participants of the same session.

Individual weights are rather dispersed in all treatments. We used them to test whether there are significant differences between matching groups or between the first and second half of a treatment. These results are reported in an online appendix.[16]

In Treatments B through E, individual weights are not independent, because subjects may respond to the behavior of other group members. Therefore, our main analyses are based on group specific weights. We estimate these, joining the data from all subjects within a matching group. Here, the regression equation is

$$a_{it} - y_t = c + \gamma(x_{it} - y_t) + u_{it},$$

where γ is the group-specific relative weight on private information.

Table 4 displays group specific weights on the private signal for all groups and treatments and compares them with theoretical weights. For treatments that were played at least 10 rounds, we spilt the data from the first and second halves so that we could analyze whether there was convergence towards equilibrium.

Session, group	Tr A	Tr B, 1st half	Tr B, 2nd half	Tr C, 1st half	Tr C, 2nd half	Tr D, 1st half	Tr D, 2nd half
1	0.522	0.266	0.158	0.419	0.408		
2	0.506	0.369	0.219	0.452	0.463		
3	0.570	0.318	0.390	0.453	0.447		
4	0.475	0.380	0.344	0.416	0.475		
5	0.453	0.328	0.098	0.340	0.393		
6	0.516	0.349	0.200	0.409	0.453		
7	0.435	0.353	0.185			0.485	0.492
8	0.472	0.346	0.209			0.498	0.466
9	0.579	0.398	0.363			0.521	0.500
10	0.496	0.339	0.135			0.480	0.460
11	0.559	0.286	0.195			0.454	0.475
12	0.494	0.257	0.210			0.477	0.473
Average (1-12)	0.506	0.332	0.226	0.415	0.440	0.486	0.478
St.dev. (1-12)	0.046	0.044	0.092	0.041	0.032	0.022	0.015
13, group 1	0.455	0.181	0.107	0.502	0.484		
13, group 2	0.583	0.149	0.201	0.367	0.338		
14, group 1	0.524	0.008	0.000	0.431	0.473		
14, group 2	0.583	0.255	0.278	0.551	0.536		
15, group 1	0.446	0.234	0.229	0.476	0.422		
15, group 2	0.534	0.134	0.039	0.495	0.540		
16, group 1	0.527	0.130	0.105	0.439	0.454		
16, group 2	0.500	0.482	0.421	0.530	0.485		
17, group 1	0.506	0.098	0.150	0.459	0.455		
17, group 2	0.570	0.184	0.067	0.486	0.459		
18, group 1	0.582	0.132	0.160	0.489	0.440		
18, group 2	0.489	0.385	0.359	0.519	0.487		
Average (13-18)	0.525	0.198	0.176	0.479	0.464		
St.dev. (13-18)	0.048	0.129	0.128	0.050	0.053		
Equilibrium weight	0.5	0	0	0.333	0.333	0.429	0.429

Table 4 – Group specific weights on the private signal

[16] Data and programs are available at http://anna.ww.tu-berlin.de/~makro/Heinemann/download/CH/ch-3.zip.

Data for Sessions 1-12 indicate that the relative weight on the private signal was around 0.5 for Treatment A and tends to be decreasing in *r* (from Treatment D over C to B). For Treatments B, C, and D, all group specific weights are larger than in equilibrium. Comparing first and second half, there seems to be a convergence towards equilibrium in Treatment B, but not in C or D.

These impressions are widely confirmed by nonparametric tests, reported below, where we use a significance level of 5% throughout. Specifically, we test whether estimated weights differ from 0.5, from the respective equilibrium values, between first and second half of a treatment, and between standard and control sessions. We also test monotony of the weights in *r*. Unless otherwise noted, all tests are based on counting group averages as independent observations. We test standard sessions (1-12) and control sessions (13-18) separately, because they are not entirely comparable. In treatments where we split the first and second halves, we apply test data from both halves separately.

Result 2: For r = 0, subjects put an equal weight on both signals consistent with Bayesian rationality.

In Treatment A, there is no significant difference between group specific estimated weights and 0.5. Two-tailed Wilcoxon matched pairs signed rank tests yield p-values of 0.73 (Sessions 1-12) and 0.12 (Sessions 13-18). A Mann-Whitney test does not reject the hypothesis that weights in Sessions 1-12 are equal to those in Sessions 13-18 (p-value 0.24). Since individual decisions are independent in this treatment, we also performed tests on individual weights from Regression (i). The hypothesis of $\gamma_i = 0.5$ cannot be rejected (p-values are 0.93 for Sessions 1-12 and 0.45 for Sessions 13-18). Neither can the hypothesis that individual weights in the two groups of sessions are equal be rejected (p-value 0.53).

Our main empirical result is that for *r* > 0, subjects put a lower weight on public signals than in equilibrium.

Result 3: For r = 0.5 and r = 0.25, subjects tend to put larger weights on public than on private signals, but the difference is smaller than theoretically predicted. There is no trend towards equilibrium.

Testing the hypotheses that estimated group-specific weights on the private signal are smaller than 0.5 and larger than equilibrium values (0.333 in Treatment C and 0.429 in Treatment D), one-tailed Wilcoxon matched pairs signed rank tests yield the p-values, reported in Table 5. While most tests confirm the hypotheses at 5%, we can reject that weights are smaller than 0.5 for the first half of Treatment D and for the first half of Treatment C in Sessions 13-18. For Treatment C, estimated weights are higher in Sessions 13-18 than in Sessions 1-12 (significant at 1% for the first half, but insignificant (p=7.7%) for the second half, using two-tailed Mann-Whitney tests). We attribute this to an order effect stemming from Treatment B that was conducted before C in Sessions 1-12 and after C in Sessions 13-18.

Hypothesis	Sessions 1-6, Treatment C, first half	Sessions 1-6, Treatment C, second half	Sessions 7-12, Treatment D, first half	Sessions 7-12, Treatment D, second half	Sessions 13-18, C, first half	Sessions 13-18, C, second half
$\gamma < 0.5$	0.0154	0.0154	0.1061	0.0303	0.1004	0.0134
$\gamma > 0.429$			0.0154	0.0154		
$\gamma > 0.333$	0.0154	0.0154			0.0002	0.0002

Table 5 – P-values of one-tailed Wilcoxon matched pairs signed rank tests.

Comparing the weights assigned to private signals in the first and second half of each treatment, we find no significant differences. Using the two-tailed Wilcoxon matched pairs test, p-values range between 15 and 16%.

Thus, we conclude that subjects tend to put larger weights on public than on private information, but the difference is smaller than theoretically predicted and there is no convergence towards equilibrium. These results seem to be corroborated by subjects' written comments in the post-experimental questionnaire[17]. 38% of subjects explicitly wrote (without being directly asked) that Y is more informative than x_i on the other participant's decision.

Result 4: For r = 1, subjects assign larger weights to public than to private information. In standard sessions, they lean towards coordinating on the public signal, but do not achieve full coordination during the course of the treatment. In control sessions, there seems to be no trend towards improved coordination.

In Treatment B, all estimated weights are below 0.5 and, with one exception (Session 14, group 1, second half), positive. Hence, the Wilcoxon matched pairs signed rank test yields p-values below 1%. Testing whether the weights assigned to private signals in the last 5 rounds were lower than in the first half, the one-tailed Wilcoxon matched pairs test yields p-values of 0.2% for Sessions 1-12 and 12.8% for Sessions 13-18. Thus, we cannot reject the hypothesis of convergence towards equilibrium for standard sessions, but we can reject it for control sessions. This rejection becomes even more pronounced if we use a sign test instead (p-value 38%). Comparing behavior between the two groups of sessions, we find that in the first 5 rounds, weights in Sessions 13-18 are significantly lower than in Sessions 1-12 (two-tailed Mann-Whitney, p<1%). In the second half, there is no significant difference (p=29%). We attribute this to an order effect, explained below.

We conclude that in the extreme case of a pure coordination game, subjects condition their choices on their private signals, which prevents full coordination. Providing private information matters and may be welfare reducing. One of the reasons for control treatments was to include treatments with only one signal, in order to test the welfare reducing effect of private signals directly by comparing payoffs. These results are reported in Section 3.3 below.

[17] The post-experimental questionnaire is presented in Appendix 8.A.3.

Result 5: Over all sessions, the weight assigned to the private signal tends to decrease in r (as predicted by theory).

A one-tailed Wilcoxon matched pairs signed rank test cannot reject the hypothesis that γ is smaller in treatments with higher *r*, if we compare Treatments A with C or C with B. P-values are always below 4%. This holds for standard and control sessions. Comparing C with D, a one-tailed Mann-Whitney test yields p-values below 3%. However, the one-tailed Wilcoxon matched pairs test rejects the hypothesis that γ is smaller in Treatment D than in Treatment A (p>10%). This means that the coordination motive when *r*=0.25 is not sufficiently strong for subjects to have a significantly different behavior than when there is no coordination motive at all (*i.e.* when *r*=0).

3.3. Payoff effects of providing additional information in a pure coordination game

In a pure coordination game as with *r*=1, private signals should be neglected if actions can be conditioned on public signals. In equilibrium, additional private signals should not affect welfare. One purpose of Sessions 13-18 was to provide a direct answer to the question of whether providing additional private signals reduces welfare. Table 6 compares average payoffs in Treatments B, B', and B''. Recall that each pair of subjects received a common and private signals in Treatment B, only a common signal in B', and only private signals in B''.

Session, Group	*Tr. B'*	*Tr. B''*	*Tr. B*	*Session*	*Tr. B*
13, Group 1	97.53		89.53	1	86.22
Group 2	99.60		85.08	2	78.84
14, Group 1	99.82		99.90	3	71.26
Group 2	93.06		75.46	4	73.01
15, Group 1	92.49		88.99	5	82.81
Group 2	72.25		89.98	6	76.59
Average (13-15)	92.46		88.16		
16, Group 1		- 3.56	90.06	7	65.47
Group 2		25.19	82.09	8	81.68
17, Group 1		24.23	95.06	9	70.07
Group 2		- 1.60	94.53	10	68.59
18, Group 1		8.73	89.33	11	78.22
Group 2		7.47	78.94	12	88.29
Average (16-18)		10.08	88.33	Average (1-12)	76.75

Table 6 – Average payoffs (treatments B, B' and B'')

In sessions 13-15, four groups achieved a lower payoff in Treatment B than in B', although Treatment B followed B'. Hence, adding a private signal to the information structure reduced their average payoffs. Two groups (Session 14, Group 1, and Session 15, Group 2) improved their payoffs. The average over all groups is lower when both signals are provided, but the difference is not significant (p=0.56). This indicates that adding private information does not reduce average payoffs in pure coordination games. On the other hand, in Sessions 1-12, average payoffs in Treatment B are significantly lower than in Treatments B and B' of Sessions 13-15 (Mann-Whitney, p<1%).

We attribute these diverse findings to the order of treatments. Treatment B was the second treatment in Sessions 1-12, while it was the last in Sessions 13-18. In Sessions 13-18, it is possible that the high payoff in B is due to learning from previous treatments. Comparing B from Sessions 1-12 with B' from Sessions 13-15 might give a better impression of how additional private information affects behavior for a given state of experience with this kind of games. However, we have to admit that we cannot provide a definite answer to the question of whether theoretically irrelevant private information impedes coordination. An experiment by Fehr et al. (2009) is better suited to answer this question and finds convincing evidence for welfare reducing effects of intrinsically irrelevant private signals in a pure coordination game.[18]

From Table 6, it is obvious for Sessions 16-18 that adding a public signal increased average payoffs compared to treatment B''.[19]

3.4. Order effects

The data indicate order effects. During the course of the experiment, subjects seem to learn that public signals are more important than private ones for estimating the likely action of other participants. In particular:

1. For Treatment C, the estimated weights for the private signal are higher in Sessions 13-18 than in Sessions 1-12. The difference is significant (p=1%) for the fist half, but insignificant (p=7.7%) for the second half, using two-tailed Mann-Whitney tests).
2. When Treatment B is conducted in an early stage (as in Sessions 1-12), the weight on public information significantly increases in the second half of the treatment (Result 4), which is not the case in Sessions 13-18 with Treatment B the last treatment played.
3. When Treatment B is conducted in a late stage (as in Session 13-18), subjects assign a larger weight to public signals from the start (first half: p<1%, second half: p=0.29, two-tailed Mann-Whitney) and coordinate better: average payoffs are higher than for Treatment B in Sessions 1-12 (p<1%).
4. Belief elicitation in Treatments E and E' (see Section 4 below): When asked for their expectation of the fundamental state θ directly after Treatment A (Sessions 13-18), subjects

[18] In a context of asymmetric information, Camerer et al. (1989) show that more information is not always better because agents are unable to ignore private information even when it is in their interest to do so.

[19] When receiving only a private signal in treatment B'', some subjects apparently tried to find a focal point that the experimental design did not allow for. During this treatment, about one participant per session asked why he could not enter either 50 or 0. He or she was told that these numbers were outside the range of admissible choices. We restricted the choice set to prevent large losses. In Sessions 13 to 15 this question never occurred, probably because subjects could coordinate on the public signal. Having allowed participants to choose from a fixed range of numbers might have helped them to coordinate on a focal point, disregard the private signal, and increase their payoffs. In a related experiment, Fehr et al. (2009) show that subjects tend to neglect imprecise private signals in a pure coordination game with a fixed choice set that includes prominent numbers. However, they also find that adding private signals impedes coordination.

assign equal weights to both signals (as in Treatment A). When asked after all other treatments (Sessions 1-12), they assign a larger weight to the public signal. The difference between E and E' is significant with p=6% for group data and 2.4% for individual data (two-tailed Mann-Whitney). The difference between the weights in Treatments E and A is significant if we compare group data (p=3.4%, two-tailed Wilcoxon matched pairs), but insignificant if individual data are used (p=0.24).

5. In Treatments E and E', the weight on the public signal for higher-order beliefs is significantly larger when beliefs are elicited at the end of all other treatments (Sessions 1-12) as compared to when they are elicited directly after Treatment A (Sessions 13-18), for both, group and individual data (p<1%, two-tailed Mann-Whitney).
6. Treatments B' and B'' do not seem to have an effect on subsequent behavior. Two-tailed Mann-Whitney tests cannot reject the hypotheses that weights in Treatments C or B are the same in Sessions 13-15 as in Sessions 16-18 (p-values are above 0.5).

4 – Two possible explanations for observed behavior

There are at least two possible explanations of why subjects do not put as much weight on the common signal as theory predicts. It may be that subjects apply limited levels of reasoning in the sense of Nagel (1995). However, observed behavior may also result from subjects' underestimating the importance of the public signal in predicting other agents' information.[20]

4.1. Limited levels of reasoning

Suppose that a player *j* attaches weight γ_k to her private signal. The best response to such behavior 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 the expected private signal of the other player 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

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

[20] A third possible explanation that cannot be dealt with in this paper was pointed out by a discussant: subjects might pay less attention to the actions of others, because they have less information about them. Strategic uncertainty turns beliefs about others' behavior into an ambiguous guess as opposed to estimating the fundamental state for which probabilistic information is available.

Table 7 summarizes the resulting weights on the private signal for increasing levels of reasoning for different values of *r*. Here, we define level 1 as the behavior of a player who wholly ignores the other player's action, so that her/his choice is determined solely by her/his expectation of θ.

Value of r	*Level 1*	*Level 2*	*Level 3*	*Level 4*	...	*Equilibrium weight (infinite levels 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 7 – Theoretical weight on x_i depending on *r* and on the level of reasoning

Result 6: For interior values of r, most estimated group weights are in accord with a level of reasoning of less than degree 2.

Comparing these with the detected weights in Table 4, shows that in Treatments C and D, average group weights are higher than the weights from level-2 reasoning with only two exceptions for Treatment C (Session 5, first half, and Session 13, Group 2). The hypothesis that group weights equal those from level-2 reasoning is rejected by two-tailed Wilcoxon matched pairs signed rank test at p-values below 4%, except for Sessions 1-6, Treatment C, first half, where p=9.4%, due to the lower weight in Session 5 (for this group of data, even level 3-reasoning cannot be rejected with p=6.25%).

In Treatment B, the estimated group weights tend to converge towards equilibrium, even though convergence is not settled within ten rounds. In control sessions (13-18) most groups achieve weights that are below level-2 reasoning, 5 groups go beyond Level 3, and one of them fully coordinates on the public signal (Session 14, Group 1). Two-tailed Wilcoxon matched pairs tests reject the hypothesis that weights in Treatment B are equal to level-2 reasoning for Sessions 1-12, first half (p<1%) in favor of higher weights. In the second half and in Sessions 13-18 the level-2 hypothesis cannot be rejected (p-values 0.42, 0.18, and 0.09). Level-3 reasoning can be rejected in Sessions 1-12 (p<1%), but not in Sessions 13-18 (p=0.052 and 0.26), where we need to go to level 4 to find a significant difference.

It is known in experimental economics that common information does not necessarily lead to a situation of common knowledge (Smith, 1991). For the guessing game and for cascade games, Stahl and Wilson (1994), Nagel (1995), and Kübler and Weizsäcker (2004) have shown that subjects' behavior is more consistent with finite levels of beliefs over beliefs than with theoretical predictions from common knowledge. Our results extend this finding to coordination games à la Morris and Shin (2002).[21]

[21] Shapiro, Shi and Zillante (2009) analyze the predictive power of level-*k* reasoning in a game that combines features of Morris and Shin (2002) with Nagel (1995). They try to identify whether individual strategies are consistent with level-*k* reasoning. They argue that the predictive power of level-*k* reasoning is positively related to the strength of the coordination motive and to the symmetry of information.

4.2. Belief elicitation

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

In the fourth stage (Treatment E), we provided public and private signals, but instead of asking for an action, 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. Thus, we directly elicit first-order and second-order beliefs. Theoretically, agents should put a weight of 0.5 on x_i in estimating θ and a weight of 0.25 in estimating the other's estimation of θ. This does not require any assumptions about behavior of others in coordination games. It follows directly from Bayesian rationality. To see this, suppose that the stated belief of subject j about θ is $e_j(\theta) = \alpha_j x_j + (1-\alpha_j) y$ and that α_j is uncorrelated with any of the signals. Then,

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

If subject i expects j to have rational first-order beliefs, then $E_i(\alpha_j) = 0.5$ and Bayesian rationality requires that $E_i(e_j(\theta)) = 0.25\, x_i + 0.75\, y$.

Data from Treatment E, summarized in Table 8, 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 estimation of θ. As in Section 3.2, we estimate these weights by fitting a linear regression to the data separately for each group. Here, we also use regressions to estimate individuals' weights.

Sessions	*Weight in estimating θ*	*Weight in estimating the partner's estimation of θ*
1	0.394	0.228
2	0.520	0.342
3	0.481	0.376
4	0.418	0.170
5	0.424	0.210
6	0.482	0.159
7	0.493	0.335
8	0.429	0.301
9	0.532	0.379
10	0.466	0.272
11	0.500	0.375
12	0.501	0.217
Average	0.470	0.280
St.dev.	0.044	0.082

Table 8 – Group specific weights on the private signal in treatment E (sessions 1-12)

For the Bayesian update of second-order beliefs we assumed that $E_i(\alpha_j) = 0.5$. In fact, data reveal that on average $\alpha_i \approx 0.47$. If subjects actually guessed α_i correctly, the optimal weight on the private signal in higher-order beliefs would be only 0.235. Thus, the higher observed weights cannot be explained by subjects' responding to distorted first-order beliefs.

Two-tailed Wilcoxon tests on estimated group-specific weights can reject neither the hypotheses that the weight in estimating θ deviates from 0.5 at the 5% level (p=0.077), nor that the weight in estimating the other player's estimation equals 0.25 (p=0.20).

Since estimating θ does not depend on others' choices, we also test individual weights. When we estimate weights separately for each subject, the average weight on x_i (over all subjects in standard sessions) in estimating θ is 0.479, while the average weight on x_i in forming higher-order beliefs is 0.287. We can reject the hypotheses that the weight in estimating θ equals 0.5 (p=0.013) and that the weight in estimating the other player's estimation of the state equals 0.25 (p<1%). 151 out of 192 subjects attribute a weight higher than 0.25 to the private signal in this task. Individuals' higher-order expectations are not independent, but since expectations of θ are generally biased towards the public signal, any adjustment in higher-order beliefs should go in the same direction, which conflicts with our observations. Although Bayesian rationality requires that the second weight be half of the first, it is in fact 59.9% of the first. Therefore, tests on group data could not convince us that subjects follow Bayesian rationality in higher-order beliefs.

It is surprising, though, that subjects underused private signals in forming their expectation of θ, especially in light of the results from Treatment A, where subjects used (on average) a weight of 0.506. The difference in coefficients from Treatments A and E is significant at 3.4%. This may be caused by 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 for estimating θ in stage 4. This, however, should also hold for forming expectations about others' expectations. Thus, without an order effect, we should see a larger weight on the private signal in the formation of higher-order beliefs.

Testing this hypothesis was the motivation for conducting Treatment E' in the control sessions. Here, we elicited beliefs directly after Treatment A and before all those treatments, in which common signals are theoretically more important than private ones. Table 9 summarizes the results from Treatment E'.

Session, group	*Weight in estimating θ*	*Weight in estimating the partner's estimation of θ*
13, Group 1	0.524	0.475
Group 2	0.417	0.389
14, Group 1	0.463	0.358
Group 2	0.538	0.550
15, Group 1	0.543	0.459
Group 2	0.447	0.317
16, Group 1	0.459	0.388
Group 2	0.555	0.499
17, Group1	0.522	0.397
Group 2	0.603	0.399
17, Group1	0.511	0.317
Group 2	0.566	0.584
Average	0.512	0.428
St.dev	0.055	0.086

Table 9 – Group specific weights on the private signal in Treatment E' (sessions 13-18)

The result is striking: the bias in estimating the state is almost absent now (p=0.12), and the average weight on the private signal in estimating the other subject's estimation of the state is higher than 0.25 in all groups (p<1%). Average individual weights were 0.52 in estimating θ and 0.437 in second-order beliefs. There is no significant difference between individual weights and 0.5 (p=0.37), while the difference in the weight for higher-order beliefs from its Bayesian value of 0.25 is significant at the 0.1% level.

Having ruled out order effects and best responses to others' deviations from complete rationality as possible explanations leaves us with the impression that subjects underestimate how informative the public signal is for predicting others' expectations. This can be viewed as a systematic error in Bayesian updating and provides an alternative explanation for results from the other treatments. Non-Bayesian higher-order beliefs may also be responsible for observed deviations from equilibrium in other experiments.

4.3. Model of bounded rationality based on non-Bayesian higher-order beliefs

Result 8: Systematic errors in higher-order beliefs are too low to explain the observed deviations from equilibrium in treatments with an interior r.

Can the underestimation of the informational value of common signals for predicting others' beliefs explain observed behavior in the games with $0 < r < 1$? To answer this question, we build a small model of boundedly rational behavior in which we assume infinite levels of reasoning but a systematic error in forming higher-order beliefs. More precisely, assume:

1. Subjects respond optimally to their expectations about the state and about their partner's action, i.e.,

$$a_i = (1-r)\,e_i(\theta) + r\,e_i(a_j), \quad j \neq i.$$

Furthermore, this behavioral assumption is common knowledge among players, which amounts to assuming infinite levels of reasoning.

2. Subjects use private and common signals correctly to forecast the state of the world, *i.e.*

$$e_i(\theta) = E_i(\theta).$$

3. Subjects make a systematic error in forming higher-order beliefs, such that

$$e_i(e_j(\theta)) = \lambda_i x_i + (1-\lambda_i) y,$$

$$e_i(e_j(e_i(\theta))) = \lambda_i^{3/2} x_i + (1-\lambda_i^{3/2}) y,$$

$$e_i(e_j(e_i(e_j(\theta)))) = \lambda_i^2 x_i + (1-\lambda_i^2) y,$$

and so on, where λ_i is a weight that we may take from Treatment E.

To understand the justification for these formulas, note that a correct Bayesian update yields $E_i(\theta) = \alpha x_i + (1-\alpha) y$ with $\alpha = 0.5$. Correct higher-order expectations are then given by $E_i(E_j(\theta)) = \alpha^2 x_i + (1-\alpha^2) y$, $E_i(E_j(E_i(\theta))) = \alpha^3 x_i + (1-\alpha^3) y$, and so on. The estimated λ for second-order expectations in Treatment E replaces α^2 in the Bayesian formula, so that the weight in third-order beliefs should be $\lambda^{3/2}$ and so on.

Combining these assumptions, we get

$$\begin{aligned}
a_i &= (1-r) e_i(\theta) + r\, e_i(a_j) \\
&= (1-r) E_i(\theta) + r\, e_i((1-r) e_j(\theta) + r\, e_j(a_i)) \\
&= (1-r) E_i(\theta) + (1-r)\left[r\left(\lambda x_i + (1-\lambda) y\right) + r^2\left(\lambda^{3/2} x_i + (1-\lambda^{3/2}) y\right) + r^3\left(\lambda^2 x_i + (1-\lambda^2) y\right) + r^4\left(\lambda^{5/2} x_i + (1-\lambda^{5/2}) y\right) + \ldots \right] \\
&= (1-r)\left(\frac{1}{2} + r\lambda \sum_{i=0}^{\infty} (r\sqrt{\lambda})^i\right) x_i + \left[1 - (1-r)\left(\frac{1}{2} + r\lambda \sum_{i=0}^{\infty} (r\sqrt{\lambda})^i\right)\right] y \\
&= (1-r)\left(\frac{1}{2} + \frac{r\lambda}{1 - r\sqrt{\lambda}}\right) x_i + \left[1 - (1-r)\left(\frac{1}{2} + \frac{r\lambda}{1 - r\sqrt{\lambda}}\right)\right] y.
\end{aligned}$$

For each group, we use the observed average weights for second-order beliefs in Treatment E to calculate the weight that this group should attach on the private signal in Treatments C or D. Table 10 compares the results of this calculation with the estimated weights on x_i in the second half of Treatments C or D. With two exceptions (session 13, group 2 and session 15, group 1), the estimated weights on the private signal are higher than those that would be explained by our model of Non-Bayesian higher-order beliefs. This result stands if we use individual data instead of group averages, where the differences between estimated weights in Treatments C or D and weights calculated from our model with estimated weights for higher-order beliefs as input is significant at the 0.1% level.

Session	Calculated weight on private signal using estimated error in higher-order beliefs	Estimated weight on private signal (C/D, 2nd half)	Session, group	Calculated weight on private signal using estimated error in higher-order beliefs	Estimated weight on private signal (C, 2nd half)
1	0.325	0.408	13, group 1	0.431	0.484
2	0.371	0.463	13, group 2	0.391	0.338
3	0.386	0.447	14, group 1	0.378	0.473
4	0.303	0.475	14, group 2	0.469	0.536
5	0.318	0.393	15, group 1	0.424	0.422
6	0.300	0.453	15, group 2	0.360	0.540
7	0.448	0.492	16, group 1	0.391	0.454
8	0.440	0.466	16, group 2	0.443	0.485
9	0.459	0.500	17, group 1	0.395	0.455
10	0.434	0.460	17, group 2	0.396	0.459
11	0.458	0.475	18, group 1	0.360	0.440
12	0.421	0.473	18, group 2	0.486	0.487

Table 10 – Comparing weights from a model of Non-Bayesian higher-order beliefs with observations

Thus, we conclude that errors in forming higher-order beliefs are in general not sufficiently strong to explain observed behavior in the game. There must be another form of irrationality in addition to non-Bayesian beliefs. Hence, we cannot reject the hypothesis of limited levels of reasoning. Combining levels of reasoning with non-Bayesian beliefs as described by this model yields higher levels of reasoning.[22] However, for interior values of *r*, aggregate behavior is in most cases consistent with levels of reasoning not higher than degree 2.[23] Therefore, we now proceed with our welfare analysis for limited levels of reasoning.

5 – Welfare analysis for limited levels of reasoning

There is currently a debate regarding the welfare effects of public disclosures. In the model by Morris and Shin (2002), 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, which 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 (2006), Lorenzoni (2009), and Woodford (2005) provide models similar to Morris and Shin (2002), but where coordination is

[22] We are grateful to Gabriel Desgranges for asking us how non-Bayesian beliefs affect the result on levels of reasoning.

[23] A detailed exposition of this exercise is laid out in Appendix B.

socially valuable. If gains from coordination enter the welfare function, as in Woodford (2005), increasing the precision of both signals enhances welfare.[24]

In this section, we analyze the welfare effects of transparency given that agents attach a weight to the public signal that compares to the findings of our experiment. The experiment has shown that subjects weigh the two signals as if they employed at most two levels of reasoning. Following this result, we analyze the 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 an infinite number of agents and normally distributed signals. We distinguish between two welfare functions, the original one from Morris and Shin (2002) and the alternative formulation by Woodford (2005) with socially desirable coordination.[25] We also analyze how the optimal degree of publicity, introduced by Cornand and Heinemann (2008), is affected by limited levels of reasoning.

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

Result 9: In stark contrast to 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 reducing.

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

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

where $L_i \equiv \int_0^1 (a_j - a_i)^2 \, dj$ 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 her/his action a_i). The second component is reminiscent of Keynes' beauty contest. The loss is increasing in the distance between i's action and the average action of the whole population. Speculators gain from predicting the average opinion better than others, but the beauty-contest is a zero-sum game. Social welfare, as defined by the (normalized) average of individual utilities, is given by

[24] 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, while private information may have an opposing effect. Gao (2007), in a framework related to Allen, Morris and Shin (2006), finds that public information has a positive market efficiency effect due to the endogenous link between the informational content role and the coordination role of public information and, therefore, finds that transparency is welfare improving.

[25] Note that we focus on games with strategic complementarities. For strategic substitutes, the welfare effects of public information may be reversed as shown by Angeletos and Pavan (2007b). But, here as well, the welfare analysis depends on whether coordination is socially valuable. This can be seen most clearly in the case of an oligopoly, where collusion is in the interest of firms but detrimental to welfare

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

As a consequence, the social planner, who cares only about social welfare, seeks to keep all agents' actions close to state θ. While coordination affects individual payoffs, it does not affect social welfare.

As in our framework, agents face uncertainty concerning θ. In Morris and Shin (2002), θ has a uniform distribution on the reals and 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. In equilibrium, prior expected welfare is given by

$$E(W) = -\frac{\alpha + \beta(1-r)^2}{(\alpha+\beta(1-r))^2},$$

which is decreasing in the precision of public signals α, if $\alpha/\beta < (1-r)(2r-1)$.

Now let us solve the Morris and Shin model for a level of reasoning of degree 2. The first level of reasoning is given by an agent's Bayesian expectation of θ. Therefore, the action of a level-1-player is

$$a_i^1 = E_i(\theta) = \frac{\beta x_i + \alpha y}{\alpha+\beta},$$

so that the weight on the private signal is $\gamma_1 = \beta/(\alpha+\beta)$ and the corresponding action associated with level 1 is $a_i^1 = \gamma_1 x_i + (1-\gamma_1)y$. Suppose that a player j attaches weight γ_1 to her private signal. The best response to this is

$$\begin{aligned} a_i^2 &= (1-r)E_i(\theta) + rE_i(a_j^1) \\ &= (1-r)E_i(\theta) + r\gamma_1 E_i(x_j) + r(1-\gamma_1)y \\ &= (1-r(1-\gamma_1))E_i(\theta) + r(1-\gamma_1)y. \end{aligned}$$

So, the action associated with level-2 reasoning is

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

and 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

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

$$= -(\gamma_2)^2\frac{1}{\beta} - (1-\gamma_2)^2\frac{1}{\alpha} = -\frac{(\alpha+\beta)^2 + r^2\alpha\beta}{(\alpha+\beta)^3}.$$

Taking derivatives and rearranging terms, we get

$$\frac{\partial E(W)}{\partial\alpha} = \frac{\alpha^2 + (2+r^2)\alpha\,\beta + (1-r^2)\,\beta^2}{(\alpha+\beta)^4} > 0$$

and

$$\frac{\partial E(W)}{\partial\beta} = \frac{(1-r^2)\alpha^2 + (2+r^2)\alpha\,\beta + \beta^2}{(\alpha+\beta)^4} > 0.$$

Hence, 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 stark 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 apply level-2 reasoning, they do not expect others to overreact to public announcements. Apparently, this is sufficient to rule out welfare detrimental effects of public announcements. Examining a wide range of parameterizations, we checked whether this is still true if agents apply third level of reasoning. We detected parameter combinations with $r > 0.6$ and a high relation β/α, for which an increase in α reduces expected welfare. Note, however, that Svensson's (2006) critique is even more applicable: the lower the level of reasoning, the higher β/α must be 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.

5.2. Expected welfare loss for level-2 reasoning with coordination entering welfare

Result 10: If coordination is socially desirable and agents apply limited levels of reasoning, increasing the precision of private information may reduce expected welfare.

In Woodford's (2005) variant of the model, coordination enters the welfare function. The payoff for agent *i* is given by

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

Summing up individual utilities, social welfare is given by:

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

If agents attach weight γ to private signals, expected welfare is

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

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

Inserting the weight γ_2, associated with level-2 reasoning, parameterizations show that the sign of $\partial E(W)/\partial\beta$ is ambiguous. More precisely, increasing the precision of private information can reduce welfare if $\beta/\alpha < 3$ and $r > 0.6$. The reason is that agents put a larger weight on private signals than is socially optimal with limited levels of reasoning. Higher precision of private signals raises the weight on private signals, which impedes desirable coordination. In the extreme case where r=1, providing subjects with private information would always be detrimental if they attach a positive weight to it. Increasing the precision of public information is always beneficial here, because it helps to achieve more coordination, which improves welfare.

5.3. Consequences of limited levels of reasoning for the optimal degree of publicity

The experiment has shown that public information does not necessarily lead to common knowledge. In practice, it seems rather difficult to achieve common knowledge among all agents of a group. Concepts of intermediate publicity as in Cornand and Heinemann (2008), Morris and Shin (2006), or Myatt and Wallace (2008) seem especially relevant for representing the way subjects deal with public signals. The model of Cornand and Heinemann (2008) is very similar to that of Morris and Shin (2002) with the same utility, distribution functions, and welfare functions. The difference is that agents have access to the public signal y only with some probability P.[26] In an application, Walsh (2007) has shown that limiting the degree of publicity may be especially valuable if cost shocks are serially correlated.

Let us solve the Cornand and Heinemann model for limited levels of reasoning of degree 2. Again, the first level of reasoning for a player who receives both signals is given by the Bayesian expectation of θ:

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

so that the weight on the private signal with a first level of reasoning is $\gamma_1 = \beta/(\alpha+\beta)$ and the corresponding action is $a_j = \gamma_1 x_j + (1-\gamma_1)y$. The best response to this is

[26] Since they have a continuum of identical agents, the fraction of agents who receive public information equals P almost certainly. Without loss of generality, agents $i \in [0, P]$ receive the public signal and agents $i \in (P, 1]$ rely on their private signals only. The signal y is "public" in the sense that the actual realization of y is common knowledge among agents $i \in [0, P]$.

$$
\begin{aligned}
a_i &= (1-r)E_i(\theta) + rE_i(a_j) \\
&= (1-r)E_i(\theta) + r[P(\gamma_1 E_i(x_j) + (1-\gamma_1)y) + (1-P)E_i(x_j)] \\
&= ((1-r) + rP\gamma_1 + r(1-P))E_i(\theta) + rP(1-\gamma_1)y \\
&= \frac{\alpha\beta(1-rP)+\beta^2}{(\alpha+\beta)^2}x_i + \frac{\alpha^2+\alpha\beta(1+rP)}{(\alpha+\beta)^2}y.
\end{aligned}
$$

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

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

As Cornand and Heinemann have shown, the expected welfare for a limited degree of publicity is given by:

$$
\begin{aligned}
E(W(a,\theta)) &= -E\left[\int_{i\in(0,1)} (a_i-\theta)^2\, di\right] \\
&= -E\left[\int_{i\in(0,P)} (\gamma x_i + (1-\gamma)y-\theta)^2\, di\right] - E\left[\int_{i\in(P,1)} (x_i-\theta)^2\, di\right] \\
&= -P\left[\gamma^2\frac{1}{\beta} + (1-\gamma)^2\frac{1}{\alpha}\right] - (1-P)\frac{1}{\beta},
\end{aligned}
$$

where γ is the weight that agents put on the private signal. If agents use equilibrium strategies, the weight on the private signal is

$$
\gamma^* = \frac{\beta(1-rP)}{\alpha+\beta(1-rP)}
$$

and the resulting optimal degree of publicity is

$$
P^* = \min\left\{1, \frac{\alpha+\beta}{3r\beta}\right\}.
$$

If, instead, agents use level-2 reasoning

$$
\begin{aligned}
E(W(a,\theta)) &= -P\left[\left(\frac{\alpha\beta(1-rP)+\beta^2}{(\alpha+\beta)^2}\right)^2\frac{1}{\beta} + \left(\frac{\alpha\beta(1+rP)+\alpha^2}{(\alpha+\beta)^2}\right)^2\frac{1}{\alpha}\right] - (1-P)\frac{1}{\beta} \\
&= -P\frac{(\alpha+\beta)^2 + r^2P^2\alpha\beta}{(\alpha+\beta)^3} - (1-P)\frac{1}{\beta}.
\end{aligned}
$$

This implies that expected welfare changes with variations in the degree of publicity P according to

$$
\frac{\partial E(W)}{\partial P} = \frac{-1}{\alpha+\beta} - 3P^2\frac{\alpha\beta r^2}{(\alpha+\beta)^3} + \frac{1}{\beta}.
$$

$$
\frac{\partial E(W)}{\partial P} \geq 0 \quad \Leftrightarrow \quad P \leq \frac{\alpha+\beta}{\sqrt{3}\,\beta r}.
$$

Thus, for level-2 reasoning the optimal degree of publicity is

$$P^*(\gamma_2) = \min\left\{1, \frac{\alpha+\beta}{\sqrt{3}\, r\, \beta}\right\}.$$

The optimal degree of publicity may be smaller than 1, even if agents have limited levels of reasoning. However, optimal publicity is larger than in an economy where agents use equilibrium strategies. For $P^*(\gamma_2)$ to be smaller than 1, we need $\alpha/\beta < \sqrt{3}\, r - 1 < 0.74$. Thus, in contrast to the conclusion from the equilibrium analysis in Cornand and Heinemann (2008), public information needs to be sufficiently less precise than private information to justify limited publicity.

6 – Conclusion

The literature in the vein of Morris and Shin (2002) has largely been interpreted in terms of central bank communication. This literature concentrates on discussing welfare effects of central bank announcements using equilibrium theory. The experiment seems to provide convincing data for the hypothesis that subjects attach larger weights to public than to private 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 behavior. In stark 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 reducing and even limiting the degree of publicity can improve welfare only if public signals are less precise than private information.

In summation, negative welfare effects of transparency requires that strategic complementarities be sufficiently strong, the private values of coordination be sufficiently higher than the social value of coordination, signals provided by the central banks be of a low precision compared to private information, and agents apply high levels of reasoning. If any one of these conditions is violated, it is unlikely that transparency will be detrimental to social welfare. Empirical evidence indicates that at least some of these conditions are violated. Thus, we conclude that strategic complementarities alone do not justify intransparency.

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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 treatments, instructions were adapted accordingly and are available upon request.[27]

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.

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:

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

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.

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

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:

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

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.

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

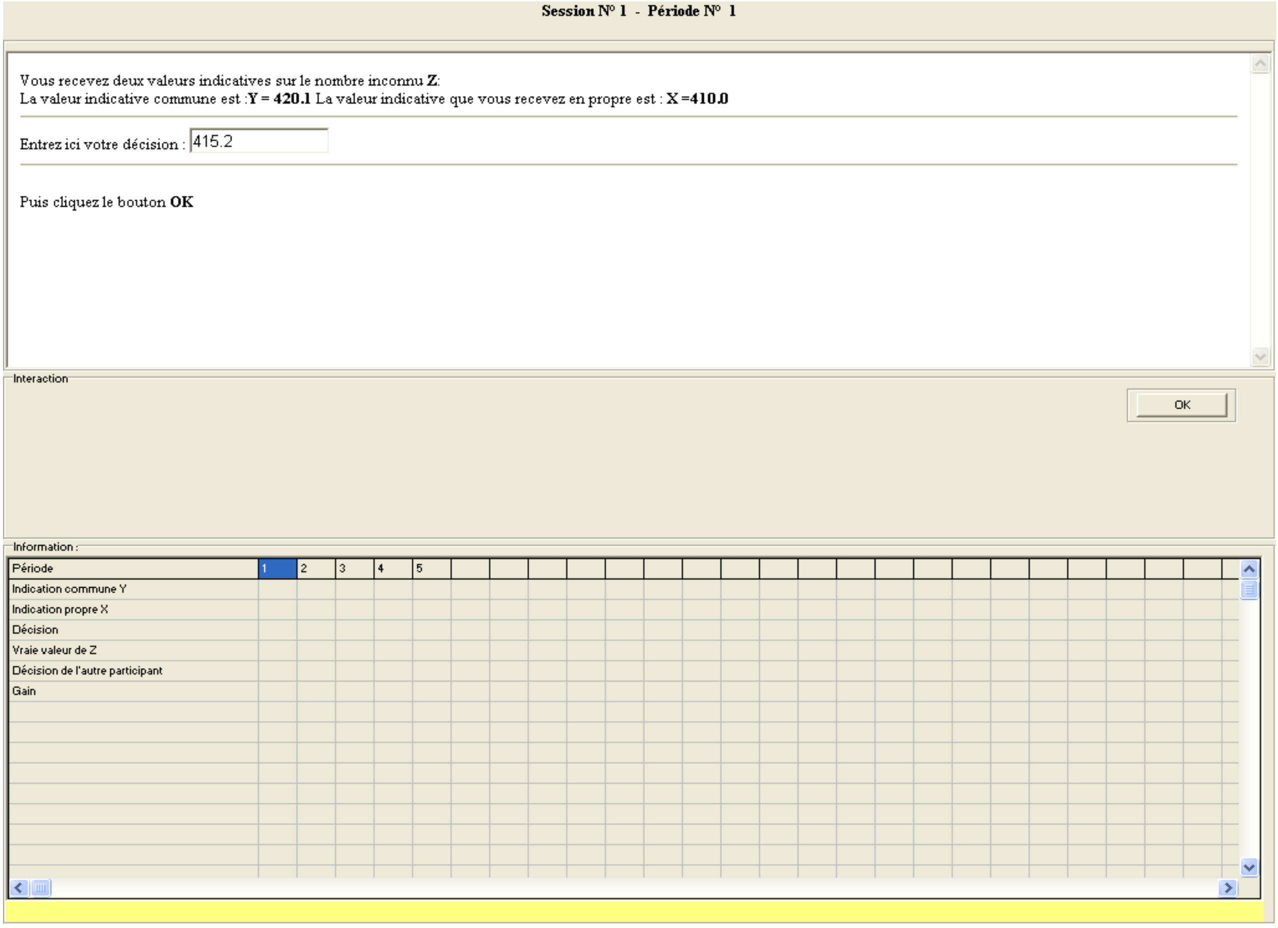
This formula says that your payoff in each situation is at most 400 ECU. It is reduced for deviations of your decisions from the unknown number *Z*, and it is also reduced for deviations between your and your partner’s decision. The closer is your decision to both the other participant’s choice and *Z*,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

$$100 - (your\ estimation\ sur\ Z - Z)^2$$

$$-(your\ estimation\ of\ the\ estimation\ of\ the\ other\ participant\ on\ Z - the\ estimation\ of\ the\ other\ participant\ on\ Z)^2$$

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. Combining non-Bayesian higher-order beliefs with limited levels of reasoning

What is the level of reasoning, if we account for non-Bayesian beliefs? Combining the model of Non-Bayesian higher-order beliefs from Section 4.3 with limited levels of reasoning yields the following weights on private signals denoted by $\hat{\gamma}_k$:

Level 1: $a_i^1 = e_i(\theta) = 0.5(x_i + y)$ => $\hat{\gamma}_1 = 0.5$.

Level 2: $a_i^2 = (1-r)e_i(\theta) + re_i(a_j^1) = (1-r)E_i(\theta) + re_i(e_j(\theta))$

$$= (1-r)E_i(\theta) + r\left[\lambda_i x_i + (1-\lambda_i)y\right]$$

$$= \left[0.5(1-r) + r\lambda_i\right]x_i + \left[0.5(1+r) - r\lambda_i\right]y \quad => \quad \hat{\gamma}_2 = 0.5(1-r) + r\lambda .$$

Level 3: $a_i^3 = (1-r)e_i(\theta) + re_i(a_j^2) = (1-r)E_i(\theta) + re_i\left[(1-r)e_j(\theta) + re_j(a_i^1)\right]$

$$= (1-r)E_i(\theta) + (1-r)re_i(e_j(\theta)) + r^2 e_i(e_j(e_i(\theta)))$$

$$= (1-r)E_i(\theta) + (1-r)r\left[\lambda_i x_i + (1-\lambda_i)y\right] + r^2\left[\lambda_i^{3/2} x_i + (1-\lambda_i^{3/2})y\right]$$

$$= \left[(1-r)\left[0.5 + r\lambda_i\right] + r^2\lambda_i^{3/2}\right]x_i + \left(1 - \left[(1-r)\left[0.5 + r\lambda_i\right] + r^2\lambda_i^{3/2}\right]\right)y$$

$$=> \hat{\gamma}_3 = (1-r)\left[0.5 + r\lambda_i\right] + r^2\lambda_i^{3/2}.$$

Level 4: $a_i^4 = (1-r)e_i(\theta) + re_i(a_j^3) = (1-r)E_i(\theta) + re_i\left[(1-r)e_j(\theta) + re_j(a_i^2)\right]$

$$= (1-r)E_i(\theta) + (1-r)re_i(e_j(\theta)) + r^2 e_i(e_j(\left[1-r)e_i(\theta) + re_i(a_j^1)\right]))$$

$$= (1-r)E_i(\theta) + (1-r)\left[r\left[\lambda_i x_i + (1-\lambda_i)y\right] + r^2\left[\lambda_i^{3/2} x_i + (1-\lambda_i^{3/2})y\right]\right] + r^3\left[\lambda_i^2 x_i + (1-\lambda_i^2)\right]$$

$$= \left[(1-r)\left[0.5 + r\lambda_i + r^2\lambda_i^{3/2}\right] + r^3\lambda_i^2\right]x_i + \left(1 - \left[(1-r)\left[0.5 + r\lambda_i + r^2\lambda_i^{3/2}\right] + r^3\lambda_i^2\right]\right)y$$

$$=> \hat{\gamma}_4 = (1-r)\left[0.5 + r\lambda_i + r^2\lambda_i^{3/2}\right] + r^3\lambda_i^2 .$$

Level $k \geq 3$: $\hat{\lambda}_k = (1-r)\left[0.5 + r\lambda \sum_{j=0}^{k-3} (r\sqrt{\lambda})^j \right] + r^{k-1}\lambda_i^{k/2}$.

Table B.1 compares the estimated weights in Treatments C and D with those that subjects should put on the private signal according to the model combining limited levels of reasoning with non-Bayesian higher-order beliefs. In total, in 13 out of 18 cases weights in Treatments C or D are between level-1 and level-2 reasoning, in only 3 cases they are lower than for level-3 reasoning. In Sessions 1 – 12, estimated weights in Treatments C and D (2^{nd} half) are all higher than those arising from non-Bayesian beliefs and level-2 reasoning. In Sessions 13 – 18, they seem to be distributed around level-2 weights.

Session, group	Estimated weight on private signal (C/D, 2^{nd} half)	Calculated weights in the model combining estimated errors in higher-order beliefs with limited levels of reasoning				
		Level 1	Level 2	Level 3	Level 4	equilibrium
1	0.408	0.5	0.364	0.334	0.327	0.325
2	0.463	0.5	0.421	0.386	0.375	0.371
3	0.447	0.5	0.438	0.402	0.391	0.386
4	0.475	0.5	0.335	0.310	0.305	0.303
5	0.393	0.5	0.355	0.326	0.320	0.318
6	0.453	0.5	0.330	0.306	0.301	0.300
7	0.492	0.5	0.459	0.450	0.449	0.448
8	0.466	0.5	0.450	0.442	0.441	0.440
9	0.500	0.5	0.470	0.461	0.459	0.459
10	0.460	0.5	0.443	0.435	0.434	0.434
11	0.475	0.5	0.469	0.460	0.458	0.458
12	0.473	0.5	0.429	0.422	0.421	0.421
13, group 1	0.484	0.5	$\underline{0.487}$	0.451	0.438	0.431
13, group 2	0.338	0.5	$\underline{0.445}$	$\underline{0.408}$	$\underline{0.397}$	$\underline{0.391}$
14, group 1	0.473	0.5	0.429	0.393	0.382	0.378
14, group 2	0.536	0.5	0.525	0.489	0.476	0.469
15, group 1	0.422	0.5	$\underline{0.480}$	$\underline{0.442}$	$\underline{0.430}$	$\underline{0.424}$
15, group 2	0.540	0.5	0.409	0.374	0.364	0.360
16, group 1	0.454	0.5	0.444	0.407	0.396	0.391
16, group 2	0.485	0.5	$\underline{0.500}$	0.463	0.450	0.443
17, group 1	0.455	0.5	0.449	0.412	0.400	0.395
17, group 2	0.459	0.5	0.450	0.413	0.401	0.396
18, group 1	0.440	0.5	0.409	0.374	0.364	0.360
18, group 2	0.487	0.5	$\underline{0.542}$	$\underline{0.508}$	$\underline{0.494}$	0.486

Table B.1 – Comparing weights from a model of Non-Bayesian higher-order beliefs and limited levels of reasoning with observations. Underlined numbers indicate cases where data are consistent with an application of more than 2 levels of reasoning.

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