Positive expectations feedback experiments and number guessing games as models of financial markets

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Abstract

In repeated number guessing games choices typically converge quickly to the Nash equilibrium. In positive expectations feedback experiments, however, convergence to the equilibrium price tends to be very slow, if it occurs at all. Both types of experimental designs have been suggested as modeling essential aspects of financial markets. In order to isolate the source of the differences in outcomes we present several new experiments in this paper. We conclude that the feedback strength (i.e. the 'p-value' in standard number guessing games) is essential for the results.

Keywords: number guessing game, beauty contest game, expectations feedback systems

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1. Motivation

In a famous quote Keynes (1936) describes financial investment as a game in which players try to predict average predictions:

"...professional investment may be likened to those newspaper competitions in which the competitors have to pick out the six prettiest faces from a hundred photographs, the prize being awarded to the competitor whose choice most nearly corresponds to the average preferences of the competitors as a whole..."[1]

This beauty contest analogy is often cited in papers on higher order beliefs[2] and has inspired an increasing number of theoretical and experimental contributions to economics and finance (for a recent theoretical study see e.g. Allen et al., 2006). Most experiments focus on (variations of) the so-called number guessing game (see e.g. Nagel, 1995). In this game all players have to simultaneously submit a 'guess' from a certain interval (typically 0-100) and the winner is the player whose choice is closest to a given fraction (typically 2/3) of the average of these chosen numbers. This game has a unique Nash-equilibrium and the distance between a specific guess and the equilibrium value can be considered a measure of the belief this player has about the rationality of the population of players, and about the distribution of the higher order beliefs about rationality in the population.

The general findings from the experimental literature on repeated number guessing games are that first period choices are not very close to the Nash equilibrium but convergence to that equilibrium is fast (typically within 4-5 periods) and stable. As a characterization of behavior of financial markets this fast convergence is surprising for at least two reasons.

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[1] The quote continues with: "...so that each competitor has to pick, not those faces which he himself finds prettiest, but those which he thinks likeliest to catch the fancy of the other competitors, all of whom are looking at the problem from the same point of view. It is not a case of choosing those which, to the best of one's judgment, are really the prettiest, nor even those which average opinion genuinely thinks the prettiest. We have reached the third degree where we devote our intelligences to anticipating what average opinion expects the average opinion to be. And there are some, I believe, who practise the fourth, fifth and higher degrees." See Keynes (1936), page 156.

[2] See for example, Biais and Bossaerts (1998), Ho et al. (1998), Camerer et al. (2004) and Costa-Gomes and Crawford (2006). An alternative financial market interpretation of the guessing game is that it models the problem of leaving a market just before prices start going down, see Duffy and Nagel (1997) and Ho et al. (1998).

First, empirical evidence suggests that asset markets are in fact not that stable. Shiller (1981, 2000), for example, shows that stock prices are excessively volatile: their variance is higher than that of the underlying fundamental value. Behavioral finance (for recent overviews see Shleifer, 2000, Barberis and Thaler, 2003) has shown that (1) many price movements are unrelated to news but are reactions to price changes (for example caused by investors using technical analyses) and that (2) prices under-react to news, causing short-term trends. Mis-pricing cannot always be arbitraged away (Shleifer and Vishny, 1997) and market prices may therefore deviate substantially from their fundamental values for a longer period of time. Mis-pricing and over- and under-reaction has also been established experimentally. Smith et al. (1988) discuss experimental asset markets that feature bubbles and crashes in asset prices. Noussair et al. (2001) show that these bubbles and crashes even emerge when the fundamental value is constant, instead of deterministically decreasing. Kirchler (2009) establishes under-reaction in an experimental asset market with a fluctuating fundamental value. Finally, in an expectation feedback experiment with some large permanent shocks to the fundamental value, Bao et al. (2010) argue that there may be under-reaction of realized prices to these shocks in the short run, but over-reaction in the long run.

Second, evidence from expectations feedback experiments (see e.g. Hommes et al., 2005, 2008, Heemeijer et al., 2009) does not seem to be consistent with the results from number guessing game experiments. Expectations feedback experiments are based upon the idea that asset markets (just like many other economic environments) are *expectations feedback systems*. Price expectations of traders determine their trading behavior which, in turn, determines the realized trading price. In an expectations feedback experiment participants have to submit their forecast of the future price of a certain asset and are paid according to their prediction accuracy. A computer program determines the optimal trades associated with the forecasts and the resulting realized trading price. The advantage of this design over traditional experimental asset markets is that it gives a clearer picture of how people form expectations in expectations feedback environments.[3] In prediction experiments with a positive expectations feedback (that is, where an increase in average predictions

[3] In more traditional asset market experiments participants are also sometimes asked to submit price predictions, but it is difficult to give the appropriate incentives for providing these predictions and often they come about as a by-product to the experiment. For a more rigorous approach to expectation formation in experimental asset markets, see Haruvy et al. (2007).

leads to an increase in the realized market price) there is a remarkable tendency for participants to coordinate on a common prediction strategy but no (or only slow) convergence to the equilibrium price.

These positive feedback prediction experiments are closely related to the number guessing game, but with very different results.[4] Nevertheless, the experimental designs do differ in a number of dimensions, particularly the *feedback strength* from expectations (guesses) to realized price (target number), the *information* given to the participants, and the *incentive structure*. It is, a priori, not evident which of these design differences is responsible for the differences in outcomes. This paper reports on a series of experiments that are designed to isolate the main determinants. Our main finding is that only feedback strength has a substantial impact upon convergence, although it does not seem to have a significant effect upon prediction accuracy or coordination of expectations. Providing more information to the participants, and/or introducing a winner-takes-all incentive scheme has no significant effect upon convergence, prediction accuracy or coordination, although the winner-takes-all incentive scheme does lead to a substantial increase in the number of "spoilers", i.e. sudden large and erratic deviations in individual predictions.

The remainder of this paper is organized as follows. In Section 2 we will briefly review the experimental literature on number guessing games and positive expectations feedback experiments and discuss the differences in design characteristics and outcomes between these two types of experiments. The design of five new experimental studies will be briefly discussed in Section 3 and the results of these new experimental studies will be analyzed in Section 4. Section 5 concludes.

2. Number Guessing Games and Expectation Feedback Experiments

2.1 Number guessing games

The typical *number guessing game* experiment has the following structure.[5] The game is played for T periods with a fixed group of H participants. In each period t

[4] When we started our experimental research on expectation feedback markets we were not fully aware of the close connection with guessing games. This connection only became apparent to us when we changed from using a market-clearing environment (Hommes et al., 2005), where participants had to predict two periods ahead, to a market-maker environment (Heemeijer, et al., 2009) where participants only have to predict one period ahead (in Section 2.2 we will discuss the differences between these two types of expectations feedback experiments in more detail).

[5] Moulin (1986) was the first to discuss this game.

participants simultaneously choose numbers $x_{h,t}^{e}$ from the interval $[l,u]$. The so-called *target number* is given as[6]

$$x_t = \alpha + \beta \overline{x}_{h,t}^{e}, \tag{1}$$

where $\alpha \geq 0$ and $0 < \beta < 1$ are fixed parameters[7] and $\overline{x}_{h,t}^{e} = \frac{1}{H}\sum_{h=1}^{H} x_{h,t}^{e}$ is the average number chosen in period t. The participant for which $\left|x_{h,t}^{e} - x_t\right|$ is smallest wins a prize in that period. If several participants have the best guess the prize is split evenly between them. The rules of the game are common knowledge and between periods participants receive feedback about the previous periods' guesses of all participants, the target number and the winning number.

From (1) it is easy to see that the Nash equilibrium of the number guessing game corresponds to $x^* = \frac{\alpha}{1-\beta}$, provided $l \leq x^* \leq u$: if all participants choose x^* the target number indeed equals x^*. Alternatively, this equilibrium can be found by iterative elimination of dominated strategies.[8]

Finding the Nash equilibrium, for example by iterative elimination of dominated strategies, requires a (potentially infinite) number of steps of reasoning. The number guessing game is a powerful device to study this *depth of reasoning*, as follows. So-called level-0 players randomly select a guess from the interval $[l,u]$. A

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6 In the literature the parameter β is often denoted *p* and the corresponding guessing game is then sometimes referred to as a "*p*-beauty contest". We depart from that convention here, since the variable *p* is used to denote prices in the remainder of this paper.

7 Most of the guessing games restrict attention to $0 < \beta < 1$, but some of the earlier studies did consider $\beta > 1$. Nagel (1995), for example, has one treatment with $\beta = \frac{4}{3}$ and Ho et al. (1998) discuss treatments with $\beta = 1.1$ and $\beta = 1.3$. Moreover, Sutan and Willinger (2009) discuss experiments on a guessing game with *negative feedback*, i.e. $\beta < 0$, and show that it converges faster than the (positive feedback) guessing game with $\beta > 0$. On the impact of the sign of the feedback, also see Heemeijer et al. (2009).

8 This works as follows. Given that choices have to be in the interval $[l,u]$ the target number always lies in the interval $[\alpha + \beta l, \alpha + \beta u]$. Numbers outside this interval are dominated and can therefore be eliminated. Assuming that no participant chooses a dominated action it follows that the target number must lie in the interval $[\alpha + \beta(\alpha + \beta l), \alpha + \beta(\alpha + \beta u)]$. This implies that all numbers in the intervals $(\alpha + \beta l, \alpha + \beta(\alpha + \beta l))$ and $(\alpha + \beta(\alpha + \beta u), \alpha + \beta u)$ are dominated and can be eliminated, and so on. Eventually, this process of iterative elimination of dominated strategies leads to x^*.

level-1 player believes all other players are level-0 players, and therefore plays a best response to the expected random choice of the level-0 players, $x^1 = \alpha + \beta x^0$, where x^0 corresponds to the expected average choice of the level-0 players.[9] A level-2 player believes that all other players are level-1 players and therefore best responds to x^1, that is, $x^2 = \alpha + \beta x^1$, and so on. By looking at first period choices the number guessing game can be used to classify subjects into different depth of reasoning types.

The number guessing game has been studied extensively in laboratory experiments (for overviews, see Nagel, 1999, and Camerer et al., 2003), typically with $\alpha = 0$ and very often with $\beta = \frac{2}{3}$ and $[l,u] = [0,100]$. The first of these experiments was reported by Nagel (1995) who considered groups of 15-18 participants playing the game for four periods. Her main conclusions are: (i) First period choices are significantly different from the Nash equilibrium prediction[10] and almost all of these choices correspond to level-0 up to level-3 depth of reasoning; (ii) In subsequent periods there is rapid convergence to the Nash-equilibrium, without an increase in the depth of reasoning.

These results have been corroborated by many other experiments. Ho et al. (1998) show that convergence to the Nash equilibrium is faster when β is farther away from 1, groups are larger, and participants are experienced. Duffy and Nagel (1997) show that when the target number is based upon the median guess (maximum guess) instead of the mean guess, convergence is faster (slower). Nagel's results where also confirmed in three large scale one-shot number guessing games, run through newspapers in Germany, Spain and the U.K. and involving thousands of participants (Bosch-Domènech et al., 2003).

All of the experiments discussed above use $\alpha = 0$, implying that the Nash equilibrium lies at the boundary of the action space (typically $x^* = 0$). Some authors have looked at number guessing games with interior equilibria ($\alpha > 0$), particularly, Camerer and Ho (1998), Güth et al. (2002) and Kocher and Sutter (2006). The last two papers also depart from the standard winner-takes-all payoff incentive scheme

[9] Note that level-1 players do not take into account their own effect upon the target number and furthermore believe that all other players are level-0 players.

[10] Even in two-player guessing games where the Nash-equilibrium prediction, 0, strictly dominates all other choices it is only chosen by less than 10% of students and by about one third of professionals (Grosskopf and Nagel, 2008).

and reward all participants based upon the absolute distance between their guess and the target number.[11] Güth et al. (2002) conjecture that, because participants try to avoid extreme choices, convergence in games with interior equilibria is faster than in games with boundary equilibria. Their experiment confirms this. Moreover, the fraction of equilibrium choices in the 'interior equilibrium' game is significantly higher than in the 'boundary equilibrium' game, which they attribute partly to the payoff scheme.[12]

The number guessing game has been used as a vehicle for investigating a number of other issues. Weber (2003) shows that participants still learn, albeit at a slower rate, if no feedback is given between periods. Kocher et al. (2007) and Sbriglia (2008) show that additional information, such as strategies of the winners in earlier periods, or strategies from participants in an earlier number guessing game, facilitates faster convergence to the Nash equilibrium. Slonim (2005) finds that experienced players, when matched with inexperienced players, win the game more often and make choices farther away from the equilibrium. Finally, Kocher and Sutter (2005) and Sutter (2005) show that teams of players learn faster than individuals and increase convergence speed.

2.2 Positive expectations feedback experiments

Consider the following textbook asset pricing model (for reviews, see Cuthberson, 1996, Campbell et al., 1997, and Brock and Hommes, 1998). There are H traders who divide their wealth between two assets. The first asset is risk free, with fixed return $R = 1 + r$, where $r > 0$ is the interest rate. This asset is in perfect elastic supply and its price is normalized to one. The infinitely lived risky asset, with price p_t in period t, is in fixed aggregate supply z^s and returns uncertain dividends y_t in period t, which are independently and identically distributed with mean $\bar{y}$. A trader's demand depends upon his expectation of $p_{t+1} + y_{t+1} - Rp_t$, which is the excess return of the risky asset. Assuming trader h is a mean-variance maximizer his demand for the risky asset in period t is given by

[11] Also Costa-Gomes and Crawford (2006) use a payoff scheme that depends upon the absolute distance between the guess and the target number.

[12] Morone and Morone (2008), however, argue that the results by Güth et al. (2002) are partly due to their parameterization. They show that, although first period choices are indeed closer to the equilibrium when the equilibrium is interior, speed of convergence to the equilibrium may be higher when the equilibrium is on the boundary.

$$z_{h,t} = \frac{E_{ht}\left(p_{t+1} + y_{t+1} - Rp_t\right)}{a\sigma^2}, \tag{2}$$

where $E_{ht}\left(p_{t+1} + y_{t+1} - Rp_t\right)$ denotes trader h's belief about next period excess return and $V_{ht}\left(p_{t+1} + y_{t+1} - Rp_t\right) = \sigma^2$ corresponds to his beliefs about the variance of excess returns, which is assumed to be constant over time and the same for all traders. Finally, a is a risk aversion parameter (again assumed to be the same for all traders).

In period t aggregate excess demand for the risky asset is given by

$$\xi_t = \sum_{h=1}^{H} z_{h,t} - z^s = \sum_{h=1}^{H} \frac{E_{h,t-1}\left(p_t + y_t - (1+r)p_{t-1}\right)}{a\sigma^2} - z^s. \tag{3}$$

In order to close the asset market model we need to specify a model for price formation.

Market clearing. Under *market clearing* the price adjusts in every period in such a way that excess demand vanishes. That is, the price p_t in period t is implicitly determined as the solution to $\xi_t = 0$. Hommes et al. (2005) report on experiments in this setting. Letting $E_{h,t}\left(p_{t+1} + y_{t+1}\right) = p^e_{h,t+1} + \overline{y}$ and assuming $z^s = 0$ they obtain

$$p_t = \frac{1}{1+r}\left[\overline{p}^e_{h,t+1} + \overline{y} + \varepsilon_t\right], \tag{4}$$

where $\overline{p}^e_{h,t+1} = \frac{1}{H}\sum_{h=1}^{H} p^e_{h,t+1}$ is the average price prediction and ε_t corresponds to (small) stochastic demand and supply shocks. Note that the actual realization of *today*'s price p_t depends upon people's belief of *tomorrow*'s price p_{t+1}. This implies that, when having to predict p_{t+1}, traders only have information about prices up to period $t-1$. Intuitively, the reason why investors have to predict two periods ahead is that in order to make a profit an investor first has to buy (short sell) an asset in period t and after that sell (buy) it in period $t+1$. Also observe that $p^f = \frac{\overline{y}}{r}$ corresponds to the *fundamental* value of the risky asset (i.e. the discounted value of the stream of future dividends). If, on average, traders predict $\overline{p}^e_{h,t+1} = p^f$ the actual price will, in expectation, equal p^f as well.

Participants in the experiment by Hommes et al. (2005) are explained that they are the advisor of a large investor, e.g. a pension fund. Their task is to predict future prices in a stock market and their reward depends on their prediction accuracy. They are told the investor will take a position in the market that depends on their prediction of future prices (see Appendix A for complete instructions) and that there are other large investors in the market advised by other participants. They are *not* told the precise formula used to calculate the realized price, but they know the direction of the feedback structure (if many participants expect high (low) prices, investors will buy (sell) more stocks and the price will increase (decrease)). The participants receive information in each period about previous prices and their *own* predictions, both in a graph and a table.

For the experiment fourteen groups were investigated, with each group consisting of $H = 6$ participants, predicting prices for 51 periods.[13] Reported predictions had to be between 0 and 100 and two decimals could be used. The risk free rate of return, $r = 0.05$, and the mean dividend, $\bar{y} = 3$, were fixed such that the equilibrium price equals $p^f = 60$.[14] The same realization of shocks ε_t, independently drawn from $N\left(0, \frac{1}{4}\right)$, was used for all groups.

Participants could earn 1300 points each period. The number of points earned in period t by participant h was inversely related to the forecast error as follows

$$e_{h,t} = \max\left\{1300 - \frac{1300}{49}\left(p_t - p_{ht}^e\right)^2, 0\right\}, \tag{5}$$

where 1300 points is equivalent to 0.50 euro. To avoid negative earnings, earnings in period t were zero when $\left|p_t - p_{ht}^e\right| \geq 7$. This payoff scheme was common knowledge.

The upper panels of Figure 1 show the prices and predictions in a representative group (group 1) from Hommes et al. (2005). Two features are apparent.

13 In some groups robot traders were added. These robot traders always predicted the fundamental price and make a trading decision based upon this prediction. The impact of robot traders in these markets is endogenous: the greater the distance between the actual price and the fundamental price the more these fundamental traders will invest, and the other way around. They therefore act as a 'stabilizing force' pushing prices in the direction of the fundamental price. Behavior in markets with robot traders is qualitatively similar to behavior in markets without robot traders (for details see Hommes et al., 2005).

14 For three of the 14 groups interest rate and dividend are given by $r = 0.05$ and $\bar{y} = 2$ resulting in an equilibrium price of $p^f = 40$, which (in contrast to the equilibrium price for the other groups) is *below* the midpoint of the interval from which predictions can be chosen. Behavior in these three groups is qualitatively the same as the behavior in the other groups (for details see Hommes et al., 2005).

First, the asset price shows persistent and significant deviations from its fundamental value (upper left panel). Secondly, the dispersion of individual predictions is remarkably small (upper right panel). Participants seem to coordinate on a common prediction strategy. Both features are robust: systematic deviations of the price from the fundamental value and coordination of prediction strategies are exhibited by all 14 groups.[15]

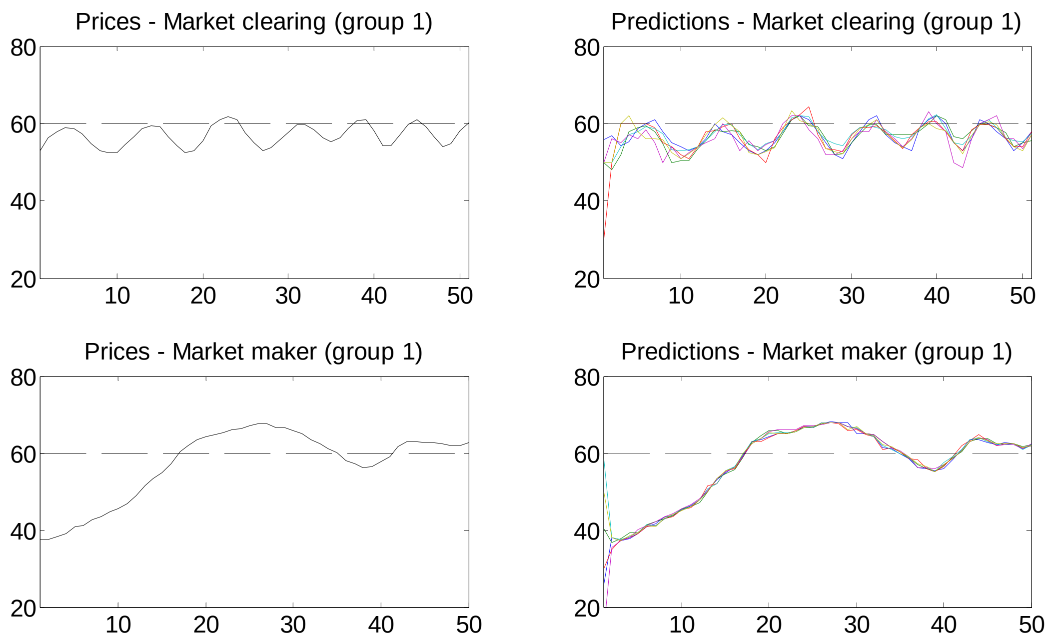

Figure 1: Upper panels show prices (left) and predictions (right) from group 1 in Hommes et al. (2005a), lower panels show prices (left) and predictions (right) from group 1 in Heemeijer et al. (2009).

Market maker. An alternative model of price formation is one where prices are set by a *market maker*. In that scenario traders report their demands (2) to a market maker who, like the well-known Walrasian auctioneer, aggregates excess demands and increases (decreases) the price of the risky asset when there is excess demand (supply) for the risky asset (see e.g. Beja and Goldman, 1980). That is, prices change according to

15 Hommes et al. (2008) discuss experiments without robot traders and without an upper limit for price predictions. Coordination of expectations persists in that framework, together with even more severe bubbles and crashes. Bottazzi and Devetag (2005) and Bottazzi et al. (2009) study a variant where participants give a confidence interval for the realized price, instead of a point prediction, and where they are rewarded on the basis of the increase in wealth their predictions generate. In this setting the incidence of bubbles decreases and heterogeneity of predictions increases.

$$p_t = p_{t-1} + \lambda\left(\sum_{h=1}^{H} \frac{E_{h,t-1}\left(p_t + y_t - (1+r)p_{t-1}\right)}{a\sigma^2} - z^s\right). \tag{6}$$

Here $\lambda > 0$ is a parameter that measures the *speed of adjustment*.

Heemeijer et al. (2009) reports on experiments with 7 groups of $H = 6$ participants each, predicting prices for 50 periods. Parameter values are fixed such that $r = 0.05$, $a\sigma^2 = 6$, $z^s = 1$, $\lambda = \frac{20}{21}$ and $E_{h,t}(y_t) = 3 + z^s$ for all h and t. This results in:

$$p_t = \frac{20}{21}\left(3 + \overline{p}_t^e\right) + \varepsilon_t. \tag{7}$$

Again $\varepsilon_t \sim N\left(0, \frac{1}{4}\right)$ is a random term, representing e.g. small random fluctuations in the supply of the risky asset. The equilibrium price is $p^f = 60$. No upper limit on the price predictions was enforced (with the exception of the first period, which had to be between 0 and 100). As before, payoffs were based upon the quadratic forecasting error function (5) and the exchange rate was 2600 points for 1 euro.

The lower panels of Figure 1 show the prices and predictions in a representative group (group 1) from Heemeijer et al. (2009). As in the market clearing experiment there is no apparent convergence to the fundamental steady state although fluctuations around the steady state appear to have a lower frequency. Moreover, again participants seem to coordinate their prediction strategies quite well. The other six groups show a similar pattern.

The main features of the positive expectations feedback experiments, systematic deviation of prices from fundamentals and coordination of predictions, therefore seem to be quite robust (see e.g. Leitner and Schmidt, 2007, for similar results in an exchange rate experiments).

2.3 A comparison of number guessing games and positive expectations feedback experiments

The asset pricing experiment from Heemeijer et al. (2009) is closely related to the standard formulation of the number guessing game. In fact, the price generating mechanism (7) is a special case of (1) with $\alpha = \frac{60}{21}$ and $\beta = \frac{20}{21}$. The results from the two types of experiments are quite different however. In number guessing game experiments choices typically convergence to the steady state within a small number of periods, whereas prices and predictions in positive expectations feedback experiments keep on fluctuating, as is obvious from Figure 1. Both findings seem to be robust.

There are several differences in the designs of the two types of experiments that may be responsible for these qualitative differences. Three important differences in design are listed below.

Structure: First, in standard number guessing games (where $\alpha = 0$) typically the Nash equilibrium is on the boundary of the action space ($x^* = 0$), whereas positive expectations feedback experiments (where $\alpha > 0$) typically have an interior equilibrium with a strictly positive price for the risky asset, $p^f > 0$. Obviously, oscillations around a boundary equilibrium are by construction impossible. On the other hand, Güth et al. (2002) argue that an interior equilibrium in a number guessing game leads to *faster* convergence. An explanation for this may be that in case of an equilibrium value at the boundary convergence is only possible with coordination of choices (all players choose the equilibrium number) while uncoordinated choices scattered around an internal equilibrium can still lead to an equilibrium outcome. A second structural difference is that the *feedback strength* parameter β is much smaller in most number guessing games (typically, $\beta = \frac{2}{3}$) as compared to the positive expectations feedback experiments discussed above (where $\beta = \frac{20}{21} \approx 0.95$). The highest feedback strength value in number guessing games that we know of (abstracting from values of β larger than 1) is $\beta = 0.9$ in Ho et al. (1998), which nevertheless leads to results that are qualitatively similar to other number guessing game experiments. Finally, in asset pricing experiments a small stochastic parameter ε_t is added in every period. Apart from these differences in structure also the fact that the number of repetitions in the guessing game is much smaller (typically 4-10) than the number of repetitions in expectation feedback experiments may contribute to some of the differences.

Information: In number guessing game experiments the game that is being played is common knowledge. Moreover, participants know the number of other players and typically even receive feedback, in each round, about the chosen numbers of these other participants. In expectations feedback experiments, on the other hand, participants only have qualitative information about the underlying game. They do not have information about the number of other players in the game, nor do they see the price predictions of these other players. The reason for not providing the participants with the price formation formula is that this remains closer to the reality of real world markets: traders typically do not know how other traders' trading decisions are related to their expectations. Also, the information in expectations feedback experiments has an economic frame, whereas in guessing games the problem is posed as an abstract game, without any reference to investment decisions, stock prices or financial markets. In general, one could argue that the level of complexity of the presentation of expectations feedback experiments is higher than that of the guessing game, which is relatively straightforward. This higher level of complexity might be a possible explanation of the difference in results for these two types of experiments.

Incentives: Guessing games use a winner-takes-all tournament structure, whereas expectations feedback experiments reward on the basis of prediction accuracy. The latter incentive structure is also based upon real life: a stock market is typically not a winner-takes-all situation. The only number guessing game experiments departing from the tournament structure are Güth et al. (2002), Kocher and Sutter (2006) and Costa-Gomes and Crawford (2006). The former suggest that the large fraction of equilibrium choices in their experiment is due to what they call the "continuous payment scheme". Moreover, Kocher and Sutter (2006) suggest that the winner-takes-all scheme might lead single players to retire mentally, or to start experimenting. On the other hand: one could argue that a tournament structure forces participants not only to predict accurately, but to predict better than others. This may inhibit satisficing behavior and force participants to think harder about the game and make prices converge faster.

In the next section we will discuss and analyze a new set of experiments to test to which of these differences in design (structure, information or incentives) the differences in results between number guessing games and asset pricing experiments can be attributed.

3. Bridging the gap: Design of new experiments

In this section we report on three new expectations feedback studies in which parameters and experimental design are varied in an attempt to bridge the gap between number guessing games and positive expectations feedback experiments. Table 1 gives an overview.

Our approach is the following. Starting with the experiment from Heemeijer et al. (2009), henceforth referred to as experiment MM, we change in each new experiment[16] one design parameter in the direction of the typical number guessing game solution, in order to find the design parameter that is responsible for the difference in outcomes between number guessing games and positive expectations feedback experiments. All other elements of the design (procedures, instructions, etc) are held constant. The subject pool consists of undergraduate students from the University of Amsterdam, typically from economics, psychology and chemistry. Earnings for participants in the winner-takes-all experiments TN, TI, LTN and LTI are, by construction, 25 euro on average (in each group of 6 participants there was a prize of 3 euro in each of the 50 periods) and range from a minimum of 9 euro to a maximum of 52 euro. Earnings in the experiments MM and LQ ranged from 19.53 to 24.72 euro, with a mean of 23.46, earnings in the (older and more complex) MC experiment ranged from 8.64 to 24.86 euro and were 21.46 euro on average Furthermore, all participants received a show-up fee of 5 euro. See appendix A for the procedures, a screenshot and the instructions.

[16] We use the term experiment instead of treatment because each study was designed after we knew the results of the previous one. Also note that we do not use a complete 2x2x2 design, but that we instead consider a sequence of design changes that will allow us to uncover the main determinant of the difference between the two types of experiments in the most efficient manner.

	Price Formation	*Incentives*	*Information*	*Equilibrium*	*Feedback strength*	*N groups x groupsize*	*Results*
Traditional number guessing game	Market maker	Tournament	Complete	Boundary (0)	0.67		Fast convergence to fundamental value
Hommes et al. (2005a) (***MC***)	**Market clearing**	**Quadratic error**	**Limited**	**Interior**	**0.95**	14x6	Fast coordination, no/slow convergence to fundamental
Heemeijer et al. (2009) (***MM***)	**Market maker**	Quadratic error	Limited	Interior	0.95	7x6	Fast coordination, no/slow convergence to fundamental
TN	Market maker	**Tournament**	Limited	Interior	0.95	6x6	Fast coordination, no/slow convergence to fundamental, spoilers
TI	Market maker	Tournament	**Complete**	Interior	0.95	6x6	Fast coordination, no/slow convergence to fundamental, spoilers
LTN	Market maker	Tournament	**Limited**	Interior	**0.67**	6x6	Fast convergence to fundamental value, spoilers
LTI	Market maker	Tournament	**Complete**	Interior	0.67	6x6	Fast convergence to fundamental value, spoilers
LQ	Market maker	**Quadratic error**	Complete	Interior	0.67	6x6	Fast convergence to fundamental value

Table 1: Properties of the traditional number guessing game and the studies reported in the present paper. The last five rows are new experiments run for this article. Bold entries refer to dimensions in the design that have changed in relation to the previous experiment.

4. Results

This section first discusses the results of the three new expectation feedback experiments one by one. After that the results of these and previous experiments are compared on the aspects convergence, prediction accuracy and coordination of expectations. Furthermore, the occurrence of spoilers is analyzed.

4.1 Incentives: Winner-takes-all instead of quadratic error payoffs

The pay-off function used in the first two studies is based on the quadratic forecasting error and is rather flat at the optimum; small errors are barely punished. Less precision in forecasting can have large consequences in a tournament if the best competitor makes forecasting errors of a comparable size. Tournament incentives can thus motivate participants to be more precise. On the other hand, it may demotivate those participants whose predictions are of a lower quality and who do not expect to win anyhow.

In experiment TN a tournament incentive structure (like the one typically used in guessing games) is used: in each period the participant with the smallest forecasting error receives a prize of 3 euro (in case of two or more winners the prize is split evenly). All other aspects of the design are the same as in Heemeijer et al. (2009), except that predictions of more than 1000 were not accepted by the computer program. We run 6 groups with $H = 6$ participants predicting prices for 50 periods.

Figure 2 shows predictions and prices for all periods and all groups. Recall that the equilibrium price corresponds to $p^* = 60$. The time series of predictions and prices has three important features. First, on several occasions one of the participants submits a very high price prediction. This is particularly evident in groups 1, 2 and 3. In groups 1 and 2 predictions of 999 are submitted, in group 3 a prediction of 999.9 is submitted and in groups 2 and 3 predictions of 1000 are submitted.

Also in the other three groups there are occasionally rather uncommon predictions. Obviously, these so-called "spoilers" destabilize the dynamics and inhibit convergence to the equilibrium price.[17] We will analyze these spoilers in more depth in Section 4.4.4. The second feature is that there is no apparent or fast convergence to the equilibrium price. The third feature corresponds to the high degree of coordination

[17] This terminology is due to Ho et al. (1998) who also find a subtantial number of these spoilers in their guessing game experiment. The occurrence of these spoilers seems to be typical for winner-takes-all incentives.

of predictions (in the absence of spoilers). These two last features can be easily checked by inspection of the graphs for groups 4, 5 and 6, but they also hold for those periods in groups 1, 2 and 3 before the first spoiler has occurred.

Summarizing, introduction of winner-takes-all incentives leads to an increase in “spoilers”, but it does not appear to have a significant influence on convergence and coordination.

4.2 Information: Complete instead of limited information

In the expectations feedback experiments discussed so far the explicit price function was not available for the participants. In the typical number guessing game, however, players know exactly how the target number is calculated from the reported numbers. We decided to run two sessions (six groups) in which the price function was given and explained to the participants. As in the TN experiment participants were rewarded on a winner-takes-all basis. Figure 3 shows the results.

Compared to the TN experiment the number of “spoilers” seems to be even higher. In particular, predictions of 999 or 1000 were submitted in four of the six groups, resulting in an overall decrease in the rate of convergence. For groups 2 and 3, however, it is obvious that there is little convergence, even in the absence of spoilers. The “pre-spoiler” predictions in groups 4, 5 and 6 also don't show a fast convergence.

Even with a winner-take-all payment structure and complete information about the price generating mechanism the dynamics are characterized by persistent deviations from the fundamental price and coordination of individual predictions.

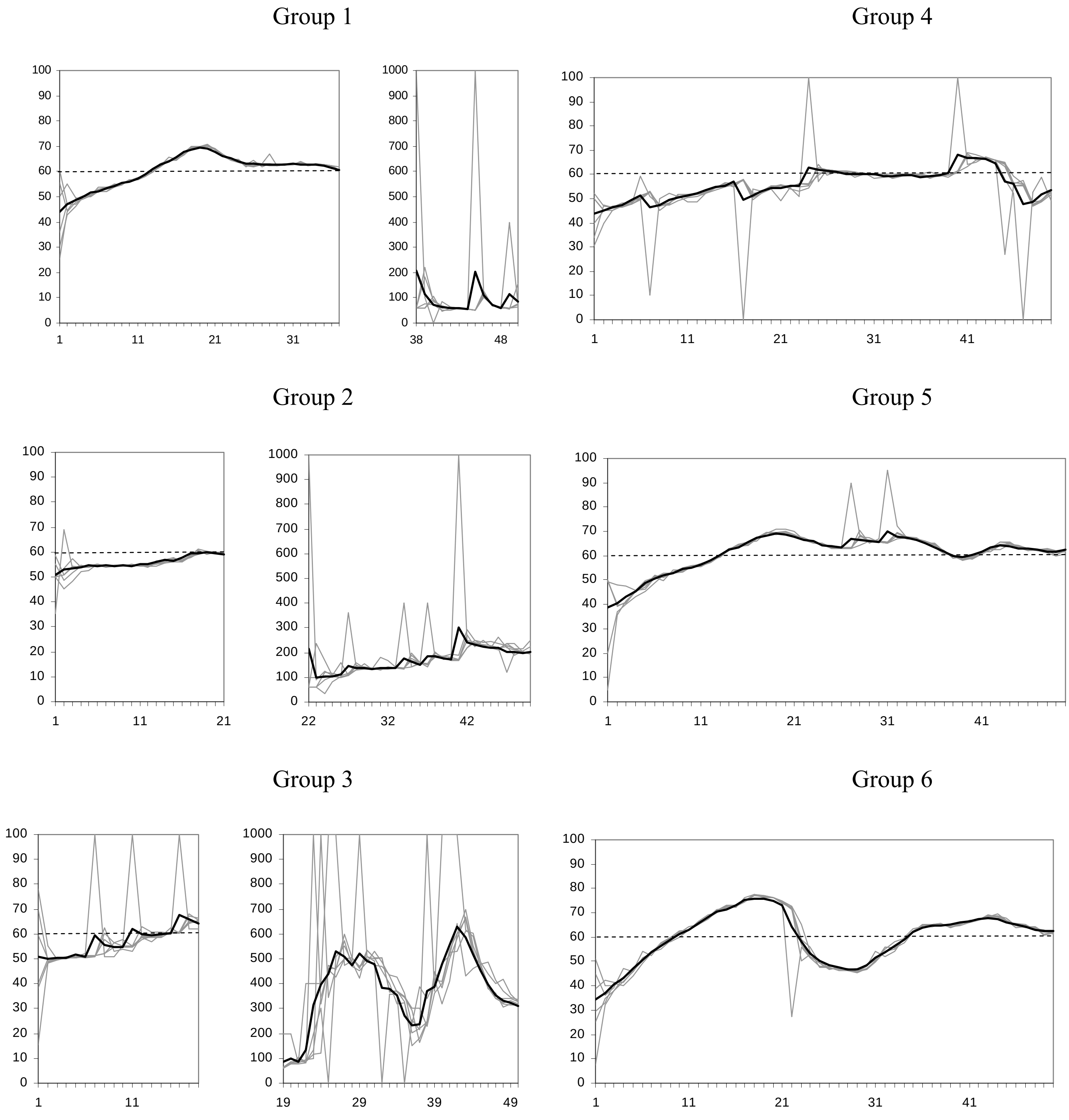

Figure 2: Prices (solid line), predictions (gray line) and equilibrium price (dotted line) in 6 groups with tournament incentives and limited information. In the first 3 groups the graph is split in two and rescaled: the periods before a participant submitted a very high prediction leading to a realized price higher than 100, and afterwards. For clarity the equilibrium price is not displayed in the rescaled parts of the figure.

4.3 Feedback strength

The remaining three experiments we ran, referred to as experiment LTN, LTI and LQ, respectively, considered a change in the feedback strength. [18] In particular, the price generating mechanism for each of these experiments was given by:

$$p_t = \frac{2}{3}\left(30 + \overline{p}_t^e\right) + \varepsilon_t, \qquad (8)$$

Note that equation (8) follows from (6) by taking parameter values $r = 0.5$, $a\sigma^2 = 6$, $z^s = 1$, $\lambda = \frac{2}{3}$ and $E_{h,t}(y_t) = 30 + z^s$ for all h and t. Also observe that (8) requires a substantial interest rate of 50%. The slope of (8) equals $\frac{2}{3}$ which corresponds to the typical value used in number guessing game experiments.

In experiments LTN and LTI participants are rewarded based upon winner-takes-all incentives, and in experiment LQ participants are rewarded based upon quadratic forecasting error. Furthermore, in experiments LTI and LQ the price function (8) was common knowledge (as in experiment TI), whereas in experiment LTN it was not (as in experiments MM and TN). As before an upper limit of 1000 to the prediction was imposed after period 1.

Ho et al. (1998) provide an indication that a less steep slope could enhance convergence. They report that a higher factor (0.9 instead of 0.7) in a standard number guessing game with 7 participants causes mean choices to be farther from the equilibrium value 0. Their Figures 2A and 2C suggest that the difference is largest in the first 5 periods. It is not clear in advance whether their results will also hold in the interior equilibrium case.

Figures 4-6 shows the results for our experiments LTN, LTI and LQ, respectively. For each experiment all six groups converge very fast to the equilibrium. Moreover, in the winner-takes-all experiments LTN and LTI there are a substantial number of spoilers, whereas in experiment LQ the number of spoilers is rather limited.

[18] Originally we only ran experiment LQ. A referee and an associate editor urged us to also run experiments LTN and LTI, in particular since the feedback strength plays a crucial role in the convergence results. We are thankful for this suggestion and believe the results are more robust with these two additional experiments.

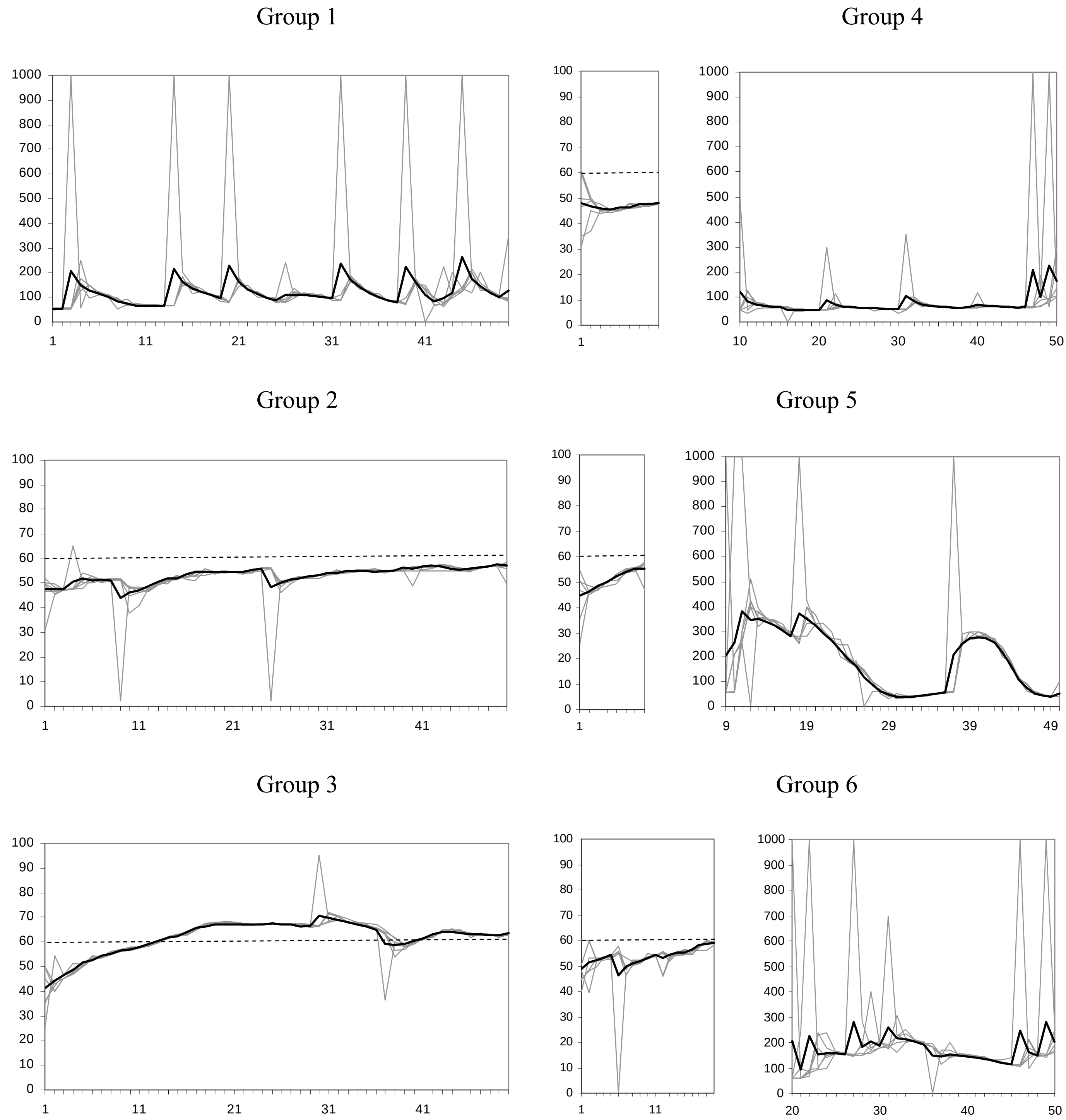

Figure 3: Prices (solid line), predictions (gray line) and equilibrium price (dotted line) in 6 groups with tournament incentives and full information. For groups 4, 5 and 6 the graph is split in two and rescaled: the periods before a participant submitted a very high prediction leading to a realized price higher than 100, and afterwards. In group 1 a prediction of 1000 was submitted already in period 3. For clarity the equilibrium price is not displayed in the rescaled parts of the figure.

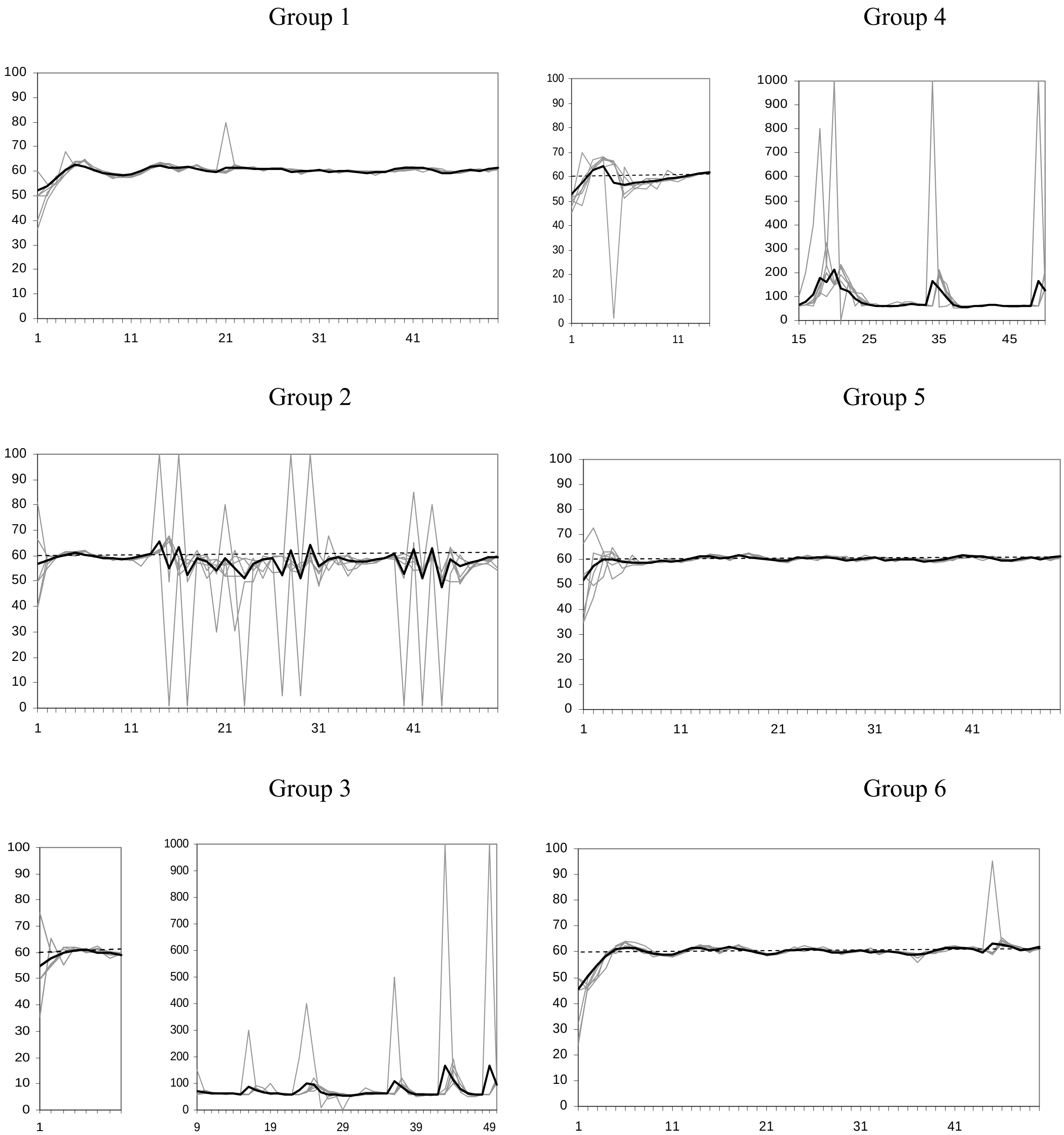

Figure 4: Prices (solid line), predictions (gray line) and equilibrium price (dotted line) in 6 groups with tournament incentives and limited information (LTN). For groups 3 and 4 the graph is split in two and rescaled: the periods before a participant submitted a very high prediction leading to a realized price higher than 100, and afterwards. For clarity the equilibrium price is not displayed in the rescaled parts of the figure.

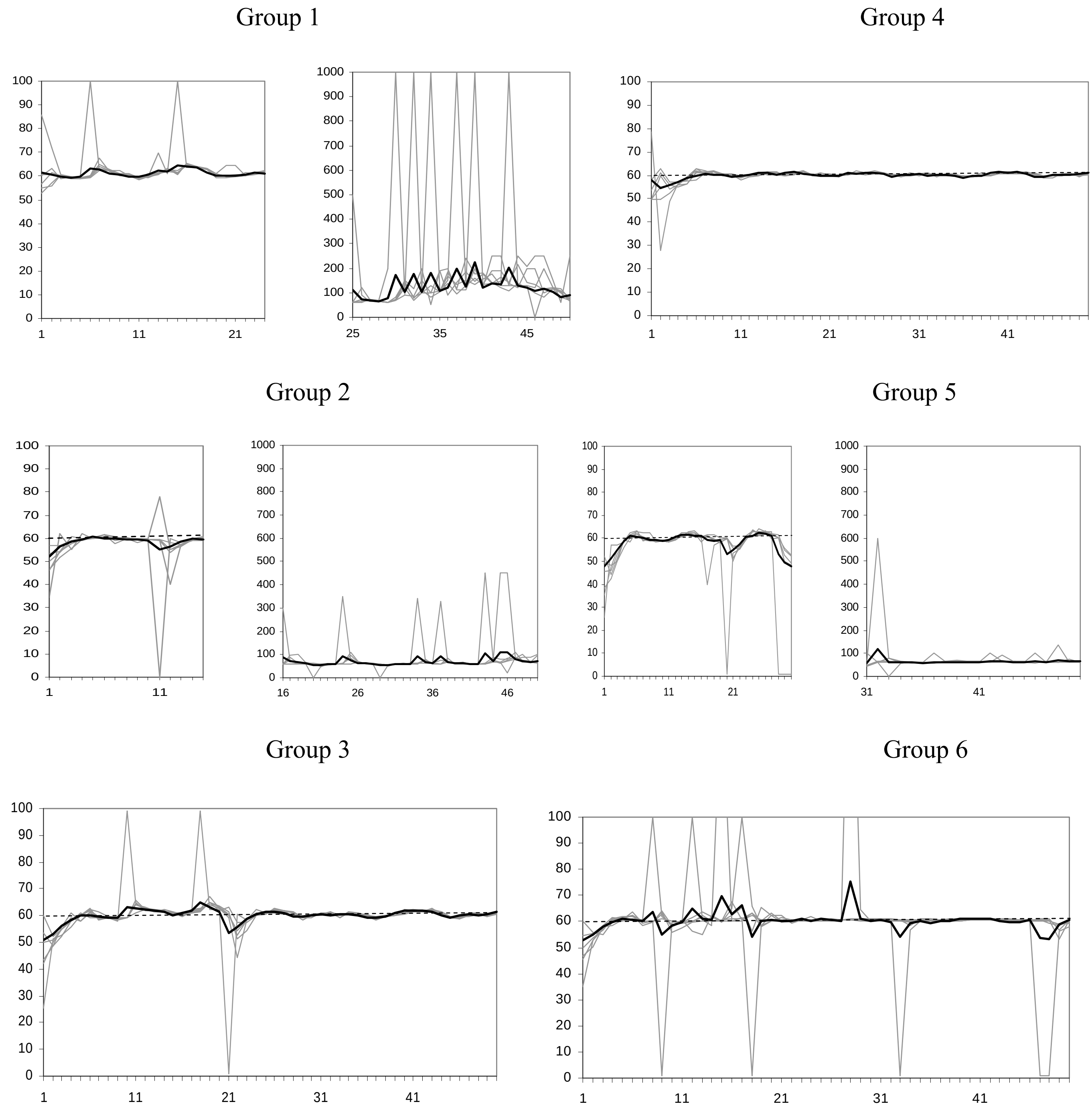

Figure 5: Prices (solid line), predictions (gray line) and equilibrium price (dotted line) in 6 groups with tournament incentives and full information (LTI). For groups 1, 2 and 5 the graph is split in two and rescaled: the periods before a participant submitted a very high prediction leading to a realized price higher than 100, and afterwards. For clarity the equilibrium price is not displayed in the rescaled parts of the figure.

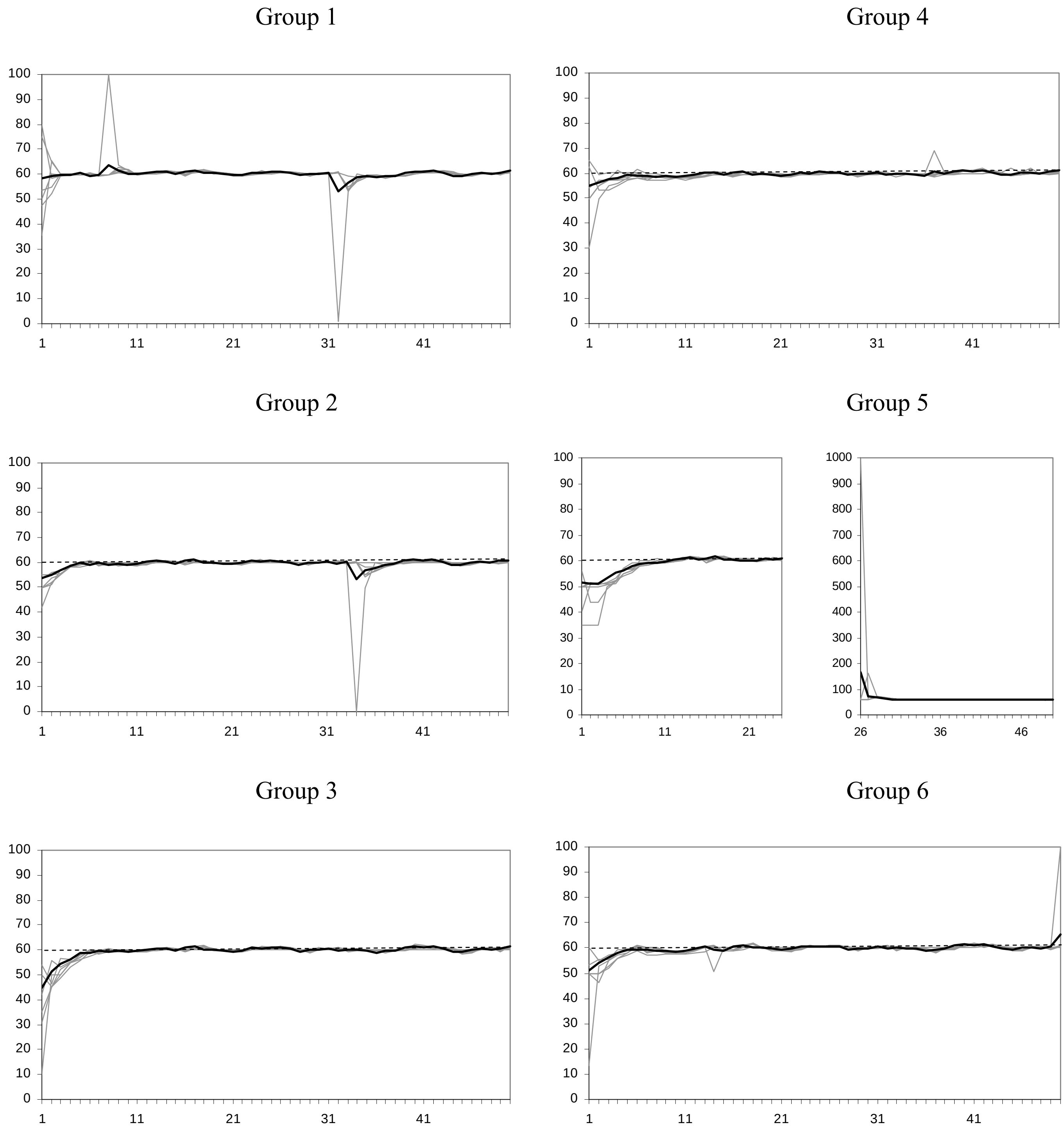

Figure 6: Prices (solid line), predictions (gray line) and equilibrium price (dotted line) in 6 groups with quadratic error incentives and low feedback strength (LQ). For group 5 the graph is split in two and rescaled: the periods before a participant submitted a very high prediction leading to a higher price than 100, and afterwards.

4.4 Comparison of the experimental results

The results described above suggest that providing more information to participants or changing to a 'winner-takes-all' incentive structure does not change the convergence and coordination properties of the positive expectations feedback experiments, but that a decrease in feedback strength is an important determinant for convergence. In this section we will try to provide some additional evidence to substantiate that claim. Moreover, in Section 4.4.4 we will analyze the increased incidence of spoilers in the 'winner-takes-all' experiments.

We are not using all available data for the analysis in this section. Obviously, a period in which one participant submits a spoiler leads to a large divergence from the equilibrium price for all predictions in that group in that period, but often there is also an effect in the following periods: the general price level increases leading to larger absolute prediction errors and a large absolute distance from the equilibrium price. Therefore, for each experimental group, we have only included those observations from periods before the first spoiler is submitted. In order to do this we need to specify which prediction outliers can be classified as a "spoiler". Our working definition of a spoiler is twofold. First the prediction of the participant has to be substantially different from the predictions of the other participants, and second, the spoiler should occur in a relatively stable market. Therefore we require the spoiler to be more than 50% larger or smaller than the median of the other five predictions in that group in that period, and we require that the relative change in realized prices in the two most recent periods is smaller than 10%. This definition captures most of the outliers that can be seen in Figures 2-6, with some exceptions.[19] Alternative procedures yield similar qualitative results. Table 7 in Appendix B gives a precise description of the data we used. Since the number of spoilers in experiments with a tournament structure is substantial the number of periods with a positive number of observations is equal to 36.

[19] Exceptions are, for example, period 27 and 31 in group 5 of experiment TN, period 30 in group 3 of experiment TI, period 21 in group 1 of experiment LTN, and period 37 in group 4 of experiment LQ. In each of these periods one participant submits a prediction that deviates substantially from the other predictions, but not enough to be classified as a spoiler.

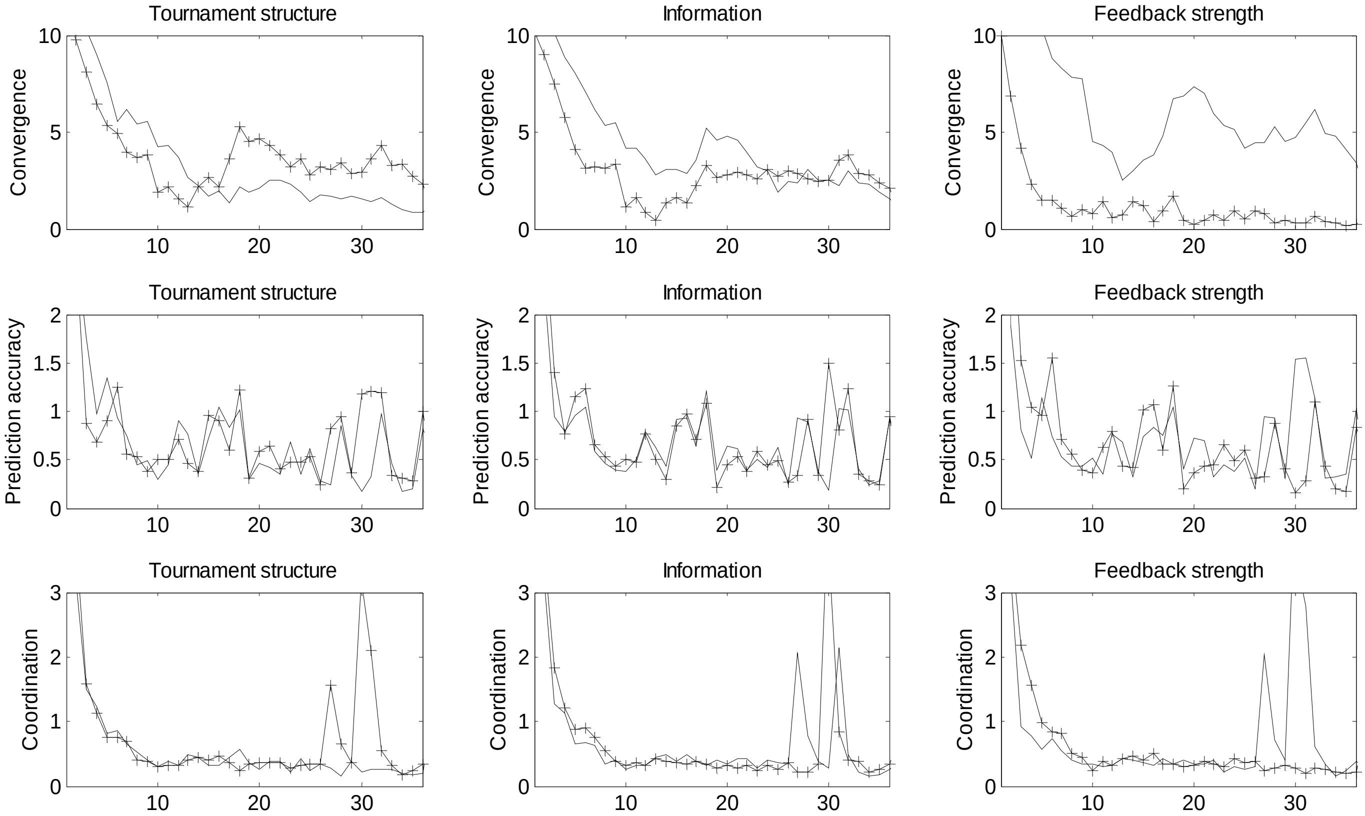

Figure 7: Measures of convergence, prediction accuracy and coordination, aggregated in three different ways (tournament (+) versus quadratic scoring (-), complete (+) versus limited information (-), high (+) versus low (-) feedback strength).

4.4.1 Convergence

Figures 1-6 suggest that convergence only occurs in the low feedback experiments LTN, LTI and LQ. In this section we measure convergence by computing, for each of the seven experiments, the median $\left| p^e_{h,t} - p^f \right|$ over all participants in the experiment of the absolute distance between the individual prediction and the equilibrium price for every period. The top row panels of Figure 7 show these medians, averaged in three different ways. The top left panel compares the average over all tournament experiments (TN, TI, LTN and LTI indicated by +) with the experiments using a quadratic scoring rule (MC, MM and LQ, indicated by -), the top middle panel compares the average over all experiments with complete information (TI, LTI and LQ, indicated by +) with those with limited information (MC, MM, TN and LTN, indicated by -) and the top right panel compares the average over experiments with low feedback strength (LTN, LTI and LQ, indicated by +) with those with high feedback strength (MC, MM, TN and TI, indicated by -). These three panels confirm our conjecture that it is the value of the feedback strength that is critical for convergence. In addition, Figure 8 top panel shows, for each experiment, the mean rank and confidence intervals of the convergence measure (using time periods 6 to 36, to allow for an initial learning phase). Clearly two subsets of experiments can be distinguished: the first consisting of the first four experiments (MC, MM, TN and TI), and the second consisting of the last three experiments (LTN, LTI and LQ), where the measure for convergence for the experiments from the first subset ranks clearly smaller than it does for the experiments from the second subset.[20]

4.4.2 Prediction accuracy

The middle row of Figure 7 shows, for each period, the median over all participants of the absolute value of the individual prediction error $\left|p^e_{h,t} - p_t\right|$, as before aggregated in three different ways (tournament vs. quadratic scoring, complete vs. limited information and high vs. low feedback strength), using the same data as above. None of the three treatment variables seem to have an impact on prediction accuracy. This is corroborated by Figure 8 middle panel, which shows the mean rank and confidence intervals of this measure of prediction accuracy for the seven different experiments. Accuracy is significantly worse in experiment MC (Friedman test, p-value of 0.0000), reflecting the difficulty of predicting two periods ahead. There is no significant difference between the other six experiments (Friedman test, p-value of 0.3970). Therefore, there is no evidence that either tournament incentives, information or feedback strength increase prediction accuracy.

[20] The Friedman test rejects the hypothesis that all experiments are the same (p-value of 0.0000). It also rejects that the high feedback strength experiments are the same (p-value of 0.0012) and that the low feedback strength experiments are the same (p-value of 0.027). However, experiments MC, TN and TI are not significantly different from each other (p-value of 0.1590). Moreover, experiment LQ is not significantly different from experiment LTN (p-value of 0.2086) and not significantly different from experiment LTI (p-value of 0.8575), although LTN and LTI are clearly significantly different (p-value of 0.0023).

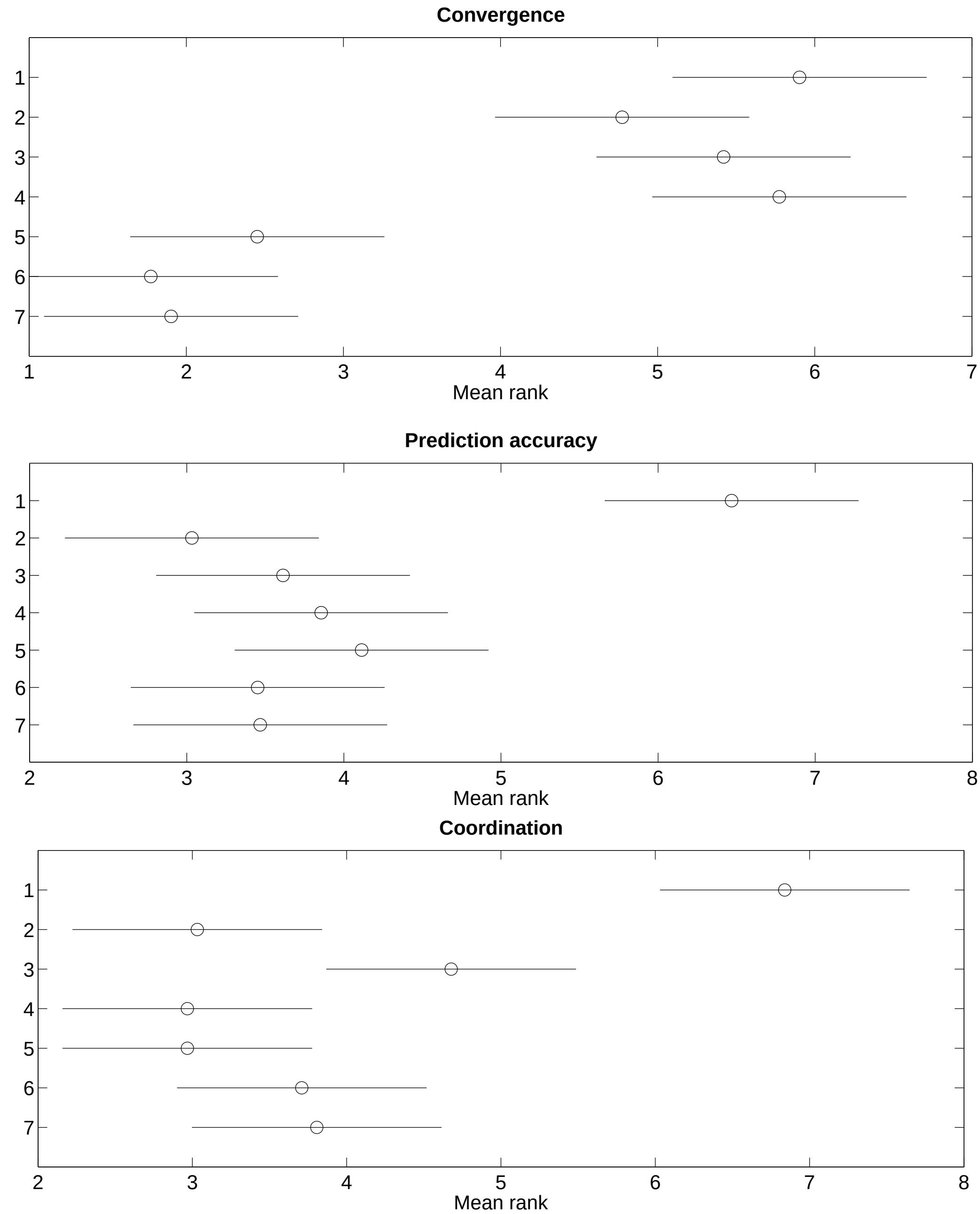

Figure 8: The mean rank and confidence intervals of the convergence measure, prediction accuracy measure and the coordination measure for the different experiments (with, from top to bottom, experiments MC, MM, TN, TI, LTN, LTI and LQ, respectively).

4.4.3 Coordination of expectations

Coordination of expectations is measured by looking at the median (over groups) of the standard deviation of predictions for each period. A low value of this standard deviation implies a high level of coordination of predictions. The bottom row of Figure 7 shows this measure, aggregated in three different ways again, reflecting the effect of incentives, information and feedback strength, respectively. None of the treatments seems to result in a structural difference in terms of coordination.[21] Figure 8 lower panel shows the mean rank and confidence intervals for the convergence measure. The only experiment that stands out again is the MC experiment.[22]

4.4.4 Spoilers

For each experiment we classified the participants and periods that correspond to spoilers, according to the definition given in the beginning of this section. Detailed information about the spoilers can be found in Appendix B. It is obvious that in the winner-takes-all experiments TN, TI, LTN and LTI more participants submit spoilers and spoilers are submitted in more periods. In particular, 40 participants in these experiments (corresponding to 28% of the participants in these experiments) were responsible for submitting a total of 92 spoilers (corresponding to a percentage of 1.3% of the total number of choices in those experiments, consequently in 7.8% of the periods at least one participant submitted a spoiler).[23] In the quadratic forecast error experiments MC, MM and LQ the percentage of participants submitting spoilers is 6.2%, submitting spoilers in 0.15% of the total number of choices in those experiments, or in 0.9% of the periods. The winner-takes-all payoff scheme therefore seems to have a substantial effect on the incidence of spoilers.[24]

By construction, the average payoff of a participant in experiments TN and TI is 25 euro (50 rounds x 3 euro / 6 participants). The average total earnings of participants that submit spoilers at least once in experiments TN, TI, LTN and LTI is 96%, 91%, 93% and 98% of 25 euro, respectively, which is not significantly different at a 5% level (the p-values from a t-test are 0.6024, 0.1857, 0.7523 and 0.8261,

[21] There are some outliers, which are due to large dispersion in predictions that are not characterized as spoilers (see footnote 19).

[22] The coordination measure for the other six experiments is also significantly different (Friedman test, p-value of 0.0018). However, the measure is not significantly different for experiments MM, TI, LTN, LTI and LQ (p-value of 0.1582).

[23] Ho et al. (1998) report that in their guessing game spoilers occur about 2.5% of the time. This is roughly in line with the numbers presented here, certainly if we take into account that their guessing game is repeated only 10 times (note that only 15 out of the 104 spoilers in our experiments occur in the first 10 periods). Ho et al. (1998, p.960) conjecture that "Spoilers [...] are probably due to frustration or to misguided attempts to single-handedly raise the mean dramatically.". We will investigate these explanations in this section.

[24] It seems that spoilers are an artifact of the incentive structure. One possibility to avoid them would be to make the realized price dependent upon the median prediction instead of the average prediction, but that is besides the point of the current paper. Obviously, one will not see spoilers in real financial markets since they would lead to huge losses. This is another reason why experiments with the tournament incentive structure may not provide the best description of actual behavior on financial markets.

respectively), implying that they do not structurally perform better or worse than the other participants.

There might be several reasons why participants submit these spoilers. Participants may, for example, be frustrated with their earnings thus far, and/or they may want to increase the probability of winning in period t by disrupting the price dynamics in period $t-1$. Each period 3 euro is won by one of the participants in a group thus on average participants earn 3/6 = 0.50 euro per period. The average earnings per period for "spoiling" participants before their first spoiler equal 43 cents in TN, 42 cents in TI, 44 cents in LTN and 38 cents in LTI, which is not significantly lower than the expected earnings per period.[25] Moreover, the median number of periods before their first "spoiler" that a spoiling participant did not earn anything is equal to 4 for the TN experiment, equal to 5 for the TI and LTN experiments and equal to 3 for the LTI experiment, (notice that on average each participants should earn something (at least) every 6 periods). Although for some individual cases it seems likely that frustration plays a role (e.g. participants 2, 3 and 4 in group 2 of experiment TN did not earn anything in the 17, 14 and 14 periods prior to submitting their first spoiler, respectively), there is no clear evidence to suggest that 'frustration' is the main driving force behind the increased number of spoilers in the experiments with tournament structure.

Finally, spoilers did not prove to be exceptionally profitable: 18 of the 92 spoilers in experiment TN, TI, LTN and LTI led to positive payoffs in the next period, which is consistent with what one would expect,[26] although some participants were indeed very successful with their spoilers (e.g. participants 5 and 6 in group 2 of experiment LTN who both won two out of three times immediately after they submitted a spoiler). For 24 participants in these experiments relative earnings after spoiling were higher than before, and for 15 they were lower.[27]

Summarizing, we did not find a unique convincing explanation for the occurrence of spoilers in our tournament experiments. Note however that the expected opportunity costs for submitting a spoiler are only 50 eurocents, and it might not be

[25] A one-sided sign test that performance before the first spoiler is equal to the expected performance is rejected for none of the four treatments (with p-values of 0.3633, 0.1334, 0.1094 and 0.2744, respectively).
[26] A one-sided sign test of the hypothesis that this is not more than to be expected is not rejected (p-value of 0.2658).
[27] The hypothesis that earnings after spoiling are not higher than before is not rejected (one-sided sign test, p-value of 0.0998).

surprising that some subjects cannot resist submitting these spoilers in some of the 50 periods of the experiment.

5. Conclusions

In our earlier papers on (positive) expectations feedback experiments we found very slow or no convergence to the equilibrium price. Number guessing games are very much related to expectations feedback experiments but typically show fast convergence to the Nash equilibrium. This striking discrepancy was the reason for designing five additional experiments where we searched for the driving force behind this difference. We found that expectation feedback games are robust to changes in the incentive structure and changes in the information provided to the participants. We consider this robustness to be good news. On the other hand, the low feedback strength experiments LTN, LTI and LQ show that presenting the number guessing game in the context of a financial market, with an interior Nash equilibrium results in very fast convergence like in the traditional number guessing game.

Since both prediction accuracy and coordination of expectations appear to be independent of the feedback strength, prediction behavior of participants in the low feedback strength experiments is not substantially different from prediction behavior of participants in the high feedback strength experiments. Instead, the convergence properties seem to be mainly due to the structure of the price generating mechanism itself. To see this, consider equations (7) and (8) again. Both price generating mechanisms give the realized price as a weighted average between the mean predicted price and the fundamental value of 60. However, the weight on the fundamental value in equation (7) is only 1/21, whereas in equation (8) it is 1/3. As an illustration, if the mean prediction equals 50, a high feedback strength experiment would give an expected realized price of 50.48, whereas the low feedback strength experiment would give a price of 53.33. Clearly, prices in the low feedback strength experiments are therefore more strongly pushed towards the fundamental price and this explains the stronger convergence in those experiments. This is confirmed by simulations with the so-called heuristic switching model that was developed in Anufriev and Hommes (2010). In their model they assume that participants switch between four typical prediction heuristics on the basis of past prediction accuracy of these heuristics. This model is quite succesful in explaining the results from the MC experiment.

Simulations of this model with the same heuristics and parameter specification but with a feedback strength of 2/3 leads to quick convergence of prices and predictions.

Let us now consider again the original beauty contest game as described by Keynes (see introduction) and compare this with the number guessing game. In the beauty contest game the task is to choose the pictures that are most often chosen by others; this is comparable with the number guessing game with $\alpha = 0$ and $\beta = 1$. In the beauty contest game there are many equilibria where all participants choose the same pictures and therefore the game in essence corresponds to a coordination problem. When $\beta = 1$ players who have higher order beliefs on different levels can still make the same decision. A number guessing game with $\beta < 1$ (and not too close to 1 in order to be able to differentiate between different levels) is a good tool to study higher order beliefs in experiments but it is not necessarily a good behavioral model of an asset market. A β that is much smaller than one corresponds to an enormous interest rate in a financial context (e.g. 50% in the experiment with low feedback strength) and a price that is mainly driven by dividends. A β close to 1 corresponds to a more realistic interest rate and investors/speculators who focus on capital gains rather than on dividends. This seems to be more in line with modern financial markets. The stylized facts about excess volatility in modern markets also point in that direction.[28] Another possible objection to an interpretation of the number guessing game as a model of financial markets is that an asset market is clearly not a tournament where the winner takes all. However, the incentive scheme appears not to be crucial for the number guessing game: in the low feedback strength experiments we find about the same results as in standard number guessing games with a tournament structure.

Concluding, we find that the β in the number guessing game is the essential design parameter: a β much smaller than 1 makes it possible to study higher order beliefs but the game is in that case not a realistic model of a modern asset market. A β closer to 1 makes a more realistic behavioral asset market model, but at the same

[28] Our results are related to those of Hirota and Sunder (2007) who present an asset market experiment with two treatments. In the long-horizon treatment participants are in the market until the asset matures and prices indeed converge to fundamental values and are mainly determined by dividends, just as in the guessing games experiment. In the short-horizon treatment, on the other hand, participants leave the experiment before the asset has matured and prices typically do not converge to their fundamental value. In the latter case dividend payments play a minor role in the determination of asset prices, just as in our expectations feedback experiments.

time makes it harder or impossible to distinguish different levels of higher order beliefs. The next question is whether Keynes was right in his proposition that higher order beliefs are an important element of asset markets. Maybe it is for some investors, but browsing internet forums suggests that many investors/speculators view the market like a living organism which movements you try to predict and not as a game in which you try to form beliefs about the beliefs of others. This interesting question can not be answered here but is a topic for future research.

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Appendix A:

Procedure and instructions

We present the procedure and a translation of the instructions for experiment MM (the instructions in experiment MC differ only in some phrasing). Boxes are included where the instructions are different in the other studies.

<u>Procedure</u>

A short welcoming message was read aloud from paper, after which the participants were randomly assigned to a cubicle in the computer lab. In each cubicle there was a computer, some experimental instructions on paper and some blank paper with a pen. The two experiments had different instructions. When all the participants were seated, they were asked to read the instructions on their desks. After a few minutes, they were given the opportunity to ask questions regarding the instructions, after which the experiment started. When the 50 time periods were completed, the participants were asked to remain seated and fill in the questionnaire, which was subsequently handed out to them. After a reasonable amount of time, the participants were called to the ante-room one by one to hand in the questionnaire and receive their earnings, in cash. The participants left the computer lab after receiving their earnings.

The experimental instructions the participants read in their cubicles consisted of three parts, totalling five pages. The first part contained general information about the market the experiment was about to simulate. The second part contained an explanation of the computer program used during the experiment. The third part displayed a table relating the absolute prediction error made in any single period to the amount of credits earned in that period. The conversion rate between credits and euros, being 2600 credits to 1 euro. (In **experiments TN** and **TI** a tournament was implemented and the tabel was omitted).

<u>Experimental instructions</u>

The shape of the artificial market used by the experiment, and the role you will have in it, will be explained in the text below. Read these instructions carefully. They continue on the backside of this sheet of paper.

General information

You are an advisor of a trader who is active on a market for a certain product. In each time period the trader needs to decide how many units of the product he will buy, intending to sell them again the next period. To take an optimal decision, the trader requires a good prediction of the market price in the next time period. As the advisor of the trader you will predict the price P(t) of the product during 50 successive time periods. Your earnings during the experiment will depend on the accuracy of your predictions. The smaller your prediction errors, the greater your earnings.

About the market

The price of the product will be determined by the law of supply and demand. Supply and demand on the market are determined by the traders of the product. Higher price predictions make a trader demand a higher quantity. A high price prediction makes the trader willing to buy the product, a low price prediction makes him willing to sell it. There are several large traders active on this market and each of them is advised by a participant of this experiment. Total supply is largely determined by the sum of the individual supplies and demands of these traders. Besides the large traders, a number of small traders is active on the market, creating small fluctuations in total supply and demand.

About the price
The price is determined as follows. If total demand is larger than total supply, the price will rise. Conversely, of total supply is larger than total demand, the price will fall.

About the price <u>experiments TI and LOW</u>
The price in each period depends upon your prediction and the prediction of the other 5 participants. Let $GV(t)$ be the average prediction in period *t*, than:

$price(t) = 2.85 + 0.95 \times GV(t)$ **(experiment TI)** (1)

$price(t) = 20 + 2/3 \times GV(t)$ **(experiment LOW)** (1)

This is the price when only the large traders (who are advised by the six particpants) would be influencing the price. The small traders on the market cause a small change of the price, sometimes negative, sometimes positive and on average zero. We will indicate this amount in period *t* by $k(t)$ and it will be almost always between -1 and 1. The value of $k(t)$ is not related to this value in other periods. The realized price in period *t* will be:

$P(t) = price(t) + k(t)$. (2)

We will give and example. The predictions of the 6 participants in period 1 are 14, 80, 76, 30, 57 and 23. The average prediction is:

$$GV(1) = \frac{14 + 80 + 76 + 30 + 57 + 23}{6} = 46.67$$

and this gives the price:

$price(1) = 2.85 + 0.95 \times 46.67 = 47.30$ **(experiment TI)**

$price(1) = 20 + 2/3 \times 46.67 = 51.11$ **(experiment LOW)**

The influence of the small traders in this first period $k(1)$ equals 0.13 and the realized price will be:

$P(1) = 47.30 + 0.13 = 47.43$ **(experiment TI)**

$P(1) = 51.11 + 0.13 = 51.24$ **(experiment LOW)**

This example and the formulas (1) and (2) show that the realized price will be near the average predicted price; if the average predicted price (GV) is low, than the realized (P) will be low and if GV is high, P will be high.

About predicting the price
The only task of the advisors in this experiment is to predict the market price P(t) in each time period as accurately as possible. The price (and your prediction) can never become negative and lies always between 0 and 100 euros in the first period. The price and the prediction in period 2 through 50 is only required to be positive. The price will be predicted one period ahead. At the beginning of the experiment you are asked to give a prediction for period 1, V(1). When all participants have submitted their predictions for the first period, the market price P(1) for this period will be made public. Based on the prediction error in period 1, P(1) - V(1), your earnings in the first period will be calculated. Subsequently, you are asked to enter your prediction for period 2, V(2). When all participants have submitted their prediction for the second period, the market price for that period, P(2), will be made public and your earnings will be calculated, and so on, for 50 consecutive periods. The information you have to form a prediction at period t consists of: All market prices up to time period t-1: {P(t-1), P(t-2), ..., P(1)}; All your predictions up until time period t-1: {V(t-1), V(t-2), ..., V(1)}; Your total earnings at time period t-1.

About the earnings
Your earnings depend only on the accuracy of your predictions. The better you predict the price in each period, the higher will be your total earnings. On your desk is a table listing your earnings for all possible prediction errors.

For example, your prediction was 13.42. The true market price turned out to be 12.13. This means that the prediction error is: 13.42 – 12.13 ≈ 1.30. The table then says your earnings are 1255 credits (as listed in the second column).

About the earnings <u>experiments TN and TI</u>

All participants start with 5 euros and whether they will earn more will depend on the quality of their predictions. In every period the participant in your group with the smallest prediction error wins 3 euro and the others earn nothing. If more than one participant have the smallest error, the prize is split. For example, if the realized price is 34.1 and two participants predicted 31.9 and one 36.3 (and the other predictions are less accurate), all three have made a prediction error of 2.2 and the earn 3/3=1 euro each, and the other participants in the group earn nothing.

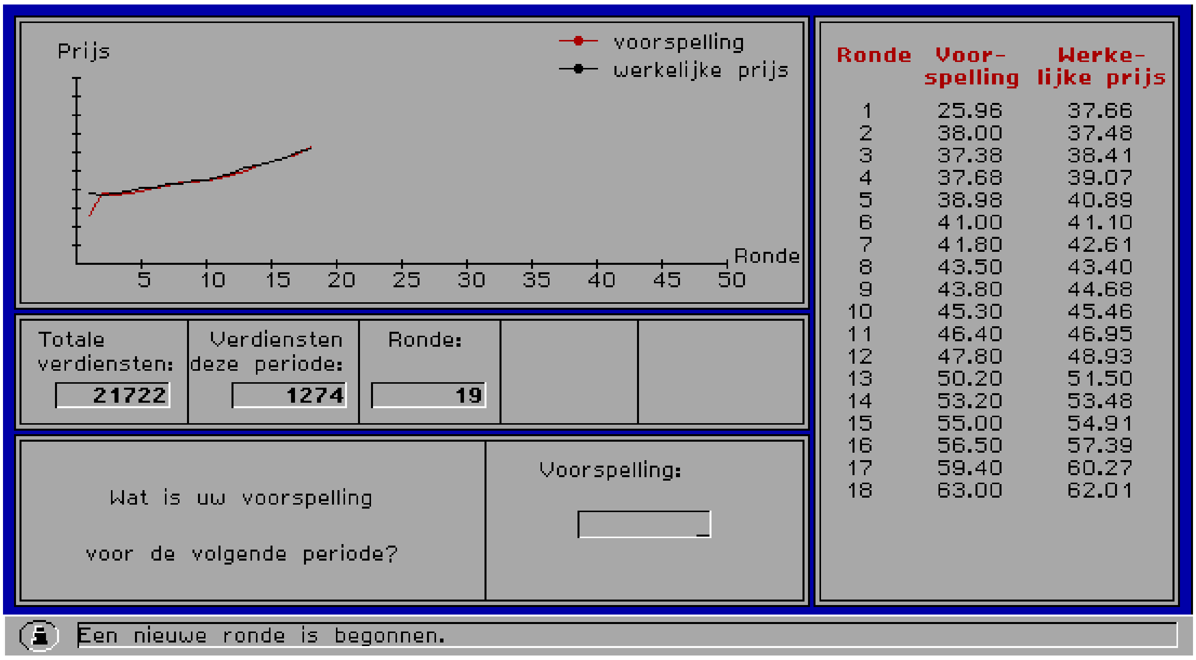
When you are done reading the experimental instructions, you may continue reading the computer instructions, which have been placed on your desk as well.

Computer instructions
The way the computer program works that will be used in the experiment, is explained in the text below. Read these instructions carefully. They continue on the backside of this sheet of paper.

The mouse does not work in this program. To enter your prediction you can use the numbers, the decimal point and, if necessary, the backspace key on the keyboard.

Your prediction can have two decimal numbers, for example 30.75. Pay attention not to enter a comma instead of a point. Never use the comma. Press enter if you have made your choice.

The available information for predicting the price of the product in period t consists of: All product prices from the past up to period t-1; Your predictions up to period t-1; Your earnings until then.



The main experimental computer screen. The Dutch labels translate as follows: “prijs” = price; “voorspelling” = prediction; “werkelijke prijs” = market price; “ronde” = round; “totale verdiensten” = total earnings; “verdiensten deze periode” = earnings this period; “Wat is uw voorspelling voor de volgende periode?” = What is your prediction for the next period?; “Een nieuwe ronde is begonnen” = A new round has started.

The computer screen. The instructions below refer to this figure.

In the upper left corner a graph will be displayed consisting of your predictions and of the true prices in each period. This graph will be updated at the end of each period.

In the rectangle in the middle left you will see information about the number of credits you have earned in the last period and the number you have earned in total. The time period is also displayed here, possibly along with other relevant information.

On the right hand side of the screen the experimental results will be displayed, that is, your predictions and the true prices for at most the last 20 periods.

At the moment of submitting your price prediction, the rectangle in the lower left side of the figure will appear. When all participants have subsequently submitted their predictions, the results for the next period will be calculated.

When everyone is ready reading the instructions, we will begin the experiment. If you have questions now or during the experiment, raise your hand. Someone will come to you for assistance.

Appendix B:

The definition of spoilers we use is the following. A prediction of a participant in a certain period is a spoiler if: i) the prediction is at least 50% higher or lower than the median prediction of the other five participants in that group for that period ; and ii) the last two changes in the realized price are smaller than 10%.

Experiment							
MC	**Periods**	1—15	16—41	42—50			
	Groups	1-14	1-12,14	1,2,4-12,14			
MM	**Periods**	1—7	8—24	25—50			
	Groups	1-7	1-4,6,7	1-4,7			
TN	**Periods**	1—6	7—21	22—37	38—50		
	Groups	1-6	1,2,5,6	1,5	5		
TI	**Periods**	1—2	3—5	6—8	9	10—36	
	Groups	1-6	2-6	2-5	3,4	3	
LTN	**Periods**	1—4	5—8	9—13	14—44	45—50	
	Groups	1-6	1-3,5,6	1,2,5,6	1,5,6	1,5	
LTI	**Periods**	1—5	6—7	8—9	10	11—16	17—50
	Groups	1-6	2-6	2-5	2,4,5	4,5	4
LQ	**Periods**	1—7	8—25	26—33	34—49	50	
	Groups	1-6	2-6	2-4,6	3,4,6	3,4	

Table B1: Data used in the construction of Figures 7—8. Note that each group consists of six participants

MC	*# (periods)*
3-1	1 (42)
13-3	1 (16)
2/84	**2/4200**
MM	**# (first time)**
5-2	1 (8)
5-4	2 (28,45)
6-4	1 (15)
6-6	1 (25)
4/42	**5/2100**
LQ	**# (first time)**
1-1	2 (8,32)
2-6	1 (34)
5-4	1 (26)
6-2	1 (50)
4/36	**5/1800**

Table B2: "Spoilers" in experiments MC, MM and LQ. The first column gives the identity of the participant (*i-j* refers to participant *j* in group *i*). The second column gives the number of spoilers and (between brackets) the periods in which the spoilers occurred. Numbers in boldface rows indicate the number of participants/periods out of all participants/periods submitting spoilers/ in which a spoiler is submitted.

TN	*# (periods)*	*Payoff*	*Payoff before (# no pay)*	*Score*
1-6	2 (38, 45)	1.14	21 – 1.14 (2)	1 /2
2-2	3 (22, 34, 37)	0.78	3 – 0.29 (17)	1/3
2-3	1 (47)	0.78	16.5 – 0.72 (14)	1/1
2-4	1 (26)	1.02	12 – 0.96 (14)	0/1
2-6	3 (22, 27, 41)	1.38	10.5 – 1.00 (4)	2/3
3-3	1 (11)	1.20	9 – 1.80 (2)	0/1
3-6	6 (7, 16, 19, 29, 32, 35)	0.72	3 – 1.00 (1)	0/6
4-3	1 (45)	0.82	17.5 – 0.80 (4)	0/1
4-4	4 (7, 17, 24, 40)	1.00	7 – 2.33 (2)	0/4
6-5	1 (22)	0.78	3 – 0.29 (4)	0/1
10/36	**23/1800**	**0.96**	**0.86 (median: 4)**	**5/23**

Table B3: Analysis of "spoilers" in experiment TN. The first two columns as above. The third column gives payoffs during the whole experiment relative to expected payoffs of 25 euro, the fourth column gives the payoffs before the first spoiler absolutely, as well as relative to expected payoffs, and (between brackets) the number of periods before the first spoiler in which nothing was earned. The last column gives the number of times earnings were strictly positive in the period following a spoiler.

TI	*# (periods)*	*Payoff*	*Payoff before (# no pay)*	*Score*
1-3	1 (3)	0.90	0 – 0.00 (2)	0/1
1-4	2 (14, 32)	0.90	3 – 0.46 (9)	1/2
2-2	1 (25)	1.26	15 – 1.25 (9)	0/1
2-5	1 (9)	1.08	3 – 0.75 (3)	0/1
3-3	1 (37)	0.80	12.5 – 0.69 (4)	0/1
4-1	2 (21, 31)	0.84	15 – 1.5 (0)	0/2
4-2	2 (10, 16)	1.20	12 – 2.67 (0)	0/2
4-3	1 (47)	0.66	13.5 – 0.29 (16)	1/1
4-6	2 (31, 40)	0.96	6 – 0.40 (16)	0/2
5-2	2 (9, 37)	1.24	6 – 1.50 (3)	0/2
5-6	1 (18)	0.66	3 – 0.35 (6)	0/1
6-4	2 (6, 27)	0.72	0 – 0.00 (5)	0/2
6-5	3 (20, 36, 46)	0.66	9 – 0.95 (12)	0/3
13/36	**21/1800**	**0.91**	**0.83 (median: 5)**	**2/21**

Table B4: Analysis of "spoilers" in experiment TI. For explanation see Table B3.

LTN	*# (periods)*	*Payoff*	*Payoff before (# no pay)*	*Score*
2-5	3 (14, 22, 27)	1.32	6 – 0.92 (6)	2/3
2-6	3 (15, 23, 40)	1.44	4.5 – 0.64 (0)	2/3
3-2	1 (23)	0.40	4 – 0.36 (12)	0/1
3-4	5 (9, 16, 36, 43, 49)	0.76	1 – 0.25 (7)	0/5
4-2	5 (5, 15, 16, 34, 49)	0.36	0 – 0.00 (4)	1/5
6-6	1 (45)	1.32	30 – 1.36 (1)	0/1
6/36	**18/1800**	**0.93**	**0.87 (median: 5)**	**5/18**

Table B5: Analysis of "spoilers" in experiment LTN. For explanation see Table B3.

LTI	*# (periods)*	*Payoff*	*Payoff before (# no pay)*	*Score*
1-4	2 (6, 29)	0.72	0 – 0.00 (5)	0/2
1-5	2 (15, 25)	0.84	6 – 0.86 (5)	0/2
2-4	2 (20, 50)	1.18	11.5 – 1.21 (1)	0/2
2-5	6 (11, 16, 24, 29, 34, 43)	1.60	10.5 – 2.10 (3)	3/6
3-5	1 (10)	0.96	3 – 0.67 (2)	0/1
3-6	2 (18, 21)	1.20	6 – 0.71 (2)	1/2
5-1	1 (17)	0.90	0 – 0.00 (16)	0/1
5-2	1 (20)	0.36	6 – 0.63 (10)	0/1
5-4	5 (28, 31, 37, 42, 46)	0.60	3 – 0.22 (11)	1/5
6-1	1 (8)	1.30	6 – 1.71 (1)	0/1
6-5	7 (9, 12, 15, 18, 28, 33, 47)	1.08	6 – 1.5 (0)	1/7
11/36	**30/1800**	**0.98**	**0.77 (median: 3)**	**6/30**

Table B6: Analysis of “spoilers” in experiment LTI. For explanation see Table B3.