Exp Econ (2007) 10: 317–329
DOI 10.1007/s10683-007-9185-x

Social learning in coordination games: does status matter?

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Published online: 9 August 2007

Abstract We report the results of experiments designed to test the impact of social status on learning in a coordination game. In the experiment, all subjects observe the play of an agent who either has high status or low status. In one treatment the agent is another player in the game; in the other the agent is a simulated player. Status is assigned within the experiment based on answers to a trivia quiz. The coordination game has two equilibria: one is payoff-dominant but risky, and the other is risk-dominant. The latter is most commonly chosen in experiments where there is no coordination device. We find that a commonly observed agent enhances coordination on the payoff-dominant equilibrium more often when the agent has high status.

Keywords Coordination game · Payoff dominance · Risk dominance · Status

JEL Classification C92 · C7 · Z13

1 Introduction

We ask whether learning occurs more readily when information comes from a high-status rather than a low-status individual. This question has important implications for equilibrium selection and, in particular, the problem of coordination failure. This research is inspired 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 high status by virtue of being commonly

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observed, and can have a strong influence on the equilibrium that others select. 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. We manipulate the status of our commonly observed agent, and compare the influence of a high-status agent with a low-status agent. This allows us to sort between the coordinating effect of any commonly observed information and the independent effect of status.

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 1999). Research in this area typically models agents as learning anonymously, in the sense that all agents are treated symmetrically when modeling the learning process. As such agents do not have identity.[1] However, recent theoretical research examines the importance of social identity—in particular, social status—in the creation and transmission of social norms or culture (Gil-White and Henrich 2001; Richerson and Boyd 2004).

In models of learning with identity, agents differentiate among others, and can choose to attend differently to different people, effectively deciding from whom to learn. Several researchers have examined models where agents interact only with a subset of the population, their "neighbors". However, other aspects of social structure have received less attention. It is plausible that people put different weights on the actions of others, as a function of status differences or known expertise. They might copy someone who is doing well, or conform to what others are doing. We focus on status differences.

While there has been a great deal of experimental research on learning, very few researchers examine interactions with heterogeneous agents or hierarchical structure. Offerman and Sonnemans (1998) structure an experiment so that subjects observe the success of others and 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. Although he does not consider it, 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.

[1] We include in this category quantal response models (McKelvey and Palfrey 1995; Anderson et al., 2001, 2002), where agents forecast the distribution of actions and best-respond to that.

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), which presents a model of learning from neighbors where agents observe their close neighbors, but also observe "the royal family", who are defined as 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, Bala and Goyal 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 for solving coordination problems. (See 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 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, 1991). Cooper et al. (1990) explores the causes of this behavior using a set of carefully-constructed games. We draw from their work for our own experimental design.

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 MacLeod (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. The experimenter, holding a privileged position (and the cash), automatically holds high status in the context of the experiment. Not surprisingly the experimenter's recommendation serves as a powerful focal point for coordination. Eckel and Wilson (2000) directly manipulate status by creating a commonly-observed, simulated player. They show that a commonly-observed signal substantially increases the play of the Pareto superior, but risk dominated equilibrium relative to the control condition. Neither study looks explicitly at low status signals. Our interest is in whether a higher-status signal is followed more often than a low-status signal.

The role of social status in decision making attracted the attention of sociologists starting in the 1960s, and they have documented the influence of higher-status participants on the decisions of others in a variety of games. (See Webster and Foschi 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 demonstrates 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 the ultimatum game. Duffy and Kornienko (2005) show a dramatic effect of status competition on giving in dictator games. Kumru and Vesterlund (2005) are the first to show the importance of status for leadership and influence. They adopt a manipulation of status similar to Ball et al., 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. Leadership is enhanced when the leader also has high status.

Following Bala and Goyal, our experimental design allows all participants to observe an agent who either has higher or lower status. The experiment tests the effect of the status of a commonly-observed agent on the strategic choices by others in a coordination game. We adopt one of the 3×3 coordination games developed by Cooper et al. (1990). Status is manipulated experimentally following Ball et al. (2001), so that the commonly observed agent has 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. This is in contrast to the notion that Bala and Goyal have in their model: their "royal family" has higher status by the very fact of being observed. We show that common observation of a lower-status royal is less powerful than if the royal has higher status, suggesting common observation alone may not be sufficient to guarantee influence.

3 Experimental design and 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 that draw broadly from the student populations. 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. On average subjects earned $14.89 for 45 minutes in the laboratory.

When subjects arrived at the laboratory they were randomly assigned to a computer carrel; the carrel prevented them from seeing one another's screen or communicating with one another. All experiments were 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 proceeded to self-paced instructions given at their computer screen. The instructions were modified slightly from those given in the Appendix of 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 3×3 matrix and were told to choose an action. The matrix looked like a game in normal form, with the subject's choices labeled as row numbers and the counterpart's choices as column numbers. Van Huyck et al. (1997) note that there are strategic and distributional consequences to labeling players as row and column players and we avoid this by assigning everyone as row players. Payoffs were adjusted

Table 1 Game matrices. 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

All payoffs are given in experimental francs; 1 franc = $.09

Table 2 Experimental design

Order	High-status	Low-Status
AB	22 Subjects (3 sessions)	24 Subjects (3 sessions)
BA	22 Subjects (3 sessions)	24 Subjects (3 sessions)

A: Real commonly-observed player (15 periods)

B: Simulated commonly-observed player (15 periods)

so that all players viewed themselves as row players and their counterparts as column players.

The payoff structure 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. We are interested in whether individuals coordinate on the efficient (Pareto superior), but risk dominated, equilibrium given by strategy pair (2, 2). Cooper et al. (1990) and Eckel and Wilson (2000) show that in this game subjects fail to coordinate on the efficient equilibrium.

Table 2 shows the experimental design and the number of subjects in each treatment. The design consists of 4 treatment combinations in a 2×2 factorial design.[2] The first factor manipulates the status of the commonly-observed agent and is held constant within a session. The second factor manipulates whether the commonly-observed agent is a simulated or a real player. A session has two 15-period phases. Subjects were told (truthfully) that there was a 50 percent chance that in one phase the commonly-observed subject is a simulated player. The 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.

The simulated agent's choice was always reported as "Column 2." This was chosen to see if subjects could be induced to coordinate on the risky, Pareto-superior equilibrium. The simulated player is introduced in order to ensure that there is one phase of the experiment where the commonly-observed agent's signal is consistently

[2]In this paper we do not include a separate control treatment without a commonly observed agent. The result of the control condition is well-established: Cooper et al. (1990) and Eckel and Wilson (2000) show that groups without a commonly observed agent coordinate almost uniformly on the risk-dominant, inefficient equilibrium.

"strategy 2". To guarantee that payoffs were not affected by simulated choices, the simulated player never played against any other subject: all subjects were told they would only be matched with others in the room, and not with the simulated player. All subjects in the experiment were randomly re-matched in each period.

In each session only one status manipulation was used. High or low status was induced by using two "generalized knowledge" quizzes. At the start of the experiment subjects took Quiz 1 which had 15 questions. Subjects were paid 10 experimental francs (about 9 cents) per correct answer. When everyone completed the quiz subjects were informed of their own score and the score of the commonly-observed player. In the high status manipulation one player was announced as the subject who made the highest score.[3] In the low status manipulation, the lowest scoring player was similarly announced as having scored lowest. Everyone was told that they would see what the common player chose in the preceding period. In addition to seeing the commonly-observed agent's choice, subjects also were reminded of their own choice in the prior period. After the first phase of the experiment (the first 15 periods) subjects took Quiz 2. Again the subjects' quizzes were graded and they were told their scores.[4] Once everyone finished, again a high or low status player was identified in a fashion similar to that noted above.

Subjects were randomly assigned a new counterpart in each period. As few as 6 subjects and as many as 12 subjects were in each experimental session; no significant difference was observed across sessions due to the number of players. 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, and 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 an array of 15 electronic cards, face down, 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.

4 Results

Table 3 presents the percentage of time each strategy was played subsequent to the choice by the commonly-observed player. Real and simulated commonly-observed agents are shown separately. Bold entries denote when the high/low-status move was

[3]In the 'real player' phase of the experiment, we announced the player ID letter (A, B, etc.) of the person who was the highest scoring subject. Ties were broken by choosing the fastest completion time. The winner was then told that she/he was the high (low) scorer and that his/her decisions would be observed by all the other players. The other players were told that a player identified by a letter was the high scorer and that his/her choices would be observed. In the simulated player phase the same procedure was used for announcing the high/low scorer. Before making decisions, subjects were reminded "Remember that * had the highest (lowest) score on the quiz. After the first period everyone will see what * did in the previous period." This was the extent of the status manipulation.

[4]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.

Table 3 Percentage of subjects choosing a strategy by commonly observed signal (number in parentheses). Bold indicates that the choice matches the signal

	Real—Move 1	Real—Move 2	Real—Move 3	Robot—Move 2
High status				
Chose 1	**72.45%**	47.76%	56.91%	54.06%
	(213)	(64)	(107)	(333)
Chose 2	23.47%	**44.78%**	25.53%	**38.31%**
	(69)	**(60)**	(48)	**(236)**
Chose 3	4.08%	7.46%	**17.55%**	7.63%
	(12)	(10)	**(33)**	(47)
Total	100%	100%	100%	100%
	(294)	(134)	(188)	(616)
Low status				
Chose 1	**69.40%**	54.17%	65.62%	66.37%
	(322)	(78)	(42)	(446)
Chose 2	20.91%	**37.50%**	18.75%	**27.83%**
	(97)	**(54)**	(12)	**(187)**
Chose 3	9.70%	8.33%	**15.62%**	5.80%
	(45)	(12)	**(10)**	(39)
Total	100%	100%	100%	100%
	(464)	(144)	(64)	(672)

copied by the other players. For example, in the high status treatment, when the real player played 1, the subsequent 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 than a low-status signal. The table also indicates that in the high status conditions subjects are more successful on average in moving to the Pareto superior equilibrium.

These findings are partly borne out in Figs. 1a–1d which plot the distribution of row choices by treatment and period. Figure 1a illustrates the difficulty of coordinating on the Pareto superior equilibrium when there is a high status real player sending a common signal in the first phase (periods 1–15). In part this is because high status players do not consistently choose the same strategy. In the second phase, when the simulated high status player sends a clear signal, subjects increasingly shift from row 1 to row 2. But a clear signal is no guarantee, as is evident from Fig. 1b. Despite the consistent common signal of row 2 by the high status simulated player, subjects largely stay with row 1 in the first phase. In the second phase (periods 16–30), subjects increasingly choose row 2, even though the high status real player is inconsistent. By contrast in both low status treatments there is little attention paid to the commonly-observed player, regardless of whether that player is real or simulated (see Figs. 1c and 1d).

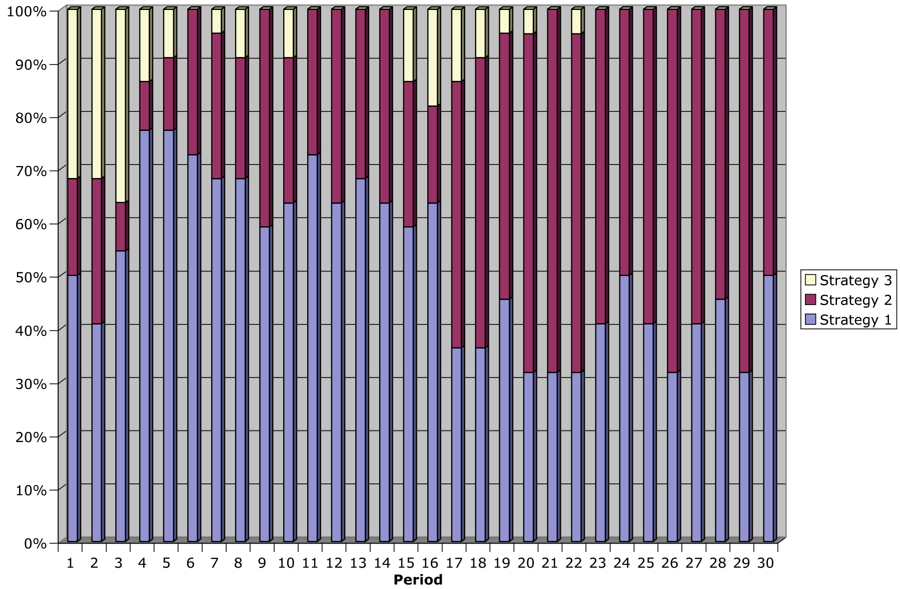

Fig. 1a Treatment with a high status real player in periods 1–15 and a high status simulated player in periods 16–30. The stacked bar chart indicates the percentage of subjects choosing the row strategy

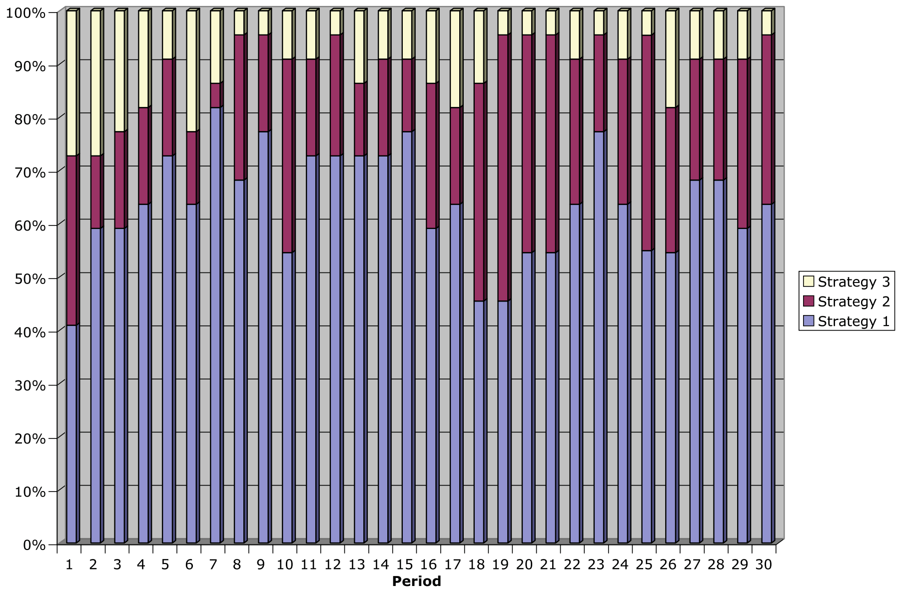

Fig. 1b Treatment with a high status simulated player in periods 1–15 and a high status real player in periods 16–30. The stacked bar chart indicates the percentage of subjects choosing the row strategy

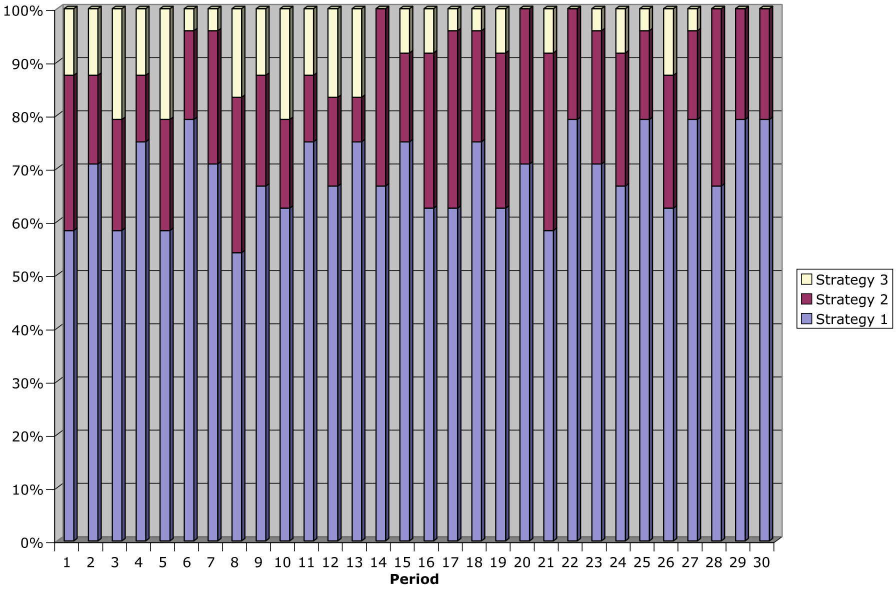

Fig. 1c Treatment with a low status real player in periods 1–15 and a low status simulated player in periods 16–30. The stacked bar chart indicates the percentage of subjects choosing the row strategy

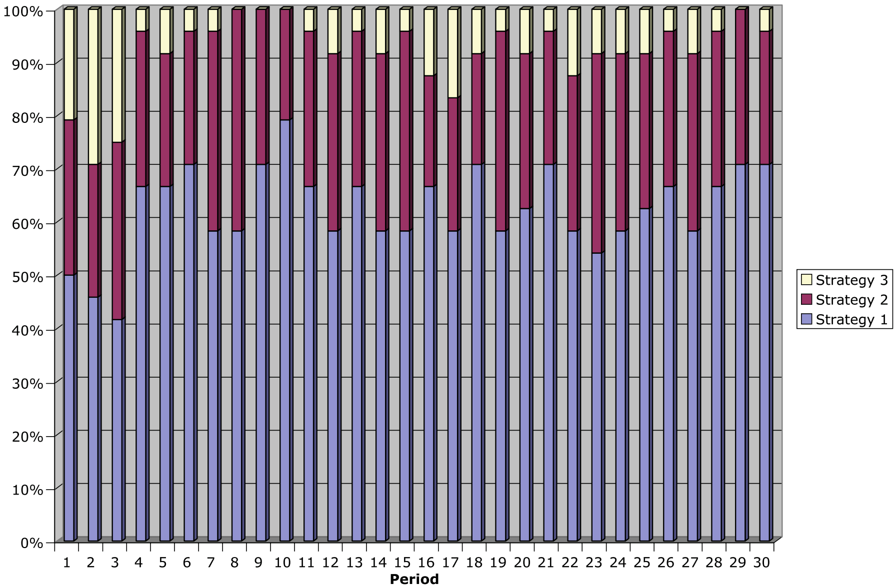

Fig. 1d Treatment with a low status simulated player in periods 1–15 and a low status real player in periods 16–30. The stacked bar chart indicates the percentage of subjects choosing the row strategy

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. A logit model is used because we are interested in whether a high status commonly-observed agent can move subjects to choose a strategy that leads to the efficient equilibrium. Since the best response to a play of strategy 1 or 3 is to play strategy 1, the critical distinction to us is whether choices can be moved away from 1 toward 2. Random effects logit is used 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. The model is estimated separately for the high and low status treatments. The dependent variable in our model is whether the subject chooses a strategy that leads to the Pareto superior equilibrium (row 2).

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 including 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.[5] 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.

Another variable controls for whether the subjects saw a real player's move during the period. We do so to account for the noisy signal that real players send and to test whether this introduces an independent effect. A dummy variable is also included to control for whether the commonly observed player chose row 2 in the prior period. Our primary focus is with the impact of this commonly observed signal. If status makes a difference it should matter only in the high status 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. Finally, we include a variable indicating 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 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 guesses about whether the commonly-observed player was real or not. At the end of the experiment we asked subjects, for each phase, whether the commonly-observed player was real or simulated. Subjects did little better than chance in guessing their counterpart's type. When we included their guesses in the models Table 4 we found no independent effects.

[5]This specification imposes a linear structure on the subjects' learning. Including the log of the period number to model nonlinearity did not significantly improve the fit or change the estimates on other variables of interest.

Table 4

	High status	Low status
Intercept	**−2.973**	**−2.266**
	(.457)	(.420)
	p < .001	*p < .001*
Time (1...30)	.003	−.005
	(.028)	(.025)
	p = .916	*p = .849*
Time in phase 2	−.040	−.031
(1...15)	(.038)	(.036)
	p = .299	*p = .394*
Phase 2	**1.186**	.266
	(.334)	(.306)
	p = .000	*p = .383*
Real commonly	.218	.015
observed player	(.284)	(.251)
	p = .443	*p = 954*
Commonly-	**.658**	.303
observed player	(.309)	(.272)
move = 2	*p = .033*	*p = .266*
Prior move = 2	**1.132**	**.898**
	(.182)	(.163)
	p < .001	*p < .001*
Prior partner's	**1.389**	**1.249**
move = 2	(.180)	(.161)
	p < .001	*p < .001*
	LLF = −551.78	LLF = −632.54

Coefficients, standard errors in parentheses, significance levels in italics

The strongest support for the value of status comes from the coefficient on "Commonly Observed Player Moved 2". 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. This supports 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, including 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 self-reported 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 commonly-observed 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 "high status" effect.

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 or low status. We find that a high status, commonly-observed agent results 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 MacLeod (1995) found that a strategy recommended by the experimenter to play the efficient (but risky) equilibrium could move subjects toward greater frequency of efficient play. At the same time a recommendation to play the inefficient equilibrium is very effective at reaching full coordination. The experimenter can be thought of as having inherently high status in the context of the experiment. In our view this signal should be more likely to be followed than if it came from someone else.

In Eckel and Wilson (2000), 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), and a signal of (3) is never followed but rather induces subjects to play (1), the best response to (3). The paper has shortcomings: first, the commonly observed agent was introduced in a deceptive way; in addition, 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. We focus only on a recommendation to play strategy 2, since the effects of recommending (1) or (3) are known. Our findings imply that the play of a commonly observed agent does not merely make one equilibrium more salient or focal, as Schelling (1960) would predict. Instead that play is more effective in influencing others if the observed agent has high status. While the difference is not large, it is significant, and in a dynamic setting could make a difference in the likelihood that an economic system will converge on a socially-desirable, efficient outcome. Merely signaling a strategy serves as a coordination device, but that signal is more powerful when it comes from an agent with higher status.

Acknowledgements Thanks to the participants in the Coordination Success Conference in Honor of Raymond Battalio for insightful suggestions, and do the reviewers and editors for their encouragement and feedback. Research assistance was provided by Christy Aroopala and Joanna Dunnaway at Rice and Christian Rojas, Alexandra Andino and Eric McLester at UT Dallas. This research was funded by the National Science Foundation: Eckel was supported by SES-0618226, and Wilson by SES-0318116.

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