Speculative Attacks and Financial Architecture: Experimental Analysis of Coordination Games with Public and Private Information[*]

Frank Heinemann[a], Rosemarie Nagel[b] and Peter Ockenfels[c]

First Version: 7. August 2001

Abstract

Speculative Attacks are a coordination game with multiple equilibria: If a sufficient number of traders expect devaluation, market pressure forces the central bank to abandon a currency peg that it would have kept otherwise. Thus, expectations are self-fulfilling. Applying the global games approach, Morris and Shin (1998) have shown that there is a unique equilibrium in threshold strategies, if traders have private information on the state of the economy. Uniqueness requires private information to be sufficiently precise when compared to public information. This has triggered a discussion on the optimal policy to release information to financial markets in order to prevent crises with self-fulfilling features.

Our experimental design imitates the speculative attacks model by Morris and Shin. We compare sessions with common and private information and test the theory of global games and the payoff dominant equilibrium as a refinement criterion. Independent from the information set-up, most subjects use undominated threshold strategies after the first round. With common information, individual thresholds show a better coordination and are significantly lower than with private information. Dispersion of average threshold across sessions does not depend on the information structure. Reactions to parameter changes follow comparative statics of the private information equilibrium. We conclude that a commitment to provide public information increases prior probability of successful speculative attacks, but the destabilizing effect of public information due to self-fulfilling beliefs may be less severe than theory predicts.

---

[*] We want to thank Hyun Shin for stimulating comments, Isabel Gödde and various collegues at Goethe-Universität Frankfurt for helpful talks about statistical procedures and tests. All errors are at our responsibility. We are indebted to Urs Fischbacher for his fabulous software package z-Tree that we used for our experiments. Finally, we thank students at Goethe-Universität Frankfurt and Universitat Pompeu Fabra in Barcelona for their serious participation in our experiments. Financial support by the Volkswagen Foundation and the Interessengemeinschaft Frankfurter Kreditinstitute is gratefully acknowledged. Frank Heinemann acknowledges financial support by the TMR network on "Financial Market Efficiency and Economic Eficiency".

[a] Goethe-Universität Frankfurt,
[b] Universitat Pompeu Fabra, Barcelona
[c] Goethe-Universität Frankfurt,

1. Introduction

Transparency and the optimal way to disclose central bank information are among the main topics within the current discussion on financial architecture. Speculative attacks and market overreactions are often viewed at as evidence for systemic indeterminacy and instability. Private reactions to montary policy may take the form of a coordination game in which the payoff to speculating on devaluation depends on the amount of capital that follows the same strategy. Coordination games are characterized by multiple Nash equilibria and strategic uncertainty. While deductive theory has no clear prediction for these games, controlled experiments can measure strategic uncertainty and its determinants.

In this paper we present an experiment that mimics the speculative attacks model by Obstfeld (1996) and Morris and Shin (1998). These models differ in information structure: While Obstfeld assumes common knowledge of fundamentals (public information) and gets multiple equilibria, Morris and Shin show that with private information on fundamentals there is a unique equilibrium. These results triggered a discusion on the optimal policy to release information in order to prevent instabilities stemming from self-fulfilling beliefs.

Our evidence suggests that the information structure has an impact on the default point: the probability of a speculative attack is higher with public than with private information. But, there is no difference in predictability that could be related to self-fulfilling features of the game with public information. In both scenarios behavior was stable across sessions and followed the comparative statics of the equilibrium with private information.

Coordination games are characterized by multiple Nash equilibria that can be ranked according to the Pareto criterion. These equilibria go along with self fulfilling beliefs and deductive theory has not really much to say about which of these equilibria are played in real situations. Selection criteria are either based on individual risk minimization, as is the maximin criterion, which predicts that subjects play the action that yields the highest payoff given the worst possible choice of other players, or on collective payoff maximization, as Harsanyi and Selten (1988), who recommend to play the payoff dominant strategy combination. Because deductive concepts cannnot attribute probabilities to situations of strategic uncertainty[1], they cannot balance considerations of payoff maximization and risk minimization. Decision theoretic concepts explicitly consider agents' information. At best, they predict that players choose a rationalizable strategy. This is based on assuming that the rules of the game are common knowledge, players choices are independent and independence is common knowledge as well. Although these are the most restrictive information assumptions one can make in a normal form game, the set of rationalizable strategies is even larger than the set of Nash equilibria, because it allows different players to pursue strategies belonging to different Nash equilibria.

---

[1] Strategic uncertainty is a Knightian uncertainty without defined probabilities for the possible events.

The theory of global games[2] embeds coordination games with strategic complementarities in an environment with private information. More precisely, the assumption of common knowledge of the payoff function is replaced by a stochastic set-up, in which a payoff relevant parameter underlies some distribution and players get independent random signals about this parameter. Instead of widening the set of equilibria, as one might expect at first glance, this less restrictive information assumption reduces the set of equilibria. If the variance of private signals is sufficiently small, the embedded coordination game has a unique equilibrium.

Applications of global games are used to explain probabilities of speculative attacks (Morris and Shin, 1998), bank runs (Goldstein and Pauzner, 2000), liquidity crises (Morris and Shin, 2001, and Hubert and Schäfer, 2001), and competition for order flow (Dönges and Heinemann, 2001). One line of theoretical research concentrates on the impact that different modes of releasing information have on uniqueness versus multiplicity of equilibria and thereby on stability of financial markets. Morris and Shin (2001) and Hellwig (2000) observed that uniqueness requires private information to be sufficiently precise when compared to public information. Heinemann and Illing (1999) showed that the probability of speculative attacks is reduced when precision of private information is increased. Metz (2001) observed that precision of public and private information may have opposing effects on the probability of crises. While policy makers often claim that a more transparent policy increases finacial stability[3], these results raise doubts by academic researchers who emphasize the endogeneity of default risks and the sensitivity with respect to the modes of information disclosure (Daníelson et al. 2001).

Previous experiments on coordination games with strategic complementarities carried out by Van Huyck, Battaglio and Beil (1990, 1991) have shown that with perfect information subjects coordinate rather quickly on an equilibrium between maximin strategies and payoff dominant equilibrium. Efficiency depends on group size and experience. While groups of two players coordinate on the payoff dominant equilibrium even in unfavorable set-ups, groups of 14 to 16 players reach the payoff dominant equilibrium only after experiencing efficient coordination in other treatments. Cabrales, Nagel and Armenter (2000) tested the global game approach by comparing otherwise equal treatments with common and private information on a payoff relevant parameter. They found no significant difference in behavior between the two information scenarios. In both cases subjects converged to coordination on the equilibrium of the private information game, which coincides with the risk dominant equilibrium of the common information game.

In this paper we present experimental evidence on a coordination game that mimics the speculative attack model by Morris and Shin (1998). We compare sessions with common and private information and test the hypothesis that players coordinate on the equilibrium proposed by the theory of global games against alternative

---

[2] The theory of global games originates from Carlsson and van Damme (1993a) and has been further developed by Morris and Shin (2000).

[3] See BIS (2001) for a recent call for more transparency in order to avoid banking crises.

hypotheses, in particular payoff dominant equilibrium and maximin strategies. We find little difference in behavior between sessions with private and common information. In both scenarios, more than 90% of all subjects played threshold strategies that are predicted by theory for private information games but not for games with common knowledge. In common information scenarios, subjects achieve a better coordination on a common threshold, and linear regressions show that the mean threshold is significantly lower (and thus the probability of a successful speculative attack is higher) than in situations with private information. But, the difference is small and not significant for some data subsets. Furthermore, dispersion of average thresholds across sessions (and thus robustness of predictions) does not depend on the information structure. Using treatments with different parameters, we see that in both scenarios aggregate behavior follows the comparative statics of the equilibrium of the private information game. We could reject hypotheses that subjects behave according to payoff dominance, risk dominance or maximin strategies. Instead, we observed that with common information subjects coordinated on equilibria somewhere in between payoff dominance and risk dominance.

We draw three main conclusions from these results: First, the modes of information disclosure may be less relevant to behavior in real situations than theory predicts. Reason may be that strategic uncertainty is severe enough to prevent common information to become common knowledge. Even though strategic uncertainty gradually disappears in common information games, initial uncertainty may be strong enough to lead subjects to behave in a similar way as in games with private information. Inertia effects are strong and we also find some evidence that positive experience of coordination within a group leads to more efficient coordination after changing to a more challenging treatment.

Second, reduced strategic uncertainty seems to be associated with coordination on more efficient equilibria. This may be responsible for the higher probability of speculative attacks in sessions with common information. Thus, even if public information may not be responsible for destabilizing an economy by inducing self-fulfilling beliefs, the associated reduction in strategic uncertainty may change the default point and raise the probability of a successful speculative attack.

Our third conclusion is that payoff uncertainty as modelled in the private information game is a suitable approximation for strategic uncertainty. Even though we can reject the hypothesis that subjects played the equilibrium of the private information game, parameter changes induced changes in behavior that followed the *qualitative* comparative statics of the private information equilibrium. In this sense, the private information equilibrium gives a better description of actual behavior than payoff dominance or the maximin criterion, which do not react accordingly.

Chapter 2 of this paper explains the speculative attacks model that underlies our experiment. Chapter 3 lays out the experimental design. Chapter 4 derives theoretical predictions for the game used in our experiment. Chapter 5 analyses experimental evidence. Chapter 6 compares our results with previous experiments and attempts to draw conclusions.

2. Speculative Attacks

Speculative attacks on a currency peg can be modelled as a coordination game with strategic complementarities as in Obstfeld (1996). A central bank pegs the exchange rate of its currency to some other currency or currency basket. Realignment is associated with fixed costs. Economic decisions by private agents depend on their expectations about future exchange rates. An agent who believes in devaluation sells the currency, which increases supply and, thereby raises costs for the central bank to maintain the peg. In critical situations, market pressure may raise costs of maintaining the peg above the costs of realignment. Here, the central bank gives in and devaluates its currency. If agents had expected the peg to hold, they had not sold the currency, and the central bank could have maintained the peg. This explains the existence of multiple equilibria with self-fulfilling beliefs.

The existence of multiple equilibria depends on underlying fundamentals: If the fundamental state of the economy is really bad, a devaluation is unavoidable, even if nobody attacks. If the shadow exchange rate is far below the peg, maintaining the peg is associated with an unsustainable outflow of reserves. Here, there is a unique equilibrium in which all agents expect devaluation and sell the currency. They “attack”, as we say. If fundamentals are sound, there is not enough capital around to enforce a devaluation, or the peg is so close to the shadow rate that maximal rewards from a speculative attack are too small to cover transaction costs. Here, it is irrational to attack. It is only in intermediate situations, in which beliefs may be self-fulfilling.

Morris and Shin (1998) used a reduced version of this model to show that there is a unique equilibrium if there is only private information on the fundamental state of the economy. They considered a game in which an infinite number of small traders $i \in [0,1]$ can decide whether to attack or not. The fundamental state is denoted by q. If the proportion of attacking traders exceeds a hurdle function $a(q)$, the attack is successful and each attacking trader receives a reward $R(q) - T$. Otherwise, attacking agents loose transactions costs T. Assuming $a' > 0$ and $R' < 0$, larger q is interpreted as a better state of the economy. At states above $\overline{q} = R^{-1}(T)$ a speculative attack is unrewarding because it does not cover transaction costs even if successful. Here, it is a dominant strategy not to attack. At states below $\underline{q} = a^{-1}(0)$ a devaluation is unavoidable. Assuming $\underline{q} < \overline{q}$, it is a dominant strategy to attack at states below $\underline{q}$. At intermediate states $q \in [\underline{q}, \overline{q}]$ there are two equilibria in pure strategies with all or none of the agents attacking.

Morris and Shin assume that fundamental state q has a uniform distribution with sufficiently large support to include $\overline{q}$ and $\underline{q}$. Traders get private signals x^i that are random with independent uniform conditional distribution in $[q - e, q + e]$, where e is sufficiently small. Now, each trader expects other traders to receive higher or lower signals than her own with equal probability. Common knowledge of the state is replaced by an equilibrium condition, at which agents compare expected returns from successful attack, weighted with the probability of success, with transaction costs that they have to pay with certainty. Morris and Shin (1998) proved that there is a unique equilibrium with thresholds x^* and q^*, such that a trader attacks if and only if she receives a signal below x^*, and the attack is successful if and only if $q < q^*$.

Heinemann (2000) has shown that these thresholds converge to the unique solution of $(1-a(q))\,R(q)=T$, for $e \to 0$. This limit point for diminishing variance of private signals, q_0^* is independent from other assumptions on the probability distributions (Frankel, Morris and Pauzner, 2000). Limit point q_0^* follows the intuition of risk dominance, introduced by Harsanyi and Selten (1988) for 2-player games[4]. In general, it is characterized by some kind of Laplacian beliefs: As Morris and Shin (2000) point out, q_0^* is the optimal threshold of a trader who beliefs that the proportion of other traders who choose to attack has a uniform distribution in [0,1]. Henceforce, we refer to threshold q_0^* as the 'Laplacian belief equilibrium' of the game with common knowledge.

Another, naïve way to define Laplacian beliefs in this game is to assume that each player believes other traders to attack independently with probability ½. In a game with infinitely many agents this leads each player to expect that exactly half of all agents attack. Hence an attack is expected to be successful if and only if $a(Y) \le 1/2$. A best reply to such beliefs is to attack if and only if $Y \le \min\{\overline{q}, a^{-1}(1/2)\}$. We refer to this point as the 'naïve Laplacian belief equilibrium'. Although it appears as a concept without any theoretical justification, we include it in our test series.

In our experiment, we wanted to avoid any connotation that might be associated with "speculation" or "attacking". Therefore, we asked subjects to choose between two actions A and B. In order to avoid negative payoffs, Action A was introduced as secure alternative, yielding a positive and constant payoff that may be interpreted as avoided costs of a speculative attack T. Action B was the risky action, yielding a payoff of Y, if the number of subjects choosing B exceeded a hurdle function $a(Y)$ with $a' < 0$, and zero otherwise. Thus, we reversed the order of states, higher Y being worse states of the economy in which subjects might gain higher payoffs. This reversal was merely done to ease subjects' understanding of the game.

3. Experimental Design

Sessions were run at a PC pool of the economics department in the University of Frankfurt and in the LEEX at Universitat Pompeu Fabra, Barcelona, from November 2000 until June 2001. In Frankfurt students were invited to participate by e-mails to

---

[4] Carlsson and van Damme (1993a,b) show that the equilibrium of the private information game converges to the risk dominant equilibrium for 2-player games, but not for general games with more than two players.

all students with an e-mail account at the department of economics and business and via leaflets and posters at various places in the university. In order to participate they replied by e-mail or phone. In Barcelona students were notified via leaflets and posters within the university and signed up on a list at the door of the laboratory. In both places, most of the participants were economics and business undergraduates. The procedure during the sessions was kept the same throughout all sessions at both places, besides the languages (German and Spanish, respectively). All sessions were computerised, using a program done with z-tree (Fischbacher, 1999). Students were seated in a random order at PCs. Instructions (see Appendix A) were then read aloud and questions were answered in private. Throughout the sessions students did neither communicate nor see others' screens.

We ran 13 sessions with common information (CI) and 12 sessions with private information (PI, see Table 1). At each session there were 15 participants. For two sessions with CI we invited subjects that had previously participated other sessions. In total, we had 345 participating students.

Each session consisted of two stages with 8 independent rounds in each stage. In each round subjects were given 10 independent situations, in each of which they had to decide between two alternatives (A or B).

For each situation, state Y was randomly selected from a uniform distribution in the interval [10, 90]. In sessions with CI players were informed about Y. In sessions with PI each subject received a private signal. Signals were randomly selected from a uniform distribution in the interval [Y – 10 , Y + 10] for each player, separately.

The two stages of each session differed by the secure payoff for A which was either first 20 and then 50 or vice versa. The payoff for B was Y, if at least $a(Y) = 15\,(80 - Y)/Z$ subjects chose B, zero otherwise. The formula was given in the instructions, but also explained via an example and a table (see Appendix A). In four sessions Z=100, in the others Z=60. Table 1 gives an overview of different sessions. Rules of the game, including the structure of uncertainty, were common information among subjects of each session.

In each session there were 15 participants interacting with each other. Having 2 stages each with 8 periods consisting of 10 situations, we got 160 decisions from each subject. For each period, the secure payoff for decision A was always shown on top of the screen (being either 20 or 50). This was followed by a table: The left hand side displayed state Y in the CI condition or private signal X in the PI condition for each of the ten situations. At the right hand side, subjects had to decide between A and B by clicking at either of two boxes. There was no presetting. Decisions could be changed until subjects clicked at an OK-button at the lower end of the screen.

Once all players had completed their decisions in one round, they were informed for each situation about Y, how many people had chosen B, whether decision B was successful or not, their individual payoff and their cumulative payoff over all 10 situations within the period. Furthermore they were reminded of their own signals (in PI-condition) and their own choices. Other information of other players were withhold.

After all players had left the information screen a new period started and information of previous periods could not be revisited.[5] Subjects were allowed to take notes and many of them did.

Z	Secure payoff T	Information	Experienced subjects	Number of sessions in Frankfurt	Number of sessions in Barcelona
100	First 20 / then 50	Private (PI)	No	1	
100	20 / 50	Common (CI)	No	1	
100	50 / 20	PI	No	1	
100	50 / 20	CI	No	1	
60	20 / 50	PI	No	2	3
60	20 / 50	CI	No	1	3
60	50 / 20	PI	No	2	3
60	50 / 20	CI	No	2	3
60	20 / 50	CI	Yes	1	
60	50 / 20	CI	Yes	1	
Total number of sessions				13	12

Table 1. Session overview.

At the end of each session we asked participants to fill in a questionnaire (via computer) asking for their personal dates (name, address), and also four questions about their behavior and whatever comments they had regarding the experiment.

Once completed the questionnaire, each person was paid in private converting their total points into DM and Pesetas, respectively. In sessions with Z=100: 250 ECU = 1 DM. In sessions with Z = 60: 200 ECU = 1 DM = 70 Ptas.

Average payment per subject varied across sessions from 34 to 44 DM in Frankfurt and from 2380 to 3140 Pesetas in Barcelona[6]. Session length was between 90 and 120 minutes.

---

[5] Within the decision phase a descending clock at the top of the screen indicated the time left. However, at the time limit subjects were only reminded to make their decision with no other consequence. In the information phase reaching the time limit meant that the screen vanished and the next period started. Time limits were originally set to 180 seconds for a decision phase and 150 seconds for the information phase. After many students showed signs of boredom in the first sessions with CI, we reduced time limits in the second treatment of sessions with CI by 30 seconds.

[6] Average payoffs in Euro varied from € 17.50 to € 22.50 in Frankfurt and from € 14.30 to € 18.90 in Barcelona.

4. Theoretical Predictions

In the game with private information, there is a unique equilibrium with a threshold X^*, such that a risk neutral player with signal X^* is indifferent provided that all other players choose B if and only if they receive signals above X^*. At state Y the probability of getting reward Y for action B is given by the probability that at least $a(Y)-1$ out of the other $n-1$ players get signals above X^* and choose B. This can be described by the binomial distribution. Note that $n=15$ throughout our experiment. The probability that a single player gets a signal above X^* at state Y is $(Y-X^*+e)/(2e)$. Denoting the round-up of $a(Y)$ by $\hat{a}(Y)$, expected utility of an agent choosing B is

$$U_B(X^*)=\int_{X^*-e}^{X^*+e} Y\ \mathrm{prob}\left(\#\left\{j\neq i\,|\,X^j>X^*\right\}\geq a(Y)-1\,\middle|\,Y\right)\mathrm{d}Y$$

$$=\int_{X^*-e}^{X^*+e} Y\left[1-Bin\left(\hat{a}(Y)-2,n-1,\frac{Y-X^*+e}{2e}\right)\right]\mathrm{d}Y,$$

where Bin is the cumulative binomial distribution. At the equilibrium threshold $U_B(X^*)=T$.

If Y is common knowledge, the game has multiple equilibria for some Y. If $Y<\underline{Y}=T$, rewards from action B are smaller than those from action A. Here, choosing A is a dominant strategy. If $Y>\overline{Y}=a^{-1}(1)=80-Z/n$, a single agent choosing B is sufficient for success. Here, choosing B is a dominant strategy. If $\underline{Y}<Y<\overline{Y}$, there are two Nash equilibria in pure strategies: If all agents choose B, B is successful and more rewarding than A. If all subjects choose A, a single agent looses by deviating to B.

If all subjects choose B, they are clearly better off than by choosing A. Thus, the payoff dominant equilibrium prescribes action B for all $Y>T$.

The worst choice, a single agent must fear, is that nobody else chooses B. In this case, the agent should choose B if and only if $Y>\overline{Y}$, which is the maximin strategy.

What we call the “Laplacian belief equilibrium,” is given by the PI equilibrium as e converges to 0. As we mentioned above, it is robust for all kinds of probability distributions. The associated threshold is the solution to $Y(n-\hat{a}(Y)+1)=nT$.

The risk dominant equilibrium, as defined by Harsanyi and Selten (1988) differs slightly from the Laplacian belief equilibrium. Its threshold is given by the solution to $Y\left[1-Bin\left(\hat{a}(Y)-2,n-1,1-T/Y\right)\right]=T$.

The threshold of the “naïve Laplacian equilibrium” is given by the state at which an agent is indifferent, who beliefs other players to attack with probability ½. It is given by the solution to $Y\left[1-Bin\left(\hat{a}(Y)-2,n-1,1/2\right)\right]=T$.

Table 2 comprises theoretical equilibrium thresholds for our different treatments.

Treatment	T=20, Z=100	T=20, Z=60	T=50, Z=100	T=50, Z=60
equilibrium of PI game	32.36	41.84	60.98	66.03
Payoff dominant equilibrium	20	20	50	50
Laplacian belief equilibrium	33.33	44.00	60.00	64.00
Risk dominant equilibrium	34.55	44.00	62.45	67.40
‘naïve’ Laplacian belief equilibrium	33.07	48.00	51.48	56.00
maximin strategy	73.33	76.00	73.33	76.00

Table 2. Theoretical equilibrium thresholds.

The main purpose of the experiment was to put the theory of global games at a test. In particular, we wanted to test four hypotheses:

1. There is no difference in behavior between sessions with common and private information.

2. Behavior converges towards the unique equilibrium in sessions with private information and towards the Laplacian belief equilibrium in sessions with common information.

3. With common information, subjects coordinate on the payoff dominant equilibrium.

4. There are inertia effects in adjustment to changing parameters.

5. Experimental Results

5.1. Threshold Strategies

As one should expect, subjects tended to choose A for low signals or states and B for high signals or states. We say that a subjects’ behavior was consistent with existence of a threshold, if the highest signal or state for which the subject chose A was smaller than the lowest signal or state for which this subject chose B. Most subjects’ behavior was consistent with existence of a threshold from second round onwards, although the threshold may have changed over time as subjects gained experience about behavior of others. Some subjects chose the same action for all signals/states in some rounds, even if this action was dominated by the other for some signals/states. E.g. some subjects chose B when they should have known that $Y < T$. In most sessions, subjects who chose dominated actions in some rounds were the same as subjects whose behavior was inconsistent with a threshold after the second round. We conclude that both kinds of behavior hint at a subject’s lack of understanding the game or at lack of motivation. This gives us a measure of whether we succeeded in controlling the experiment. We call a subject's behavior 'rational' if her or his behavior was consistent with existence of a threshold and did not exhibit any dominated actions. Table 3 shows the average number of subjects who behaved 'rational' for each round. Sessions are distinguished by location, information, and subjects' experience. The total number of participants was 15 in each session.

Location	Barcelona	Barcelona	Frankfurt	Frankfurt	Frankfurt
Information	Private	Common	Private	Common	Common
Experience	no	no	no	no	yes
No. of sessions	6	6	6	5[7]	2
Round 1	11.50	9.67	11.00	11.00	12.50
Round 2	12.50	11.67	12.83	14.00	14.50
Round 3	12.83	12.33	14.17	13.60	15.00
Round 4	12.50	13.00	14.33	14.60	15.00
Round 5	13.50	13.33	13.83	14.20	14.00
Round 6	13.67	13.83	14.33	14.40	15.00
Round 7	14.00	13.83	14.67	14.40	15.00
Round 8	14.00	14.00	14.50	14.40	15.00
Round 9	12.83	13.17	14.33	14.20	14.00
Round 10	14.00	13.33	14.50	14.00	15.00
Round 11	13.83	14.17	14.67	14.60	15.00
Round 12	14.17	13.67	14.17	14.40	15.00
Round 13	14.33	14.50	14.50	14.80	15.00
Round 14	14.50	14.50	14.50	15.00	14.50
Round 15	14.00	14.67	14.33	15.00	14.50
Round 16	14.50	14.67	14.00	15.00	14.50
Average	13.54	13.40	14.04	14.18	14.59

Table 3: Average number of subjects, whose behavior was consistent with undominated threshold strategies.

We could not find any significant difference in behavior between sessions with private and common information, nor between different treatments. However, in Barcelona the number of 'rational' subjects was significantly smaller than in Frankfurt. One test is a simple regression between location and the average number of subjects, who behaved 'rational' in each session. The location dummy is significant at the 5%-level (Results of these regressions are stated in Appendix C). We do not have a clear explanation for this difference. One difference between subjects at the two locations was that subjects in Barcelona were used to participating in game theoretic experiments, while students in Frankfurt were not. Given that students in Frankfurt regarded participation in an experiment as something extraordinary, they may have been better motivated. Table 3 shows, however, that in first and final rounds the difference between locations was small.

---

[7] Because of computer problems one session with common information in Frankfurt stopped after 13 rounds, another one lost data of round 16.

Two sessions with subjects, who had participated in one of the other sessions before, showed a higher proportion of threshold strategies than sessions with subjects, who participated for the first time. However, the selection of subjects, who agreed to participate a second time, is endogenous, and figures must be compared with caution.

In the first round the number of inexperienced subjects behaving 'rational' varied from 5 to 14 with an average of 10.78. In the second round, after they received information about their achieved payoffs and about aggregate behavior of other subjects, the average number of 'rational' subjects increased by about two to 12.70. Those subjects seemed to need first feedback to understand the advantage of threshold strategies. The number of 'rational' subjects tended to increase over time, although with the change of treatment in round 9, we observed this number to drop, especially in Barcelona. We attribute this to confusion stemming from the parameter change. In the last four rounds we observed at least 13 out of 15 participants behaving 'rational' in *all* sessions. Figure 1 plots the average number of 'rational' subjects from all sessions with inexperienced subjects for each round.

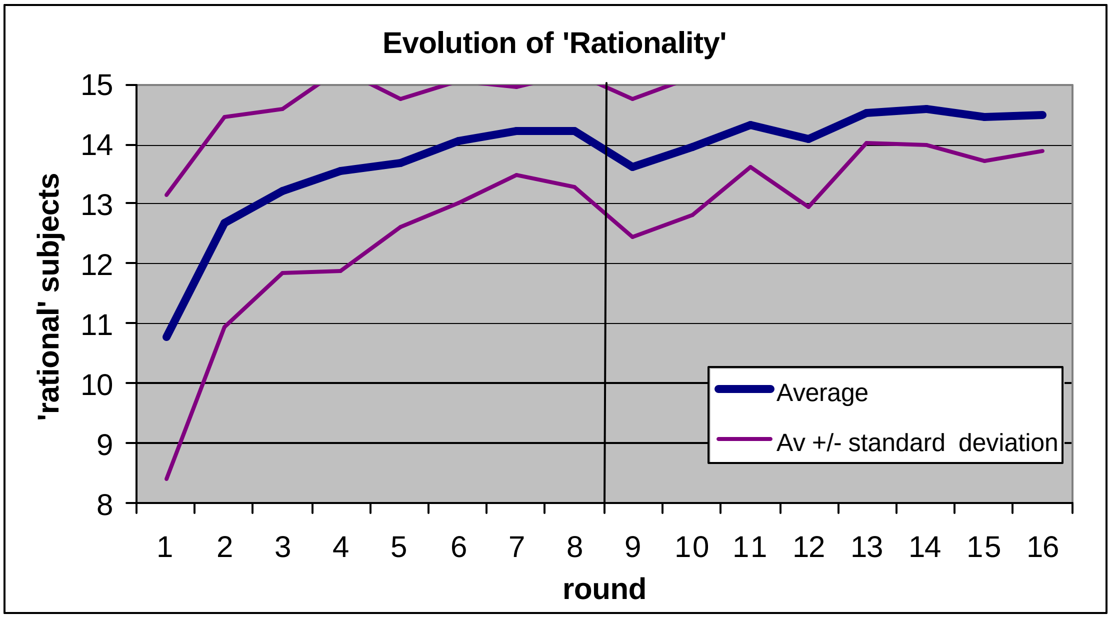

Figure 1. Number of inexperienced subjects per session, whose behavior was consistent with undominated threshold strategies.

On one hand it is not surprising that most participants seem to have played threshold strategies: The hurdle for success of B is decreasing in Y, the payoff to B in case of success is increasing. Common sense tells us that we should choose B for high states and A for low states or signals. On the other hand, deductive reasoning needs very strong assumptions to get this result: In games with private information, theory predicts threshold strategies but requires common knowledge of the game structure. As we know from other experiments by Stahl and Wilson (1994) and Nagel (1995), real subjects fail to reason more than at most 3 layers of beliefs over beliefs. In games with common information non-threshold strategies may even occur in Nash equilibria.

The strength of threshold strategies lies in their robustness. If a subject expects others to play threshold strategies or randomize, her or his best response is a threshold strategy. Even though other strategies might form an equilibrium in common knowledge games, the best response to any naive belief deviating from common knowledge is a threshold strategy. As there is strategic uncertainty at least in the first rounds of a treatment, threshold strategies are a natural way to play. Once a sufficient number of subjects plays threshold strategies, the best response is again a threshold strategy. Other strategies may be an equilibrium under common knowledge, but they are not robust against even slightest deviations from common knowledge.

5.2. Revealed Thresholds

There are several ways to estimate individual thresholds. Since we have only ten data points per subject in each round, intervals between the highest signal with choice of A and the lowest signal with choice of B are often rather large. We can restrict estimates of individual thresholds by combining data of consecutive rounds under the hypothesis that thresholds did not change within these rounds.

Starting with the last round of each treatment, we consider as many rounds as possible, maintaining the hypothesis that the individual threshold did not change in these rounds (see Figure 2). For the few subjects, whose behavior was inconsistent with a threshold in the last round, we take the highest state/signal, up to which this subject chose A exclusively, and the lowest state/signal, from which on the subject always chose B. Default values are 0 and 100.

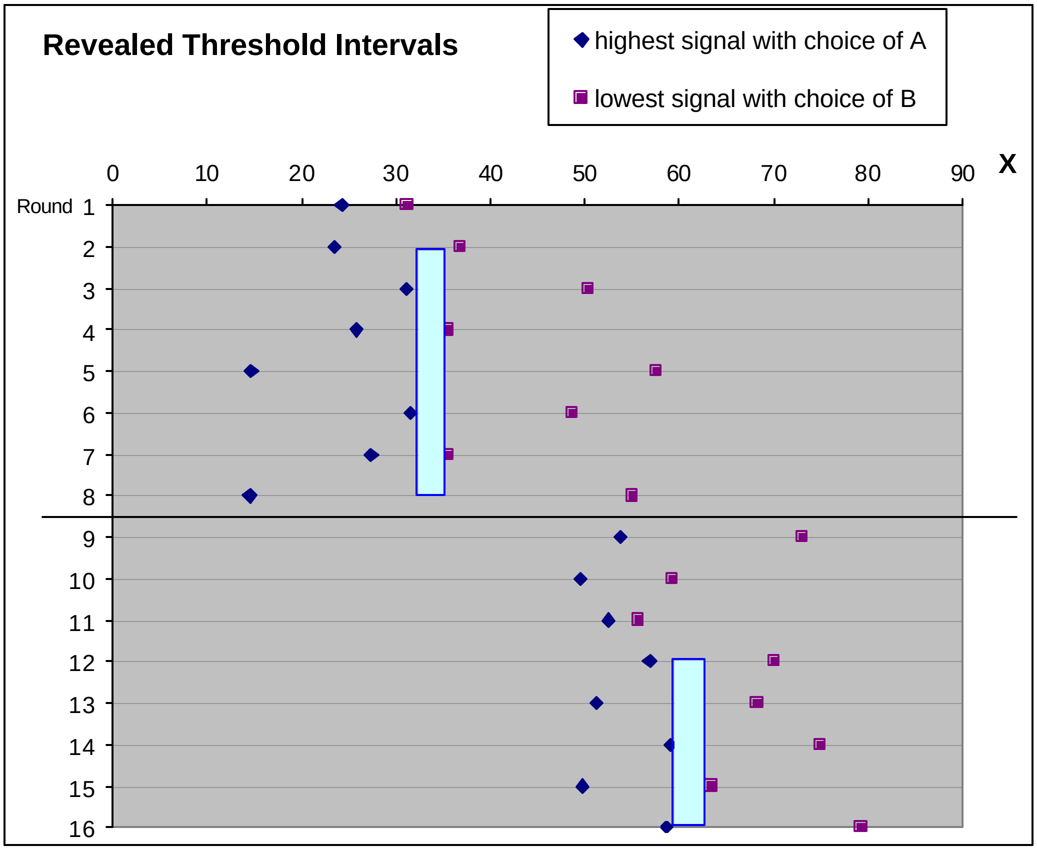

Figure 2. Example for the procedure of measuring ’revealed thresholds’. Data from session 001115, subject 1. Rectangular areas indicate threshold intervals.

Assuming that subjects would choose A for all signals/states below the lower bound of the interval resulting from this procedure and B for all states/signals above the upper bound of this interval, we can predict upper and lower bounds on the number of subjects whithin a group, who would choose B for any state or signal, respectively. Figure 3 and 4 illustrate this for one session with common and one with private information.

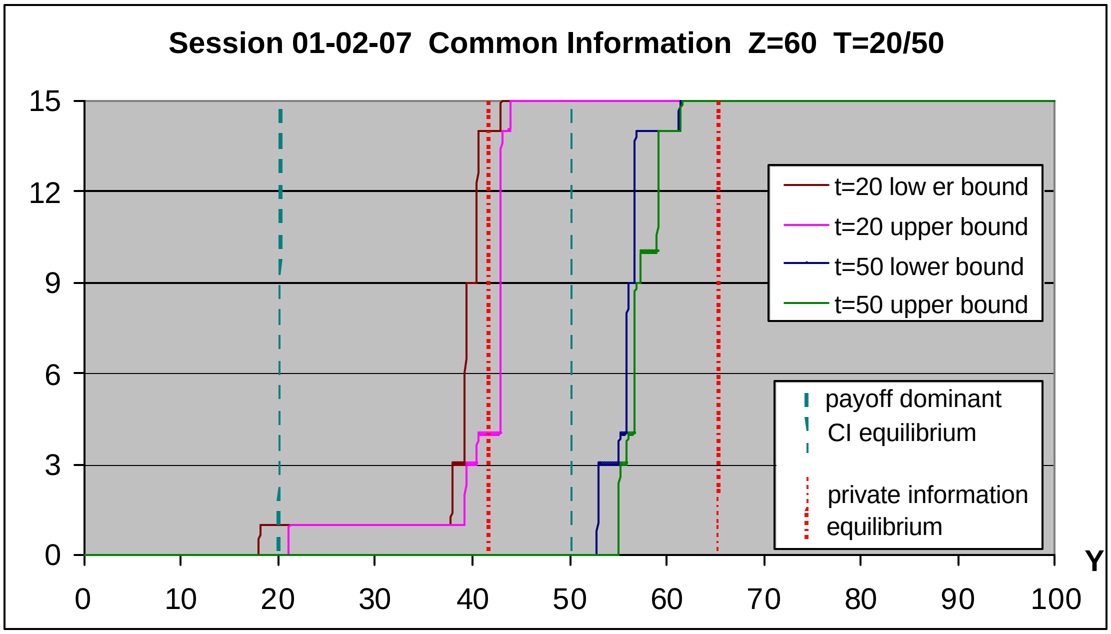

Figure 3. Example for estimations of the number of subjects, who choose B according to the method of revealed thresholds in a session with common information. Coordination occured at steps of the hurdle function at Y=40 and 56, respectively

In most sessions with common information, this method reveals that a vast majority of subjects coordinates on a threshold that can be identified as one of the steps of the hurdle function $a(Y)$ that subjects saw on the table in their instructions (see Appendix A). These numbers appear to be natural candidates for coordination. In a few sessions with common information, subjects achieved coordination on the payoff dominant equilibrium, where the threshold is T. Coordination was possible here, because subjects got a feedback on behavior of other subjects at each state Y, which was the information, on which individual decisions were founded.

In sessions with private information, subjects learnt aggregate behavior conditional on Y, but not on the signals that were underlying actual decisions. Thus, even if subjects had achieved perfect coordination on one threshold, they would not have been sure about that, because they got no information about the signals at which other agents chose A or B.

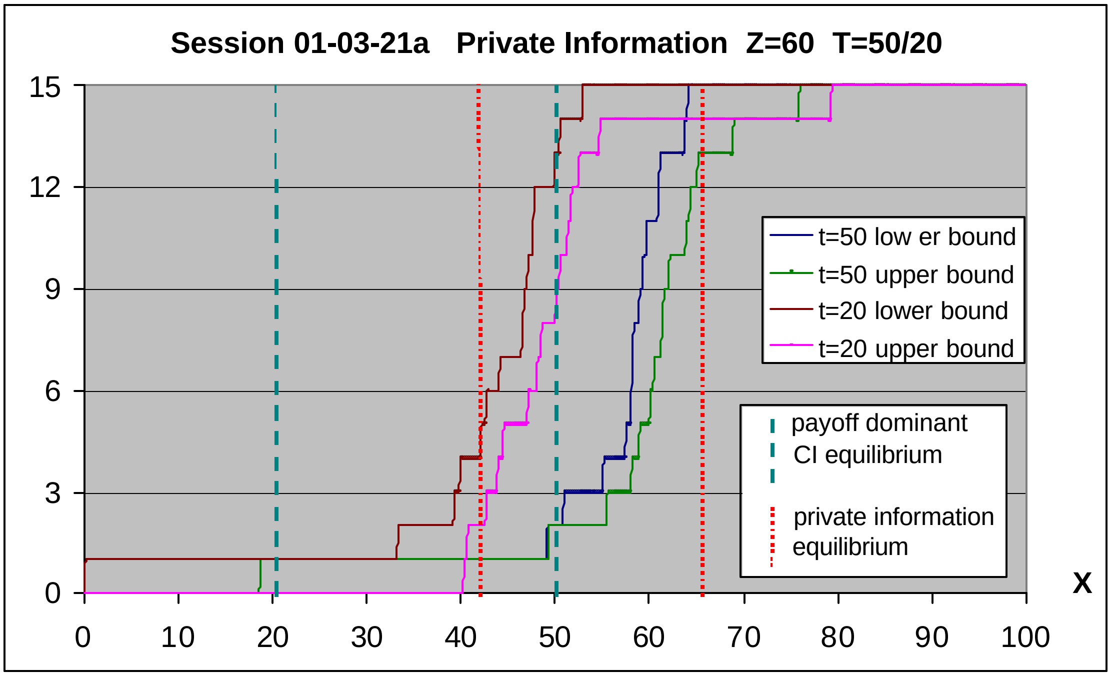

Figure 4. Example for estimations of the number of subjects, who choose B according to the method of revealed thresholds in a session with private information

Sessions with Common Information					Thresholds for success	
Session no.	Z	Location	Experience	Order	T = 20	T = 50
001206	100	Frankfurt	no	T=20/50	33.33 (8) *	53.33 (8) *
010131	100	Frankfurt	no	50/20	19-21 (8) **	50 (8) **
010207	60	Frankfurt	no	20/50	41-42 (7) *	56 (8) *
010321b	60	Frankfurt	no	50/20	36-38 (8) *	51-52 (8) *
010530b	60	Frankfurt	no	50/20	32-35 (8) *	48-51 (8) **
0531PB	60	Barcelona	no	20/50	40 (8) *	52 (8) *
0606PA	60	Barcelona	no	20/50	40-41 (8) *	50-53 (8) */**
0608PE	60	Barcelona	no	20/50	48 (8) *	52-55 (8) *
0607P9	60	Barcelona	no	50/20	37-42 (7) *	55-56 (8) *
0614L9	60	Barcelona	no	50/20	45 (8)	53.5 (8)
0614P9	60	Barcelona	no	50/20	40 (8) *	51 (8)
010516b	60	Frankfurt	yes	20/50	33-36 (8) *	56 (8) *
010516a	60	Frankfurt	yes	50/20	31-32 (8) *	50-51 (8) **

Table 4. Thresholds for success in final rounds of sessions with common information. Numbers in brackets indicate the number rounds that were consistent with these thresholds. One star (*) indicates that thresholds are consistent with a step of the hurdle function. Efficient coordination is indicated by two stars (**).

Table 4 exploits data from sessions with common information only. It shows which thresholds explain actual success and failure of action B in the last rounds of each treatment. More precisely, the numbers given in the last two rows of Table 4 separate states at which action B had been successful in the final rounds from those, where B had not been successful. We considered as many round as possible without getting an overlap of states at which B was successful or not. Ranges indicate data gaps due to random selection of states. In brackets we give the number of rounds that we could consider for this procedure.

In 18 out of 26 treatments, identified thresholds are a step of the hurdle function or a range, covering one of these steps. In 4 cases coordination achieved the payoff dominant equilibrium, in one case, we had a range covering the payoff dominant equilibrium and the next step of the hurdle. The remaining three cases showed odd numbers, in one case (session 0614L9, T=20) this was exclusively due to decisions in the first round. Using just the last seven rounds would have given us a range covering step-value 44.

In 24 treatments there was complete data separation between states at which B was successful and those where it failed. Here, all 8 rounds were consistent with a common threshold for success. This strengthens a result of the experiment by Van Huyck, Battaglio and Beil (1991), where initial behavior determined coordination throughout a treatment.

Comparing sessions with different Z, we see a more efficient coordination in sessions with the low hurdle (Z=100) for T=20. Note that for Z=60, the number of subjects needed for success at states close to 20 was rather high. We never observed action B to be successful when more than 12 B-players were required for success. This adds to an observation by Van Huyck, Battaglio and Beil (1990) who saw coordination breakdown, when all members of a group size 14 – 16 were needed to prevent it.

5.3. Estimated Thresholds

To get comparable figures for sessions with common and private information, we estimate average thresholds for each session using a logistic regression. Estimates on a round by round basis do not show much variation after the first three rounds of each treatment. In order to improve quality of results, we combine data of the last four rounds of each treatment and estimate the parameters of the logistic distribution that fit those data best. Results are logistic distributions that may be interpreted as estimated probabilities for subjects choosing B conditional on state Y or signal X, respectively. Figure 5 and 6 give an impression of the data fit obtainable by logistic regressions. The cumulative logistic distribution is given by $prob(B) = \frac{1}{1+\exp(a-bx)}$.

The mean is $\frac{a}{b}$, standard deviation is $\frac{p}{\sqrt{3}\,b}$.

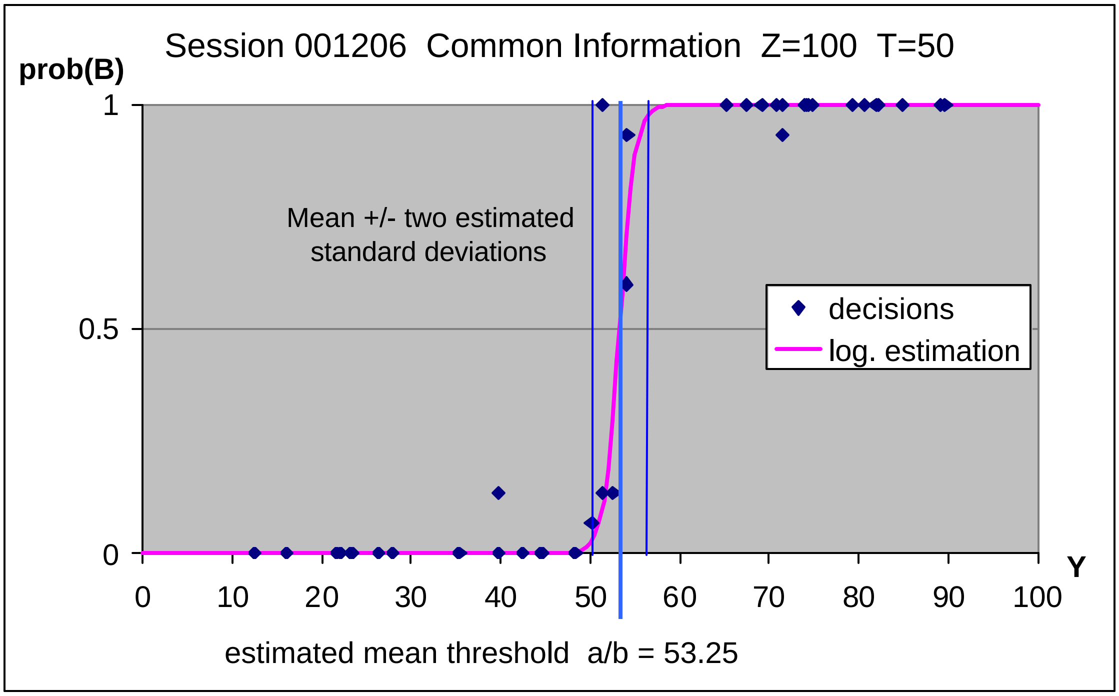

Figure 5. Data and logistic estimation of probability to choose B during the last for rounds of a treatment with common information. The displayed treatment was the one with the smallest estimated standard deviation in subjects' behavior.

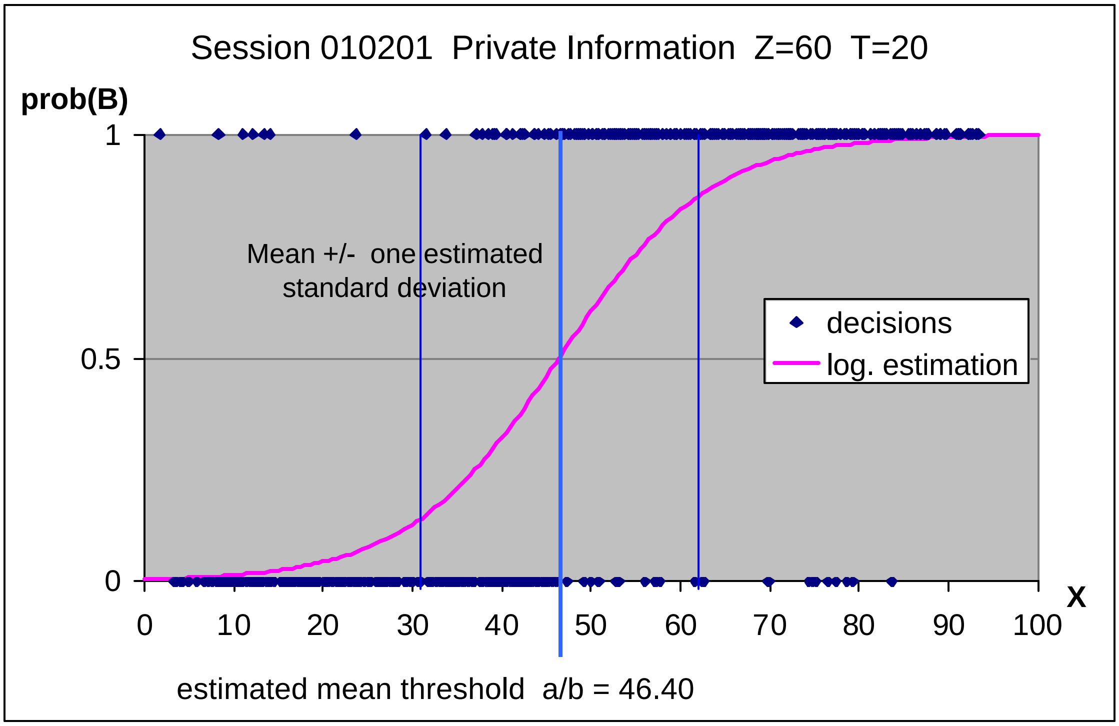

Figure 6. Data and logistic estimation of probability to choose B during the last for rounds of a treatment with private information. The displayed treatment was the one with the largest estimated standard deviation in subjects' behavior.

Detailed results of logistic regressions (based on decisions in the last four rounds of each treatment) for all sessions and treatments are displayed in Appendix B.

In order to explain differences in estimated mean thresholds and standard deviation, we run various linear regressions. As explanatory variables we use dummies for session specific parameters (Z, information, location, experience, and order of treatments). Further explanatory variables are the payoff to action A and the average number of 'rational' subjects in each treatment. Due to the almost orthogonal structure of exogenous treatment parameters, linear regressions provide sufficient test statistics. To exclude possibly disturbing effects from experienced subjects we run separate regressions for sessions with inexperienced subjects only. Because of the non-linearity of the payoff function, we also run separate regressions for sessions with Z=60 and treatments with T=20 and T=50. Appendix C displays results for some of these regressions.

We find significant influence on the estimated mean threshold (a/b) by the parameters of the payoff function Z and T, by the information scenario, and by the order of treatments:

In sessions with a high hurdle function (Z=60) the threshold was higher than at sessions with a low hurdle function (Z=100). The difference was 12 and highly significant, when T=20 (see Regression 7). In treatments with T=50, the difference was smaller than 1 and not significant (see Regression 8).

The largest impact on behavior stems from T, the payoff of the secure action. A higher payoff for secure action A increases the threshold at which subjects switch to action B. In sessions with Z=60, the threshold in treatments with T=50 was about 15 higher than in treatments with T=20 (see Regression 9).

With private information, the mean threshold was about 4 higher than with common information. This difference was robust to all model alternations and significant at the 5% level[8] (see Regression 4 – 11). This contrasts earlier results by Cabrales, Nagel and Armenter (2000), who did not find different behavior for sessions with common and private information.

Surprisingly, in sessions, where the payoff for action A was first 20 and then 50, both thresholds tended to be higher than in sessions with reversed order of treatments. Originally, we expected the opposite effect as a sign of sluggish numerical adjustment of thresholds to the parameter change in period 9. In treatments with T=20 this difference was about 4 – 5 and significant (see Regressions 7 and 10). In treatments with T=50 the difference was smaller than 2 and not significant (see Regressions 8 and 11).

Estimated mean thresholds were slightly higher in Frankfurt (0.5-1.3) but the location dummy was never significant. However, separate regressions with data from Frankfurt and Barcelona show that information and order dummies had less impact in Barcelona and were not significant there (see Regression 12). On the other hand, we found that in Barcelona thresholds in second treatments were about 2 lower than in

---

[8] A t-value of 1.96 indicates significance at the 5% level. A t-value of 2.57 indicates significance at the 1% level.

comparable first treatments. The difference is significant at a 10% level and can be better explained by increasing rationality (see Regressions 13 and 14). These effects were not significant in Frankfurt. Differences between locations do also show up at separate regressions for common and private information (Regressions 15 – 18). In Frankfurt average thresholds were about 4 lower than in Barcelona in sessions with common information and about 4.7 higher in sessions with private information. The different signs of these coefficients explain why we cannot find a difference in location, when we use combined data from both information conditions.

Non-parametric tests also show a significant difference between sessions starting with T=20 and sessions starting with T=50. Here, we had to use separate tests for each T. While information was significant at 5% in Mann-Whitney-Tests for both T-values, it failed to be significant at 5% for Kolmogoroff-Smirnov-Tests.

When we included sessions with experienced subjects, we observed that experience lowered the mean threshold by about 2.6 (see Regression 5). But, with only four data points for experienced subjects, this difference failed to be significant.

Another surprise was the stability of thresholds across sessions with equal treatments. Regressions 5 – 6 show that more than 80% of the variation of data can be explained by controlled exogenous variables. A large part of this explanatory power comes from the variation of parameters of the payoff function T and Z. However, Regressions 10 and 11 indicate that information and order of treatments explain more than one third of the remaining data variation.

Table 5 gives simple statistics of estimated mean thresholds for treatments with common and private information. Although theory does not give a clear prediction of equilibrium in the common information game, data variation seemed to be about the same for both information conditions.

Payoff for A	20	20	50	50
Information	Private	Common	Private	Common
Average of estimated mean thresholds	41.96	37.84	57.02	52.56
Standard variation	3.92	3.37	3.18	3.88

Table 5. Summary statistic of estimated mean thresholds from sessions with Z=60 and subjects without experience.

Explaining dispersion of individual thresholds: postponed to second version of the paper.

5.4. Testing Theoretical Equilibria

Using the estimated thresholds of the last four periods in each treatment, we test the hypotheses that mean thresholds are either of the theoretically predicted equilibria. We test hypotheses by using models that include only those variables that influence the theoretically predicted equilibrium. We have 11 sessions with common information and inexperienced subjects, generating 22 data points for the different treatments. We use these data to test payoff dominance, Laplacian beliefs, risk dominance, 'naïve' Laplacian beliefs and maximin strategies. In 12 sessions inexperienced subjects had private information, generating 24 data points that we use to test the equilibrium of the private information game. Appendix D exhibits precise results of these tests.

The hypothesis that subjects play Maximin strategies can be most clearly rejected, as all estimated mean thresholds were far below the thresholds associated with Maximin strategies.

The hypothesis that subjects play the payoff dominant equilibrium in sessions with common information is rejected at the 1% level if we jointly use data from treatments with T=20 and T=50. However, for T=50 estimated thresholds came rather close to the payoff dominant equilibrium, two were even lower. Using data from treatments with T=50 only (11 observations), we can still reject the payoff dominant equilibrium at the 5% level. But, if we further restrict the data set to Z=60 (9 remaining observations, 8 degrees of freedom), the p-value is at 10.0%. However, the lower hurdle at Z=100 should lower the thresholds played, and the fact that we cannot reject payoff dominance for this restricted data set seems to be due to the low number of observations.

The hypotheses that subjects play the Laplacian belief or the risk dominant equilibrium could be rejected at a p-level of 1%. A look at Table 8 (Appendix B) reveals that estimated mean thresholds have been below these equilibria in *all* treatments with common information.

The hypotheses that subjects play the 'naïve' Laplacian belief equilibrium could be rejected for all data with Z=60 and for all data with T=20. For data with T=50, the p-value was at 6.8% and did not allow to reject this hypothesis.

The hypothesis that subjects play the unique equilibrium in games with private information was rejected at the 1% level when we used all data from sessions with Z=60. However, for data from treatments with T=20, we could not reject this hypothesis. In fact, it seemed a pretty good predictor here. A look at the data reveals that estimated thresholds are distributed around equilibrium for treatments with private information and T=20, while they are clearly below equilibrium for all treatments with T=50.

In section 5.3 above, we pointed out that average thresholds were higher for higher T or lower Z. This is actually another reason to reject payoff dominance or minimax strategies, because the payoff dominant equilibrium does not depend on Z and the minimax strategy does not depend on T. The other theoretical equilibria follow these parameter changes in the observed direction.

Finally, we use revealed thresholds from Table 4 above, and count, how often revealed thresholds were in a neighbourhood of various theoretical equilibria. Results of this heuristic procedure are summarized in Table 6. The success rate indicates the percentage of treatments from sessions with CI and inexperienced subjects, in which revealed threshold intervals overlap with a neighborhood of 2 around theoretical equilibria.

Equilibrium	All 22 observations	T=20 only (11)	T=50 only (11)
Payoff dominant equilibrium	36%	9%	64%
Laplacian belief equilibrium	18%	36%	0
Risk dominant equilibrium	18%	36%	0
‘naïve’ Laplacian belief equil.	32%	18%	45%
Maximin Strategy	0	0	0
neither of the above	27%	45%	9%

Table 6. Success rates of theoretical equilibrium thresholds +/– 2.

While observed behavior came rather close to payoff dominance in treatments with T=50, risk dominance and Laplacian beliefs were a better approximation to behavior for T=20. Reason might be the higher hurdle for success at low values of Y.

Surprisingly, ‘naïve’ Laplacian beliefs did not bad in this comparison. We can even do better, if we replace the belief that other players choose B with probability ½ by higher probabilities. Maximizing the success rate (as defined above) leads to beliefs of some 0.6 – 0.7 for other players choosing B. To be more precise, if each player beliefs that each other player chooses B with probability p, the best response is a threshold Y, solving $Y\left[1 - Bin\left(\hat{a}(Y) - 2, n-1, p\right)\right] = T$. If we take p = 2/3, we get equilibrium thresholds and according success rates as displayed in Table 7.

Parameters	Z=100, T=20	Z=60, T=20	Z=100, T=50	Z=60, T=50
Best response threshold to p=2/3	23.515	40.00	50.035	52.00
Success cases	0 out of 2	6 out of 9	1 out of 2	7 out of 9
Success rate	54%		73%	

Table 7. Best response thresholds to belief that other players choose B with probability 2/3 and success rates of these thresholds +/– 2.

The overall success rate of thresholds that are a best response to p = 2/3 is 64%. The success rate does not change for p $\in$ [0.6, 0.68] and is lower for any p outside this interval. In F-tests the hypothesis that subjects play a best response to p=2/3 could not be rejected, while all the other theoretical equilibria could (see Appendix D). However, this is not a fair test, as we did not plan to test this equilibrium beforehand, but rather arrived at it endogenously.

Thresholds, associated with a best response to subjects believing that others choose B with probability p=2/3 can explain observed behavior in sessions with common information very well. This might be an artifact of our experiment and might just hold for sessions with Z=60. But, it might also be possible that optimising the success rate by choosing a proper p, and explaining those p-values by parameters of the game, opens a way to measure strategic uncertainty in two-action-games.

6. Conclusions

Previous experiments on coordination games with strategic complementarities have shown that we should distinguish between two kinds of coordination: Coordination on *an* equilibrium and coordination on the *efficient* equilibrium. Comparing our results with those of Van Huck, Battaglio und Beil (1990, 1991), we find some similarities and some clarifications:

- As in their experiment, we find a fast convergence towards an equilibrium in sessions with common information.
- Groups of 14 – 16 did never succeed to reach an equilibrium better than maximin-strategies in the experiment of Van Huyck et al. (1990), where all members were needed to coordinate for this purpose, while coordination by two players was achieved even in a random matching. In our experiment, we never observed an equilibrium that needed coordination of more than 12 out of 15 subjects.
- In their median treatments, behavior always converged to an equilibrium determined by the median of the first round, hinting at extremly strong inertia effects in this sort of games. In our game, it should be easier to observe changes in the equilibrium played, because of random draws and continuous strategy space. Even so, in 24 out of 26 treatments, we did not observe the threshold for success of action B to move. In only two sessions, there was a slight change of the threshold after the first round of a treatment.
- A change of treatment and associated experience with coordination, led subjects to play more efficient equilibria in Van Huyck et al. (1991). We observed similar effects: Experience lowered average thresholds and the order effect might also be explained by experience with coordination.
- Van Huyck et al. found that subjects coordinate on equilibria that are somewhere between maximin-strategies and the payoff dominant equilibrium. However, in their game there is no difference between maximin- and risk dominant strategies. In our experiment, all observed equilibria were between the risk dominant and the payoff dominant equilibrium.

In a previous experiment on the theory of global games Cabrales, Nagel and Armenter (2000) did not find any difference in behavior between sessions with common and private information. However, their stage game had only five possible states and signals and might have been too discrete to discover the subtle effects of information. In our game, with a continuous space for states and signals, we observed that with common information, coordination of agents was much better than with private information. In addition, the average threshold, and thus, the prior probability of failure of the risky action, was significantly higher with private information. On the other hand, we did not find any significant difference in the proportion of subjects using threshold strategies or in the dispersion of achieved mean thresholds across different sessions.

This leads us to conclude that the destabilizing effects of public information, due to existence of multiple equilibria, may be less severe than theory predicts. However, public information does change the threshold, and might therefore increase the probability of successful speculative attacks. In liquidation games as Hubert and Schäfer (2000) or Morris and Shin (2001), the probability of inefficient liquidation should be lower with common than with private information, for the same reason.

The current discussion on the optimal modes of information disclosure concentrates on the multiplicity of equilibria associated with public information. Our experiment suggests that this may be a subordinate effect. The major effect might be that public information reduces strategic uncertainty and thereby leads players to coordinate on an equilibrium with a higher payoff. Efficiency (from the players' viewpoint) may be desirable for some coordination games, e.g. to avoid inefficient liquidation. For others, it may be the opposite. In order to avoid speculative attacks, a central bank should minimize expected gains from speculation. Hence, our experiment suggests that a commitment to provide public information raises the prior probability of a successful speculative attack.

In our view, strategic uncertainty is the major force that drives subjects to play threshold strategies, explains the low variation of observed equilibria in common information games, and also explains most of the comparative statics. We think that the deviation of observed behavior in sessions with private information from Nash-equilibrium into the direction of 'naïve' Laplacian beliefs might also be due to strategic uncertainty that must be added to exogenous uncertainty in these games. Even though we could reject all pre-selected equilibrium concepts, the concepts that considered both, possible gains from coordination as well as the hurdle to achieve these gains, did best in predicting observed comparative statics. In particular, the equilibrium that we called 'Laplacian belief equilibrium', introduced by Carlsson and van Damme (1993a) and Morris and Shin (2000) as limiting equilibrium of global games for diminishing uncertainty of private information, combines the advantages to exhibit comparative statics as we observed them *and* to be easy to calculate. Risk dominance is difficult to calculate in some games, because the tracing procedure may be quite complicated. The 'naïve' Laplacian belief equilibrium fails to react to changes in the payoff function apart from those tried in our experiment, and lacks a theoretical justification.

In our experiment actual behavior in common information settings was restricted by payoff dominance on one side and risk dominance or 'Laplacian beliefs' on the other side. More research needs to be done to explain how strategic uncertainty interferes with these selection criteria. We believe it to be possible to measure the degree of strategic uncertainty that is associated with different games. Going into this direction could help explaining patterns of behavior in multiple equilibrium games and appears to be a promising task for future research.

References

BIS (2001) The New Basel Capital Accord, Consultative Document of the Basel Committee on Banking Supervision.

Cabrales, Antonio, Rosemarie Nagel and Roc Armenter (2000) Equilibrium Selection through Incomplete Information in Coordination Games: An Experimental Study, mimeo, Universitat Pompeu Fabra, Barcelona.

Carlsson, Hans and Eric van Damme (1993a) Global Games and Equilibrium Selection, *Econometrica* 61, 989--1018.

Carlsson, Hans and Eric van Damme (1993b) Equilibrium Selection in Stag Hunt Games, in: K. Binmore, A. Kirman and P. Tani (eds.), *Frontiers of Game Theory,* MIT-Pres, Cambridge, Mass.

Daníelson, Jón et al. (2001) An Academic Response to Basel II, Financial Markets Group Special Paper No. 130, London School of Economics, http://fmg.lse.ac.uk/basel/index.html.

Dönges, Jutta and Frank Heinemann (2001) Competition for Order Flow as a Coordination Game, Goethe-University Frankfurt am Main, Working Paper Series: Finance & Accounting, working paper no. 64, http://games.wiwi.uni-frankfurt.de/personal/Heinemann/publics/cn.htm.

Fischbacher, Urs (1999) z-Tree 1.1.0.: Experimenter's Manual, University of Zurich, Institute for Empirical Research in Economics, http://www.iew.unizh.ch/ztree/index.php.

Frankel, David, Stephen Morris und Ady Pauzner (2000) Equilibrium Selection in Global Games with Strategic Complementarities, Tel Aviv University, http://www.tau.ac.il/~dfrankel.

Goldstein, Itay and Ady Pauzner (2000), Demand Deposit Contracts and the Probability of Bank Runs, Tel Aviv University, http://www.tau.ac.il/~pauzner.

Harsanyi, John C. and Reinhard Selten (1988), *A General Theory of Equilibrium Selection in Games*, MIT--Press.

Heinemann, Frank (2000) Unique Equilibrium in a Model of Self--Fulfilling Currency Attacks: Comment, *American Economic Review* 90, 316--318.

Heinemann, Frank and Gerhard Illing (1999) Speculative Attacks: Unique Sunspot Equilibrium and Transparency, Goethe-University Frankfurt am Main, Frankfurter Volkswirtschaftliche Diskussionsbeiträge, working paper no. 97 (forthcoming in *Journal of International Economics*).

Hellwig, Christian (2000) Public Information, Private Information and Coordination Failures: A Note on Morris and Shin (1998), mimeo, London School of Economics.

Hubert, Franz and Dorothea Schäfer (2001), Coordination Failure with Multiple Source Lending, Freie Universität Berlin, http://www.wiwi.hu-berlin.de/hns/fh/fhpub.htm.

Metz, Christina (2001), Private and Public Information in Self-Fulfiling Currency Crises, Universität Gesamthochschule Kassel, http://www.wirtschaft.uni-kassel.de/michaelis/mitarbeiter/Christina/schriften2.html.

Morris, Stephen and Hyun Song Shin (1998) Unique Equilibrium in a Model of Self--Fulfilling Currency Attacks, *American Economic Review* 88, 587-597.

Morris, Stephen and Hyun Song Shin (2000) Global Games: Theory and Applications, Yale University, http://www.econ.yale.edu/~sm326/research.html.

Morris, Stephen and Hyun Song Shin (2001) Coordination Risk and the Price of Debt, Nuffield College, http://www.nuff.ox.ac.uk/users/Shin/working.htm.

Nagel, Rosemarie (1995) Unraveling in Guessing Games: An Experimental Study, *American Economic Review* 85, 1313-1326.

Obstfeld, Maurice (1996) Models of Currency Crisis with Self--fulfilling Features, *European Economic Review* 40, 1037--1047.

Stahl, Dahl O. and Paul W. Wilson (1994) Experimental Evidence on Players' Models of Other Players, Journal of Economic Behavior and Organization 25, 309-27.

Van Huyck, Lohn B., Raymond C. Battaglio and Richard O. Beil (1990) Tacit Coordination Games, Strategic Uncertainty, and Coordination Failure, *American Economic Review* 80, 234-248.

Van Huyck, Lohn B., Raymond C. Battaglio and Richard O. Beil (1991) Strategic Uncertainty, Equilibrium Selection, and Coordination Failure in Average Opinion Games, *Quarterly Journal of Economics* 106, 885-910.

Appendix A. Instructions

Instructions to participants varied according to different treatments. Here, we present an English translation of instructions for a session with private information, Z=60 and starting with T=20 in full length. For the other sessions instructions were adapted accordingly.

General information

Thank you for your participation in an economic experiment, in which you have the chance to earn money. We ask you not to communicate from now on. If you have a question, then raise your hand, and one of the instructors will come to you.

You are one of 15 persons, who interact with another. The rules are the same for all participants. The experiment consists of 2 stages with 8 independent rounds in each stage. In each round you will receive 10 independent situations, in each of which you have to make a decision (A or B).

Rules of the first stage (the two stages differ only by the payoff for decision A):

Decision situation:

For each situation a number called Y is selected randomly from the interval 10 to 90. This number is the same for all participants. All numbers in the interval [10, 90] have the same probability to be drawn. When you make your decision, you will *not* know the drawn number Y.

However, each participant will receive a hint number for the unknown number Y. This hint number is randomly selected from the interval [Y-10, Y+10]. All numbers in this interval have the same probability to be drawn. Hint numbers of different participants are drawn independently from the same interval

On basis of your hint number you can decide in each situation between two different decisions: A or B.

If you decide for A, then an amount of 20 ECU (Experimental Currency Unit) is credited to your account. This amount is the same for all rounds of the first stage and for all participants (in the second stage the amount is raised to 50 ECU).

If you decide for B, then your payoff depends on how many participants select the same decision B and also depends on how large is the unknown number Y. Decision B is the more successful, the more participants decide for B and the larger the number Y is. If the number of participants who decide for B is at least $20 - Y/4$, then each participant, who decided for B, receives the amount of Y ECU. A more exact explanation of this formula is given with the help of an example and the table at the end of the instructions. If fewer participants decided for B, then those choosing B receive zero ECU.

Once all participants made their 10 decisions for the 10 games, a round is terminated. (Remember there are 8 rounds in each of the two stages)

.

Information after each round

Each participant will be informed after each round for each of the 10 games on

(1) the number Y,
(2) how many participants decided for A or B,
(3) the own payoff and also the total sum of the own payoffs over all 10 games.

Example :

The number of participants is 15. The payoff for A is always 20. The unknown number Y, which was drawn, is 48,65.

The hint numbers drawn for the fifteen participants are: 38.89, 45.24, 42.67, 56.40, 52.92, etc.

The participant with the hint number 38.89 knows that Y is between 28,89 and 48,89,
the participant with the hint number 45.24 knows that Y is between 35,24 and 55,24, etc.

Six participants decide for A, nine participants decide for B.

The participants, who chose A, receive 20 ECU.

In order to receive a positive payoff for B, at least 20-48.65/4 = 7.84 (remember the formula (20-y/4)) participants have to decide for B (that is 8 or more). Since 9 participants selected B, each of them receives Y = 48.65.

For the calculation of the minimum number of the participants needed such that payoff for B is positive see attached table:
Since Y = 48.65, the number of participants must be 8 in order to get a positive payoff for decision B.

Note: You don't know the true value of Y, but you receive a hint number, which is an approximation of Y. Therefore you cannot exactly determine, how many players must select B, in order to get a positive payoff.

For the calculation of the minimum number of participants who have to choose B in order to get a positive payoff for B:

Participants who choose B, receive a positive payoff, only if at least 20-Y/4 participants choose B.

In the right hand column you find the minimal number of participants and in the left column the according intervals for Y.

If the unknown number Y is in the interval, (Note: Y is between 10 and 90)	Then at least … of the 15 participants (including yourself) have to select B, in order to get a positive payoff
20,00 bis 23,99	15
24,00 bis 27,99	14
28,00 bis 31,99	13

32,00 bis 35,99	12
36,00 bis 39,99	11
40,00 bis 43,99	10
44,00 bis 47,99	9
48,00 bis 51,99	8
52,00 bis 55,99	7
56,00 bis 59,99	6
60,00 bis 63,99	5
64,00 bis 67,99	4
68,00 bis 71,99	3
72,00 bis 75,99	2
76,00 bis 90,00	1

Instructions for PC:

Each round is divided into a decision phase and into an information phase. During the decision phase the screen shows the current round in the heading line (period). The second line informs you about the sure payoff for decision A. The following table shows your hint number for each game in the left column. In the right column you must click which decision you want to select. Once you decided for all 10 games, you must press the red OK button. As long as you have not pressed the red button, you can still modify your decisions. When exceeding the time limit you are reminded to make your decisions.

When all participants have pressed the OK-button, the decision phase of a round is terminated and the information phase begins. The display in the information phase indicates line by line for each situation of this round the true value Y, the number of players, who decided for B, your own decision, and the change of your account balance. After the time limit the next round starts. In addition you can leave the information phase beforehand through the gray OK button. After leaving the information screen you have no more possibility to inform yourself about passed decisions.

Questionnaire:

At the end of the experiment (after the second stage) we ask you to fill out a questionnaire. The personal data asked for are treated strictly confidential and used for research purposes only.

Payoffs:
Also at the end of the experiment the ECUs you have obtained are converted into DM [Pesetas] and paid in cash. 1 ECU corresponds to 0,5 Pfennig [3.5 Pesetas], so that 200 [100] ECU are converted to 1 DM [35 Pesetas].

In Sessions with common information, the first part of the description of the decision situation was replaced by

For each situation a number called Y is drawn randomly from the interval 10 to 90. This number is the same for all participants. All numbers in the interval [10, 90] have the same probability to be drawn.

On the basis of your this number you can decide in each situation between two different decisions: A or B.

The remaining text was adapted to common information accordingly.

In sessions that started with T=50, we only changed these parameters.

Appendix B: Results of Logistic Regression

Table 8 gives detailed information on aggregate behavior of all sessions and treatments.

1	2	3	4	5	6	7	8	9	10	11	12
session	Z	location	Experience	Information	order	T	Average number of ‘rational’ subjects	Parameter estimation		Estimated mean	Estimated standard deviation
								a	b	a/b	
001115	100	Frankf.	no	PI	20/50	20	14.25	4.78	0.146	32.75	12.43
						50	14	10.54	0.185	56.91	9.79
001207	100	Frankf.	no	PI	50/20	50	14.5	10.63	0.199	53.53	9.13
						20	14.875	9.35	0.349	26.77	5.19
001206	100	Frankf.	no	CI	20/50	20	14	8.96	0.271	33.03	6.68
						50	14.75	62.18	1.168	53.25	1.55
010131	100	Frankf.	no	CI	50/20	50	13.625	9.67	0.184	52.66	9.87
						20	14.6	9.04	0.462	19.57	3.93
010201	60	Frankf.	no	PI	20/50	20	13.5	5.43	0.117	46.40	15.50
						50	13.625	7.48	0.124	60.21	14.59
010321a	60	Frankf.	no	PI	50/20	50	12.375	7.67	0.131	58.45	13.82
						20	14.125	7.05	0.151	46.62	11.99
010523	60	Frankf.	no	PI	50/20	50	14	12.75	0.210	60.75	8.64
						20	14.875	10.18	0.246	41.42	7.38
010530a	60	Frankf.	no	PI	20/50	20	13.625	7.42	0.167	44.59	10.89

						50	14.75	14.86	0.247	60.10	7.34
010207	60	Frankf.	no	C I	20/50	20	14.375	8.18	0.212	38.52	8.54
						50	14.625	24.74	0.434	56.96	4.18
010321b	60	Frankf.	no	C I	50/20	50	12.5	7.60	0.166	45.75	10.92
						20	14	9.27	0.285	32.57	6.38
010530b	60	Frankf.	no	C I	50/20	50	14.625	13.19	0.280	47.16	6.49
						20	14.875	4.90	0.154	31.90	11.82
0529LN	60	Barcel.	no	P I	20/50	20	12.375	7.80	0.183	42.72	9.93
						50	13	7.79	0.144	54.07	12.60
0530L1	60	Barcel.	no	P I	50/20	50	13.875	12.62	0.234	54.00	7.76
						20	14.5	9.26	0.255	36.28	7.11
0530P3	60	Barcel.	no	P I	20/50	20	11.375	7.48	0.167	44.77	10.85
						50	14.375	13.41	0.262	51.19	6.93
0607L6	60	Barcel.	no	P I	50/20	50	13.5	9.47	0.173	54.73	10.48
						20	14	9.63	0.252	38.25	7.20
0606L8	60	Barcel.	no	P I	20/50	20	13.75	7.51	0.173	43.48	10.51
						50	14.125	12.24	0.238	59.87	7.62
0608LA	60	Barcel.	no	P I	50/20	50	13.5	13.01	0.229	56.83	7.92
						20	14.125	6.94	0.198	35.02	9.15
0531PB	60	Barcel.	no	C I	20/50	20	12.5	6.44	0.165	38.93	10.97
						50	13.25	9.67	0.193	50.16	9.41
0607P9	60	Barcel.	no	C I	50/20	50	12.5	15.78	0.289	54.55	6.27
						20	14.625	9.33	0.249	37.50	7.29
0606PA	60	Barcel.	no	C I	20/50	20	14.25	18.99	0.472	40.24	3.84
						50	14.875	33.32	0.618	53.88	2.93
0614L9	60	Barcel.	no	C I	50/20	50	11.375	9.05	0.164	55.16	11.05
						20	14.25	12.21	0.288	42.36	6.29
0608PE	60	Barcel.	no	C I	20/50	20	12.375	6.11	0.149	41.14	12.21
						50	12.875	9.88	0.173	57.30	10.51
0614P9	60	Barcel.	no	C I	50/20	50	13.25	15.09	0.290	52.08	6.26
						20	14.625	16.30	0.436	37.41	4.16
010516b	60	Frankf.	yes	C I	20/50	20	14.375	12.18	0.370	32.94	4.91

						50	14.625	19.87	0.346	57.38	5.24
010516a	60	Frankf.	yes	CI	50/20	50	14.625	57.50	1.146	50.19	1.58
						20	14.75	16.04	0.516	31.08	3.51

Table 8. The first row is the session number. The next five rows give session specific conditions. Row 7 indicates the treatment specific payoff to action A. Row 8 gives the average number of subjects, whose behavior was consistent with undominated threshold strategies in all 8 rounds of the treatment. Rows 9 and 10 are results of logistic regressions based on data of the last four rounds of each treatment. Rows 11 and 12 show the estimated mean and standard variation of mean thresholds, calculated from estimates a and b.

The closer the estimated mean a/b came to the payoff for the secure action T, the closer came subjects towards the payoff dominant equilibrium. A low estimated standard deviation indicates a high degree of coordination across subjects within the last rounds of a treatment.

Appendix C: Results of Linear Regressions

We use linear regressions to explain the average number of subjects, whose behavior is consistent with an undominated threshold strategy, and to explain the summery statistics obtained by logistic regressions for each session and treatment.

Variables are defined as is displayed in Table 9. Tables 10 and 11 give the results of some linear regressions:

Name	Nature	Definition	
T	integer	payoff for action A	
Z	dummy	0: session with Z=100	1: session with Z=60
Location	dummy	0: session in Barcelona	1: session in Frankfurt
Experience	dummy	0: session with inexperienced subjects	1: session with experienced subjects
Information	dummy	0: session with common information	1: session with private information
Order	dummy	0: session starting with T=50	1: session starting with T=20
Treatment	dummy	0: second treatment	1: first treatment
Rationality	number	Average number of subjects whose behavior is consistent with an undominated threshold strategy	
a	number	Results from logistic estimation on basis of last four rounds	
b	number	Results from logistic estimation on basis of last four rounds	
Mean	number	a / b = estimated mean threshold	
Standvar	number	$\pi /(b\sqrt{3})$ = estimated standard deviation of individual thresholds	

Table 9. Variables used in linear regression.

Regressions explaining Rationality

No.	Data source (Number of observations)	Explanatory variables: Coefficients of included explanatory variables (t-values)								R^2
		Intercept	T	Z	Location	Experience	Information	Order	Treatment	adjusted R^2
1	All sessions (50)	14.576 (31.74)	-0.0066 (-0.97)	-0.3496 (-1.09)	0.5066 (2.08)	0.6297 (1.47)	0.0227 (0.11)	-0.1969 (-0.96)	-0.8760 (-4.28)	0.4579 0.3676
2	All sessions (50)	13.911 (78.42)			0.6438 (3.05)	0.4813 (1.24)			-0.8840 (-4.38)	0.4198 0.3819
3	Inexperienced subjects (46)	13.941 (73.85)			0.6438 (2.99)				-0.9446 (-4.39)	0.3960 0.3679

Table 10. Regressions with Rationality as dependent variable.

Regressions explaining Mean Thresholds

No.	Data source (Number of observations)	Explanatory variables: Coefficients of included explanatory variables (t-values)									R^2
		Intercept	T	Z	Location	Experience	Information	Order	Treatment	Rationality	Adj. R^2
4	All sessions (50)	23.503 (1.82)	0.5694 (14.67)	6.700 (3.66)	1.042 (0.72)	-2.7995 (-1.13)	3.8439 (3.19)	3.5074 (3.00)	0.749 (0.54)	-0.521 (-0.60)	0.866 0.840
5		17.292 (8.20)	0.5745 (15.19)	6.389 (4.09)		-2.6118 (-1.19)	3.8768 (3.27)	3.5687 (3.14)			0.861 0.845
6	Inexperienced subjects (46)	17.730 (8.17)	0.5613 (14.05)	6.387 (4.04)			3.8806 (3.23)	3.4852 (2.90)			0.851 0.836
7	Treatments with T=20, inexperienced subjects (23)	23.580 (12.18)		12.012 (6.51)			3.7643 (2.69)	5.1357 (3.67)			0.770 0.734
8	Treatments with T=50, inexperienced subjects (23)	51.172 (25.63)		0.761 (0.40)			3.9969 (2.77)	1.8348 (1.27)			0.340 0.236
9	Z=60, inexperienced subjects (38)	26.495 (16.29)	0.4966 (13.24)				4.1241 (3.65)	2.9719 (2.63)			0.853 0.840
10	Z=60, T=20, inexperienced subjects (19)	35.9912 (27.91)					3.8827 (2.56)	4.1613 (2.74)			0.482 0.417
11	Z=60, T=50, inexperienced subjects (19)	51.763 (35.56)					4.3654 (2.55)	1.7826 (1.04)			0.330 0.247
12	Barcelona sessions only (24)	28.659 (17.30)	0.4881 (12.84)				0.8750 (0.77)	1.9650 (1.72)			0.894 0.878
13		29.024 (19.06)	0.4881 (13.12)						2.112 (1.89)		0.893 0.883

14	(24)	45.679 (5.76)	0.4810 (12.98)							-0.140 (2.00)	0.895 0.885
15	CI, inexperienced subjects (22)	22.071 (5.86)	0.5628 (9.59)	3.126 (1.17)	-3.973 (-1.90)			3.65875 (2.05)			0.863 0.831
16	CI, Z=60 inexperienced subjects (18)	28.118 (14.55)	0.4905 (10.76)		-4.102 (-2.79)			2.8819 (2.07)			0.903 0.882
17	PI, inexperienced subjects (24)	16.742 (5.88)	0.5599 (12.32)	9.827 (4.80)	4.717 (3.09)			2.8675 (2.10)			0.905 0.885
18	CI, Z=60 inexperienced subjects (20)	28.772 (16.6)	0.5022 (12.50)		4.717 (3.83)			2.5050 (2.08)			0.916 0.901

Table 11. Regressions with Mean as dependent variable.

Appendix D: Testing Theoretical Equilibria

The payoff dominant equilibrium depends on T, but not on Z. Using all 22 data from CI sessions with inexperienced subjects, we use the model $Mean_j = a + b\,T_j + u_j$. The payoff dominant equilibrium predicts $a = 0$ and $b = 1$. Thus our null Hypothesis is $H_j^0 = T_j$. Estimated coefficients in this model are $\hat{a}$ =24.486 and $\hat{b}$ = 0.563. The test statistic is defined by

$$F = \frac{\sum_j (Mean_j - H_j^0)^2 - \sum_j \hat{u}_j^2}{\sum_j \hat{u}_j^2}\;\frac{m-k}{k},$$

where m is the number of observations (22) and k is the number of regressors in the model (2). The distribution of the test statistic is assumed to be $F \sim F(k, m-k)$. Let $\Phi(F, k, m-k)$ be the value of the cumulative F-distribution at F. We reject Hypothesis H^0, if $\Phi(F, k, m-k)$ < 5%. Maximin strategy does only depend on Z. Other theoretical equilibria depend on both, T and Z. Here, we test hypotheses accordingly using either all data with the same Z or all data with the same T. Tables 12 and 13 give the results of these tests.

Hypothesis	Data from CI sessions with inexperienced subjects	m	Model	k	$\Phi(F,k,m-k)$	Result
Payoff dominant equilibrium	All	22	$Mean_j = a + b\,T_j + u_j$	2	0.000	Reject
	T=20	11	$Mean_j = a + u_j$	1	0.000	Reject
	T=50	11	$Mean_j = a + u_j$	1	0.040	Reject

	T=50, Z=60	9	$Mean_j = a + u_j$	1	0.100	Accept
Laplacian belief equilibrum	Z=60	18	$Mean_j = a + b\ T_j + u_j$	2	0.000	Reject
	T=20	11	$Mean_j = a + b\ Z_j + u_j$	2	0.005	Reject
	T=50	11	$Mean_j = a + b\ Z_j + u_j$	2	0.000	Reject
Risk dominant equilibrium	Z=60	18	$Mean_j = a + b\ T_j + u_j$	2	0.000	Reject
	T=20	11	$Mean_j = a + b\ Z_j + u_j$	2	0.004	Reject
	T=50	11	$Mean_j = a + b\ Z_j + u_j$	2	0.000	Reject
‘naïve’ Laplacian belief equilibrium	Z=60	18	$Mean_j = a + b\ T_j + u_j$	2	0.000	Reject
	T=20	11	$Mean_j = a + b\ Z_j + u_j$	2	0.000	Reject
	T=50	11	$Mean_j = a + b\ Z_j + u_j$	2	0.068	Accept
Best response to belief that others play B with probability 2/3	Z=60	18	$Mean_j = a + b\ T_j + u_j$	2	0.252	Accept
	T=20	11	$Mean_j = a + b\ Z_j + u_j$	2	0.309	Accept
	T=50	11	$Mean_j = a + b\ Z_j + u_j$	2	0.542	Accept
Maximin Strategy	All	22	$Mean_j = a + b\ Z_j + u_j$	2	0.000	Reject

Table 12. F-tests for selection criteria in sessions with common information.

Hypothesis	Data from PI sessions (inexperienced subjects)	m	Model	k	$\Phi(F, k, m-k)$	Result
Private information equilibrium	Z=60	20	$Mean_j = a + b\ Z_j + u_j$	2	0.000	Reject
	T=20	12	$Mean_j = a + b\ Z_j + u_j$	2	0.682	Accept
	T=50	12	$Mean_j = a + b\ Z_j + u_j$	2	0.000	Reject

Table 13. F-tests for equilibrium in sessions with private information.