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Social Learning in Coordination Games: Does Status Matter?

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Abstract:

We report the results of experiments designed to test the impact of social status on learning in a coordination game.

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1. Introduction

We draw on several strands of research to examine the question of whether learning occurs more readily when information comes from a higher-status individual. This research is inspired in part by the network model of Bala and Goyal (1998), which explores the effect on equilibrium selection of a commonly observed agent, or "royal family". In their model the royal agent has higher status by virtue of being commonly observed, and can have a strong influence on the equilibrium that evolves in a society. We build on the considerable body of research on learning and equilibrium selection in coordination games; by incorporating a commonly observed agent into a standard coordination game experiment we provide a loose test of the ideas developed by Bala and Goyal. Finally, we go beyond common observability and incorporate recent work on the importance of social status in decision making by manipulating the status of our commonly observed agent, and compare the influence of "royalty", a higher-status agent, with a "commoner" lower-status agent. This approach allows us to distinguish between the coordinating impact of any commonly observed information, which can make one equilibrium a focal point (Schelling, 1960), and the influence of status. This work also touches on theories of the evolution of culture involving the transmission of information across generations by imitation of higher-status individuals (Gil-White and Henrich, 2001). If higher-status agents are more influential, this result supports the idea that attention is paid more readily to those agents.

2. Motivation

Social learning occurs when people observe and imitate others. Learning in a game is said to occur when an agent changes a strategy choice in response to new information. That information can come from the agent's own experience, where higher earnings mean that successful strategies are reinforced (e.g., Roth and Erev, 1998), or from changes in beliefs (or forecasts) about the play of others that arise from experience in the game cast more broadly (Camerer and Ho, 1998). Research in this area typically models agents as learning anonymously, in the sense that all agents are treated symmetrically in modeling the learning process. As such agents do not have identity.[1] However, recent theoretical research examines the potential importance of social identity – in particular, social status – in the creation and transmission of social norms or culture (Gil-White and Henrich, 2001; Boyd and Richerson, 2005; Richerson and Boyd, 2004.)

In models of learning with identity, agents differentiate between others, and can learn differently from different people. Several researchers have examined models where agents interact only with a subset of the population, their "neighbors". However other aspects of social structure have not received much attention. It is plausible that people put different weights on the actions of others, depending on connections or status differences, or known expertise. They might copy someone who is doing well, or conform to what others are doing. It is this category that we wish to focus on.

While there has been a great deal of experimental research on learning, very few researchers have examined interactions with heterogeneous agents or hierarchical

[1] We include in this category quantal response models, (McKelvey and Palfrey, 1995, 1998; Goeree and Holt, 1999; Anderson, Goeree, and Holt, 2001, 2002;), where agents forecast the distribution of actions and best-respond to that. In a variation on this approach, Nyarko and Schotter (2002) elicit beliefs, rather than assume a particular belief-formation rule.

structure. Offermans and Sonnemans (1998) structure their experiment so that subjects observe the success of others. They find that people learn both from experience and by imitating successful others; subjects imitate the forecast of successful players when given the opportunity. Brown (1994) notes the importance of reference points in determining the path of the learning process; though he does not consider this case, such reference points could be provided by the observed decisions or advice of higher-status players. Schotter and Sopher (2003) examine social learning when members of one generation can give advice to a subsequent generation, and find strong evidence that word of mouth learning affects the creation of social conventions.

Local interaction learning models explore equilibria of systems with boundedly rational agents, but these models seldom explore the potential role of status differences. An exception is the work of Bala and Goyal (1998), who present a model of learning from neighbors where agents observe their close neighbors, but also observe "the royal family", a small set of agents observed by everyone. Because everyone sees them, they are unduly influential, for better or worse. The royal family can improve coordination, but may cause the society to veer to a suboptimal equilibrium. In the context of technology adoption, they show that the structure of information flows can lock in an inferior technology.

A great deal of experimental research has explored the problem of equilibrium selection in coordination games, with particular emphasis on the role of communication and learning in repeated interaction in solving coordination problems. (See Jack Ochs, 1995, for a survey.) A regularity that emerges from this work is that subjects pay attention to payoffs that are associated with out-of-equilibrium play, and are particularly sensitive to the penalties for deviating from an equilibrium. For example, subjects appear to be risk averse and choose lower-payoff risk-dominant equilibria over higher-payoff, riskier equilibria. Likewise, learning tends to lead subjects away from Pareto superior but risky equilibria; the norm of behavior that emerges after repeated interaction is more frequently the less-risky but Pareto inferior equilibrium, as shown in Van Huyck, et al (1990) and (1991). Cooper et al (1990) explore the causes of this behavior using a set of carefully-constructed games. We draw from their work for our own design, as explained below.

Cheap talk in the form of nonbinding pregame communication can help facilitate coordination on the higher-payoff equilibrium. A commonly-observed signal can also facilitate coordination. Brandts and McLeod (1995) conduct experiments using a game with payoffs similar to Cooper, et al, where a commonly observed signal is used in an attempt to manipulate the equilibrium that is selected. The signal is in the form of a recommendation read aloud by the experimenter about which strategy to play. Thus their manipulation is not only a signal of what equilibrium to play, but one that comes directly from the experimenter in the form of a recommendation. Nevertheless, subjects often ignore the signal, especially when the recommendation is to pay the risky, efficient equilibrium. Eckel and Wilson (2000) manipulate status by creating a commonly-observed, simulated player, and show that a commonly-observed signal substantially increases the play of the efficient equilibrium.

The role of social status in decision making attracted the attention of sociologists starting in the 1960s, and have documented the influence of higher-status participants on the decisions of others in games and other situations. (See Webster and Fosci, 1988 for an overview). Beginning with Becker's explorations of discrimination (1971) and professional distinction (1974), economists have noted the importance of status and status competition. Recent experimental research explores the impact of social hierarchies in a variety of settings. Using artificially-induced status differences, Ball, et al, 2001, show that higher status participants earn more in a market setting, and Ball and Eckel, (1996, 1998) find that subjects offer more to higher-status counterparts in an ultimatum game experiment. Kumru and Vesterlund (2005) adopt a similar manipulation of status and show that in a sequential voluntary contribution game, if a higher-status person moves first, contributions are higher than if a lower status person leads. Duffy and Kornienko (2005) show a dramatic effect of status competition on giving in dictator games. None of these studies has directly addressed the issue of the impact of status on influence that was raised in the original sociological research.

Following Bala and Goyal, our experimental setup allows all participants to observe a higher or lower status agent. The experiment tests the effect of the status of a commonly-observed agent on the choice of strategy in a coordination game. We adopt one of the 3x3 coordination games developed by Cooper, DeJong, Forsythe, and Ross (1990). Status is manipulated experimentally following Ball, et al. (2000), so that the commonly observed agent is either higher or lower status than the other players. Our results show that observing a "royal" player can affect the behavior of agents, but observing "commoner" player does not.

3. Experimental design and procedure

We designed experiments to test the influence of social status on social learning and coordination success in simple 3x3 coordination game. The game used in the experiment is shown in Table 1, and replicates one of the coordination games (game 4) used in the study of equilibrium selection by Cooper, et al. (1990). The game has several interesting properties. First, there are two equilibria in the game, given by the strategy pairs (1,1) and (2,2). The second equilibrium carries higher payoffs for both players. However, the equilibrium (1,1) is less risky, in the sense that if the player puts any prior probability on his opponent choosing the dominated strategy 3, choosing strategy 1 avoids the possibility of receiving zero. The choice of a strategy in these games is akin to the choice of a product or technology by an individual, where one's choice is more valuable when others choose the same product. While coordination success implies individuals settling on either of the two equilibria, we are especially interested in whether individuals coordinate on the efficient (Pareto superior), but risk dominated, equilibrium (2,2,).

Table 1 here

Table 2 shows the experimental design and the number of subjects in each treatment. The design consists of 4 treatment combinations in a 2x2 factorial design. The first factor is the status of the commonly-observed agent, which is held constant within a session. The commonly observed agent's status is manipulated to be either high or low, as explained below. The second factor is whether the commonly-observed agent is a simulated player or a real player. Each session consisted of two 15-period phases. Subjects were told (truthfully) that there was a 50 percent chance that, in any phase, the commonly-observed subject was a simulated player. Our design was counter-balanced so that in half the sessions subjects observed the simulated player in the first phase and in the remaining sessions the simulated player was in the second phase.

Table 2 here

Subjects are recruited in groups of 6-12, and participate in a sequence of two-person 3x3 coordination games via a computer interface. Subjects play two treatments in each session in two phases, each consisting of 15 periods, and are randomly rematched (with replacement) in each period. One game in each phase is chosen for payment (the subject chooses a card on the computer screen to determine the period.) The game is presented as a matrix, but all players are row players -- at least from their perspective.

The simulated agent was always labeled “A.” Player A’s choice was always reported as being Column 2 – in an attempt to get subjects to coordinate on the Pareto superior equilibrium. We introduced the simulated player in order to have at least one phase where the commonly-observed signal was consistent over time. This gave us the best chance of detecting the influence of status. To ensure that payoffs were not affected by simulated choices, the simulated player never played against any other subjects; all subjects in the experiment were randomly paired with one another. Real players (non-simulated) whose past move was commonly observed were always labeled as “C.” Whatever that player chose in the prior period was then shown to all subjects. Thus player C frequently produced inconsistent signals, making the impact harder to detect.

All subjects were told in the instructions that they would see what player A/C did in the prior period. Subjects also were reminded of their own choice in the previous period. All of this information was displayed on the computer screen as they were making their choice for the next period.

In any given session, only one status manipulation was used, and subjects were fully informed in the instructions. High or low status was induced by using two "generalized knowledge" quizzes. Prior to beginning the experiment subjects took the first quiz. To get the subjects to take the quiz seriously they were paid a small amount (10 francs, or about 9 cents) per correct answer. When all quizzes were completed, subjects were informed of their own score on the quiz, and the score of the commonly observed player. Depending on the manipulation, either the highest or lowest score was displayed. In the high status manipulation either the simulated "player A" was announced as making the highest score or the highest scoring real "player C" was announced as making the highest score. In the real-player phase, Player C was informed that he was the high scorer, that he was reassigned to be Player C and that his play would be observed. In the low status manipulation, lowest scoring player was similarly informed. Once subjects completed the first phase of the experiment (the first 15 periods) they took a second quiz. Again the subjects' quizzes were machine graded and they were told their score.[2] Once everyone finished, again a high or low status player was identified in a fashion similar to that noted above.

V. Procedure

A total of 92 subjects were recruited from the student populations at Rice University (8 sessions) and University of Texas, Dallas (4 sessions). Subjects were recruited from subject pools built by the authors and through email solicitations. The recruitment pools draw broadly from the student populations; participants were initially

[2] The average score on quiz 1 was 10.1 with a standard deviation of 1.9. The average score on quiz 2 was 9.5 with a standard deviation of 2.3.

recruited from introductory social science courses. While some subjects previously had participated in decision making experiments, none were familiar with this particular design. The sex composition of subjects was skewed toward males (66.3 percent). When recruited, subjects were told they would be given a show-up fee of $5 and that they could earn additional cash during the course of the experiment. Subjects were not told how much they could earn, and if pressed, were told they could make more than the minimum wage for less than 60 minutes of their time in the laboratory.

When subjects showed up at the laboratory they were randomly assigned to a computer. Subjects were seated at computer carrels that prevented them from seeing one another's screen or communicating with one another. All experiments conducted by a female experimenter who read a standard protocol, cautioning subjects not to speak with one another during the course of the experiment and to direct all questions to the experimenter. Subjects then proceeded to self-paced instructions given at their computer screen. The instructions were modified slightly from those given in the appendix for Cooper et al. (1990). At various places in the instructions subjects were tested for comprehension before being allowed to continue. In a post experimental questionnaire, 86 of 92 subjects agreed that the instructions were clear.

Subjects faced a 3x3 matrix and were told to choose an action. The matrix looked like a standard game matrix in normal form, with the subject's choices labeled as row numbers and the counterpart's choices as column numbers. While van Hyck et al. (1997), note that there are strategic and distributional consequences to labeling players as row and column players, we avoided this by assigning all players as row players. The computer adjusted payoffs in each of the cells so that all players viewed themselves as row players and their counterparts as column players. Subjects were told that they would be randomly assigned a new counterpart in each period and that they might face the same person more than once. As few as 6 subjects and as many as 12 subjects were used in each experimental session; no significant difference was observed across sessions due to the number of players. Players were all assigned an identity at the outset. Subjects that were not part of the treatment were told that they were "Player B". Players who scored high or low on the quizzes (depending on the treatment) were told they were reassigned to be "Player C." No mention was made of the identity of their counterpart in any period. Consequently, subjects had no basis for knowing with whom they were matched at any period.

Once subjects made a row choice, they were told to wait. After everyone made a decision, the outcome for that period was displayed. All earnings were given in experimental francs and subjects were told that the official rate of exchange was 90 francs to the dollar. Subjects were told they would participate in two phases of the experiment, with each phase lasting 15 periods. Subjects were told in advance that they would only be paid for one period in each phase of the experiment. At the end of a phase, subjects were presented with 15 electronic cards and asked to pick one. Once selected, the card was flipped over and displayed the period that was randomly chosen for payment. At the conclusion of the experiment subjects were paid in cash and in private. On average subjects earned $14.89 for 45 minutes in the lab.

3. Results

Table 3 indicates the percentage of time each strategy was played in response to each possible signal from the commonly-observed player, separating out real from simulated agents. Bold entries indicate when the high-status moved was copied by the other players. For example, in the high status treatment, when the real royal player played 1, the next period 72.45% of the subjects chose strategy 1; when the signal was 2, 44.78% played 2; when the signal was 3, 17.55% chose 3. When the simulated player signaled 2, 38.1% played 2. The effect of status can be observed by comparing the bold cells in the upper part of the table with the corresponding cells in the bottom part. In every case, a higher-status signal is more likely to be followed. The table also indicates that in the high status conditions subjects are more successful on average in moving to the efficient equilibrium: the entries for "Chose 2" are higher for the high-status manipulation.

Table 3 about here

These findings are partly borne out in figures 1a-d which plot the distribution of row choices, by treatment and period. Clearly in Figure 1a, in which there is a high status real player sending a common signal in the first phase, it is difficult to coordinate. In part this is because those high status players are not consistently choosing the same strategy. However, when the clear signal from the high status simulated player arrives, then subjects shift from row 1 to row 2 play. But a clear signal is no guarantee, as is evident from figure 1b. Subjects quickly get on a path in which they choose row 1, despite the common signal by the simulated player of row 2. By contrast in both low status treatments there seems to be little attention paid to the "commoner" player, regardless of whether that player is real or simulated.

Figures 1a,b,c,d about here

To better understand the effect of the signals, we estimate a random-effects logit model to calculate the effect of the commonly-observed agent on subjects' choices while controlling for their individual histories[3]. We use a logit model because we are primarily interested in whether the existence of a commonly observed agent can move subjects to chose strategy 2, the strategy that would lead to the more efficient equilibrium, versus the other two available strategies. Since the best response to a play of strategy 1 or of strategy 3 is to play 1, and we see that response is robust in the aggregate data, the critical distinction to us is whether choices can be moved away from 1 toward 2. We use a random effects logit because an individual's strategic play from one period to the next is not independent. All of our models indicate that there are individual effects that should be accounted for in the estimates. We also estimate the model separately for the high and low status treatments.

The dependent variable in our model is whether the subject chooses a strategy that would lead to the efficient equilibrium (row 2). From other work (Cooper et al., 1990; Eckel and Wilson, 2000) we know that, without guidance, subjects usually choose strategy 1. Brants and McLeod (1995) show that even an announced recommendation to play 2 is frequently unsuccessful in inducing subjects to play 2. There is a handful of subjects who choose strategy 3, which is strictly dominated. This latter number is small and creates problems with estimating all three categories under a multinomial logit. Given that our main concern is with the effect of status for successfully coordinating on the pareto superior outcome, will view this as an appropriate approach to modeling the dependent variable.

[3] While multinomial logit might seem a more obvious choice, inference in the random effects multinomial logit model is complicated because it requires evaluation of multi-dimensional integrals. Presumably for this reason, no statistical software packages allow for direct estimation of the model.

Each session consisted of two fifteen-period phases. To partly model the possibility of learning over time, we include a variable that is a simple count for the period in which the subject is making a decision. To model the data as an interrupted time series, a second variable is introduced setting the first 15 periods to zero and then building a counter beginning from 1 for periods 16 through 30. This produces an estimate of a new slope for the second phase of the experiment.[4] To account for a shift in the intercept for the two-phase treatment a dummy variable is included that is 0 for the first 15 periods and 1 thereafter.

The next variable controls for whether the subjects saw a real player's move during the period. We do so partly to account for the noisy signal that real players send and to test for the possibility of an independent effect. Our primary focus is with the impact of this commonly observed signal. If status makes a difference it should matter in the high status condition and not in the low status condition.

We also control for the immediate past experience on the part of the subject by including variables that capture the information observed by the subject at each time period. First we do this by calculating whether the prior move by the subject was strategy 2. This allows us to control for any "stickiness" in strategy choices. We also include a variable that indicates whether the previous partner chose strategy 2. This should provide a reinforcing effect for choosing the efficient equilibrium.

The results for this model are given in Table 4. We report separate models for the high status and low status treatments. We find no period effects for either model, although there is a large and significant effect for the second phase in the high status

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[4] This specification imposes a linear structure on the subjects' learning. Including the log of the period number to model nonlinearity did not change the estimates.

treatment. We find no independent effect for the real player across either status treatment. This gives us some confidence that subjects were not responding to something about the type of player they thought they were facing. We collected data at the end of the experiment asking subjects whether they thought that Player A or Player C were real or a simulated players. Subjects did little better than chance in guessing their counterpart's type. When we included their beliefs in the models Table 4 we found no independent effects for those beliefs.

Table 4 about here

The strongest support for the value of status comes from the coefficient on "Royal Player Moved 2" (shaded). A signal of "2" by the commonly observed player has a positive impact on the play of 2 only in the high status treatment. While the sign is in the same direction for the low status condition, the parameter estimate is insignificant. We regard this as support for the idea that higher-status people are more influential than lower-status people. Greater attention is paid to a signal that comes from a higher-status individual.

We also find that immediate prior experience matters. When a subject previously chooses strategy 2 they are more likely to do so again. The same is true when their prior partner has chosen strategy 2. Both are reinforcing.

A number of alternative specifications were tested. We tried a number of different ways of modeling the learning process by subjects. We also controlled for characteristics of the individual: there is no difference between women and men, and there is no effect of grade point average (which we used as a proxy for understanding the game structure). We examined the percentage of times strategy 2 was chosen by the "royal player" as a way of controlling for path dependence in the game. None of these alternatives added significant explanatory power to the model or affected the direction or significance of the "royal" effect.

To better understand the pattern of behavior, we next examine the consistency of the moves made by the real commonly-observed players. To illustrate this point, figures 2a and 2b plot the moves by real players through the course of each session. In the first condition, with a high status royal player in the first 15 periods, it is clear that there were few consistent signals. Royal players 1 and 2 in Figure 1a both started by choosing strategy 3. Quickly they switched to strategy 1. Meanwhile royal player 3 oscillated in a pattern of playing all strategies. The patterns were even more peculiar for the second condition in which the high status royal player was observed in the second phase of the experiment. Again, in two sessions royal players (5 and 6) started by playing strategy 3. Quickly player 5 shifted to strategy 1 and stuck with it. Player 6 chose to stay with strategy 3 throughout, thereby leading the other subjects to play best response – strategy 1. Only royal player 5 chose strategy 2 with any consistency, but vacillated between 1 and 2 in the first 8 of those periods.

Figure 2 about here

The story is similar for the low status "commoner" players in the last two conditions. Figures 3a and 3b illustrate what was observed about the play of each of the low status players. In figure 3a we see that player 8 consistently played strategy 1 (with one try at strategy 3). The other two commoner players bounced around a good deal. In the last condition, where the low status player was observed in the second phase, only player 12 was consistent, always choosing strategy1. The other two players were variable in their choices, providing little consistent signal.

Figure 3 about here

5. Conclusion:

In this experiment we examine the effect of social status on social learning in a simple coordination game. Subjects observe a common signal consisting of the previous period play by either a real or a simulated player. These commonly observed players have either high status (royal players) or low status (commoner players). We find that a high status, commonly-observed agent can result in a higher proportion of subjects coordinating on an efficient equilibrium.

In previous experiments with similar games, players that observe only the decisions of their counterparts (neighbors) tend to evolve towards play of an inferior but lower-risk equilibrium (Cooper et al 1990, 1991). Brandts and McLeod (1995) found that an intervention consisting of an announced recommendation to play a strategy that would lead to the efficient (but risky) equilibrium could (but did not necessarily) move subjects toward greater frequency of efficient play. A recommendation to play the inefficient but risk-dominant equilibrium, on the other hand, was very effective. We consider their manipulation to be a very strong signal. The information about what to play comes to subjects in the form of a verbal announcement by the experimenter. Since the experimenter is inherently a high status person, this signal should be more likely to be followed than if it came from someone else. That it was not always effective suggests the difficulty of encouraging efficient play when a less-risky alternative is available.

In our own previous work (Eckel and Wilson, 2000), we conduct an experiment similar to Brandts and McLeod. In our paper, subjects observe the previous play of a simulated commonly-observed player who chooses either (1) the safe, inefficient strategy; (2) the risky, efficient strategy; or (3) the dominated but higher-payoff strategy. We find that, relative to a no-information control condition, a signal to play (1) is readily followed, a signal to play (2) significantly increases the play of (2), but a signal of (3) is never followed but rather induces subjects to play (1), the best response to (3). The shortcomings of that paper are that the commonly observed agent was introduced in a deceptive way, and that because all signals were from a high-status agent, we could not tell if the results were due simply to making one equilibrium more focal.

In this experiment we avoid all deception by introducing a 50% chance of facing a real or a simulated player: subjects are told that the commonly observed agent may be simulated. In response to debriefing questions, subjects are unable to distinguish the simulated player. We focus only on a recommendation to play (2), since the effects of recommending (1) or (3) are known. Our findings imply that the play of a commonly observed agent does not simply make one equilibrium more salient or focal, as Schelling (1967) would predict. Instead that play is effective in influencing others only if the observed agent has high status.

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Table 1: Game Matrices
All payoffs are given in experimental francs; 1 franc = $.09

Game 4.

	Col. 1	**Col. 2**	**Col. 3**
Row 1	350,350	350,250	700,0
Row 2	250,350	550,550	0,0
Row 3	0,700	0,0	600,600

Table 2: Experimental Design
(Number of subjects
Number of sessions)

	High-status	**Low-Status**
Real player moves first; Robot moves second	22 Ss (*3*)	24 Ss (*3*)
Robot moves first; Real player moves second	22 Ss (*3*)	24 Ss (*3*)

Table 3. Percentage of subjects choosing a strategy conditional on the commonly observed signal by the "royal" player. The number of choices is given in parentheses.

High Status				
	Real – Move 1	Real – Move 2	Real – Move 3	Robot – Move 2
Chose 1	**72.45% (213)**	47.76% (64)	56.91% (107)	54.06% (333)
Chose 2	23.47% (69)	**44.78% (60)**	25.53% (48)	**38.31% (236)**
Chose 3	4.08% (12)	7.46% (10)	**17.55% (33)**	7.63% (47)
Total	100% (294)	100% (134)	100% (188)	100% (616)

Low Status				
	Real – Move 1	Real – Move 2	Real – Move 3	Robot – Move 2
Chose 1	**69.40% (322)**	54.17% (78)	65.62% (42)	66.37% (446)
Chose 2	20.91% (97)	**37.50% (54)**	18.75% (12)	**27.83% (187)**
Chose 3	9.70% (45)	8.33% (12)	**15.62% (10)**	5.80% (39)
Total	100% (464)	100% (144)	100% (64)	100% (672)

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Table 4
Coefficients, Standard Errors in Parentheses, Significance Levels in Italics

	High Status	*Low Status*
Intercept	**-2.973** (.457) *p<.001*	**-2.266** (.420) *p<.001*
Time (1 … 30)	.003 (.028) *p=.916*	-.005 (.025) *p=.849*
Time 2nd Phase (1 … 15)	-.040 (.038) *p=.299*	-.031 (.036) *p=.394*
Phase 2 Dummy	**1.186** (.334) *p=.000*	.266 (.306) *p=.383*
Royal Player 1=Real 0=Robot	.218 (.284) *p=.443*	.015 (.251) *p=954*
Royal Player Moved 2	**.658** (.309) *p=.033*	.303 (.272) *p=.266*
Previous Move=2	**1.132** (.182) *p<.001*	**.898** (.163) *p<.001*
Prior Partner's Move=2	**1.389** (.180) *p<.001*	**1.249** (.161) *p<.001*
	LLF=--551.78	LLF=-632.54

Figure 1a. Treatment with a high status real player in periods 1-15 and a high status simulated player in periods 16-30. The stacked bar chart indicates the percentage of subjects choosing the row strategy.

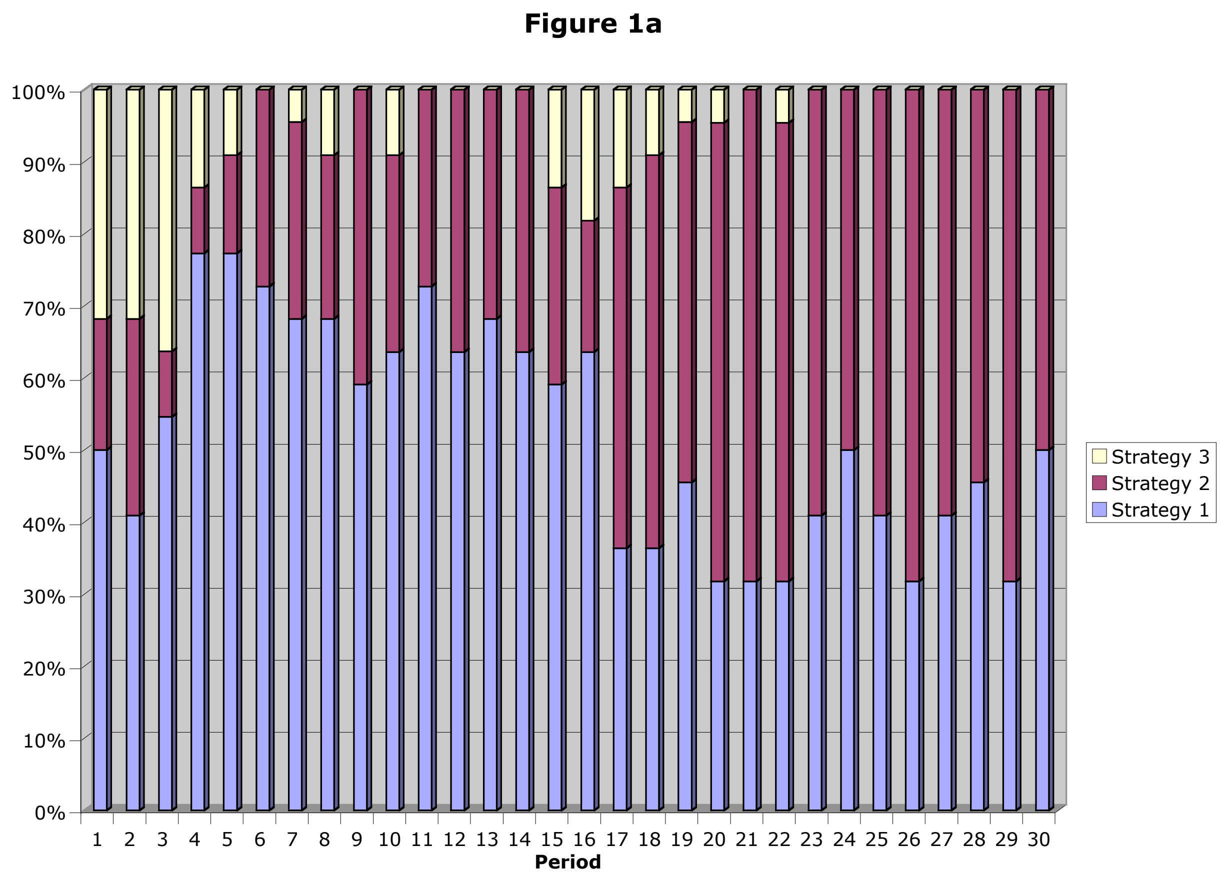

Figure 1b. Treatment with a high status simulated player in periods 1-15 and a high status real player in periods 16-30. The stacked bar chart indicates the percentage of subjects choosing the row strategy.

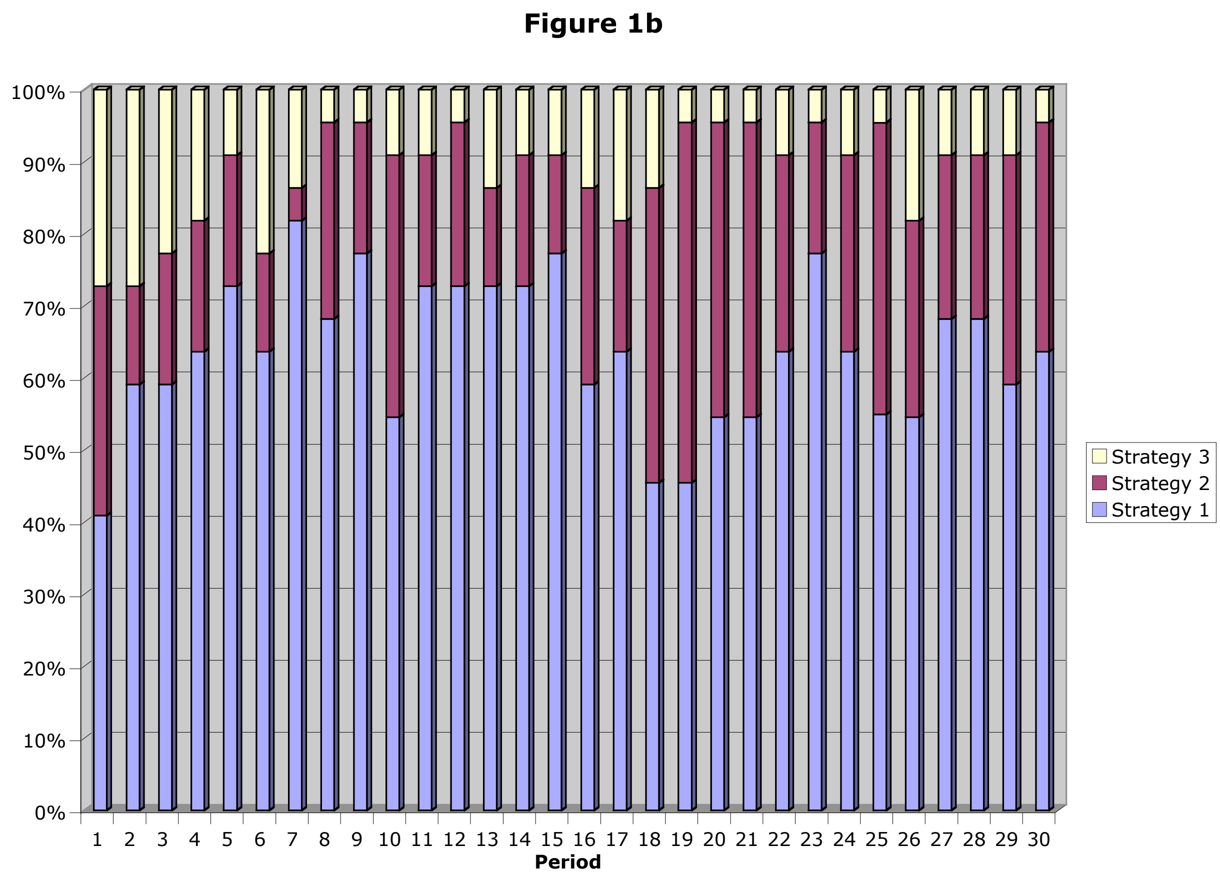

Figure 1c. Treatment with a low status real player in periods 1-15 and a low status simulated player in periods 16-30. The stacked bar chart indicates the percentage of subjects choosing the row strategy.

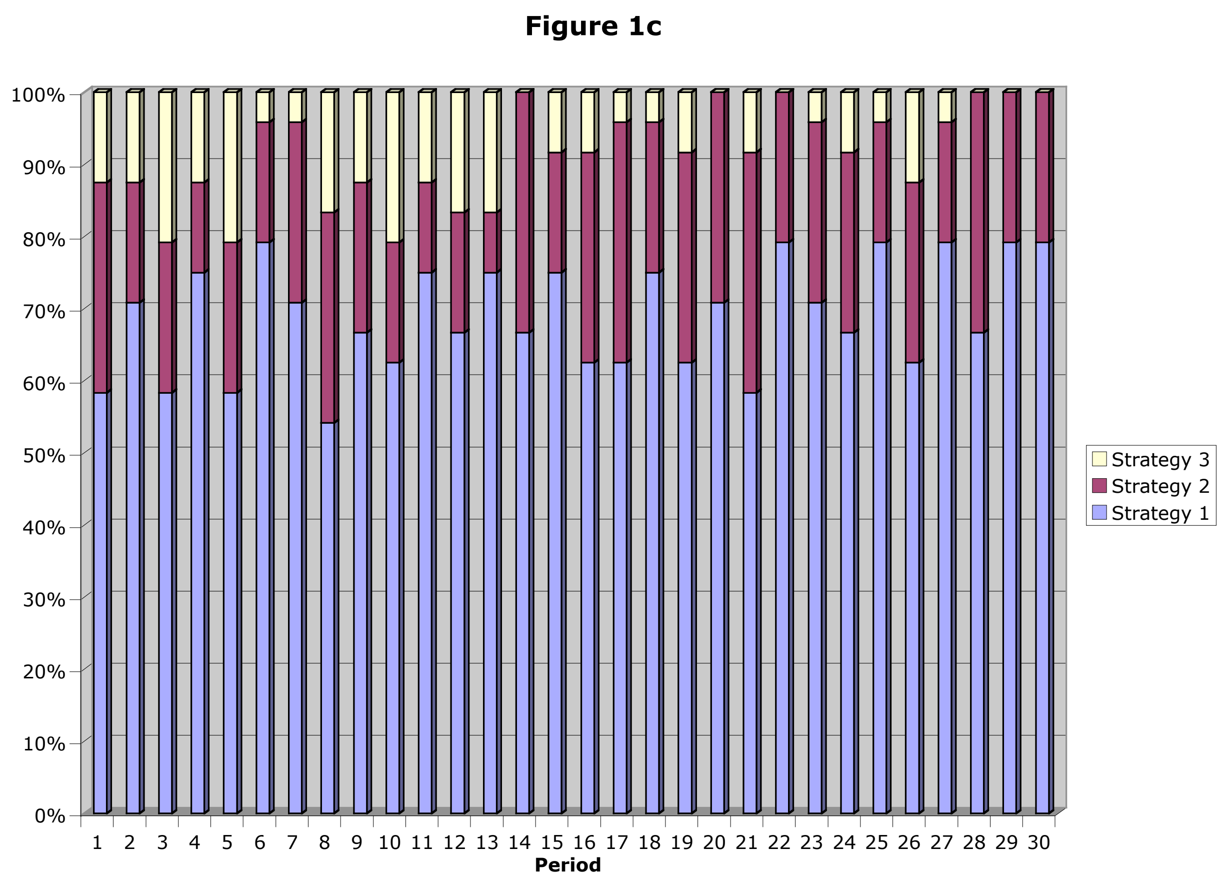

Figure 1d. Treatment with a low status simulated player in periods 1-15 and a low status real player in periods 16-30. The stacked bar chart indicates the percentage of subjects choosing the row strategy.

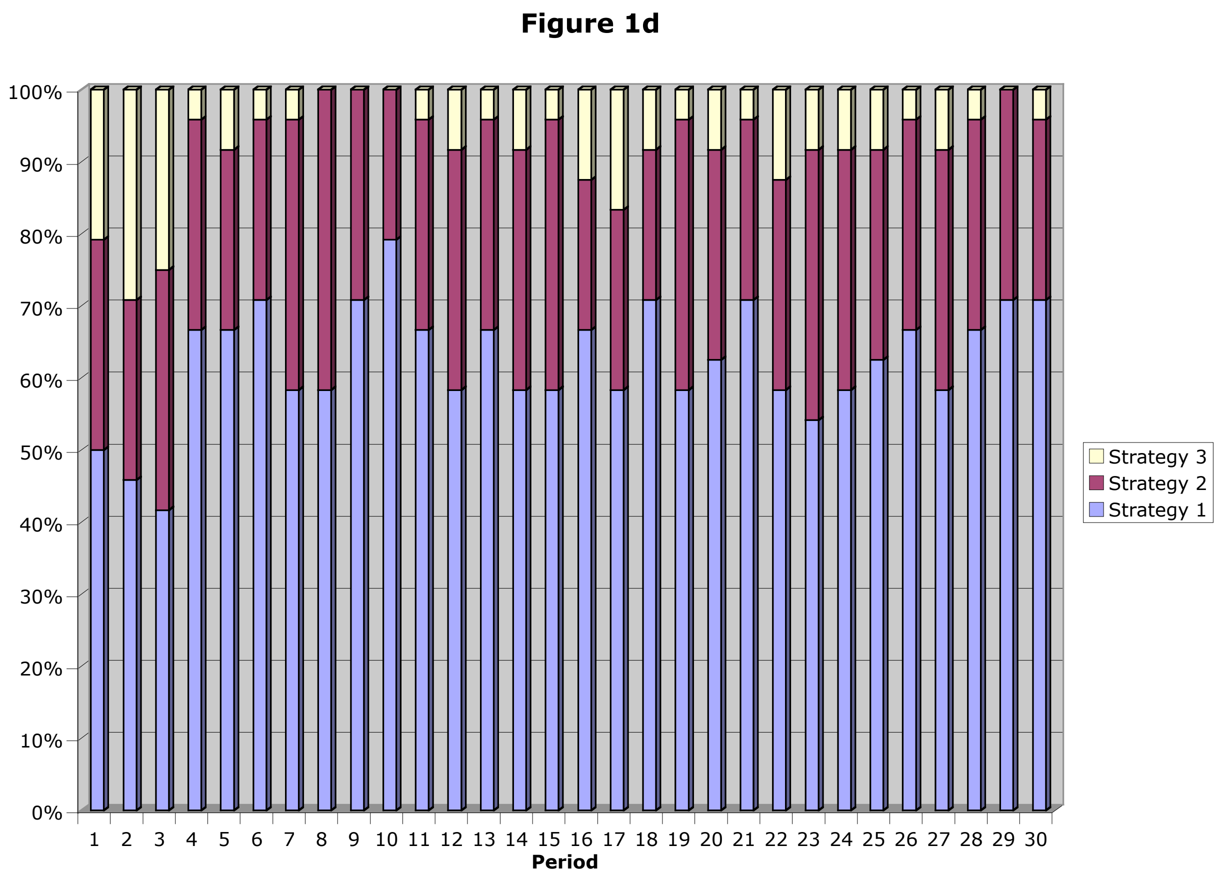

Figure 2a. High status “royal” player in the first 15 periods.

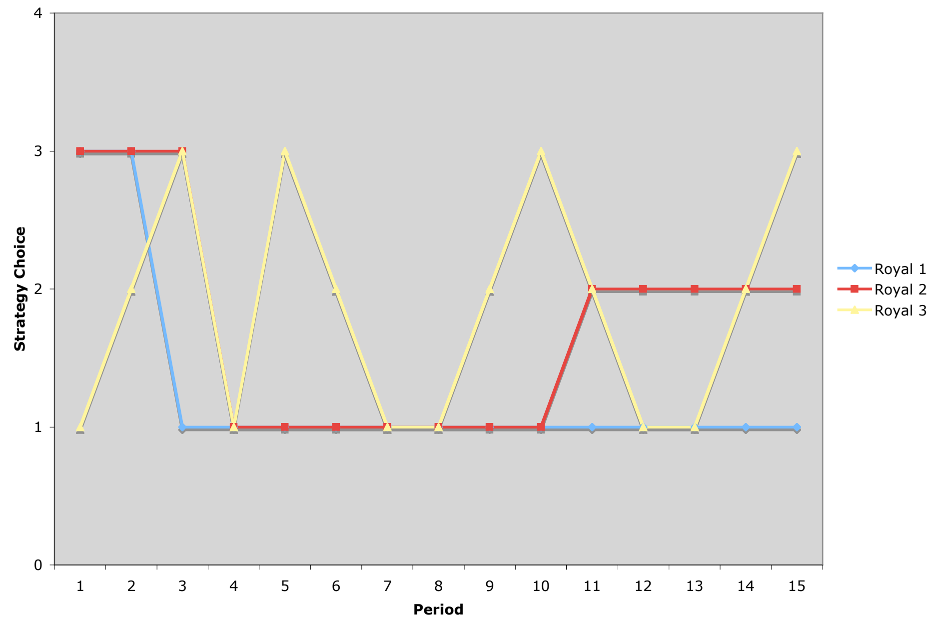

Figure 2b. High status “royal” player in the second 15 periods.

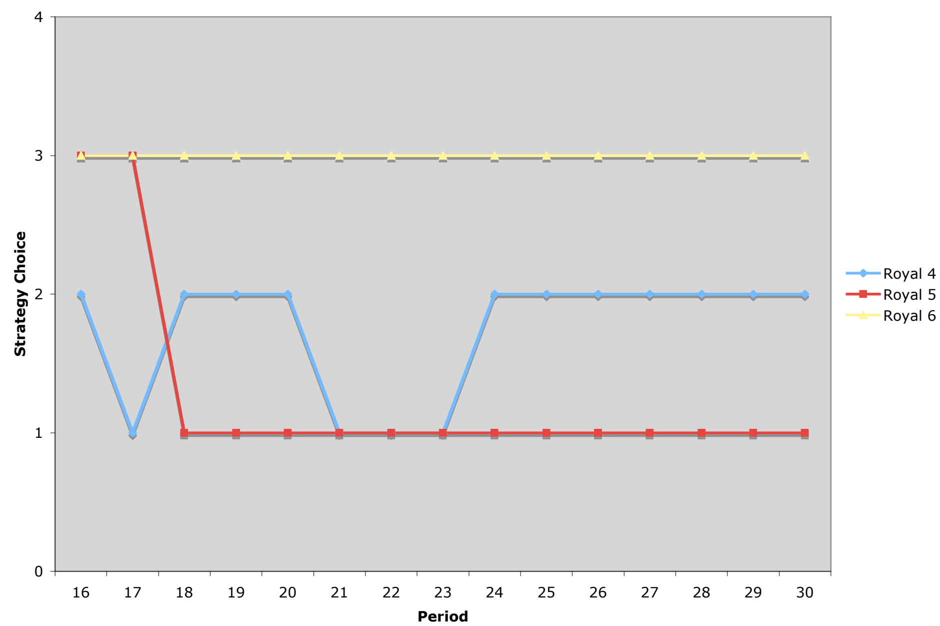

Figure 3a. Low status “commoner” player in the first 15 periods.

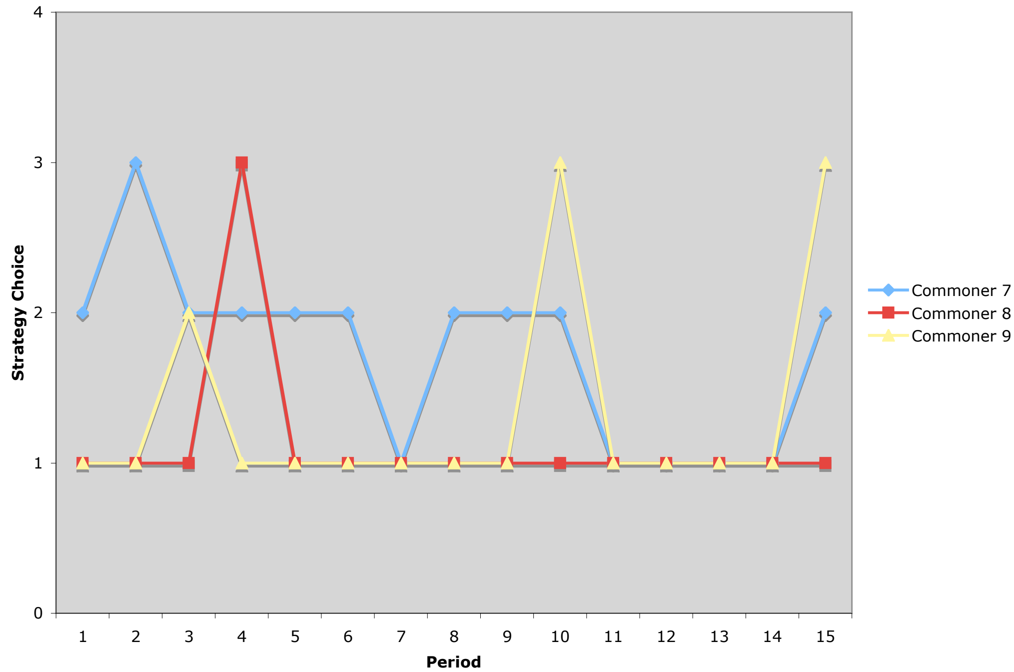

Figure 3b. Low status “commoner” player in the first 15 periods.