TI 2007-015/1
Tinbergen Institute Discussion Paper

The Impact of Naïve Advice and Observational Learning in Beauty-contest Games

Martin Kocher[1]
Matthias Sutter[2]
Florian Wakolbinger[2]

[1] University of Amsterdam, and Tinbergen Institute;
[2] University of Innsbruck.

Tinbergen Institute
The Tinbergen Institute is the institute for economic research of the Erasmus Universiteit Rotterdam, Universiteit van Amsterdam, and Vrije Universiteit Amsterdam.

Tinbergen Institute Amsterdam
Roetersstraat 31
1018 WB Amsterdam
The Netherlands
Tel.: +31(0)20 551 3500
Fax: +31(0)20 551 3555

Tinbergen Institute Rotterdam
Burg. Oudlaan 50
3062 PA Rotterdam
The Netherlands
Tel.: +31(0)10 408 8900
Fax: +31(0)10 408 9031

The impact of naïve advice and observational learning in beauty-contest games*

Martin Kocher[#,+], *Matthias Sutter*[+,§] *and Florian Wakolbinger*[+]

[#] University of Amsterdam

[+] University of Innsbruck

This version: January 24, 2007

Abstract:

We study the impact of advice or observation on the depth of reasoning in an experimental beauty-contest game. Both sources of information trigger faster convergence to the equilibrium. Yet, we find that subjects who receive naïve advice outperform uninformed subjects permanently, whereas subjects who observe others' past behavior before making their decision do only have a temporary advantage over uninformed subjects. We show in a simulation that the latter result is due to subjects failing to make the most out of observing others.

JEL classification: C70, C72, C91

Keywords: social learning, advice, observational learning, beauty-contest game

* We would like to thank Rudolf Kerschbamer, Janette Walde, participants of the ESA World Meeting 2006 in Atlanta, the Annual meeting of the Verein für Socialpolitik 2006 as well as seminar participants at Cologne University for very helpful comments. Financial support from the *Center of Experimental Economics* at the University of Innsbruck (sponsored by *Raiffeisen-Landesbank Tirol*) and from the *Tiroler Wissenschaftsfonds* is gratefully acknowledged. Kocher acknowledges financial support from the ENABLE Project under the European Union 6th Framework Program. The paper reflects the views of the authors, and the Community is not liable for any use that may be made of the information contained therein.

§ Corresponding author: Department of Public Economics, University of Innsbruck. Universitaetsstrasse 15/4, 6020 Innsbruck, Austria. E-mail: matthias.sutter@uibk.ac.at

1. Introduction

Before making a decision, many subjects seek advice from others or simply observe what others have been doing in the past. For example, many consumers read customer reviews before buying a new book in an Internet bookstore, or it is quite common to take a curious look into some friends' garages prior to buying a new car. Likewise, before deciding in which assets to invest, people tend to investigate the past performance of the asset or ask their friends about their experience.

A recent strand of the economics literature (initiated by Andy Schotter and several co-authors) has shown that both sources of information – receiving advice and observing others – can have a significant impact on decisions. One important finding of this literature (see, e.g. Chaudhuri et al., 2001, 2006; Schotter and Sopher, 2003, 2007; Celen et al., 2006) is that both advice and observation need not origin from experts to be effective. Rather, it is sufficient for the advice and the observed data to stem from subjects who have made one single experience with the decision to be taken. This is why the term *naïve advice* (Schotter, 2003) has been termed to describe the advice from such non-experts. Learning from previous decisions taken by unrelated others in the past is typically referred to as *observational learning*. The term *social learning* captures both sources of information, naïve advice and observational learning.

In this paper we study the impact of naïve advice and observational learning on the depths of reasoning and performance in an experimental beauty-contest game (Nagel, 1995). This game has been likened to professional investment activity since Keynes (1936) has compared investment decisions on financial markets to the beauty-contests run in newspapers at his time (where readers had to decide on which out of several faces they considered to be the most popular among the general readership of the newspaper). Obviously, both naïve advice and observational learning play an important role for investment decisions, in particular for private investors who seek out advice on and observe past performance of financial assets.

Not only that the beauty-contest game has a straightforward application in financial markets, it is also ideal for studying an individual's depths of reasoning and the performance of subjects who receive advice or observe others compared to those subjects who do not. More generally, the beauty-contest game is an excellent tool for analyzing reasoning processes, because (i) it is relatively simple, but still captures all important aspects of an interactive game where it is crucial to anticipate what others do; (ii) learning and reasoning can be observed and studied easily in this game; (iii) performance can be measured directly since in its standard form it is a winner-takes all game; and (iv) social preferences, risk aversion or loss aversion are practically irrelevant, contrary to most other games for which the influence of naïve advice and observational learning have been studied so far.[1]

Although there is an extensive literature on the beauty-contest game (see, e.g., Nagel, 1995; Duffy and Nagel, 1997; Ho et al., 1998; Bosch-Domenech et al., 2002; Güth et al., 2002; Camerer et al., 2003; Weber, 2003; Kocher and Sutter, 2005; Sutter, 2005; Kocher et al., 2006; Costa-Gomes and Crawford, 2007; Grosskopf and Nagel, 2007), there are only a few recent papers that are more closely related to the research questions in this paper. Sbriglia (2004) presents a beauty-contest experiment where the winner of each period drops out from the game, but has to give an explanation for his choice that is subsequently shown to the remaining participants at the beginning of the next period. Such a procedure accelerates the learning process and the convergence to the equilibrium considerably, compared to a control setting without an explanation of the winner. Hence, advice (in the form of an explanation for one's choice) seems to increase the depths of reasoning. [2]

Slonim (2005) examines the influence of experience in a beauty-contest game. He lets experienced subjects (who have already played the game before) compete against inexperienced ones. He finds that experienced subjects win the game significantly more often in each single period. However, the relative advantage of experienced subjects diminishes over time. Of course, having experienced the game oneself before is a similar form of information as observational learning, yet it is not what one would claim to be equivalent to observational learning.

In our paper we are interested in the effects of observing *others'* past behavior (observational learning), which clearly distinguishes our paper from Slonim (2005). In contrast to Sbriglia (2004) we let subjects who receive advice compete against subjects who do not receive advice. Hence, we are able to check whether receiving advice increases one's relative performance, which is not possible in the design of Sbriglia (2004) where all

[1] The literature on naïve advice and observational learning has, so far, focused on bargaining and coordination games (Chaudhuri et al., 2001, 2006; Schotter and Sopher 2003, 2007). Naïve advice facilitates coordination and yields higher cooperation levels in a battle-of-the-sexes game (Schotter and Sopher; 2003), minimum games (Chaudhuri et al., 2001) and in public goods games (Chaudhuri et al., 2006). Ultimatum bargaining gets tougher with advice and observation, as it yields lower offers and more rejections (Schotter and Sopher, 2007). Celen et al. (2006) study the impact of advice and observation on the processing of information in an information cascade experiment and find that subjects tend to follow the advice of others, but mostly ignore the past behavior of others (even though both types of information are equally informative in their setup).

[2] An example for a paper that is studying the effects of *expert* advice in a related game, the traveler's dilemma, is Capra et al. (2003).

members of a group receive the advice from the previous period's winner. Finally, in our experimental design we can comparatively assess the value of naïve advice and of observational learning.

Our results indicate that both naïve advice and the observation of past behavior accelerate convergence to the equilibrium significantly. Thus, both types of information have a qualitatively similar effect on convergence. Regarding profits we find that subjects who observe others do only have a temporary advantage over uninformed subjects, whereas subjects receiving advice outperform uninformed subjects consistently. The latter finding indicates a clear advantage of receiving advice over observing others' past behavior.

The paper proceeds as follows. Section 2 introduces the beauty-contest game, and Section 3 describes our experimental design. Section 4 is devoted to derive predictions. Results are presented in Section 5, and Section 6 concludes the paper by discussing our findings.

2. The beauty-contest game

In a beauty-contest game n decision-makers $i = 1,\ldots,n$ simultaneously choose a real number $x_i \in X = [0,100]$. The winner is the participant whose number is closest to a target value $\bar{x}$, which is defined as p times the average chosen number, with $0 < p < 1$.

$$\bar{x} = p\frac{\sum_{i=1}^{n} x_i}{n} \qquad (1)$$

The winner receives a given prize $r > 0$, while all other n-1 players get nothing. This game is dominance-solvable by assuming common knowledge of rationality. The process of eliminating weakly dominated strategies starts with the observation that any number higher than $100 \cdot p$ is weakly dominated, because $100 \cdot p$ is the highest possible winning number. Given this first step of elimination, it is straightforward to see that the process continues by eliminating all numbers above $100 \cdot p^2$, then all numbers above $100 \cdot p^3$, and so on. After an infinite series of steps of reasoning the number zero remains as the only undominated choice.

All previous experimental studies show that the unraveling process stops, on average, after only a few steps of reasoning. Obviously, this can be due to an insufficient depth of reasoning or due to specific beliefs over the other players' depths of reasoning, or even specific beliefs over the beliefs over the distribution of other players' depths of reasoning, and so on. The interactive component of decision making and the difficulty of belief management are the basis for the intuitive expectation that naïve advice and observational learning might play an important role for the behavior in beauty-contest games as both sources reveal information about other subjects.

3. Experimental design

The beauty-contest game was played in groups of three persons and repeated for four periods in all experimental treatments. We set the parameter $p = 2/3$. The prize of winning the game was € 7 in each period. In case of a tie the prize was shared equally among those who tied. All participants received a show-up fee of € 3. After each period subjects were informed about the three numbers chosen in their group, the average number and the target value $\bar{x}$. Three treatments of this basic game were implemented.

1. *Control*-treatment. In this treatment subjects played the beauty-contest game as described above. At the end of the experiment, participants in *Control* were asked to fill in an advice sheet that should contain the following three items: (i) a suggested number for period 1, (ii) a brief statement why this number should be chosen, and (iii) a descriptive strategy how to choose one's numbers in periods 2 to 4. These advice sheets were used in subsequent sessions with advice. Note that participants in *Control* were not informed about our request to fill in an advice sheet before the end of period 4 in order to avoid any confounding influence on their decisions. To incentivize advice-giving, participants in *Control* were told that four randomly selected advice sheets would be distributed to participants in later sessions such that only one out of three group members in these later sessions would receive advice. If a particular subject's advice sheet were to be distributed in a later session, this subject would earn three times the amount of one randomly drawn participant who had access to the particular advice sheet. Profits for advice for the randomly selected four participants in *Control* were paid after the advice sessions had been carried out.[3] Since advisors benefit from giving valuable advice with positive probability, the procedure is incentive compatible. Furthermore, we did everything to ensure credibility of the procedure, and given the elaborate advice statements, we are sure that the incentive mechanism worked well. Note finally that

[3] Subjects were asked to provide their e-mail addresses, and the four randomly selected advice-givers received a notification e-mail with information about the additional amount they had earned through their advice. The money was transferred on their bank account which they had to provide at that stage only. Thus, transaction costs in connection with their claim were very small for the winners compared to the quite substantial additional earnings, which ranged from € 42 to € 84.

advice-giving was anonymous, which means that the advisee did not get any information about his or her specific advisor and vice versa.[4]

2. *OneHist*-treatment. Conditions in *OneHist* were identical to *Control* except that one randomly determined group member received the information shown in Table 1. Henceforth, we will refer to the information in Table 1 as *history*. The table contains the average chosen number and the average target value in each period of *Control*.[5] Participants in *OneHist* knew that only one member per group was equipped with information about the average choices of the 33 participants in the *Control*-treatment. When giving feedback on the chosen numbers within a group after each period, we did not reveal which of the submitted numbers had come from the member with *history* information.

Table 1
History of Control

	Period 1	Period 2	Period 3	Period 4
Overall averages (N = 33)	32.9	22.9	20.5	12.3
Average target value	21.9	15.3	13.6	8.2

3. *OneAdv*-treatment. This treatment was identical to *Control* except that one randomly selected group member received four randomly selected advice sheets out of the 33 sheets that had been collected from participants in *Control*.[6] The contents of the four selected advice sheets are displayed in Table 2 and will be referred to as *advice* in the following. To make the procedure as credible as possible we distributed the Xerox-copied hand-written sheets instead of providing the advice statements in any other form (e.g. electronically on the screen). We used the same four sheets of advice in each group in order to keep *OneAdv* and *OneHist* as comparable as possible for a comparative assessment of the impact of naïve advice and the observation of *history*. For the same reason we decided to use four sheets of advice – instead of any other number – because subjects in *OneHist* received information on four periods; this is as close as we believe one can get. Like in *OneHist* subjects in *OneAdv* were informed that

[4] Details can be found in the experimental instructions that are provided in Appendix A.

[5] Actually, the median would be more informative than the average if there were a substantial number of outliers. Since that was not the case we decided to provide subjects with the mean (which is typically also more quickly understood than the median).

[6] In Appendix B we provide all 33 sheets of advice that we collected in *Control*. We let a student in one of our classes draw the four advice sheets out of the 33.

only one group member would receive the advice sheets, and this member was not unveiled when all group members received feedback on the chosen numbers.

Table 2
Sheets of advice distributed in OneAdv

	Number suggested for period 1	Reason	Strategy
Advice sheet 1	27	Since 2/3 of the average of all numbers is the target, the target number is not too high (between 10 and 20). However, some of the participants do not know this, and the target in Period 1 is above the targets of later periods.	Decrease the number from period to period. The participants realize that they should decrease the numbers (actually, the target should converge to zero). However, you cannot count on the others: set a number between 13 and 21.
Advice sheet 2	13.5	Most of the time the game starts with a low number.	Slowly increase the number from period to period. Increase the number by not more than 10 in one step.
Advice sheet 3	0	To see how the other participants behave. If the others think that the people in the group are "rational", they should also choose zero.	You can see whether the other participants know what the game is about or whether they just guess. If they guess, you should set around the target number of the previous period. If they do not guess, set zero again.
Advice sheet 4	30	Since there are three participants, the average number out of 0 – 100 is 33. Hence, people like to choose this number. However, the average will be multiplied by 2/3, which will reduce the target value. This means that a number below 33 might be closest to the target value.	There is tendency to decrease numbers, which results from the multiplication of the average by 2/3. Hence, lower numbers should come closer to the final value.

All experimental sessions were run at the University of Innsbruck (using zTree, Fischbacher, 2007). A total of 96 students in their first and second year participated. None of them had ever attended a game theory class or participated in a beauty-contest experiment before. Sessions lasted approximately 30 minutes, and subjects earned € 12.33 on average (including the show-up fee of € 3).

4. Predictions

The possible access to *advice* or *history* obviously does not change the equilibrium prediction of the game when assuming common knowledge of rationality. However, the behavior observed in beauty-contests does not support standard predictions. In order to derive alternative predictions, it, thus, makes sense to rely on empirical models that assume (at least partly) bounded rationality. Relaxing the assumptions of full rationality and common knowledge of it may then yield *advice* or *history* a valuable source for a decision maker because they convey information about the average depths of reasoning and beliefs in the population.

One prominent model to explain behavior in the beauty-contest game has been proposed by Camerer et al. (2004) and is known as the *cognitive hierarchy model* (CHM). In general, this model is based on the assumption of step-level reasoning, meaning that subjects may differ in the number of steps of iterated thinking (i.e. iterated elimination of weakly dominated strategies). A step-zero player is assumed to randomize from the available strategy space. A step-one player plays best response to step-zero players, but disregards the existence of players with higher steps of reasoning. More generally, a step-d player responds optimally to lower-step players. Hence, the cognitive hierarchy model retains the best-response characteristic of standard game-theoretic equilibria (with the exception of zero-step players), but weakens equilibrium properties such as belief and choice consistency.

More specifically, the CHM assumes that the frequency of players applying a different level of steps of reasoning is distributed according to a Poisson distribution with mean and variance τ. Thus, d steps of reasoning are assumed to occur with probability $f(d) = e^{-\tau}\tau^{d} / d!$. Using several data sets from experimental beauty-contest games, Camerer et al. (2004) estimate the parameter τ to be typically in the range [1, 2].[7]

For the sake of succinctness we are not going into the details of the CHM here. Rather, we will assume for our analysis in the results section that subjects behave according to the model.[8] In the following, we would like to concentrate on deriving predictions for the behavior of subjects who receive either *advice* or *history*. Since the CHM is a static model (that does not imply predictions for behavior in later periods), we will only consider behavior in the first period.[9]

---

[7] Gneezy (2005) shows that bidding behavior in auctions can also be captured by the CHM and that the estimated τ for auctions is close to the one for beauty-contest games (see also Kovac et al., 2004, on the latter).

[8] Two assumptions are noteworthy. First, step-d players are assumed (a) to neglect that there are other players who are actually doing more than d steps of reasoning and (b) to fail realizing that there are other players with step-d reasoning. Second, step-d players are assumed to predict the relative proportion of players with less than d steps of reasoning accurately. The interested reader is referred to Camerer et al. (2004) for details.

[9] Deriving predictions for periods 2 to 4 on the basis of the cognitive hierarchy model is beyond the scope of this paper. Such an endeavour would require several non-trivial modelling decisions on the learning dynamics of different types of players, for instance (i) which fraction of step-zero players becomes step-d players (with d > 0), (ii) how the depth of reasoning evolves for step-d players at different initial steps of reasoning, and (iii) whether and to what extent beliefs about others' choices are affected from the data in previous periods.

Let us start with treatment *OneHist*. Our analysis rests upon the assumption that the population distributions of choices in *Control* and *OneHist* are the same. Since subjects were invited from a large subject pool (of over 3000 students) and since assignment to treatments was random, this assumption is rather innocuous.

Let $\tilde{x}$ be the number chosen by the subject who knows the *history* (from the *Control*-treatment) and let x_1 and x_2 be the numbers of the two other group members in *OneHist*. Recall that *p* denotes the factor that multiplies the group average and *n* the number of group members. The group member with *history* will win if his number is closest (or equally close) to the target value, i.e. if the following two conditions are satisfied:

$$\left|\tilde{x} - \frac{p}{n}\cdot(\tilde{x} + x_1 + x_2)\right| \le \left|x_1 - \frac{p}{n}\cdot(\tilde{x} + x_1 + x_2)\right| \text{ and } \left|\tilde{x} - \frac{p}{n}\cdot(\tilde{x} + x_1 + x_2)\right| \le \left|x_2 - \frac{p}{n}\cdot(\tilde{x} + x_1 + x_2)\right| \quad (2)$$

Since it is the absolute difference to the target value that determines winning or not, we have to perform a case-wise analysis that considers all possible combinations of positive or negative deviations from the target value. The details of this analysis are relegated to Appendix C, and we present only the main result here. For unimodal distributions of the depths of reasoning (like for instance the Poisson or the normal distribution) it can be shown that choosing $\tilde{x}_h*$ maximizes the probability of winning, where

$$\tilde{x}_h* = \frac{p}{n-p}\cdot(n-1)\cdot h_1 \,. \quad (3)$$

The variable h_1 in equation (3) denotes the average number shown in the *history* for period 1 (see Table 1), which is used as the best estimator for the numbers chosen by the other two group members, i.e. for $\sum_{i=1}^{n-1} x_i/(n-1)$.[10] The optimal choice $\tilde{x}_h*$ then results from taking into account one's own number $\tilde{x}$, i.e. the optimal choice is $\tilde{x}_h* = \tilde{x} = p(\sum_{i=1}^{n-1} x_i + \tilde{x})/n$.

For the *OneAdv*-treatment it is possible to reach very similar conclusions. One very intuitive way to deal with the information in *advice* is to choose the average number of the numbers suggested in the four advice sheets, $a = \sum_{a=1}^{4} x_a/4$, as an estimator for $\frac{p}{n-p}\cdot\sum_{i=1}^{n-1} x_i$. Note that the values for *a* and $\frac{p}{n-p}(n-1)h_1$ are very close to each other in our experiment (17.63 versus 18.79), meaning that *history* and *advice* convey very similar suggestions for

[10] To keep the notation simple we suppress the period subscript in cases where it is not necessary.

which numbers to choose. Therefore, there is no reason a priori to expect any of both sources to be more valuable than the other.

5. Results

Table 3 reports the medians, means and standard deviations of chosen numbers in the three treatments. Notice in all treatments the typical pattern of chosen numbers to decline over periods ($p < 0.01$ for any two consecutive periods and for any treatment[11]; two-sided Wilcoxon signed ranks tests). In the first two periods, the chosen numbers do not differ across treatments. However, in periods 3 and 4 the numbers are significantly higher in *Control* than in the other two treatments ($p < 0.05$; two-sided Mann-Whitney U-test). Hence, it seems that providing information (in the form of *advice* or *history*) triggers significantly faster convergence to the equilibrium in the later periods of the experiment.

Table 3
Median and mean numbers

	Period 1		Period 2		Period 3		Period 4	
Treatment	median	mean*	median	mean	median	mean	median	mean
Control (N = 11)	33.6	32.92 (24.06)	18.5	22.91 (17.50)	14.6	20.48 (18.46)	10	12.32 (8.53)
OneHist (N = 11)	25.2	33.56 (20.08)	18.5	23.42 (15.75)	10.2	13.43 (11.2)	6.8	8.92 (9.57)
OneAdv (N = 10)	24.4	29.44 (20.82)	17.5	20.55 (15.40)	8.8	16.6 (20.63)	5.0	10.16 (14.22)

* Standard deviations are in parentheses. N is the number of groups (with 3 members), i.e. independent observations.

In Table 4 we compare the median and mean choices of the informed as well as the uninformed group members. In *OneHist* we find that the informed members (those who see the *history*) choose significantly lower numbers in periods 1 and 2 ($p < 0.05$; two-sided Wilcoxon signed ranks test[12]), yet there is no significant difference in periods 3 and 4. In *OneAdv* the informed members (who receive *advice*) submit significantly smaller numbers in every single period ($p < 0.05$; two-sided Wilcoxon signed ranks test).

[11] Obviously, the letter p denotes p-values in the context of test results. Note that we have used the same letter to denote the factor that multiplies the average of chosen numbers in the game, because it is often also referred to as p-beauty-contest game (see, e.g., Ho et al., 1998).

[12] For this test we match numbers of the informed member with the average numbers of the uninformed members within a group.

Table 4

Median and mean numbers of informed and uninformed subjects

		Period 1		Period 2		Period 3		Period 4	
		median	mean*	median	mean	median	mean	median	mean
OneHist	uninformed	33.3	39.01 (21.41)	22.1	26.12 (18.33)	9.2	11.46 (12.87)	4.7	8.85 (9.53)
	History (informed)	21.0	22.64 (11.44)	17.0	18.01 (6.33)	14.0	11.37 (6.79)	7.0	9.07 (10.02)
OneAdv	uninformed	34.7	35.17 (28.04)	22.3	23.47 (17.08)	8.8	19.99 (24.20)	5.5	11.94 (16.43)
	Naïve advice (informed)	20.5	18 (7.58)	12.0	14.7 (9.77)	8.0	9.8 (7.65)	4.0	6.62 (7.77)

* Standard deviations are in parentheses

The findings in Table 4 are supported by an estimation of the depths of reasoning of informed and uninformed subjects in *OneHist* and *OneAdv*. Applying the cognitive hierarchy model of Camerer et al. (2004) yields the estimated values of τ in Table 5. We see for the *Control*-treatment that the estimated depth of reasoning is slightly above one and, thus, in the typical range. In treatments *OneHist* and *OneAdv* the depths of reasoning of informed group members is substantially higher than those of uninformed members.[13]

Table 5

Estimated depths of reasoning in period 1

		estimated τ
Control		1.07
OneHist	uninformed	0.96
	History (informed)	2.36
OneAdv	uninformed	0.65
	Naïve advice (informed)	3.13

One noteworthy feature of the data in Table 4 is the finding that the numbers chosen in the first period by uninformed members are not significantly different from those chosen in

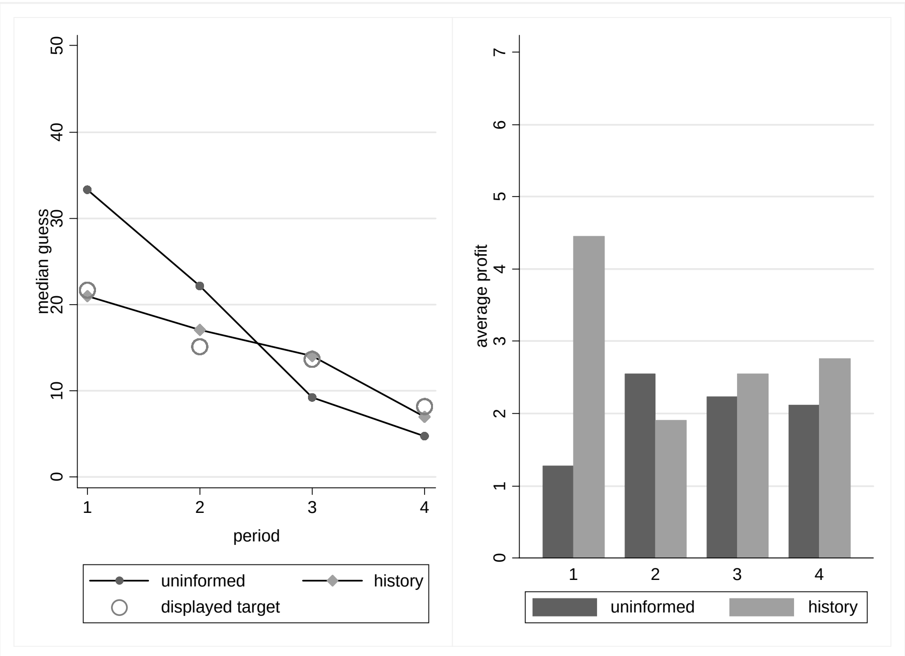
[13] In order to estimate the depths of reasoning of several subsamples of players we search for the parameter τ that minimizes $\left|\overline{x_\tau} - \overline{x}\right|$, where $\overline{x_\tau}$ is the predicted average choice for some τ and $\overline{x}$ is the actual average choice in the subsample.

Control. This holds true for both *OneHist* and *OneAdv* (compare the entries in the rows "uninformed" of *OneHist* and *OneAdv* in Table 4 with the entry in row "*Control*" of Table 3). From that one can conclude that, on average, uninformed group members do not take into account the fact that another group member is provided with either *advice* or *history*. An immediate implication of this finding is that using the information contained in *advice* or *history* should increase the likelihood of winning the beauty-contest game. We now turn to a more detailed analysis of this issue.

5.1 The impact of *history* on performance in *OneHist*

The left-hand panel of Figure 1 shows the median numbers chosen by group members with access to *history* and those without. The empty circles indicate the target value contained in *history* (see Table 1). The right-hand panel of Figure 1 displays the average profits of informed and uninformed group members.

Figure 1
Median numbers and profits in OneHist



Note from the chosen numbers that the median number of group members with access to *history* is very close to the suggested target in each period. This means that about 50% of the informed subjects choose *higher* numbers than those suggested by the *history* table. However, no subject with *history* information ever simply imitated the target value shown in Table 1. Furthermore, on average informed subjects do not take into account the influence of their own number on the group's target number. Recall from the previous section that subjects with access to *history* should choose $\tilde{x}_h{*} = p \cdot (n-1) \cdot h/(n-p)$ in the first period, which would be 18.79. Yet, the median is 21.0 in the first period. While in periods 1 and 2 the median numbers of subjects with *history* information are nevertheless significantly smaller than those of uninformed subjects, the latter choose smaller numbers in periods 3 and 4, meaning that subjects with information neglect the dynamics of chosen number in their choices and instead focus on the information contained in *history*.

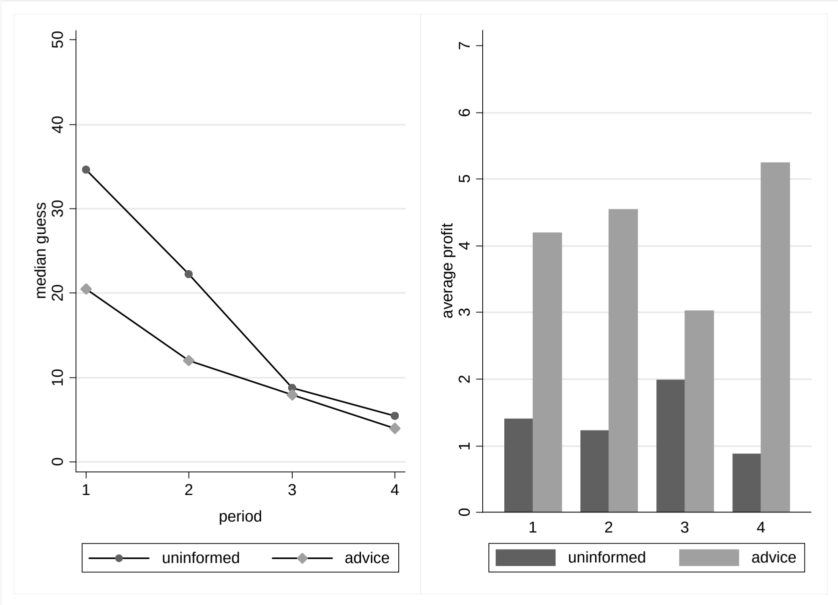
Subjects with *history* information win the game significantly more often in period 1 and, thus, earn more money than uninformed subjects ($p = 0.03$; two-sided Wilcoxon signed ranks test), but there is no significant difference in later periods ($p > 0.6$ in periods 2-4), and the overall earnings of informed (€ 11.76) and uninformed (€ 8.16) group members are not significantly different either ($p > 0.4$).

5.2 The impact of *advice* on performance

Figure 2 shows in the left-hand panel that group members receiving advice choose on average lower numbers than uninformed group members in each period. Recall from the previous section that we expected subjects with advice to choose $\tilde{x}_a{*} = a = 17.63$ in the first period. In fact, we observe a slightly higher average number. However, in terms of profits, group members with naïve advice clearly outperform the other two members in each single period (with $p < 0.05$ in periods 1, 2, and 4; two-sided Wilcoxon signed ranks test). Overall earnings are also significantly higher for informed (€ 17.03) than for uninformed (€ 5.48) subjects ($p < 0.01$).

Figure 2

Median numbers and profits in OneAdv



5.3 The comparative effects of *advice* and *history*

Comparing the impact of *advice* and *history* we have found that both types of information lead in the aggregate to significantly lower numbers in periods 3 and 4, compared to the *Control*-treatment, and thus to quicker convergence to the equilibrium. Concerning the performance of those subjects endowed with either *advice* or *history* we have found that access to *advice* yields much higher payoffs than access to *history* (17.03€ in *OneAdv* vs. 11.67€ in *OneHist*). Yet, the difference in earnings is not significant on conventional levels ($p = 0.16$; two-sided Mann Whitney U-test).

Contrary to the actual payoffs for subjects with either *advice* or *history*, we had expected *history* to have a stronger relative influence on the winning probability for the following reasons: The information in *history* is easier to interpret and it is based on a larger sample, and this is known to subjects receiving *history*. Furthermore, some pieces of advice contained explicitly wrong statements, such as advice 3 suggesting that "*Since there are three participants, the average number out of 0-100 is 33*", or advice 2 proposing to *in*crease rather than decrease the chosen numbers over time. While statements in *advice* simply suggest that there is a "*tendency to decrease numbers"* or that "*you should decrease the number from period to period"*, the information in *history* carries precise data by how much average numbers and target values decrease over time.

Given that the information in *history* provided a single number for each period, whereas *advice* contained four different suggestions, we would also have expected a smaller variance of the numbers chosen in *OneHist* than in *OneAdv*. Yet, the variances (see Table 4) are not significantly different (Levene test, $p > 0.3$), but on average even smaller in *OneAdv* than in *OneHist*. In sum, it seems that access to *advice* is more useful than the information contained in *history*. The following subsection examines why this might be the case.

5.4 Some ex-post simulations on how *history* could have been used better

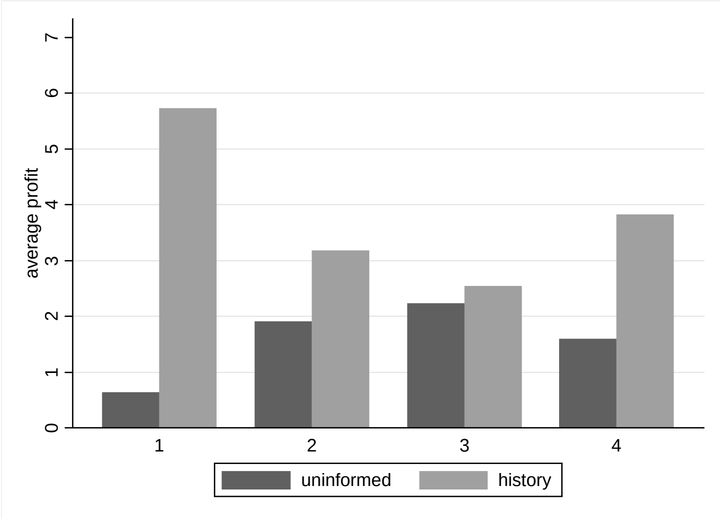
Given the rather poor performance of group members with access to *history* we were interested in whether some straightforward strategies for the use of the information contained in *history* could have yielded higher profits for the members with *history*. We will consider three different strategies: (i) pure imitation of the target values contained in *history*, (ii) best reply to the target values in *history*, and (iii) dynamic best reply to the target values.

Of course, doing simulations on the strategies of the informed group members requires some assumptions regarding the choices of the uninformed group members. The first period choices of uninformed members can be taken as they are because they are completely independent. Yet, for periods 2 to 4 we have to assume how uninformed subjects would have reacted to the different (i.e. simulated) numbers of the informed members. We have decided to adjust the actual choice of the two group members without access to *history* by an adjustment factor $f_t = \frac{x_{i,t}}{\sum_{i=1}^{3} x_{i,t-1}/n}$ (their choice divided by the group average of the previous period) in order to determine their simulated choice in the current period, $x_{i,t}^{sim}$, by $x_{i,t}^{sim} = f_t x_{t-1}^{sim}$ (where $x_{i,1}^{sim} = x_{i,1}$)These adjustment factors are calculated from *OneHist* and applied to the simulations.

Simulation 1 uses the simplest of all possible strategies: *imitation* of the target value stated in *history*. Hence, we assume that the informed members choose the following number in each period *t*: $x_{i,t} = p \cdot h_t$. Figure 3 shows the hypothetical profits for this simulation. Using this simple strategy, subjects with *history* would have earned significantly more than the uninformed subjects in periods 1 and 4 as well as over the whole experiment ($p < 0.05$ in all cases; two-sided Wilcoxon signed ranks tests). Recall that in the real sessions (see Figure 1) subjects with *history* did *not* earn more than uninformed subjects over the entire experiment (nor in period 4). Hence, simple imitation would have yielded higher earnings than subjects actually achieved.

Figure 3

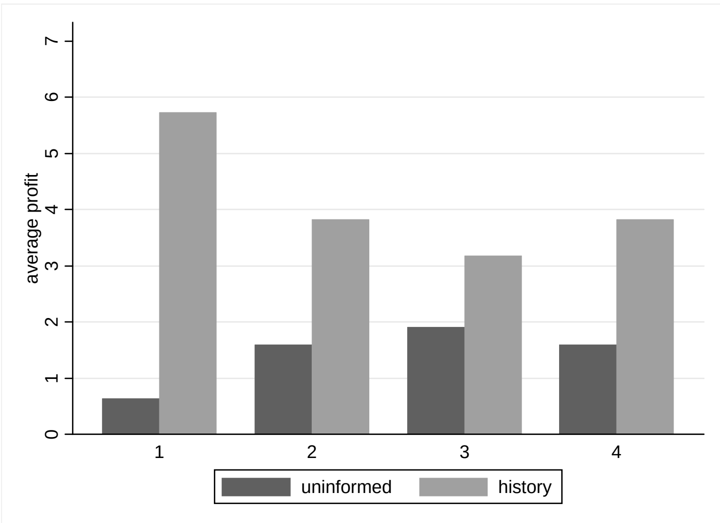
Simulation 1: Hypothetical average profits for Imitation



Simulation 2 assumes that informed subjects use a *best-reply* strategy that responds optimally to the target value shown in *history*. Hence, informed subjects would choose $x_{i,t} = p \cdot (n-1) \cdot h_t / (n-p)$ in each period. Figure 4 indicates that such a best-reply strategy would have resulted in even higher profits than in case of simple imitation. Subjects with history information would have earned significantly more money than uninformed subjects in periods 1, 2 and 4 as well as overall ($p < 0.01$ for period 1 and overall, $p < 0.1$ for periods 2 and 4; two-sided Wilcoxon signed ranks tests).

Figure 4

Simulation 2: Hypothetical average profits for Best-reply

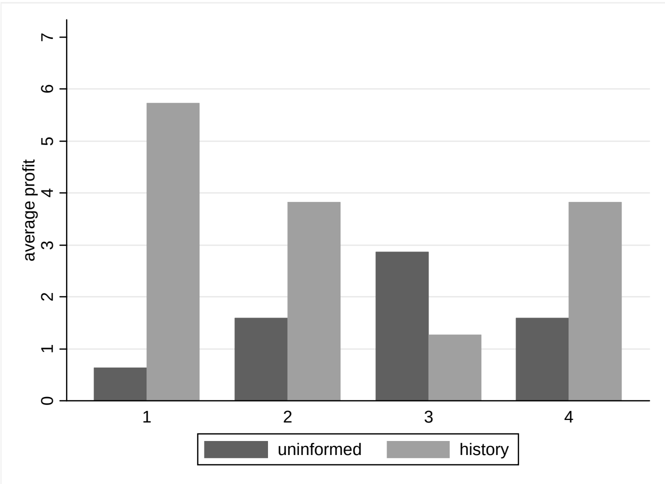


Simulation 3 assumes a *dynamic best-reply* strategy of informed subjects where they do not only take into account the information in *history*, but also the dynamics of chosen numbers in their own group. Hence, in period 1 we let subjects with *history* choose the target value for period 1. In later periods, informed subjects are assumed to calculate the convergence – or adjustment-rate – from period t to period t+1 from *history* and apply it to the target number of the own group in period t in order to determine the number for period t+1. Thus, the simulated numbers of informed subjects are as follows (where $\bar{x}_{t-1}$ refers to the average number observed in the informed subject's group in period t–1.

$$x_{i,t} = \begin{cases} ph_t & \text{for } t = 1 \\ \bar{x}_{t-1} \cdot \dfrac{h_t}{h_{t-1}} & \text{for } t > 1 \end{cases} \quad (4)$$

Figure 5 shows the hypothetical earnings for a dynamic best-reply strategy. Informed subjects would have earned significantly more money than uninformed subjects in periods 1, 2 and 4 as well as overall ($p < 0.01$ for period 1 and overall, $p < 0.1$ for periods 2 and 4; two-sided Wilcoxon signed ranks tests).

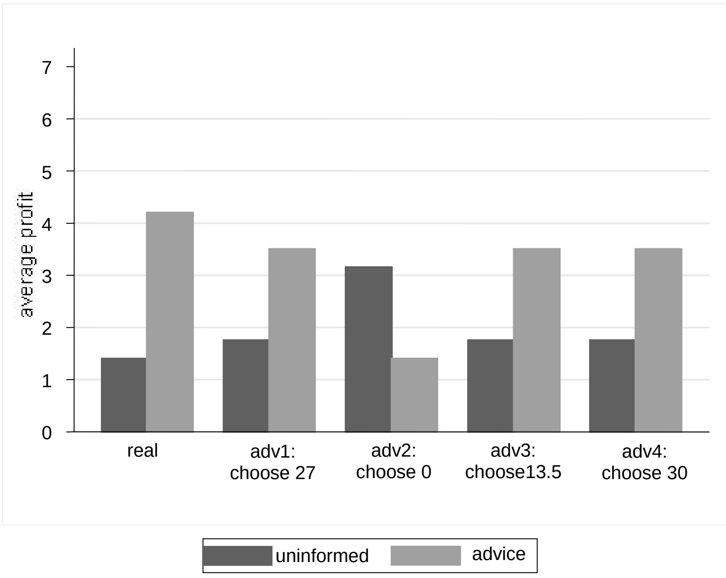
Figure 5
Simulation 3: Hypothetical average profits for Best-reply adjustment



Comparing hypothetical profits in the three simulations with actual ones in *OneHist* (see Figure 1) we find that the overall hypothetical profits are significantly higher in each of the three simulations than the actual profits ($p < 0.07$ for simulations 1, 2 and 3; two-sided Wilcoxon signed ranks tests). This clearly indicates that subjects with access to *history* are not able to make the most out of it. Even rather straightforward strategies like imitation or best-reply would have performed better than the actual play of informed subjects in the experiment. Of course, this statement depends on our assumption about the behavior of uninformed subjects in periods 2-4. Therefore, it is probably noteworthy that the average hypothetical profits of informed subjects were about 20% higher than the actual ones already in period 1, for which we plugged in the actual choices of uninformed subjects.

At the end of this subsection we would like to provide some further evidence on the possible use of the suggestions in *advice*. Since each sheet of advice contained a proposed number for period 1, we can check whether following any of them separately would have paid off. Figure 6 shows the actual average profits of informed subjects as well as the hypothetical profits if informed subjects had picked any of the four numbers stated in *advice*. Note that advice 2 suggests choosing the equilibrium, i.e. zero. Yet, this would have yielded the lowest average earnings for informed subjects. Note also that the actual average earnings of informed subjects are higher than if they had chosen any particular number. This seems to indicate that subjects receiving *advice* do not focus on a single number, but rather try to form a synthesis of several pieces of advice, and this also contributes to the relatively better performance of subjects with *advice* than those with *history*.

Figure 6
Hypothetical average profits if subjects had followed one of the suggestions in advice



6. Conclusions

In this paper we have studied the effects of social learning on the depths of reasoning in an experimental beauty-contest game. In particular we have examined how the access to historical data and how receiving advice from others influence decision making in this game. A particular feature of our experiment is that we have set up groups in which only one member got access to either advice or history. This approach with asymmetric access to advice or history and the use of the beauty-contest game distinguish our paper from previous papers on social learning (Chaudhuri et al., 2001, 2005; Schotter and Sopher, 2003, 2007; Celen et al., 2006). As a consequence of our setup, we have been able to compare the performance of informed versus uninformed subjects, but also of subjects receiving advice versus subjects with access to history information.

Receiving advice and observing historical data has been shown to have a large and significant impact on the probability of winning in the very first period. As such, both sources of information seem to be a substitute for own previous experience of the game. Camerer and Ho (2002) and Slonim (2005), for instance, have shown that experienced subjects (who have played a beauty-contest before) approach the equilibrium much faster and outperform inexperienced subjects. Similarly, the effects of advice and history are related to the evidence on group decision making in the beauty-contest game. (Inexperienced) Groups win the beauty-contest game significantly more often than (inexperienced) individuals (Kocher et al., 2006) due to the experience they can gain in the group discussion. Given these similarities in findings, our paper can establish a link between several lines of research that have not been closely related so far.

Another interesting finding of our paper is the asymmetry of advice and history as regards their longer-run impact on winning probabilities. Whereas subjects with access to advice are able to earn significantly more money over the entire experiment in comparison to their uninformed group members, subjects who know the history of previous unrelated sessions are not able to earn significantly more than their uninformed counterparts within the group.

In a particular sense this finding is reminiscent of previous studies, which have established that advice has a stronger influence on decisions than mere observation (see, for example, Schotter and Sopher, 2003, 2007 or Celen et al., 2006). However, in these previous studies – as already mentioned before – all subjects in a group had access to the additional information, whereas in our case we let informed subjects directly compete against uninformed ones. In accordance with the literature, we may conclude that words (i.e. *advice*) have a stronger impact on behavior and the probability of winning a beauty-contest game than actions (i.e. *history*). It seems that subjects who receive advice think more carefully about the decision making task than subjects with access to history. The divergent opinions expressed in the four sheets of advice may actually support higher depths of reasoning as they force a subject to digest the different suggestions and build an own opinion. This is not to say that subjects with access to *history* do not think about the problem themselves. Recall that no subject with *history* information ever simply imitated the target value shown in Table 1. Our simulations at the end of the results section show, however, that subjects with history information could have earned more money by doing so (and even more money by using a best-reply strategy).

Of course, one has to be extremely careful with direct applications of our results from a stylized experiment to real-world phenomena. Nevertheless, we believe our experiment provides a few new and interesting arguments why people in reality often seem to prefer non-expert advice over history statistics. They probably prefer the brain-teaser of different opinions to derive an own strategy instead of history information, and our experiment shows that they may be right in doing so.

7. References

Bosch-Domènech, A., García-Montalvo, J., Nagel, R. and Satorra, A. (2002). One, two, (three), infinity, ..: Newspaper and lab beauty-contest experiments. *American Economic Review* 91, 1687-701.

Camerer, C and Ho, T.H. (2002). Sophisticated experience-weighted attraction learning and strategic teaching in repeated games. *Journal of Economic Theory* 104, 137-88.

Camerer, C. F., Ho, T.-H. and Chong, J.-K. (2003). Models of thinking, learning, and teaching in games. *American Economic Review, Papers and Proceedings* 93, 192-5.

Camerer, C. F., Ho, T.-H. and Chong, J.-K. (2004). A cognitive hierarchy model of games. *Quarterly Journal of Economics* 119, 861-98.

Capra, C. M., Cabrera, S. and Gómez, R. (2003). The effects of common advice on one-shot traveler's dilemma games: explaining behavior through an introspective model with errors. Working Paper E2003/17, Fundación Centro de Estudios Andaluces.

Celen, B., Kariv, S. and Schotter, A. (2006). An experimental test of advice and social learning. Working Paper, UCLA Department of Economics.

Chaudhuri, A., Graziano, S. and Pushkar, M. (2006). Social learning and norms in a public goods experiment with inter-generational advice. *Review of Economic Studies* 73, 357-80.

Chaudhuri, A., Schotter, A. and Sopher, B. (2001). Talking ourselves to efficiency: Coordination in inter-generational minimum games with private, almost common and common knowledge of advice. Working paper, New York University.

Costa-Gomes, M. A. and Crawford, V. P. (2007). Cognition and behavior in two-person guessing games: an experimental study. *American Economic Review*, forthcoming.

Duffy, J. and Nagel, R. (1997). On the robustness of behaviour in experimental "beauty contest" games. *Economic Journal* 107, 1684-700.

Fischbacher, U. (2007). z-Tree: Zurich toolbox for ready-made economic experiments-experimenter's manual. *Experimental Economics*, forthcoming.

Gneezy, U. (2005). Step-level reasoning and bidding in auctions. *Management Science* 51, 1633-42.

Grosskopf, B. and Nagel, R. (2007), Rational reasoning or adaptive behavior? Evidence from two-person beauty contest games. *Games and Economic Behavior*, forthcoming.

Güth, W., Kocher, M. G. and Sutter, M. (2002). Experimental "beauty-contests" with homogeneous and heterogeneous players and with interior and boundary equilibria. *Economics Letters* 74, 219-28.

Ho, T. H., Camerer, C. and Weigelt, K. (1998). Iterated dominance and iterated best response in experimental p-beauty-contests. *American Economic Review* 88, 947-69.

Keynes, J. M. (1936). *The general theory of employment, interest and money.* New York: Harcourt Brace and Co.

Kocher, M. G., Strauss, S. and Sutter, M. (2006). Individual or team decision making - causes and consequences of self-selection. *Games and Economic Behavior* 56, 259-70.

Kocher, M. G. and Sutter, M. (2005). The decision maker matters: Individual versus group behavior in experimental beauty-contest games. *Economic Journal* 115, 200-23.

Kovac, E., Ortmann, A. and Vojtek, M. (2004). Comparing guessing games with homogeneous and heterogeneous players: Experimental results and a CHM explanation. Working Paper, CERGE-EI Prague.

Nagel, R. (1995). Unraveling in guessing games: An experimental study. *American Economic Review* 85, 1313-26.

Sbriglia, P. (2004). Revealing the depth of reasoning in p-beauty-contest games. University of Naples II, Working Paper.

Schotter, A. (2003). Decision making with naïve advice. *American Economic Review, Papers and Proceedings* 93, 196-201.

Schotter, A. and Sopher, B. (2003). Social learning and coordination conventions in inter-generational games: An experimental study. *Journal of Political Economy* 111, 498-528.

Schotter, A. and Sopher, B. (2007). Advice and Behavior in Intergenerational Ultimatum Games: An Experimental Approach. *Games and Economic Behavior* 58: 365-93.

Slonim, R. L. (2005). Competing against experienced and inexperienced players. *Experimental Economics* 8, 55-75

Sutter, M. (2005). Are four heads better than two? An experimental beauty-contest game with teams of different size. *Economics Letters* 88, 41-6.

Weber, R. A. (2003). “Learning” with no feedback in a competitive guessing game, *Games and Economic Behavior* 44, 134-44.

Appendix A: Instructions (translated from German) – for referees' convenience, not necessarily for publication

[The following instructions have been read aloud prior to the sessions. In all sessions, we used the same description of the beauty-contest game. However, parts of the instructions are specific to particular treatments. This will be indicated by italic letters.]

Welcome and thank you for participating! Please do not talk to other participants from now on. This experiment is about economic decision-making. You will earn "real" money, which will be paid out right after the experiment, including a show-up fee of € 3.

Groups and number of periods

In this experiment we will randomly form groups of three persons. The composition of groups will remain fixed and group members will interact for four periods. Your decisions will remain anonymous, i.e. you will not learn the identity of the other group members nor will they learn your identity.

The decision

You are a member of a group of three persons. At the beginning of each period, each person chooses a number x_i out of the range from 0 to 100, including 0 and 100. The number does not necessarily have to be an integer, but may not have more than two digits after the comma.

Your payoff depends on the absolute difference between your number and a target value. The group member whose number is closest to the target value of a given period receives € 7, the others receive zero in the respective period. If the numbers of two or three group members are equally close to the target value, then those participants share the price of € 7 equally and receive € 3.5, respectively € 2.33.

Calculation of the target value

To calculate the target value we first compute the average of the three numbers x_i in your group. Then, the average is multiplied by 2/3. This yields the target value. In mathematical notation:

$$\text{Target value} = \frac{2}{3} \cdot \left(\frac{\sum_{i=1}^{3} x_i}{3} \right),$$

where x_i refers to the chosen numbers within a group in a given period. Thus, to win the game in a given period you have to come as close as possible to two thirds of the average number in your group.

Control-treatment: Advice for participants of future experiments [announced at the end of the experiment]

The experiment explained above will be run several times during the summer term 2005. Please give participants of those future experiments an anonymous advice on how to best play this game. You find an advice sheet attached to these instructions. Please fill it out. The advice has to include 1) a number the player should choose in period 1 and a reason for it as well as 2) a strategy you suggest to pursue in the following periods.

We will randomly pick four advice sheets and distribute these four sheets to participants in future experiments. In these experiments one out of three group members will receive the four selected sheets. After the experiments will have been run, we will randomly choose four players who received the advice sheets. Then, we will match each of those four subjects with an advisor (drawn randomly from your experiment) whose advice has been distributed. The advisor receives three times the payoff of the person who has been matched with him or her.

Thus, your payoff from giving advice depends on whether your advice has been randomly selected and, if so, it depends on the earnings of the person who has been matched with you. The more this person earns (the better your advice was), the more you will earn.

In order to be able to inform you on your additional earnings from the advice, please note your email-address on the form. We will not reveal it to participants of the future experiments.

OneHist, Oneadv: Data of previous experiments

The experiment explained above has been run previously at this university in the summer term 2005. A total of 33 persons have participated in these previous sessions (= 11 groups of three).

OneHist: *We have recorded the data of those games. The data include the average chosen number of all 33 participants and the average target value per period. Today, one randomly selected group member in each group will receive the data from these previous* *sessions before the experiment starts. This means that one randomly selected group member will get a table which shows the overall average number in each period and the corresponding average target value.*

OneAdv: *At the end of their experiment, the 33 subjects were asked to fill out an advice sheet on how to best play this game. The advice includes a suggested number for period one, an explanation for the suggested number, and a suggested strategy to pursue in the following periods. We have randomly selected four sheets of advice and have photocopied these sheets. The copies will be distributed to one randomly selected group member in each group before the experiment starts. Note that the subjects who have written the advice will benefit from your earnings. In case you receive some advice sheets and in case one of the four sheets you receive is randomly selected at the end of the experiment (with a chance of 1:11), the subject who wrote this advice will receive three times your earnings. Note also that this procedure does not at all affect your earnings.*

Appendix B: Advice sheets (translated from German) – for referees' convenience, not for publication

B1: Advice sheets that have been distributed in *OneAdv*

Advice 1:

Choose 27 in Period 1
Why: Since 2/3 of the average of all numbers is the target, the target number is not too high (between 10 and 20). However, some of the participants do not know this, and the target in Period 1 is above the targets of later periods.
Strategy: Decrease the number from period to period. The participants realize that they should decrease the numbers (actually, the target should converge to zero). However, you cannot count on the others: set a number between 13 and 21.

Advice 2

Choose 13.5 in Period 1
Why: Most of the time the game starts with a low number.
Strategy: Slowly increase the number from period to period. Increase the number by not more than 10 in one step.

Advice 3

Choose 0 in Period 1
Why: To see how the other participants behave. If the others think that the people in the group are "rational", they should also choose zero.
Strategy: You can see whether the other participants know what the game is about or whether they just guess. If they guess, you should set around the target number of the previous period. If they do not guess, set zero again.

Advice 4

Choose 30 in Period 1
Why: Since there are three participants, the average number out of 0 – 100 is 33. Hence, people like to choose this number. However, the average will be multiplied by 2/3, which will reduce the target value. This means that a number below 33 might be closest to the target value.
Strategy: There is tendency to decrease numbers, which results from the multiplication of the average by 2/3. Hence, lower numbers should come closer to the final value.

B2: Advice sheets that have not been distributed in *OneAdv*

Advice 5

Choose 25 in Period 1
Why: Never set above 50. The chance will be higher, because in total there will be some 50 points. Most people do not set that high numbers. The first time is the most difficult.
Strategy: Normally, the number declines, since after the first period, every player sets the target of the previous round. The NEW target is thus somewhere around 2/3 of the OLD target.

Advice 6

Choose 10 in Period 1
Why: You are in a group with some other players. Some will choose high, some low numbers or zero. Therefore, the probability that you win is highest if you set 10.
Strategy: If you really win with number 10, then repeat your guess in the next round. If you realize that the others adjust and set 10 as well, then choose a lower number (i.e. 7). If you do not win in the first round, then set the number of the winner in the second round and in the following rounds a lower number.

Advice 7

Choose 7 in Period 1

Why: Because at the beginning all choose very low numbers. Never set a number above 75 (mind the formula). If one thinks through the formula, the outcome should be low numbers.

Strategy: Pay attention to whether the others set high or low numbers. If they set low numbers, then choose numbers in the low range as well. If they set high numbers, do so as well.

Advice 8

Choose 31.5 in Period 1

Why: Many will go for a number in the center, around 50. The target is 2/3 times the average in the group. Thus, one should set a little lower (the best choice is around 30).

Strategy: Many players will follow the hint from period 1. One can see it from the results of every period. This means that one should set a lower number afterwards.

Advice 9

Choose 35 in Period 1

Why: Since the average is multiplied by 2/3 and under the assumption that the opponents choose somewhere between 40 and 60, 35 is a dominant strategy. Lower numbers are likely to be too far off the target value, and the ones higher are because of the formula not interesting.

Strategy: It is a dominant strategy to set less points than the opponents. Since they will pursue the same strategy, the number must, according to the estimated decline of the opponents, be reduced itself.

Advice 10

Choose 15 in Period 1

Why: The number must be lower than 66.6, since if all players choose 100, the target is only 2/3 times 100. But since not all will set 100, it is the best to choose a relatively low number

Strategy: Always set >2/3 of the target of the last period. (i.e. if the target was 30, then set around 18, because the others will lower their x_i as well.

Advice 11

Choose 33 in Period 1

Why: I have reached the target value. I have tried to calculate a mean.

Strategy: Set below the value of your 2 opponents from the last period. Stay in the range of the previously chosen values.

Advice 12

Choose 15 in Period 1

Why: This is most likely to correspond to the average numbers of the group

Strategy:

Advice 13

Choose 10 in Period 1

Why: Low numbers are preferred. Therefore, the target value is below 10 most of the time.

Strategy: Always set numbers that are a little below those of your opponents.

Advice 14

Choose 20 in Period 1

Why: Generally, it is expected that the participants orientate themselves to the center and choose 50. The target is thus 2/3 times 50. If the participants set around 30 to 35, the target is around 20.

Strategy: General downward tendency, thus decrease your number by around 5 in every round.

Advice 15

Choose 8 in Period 1

Why: In the first round, many do not realize that the number must be lower than the average. If all had fully realized it at the beginning, the number would have to approach 0.

Strategy: normally, the number declines, therefore, I would try numbers between 0 and 2 in the following periods.

Advice 16

Choose 10 in Period 1

Why: Probably the others set higher (~ 50) or much lower. Because of the high number the target value increases (this participant does not reach it anymore) and you are in between.

Strategy: Observe what your "partners" set, and stay (if someone sets higher than 50) with your number between 10 and 15. If they all decline already in the 2nd period, stay (3rd period) below 10. Good luck.

Advice 17

Choose 50 in Period 1

Why: That is a first approximation. One can see whether the other participants rather set lower ore higher numbers.

Strategy: If the participants have set low numbers (0-49), then also set between 0 and 49. If that is not the case, set a number between 50 and 100.

Advice 18

Choose 18 in Period 1

Why: With it you will be in the middle + you check both directions (2 participants who set very low + 2 participants who set very high numbers.

Strategy: bare in mind what the participants 1 and 2 choose (if someone chooses very high/low) and adjust. You can orientate on the target value of the first period.

Advice 19

Choose 16 in Period 1

Why: It tends to be that at the beginning the participants will set lower numbers. The number should in any case never exceed 20, since only 2/3 of the average is the target value.

Strategy: Follow the tendency of the previous period. (I e. 1 10 50, set the target between one and 10. The other players will move into the same direction. Sheering out does not pay.

Advice 20

Choose 34.5 in Period 1

Why: A number between 30 and 40, because of the formula for the target value (average of all 3 numbers times 2/3), suitable for period 1. This is the case because in my point of view, the other players will choose out of this range as well.

Strategy: Set a number around half of the number you have chosen in period 1.

Advice 21

Choose 33 in Period 1

Why: The players will choose numbers out of the range slightly above the center (50 – 70) because they have to reach the value of 2/3. In my case, it worked.

Strategy: Now the others will move (with some luck) downwards as well. Then, you have to do that as well, but if the block moves upwards, go with it. Unfortunately, I did not do so.

Advice 22

Choose 13 in Period 1

Why: Set a number between 10 and 20, in order to find out in what range the sought numbers are. First, start with a low number. The probability of a low number is higher (personal opinion)

Strategy: Right afterwards, try a relatively high number. Then you approximately see how big the numbers are. Also, pay attention to the numbers of the other group members.

Advice 23

Choose 14 in Period 1

Why: Assumption, that people start with 20 or slightly lower. 20 times 2/3 is 14.

Strategy: Downward adjustment of the others. First only little (around 8 to 10) then more (around 2 to 5), always according to the average that has been reached by the numbers of the others.

Advice 24

Choose 26.55 in Period 1

Why: You have the choice between 0 and 100. One could assume that one of the three sets around 50 and the other one has already realized that and therefore sets only 15-20. Thus it is best to be in the center.

Strategy: Observe what the others do. The number tends to decline. The number should always be set lower, since the other players will as well always set lower numbers. One orientates herself to the target value.

Advice 25

Choose 0 in Period 1
Why: The higher the number you choose, the lower is the probability to reach the target value. If everyone always sets 0, then everyone in the group will win something in every period.
Strategy: The tendency of numbers goes down, and numbers between 0 and 10 are recommended. The closer to zero, the better. Try also decimal digits, not just integer numbers.

Advice 26

Choose 26 in Period 1
Why: If on average everyone sets half of the highest feasible number (50); (50+50+50)/3 = 50; 2/3 times 50 = 36. Since experience shows that the other players tend to set lower numbers, set 26.
Strategy: Try to be the player with the lowest bid. In any case, choose a number below 40.

Advice 27

Choose 35 in Period 1
Why: Theoretically, everyone sets 50. Target value ~ 32. If 2 persons set only ~ 30, then you loose, if not, then you win.
Strategy: 18, because now all players try to set 35. You win because of the reaction, see where the experiment goes to, you decline to be closer to the theoretical target value, because the others try to cooperate, always underbid the target value.

Advice 28

Choose 50 in Period 1
Why: It is safest, lies in the center. You do not know what the other participants choose.
Strategy: Then 25, then I would take 75. And have always a look which numbers the other participants in your group take, and choose them next time. This is more important than 25+75.

Advice 29

Choose 0 in Period 1
Why: If everyone has understood the experiment, everyone will set 0, because then everyone will win something (although the prize will be divided by 3). Even if not everyone has understood in the first round, the probability that there is a small target value is high, and you have a chance of winning.
Strategy: Always set 0. If everyone thinks so, everyone earns 4 times 7/3 = 9.30. If not everyone follows the system, the target value declines not proportionally (i.e. 20, 10, 4, 0.9). Good luck.

Advice 30

Choose 30.1 in Period 1
Why: 66.6 is the highest feasible number. If you assume that everyone knows that, everyone will choose a small number. Therefore 30.1. At least in that range or lower.
Strategy: Always set lower than the number that has won in the previous period. Last number times 2/3 >= next choice.

Advice 31

Choose ~20 in Period 1
Why: If everyone would set 100, then the average would be 66.6. Thus, it is better to set lower than 100. Everyone will do so. Therefore, set even lower. You could go on with that infinitely until 0. Since not everyone thinks so thoroughly, 20 is a good starting point.
Strategy: The number will continuously decline. The other participants will learn from the results of the previous periods.

Advice 32

Choose 25.33 in Period 1
Why: According to probability, the 3 participants set 150 in total. This number divided by 3 is 50 per participant. The target value is 2/3 of it, 33.67. Because you want to approximate it, set 33.67 – 5.67 (your difference of 50 – 33.67 – x) – 2.67.

Strategy: In what follows all players will decrease their points. You as well! A little more than the others. Please: Set your new number below the target value of the previous period. times 2/3 times 0.9. This means: Target value Period 3 i.e. 27, then times 2/3 = 18 times 0.9 is 16.2.

Advice 33

Choose 10 in Period 1
Why: a low number increases the chance, because my opponents set only in this range of numbers, at least very often (3 out of 4 times).
Strategy: Orientate yourself to the numbers of the others. In my case there was a tendency to continuously decline the numbers.

Appendix C: Derivation of predictions for subjects with *advice* or *history*

Here we prove that choosing $\tilde{x}_h^* = \frac{p}{n-p} \cdot (n-1) \cdot h_1$ in period 1 is an optimal strategy for a subject endowed with *history*. Moreover, we will argue that by the same logic, it is optimal for a subject endowed with *advice* to choose $\tilde{x}_a^* = a = \sum_{a=1}^{4} x_a / 4$.

As noted above, *p* is the factor that multiplies the group average, *n* is the number of group members, and h_1 is the group average of period 1 included in *history*. Equation 2 repeats the two conditions by which the group member endowed with *history* will win.

$$\left|\tilde{x}_h - \frac{p}{n} \cdot (\tilde{x} + x_1 + x_2)\right| \leq \left|x_1 - \frac{p}{n} \cdot (\tilde{x} + x_1 + x_2)\right| \text{ and } \left|\tilde{x}_h - \frac{p}{n} \cdot (\tilde{x} + x_1 + x_2)\right| \leq \left|x_2 - \frac{p}{n} \cdot (\tilde{x} + x_1 + x_2)\right| \quad (2)$$

Since it is the absolute value that determines winning or not, and the differences between the individual and target numbers can be either positive or negative, we have to do a case-wise analysis. Table 6 has a column for each of the differences included in (2). Since three differences can be either positive or negative, we have *$2^3 = 8$* different cases to analyze.

Table 6
cases (signs of differences)

case	$\tilde{x}_h - \frac{p}{n} \cdot (\tilde{x} + x_1 + x_2)$	$x_1 - \frac{p}{n} \cdot (\tilde{x} + x_1 + x_2)$	$x_2 - \frac{p}{n} \cdot (\tilde{x} + x_1 + x_2)$
1	+	+	+
2	+	-	+
3	+	+	-
4	+	-	-
5	-	+	+
6	-	-	+
7	-	+	-
8	-	-	-

Case 8 cannot occur, since it is not possible that all numbers are lower than the target value. In case four, either one of the two opponents will win, but never the subject with *history*. We thus restrict our analysis on the remaining cases. For each of those, it is possible to readjust the conditions from (2) by simple algebra.

Since *history* is generated from games using the same subject pool than the game that the subject endowed with *history* is playing herself (and this is common knowledge), she can reasonably assume that her opponents' numbers in period 1 are from a distribution with the expected value shown in *history*. Let $f(x_1)$ and $f(x_2)$ be the distribution functions of the opponents' numbers, and let $F(x_1)$ and $F(x_2)$ be the cumulated distribution functions. The following analysis works for a wide variety of such distributions, including all unimodal distributions as well as many others. However, for some bimodal distributions the optimal behavior is different from the one derived below.[14]

- **Case 1:** All differences are positive.

  If all differences between the individual numbers and the target value are positive, the group member with additional information will win if $\tilde{x}_h \leq x_1$ and $\tilde{x}_h \leq x_2$. Given the distributions from above, the probability of her victory is $(1-F(\tilde{x}_h))^2$. Since by definition, $F(\tilde{x}_h)$ is increasing in $\tilde{x}_h$, the probability must be decreasing in it. Thus, the probability of victory can be maximized by setting the lowest possible number which fulfils the criterion that all numbers must be above or equal to the target value. In case 1, it is thus best to expect the opponents to set the average of $f(x_1)$ and $f(x_2)$ and to give an optimal reply by choosing the number such that $\tilde{x}_h = \frac{p}{n-p}.E(x_1+x_2) = \frac{p}{n-p}\cdot(n-1)\cdot h_1$. Note that the latter expression is the expected target value in period one of the current game, given h_1.

- **Cases 2 and 3:** The number of one opponent is lower than the target value.

  In those cases, the group member endowed with *history* will win if $\tilde{x}_h \leq \frac{2\cdot p}{(n-2\cdot p)}\cdot x_1 - x_2$ (Case 2) or $\tilde{x}_h \leq \frac{2\cdot p}{(n-2\cdot p)}\cdot x_2 - x_1$ (Case 3). This results in probabilities of victory of $1-F\left(\frac{n-2\cdot p}{2\cdot p}\cdot\tilde{x}_h\right)-F(x_2)$ (Case 2) and

[14] For a large number of distributions, the analysis suggests a best reply on the expected sum of opponents' choices to be optimal. However, assume for example that each uninformed member chooses 80 with a probability of 10 percent and 0 with a probability of 90 percent. The best reply with respect to the sum of expected choices (16) is 4.47. But this number will win only with a probability of 10 percent, while 0 will win for sure.

$1-F\left(\frac{n-2\cdot p}{2\cdot p}\cdot\tilde{x}_h\right)-F(x_1)$ (Case 3). The probabilities are dependent on either x_1 or x_2.

Again, since $F\left(\frac{n-2\cdot p}{2\cdot p}\cdot\tilde{x}_h\right)$ is increasing in $\tilde{x}$, it is best to choose the lowest possible number, which is the expected target value.

- **Case 5:** Only the informed subject chooses below the target value.

  Here, the subject with *history* will win if both $\tilde{x}_h \geq \frac{2\cdot p}{n-2\cdot p}\cdot x_2 - x_1$ and $\tilde{x}_h \geq \frac{2\cdot p}{n-2\cdot p}\cdot x_1 - x_2$ hold. The resulting probability of victory is $\left(F\left(\frac{n-2\cdot p}{2\cdot p}\cdot\tilde{x}_h\right)+F(x_{1,2})\right)^2$. It is increasing in $\tilde{x}$, meaning that the subject should choose the highest possible value that fulfils the constraints of case 5, which again is the expected target value.

- **Cases 6 and 7:** One of the opponents chooses above the target value.

  In the final two cases, the relevant conditions are $\tilde{x}_h \geq \frac{2\cdot p}{(n-2\cdot p)}\cdot x_2 - x_1$ (Case 6) and $\tilde{x}_h \geq \frac{2\cdot p}{(n-2\cdot p)}\cdot x_1 - x_2$ (Case 7). This results in probabilities of victory of $F\left(\frac{n-2\cdot p}{2\cdot p}\cdot\tilde{x}_h\right)+F(x_1)$ (Case 6) and $F\left(\frac{n-2\cdot p}{2\cdot p}\cdot\tilde{x}_h\right)+F(x_2)$ (Case 7). Since $F\left(\frac{n-2\cdot p}{2\cdot p}\cdot\tilde{x}_h\right)$ is increasing in $\tilde{x}_h$, it is best to choose the highest possible number, which is the expected target value.

Assuming distributions of non-informed members as described above, we have shown that it is optimal in all possible cases to expect the opponents to choose the average number given in *history* and to give a best reply by taking also into account that the own number has an influence on the target value. This is applying the formula $\tilde{x}_h = \frac{p}{N-p}.E(x_1+x_2) = \frac{p}{n-p}\cdot(n-1)\cdot h_1$, which has been referred to already in case 1.