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LEARNING IN SENDER-RECEIVER GAMES

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Headnote

This paper compares a well-known stimulus-response (SR) model and a belief-based learning (BBL) model using experimental data from sender-receiver games. When the models are fitted to the data by maximum likelihood, the fits are good for both models. In contrast to Camerer and Ho [1997], we compare the models using a formal statistical procedure based on the Davidson and MacKinnon P-test for non-nested hypotheses. The motivation for using this test is that the models are naturally non-nested. Both models involve a certain adjustment parameter, which measures the importance of forgetting. Our results show that the outcome of the test is sensitive to the value chosen for the adjustment parameter. Depending on the value selected, the P-test favors the SR model, the BBL model or neither of the models. A point often overlooked in empirical work is that information from learning can only come from observations where learning occurs. A preliminary examination suggested that our results were affected to some degree by observations taken after behavior has converged. We adjusted the data for this convergence effect and found that the results where not markedly different from our original findings.

Keywords: Games, experiments, non-nested testing

JEL codes: C7, C9, C13

1. INTRODUCTION

Our objective in this paper is to use experimental data to investigate how much available information players use when learning how to play a game. Three appealing criteria for a learning model are the following: (1) learning at the level of the individual; (2) stochastic choice; and (3) parsimony. The well-known stimulus-response SR model of Roth and Erev [1995] satisfies all three of these criteria, besides being consistent with some of the stylized facts established in the psychological learning literature. The key feature of SR learning is that it requires only minimal cognitive abilities on the part of players. This feature of the model is appealing for those who want to show that high-rationality predictions can be derived from a low-rationality model. A closely related feature is that SR learning requires only minimal information. All that players need to know are the payoffs from their own past actions; they need not know that they are playing a game, they need not know their opponents' payoff or their past play. These two closely related features make the SR model a natural benchmark in our investigation. In addition, it can be applied in a wide variety of settings.

On the other hand, it seems quite likely that individuals would try to exercise more cognitive ability and hence try to use other available information. In belief based learning (BBL) models, like fictitious play (Robinson, 1951) or one of its stochastic variants (Fudenberg and Kreps, 1993), the players use more information than their own historical payoffs. This information may include their own opponents' play, the play of all possible opponents and the play of all players. Models of this kind embody a higher level of rationality; e.g. fictitious play can be interpreted as optimizing behavior given beliefs that are derived from Bayesian updating. In our investigation, we compare a simple BBL model against the SR model.

We consider five experimental treatments, each with three replications. Each replication is divided into two sessions, Session I, which is common to all treatments and Session II, which varies across treatments, both of which last for 20 rounds. We concentrate on Session II data. In each treatment, there are two populations of players, senders and receivers, where in each round one sender is randomly matched with one receiver to play a given sender-receiver game. The treatments examined here differ in terms of the players' incentives and the information that is available after each round of play. For one treatment, the only information available to a player is the history of play in her own past matches. Two questions are examined for these cases. The first is whether learning takes place. If learning does take place, the second question is whether the learning is captured by SR model. In all the other treatments, there is information available to the players in addition to the history of play in their own past matches. For both senders and receivers, this information is the history of play of the population of senders. Three questions are examined for these treatments. The first again is whether learning takes place. If learning does take place, the second question is whether learning is different from that in the previous treatment, and the third is whether the BBL model better describes learning than the SR model. The data used in this paper is from the experiments in Blume, DeJong, Kim and Sprinkle [1997].

The initial step in our analysis was to fit the SR and BBL models to the data generated by the various treatments. We found that, regardless of treatment, both models fit about equally well as measured by the coefficient of determination.

We let the BBL model take a form that is analogous to the SR model. In both cases choice probabilities depend on propensities. The models differ in how the propensities are updated. In the SR model the propensity for taking an action is solely dependent on a player's own past payoffs from that action, whereas in the BBL model the propensity depends on the average payoff across all players who took that action. Owing to the similar structure, it would appear that the SR and BBL models can be nested in an encompassing model, like that of Camerer and Ho [1997]. However, the approach of Camerer and Ho [1997] is misleading since the propensities, which are calculated from the estimated parameter values, differ across the models. One way to calculate the unobserved propensities is to impose the parameter values used in the literature. We found, however, that these values are overwhelmingly rejected by data. For the purpose of comparing the models, a more natural approach is to use a non-nested testing procedure. In this paper, we employ the Davidson and MacKinnon P-test for non-nested hypotheses. The outcome of the P-test is sensitive to the value chosen for the adjustment parameter. We show that depending on the value selected, the P-test favors the SR model, the BBL model or neither of models

A point often overlooked in empirical work is that information about learning can only come from observations where learning occurs. Once behavior has converged, observations have no further information about learning. Including such observations will make the model appear to fit better, while at the same time reducing the contrast between the models, making it difficult to distinguish the models empirically. We call this effect convergence bias. A preliminary examination suggested that our results were affected to some degree by convergence bias. Accordingly, we eliminated observations where it appeared that learning has ceased and reanalyzed the remaining data. The results of this reexamination were not markedly different from our original finding.

2. GAMES AND EXPERIMENTAL DESIGN

Our data are generated from repeated play of sender-receiver games among randomly matched players. Players are drawn from two populations, senders and receivers, and rematched after each round of play. The games played in each round are between an informed sender and an uninformed receiver. The sender is privately informed about his type, t_1 or t_2, and types are equally likely. The sender sends a message, * or #, to the receiver, who responds with an action, a_1, a_2 or a_3. Payoffs depend on the sender's private information, his type, and the receiver's action, and not on the sender's message. The payoffs used in the different treatments are given in TABLE I below, with the first entry in each cell denoting the sender's payoff and the second entry the receiver's payoff. For example, in Game 2, if the sender's type is t_1 and the receiver takes action a_2, the payoffs to the sender and receiver are 700,700, respectively.

A strategy for the sender maps types into messages; for the receiver, a strategy maps messages to actions. A strategy pair is a Nash equilibrium if the strategies are mutual best replies. The equilibrium is called separating if each sender type is identified through his message. In a pooling equilibrium, the equilibrium action does not depend on the sender's type; such equilibrium exists for all sender-receiver games. In Game 2, an example of a separating equilibrium is one where the sender sends * if he is t_1 and # otherwise and the receiver takes action a_2 after message * and a_1 otherwise. An example of a pooling equilibrium is one in which the sender, regardless of type, sends * and the receiver always takes action a_3.

A replication of a game is played with a cohort of twelve players, six senders and six receivers. Players are randomly designated as either a sender or receiver at the start of the replication and keep their designation throughout. In each period of a game, senders and receivers are paired using a random matching procedure. Sender types are independently and identically drawn in each period for each sender.

In each period, players then play a two-stage game. Prior to the first stage, senders are informed about their respective types. In the first stage, senders send a message to the receiver they are paired with. In the second stage, receivers take an action after receiving a message from the sender they are paired with. Each sender and receiver pair then learns the sender type, message sent, action taken and payoff received. All players next receive information about all sender types and all messages sent by the respective sender types. This information is displayed for the current and all previous periods of the replication.

To ensure that messages have no a priori meaning, each player is endowed with his own representation of the message space, i.e. both the form that messages take and the order in which they are represented on the screen is individualized. The message space M = {*, # } is observed by all players and for each player either appears in the order #,* or *, #. Unique to each player, these messages are then mapped into an underlying, unobservable message space, M = {A,B}. The mappings are designed such that they destroy all conceivable focal points that players might use for a priori coordination, Blume et al.[1997]. The representation and ordering are stable over the replication. Thus, the experimental design focuses on the cohort's ability to develop a language as function of the game being played and the population history provided.

Note that in this setting we learn the players' action choices, not their strategies. Also, the players themselves receive information about actions, not strategies. They do not observe which message (action) would have been sent (taken) by a sender (receiver) had the sender's type (message received) been different. This is important for how we formulate our learning rules; e.g. in our setting the hypothetical updating (see Camerer and Ho (1997)) of unused actions that occurs in BBL cannot rely on knowing opponents' strategies but instead uses information about the population distribution of play. For example, for the receiver the best reply to a message that he did not receive is determined by the distribution of sender types that sent the messages.

The data consist of three replications for each game. Replications for Game 1 and 2 were played for 20 periods and Game 3 and 4 for 40 periods.[1] There were two different treatments conducted with Game 1, one with and one without population history. In the treatment with history, senders and receivers observe sender history, that is, the types of drawn and the messages sent by type in all prior periods. In each replication two sessions of the game were played. In this paper we focus on the analysis of sender behavior using the data from the second session. The attraction of concentrating on sender behavior is that senders have the same number of strategies in all of our treatments. A potential drawback of this focus is that since senders do not receive information about the history of receiver play at the population level, they cannot form beliefs based on that information. Instead they have to make inferences from what they learn about sender population. We also examined receiver behavior and found essentially the same results as for senders.

3. TESTING SR AND BBL MODELS

In this section we report the results of estimation of SR and BBL models of behavior. The models are similar in that both use propensities to determine choice probabilities. In our extensive form game setting, we have to make a choice of whether we want to think of players as updating propensities of actions or of strategies. Both choices constrain the way in which the updating at one information set affects the updating at other information sets. If actions are updated, then there are no links across information sets. If strategies are updated, then choice probabilities change continually at every information set. We chose updating of actions, which amounts to treating each player-information set pair as a separate player. We use the index i to refer to such a pair (n, θ), where n denotes one of the six senders, θ a type realization for that sender, and the pair i is called a player.

By SR we mean that individual play is affected only by rewards obtained from own past play. Specifically, following Roth and Erev [1995] define the propensity, $Q_{ij}(t)$, of player i to play action j at time t as:

$$Q_{ij}(t) = \varphi_0 Q_{ij}(t-1) + \varphi_1 X_{ij}(t-1) \tag{3.1}$$

where $X_{ij}(t\text{-}1)$ is the reward player i receives from taking action j at time t-1. Time here measures the number of occurrences of a specific type for a fixed sender; φ_0 measures the lag effect (i.e. the importance of forgetting), and φ_1 the contribution of the most recent observation. Note that t is not real time. Given this specification of propensities, the probability that player i takes action j is a logit function[2]

$$P_{ij}(t) = \Pr(\textit{Player i takes action j at time t}) = \frac{Q_{ij}(t)}{\sum_{j'} Q_{ij'}(t)}. \tag{3.2}$$

To complete the specification of the SR model we require an initial condition for the propensities- the values of $Q_{ij}(1)$. Values chosen for $Q_{ij}(1)$ affect $P_{ij}(1)$ and the speed with which rewards change probabilities of playing a particular action. In the spirit of Roth and Erev [1995] we set $Q_{i1}(1) = Q_{i2}(1) = 500$, which is on the scale of rewards received by participants in the experiments. [3]

For these experiments we examine the behavior of senders, who can be of two types. Each type could send message "1" or "2". Let y = I{message=="2"}, where I{A} is the indicator function that takes the value 1 if event A occurs and 0 otherwise. The log likelihood function for the sender data is

$$(3.3)\quad \ln l(\varphi_0,\varphi_1)=\sum_{i=1}^{N}\sum_{t=2}^{T} y_{it}\ln(P_{i2}(t))+(1-y_{it})\ln(1-P_{i2}(t)).$$

The likelihood function was maximized separately for each of the 15 replications using observations from round 2 onwards. Because the quantal choice model has a regression structure the maximization can be achieved by Iteratively Re-Weighted Least Squares, which provides measures of fit for the non-linear regression. We use these measures to describe the explanatory power of each specification. The results of doing so are shown in TABLE II. Columns 2 and 3 of the table contain the estimates of φ_0 and φ_1 and columns 4 and 5 contain the log likelihood value at the optimum, and the R^2 statistic, the squared correlation coefficient between the binary dependent variable and its predicted value. Column 6 contains the likelihood ratio test statistic for the hypothesis H_0: $\varphi_0 = \varphi_1 = 1.0$, parameter values consistent with mean updating.

Two features stand out in the table. First, estimates of φ_0 and φ_1 are generally quite far from 1, judging from the LR test p-values reported in column 7. Only for G1NHR2 would the hypothesis not be rejected by the conventional statistical test. Second, the SR model, when the parameters are chosen optimally, fits the experimental data well, judging by the R^2 values reported in column 5. Over the 15 replications the SR model explained 79% of the variation in messages sent by the participants. Figure 1 shows the probability of sending message 2 for each agent type by period for the 15 replications. The line marked with the numeral "1" shows the fraction of type 1 agents playing message "2" each period while the line marked with a circle shows the model's predicted fraction of type 1 agents playing message 2. Precisely the same information is shown for type 2 agents by the line marked with the numeral 2 (actual fraction) and a triangle (predicted fraction). Thus, in the game shown in the top left-hand graph in the figure - G1R1 - 50% of type 1 agents play message 2 in round 1, as do 50% of type 2 agents. By period 7, 100% of type 1 agents play message 2, and 100% of type two agents play message 1. A similar pattern appears in replications 2 and 3 of Game 1, and in all three replications of Game 2. A complete discussion of the empirical patterns in these experiments is given in Blume, DeJong, Kim, and Sprinkle [1997]. Figure 1 demonstrates that SR when fitted to the experimental data of BDKS fits that data closely.

An alternative literature (e.g., van Huyck, et al [1996], Cheung and Friedman [1997]), and Camerer and Ho [1997]) argues that variants of fictitious play –BBL --are better characterizations of play in experiments. BBL is expected to dominate SR because BBL uses more information, namely in our setting the experiences of other participants. Specifically, define the propensity, $Q_{ij}(t)$, of player i to play action j at time t as:

$$(3.4) \quad Q_{ij}(t) = \beta_0 Q_{ij}(t-1) + \beta_1 \bar{X}_j(t-1)$$

that $\bar{X}_j$ (t-1) is the average reward all players received from taking action j at time t-1. Note that $\bar{X}_j = k^*X_{ij} + (1\text{-}k) X_{-ij}$, where k = 1/#{persons playing action j in round t} and X_{-ij} is the average return to all individuals other than i who play action j. Note that if H_0: $\beta_0 = \beta_1$, this is (weighted) mean updating. The choice probabilities again are logit as in (2.2) with (2.4) replacing (2.1) as the definition of Q_{ij}, and the likelihood function is (2.3).

TABLE III contains the estimates for each of the 15 replications. Columns 2 and 3 of the table contain the estimates of β_0 and β_1, and columns 4 and 5 contain the log likelihood value at the optimum, and the R^2 statistic evaluated at the mle. Column 6 contains the likelihood ratio test statistic for the hypothesis H_0:$\beta_0 = \beta_1$, the value implied by strict mean updating.

Again, two features stand out in the table. First, estimates of β_0 generally are fairly close to 1, but estimates of β_1 are substantially larger, indicating that recent rewards are given greater weight. The hypothesis that they are given equal weight is rejected by a wide margin in all cases, as judged by the likelihood ratio test shown in column 6. Second, the BBL model, when the parameters are chosen to maximize a likelihood function, also fits the experimental data well. In the 15 cases the R^2 value ranges from .62 to .97; on average the BBL model explains 79.9% of the variation in messages sent. The fit of this model is illustrated in Figure 2, which shows the relation of predicted response and actual response by period. The comparison of R^2's is suggestive: BBL "wins" in most cases with population history information, SR wins without that information.

4. COMPARING SR AND BBL MODELS

Figures 1 and 2 demonstrate the problem of distinguishing SR and BBL models of behavior. Both SR and BBL learning fit the data well; hence, distinguishing these models is difficult. For example, in the BDKS data the average difference in R^2 is .008, and the typical sample size is 108. Thus, an eyeball test does not show a great preference for one learning specification. But an eyeball test may not be very powerful in these circumstances so resort might be had to a more formal testing procedure. Making probabilistic comparisons between the SR and BBL models is difficult because the models are *non-nested.* By a *nested model* we mean that the model being tested, the null hypothesis, is simply a special case of the alternative model to which it is compared. In contrast to Camerer and Ho [1997], we compare the SR and BBL models in a non-nested framework. In particular, we employ Davidson and MacKinnon's P-test for non-nested hypothesis testing in the following manner. Write the models described in equations (3.1) and (3.4) and a nested composite model as:

$$
\begin{aligned}
&H_1\!: E(y_1(t)) = F_1(\hat{Q}_{t-1}, X_{ij}(t-1); \varphi) \\
(4.1)\qquad &H_2\!: E(y_1(t)) = F_2(\tilde{Q}_{t-1}, \overline{X}_j(t-1); \beta) \\
&H_c\!: E(y_1(t)) = (1-\alpha)F_1(\hat{Q}_{t-1}, X_{ij}(t-1), \varphi) + \alpha F_2(\tilde{Q}_{t-1}, \overline{X}_j(t-1); \beta)
\end{aligned}
$$

where φ and β are (k+1) x 1 dimension vectors with k the dimensionality of X_{ij}, and $\hat{Q}_{ij}$ and $\tilde{Q}_{ij}$ are the propensities evaluated at the estimated parameter values. Models 1 and 2 differ because $X_{ij} \neq \overline{X}_i$ in general, and because the Q's differ unless the parameter restrictions discussed above hold. (To reinforce the latter point, the two Q's are distinguished by using a hat and a tilde.). Following Davidson and MacKinnon [1984, 1993] we test H_1 against H_C by replacing β by its mle, $\hat{\beta}$, and constructing the artificial regression:

$$(4.2) \quad \hat{V}^{-1/2}(y-\hat{F}_1)=\hat{V}^{-1/2}\hat{f}_1 X_{ij}\hat{\varphi}+\alpha\hat{V}^{-1/2}(\hat{F}_2-\hat{F}_1)+residual$$

where $V^{-1/2}$ is the square root of the variance covariance matrix for the dichotomous variable y. In the same fashion we test H_2 against H_C by replacing φ by its mle, $\hat{\phi}$, and constructing a similar artificial regression. The test for nesting is a test of the hypothesis that $\alpha = 0$. There are four outcomes that can arise from this pair of tests: (a) accept both models; (b) reject both models; (c) accept model 1 and reject model 2; and (d) accept model 2 and reject model 1. Obviously, the first outcome provides no evidence favoring either model of behavior. A rejection of both models could occur for several reasons, one of which is a mixture model. Specifically, the composite hypothesis $H_{C,}$ can also be interpreted as the hypothesis that α percent of the population plays according to BBL and $(1-\alpha)$-percent use SR.

There are two reasons why the BBL and SR models are non-nested hypothesis, and consequently we proceed in two steps to test the hypotheses. In the first stage we maintain the assumption that the adjustment parameters are the same: $\beta_0 = \varphi_0$, and that only the specification of the X's differs between the models. Because the test statistic depends upon which (common) value is assigned to the adjustment parameter, we evaluate it once at the mle for φ, computed assuming that the SR model is correct, and once at the mle for β, computed assuming that BBL is correct. Finally, we compute the P-test statistic without any constraint on the adjustment parameter. TABLE IV contains the absolute value of the t-statistics for testing the hypothesis $\alpha = 0$ for each of the 15 sets of experiments. In discussing these tests we use a 95% confidence interval as a reference. Summaries of the tests are shown in Figure 3.

Figure 3 shows that the choice of how propensities or attractions are updated has a large influence on the test results. When $\beta_0 = \varphi_0 = \beta^{SR}$, as in (a), the data accept SR in 13 of 15 cases. When $\varphi_0 = \beta_0 = \beta^{BBL}$, as it does in Figure 3, the evidence becomes persuasive for BBL. In only 4 of 15 cases is SR accepted, while BBL is accepted in 14 of

15. But when β_0 and φ_0 are unconstrained, $\varphi_0 = \varphi_0^{\bullet}$ and $\beta_0 = \beta_0^{\bullet}$, as in the artificial regressions summarized in (c), the evidence supporting either theory is equivocal. In 8 of 15 cases SR is accepted, and in 8 cases BBL is accepted. Ignoring the 5 cases where both models are accepted and the 4 cases where both cases are rejected, leaves 3 cases where SR is accepted and BBL rejected, and precisely 3 cases where the reverse is true. Thus the data render a Scotch verdict: the case for either model is not proved.

There is a pattern in the tests that suggests an explanation for these results. The 3 cases that support SR and reject BBL all come from the experiments G1NH. These are the experiments where history of play information was not made available to senders or receivers. Consequently, the case for SR behavior is strongest here. Similarly, 2 of the 3 cases that support BBL and reject SR come from the G1 experiments where information about the history of play was made available to receivers. In these experiments a stronger *a priori* case for BBL can be made. Thus, it appears that the structure of the experiment has an important effect upon the modality of learning behavior that occurs.

We also examined receiver behavior. Unlike senders, receivers actually have the information that is needed to engage in forms of learning like fictitious play. Thus a comparison between sender and receiver learning can inform us about the importance of the type of information that is available at the population level. A direct comparison between sender and receiver behavior is not possible for all experiments because receivers frequently had more actions that they could take. However, for Game1, both with and without information on history of play, both senders and receivers faced binary choices and a comparison could be made. Generally, the results for the receiver data looked much like the sender data. Both SR and BBL models fit the data well, with SR doing slightly better in the games with no history, and BBL doing better where history of play information was available. The average difference in R^2 was 0.03 across the six games/replications. Figure 4 summarizes the six experimental results. Overall, the test results in Panel (c) of the figure show SR being the preferred model in 4 of the 6 cases, with BBL the preferred model in the other two cases, both of which were from the games with history of play information available. Though consistent with a learning story, this is only weak evidence in favor of one model, and underscores our point that it is difficult to tell these models apart with any degree of precision.

5. CONVERGENCE BIAS

It is common practice to include all observations from a particular experiment in any statistical estimation or testing exercise based on that experiment. Yet information about learning can only come from observations where learning occurs. Once behavior has converged, observations have no further information about learning. Including such observations will make the model appear to "fit" better, while at the same time reducing the contrast between models, making it difficult to distinguish the models empirically. The data shown in Figures 1 and 2 indicate that convergence typically occurs within 5 to 7 rounds, while observations are included in the estimation for the entire period, in these data up to 20 periods. To illustrate the possible bias that this might cause we calculated R^2 and average log likelihood (=maximized log likelihood/ # of observations used) by progressively removing observations from the right tail, that is, by removing observations that include convergence. Figure 5 illustrates this bias for the experiments of game 1. Under the hypothesis of no convergence bias we would expect the slopes of each line in panels (a) and (b) of the figure to have zero slope. In fact, all four lines have positive slope, which is characteristic of convergence bias. However, the difference between the lines in each panel is approximately constant in these data, which suggests that convergence bias makes both models appear to fit the data better, but does not otherwise bias the comparison of SR and BBL.

To measure the amount of bias requires taking a position on when convergence has occurred, a classification that is better made on individual data. We define the convergence operator $T_p(y_{it})$ by

(5) $$T_p(y_{it}) = 1 \; if \;\; y_{it} = y_{it-1} = \ldots = y_{it-p}$$
$$= 0 \; else$$

In other words a player's (pure) strategy is said to have converged if the same action is taken p times in a row.[4] To eliminate convergence bias one simply excludes observation where $T_p=1$. We used this procedure with p = 3 and p = 4 to assess the extent of this bias. We found that at least for these data, the extent of the bias was small. For example, the non-nested hypothesis tests shown in Figure 3(c) had the same off-diagonal values (3), while the accept -accept entry changed from 5 to 3, while the reject-reject entry changed from 4 to 6. In other words, correcting for convergence bias sharpened the distinction between the two models but it did not favor either model.

6. RELATED LITERATURE

There is an extensive and growing literature in experimental economics on learning, e.g. Boylan and El-Gamal [1993], Camerer and Ho [1997], Cheung and Friedman [1997], Cooper and Feltovich [1996], Cox, Shachat and Walker [1995], Crawford [1995], Roth and Erev [1995]. The literature generally focuses on two broad classes of learning models, stimulus-response and belief based play. A wide variety of games are considered with various designs, e.g., whether or not players are provided with the history of the game. The performances of the learning models are evaluated using simulation and various statistical techniques. Unfortunately, the findings are mixed at best. This could be due to statistical issues, Fudenberg and Levine's [1997] conjecture that with convergence to Nash in the "short term," the models maybe indistinguishable, or a combination of the two.

The seminal paper that deals with the stimulus-response model is Roth and Erev [1995]. Their concern is high (super rationality) versus low (stimulus-response) game theory and intermediate (e.g., Fudenberg and Levine's "short term") versus asymptotic results. Roth and Erev focus on a simple individual reinforcement dynamic in which propensities to play a strategy are updated based upon success of past play. Using simulation, they avoid the problem of estimation and compare the simulations to their experiments. The simulated outcomes are similar to observed behavior and, more importantly, vary similarly across the different games considered. They interpret this as robustness of the intermediate run outcomes to the chosen learning rule.

Roth and Erev's dynamic is similar to the reinforcement dynamic of Bush and Mosteller [1955] and Cross [1983] except that in the latter probabilities are updated instead of propensities. Mookherjee and Sopher [1994] evaluate the rote model of Bush and Mosteller by comparing its performance to the belief based fictitious play model using logit and other statistical comparisons. In the matching pennies game with a mixed strategy equilibrium (with and without game history), nothing works in the no information condition. With information, average payoffs and Bush-Mosteller had some explanatory power but not fictitious play.

Cheung and Friedman's [1997] conclusions are just the opposite. Using a variety of games and an information condition (with and without game history), Cheung and Friedman compare the performance of Cournot, fictitious play and rote learning. In the extended probit, the belief based model has more support than rote learning and information matters. Boylan and El-Gamal [1993] compare Cournot and fictitious play in a broad cross-section of games obtained from other researchers. Using a Bayesian approach, fictitious play is the overwhelming choice.

Van Huyck, Battalio and Rankin [1997] focus on 2x2 symmetric coordination games and evaluate the performance of the replicator dynamic, fictitious and exponential fictitious play. Models of reinforcement learning can be used to justify the replicator dynamic (e.g., Boergers and Sarin [1995]). Using the standard logit model to rank performance, exponential fictitious play does best, followed by fictitious play and then the replicator dynamic.

McKelvey and Palfrey's [1995] model of quantal response equilibria in normal form games deserves some attention here. The quantal response model is Nash with error. Mckelvey and Palfrey develop this model with a logistic quantal response function. The equilibrium is then evaluated using the logit model and the developed maximum likelihood estimates for the data considered. They find that the quantal response model wins when compared to Nash without error and random choice. Important for us is their conclusion that errors are an important component in explaining experimental results. This has been implicitly assumed in the previous studies when logits and probits are used to analyze data and explicitly assumed in the Erev and Roth study cited below.

The lack of general findings in these and other papers has prompted a new series of studies.[5] The studies can be broadly described by the approaches they take. Camerer and Ho [1996] essentially give up on the horse race and develop a general model, which has as special cases the principle learning models in the literature. The key that ties the SR models to the belief BBL models is the reinforcement used. In the SR model the reinforcement is the average of past payoffs and for BBL models it is previous expected payoffs. When average and expected payoffs are the same so are the models. Using maximum likelihood estimation under the constraints of logit, Camerer and Ho evaluate the possible contribution of the general model across a variety of games. As one would hope, the general model explains more of the variation; note, however, that we have to interpret this result with caution because of the lack of nesting.

Erev and Roth [1997] continue their focus on the SR model via simulation but expand their analysis in two ways. First, they obtained data for games that were conducted for 100 periods or more. Second, they use the error structures from the simulations with statistical tests to compare the performance of their model with alternative models. In particular, the Nash equilibrium prediction, deterministic and stochastic fictitious plan at the aggregate level and for the within subjects comparison, they added exponential stochastic fictitious play and best reply to the previous period. Their model outperforms the others. The key insight is that a very simple SR model generates simulated data that closely mirror experimental data under a wide variety of circumstances. Furthermore, the SR model cannot easily be improved on by other (more sophisticated) models.

Selten [1997] is the true agnostic. He claims there is not enough data to form any conclusions, either theoretical or statistical. The best we can do is very general qualitative models (e.g., learning direction theory) in which there are tendencies that are distinct from random behavior but nothing more. This view brings us full circle to Fudenberg and Levine's conjecture about whether you can distinguish among the models if equilibrium play is observed in the "short term"or alternatively, the statistical issues make such comparisons moot.

The resolution of this debate is ultimately an empirical one. Based on the data in this paper, we find that it is difficult to discriminate between the SR and BBL models. In general, it appears that care are must be exercised when constructing the statistics for the horse races and simulation comparisons that are made.

7. SUMMARY AND CONCLUSIONS

In this paper we investigated how well SR and BBL models describe learning in a sender-receiver game experiment. In the experiment an extensive form game is played repeatedly among players who are randomly matched before each round of play. This population-game environment is particularly appropriate for a comparison of myopic learning rules, if we believe that it lessens the role of repeated game strategies. Sender-receiver games with costless and *a priori* meaningless messages have the advantage that no matter how we specify the incentives, coordination requires learning. One consequence of studying learning in extensive form games is that since players in the experiment observe only each others' actions, not strategies, the principle difference between the two learning models is in the roles they assign to own experience versus population experience. Another consequence is that there are different natural specifications even for a learning model as simple as SR; we chose the cognitively least demanding one, in which experience at one information set does not transfer to other information sets.

We found that both models fit our data well and the predicted choice probabilities closely track the actual choice frequencies. It is suggestive that the BBL model fits slightly better than SR when population information is available, and vice versa without such information. However the differences are not large enough to be conclusive.

There have been recent efforts to embed both models in an encompassing model and to perform nested tests on this model. However, such tests are invalid if the supposed encompassing model involves unobserved variables whose values have to be calculated from the parameters of the model. Therefore a non-nested test is appropriate. Using such a test, we found that depending on parameter choices, this test may favor either model. If parameters are unrestricted, both models are approximately equally often accepted and rejected. Thus, like the comparison of fits, the formal test does not permit us to choose one model over the other. We raise the issue of convergence bias and show that for our data correcting for this bias does not lead to better discrimination between the two models.

Our treatment of testing with experimental data has been, from a statistical point of view, entirely conventional. We have assumed that standard asymptotic theory provides a reliable guide for inference in models with sample sizes encountered in experimental economics. Consequently, we have not studied issues such as the size and power of these tests, nor have considered the optimal design of experiments. We note however that the theories of learning in games are unusually rich in that they specify the data generation process precisely enough so that statistical performance under a specific null hypothesis can be assessed prior to obtaining the data by experimentation. Current practice in experimental economics chooses some experimental design parameters by whimsy, e.g., assigning participants to "types", choosing the number of periods to run an experiment, allocating payoffs for particular response. Consideration of optimal experimental design issues will allow a principled choice of these parameters, as has long been the case in other sciences with experimental data.

In summary, we provide further evidence that often very simple low-rationality models provide good descriptions of experimental learning data. While the availability of more information appears to favor the model that makes use of that information, it is difficult to significantly improve on the cognitively least demanding model. The issue of how well the available information is used appears to be difficult resolve and may require examination of (1) whether the classical tests are valid in such settings and (2) what size and type of data sets would permit a distinction.

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FOOTNOTES

[1] We gratefully acknowledge the helpful comments of Drew Fudenberg in early discussions and Al Roth and Ido Erev for giving us access to their software.

[2] All replications had a common session, which preceded the games described above. In particular, each cohort participated in 20 periods of a game with payoffs as in Game 1 and a message space of $M = \{A,B\}$. The common session provides players with experience about experimental procedures and ensures that players understand the structure of the game, message space and population history.

[3] The specification of the logit function in (2.2) exploits the fact that all rewards, X, in the games that we examine are non-negative. Were this not the case, a transform that keeps the value of the payoffs non-negative, such as the exponential function, can be used.

[4] We note that in principle one could treat $Q_{ij}(1)$ as a factor common to all agents and estimate its value by exploiting cross-sectional differences in play. For the experiments that we analyze in this paper the number of cross-section units is 6, so this is not a useful strategy.

[5] Defining convergence for mixed strategies is conceptually the same as the pure strategy case; empirically identifying convergence is more difficult.

[6] The number of studies is growing at an increasing rate. Consequently, we select representatives from the set and apologize for any omissions.

Table I

Payoffs of Games in Experiments

Panel (a)

Types		Game 1		Game 2		
		Actions				
		a_1	a_2	a_1	a_2	a_3
	t_1	0,0	700,700	0,0	700,700	400,400
	t_2	700,700	0,0	700,700	0,0	400,400

Panel (b)

Types		Game 3			Game 4		
		Actions					
		a_1	a_2	a_3	a_1	a_2	a_3
	t_1	0,0	200,700	400,400	0,0	200,500	400,400
	t_2	200,700	0,0	400,400	200,500	0,0	400,400

TABLE II

Maximum Likelihood Estimates of SR Model

Model	φ_0	φ_1	Lnl	R^2	LR-stat	P-value
G1R1	0.3802	0.6477	-35.44	.8233	19.9	0.0%
(N=108)	(0.156)	(0.546)				
G1R2	0.4996	0.6125	-25.89	.8728	9.4	0.2%
(N=108)	(0.156)	(0.425)				
G1R3	0.5137	0.8767	-44.26	.8064	13.5	0.0%
(N=138)	(0.159)	(0.786)				
G1NHR1	0.5903	0.5519	-49.46	.7309	10.9	0.1%
(N=108)	(0.107)	(0.355)				
G1NHR2	0.9590	0.8658	-57.42	.6327	0.2	67.3%
(N=108)	(0.121)	(0.555)				
G1NHR3	0.7133	0.9876	-40.39	.7744	13.1	0.0%
(N=108)	(0.098)	(0.528)				
G2R1	0.3357	0.8265	-13.77	.9257	28.8	0.0%
(N=108)	(0.189)	(0.713)				
G2R2	-0.0012	-0.0023	-9.02	.9487	32.7	0.0%
(N=108)	(0.086)	(0.164)				
G2R3	-0.0003	-0.0009	-25.84	.9253	7.7	0.5%
(N=108)	(0.115)	(0.308)				
G3R1	-0.0018	-0.0143	-9.03	.8992	58.0	0.0%
(N=108)	(0.179)	(0.002)				
G3R2	0.7710	1.4869	-70.25	.6824	4.1	4.4%
(N=138)	(0.095)	(0.938)				
G3R3	1.0040	2.0306	-63.40	.6802	4.1	4.4%
(N=138)	(0.120)	(1.165)				
G4R1	0.6151	0.2335	-70.01	.6472	15.7	0.0%
(N=138)	(0.098)	(0.203)				
G4R2	0.6586	3.9301	-30.40	.8684	30.3	0.0%
(N=138)	(0.097)	(2.241)				
G4R3	0.6253	0.3408	-76.28	.6445	7.2	0.7%
(N=138)	(0.087)	(0.264)				

Notes: Standard errors in parentheses beneath coefficient estimates.

TABLE III

Maximum Likelihood Estimates of BBL Model

Model	β_0	β_1	Lnl	R^2	LR-stat	P-value
G1R1	0.7839	8.2170	-27.55	.8633	24.5	0.0%
(N=108)	(0.201)	(3.580)				
G1R2	0.1374	8.6047	-27.37	.8780	9.4	0.2%
(N=108)	(0.144)	(2.989)				
G1R3	0.8388	16.6120	-36.04	.8327	32.3	0.0%
(N=138)	(0.129)	(8.276)				
G1NHR1	1.1054	1.8291	-56.92	.6831	6.0	1.4%
(N=108)	(0.276)	(1.456)				
G1NHR2	0.9760	1.8845	-61.72	.6173	2.0	15.3%
(N=108)	(0.179)	(1.136)				
G1NHR3	0.5252	4.7377	-44.23	.7623	9.3	0.2%
(N=108)	(0.171)	(1.781)				
G2R1	0.1048	32.7738	-13.54	.9436	29.7	0.0%
(N=108)	(0.062)	(17.245)				
G2R2	0.3271	13.3560	-11.61	.9452	24.8	0.0%
(N=108)	(0.117)	(4.354)				
G2R3	0.4319	9.3153	-14.76	.9738	23.8	0.0%
(N=108)	(0.135)	(2.944)				
G3R1	0.0034	52.0285	-15.61	.9249	47.1	0.0%
(N=108)	(0.037)	(19.110)				
G3R2	1.5835	-0.2113	-70.23	.6883	3.8	5.0%
(N=138)	(0.269)	(0.912)				
G3R3	1.9936	-1.8643	-64.65	.6700	17.1	0.0%
(N=138)	(0.215)	(0.337)				
G4R1	1.7421	-0.8367	-64.45	.6706	17.4	0.0%
(N=138)	(0.271)	(0.582)				
G4R2	1.0725	11.7503	-27.16	.8825	46.5	0.0%
(N=138)	(0.220)	(5.216)				
G4R3	1.2841	0.6672	-74.00	.6488	7.1	0.8%
(N=138)	(0.251)	(1.146)				

Notes: Standard errors in parentheses beneath coefficient estimates.

TABLE IV

Non-Nested Tests

	Model assumed True: SR			Model assumed True: BBL		
	$\beta_0 = \varphi_0^{SR}$	$\varphi_0 = \beta_0^{BBL}$	$\varphi_0 = \varphi_0^{*}$	$\beta_0 = \varphi_0^{S}$	$\varphi_0 = \beta_0^{BBL}$	$\beta_0 = \beta_0^{*}$
GAME	t-statistics			t-statistics		
G1R1	0.00	6.29	6.58	0.56	0.83	0.87
G1R2	0.00	0.00	1.97	2.31	2.30	2.27
G1R3	0.00	4.56	4.47	0.63	0.30	0.33
G1NHR1	0.00	1.46	0.44	4.96	4.17	5.31
G1NHR2	0.00	0.38	0.38	3.28	3.23	3.33
G1NHR3	0.00	0.00	1.95	3.01	3.47	6.94
G2R1	0.00	0.00	5.32	0.98	2.14	3.01
G2R2	0.00	0.00	5.79	0.00	2.32	2.22
G2R3	0.00	0.00	3.68	4.75	3.63	4.46
G3R1	0.00	1.10	0.88	1.63	0.82	0.77
G3R2	2.00	2.33	1.90	1.06	1.28	1.27
G3R3	1.79	2.39	1.88	0.17	0.90	1.95
G4R1	0.78	1.14	1.43	6.06	0.00	0.53
G4R2	2.94	6.09	4.33	1.81	1.41	1.36
G4R3	0.00	0.28	1.73	0.98	0.34	0.70

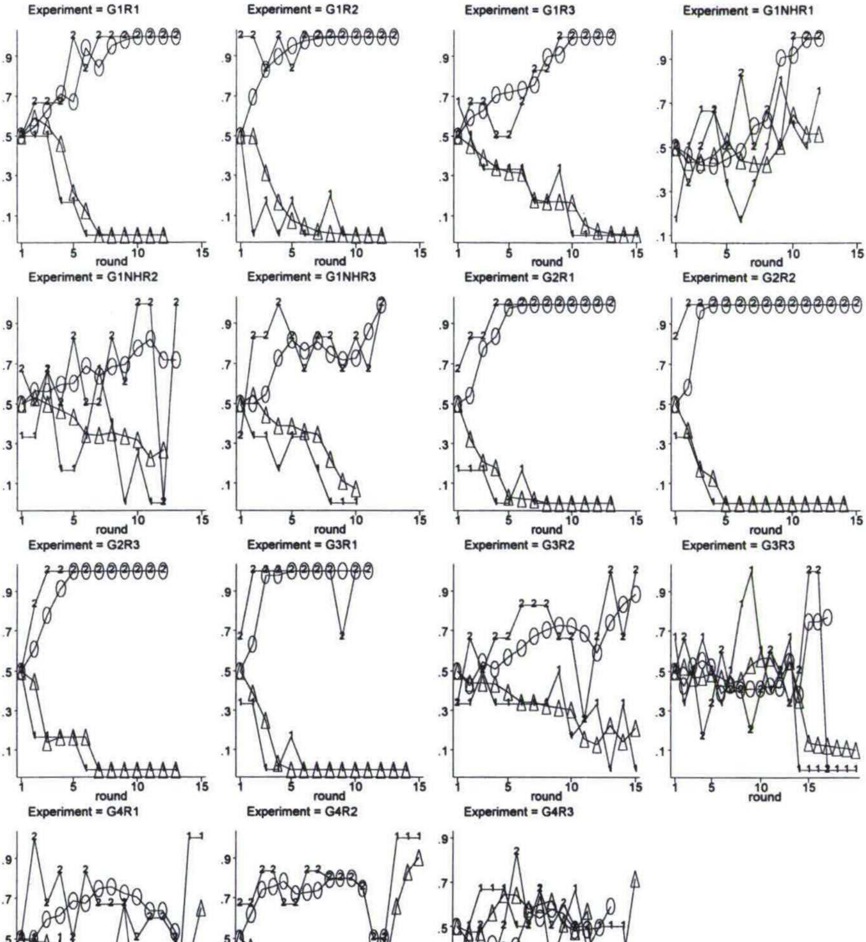


Figure 1.—Plots of the actual and predicted fraction of players sending message 2 by type when the SR model is true.

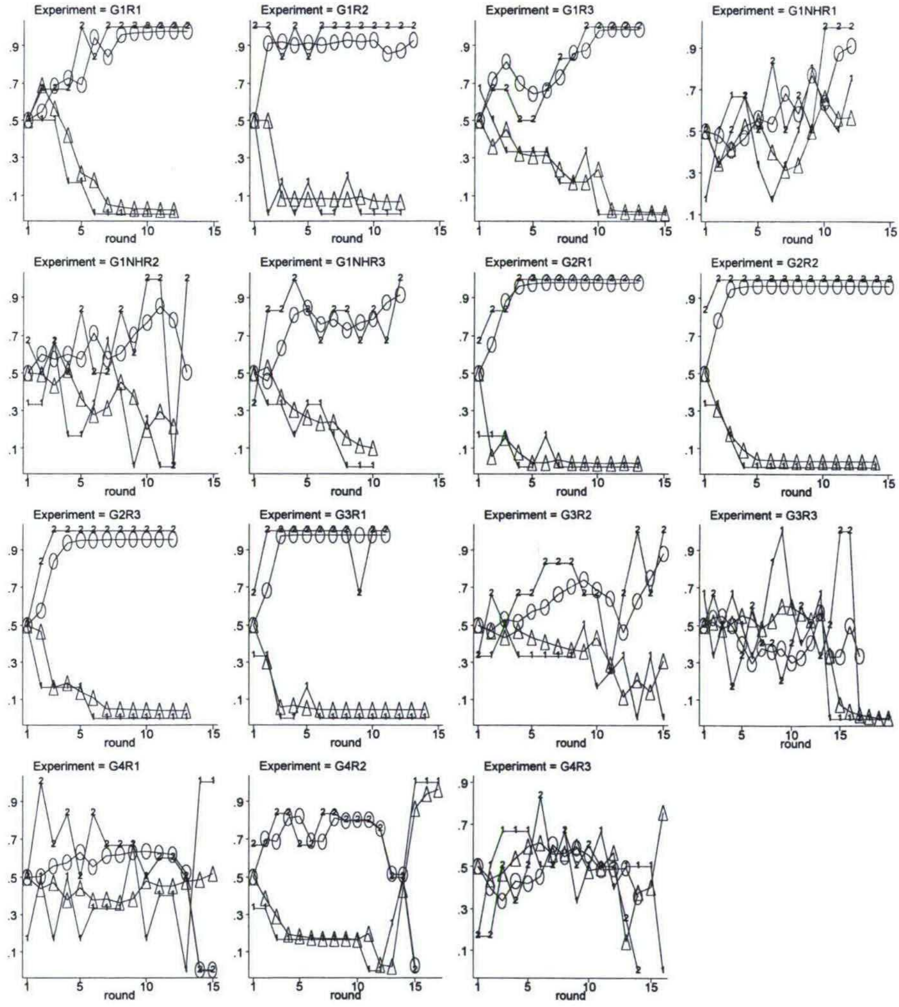


Figure 2.—Plots of actual and predicted fractions of players sending message 2 by type when the BBL model is true.

Panel (a)

$\beta_0 = \varphi_0{}^{SR}$

	Accept BBL	Reject BBL	
Accept SR	7	6	13
Reject SR	2	0	2
	9	6	

Panel(b)

$\varphi_0 = \beta_0{}^{BBL}$

	Accept BBL	Reject BBL	
Accept SR	3	1	4
Reject SR	11	0	11
	14	1	

Panel (c)

β_0, φ_0 unconstrained

	Accept BBL	Reject BBL	
Accept SR	5	3	8
Reject SR	3	4	7
	8	7	

Figure 3. --Summary of testing results for sender data

Panel (a)

$\beta_0 = \varphi_0{}^{SR}$

	Accept BBL	Reject BBL	
Accept SR	2	4	6
Reject SR	0	0	0
	2	4	

Panel(b)

$\varphi_0 = \beta_0{}^{BBL}$

	Accept BBL	Reject BBL	
Accept SR	1	3	4
Reject SR	2	0	2
	3	3	

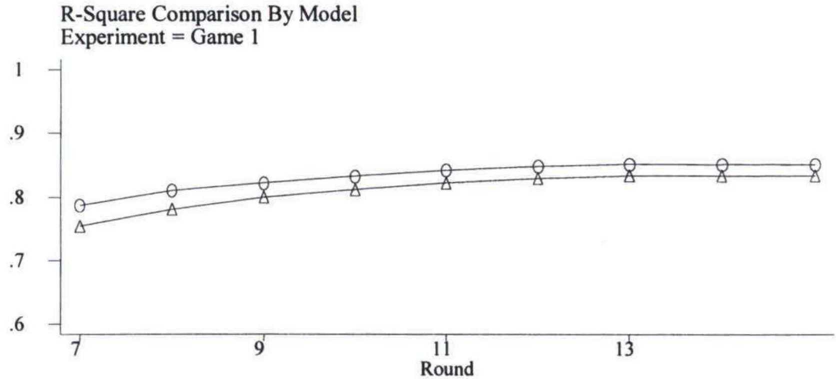
Panel (c)

β_0, φ_0 unconstrained

	Accept BBL	Reject BBL	
Accept SR	0	4	4
Reject SR	2	0	2
	2	4	

Figure 4. --Summary of testing results for receiver data.

Panel (a)



Panel (b)

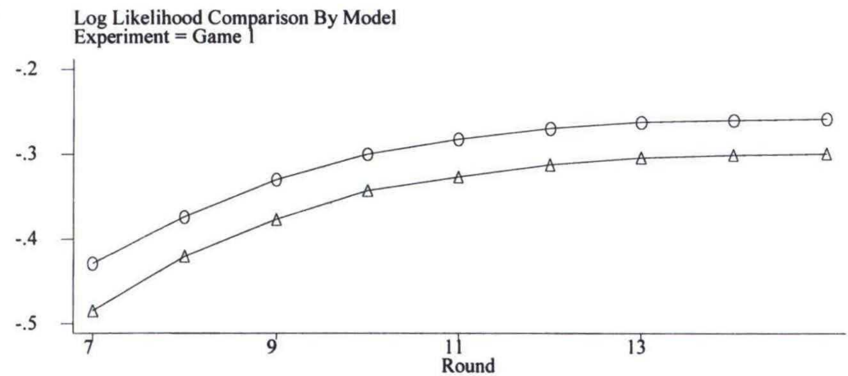


Figure 5—Convergence bias

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