Information quality and regime change: Evidence from the lab*

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Abstract

We experimentally test the effects of information quality in a global game of regime change. The game features a payoff structure such that more dispersed private information induces agents to attack more often and reduces regime stability in the Bayesian Nash Equilibrium. We show that subjects in the lab do not play as predicted by equilibrium theory. Instead, more dispersed information makes subjects more cautious, increasing regime stability. We show that this finding is consistent with a modified global game model in which agents engage in level-k thinking. In the level-k model, information quality affects agents' actions through a novel channel, that enables a strategic attenuation effect. As information quality worsens, strategic complementarities between different level-k types weaken, generating a force that is capable of reversing the comparative statics from the equilibrium model.

Keywords: global games, information quality, level-k thinking, across-type strategic complementarity, finite mixture model.

JEL Codes: C72, C9, D82, D9

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1 Introduction

Global games of regime change are commonly used to analyze important economic phenomena involving elements of coordination, such as currency crises, bank runs, and political change.[1] A central question in this literature is how information quality – the precision of agents' private information – affects the probability of a successfully coordinated attack. Understanding the particular role of information quality is important from both theoretical and applied perspectives. For example, in contexts such as speculative currency attacks or debt rollover crises, it can inform policymakers about the likely effects of various disclosure policies on financial stability.

In this paper we experimentally test how a change in private information precision affects regime stability in a standard global game of regime change. Specifically, we let several groups of subjects play a series of games, in which they take binary decisions – attack or not attack. Each subject's payoff from attacking depends on both an underlying state and the actions of the other subjects in the group. If a sufficient number of agents choose to attack (given the value of the underlying state), then all attacking agents obtain a discretely higher payoff relative to not attacking. In addition, the higher the value of the state, the higher the payoff gain from a successful attack.[2] Finally, subjects obtain private signals about the underlying state with some noise. We set up the payoffs of subjects to correspond to the literature on speculative currency attacks, where theory predicts that higher private information dispersion makes agents more likely to attack in equilibrium, decreasing regime stability (Heinemann and Illing, 2002, Iachan and Nenov, 2015).[3] In this setting, we compare subjects' behavior in two treatments: one where private information dispersion is low ("Low

[1]See Morris and Shin (1998) for currency crises, Rochet and Vives (2004) and Goldstein and Pauzner (2005) for bank runs, and Edmond (2013) for political change.

[2]Therefore, a higher state in our abstract game can be interpreted as a lower value of a common economic fundamental.

[3]In general, the effect of a change in private information dispersion on regime stability is ambiguous and depends on the payoff structure that is generated by the underlying economic environment (Iachan and Nenov, 2015). To provide a clear theoretical prediction to test in a laboratory setting, we opt for a specific payoff structure that leads to the aforementioned comparative static.

Noise treatment") and one where the information dispersion is high ("High Noise treatment").

The observed behavior in both treatments is consistent with subjects following cutoff strategies – attacking whenever their private signal is above a specific value – as predicted by the theory. However, contrary to the equilibrium prediction described above, subjects tend to play according to cutoff strategies with *lower* cutoffs in the Low Noise treatment compared to the High Noise treatment. This is true both for individual subject cutoffs, which tend to follow a distribution that is shifted to the right in the High Noise treatment relative to the Low Noise treatment, as well as for average cutoffs across groups, which tend to be *lower* in the Low Noise treatment compared to the High Noise treatment. Therefore, in a lab environment, noisier private information makes agents less, not more, aggressive, contradicting the baseline equilibrium theory.

We then argue that a theoretical framework in which players have limited depth of reasoning is both qualitatively and quantitatively consistent with our main experimental result. In particular, we focus on one specific non-equilibrium theory that has received recent experimental and theoretical attention in the literature on global games and informational frictions (Kneeland, 2016, Angeletos and Lian, 2017): level-k thinking (Nagel, 1995; Stahl and Wilson, 1995).[4] Models of level-k thinking assume that agents have limited depth of reasoning and, at the same time, provide a specific structure to agents' beliefs. Despite this deviation from the equilibrium theory, agents still follow cutoff strategies, similar to the equilibrium model. Moreover, agents with higher depth of reasoning (higher level-k types) play according to cutoff strategies that are closer to the equilibrium cutoff, compared to agents with lower depth of reasoning (lower level-k types), whose behavior is influenced by the perceived play of a fictitious and fully behavioral $L0$ type. In that sense, the level-k global games model naturally extends to the equilibrium global games model to incorporate possible bounded rationality in the behavior by experimental subjects.

We show, however, that this departure from equilibrium play can significantly alter the

[4] An overview of models and evidence of non-equilibrium strategic thinking is provided in Crawford, Costa-Gomes, and Iriberri (2013).

prediction of the standard global games model. Specifically, with level-k thinking, the effect of more dispersed information on agents' actions and regime stability can be reversed for low level-k types, compared to the equilibrium model and consistent with the experimental results. Intuitively, with level-k thinking, different cognitive types have different strategic cutoffs. At the same time, there is a strategic complementarity across subsequent level-k types, which anchors the aggressiveness of a specific cognitive type to that of the preceding cognitive type. Therefore, an aggressive agent with relatively low depth of reasoning, makes the cognitive type above her more aggressive, which in turn increases the aggressiveness of the subsequent cognitive type, and so on.

In that environment, higher information dispersion acts to attenuate the across-type strategic complementarity and de-anchor the behavior of different cognitive types. This is because more dispersed information makes agents less coordinated when attacking and reduces their ability to forecast the actions of other agents, thus flattening the best response function that links an agent's strategic cutoff to the strategic cutoff that she believes other players are using. Therefore, whenever lower cognitive types are relatively more aggressive, the de-anchoring induced by more dispersed private information acts towards reducing the higher cognitive types' willingness to attack. We call this de-anchoring effect of information dispersion *strategic attenuation*. If the strategic attenuation effect is strong enough, some cognitive types actually become less aggressive with higher information dispersion.

To assess whether the level-k model is quantitatively consistent with our experimental findings, we follow Kneeland (2016) and structurally estimate a finite mixture model of play with different level-k types, using data from both treatments. We make two assumptions that allow the novel strategic attenuation effect to play a countervailing role and to be quantitatively relevant. First, and in line with previous work on global games with level-k types (Kneeland, 2016), we assume that $L0$ types play more aggressively than uniform randomization.[5] Second, we allow for risk aversion by assuming that agents have constant relative

[5]Assuming that $L0$ types randomize uniformly in a global game leads to the global game equilibrium prediction (see e.g. Morris and Shin (2003)). Therefore, global games models with level-k agents, such as

risk aversion (CRRA) preferences over payoffs obtained in each decision round and jointly estimate the coefficient of relative risk aversion together with the other model parameters.[6]

In our baseline structural estimation, we estimate a common distribution of level-k types for both treatments, assuming fixed play by $L0$ types across the treatments. Therefore, our results are not driven by variation in $L0$ types' perceived play or different distributions of the level-k types across the two treatments but are purely due to the effect of information dispersion on actions in our modified level-k model.

We estimate a large share (around 73%) of level-k types, which results in a weighted-average of strategic cutoffs in both treatments in line with the estimated average cutoffs in the experiment. A caveat for comparing these averaged cutoffs is that they do not incorporate trembles, which our estimated model allows for. We therefore use our estimated model to simulate the outcomes of many sessions for each of the two treatments. Around 98% of our simulated sessions result in a negative cutoff difference, with around 15% of simulated sessions having a cutoff difference equal to or larger (in magnitude) than the average cutoff difference in the experimental data. Therefore, our structural estimation shows that a level-k model, augmented with relatively aggressive $L0$ types and risk averse agents can quantitatively explain the observed differences in the strategic cutoffs in our experiment.

Related literature

We next detail our contribution relative to the experimental literature on global games. Initial coordination experiments focused on static games with complete information (Cooper, DeJong, Forsythe, and Ross, 1990, 1992; Straub, 1995; Van Huyck, Battalio, and Beil, 1990).

Kneeland (2016) assume that $L0$ types deviate from uniform randomization. Moreover, as us, Kneeland (2016) assumes that $L0$ types play more aggressively than uniform randomization, in line with the literature on experimental coordination games which shows that initial play tends to be biased towards the payoff dominant actions.

[6] Allowing for some risk aversion helps match the level of strategic cutoffs across treatments. Risk aversion on its own cannot reverse the comparative statics in a standard global game model, since it only weakens the equilibrium effect of information dispersion in that model. Furthermore, we estimate a coefficient of relative risk aversion of 0.48, which is broadly in line with other studies estimating risk attitudes based on data from experimental games, individual decision making experiments, as well as field studies. See Section 5.2 for further discussion.

Such games have multiple equilibria and strategic uncertainty comes to the forefront. As a response to this indeterminacy, the theory of global games was initially developed by Carlsson and van Damme (1993). The theory was later advanced by Morris and Shin (1998) to macroeconomic applications. The global games framework provides an explicit model of strategic uncertainty. It shows that coordination games with multiple equilibria under complete information may have a unique equilibrium if certain parameters of the payoff function are private information instead of common knowledge.

Much of the experimental literature on global games has focused on comparing the effects of private versus public (or common) information. Heinemann, Nagel, and Ockenfels (2004) tests the predictions of the theory of global games under both private and public information.[7] In the unique global games equilibrium agents use monotone cutoff strategies. Heinemann, Nagel, and Ockenfels (2004) show that subjects tend to use such strategies, both under public and private information.[8,9] Moreover, and contrary to the standard theory, they show that subjects are more coordinated under public information and play strategies close to the payoff-dominant equilibrium. Cabrales, Nagel, and Armenter (2007) test the global games theory in a series of two-person games with a simplified information structure. The design ensures that equilibrium is reached after only four rounds of elimination of (interim) strictly dominated strategies. Similar to Heinemann, Nagel, and Ockenfels (2004), they find

[7]There is a rich experimental follow up literature. Duffy and Ochs (2012), Shurchkov (2013), and Shurchkov (2016) study coordination experimentally in dynamic global games. Heinemann and Moradi (2018) provide conditions for a unique equilibrium where agents follow a sunspot announcement depending on the realization of an informative private signal, and compare this equilibrium to a global games equilibrium in an experiment. They find that observed behavior converges to the global games equilibrium in the majority of groups, while one third of the groups coordinate on the sunspot equilibrium. Heinemann (2018) solves for the global games selection with asymmetric players, and runs treatments with symmetric and asymmetric players in an experiment. He finds that the global-games selection predicts actions well with symmetric players but fails miserably with asymmetric players.

[8]As in that paper, we also find that agents use monotone cutoff strategies. Widespread use of monotone, or near monotone, cutoff strategies has also been documented in a broader class of global games experiments (Cornand and Heinemann, 2014; Avoyan, 2017; Szkup and Trevino, 2020). Heinemann, Nagel, and Ockenfels (2009) develop a method to measure strategic uncertainty as an alternative to varying the parameters of the game exogenously. They also find widespread use of cutoff strategies. Heggedal, Helland, and Joslin (2018) find widespread use of cutoff strategies in a coordination game with type uncertainty rather than uncertainty about fundamentals.

[9]Cornand (2006) expands on Heinemann, Nagel, and Ockenfels (2004) by demonstrating that public information is stabilizing in an experiment using a currency attack game.

that subjects converge to the unique global games equilibrium under private information, but that under public information they tend to play closer to the payoff-dominant equilibrium. We complement these papers by comparing how different levels of *private* information quality affect experimental play in a global game of regime change, thus testing a different prediction of the theory of global games compared to this previous literature.[10,11]

Kneeland (2016) analyses global games in which agents engage in level-k thinking as a way of rationalizing the findings in Heinemann, Nagel, and Ockenfels (2004). Using experimental data from Heinemann, Nagel, and Ockenfels (2004), she shows that the level-k model fits the data better than the fully rational model. Relative to her paper, we provide a novel prediction on the effect of changes in information dispersion on different cognitive types' strategic cutoffs and show experimental support for this prediction.[12]

In a recent contribution, Szkup and Trevino (2020) also consider an experimental setting in which the precision of private signals varies across treatments. Like us, they find that the comparative statics are reversed relative to what the theory predicts. To explain the reversal in the theoretical comparative statics Szkup and Trevino (2020) argue that there is a link between players' perception of strategic uncertainty and fundamental uncertainty. They propose a "sentiment theory", where as fundamental uncertainty increases, players also become more pessimistic about the actions of others.

We complement Szkup and Trevino (2020) along several dimensions. First, we differ in the experimental setting. Specifically, they investigate a two-player investment game similar to Carlsson and van Damme (1993), while we consider a larger coordination game of regime change. Second, and more importantly, we explain the reversed comparative statics with a theory based on bounded rationality and limited depth of reasoning. In that theory,

[10] If one is to interpret public information in experimental global games as a situation with (arbitrarily) precise private information, then the findings in Heinemann, Nagel, and Ockenfels (2004) and Cabrales, Nagel, and Armenter (2007) that agents tend to play more aggressively under public information are consistent with our experimental findings.

[11] For further analysis of the effects of precision in public signals see Baeriswyl and Cornand (2016), and for relative precision of private and public signals see Dale and Morgan (2012).

[12] Cornand and Heinemann (2014) analyze the relative weighting of public and private signals in a global game by a k-level model and a cognitive hierarchy model.

we identify a novel effect of information quality on agents' actions, which is absent in the equilibrium model. We then evaluate the ability of this theory to explain the data.

2 Information quality and the payoff sensitivity effect

We consider a regime-change game that can serve as a simple representation of the strategic interactions involved in currency crises (Morris and Shin, 1998), debt rollover (Rochet and Vives, 2004, Goldstein and Pauzner, 2005), and political change (Edmond, 2013). We follow the notation from Iachan and Nenov (2015), with a few modifications necessary for the experimental test of the effect of information dispersion on players' actions and regime stability. Most importantly, we assume that there is a discrete number N of players.

Agents take a binary action $s_i \in \{0, 1\}$ simultaneously. We interpret $s_i = 1$ as player i attacking the status quo. We let $Z = \sum_i s_i$ denote the number of agents who choose $s_i = 1$. A state variable Y (the fundamentals) determines agents' payoffs, and also the minimal number of agents required for a successful attack. We assume that Y is distributed uniformly on $[0, M]$, for $M > 0$ and is not directly observed by agents, who hold this distribution as their prior belief about the state.

Regime change occurs if at least a fraction $g(Y)$ of agents attack, where $g(\cdot)$ is a *decreasing* function of the fundamentals.[13] We define $G_N(Y) \equiv \lfloor g(Y) N \rfloor$, so that regime change occurs if, and only if, $Z \geq G_N(Y)$. We assume that the net payoff to a player from choosing $s_i = 1$ over $s_i = 0$ is $D(Y)$ in case of regime change and $U(Y)$ in case of status quo survival. We assume that $D(Y) > 0$ and $U(Y) < 0$ and that both are either constant or strictly increasing in Y. As a consequence, actions are strategic complements.

Before choosing actions, agents observe noisy signals about the state Y. Specifically, we assume that player i observes a signal $x_i = Y + \eta_i$, where η_i's are distributed uniformly on

[13] Therefore, higher Y means weaker fundamentals in this setting. Furthermore, to ensure equilibrium uniqueness, we assume that there exist upper and lower dominance regions of the following form: There exists a $\underline{Y} \in (0, M)$ and a $\overline{Y} \in (0, M)$, such that for $Y < \underline{Y}$, $g(Y) > 1$ and for $Y > \overline{Y}$, $g(Y) < 0$. For $Y \in (\underline{Y}, \overline{Y})$, $g(Y) \in (0, 1)$.

$[-\epsilon, \epsilon]$, $\epsilon > 0$, and $\epsilon \ll M$. The draws of η_i are independent across players and, also, independent of the realization of Y. We denote the expectation with respect to the information set of an agent that receives signal x_i by $E_{x_i}[\cdot]$.

Equilibrium. The definition of a Bayesian Nash Equilibrium for our game is standard (see Morris and Shin (2003)). We restrict attention to equilibria in monotone strategies. A monotone strategy Y^* is such that $s(x_i) = 1$ iff $x_i > Y^*$. In that case it is straightforward to apply standard results from global games to show that there is a unique equilibrium. Furthermore, the restriction is without loss of generality (Morris and Shin, 2003).

We call the critical value Y^* the *strategic cutoff.* Note that for a given value Y^* in the finite-player case, the number of players who observe a signal above Y^* and thus choose $s_i = 1$ is stochastic. Given a value of the fundamental Y, with signals uniformly distributed on the interval $[Y - \epsilon, Y + \epsilon]$, the probability that at least K players get a signal above Y^* is given by the tail distribution of a Binomial random variable

$$\overline{F}_N(K, Y, Y^*) = \sum_{k \geq K}^{N} \left(\begin{array}{c} N \\ k \end{array} \right) p(Y, Y^*, \epsilon)^k (1 - p(Y, Y^*, \epsilon))^{N-k} \tag{1}$$

where

$$p(Y, Y^*, \epsilon) = \min\left\{ \max\left\{ 0, \frac{Y + \epsilon - Y^*}{2\epsilon} \right\}, 1 \right\} \tag{2}$$

Therefore, the probability of regime change given a state Y is

$$P(Y, Y^*) \equiv \overline{F}_N(G_N(Y), Y, Y^*) \tag{3}$$

Note that $P(Y, Y^*) = 1$ for $Y \geq Y^* + \epsilon$ and $P(Y, Y^*) = 0$ for $Y \leq Y^* - \epsilon$. It is also convenient to define the probability of regime change for a player that attacks ($s_i = 1$). We define this probability by $\tilde{P}(Y, Y^*)$. Specifically, a player that attacks expects regime change to occur if at least $G_N - 1$ of the remaining $N-1$ other players attack, which gives

$$\tilde{P}\left(Y, Y^*\right) \equiv \overline{F}_{N-1}(G_N(Y) - 1, Y, Y^*) \tag{4}$$

Given this probability of regime change, Y^* is determined by an indifference condition for a marginal agent – a player who observes a signal $x_i = Y^*$. Specifically, Y^* solves

$$E_{Y^*}\left[D\left(Y\right)\tilde{P}\left(Y, Y^*\right) + U(Y)\left(1 - \tilde{P}\left(Y, Y^*\right)\right)\right] = 0. \tag{5}$$

That is, for a marginal agent, the expected payoff from attacking over not attacking is equal to zero.

Payoff sensitivity effect. As shown by Iachan and Nenov (2015), with a continuum of players, the effect of information quality on the equilibrium of this game depends on a comparison of the sensitivities of payoffs in the case of regime change and status quo survival. In our experiment, we focus on the case where $U\left(Y\right) = U < 0$ and $D\left(Y\right)$ is strictly increasing in Y. The prediction of the model that we aim to test experimentally is the comparative static of Y^* with respect to ϵ. In the context of a continuum of players, increased information dispersion is destabilizing (Iachan and Nenov, 2015). Put differently, if $U(Y) = U < 0$, $\forall Y$, and $D(Y)$ is strictly increasing, then $\frac{\partial Y^*}{\partial \epsilon} < 0$, so agents are more aggressive when attacking. This comparative static continues to hold in our specific experimental set-up with a finite number of players as Figure 4 in Section 5.1 illustrates. Below we refer to this effect of information dispersion on the strategic cutoff Y^*, the *payoff sensitivity effect.* The intuition for this effect is the following.

Under imperfect information, the expected net payoff associated with regime change provides incentives to attack, while the expected net payoff in the case of status quo survival provides opposing incentives. Less precise information makes extreme realizations of the fundamentals more likely, changing expected net payoffs. Whenever the net payoff from attacking increases more strongly with fundamentals than the net payoff in case of regime survival, the first force dominates and less precise information make agents more likely to attack.

3 Experimental implementation

In order to test the payoff sensitivity effect in the lab, we follow closely Heinemann, Nagel, and Ockenfels (2004).[14] The experiment consists of a series of 8 independent rounds. In each round each subject makes 10 independent binary choices. We organize subjects in groups of $N = 10$, with subjects indexed by i. Subjects stay in the same group for all eight rounds. The rules of the game are made public knowledge through the reading of instructions aloud.[15] Unique subjects are used in all sessions. The language of the experiment is neutral.

At the beginning of each round, 10 different values of Y are drawn, where Y is distributed uniformly on $[0, 100]$. For any realization of Y, individual signals x_i are then drawn independently according to a uniform distribution on $[Y - \epsilon, Y + \epsilon]$. Each individual signal is revealed to subject i but not to the other subjects in the group. Within a treatment and a given round, the list of fundamentals (Y) are identical for the subjects in different groups, while the list of signals (x_i) varies over subjects. Given their signals, subjects are asked to make a decision (A or B) for each of the 10 decision situations in that round. In the context of the model outlined above, A corresponds to $s_i = 0$ and B corresponds to $s_i = 1$. Subjects get a feedback after each round. For each of the 10 games on the list, this feedback consists of the number Y, the number of subjects that decided for A and B, and the subject's own payoff. Subjects earn profits for each decision taken in the experiment.[16]

[14] We adopt the same payoff functions and other parameters as in their (T=20; Z=60) treatments. Our experiment is based on the same zTree files and the same instructions as their experiment. The only differences, aside from the subject pool, is that we follow Heinemann, Nagel, and Ockenfels (2009) in considering groups of 10 rather than 15 subjects, and that we replace the complete information treatment of Heinemann, Nagel, and Ockenfels (2004) with our High Noise treatment.

[15] Instructions for the High noise treatment are available at: http://www.leifhelland.net/working-papers/ and also in the Online Appendix.

[16] An alternative would be to draw one decision in the experiment randomly and pay subjects for the

If a subject chooses A, she receives an endowment of 20 experimental currency units. If the subject chooses B, she receives a payoff which depends on both the number of other subjects who chose B and the state Y. Regime change takes place if $G_{10}(Y) = [10\,(80 - Y)\,/60]$ individuals choose B. More specifically, our payoff structure is as follows. Let Z be the number of agents in a group that attack. $\pi(Y, Z)$ is the net payoff from choosing B, given the fundamental Y and the actions of the group members. $\pi(Y, Z)$ is increasing in Y.

$$\pi(Y, Z) = \begin{cases} Y - 20 & : Z \geq G_{10}(Y) \\ -20 & : Z < G_{10}(Y) \end{cases} \tag{6}$$

With this set-up, observe that playing A is dominant if $Y < 20$ and playing B is dominant if $Y > 74$.

We run a simple design in which the only treatment is the dispersion in the private signals, parameterized by the noise term ϵ. Specifically, we consider two treatments – a Low noise treatment with $\epsilon_L > 0$, and a High noise treatment with $\epsilon_H > \epsilon_L$. Let Y_j^* denote the theory-implied strategic cutoff for treatment $j = \{L, H\}$. The theoretical predictions are summarized in Table 1.

We collected data on 8 groups in the High Noise treatment and 8 groups in the Low Noise treatment, a total of 160 subjects. The sessions were run in the BI Norwegian Business School Research Lab. The experiment was programmed in z-Tree (Fischbacher, 2007) and subjects were recruited from the general student populations of BI Norwegian Business School and the University of Oslo using the software ORSEE (Greiner, 2015).

outcome of the corresponding game. This incentivizes all decisons while neutralizing wealth effects created by the accumulation of profits. A drawback stems from more complicated instructions adding to the risk of confusing subjects. Both methods have been employed in the experimental literature on global games. Results in Heinemann, Nagel, and Ockenfels (2004) (that run two high-stake treatments) indicate that the random payment scheme induces more risk-averse behavior. We shed further light on this in our analysis of the evolution in risk-attitudes below.

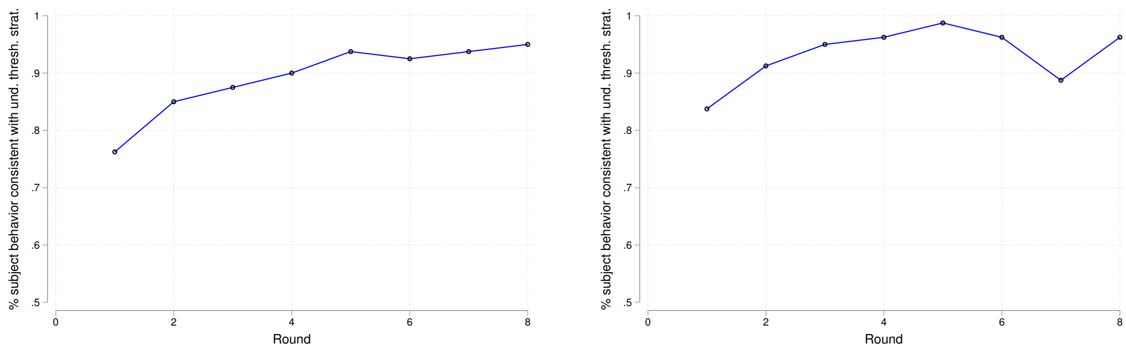
Treatments	Low Noise ($\epsilon = 10$)		High Noise ($\epsilon = 20$)
Strategic cutoff	$Y_L^*= 41.4$		$Y_H^*= 37.8$
Expected treatment difference	$Y_L^* - Y_H^*$	$=$	3.6

Table 1: Equilibrium predictions.

4 Experimental results

Our first objective is to test to what extent subjects follow the equilibrium requirement of using undominated cutoff strategies in our experiment. For each player i and each round t, let x_{it}^A be the highest signal at which subject i chooses A and x_{it}^B be the lowest signal at which she chooses B. We say that a subject's behavior is consistent with a cutoff strategy in round t, if $x_{it}^B \geq x_{it}^A$. Letting ϵ be the noise in each treatment ($\epsilon \in \{10, 20\}$), observe that playing B is dominated by A whenever $x_{it} < 20 - \epsilon$ and A is dominated by B whenever $x_{it} > 74 + \epsilon$. We say that a subject's behavior is consistent with an undominated cutoff strategy if it is consistent with a cutoff strategy and $x_{it}^B \geq 20 - \epsilon$ and $x_{it}^A \leq 74 + \epsilon$.

Overall, the observed behavior of the subjects is largely consistent with playing undominated cutoff strategies. On average, 89% of the subjects play in a way consistent with undominated cutoff strategies in the Low Noise treatment. In the High Noise treatment, the corresponding number is 92%. There is also some evidence of an increasing reliance on undominated cutoff strategies over time. Figure 1 shows the evolution in the use of cutoff strategies over time for each of our treatments. The percentage of subjects whose behavior is consistent with undominated cutoff strategies increases as play progresses.



Low Noise High Noise

Figure 1: Percentage of subjects, whose behavior is consistent with undominated cutoff strategies.

Result 1 (Cutoff strategies): *Subjects play consistently with undominated cutoff strategies.*

Therefore, in the remaining analysis of this section, we follow Heinemann, Nagel, and Ockenfels (2009) and focus on subjects who play according to a cutoff strategy. To estimate individual strategic cutoffs for each round, we take the individual-level average of the highest signal for which a subject chooses A and the lowest signal for which the subject chooses B. We then take the mean of these individual cutoffs within each group and refer to that mean as the *group cutoff*.

In what follows we focus on first-round behavior. The level-k model we study in Section 5 below is meant to address initial play in unfamiliar environments, before learning kicks in (Crawford, 1995).[17] Experimental evaluations using models of limited depths of reasoning, therefore, typically focus on first round behavior (e.g., Crawford, 1995, Camerer, 2011, chapters 1 and 6). Moreover, result 4 below suggests that there is some learning taking place over time. Since we do not study learning dynamics in this paper, we focus on first-round behavior in our main analysis.[18]

[17]See also the discussion in Heinemann, Nagel, and Ockenfels (2009), p.191, regarding path dependence from initial play in repeated coordination games.

[18]Note, however, that results are qualitatively and quantitatively similar when using all data and preforming a logit estimation of individuals likelihood of attacking conditional on their signals, the treatment and an interaction-term. Also, in Section 5.2.2 we explore the evolution of risk-attitudes and the distribution of behavioral types using the full data set.

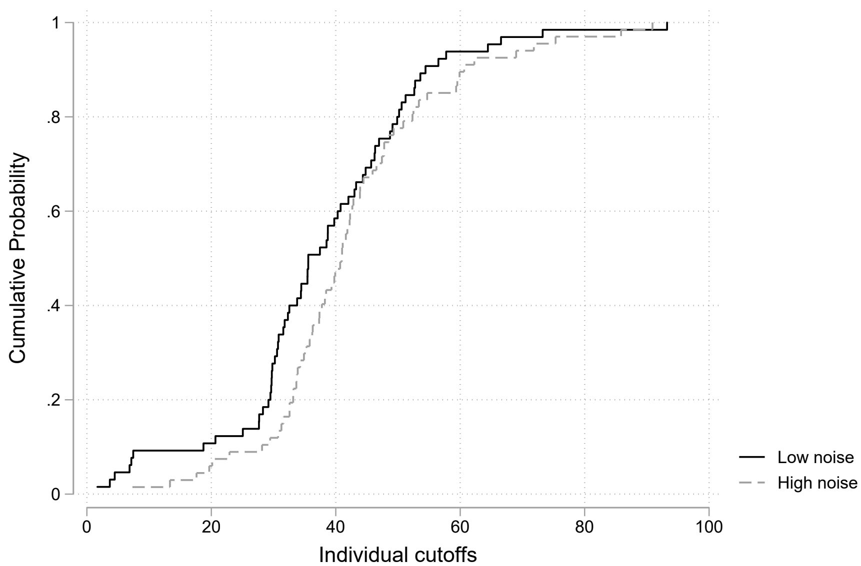


Figure 2: Empirical CDFs of individual cutoffs, by treatment.

Figure 2 shows the empirical CDFs of the individual cutoffs according to treatment. Interestingly, the CDF of individual cutoffs in the High Noise treatment is a rightward shift of the CDF of individual cutoffs in the Low Noise treatment. Table 2 reports the group cutoffs for each group. Data in the table are ranked in ascending order for each treatment based on the value of the group cutoff. We also include the lowest and highest signal used to compute the cutoffs, as well as the equilibrium strategic cutoffs for each treatment. As is evident, in each ordered pair of groups, the group cutoff is higher in the High Noise treatment.

According to the payoff sensitivity effect (cf. Section 2), subjects should play more aggressively in the High Noise treatment compared to subjects in the Low Noise treatment. Both Figure 2 and Table 2 indicate that this is not the case. Figure 2 shows that the CDF of individual cutoffs in the High Noise treatment is a rightward shift of the CDF of individual cutoffs in the Low Noise treatment. Therefore, individuals are more cautious when their information is less precise. Using a two-sample Kolmogorov-Smirnov test, we can reject the hypothesis that the two empirical CDFs are identical in favor of an alternative hypothesis of smaller cutoffs in the Low Noise treatment with a p-value of 2.5%. However, this test

Group #	**Low noise**			**High noise**		
	Group cutoff	*Lowest ind. cutoff*	*Highest ind. cutoff*	*Group cutoff*	*Lowest ind. cutoff*	*Highest ind. cutoff*
1	22.8	3.7	35.5	31.8	7.3	43.9
2	30.2	1.7	48.7	35.8	17.6	47.8
3	35.9	27.6	46.3	39.4	28.2	71.8
4	38.0	4.5	93.25	41.3	13.3	59.6
5	39.9	7.4	73.3	44.6	34.3	59.9
6	40.8	7.14	57.7	48.7	20.1	90.9
7	44.4	32.5	56.5	49.5	33.7	69.0
8	44.8	25.1	66.5	51.5	29.5	85.86
Average	37.1			42.9		
Standard deviation	7.4			6.9		
Equilibrium cutoff	41.4			37.8		

Table 2: Group cutoffs; first round data only, ranked groups.

Treatment	Average cutoff	Std. dev.
Low noise	37.1	7.4
High noise	42.9	6.9
	Difference	p-value
	-5.7	0.067

Table 3: One-sided t-test of average cutoff difference across treatments

does not take into account common within-group disturbances. Therefore, we proceed with a more conservative approach based on the group cutoffs from Table 2.

Table 2 shows that the average (group) cutoff in the High Noise treatment is 5.7 units higher than in the Low Noise treatment, which has the opposite sign compared to the difference in equilibrium cutoffs. To formally test whether the difference across treatments is significant, we use a one-sided t-test.[19] The results are reported in Table 3. We reject the null hypothesis with a p-value of 6.7%, when considering an alternative hypothesis of a lower threshold in the Low Noise treatment. We conclude that contrary to equilibrium theory, subjects play less aggressively in the High Noise treatment compared to the Low Noise treatment.

Result 2 (Information quality comparative statics): *The estimated average strategic cutoff is lower in the Low Noise treatment compared to the High Noise treatment.*

5 A model with limited depth of reasoning

The results in the previous section show that higher information dispersion makes subjects behave less aggressively. This is the exact opposite of the payoff sensitivity effect that one should expect according to equilibrium play (cf. Section 2). In this section, we develop and test a model that is consistent with this experimental finding. Our model assumes that players deviate from equilibrium play and instead have limited depth of reasoning in the

[19] In the Appendix, we follow a more conservative approach and run a Mann-Whitney U test. The difference across thresholds is significant with a p-value of 8.5 %.

form of level-k thinking.

5.1 Level-k thinking

Level-k thinking is a frequently used solution concept in Behavioral Game Theory.[20] It features limited depths of reasoning, adds a specific structure to agents' beliefs, and is particularly meant to capture players' initial behavior in strategic games, before learning induces higher levels of sophistication. The main appeal of level-k thinking in our setting is that it can change the comparative statics from the equilibrium theory on how information dispersion affects players' actions and regime stability. In this section, we illustrate and discuss the theoretical mechanism through which this happens.

5.1.1 Set-up

We consider again the set-up from Section 2 with net payoffs $U(Y)$ and $D(Y)$ given as in our experimental implementation, so that $U(Y) = U < 0$, $\forall Y$, and $D(Y)$ is linear and increasing in Y. Assume that agents have limited depth of reasoning. Specifically, each player $i \in \{1, 2, ..., N\}$ is assumed to have a type Lk drawn from a discrete distribution over $k \in \{1, \dots, \infty\}$, where Lk denotes a type that engages in k rounds of reasoning. An Lk type best-responds to the belief that all other agents play as $L(k-1)$ types, for $k > 1$. Finally, $L1$ types best respond as if all other agents act as $L0$ types, where the behavior of $L0$ types is specified as a model primitive. Note that there are no actual $L0$ types among the players.

Following Kneeland (2016), we assume that $L1$ types believe that the aggregate behavior of $L0$ types is described by the cumulative distribution function $Q(z|Y)$, where z denotes the fraction of agents that attack. Here, $Q(z|Y)$ is continuously differentiable and weakly decreasing in Y, so that $L1$ types believe that a higher value of Y leads to a larger share of $L0$ types attacking.

[20] See, for instance, Nagel (1995); Stahl and Wilson (1995); Kubler and Weizsacker (2004); Crawford, Costa-Gomes, and Iriberri (2013).

5.1.2 Characterization

Let Y_k^* denote the signal value of an Lk type, for which that type is indifferent between attacking and not attacking. In other words, Y_k^* is the strategic cutoff for an Lk type. An Lk type that attacks expects that regime change occurs with probability $\tilde{P}_k(Y)$. Given the level-k assumptions, it follows that

$$\tilde{P}_k(Y) = \begin{cases} 1 - Q\left(\frac{G_N(Y)-1}{N}|Y\right), & k = 1 \\ \tilde{P}\left(Y, Y_{k-1}^*\right), & k > 1 \end{cases}, \tag{7}$$

where $\tilde{P}(Y,x) = \overline{F}_{N-1}(G_{N-1}(Y), Y, x)$. Therefore, Y_k^* solves

$$E_{Y_k^*}\left[(D(Y) - U)\,\tilde{P}_k(Y)\right] + U = 0 \tag{8}$$

One can also characterize Y_k^* as follows. Define implicitly the function $h(x)$ by

$$E_{h(x)}\left[(D(Y) - U)\,\tilde{P}(Y,x)\right] + U = 0.$$

Therefore, $h(x)$ is the strategic cutoff given that all other players use cutoff strategies with cutoff x. Because the strategic cutoff fully characterizes the strategies of Lk types, it follows that one can think of $h(x)$ as the best response function of an agent who anticipates that all other players use cutoff x. To find the strategic cutoffs for all Lk types, let Y_1^* solve (8) and let $Y_k^* = h\left(Y_{k-1}^*\right)$ for $k > 1$.

Remark 1. Recall from Section 2 that Y^* denotes the strategic cutoff according to equilibrium play. Then Y^* is the fixed point of $h(.)$ and satisfies $Y^* = h(Y^*)$. Moreover, continuity of $h(x)$ implies that $Y_k^* \to Y^*$.

Remark 2. It is straightforward to show that $h(x)$ is strictly increasing in x. Therefore, there is a strategic complementarity across Lk types. Specifically, the aggressiveness of the level-$(k-1)$ types (i.e. the location of their cutoff) influences the level-k type's cutoff (and, through that cutoff, affects level-$(k+1)$ agents, and so forth). The lower the value of Y_{k-1}^{*} (the more aggressive the $L(k-1)$ type), the lower the value of Y_{k}^{*}, Y_{k+1}^{*}, etc.

5.1.3 Effects of changes in information quality

Next, we discuss how changes in ϵ affect the behavior of Lk types (i.e. the effects of ϵ on Y_{k}^{*}). To this end we first show a specific example which illustrates the novel channel through which information quality affects the behavior of Lk types. For analytical convenience, we take $N \to \infty$, so that $\tilde{P}(Y, x)$ converges to a step function with a discontinuity at $Y^{f}(x)$, in which $Y^{f}(x)$ satisfies

$$g\left(Y^{f}(x)\right) = \frac{1}{2} + \frac{Y^{f}(x) - x}{2\epsilon}. \tag{9}$$

We also follow our experimental implementation and the structural estimation below, and assume that $g(Y)$ is linear and the distribution of play of $L0$ types is independent of Y and has an increasing density. Finally, we assume that $D(Y) = D > 0$ is a constant. This particular assumption is made to switch off the payoff sensitivity effect from changes in information dispersion ϵ (cf. Section 2 and Corollary 1 in Iachan and Nenov (2015)). As a result, the equilibrium strategic cutoff, Y^{*}, does not vary with information dispersion ϵ. In contrast to the noise invariance of equilibrium play, we have the following result for the strategic cutoff of Lk types.

Proposition 1. ***(Strategic attenuation)*** *Consider the game described above and let $D(Y) = D > 0$. Suppose that $L0$ types play more aggressively than uniform randomization, so $Y_{1}^{*} < Y^{*}$. Then, $\frac{\partial Y_{k}^{*}}{\partial \epsilon} > 0$: Higher noise makes Lk types less aggressive.*

Proof. See the Online Appendix. □

Figure 3 illustrates this result. A higher value of ϵ flattens the best response function $h(x)$ (dashed line) and rotates it clockwise around the fixed point Y^{*}, which is noise invariant. To the left of Y^{*}, $h(x)$ increases. Intuitively, an increase in the dispersion of private noise attenuates the strategic complementarity across Lk types, because it makes agents less coordinated when attacking and also reduces their ability to forecast the actions of other agents. This shows up as a flattening of the best response function that links an Lk type's strategic cutoff with the strategic cutoff she believes $L(k-1)$ types follow. Therefore, whenever $L(k-1)$ types are more aggressive than Lk types, higher information dispersion makes Lk types react *less* to the aggressiveness of $L(k-1)$ types; Lk types become *less* aggressive with higher information dispersion. We call this effect of information dispersion *strategic attenuation*.[21]

The strategic attenuation effect suggests that the effects of information quality can be reversed for some Lk types relative to the equilibrium model. Figure 3 suggests that the effect is stronger for more aggressive $L1$ types (a low initial value of Y_1^*) and for low values of $k > 1$, that is for Lk types who engage in few rounds of reasoning. In terms of model primitives, $L1$ types tend to be more aggressive when they expect $L0$ types to play more aggressively. Therefore, in our structural estimation below we will assume that $L0$ types play relatively aggressively, so that the strategic attenuation effect operates.

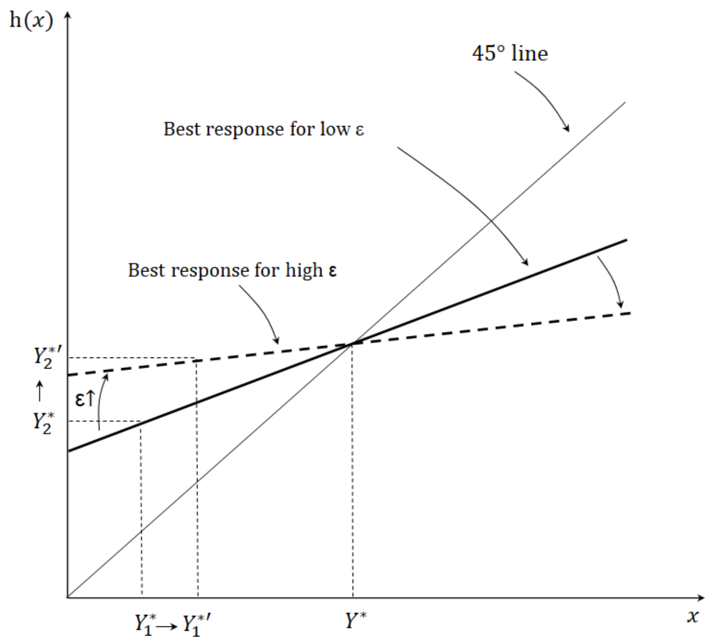
Remark 1. The flattening of the best response function h when ϵ is increased is closely related to the mechanism through which strategic complementarities in combination with small amounts of private information lead to a unique equilibrium in global games of regime change. Indeed, if $\epsilon = 0$, then in the example above, $h(x)$ has a slope of one, leading to multiple equilibria.

Remark 2. The strategic attenuation effect operates at each level of reasoning, for $k > 1$. Therefore, even in a case in which the behavior of $L1$ players is noise invariant, the strategic attenuation effect kicks in for all higher levels of k.

Next, we show that the strategic attenuation effect also operates in the context of our experimental set-up with a finite number of players and where payoffs are such that the payoff sensitivity effect also operates (including for Lk players). Figure 4 plots the strategic

[21] Figure 3 also illustrates that strategic attenuation would also operate if $L1$ types are less aggressive than the equilibrium agents. In that case, provided that Y_1^* does not vary much with ϵ, a higher value of ϵ would imply that Lk types become more aggressive.

Figure 3: Strategic attenuation from higher noise ϵ.



cutoffs of different level-k types for the two different values of ϵ used in the experiment. The left-hand panel is for risk neutral agents, while the right-hand panel is for risk averse agents with constant relative-risk aversion (CRRA) type preferences and coefficient of relative risk aversion equal to the estimated coefficient in our structural estimation below.[22] The figure also includes the average cutoffs for each experimental treatment. The figure shows how Y_k^* converges to a stable value as k increases, which is also the strategic cutoff in the equilibrium model, Y^* (denoted by ∞ on the x-axis of each panel).

There are several observations we can make from this figure. First, in the context of our experimental set-up, changes in information quality impact the strategic cutoff of some level-k types differently from the equilibrium model. Indeed, while equilibrium types (which we define as agents that use a cutoff equal to the cutoff in the equilibrium model) end up playing more aggressively with high noise, $L2$ and $L3$ types play less aggressively irrespective of whether they are risk neutral or risk averse. This difference in behavior is due to the

[22] The assumption of $L0$ play is as in our baseline structural estimation below, i.e. the fraction of attacking $L0$-types is distributed according to a Beta(α, β) distribution with shape-parameters $\alpha = 10$ and $\beta = 1$. In the Appendix, we characterize the strategic cutoffs for level-k types with CRRA preferences.

strategic attenuation effect dominating the payoff sensitivity effect in the case of $L2$ and $L3$ types.

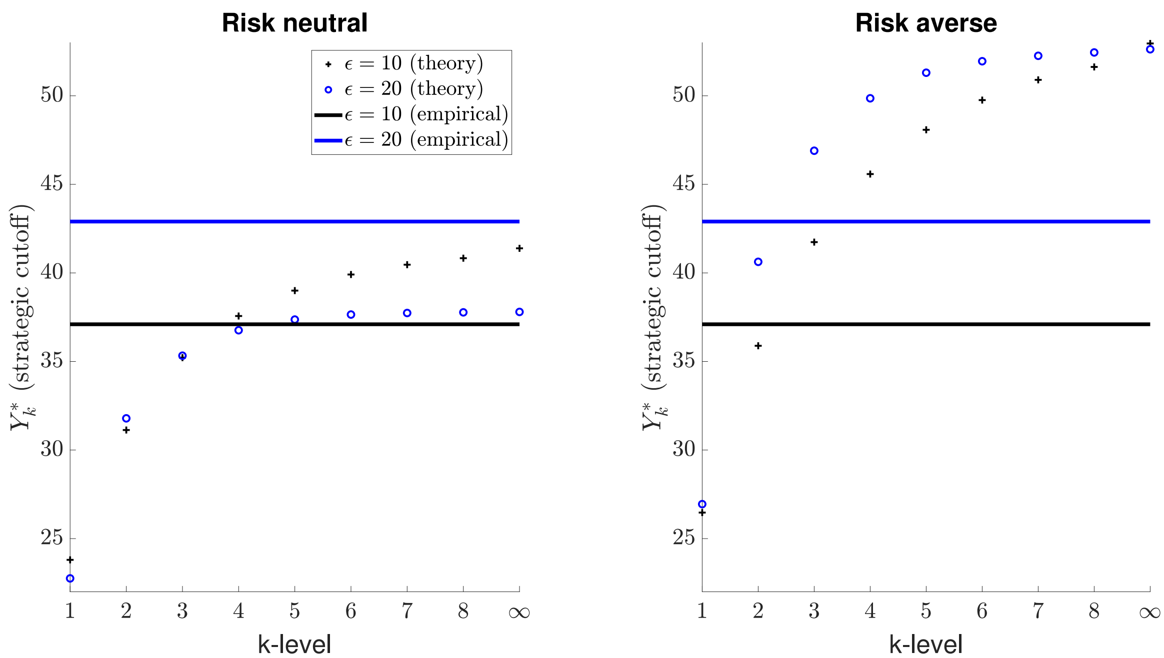
Second, risk aversion has a two-fold effect on behavior. First, it raises the strategic cutoffs for all level-k (and the equilibrium) types. As a result, while with risk neutral agents the estimated average cutoffs for each treatment are closer to those of the equilibrium types, with risk averse agents, the estimated average cutoffs are more in line with the strategic cutoffs of lower level-k types, such as $L1$ and $L2$ types. In addition, risk aversion significantly weakens the "payoff sensitivity" effect as evident from the effect of higher information dispersion on the strategic cutoff of equilibrium types.

Therefore, although risk aversion on its own does not reverse the comparative statics with respect to information precision, and the comparative statics are reversed for low level-k types even with risk neutral agents, risk aversion is important for the magnitude of the reversal. In addition, it ensures that the levels of the strategic cutoffs of the Lk types are in line with our empirical data. For these reasons, in our structural estimation below we allow agents to have CRRA preferences and estimate the coefficient of relative risk aversion for these agents jointly with the other parameters of our structural model.[23]

5.2 Empirical evaluation

In this section, we evaluate empirically whether the level-k model can account for the deviations from equilibrium theory that we have documented in Section 4. We first structurally

[23]Notice that for $L1$ types the strategic cutoff changes little with information dispersion in both the risk neutral and risk averse cases. Moreover, the comparative statics for $L1$ types can go either way. The reason is that the payoff sensitivity effect impacts the behavior of $L1$ types, as it does higher level-k types, and pushes for $L1$ types to be more aggressive when ϵ increases. On the other hand, the strategic attenuation effect does not operate for $L1$ types, and instead changes in ϵ interact with the distribution of play of $L0$ types. If the distribution of play of $L0$ types has an increasing density, then a higher value of ϵ pushes towards $L1$ types being less aggressive. However, this effect tends to be weak, so that when agents are risk neutral, the payoff sensitivity effect dominates for $L1$ types, while with risk averse agents, the payoff sensitivity effect is also weak, so the two effects almost balance out.



Notes: In the left panel, agents are risk-neutral. In the right panel, agents are risk-averse with a CRRA-coefficient equal to the estimate from the baseline model in Table 4.

Figure 4: Strategic cutoffs of level-k and equilibrium types and estimated average cutoffs for low and high level of ϵ.

estimate a finite mixture model of play on our experimental data and then use the estimated model to explain the deviations from equilibrium theory that we observe.

5.2.1 Methodology

To separate the subjects into different level-k types, we follow Kneeland (2016) and estimate a finite mixture model on our experimental data.[24] For our baseline estimation, we allow subjects to be $L1$, $L2$, and equilibrium types, which we denote by Lk, $k \in \{1, 2, \infty\}$.[25] We denote the share of Lk types by p_k, withp_∞ denoting the share of equilibrium types. Following the discussion from the previous section, we assume that players have CRRA

[24] We follow the estimation procedure for finite mixture models in Mofatt (2016), chapter 8.

[25] We follow a structural approach and estimate parameters of a pre-specified model, including the proportion of players that follow each particular Lk-type behavior. As described soon, players are assumed to make errors in their play. The frequency of those errors, combined with their variance, can be understood as a measure of mismatch between the behavior assumed for a particular type and what is observed.

As in Kneeland (2016), equilibrium types engage in infinite rounds of reasoning, so they play according to the equilibrium strategies from the global games model.

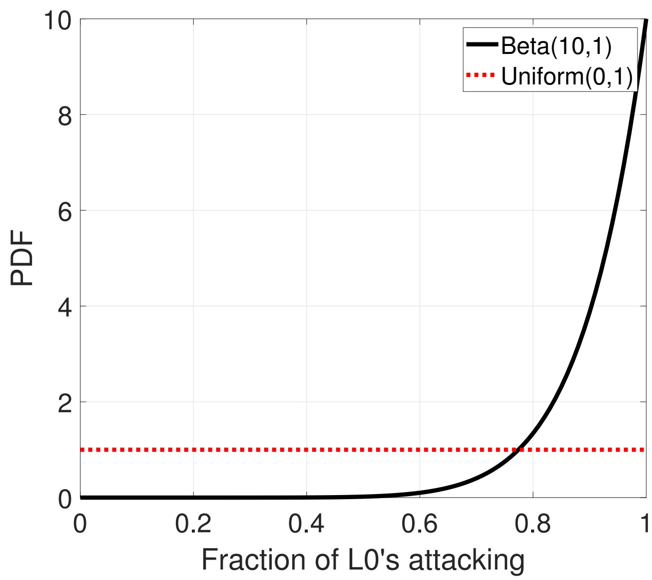


Figure 5: Beta(10,1) vs. uniform distribution.

preferences over the (gross) payoffs from each decision with a coefficient of relative risk aversion of α, and estimate α jointly with the other model parameters. We further assume that each subject follows the action of a particular Lk type with some error. Specifically, in each decision round a subject makes a decision consistent with her type with probability $1-\nu$ and makes an error with probability ν. If the player makes an error, the choice depends on an error density $d^k\left(a_q^i, \lambda\right)$ specified in equation (11) below.

As illustrated in Proposition 1, the effect of information quality can be reversed for some level-k types compared to the equilibrium model. This happens if $L0$ types are expected to play sufficiently aggressively, so that level-k types play more aggressively than the equilibrium types. Therefore, we assume that $L0$ types are expected to play aggressively. Specifically, in our baseline estimation we assume that the fraction of attacking $L0$-types is distributed according to a Beta$(10, 1)$ distribution, which has a mean of 0.909.

In Figure 5, we illustrate how this distribution of $L0$-types' play differs from the case where $L0$-types randomize uniformly over actions. While most of the literature in which level-k models are used to explain data from experimental games makes the latter assumption, (Crawford, Costa-Gomes, and Iriberri, 2013, and references therein), the literature on experimental coordination games has shown that initial play tends to be biased towards payoff dominant actions (Costa-Gomes, Crawford, and Iriberri, 2009).[26,27] Moreover, assuming that $L0$ types randomize uniformly in global games of regime change (so that the share of agents attacking is uniformly distributed) leads to the equilibrium outcome, since $L1$ types end up holding (and reacting to) Laplacian beliefs about the remaining players' actions (Morris and Shin, 2003). For these reasons Kneeland (2016) assumes that $L0$ types play more aggressively than uniform randomization in her empirical investigation of a level-k model in an experimental global game.[28]

Our assumption of aggressive $L0$ types is, therefore, in line with this previous work on level-k models in experimental global games. To investigate the robustness of our results to the assumed behavior of $L0$ types, below we also investigate the performance of the model under several alternative assumptions.

Let $Q = \{1, 2, ..., 10\}$ denote the set of all decisions, $q \in Q$ denote a specific decision instance, and a_q^i denote the choice of subject i in instance q. For each subject×type, we define the set $Q^{ik} \subset Q$, which consists of all instances q, where subject i made a choice

[26] A deviation from the assumption of uniform randomization over actions for $L0$ types is also found in the literature on auctions, where such types are assumed to bid their value conditional on their own signal (Crawford and Iriberri, 2007).

[27] Initial bias towards the payoff dominant action is also observed in the weakest-link game analyzed by Van Huyck, Battalio, and Beil (1990). Their version of the game has seven Pareto ranked equilibria characterized by all players choosing the same (out of seven available) effort levels. While a large fraction of subjects initially try to coordinate on the payoff dominant equilibrium by choosing the highest effort level, behavior quickly converges to coordination on the least efficient equilibrium where all players choose their minimum effort level (which ensures their maximin payoff). The hurdle function in Van Huyck, Battalio, and Beil (1990) is demanding in that it requires that all players coordinate on the same effort level for coordination to succeed. In parallel, to this, experiments on global games find that the frequency of subjects choosing the risky action goes down with a more demanding hurdle (see for instance the results in Heinemann, Nagel, and Ockenfels (2009)). Goeree and Holt (2005) note that efficiency in weakest-link experiments improves significantly when the cost of coordination failure is reduced (while preserving the Nash equilibrium structure), and that the logit QRE equilibrium explains this phenomenon well.

[28] A theoretical literature using non-equilibrium concepts to understand coordination in a global games setting is emerging. Heinemann, Noussair, and Cornand (2015) shows that the over-reaction to public signals observed in the experimental literature is welfare improving if agents have limited levels of reasoning. Shapiro, Shi, and Zillante (2014) investigate the predictive power of the global games set-up when agents have limited depths of reasoning. Their model focuses on the strength of the coordination motive and wether information is symmetric (two public signals) or asymmetric (one public and one private signal). Their k-level model predicts that coordination is increasing in the strength of the coordination motive and with symmetric information. Their experiment lends support to these predictions.

a_q^i consistent with type Lk. Weighting over the different types (k) and summing over all subjects (i), we get that the log-likelihood of observing a particular set of choices is

$$\mathcal{L} = \sum_{i=1}^{N} \log \left[\sum_{k \in \{1,2,\infty\}} p_k \left(\Pi_{q \in Q^{ik}} \left(1 - \nu + \nu d^k \left(a_q^i, \lambda \right) \right) \right) \left(\Pi_{q \notin Q^{ik}} \nu d^k \left(a_q^i, \lambda \right) \right) \right]. \tag{10}$$

The parameter λ is a precision parameter in the error density

$$d^k \left(a_q^i, \lambda \right) = \frac{\exp \left\{ \lambda S_q^k \left(a_q^i \right) \right\}}{\exp \left\{ \lambda S_q^k \left(\text{attack} \right) \right\} + \exp \left\{ \lambda S_q^k \left(\text{not attack} \right) \right\}}, \tag{11}$$

where $S_q^k \left(a_q^i \right)$ denotes the expected payoff of an agent of type Lk at decision instance q, who makes a choice a_q^i.

The unit of analysis is now individual decisions, which is in line with the existing literature on coordination experiments (Costa-Gomes, Crawford, and Iriberri, 2009; Crawford, Gneezy, and Rottenstreich, 2008). In our benchmark estimation, we estimate 5 independent parameters, namely p_1, p_2 (the fractions of types $L1$ and $L2$), λ, ν, and α on data from the first round for both treatments.

5.2.2 Results

We report the estimation results in Table 4. In Column (1) we report the results from estimating our baseline model. The fraction of level-k types is estimated to be 72%. This is roughly in line with the estimate from Kneeland (2016). Most agents are classified as $L2$ types. In terms of trembling probabilities, we estimate a relatively large trembling rate ν. Note however, that the actual trembling probabilities, which are given by $\nu d^k \left(a_q^i, \lambda \right)$ are substantially lower than ν, as illustrated in Figure 9, which plots the distributions of $\nu d^k \left(a_q^i, \lambda \right)$ by subjects and decision rounds for the three different Lk-types in the model given the round one data. As the Figure illustrates the trembling probabilities tend to be low, suggesting that for the most part observed play is consistent with the theoretical predictions of the level-k model. The estimated CRRA parameter of 0.48 is broadly in line with other studies estimating risk attitudes based on data from experimental games, individual decision making experiments, as well as field studies.[29]

In Column (2), we report the results from estimating a model where agents are instead assumed to be risk neutral. With risk neutral agents the estimation procedure ends up classifying the majority of players (around 62%) as equilibrium types, This should not be surprising in light of Figure 4 above, which shows that with risk neutral agents the estimated average cutoffs in the experimental data are closer to the theoretical cutoffs of equilibrium types than of $L1$ or $L2$ types.

In Column (3), we report the results from estimating a model without level-k types but only with risk averse equilibrium types. Importantly, the log-likelihoods of both models (2) and (3) are substantially lower than the log-likelihood in our baseline model (1), indicating that the data is substantially more likely to be generated by a level-k model with risk averse agents. Specifically, we can test for risk neutrality versus risk aversion using a likelihood-ratio test. Under the null hypothesis of risk neutrality, the distribution of $-2\ln\left(\frac{\mathcal{L}_{Model\ (2)}}{\mathcal{L}_{Model\ (1)}}\right)$ is χ^2 with one degree of freedom. The value of the test statistic is 107.4, which implies a p-value of less than 0.001. Therefore, risk neutrality is strongly rejected in the data. We can use a similar approach to test for only equilibrium types by comparing the log-likelihood of model (3) against our benchmark model. Under the null hypothesis of only equilibrium types, the distribution of $-2\ln\left(\frac{\mathcal{L}_{Model\ (3)}}{\mathcal{L}_{Model\ (1)}}\right)$ is χ^2 with two degrees of freedom. The value of the test statistic is 136, which also implies a p-value of less than 0.001. Therefore, having only equilibrium types is also strongly rejected in the data.

Next, we investigate the robustness of our assumption of relatively aggressive play by $L0$

[29] In auction experiments, the estimated risk-aversion parameter $\hat{\alpha}$ lies in the interval 0.48-0.67 (Cox and Oaxaca (1996); Chen and Plott (1998); Goeree, Holt, and Palfrey (2002)). In comparison, Campo (2012) and Lu and Perrigne (2008) estimate $\hat{\alpha}$ to be 0.55 for field data on timber auctions. In experiments on 2X2 asymmetric matching pennies games (Goeree, Holt, and Palfrey (2003)) $\hat{\alpha}$ lies in the intervall 0.42-0.44. In a dynamic legislative bargaining experiment Battaglini and Palfrey (2012) find $\hat{\alpha}$ to be 0.50. Heggedal, Helland, and Morton (2020) estimate an $\hat{\alpha} = 0.35$ when investigating a probabalistic voting model. Albeit not a coordination game, exogenous uncertainty are central features of a probabilitic voting game as well as a global game. In comparison, for the lottery choice experiments (i.e., individual decision making) of Holt and Laury (2002, 2005)) $\hat{\alpha}$ is found to be in the range 0.30-0.60.

types. In column (4), we assume that the fraction of $L0$ types attacking is governed by a Beta(x,1) distribution and estimate x directly. Importantly, this model nests uniform randomization over actions by $L0$ types, as a uniform distribution and a Beta(1, 1) distribution coincide. The resulting x is 34.2, which translates into a mean attack rate of approximately 97%. Importantly, when we estimate the implied behavior of $L0$ types, the log-likelihood improves further compared to the baseline. Therefore, the data favors even *more aggressive* perceived play by $L0$ types compared to our baseline assumption.

We can also compare the log-likelihoods from models (3) and (4) to explicitly test if uniform randomization by $L0$ types is rejected in the data. The reason for this is that model (4) nests model (3), since as already discussed above, a fully rational model and a model with level-k types in which the $L0$ types are perceived to randomize uniformly over actions lead to identical cutoffs for all level-k types that equal the strategic cutoff of equilibrium types. Under the null hypothesis of uniform randomization by $L0$ types, the distribution of $-2\ln\left(\frac{\mathcal{L}_{Model\ (3)}}{\mathcal{L}_{Model\ (4)}}\right)$ is χ^2 with one degree of freedom. The value of the test statistic is 186 (p-value < 0.001), so uniform play by $L0$ types is strongly rejected in our data.[30]

To additionally assess the robustness of our estimates to the assumed behavior of $L0$ types, in Columns (5) and (6) we return to the case where $L0$ behavior is fixed, and report the results under two alternative assumptions about $L0$ behavior. In column (5) we estimate a model where the proportion of $L0$ types attacking approaches 1, while in column (6) we estimate model where $L0$ types do not attack in the lower dominance region. In both cases, all parameters and the log-likelihoods are similar to the baseline. Finally, in column (7) we report the results from estimating a level-k model when also including $L3$ types. In this case, the estimated risk aversion declines somewhat and the fraction of $L1$ and $L2$ types decline. The log-likelihood is similar to the different models with two level-k types, and a likelihood ratio test cannot reject that the data is generated by our baseline model with only

[30]Note that a likelihood ratio test of our baseline model against model (4) also rejects our assumption of a Beta(10,1) distribution of play for $L0$ types in favor of the more aggressive play estimated in model (4) at conventional levels of significance. Therefore, in our baseline model we assume that the $L0$ types are *too cautious* relative to what the data appears to favor.

two level-k types.

We proceed by investigating whether the estimated baseline level-k model can rationalize the experimental effect of information quality (Result 2 from Section 4). A simple first pass is to compute the weighted-average of the theory-implied strategic cutoffs given the estimated risk-aversion coefficient and using the estimated distribution of Lk types from Table 4 as weights. We can then investigate whether the resulting average cutoff in the Low Noise treatment is lower than the average cutoff in the High Noise treatment. This exercise yields an average strategic cutoff of 38.73 in the Low Noise treatment, and 41.26 in the High Noise treatment. Put differently, agents are, on average, *less* aggressive in the High Noise treatment compared to the Low Noise treatment, which is in line with our experimental findings. To further shed light on how the estimated level-k model performs relative to the data, we test whether the level-k implied cutoffs differ significantly from our estimated average cutoffs. The results are shown in Table 5. In both cases, we cannot reject that the average strategic cutoffs implied by the level-k model differs significantly from the estimated average cutoffs in the data.

Treatment	Estimated average cutoff	Structural model prediction	p-value
Low noise	37.1	38.73	0.53
High noise	42.9	41.26	0.56

Table 5: Two-sided t-test of the estimated average cutoff versus the average cutoff from the level-k model.

A caveat with the preceding exercise is that it does not take into account that our estimated model allows for trembles. We therefore proceed with the following simulation exercise: We simulate 1000 sessions, whereby in each session 640 games are played. For each session and game, agents draw a type according to the estimated type distribution from Table 4. We then assume that agents play according to their drawn type, with a probability of trembling equal to our estimated ν. Conditional on trembling, agents choose actions subject to to a spike-logit error, governed by the precision parameter λ, leading to decision frequencies specified in equation (11). For each game, we then compare the mean cutoffs

Model:	(1) Baseline	(2) Risk-neutral	(3) Equilibrium types	(4) Endo. $L0$	(5) Fixed $L0$: Alt 1.	(6) Fixed $L0$: Alt 2.	(7) Three Lk-types
Fraction of level-1 agents (p_1)	0.1935	0.0558	-	0.1860	0.0899	0.1113	0.1512
	[0.0865]	[0.0717]		[0.0633]	[0.0562]	[0.0531]	[0.0838]
Fraction of level-2 agents (p_2)	0.5328	0.3203	-	0.5899	0.5385	0.5631	0.2410
	[0.1295]	[0.0586]		[0.1068]	[0.1289]	[0.1218]	[0.0862]
Fraction of level-3 agents (p_3)	-	-	-		-	-	0.4744
							[0.0881]
Fraction of equilibrium types ($1 - p_1 - p_2 - p_3$)	0.2737	0.6239	-	0.2241	0.3716	0.3256	0.1334
	[0.1468]	[0.0868]		[0.1295]	[0.1374]	[0.1313]	[0.1232]
Trembling rate (ν)	0.6481	0.4819	0.6691	0.8269	0.6311	0.7240	0.6715
	[0.1936]	[0.0760]	[0.0684]	[0.1899]	[0.1878]	[0.1843]	[0.1430]
Precision of error density (λ)	0.2211	0.0255	0.0525	0.3694	0.2122	0.2500	0.2218
	[0.0860]	[0.0053]	[0.0054]	[0.1383]	[0.1094]	[0.0825]	[0.0819]
Coefficient of relative risk aversion (α)	0.4756	-	0.1199	0.4248	0.4577	0.4804	0.3253
	[0.0744]		[0.0878]	[0.0907]	[0.0919]	[0.0813]	[0.0859]
Shape-parameter	-	-	-	34.2829	-	-	-
				[16.1759]			
Log-likelihood	-617.0765	-670.8003	-685.3984	-604.8430	-614.6746	-615.1004	-618.1589
	1600	1600	1600	1600	1600	1600	1600

Notes: Bootstrapped standard errors in brackets.. The table reports parameter estimates from estimating equation (10) on data from round 1. In Column (1) we estimate a model with $L1$,$L2$ and equilibrium types under the assumption that the proportion of $L0$ attacking is drawn from a Beta$(10, 1)$ distribution and where all agents are risk-averse. In Column (2) we estimate a similar model as in (1), but where agents are risk-neutral. In Column (3) we estimate a model with only risk-averse equilibrium types. In Column (4) we estimate a similar model as in (1), but where the proportion of $L0$ attacking is drawn from a Beta$(x, 1)$ distribution and where x ("Shape-parameter") is estimated. In Column (5) we estimate a similar model as in (1), but where the proportion of $L0$ attacking $\rightarrow 1$. In Column (6) we estimate a similar model as in (5), but where $L0$ types do not attack in the lower dominance region. Finally, in Column (7) we estimate a model as in (1), but where we allow for a $L3$ type as well.

Table 4: Parameter estimates and log-likelihoods.

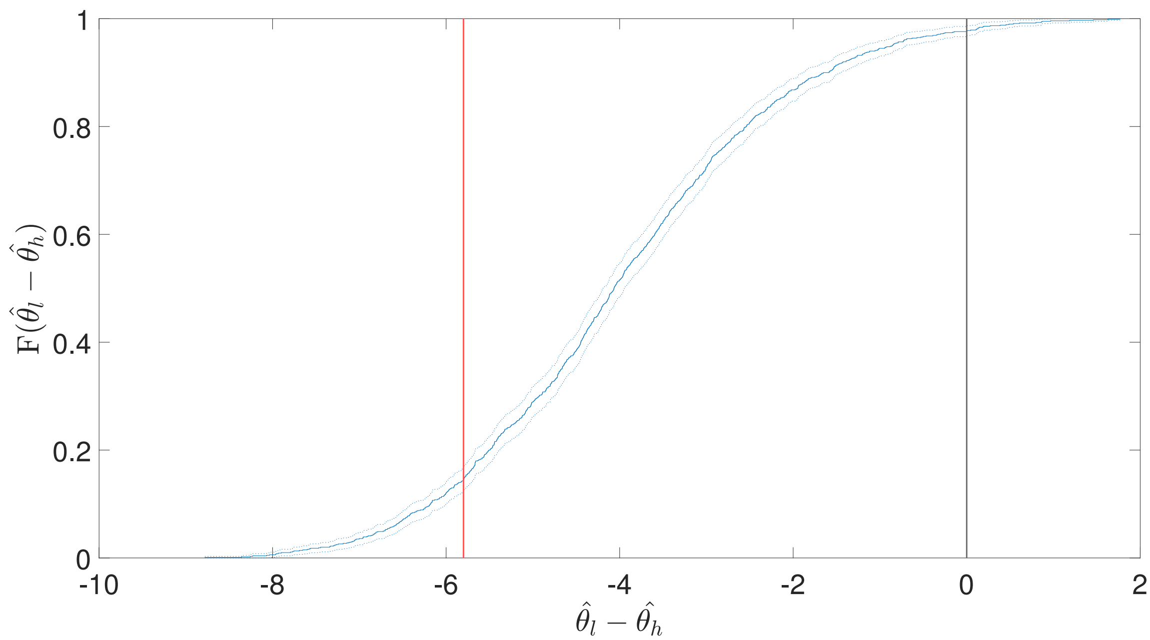


Figure 6: Empirical CDF of estimated cutoff differences from 1000 simulated sessions. Dotted lines indicate 95% confidence intervals as computed by Greenwood's formula. Red vertical line correspond to the average cutoff difference in the data (-5.8).

across a high and a low noise treatment, denoted by $\hat{\theta}_l$ and $\hat{\theta}_h$, respectively. In the Appendix, we provide further information on the simulation exercise.

Figure 6 plots the empirical CDF of the differences between average cutoffs across the two treatments ($\hat{\theta}_l - \hat{\theta}_h$) using the simulated data. Approximately 98% of the simulated sessions have negative differences in average cutoffs. In addition, around 15% of sessions have an average cutoff difference equal to or larger (in magnitude) than the average cutoff difference we observe in the experimental data as illustrated by the red, vertical line. We take this as additional evidence that the level-k model can explain our main experimental finding.

To investigate learning dynamics, in Figure 7, we compare the estimated fraction of level-k agents, for $k \in \{1, 2\}$, and the estimated CRRA-coefficient from the model estimated on period 1 and period 8 data, respectively. The estimated fraction of level-k agents is lower when using period 8 data compared to period 1, suggesting that agents tend to behave

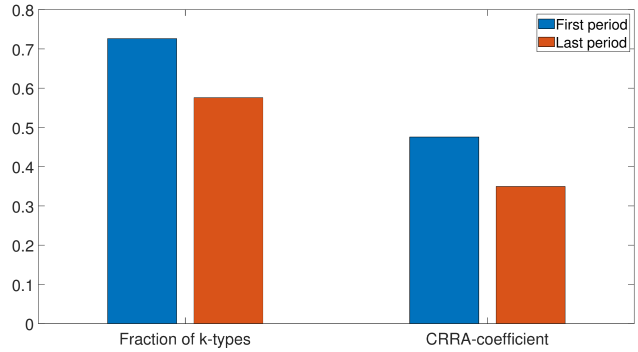


Figure 7: Evolution of model parameters over time. First and last period.

more like equilibrium types in the latter stages of the experiment. This is consistent with the evolution of the estimated average cutoffs (see Figure 8 in the Appendix). However, the decline is relatively small, which suggests that learning is slow in this environment.[31] Note that the observed decline in the estimated CRRA coefficient is consistent with the accumulation of profits over the course of the experiment, creating a wealth effect, further justifying our focus on first period behavior in the main analysis.

6 Concluding remarks

In this paper we experimentally test how changes in private information precision affect regime stability in a standard global games model. We show that contrary to the equilibrium predictions of a standard global game, higher information precision is destabilizing. We show that augmenting the standard global games set-up with boundedly rational agents that engage in level-k thinking can help explain our experimental finding. In the level-k model, information quality affects agents' actions through a novel channel, which does

[31] However, we caution against drawing strong conclusions about long run behavior from these results. Convergence may require a considerable number of rounds even in simple games (Friedman, Huck, Oprea, and Weidenholzer, 2015). Though some evidence of convergence to Nash equilibrium exists for p-beauty contests (Camerer, 2011:318-322), the global game model is more complex than a p-beauty contest. Complexity is known to impact on the form and speed of learning in games (Paich and Sterman (1993), Ho and Weigelt (1996)). In our view, the study of long run behavior in global games is a promising venue for future research. Traction may require change over time in the (perceived) behavior of $L0$ types. Using the sentiment theory presented in Szkup and Trevino (2020) might be an interesting avenue for this.

not operate in the fully rational model. Specifically, higher private information dispersion attenuates the across-type strategic complementarity in the level-k model, which can reverse the comparative statics with respect to changes in information quality.

The fact that the fully rational and level-k models can differ so dramatically in their predictions about the effect of information quality on behavior points to the importance of studying more carefully global coordination games with boundedly rational agents, both theoretically and experimentally. We view this as a promising venue for future research.

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Online Appendix

Proof of Proposition 1.

We first characterize the response of the strategic cutoff for $L1$ types, Y_1^*. We have that Y_1^* satisfied equation (8)

$$E_{Y_1^*}\left[(D-U)\tilde{P}_1(Y)\right]+U=0$$

or

$$\int_{Y_1^*-\epsilon}^{Y_1^*+\epsilon}(D-U)\left[1-Q\left(g\left(Y\right)\right)\right]\frac{1}{2\epsilon}dY+U=0.$$

We can use a change of variables $t=\frac{Y-Y_1^*}{\epsilon}$ to write the left-hand side of this condition as

$$\Psi^{L1}\left(Y_1^*,\epsilon\right)=\frac{1}{2}\int_{-1}^{1}(D-U)\left[1-Q\left(g\left(Y_1^*+\epsilon t\right)\right)\right]dt+U,$$

where $g=\alpha+\beta Y$, for $\beta<0$. By the implicit function theorem,

$$\frac{\partial Y_1^*}{\partial\epsilon}=-\frac{\partial\Psi^{L1}/\partial\epsilon}{\partial\Psi^{L1}/\partial Y_1^*}.$$

Note that

$$\frac{\partial\Psi^{L1}}{\partial Y_1^*}=-\frac{1}{2}\int_{-1}^{1}(D-U)\,q\left(g\left(Y_1^*+\epsilon t\right)\right)g'\left(Y_1^*+\epsilon t\right)dt,$$

where $q(.)$ denotes the density of $Q(.)$. Since $g'=\beta<0$, it follows $\partial\Psi^{L1}/\partial Y_1^*>0$. Next, note that

$$\begin{aligned}\frac{\partial\Psi^{L1}}{\partial\epsilon}&=-\int_{-1}^{1}(D-U)\,q\left(g\left(Y_1^*+\epsilon t\right)\right)g'\left(Y_1^*+\epsilon t\right)t\ dt\\&=(D-U)\,\beta\int_{0}^{1}t\left[q\left(g\left(Y_1^*-\epsilon t\right)\right)-q\left(g\left(Y_1^*+\epsilon t\right)\right)\right]\ dt\le 0,\end{aligned}$$

since $\beta < 0$ and

$$q\left(g\left(Y_1^* - \epsilon t\right)\right) - q\left(g\left(Y_1^* + \epsilon t\right)\right)$$
$$= q\left(\alpha + \beta Y_1^* - \beta\epsilon t\right) - q\left(\alpha + \beta Y_1^* + \beta\epsilon t\right) \geq 0,\ \forall t \in [0,1],$$

since $q(.)$ is increasing. Therefore,

$$\frac{\partial Y_1^*}{\partial \epsilon} \geq 0. \tag{12}$$

Next, note that the best response function $h(x)$ satisfies

$$\int_{Y^f(x)}^{h(x)+\epsilon} (D-U)\frac{1}{2\epsilon}dY + U = 0, \tag{13}$$

where $Y^f(x)$ satisfies

$$g\left(Y^f(x)\right) = \frac{1}{2} + \frac{Y^f(x) - x}{2\epsilon}.$$

Therefore, by the implicit function theorem,

$$\frac{\partial Y^f(x)}{\partial x} = \frac{1}{1 - 2\epsilon g'\left(Y^f(x)\right)} = \frac{1}{1-2\epsilon\beta},$$

and

$$h'(x) = \frac{\partial Y^f(x)}{\partial x} = \frac{1}{1 - 2\epsilon g'\left(Y^f(x)\right)} = \frac{1}{1-2\epsilon\beta} \in (0,1).$$

Noting that $\frac{\partial}{\partial\epsilon}\left(h'(x)\right) < 0$, it follows that an increase in ϵ leads to flattening of $h(x)$. Furthermore, we have that $\frac{\partial Y^*}{\partial\epsilon} = \frac{\partial h(Y^*)}{\partial\epsilon} = 0$ (by Corollary 1 in Iachan and Nenov). Hence, it follows that for $x < Y^*$, $h(x)$ must strictly increase in ϵ, while for $x > Y^*$, $h(x)$ must strictly decrease in ϵ, as shown in Figure 3. Moreover,$\{Y_k^*\}_{k=1}^{\infty}$ is monotone, so that if $Y_1^* < Y_2^*$, then $Y_{k-1}^* < h\left(Y_{k-1}^*\right) = Y_k^*$, $k > 1$, and vice versa for $Y_1^* > Y_2^*$. Noting that $Y_2^* > Y_1^*$, implies $Y_1^* < Y^*$ we conclude that

$$\frac{\partial Y_2^*}{\partial\epsilon} = \frac{\partial h\left(Y_1^*\right)}{\partial\epsilon}\frac{\partial Y_1^*}{\partial\epsilon} > 0.$$

By induction, it follows that

$$\frac{\partial Y_k^*}{\partial \epsilon} > 0$$

for any $k > 1$. Together with (12), we have that $\frac{\partial Y_k^*}{\partial \epsilon} > 0$, $k \geq 1$. $\square$

Test of equilibrium behavior

Table 6 reports the results from two t-tests, where we test whether the average cutoffs differ from the theoretical predictions in each treatment. In the Low Noise treatment, we cannot reject the null hypothesis of equality at standard levels of statistical significance, while in the High Noise treatment the average cutoff is significantly different from the equilibrium prediction at a 10% significance level.[32]

Treatment	Average cutoff	Equilibrium cutoff	p-value
Low noise	37.1	41.4	0.15
High noise	42.9	37.8	0.08

Table 6: Two-sided t-test for equality of average cutoffs and equilibrium cutoffs.

Evolution of average cutoffs

The evolution of the average cutoffs over time is shown in Figure 8, which plots the different average cutoffs in the two treatments for all 8 rounds of play.

[32] 44 decisions are based on signals within the range [37.8, 41.4] in the first round of play. Within this range, the proportion of subjects attacking in the High Noise treatment is 61 %, while the proportion of subjects attacking in the Low Noise treatment is 56 %. The difference is not statistically significant at conventional levels of significance.

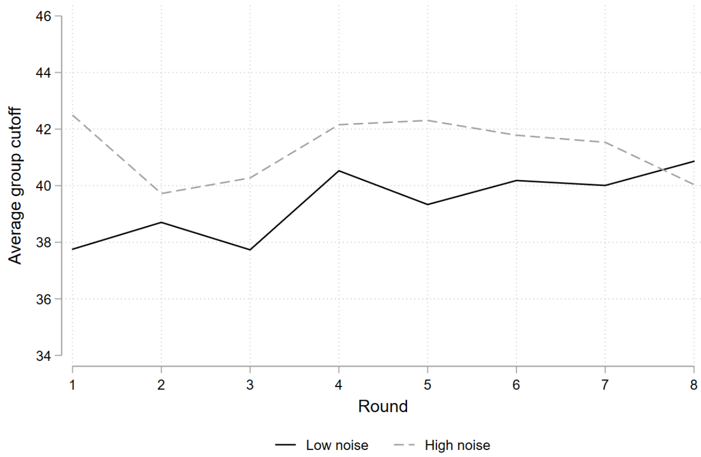


Figure 8: Average cutoff for each treatment by round.

Testing for treatment differences using a rank-sum test

We follow a conservative approach and run a Mann-Whitney U-test where we compare the rank-sums of group cutoffs. The null hypothesis is that the group cutoffs are not higher in the Low Noise treatment than in the High Noise treatment. The results are shown in Table 7. We reject the null hypothesis with a p-value of 8%.

Treatment	Obs	Expected Rank sum	Rank sum
Low Noise	8	68	55
High Noise	8	68	81
	p-value		0.08*

Table 7: One-sided Mann-Whitney U-test of group cutoffs in the two treatments.

Payoffs and strategic cutoffs in the estimated level-k model

In our experiment, each subject received 20 ECU if not attacking. If a subject chooses to attack, the payoff is Y ECU in the case of a successful attack and zero otherwise. We assume players have CRRA preferences with coefficient of relative risk aversion given by α. Therefore, the strategic cutoff of the Lk type, Y_k^*, solves

$$E_{Y_k^*}\left[\frac{Y^{1-\alpha}}{1-\alpha}\tilde{P}_k\left(Y\right)\right]=\frac{20^{1-\alpha}}{1-\alpha}, \tag{14}$$

where the probability of a successful attack that an Lk type assigns, $\tilde{P}_k(Y)$, is defined in Equation (7).

Details on the simulation of the structural model

To simulate the model, we mimic the actual experiment as close as possible and generate simulated experimental data as follows. Each group in the simulated data consists of 10 players who play 80 simulated games (10 games per round $\times 8$ rounds) where a new fundamental is drawn for each simulated game. In each simulated game, each player is assigned a type according to the estimated model and plays according to her type with some probability of trembling. Since each treatment contains 8 groups, we simulate the outcomes from 640 games per treatment ($80 \times 8 = 640$). We compute the mean estimated threshold based on the signals and actions across the 640 games. We then repeat the simulation 1000 times. A more detailed pseudo-code is given below.

Algorithm 1 Simulation

```
G_sim = 640
n_sim = 1000

while n ≤ n_sim

    foreach τ ∈ {l,h}
        while G ≤ G_sim
            - Draw a fundamental Y_G from U[0,100]
            - For each fundamental Y_G, draw 10 sig-
            nals x_{i,G} ~ U[Y_G − ϵ_τ, Y_G + ϵ_τ]
            - Distribute players into different level-
            k types
            - Derive the probability of tremble and as-
            sociated action
            - Assign actions based on trembling proba-
            bility and player type
            - Store individual signals and actions a_{i,G}
        end
        Define A̲_τ ≡ min({x_{i,G}}_{G=1}^{G_sim}) if a_{i,G} = 1
            {x_{i,G}} is the vector of sig-
            nals across all G.
        Define Ā_τ ≡ max({x_{i,G}}_{G=1}^{G_sim}) if a_{i,G} = 0
            {x_{i,G}} is the vector of sig-
            nals across all G.
        Define θ̂_{τ,n} ≡ (A̲_τ + Ā_τ)/2
    end
    Define Δθ̂_n ≡ θ̂_{l,n} − θ̂_{h,n}

end
Plot the CDF of Δθ̂_n
```

Estimated trembling probabilities in the structural model

Figure 9 plots histograms of $\nu d^k \left(a_q^i, \lambda\right)$, the estimated actual trembling probability for each subject i and decision instance q, for the three different Lk-types in our baseline structural model, namely $k \in \{1, 2, \infty\}$, where d^k is given in equation (11). The trembling probabilities are computed using the estimated parameters from Table **??** and the actual signals observed and decisions made by subjects in the first round of play.

Experimental instructions for high-noise treatment (on next page)

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

You will stay in the same group of 10 persons through all of the 8 independent rounds.

Decision situation:

For each situation a number called Y is selected randomly from the interval 0 to 100. This number is the same for all participants. All numbers in the interval [0, 100] 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-20, Y+20]. 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 and for all participants.

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 1/6*(80-Y) , 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.

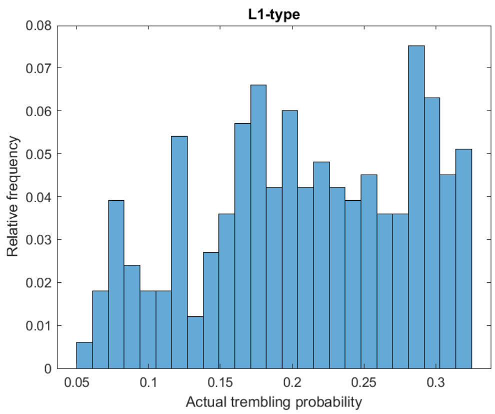
Once all participants made their 10 decisions for the 10 games, a round is terminated. (Remember there are 8 rounds in total).

Information after each round

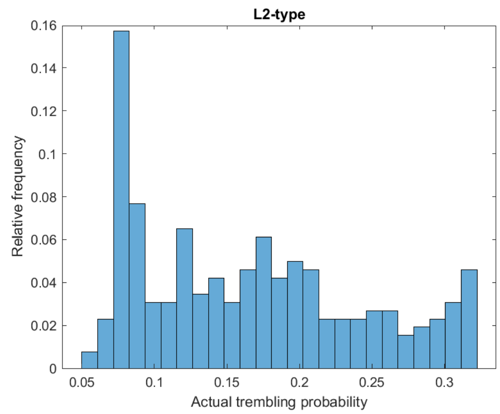
Each participant will be informed after each round for each of the 10 games on
(1) the number Y,
(2) how many participants decided for A or B,
(3) the own payoff over all 10 games.

Figure 9: Estimated trembling probabilities.

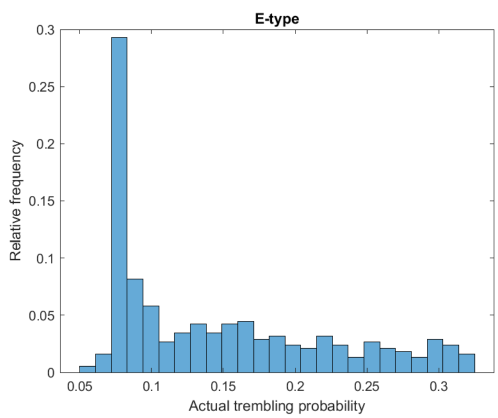
(a) $L1$-types



(b) $L2$-types



(c) Equilibrium types



Example:

The number of participants is 10. The payoff for A is always 20. The unknown number Y, which was drawn, is 40.

The hint numbers drawn for the ten participants are: 35.89, 45.24, 42.67 etc.

The participant with the hint number 35.89 knows that Y is between 15,89 and 55,89, the participant with the hint number 45.24 knows that Y is between 25,24 and 65,24, etc.

Three participants decide for A, seven participants decide for B.

The participants who chose A receive 20 ECU.

In order to receive a positive payoff for B, at least 1/6*(80-40)=6,68 $\approx$ 7 (remember the formula 1/6*(80-Y)) participants have to decide for B (that is 7 or more). Since 7 participants selected B, each of them receives Y = 40.

For the calculation of the minimum number of the participants needed such that payoff for B is positive see attached table:

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 1/6*(80-Y) 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 0 and 100)	Then at least ... of the 10 participants (including yourself) have to select B in order to get a positive payoff
0 to 19.99	More than 10
20.00 to 25.99	10
26.00 to 31.99	9
32.00 to 37.99	8
38.00 to 43.99	7
44.00 to 49.99	6
50.00 to 55.99	5
56.00 to 61.99	4
62.00 to 67.99	3
68.00 to 73.99	2
74.00 to 100.00	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 we ask you to fill out a questionnaire.

Payoffs:

Also at the end of the experiment the ECUs you have obtained are converted into NOK and paid in cash. 17 ECU corresponds to 1 NOK.