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Experimental evidence on players' models of other players

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Abstract

We pose a hierarchial model of strategic thinking and conduct an experiment to test this theory as well as other solution concepts for symmetric (3×3) games. A level-0 type plays unpredictably, a level-1 type acts as if everyone else were level-0 types, and a level-2 type acts as if all other players were level-0 and level-1 types. In a model with level-0,..., level-2, and Nash types, we estimated that an insignificant portion of the participants were level-0 types, 24% were level-1 types, 49% were level-2 types, and the remaining 27% were Nash types.

Keywords: Game theory; Laboratory experiments; Semi-parametric methods

JEL classification: C70; C72; C90; C91

1. Introduction

We develop and test a theory of human behavior in simple games. The experimental evidence supports the hypothesis that a human player can be characterized by one of four thought-processes that belong to a hierarchy of models of other players, which themselves contain models of other players,.... In contrast to abstract game theory, we provide a stepping stone towards a descriptive and prescriptive theory of games.

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The elegant theories of rational choice developed by Savage (1954) and Anscombe–Aumann (1963) can be applied to multi-decision-maker problems (i.e., games) simply by supposing that each player has a subjective belief about how the other players will behave. The crucial issue is formulating beliefs that are consistent with the knowledge the players have about each other. If it is common knowledge that all players are Bayesian rational, then no player will choose an iteratively strictly dominated strategy (Tan and Werlang, 1988). It takes much stronger assumptions to derive finer predictions. With only a handful of exceptions, game theory has taken as an implicit axiom that all players are super-intelligent: i.e., possessing omnicient powers beyond those supposed in single decision-maker problems. For example, in many games, all players priors and the selection rule for the solution concept must be common knowledge.

Stahl (1993) modeled the evolution of strategic intelligence with player types drawn from a hierarchy of 'smartness' analogous to the levels of iterated rationalizability. In that model, non-rationalizable strategies die out, but when higher levels of smartness incur maintenance costs, 'being right is just as good as being smart': i.e., dumb players who luckily choose the correct strategy do just as well as smart players who use reasoning to discover the correct strategy. Moreover, if a manifest way to play emerges, then dumb players never die out, while smarter players with positive maintenance costs vanish. These results call to question the standard game-theoretic assumption that all players are super-intelligent.

If players are drawn from a population with varying degrees of intelligence, then an intelligent and rational player needs information about the distribution of abilities in the population and the behavioral implications of those abilities. Given a typology of abilities, we can gather empirical data on the population distribution.

Nagel (1993) proposed a hierarchial model for an N-person game in which each player tries to guess ρ times the mean of everyone's choices from the [0,100] interval. A step-k type guesses ρ^k times the previous mean. Her statistical analysis of the first period experimental data supports her model using 50 as the default value for the previous mean.

We extend these hierarchial models as follows. A level-0 type simply chooses a strategy randomly. [1] A level-1 type believes all other players are level-0 types and (invoking the principal of insufficient reason) chooses a best-response to uniform play. A level-2 type believes all other players are level-0 and level-1 types, and chooses a best-response to this subjective belief; etc. Thus, a level-$(n+1)$ type is smarter than a level-n type in being able to think about the behavior of level-n types. However, no player can anticipate what equally smart or smarter players will do. This feature of our model circumvents the need for any player to solve a fixed-point problem and to select from multiple fixed points.

[1] Level-0 players are analogous to the zero-intelligence traders of Gode and Sunder (1993).

The benefits of being smarter depend on the population distribution. For example, if there are four levels {0,1,2,3}, but 99.9% of the population is below level-2, then the level-3 best-response will (almost always) be the same as the level-2 best-response, implying negligible incremental benefits to level-3 types. This example suggests that if higher-level types incur higher maintenance costs, then most of the population may consist of fairly low-level types.

We designed an experimental environment in which we could identify the presence of several types of players in the proposed hierarchial model. Since the type of player should not depend on the specific game, we chose ten different games to control for idiosyncratic features of any one game. Further, since we wanted to focus on the 'model of the world' that players bring to a new strategic setting (instead of focusing on learning-by-doing or the limit behavior after learning [2]), each player played each of the ten games exactly once with no feedback between games. (More experimental details will be given in Section 3).

Our data allow us to test any number of models, including Nash equilibrium theory. Section 2 presents the formal theory and the prediction equations. Section 3 describes the experiment and data. Section 4 presents the statistical analysis, and Section 5 discusses our conclusions.

2. Development of formal theory

Consider a symmetric two-player game with three strategies. Let $j \in \{1,2,3\}$ denote a strategy, and let U_i denote the 3×3 matrix of expected utility payoffs to the row player for game $i \in \{1,2,\ldots,10\}$; U_{ijk} is the payoff to the row player in game i when that player chooses strategy j and the column player chooses strategy k.

Let $p \in \Delta \equiv \{x \in R^3_+ \mid \Sigma_j x_j = 1\}$. Then, $U_i p$ is the (3×1) vector of expected utility payoffs and $U_{ij}\ p$ is the expected payoff of strategy j facing a population whose distribution across strategies is p.

For notational convenience, let $\underline{1}$ denote the uniform distribution over {1,2,3}. We then let $b(\underline{1})$ denote the best response to the uniform distribution, and $b^2(\underline{1}) \equiv b[b(\underline{1})]$ denote the best response to the best response to the uniform distribution.

2.1. An empirical NE theory

A *symmetric Nash equilibrium* (hereafter NE) of game i is a $p^* \in \Delta$ such that (1) $U_{ij} \cdot p^* \leqslant p^* \cdot U_i p^*$ for all j, and (2) $p^*_j > 0$ implies $U_{ij} p^* = p^* \cdot U_i p^*$.

[2] The latter issues are certainly very important, but so is the initial behavior, and there has been relatively little attention devoted to it. Afterall, in a strict interpretation of game theory, given intelligent and rational players for whom a one-shot normal-form game and the rationality of all players is common knowledge, specific experience with the particular one-shot game or the current population of players is irrelevant.

Since we are considering symmetric games, and a single population of players, asymmetric equilibria are inappropriate; more specifically, an asymmetric equilibrium cannot be a 'consistent common belief' in the population of potential players [3].

Note that if a game has a unique pure-strategy NE, and if just one player in an experimental sample fails to choose the NE strategy (as they did), then the log-likelihood of the data will be $-\infty$, which will resoundly reject the NE theory in favor of any theory that yields non-degenerate predictions. To salvage the NE theory, it is necessary to amend it to permit 'unintended' errors in behavior. The simplest such modification is simply to shave ϵ probability mass from the NE prediction and spread it uniformly over all the strategies [4]. Then, ϵ becomes a parameter to estimate from the data.

Let $\mathrm{p}_i^{NE} \in \Delta$ denote the Nash equilibrium strategy vector for game i. Then, the modified NE theory predicts the actual probability of choice as

$$P_{Nij}(\epsilon) \equiv (1-\epsilon)\, p_i^{NE} + \epsilon \underline{1}, \tag{1}$$

for some $\epsilon \in [0,1]$.

2.2. A hierarchy of logit choice models

We hypothesize the existence of three types of players: level 0, 1 and 2. Level-0 types simply choose randomly from a uniform distribution.

Level-1 types assume that $p = \underline{1}$ and compute $U_i\underline{1}$, but they are subject to errors in this computation. Let $\underline{\epsilon}_i$ denote the (3×1) vector of additive computation errors. For tractability, we assume each ϵ_{ij} is independent and identically distributed Weibull noise. Given $\underline{\epsilon}_i$, a level-1 type chooses the strategy corresponding to the largest component of the (3×1) vector $U_i\underline{1} + \underline{\epsilon}_i$. Thus, the choice probabilities have a logit form (McFadden, 1974):

$$P_{1ij}(\gamma_1) = \frac{\exp(\gamma_1 U_{ij}\underline{1})}{\Sigma_k \exp(\gamma_1 U_{ik}\underline{1})}. \tag{2}$$

The parameter γ_1 is the 'precision' of the level 1 type's expected utility calculation.

Level-2 types assume that the population consists of level-0 and level-1 types. The probability distribution over strategies of this subpopulation can be approxi-

[3] For example, in a 2×2 symmetric game with two asymmetric equilibria, say LR and RL, the LR equilibrium entails one player choosing L believing that everyone else will choose R, while the other player chooses R believing that everyone else will choose L. Whenever there are more than two potential players in the population, clearly these beliefs are not compatible with a single common belief throughout the population.

[4] Alternatively, we could have spread the probability over only the strategies that are not strictly dominated. Since no participant ever chose a strictly dominated strategy, this alternative would have yielded better predictions, but the relative comparison with the models below would not have been qualitatively affected.

mated by a single logit model of the form given by (2), in which the presence of more level-0 types is captured by a lower precision parameter. Let μ represent the precision parameter in level-2 type's model of level-0 and level-1 types. Define

$$q_{ij} = \frac{\exp(\mu U_{ij}\underline{1})}{\sum_k \exp(\mu U_{ik}\underline{1})}, \tag{3}$$

and

$$y_{ij} \equiv \sum_k U_{ijk} q_{ik}. \tag{4}$$

In other words, for game i, q_i represents a level-2 type's subjective belief about other player's choices, and y_i is the associated vector of expected utility payoffs. We assume that a level-2 type chooses the strategy corresponding to the largest component of $y_i + \underline{\epsilon}_i$ where $\underline{\epsilon}_i$ is Weibull-distributed noise. Thus, the choice probabilities have a logit form:

$$P_{2ij}(\mu,\gamma_2) = \frac{\exp(\gamma_2 y_{ij})}{\sum_k \exp(\gamma_2 y_{ik})}. \tag{5}$$

The parameter γ_2 is the 'precision' of the level-2 type's expected utility calculation. The parameters that specify level-2 types are $\{\mu,\gamma_2\}$.

2.3. Aggregating the hierarchy

Let $s(h,i) \in \{1,2,3\}$ denote the strategy chosen by participant h in game i, and let $s^h \equiv \{s(h,i),\ i = 1,\ldots,10\}$ denote the joint choices of participant h. Assuming that a participant's type is fixed for all games, the probability of participant h's joint choices conditional on being a level-k type ($k = 1,2$) is given by

$$P_k^h(\gamma,\mu) \equiv \prod_i P_{kis(h,i)}, \tag{6}$$

where $\gamma \equiv (\gamma_1,\gamma_2)$.

The actual population of players can consist of all three types. Let α_0 denote the proportion of the population that is level-0, and let α_1 denote the proportion that is level-1; hence, $(1 - \alpha_0 - \alpha_1)$ is the proportion that is level-2. We let $\alpha \equiv (\alpha_0,\alpha_1)$. Then, the ex ante likelihood of participant h's joint choices is given by

$$L(s^h \mid \alpha,\gamma,\mu) \equiv \alpha_0\underline{1} + \alpha_1 P_1^h(\gamma,\mu) + (1 - \alpha_0 - \alpha_1) P_2^h(\gamma,\mu), \tag{7}$$

and the log-likelihood used in estimation is

$$L \equiv \sum_h \log[L(s^h \mid \alpha,\gamma,\mu)].$$

3. The experimental design

Ten symmetric (3×3) games were selected with a variety of characteristics: three were strict dominance solvable, two were weak dominance solvable, six had unique pure-strategy NE while two had unique mixed-strategy NE, and two had multiple NE (but all but one of the NE were iteratively weakly dominated). The purpose of this variety was to allow us to identify general behavioral patterns rather than only specific behavior dependent on a specific game. The payoff matrices for the row player are presented in Table 1; the transposes of these matrices give the payoffs for the column player.

A participant chose a single pure strategy for each game (always as a row player). Then, each participant's choices were matched with every other participant. For each game, a participant's average payoff points were computed, and this number gave the percentage chance of winning \$2.50 for that game. A random number was generated from a uniform distribution on [0,100], and the player won \$2.50 if and only if the random number did not exceed his/her point score. Each participant was essentially 'playing against the field': i.e., against the empirical

Table 1
Games used in experiment

Game		'T'	'M'	'B'	Game		'T'	'M'	'B'
		<u>11</u>	<u>0</u>	<u>29</u>			<u>26</u>	<u>7</u>	<u>7</u>
	T	40	10	70		T	20	0	100
1	M	20	80	0	2	M	60	20	0
	B	30	100	60		B	0	60	40
		<u>14</u>	<u>26</u>	<u>0</u>			<u>0</u>	<u>27</u>	<u>13</u>
	T	20	30	100		T	50	0	20
3	M	40	40	60	4	M	50	10	100
	B	30	0	40		B	40	40	40
		<u>18</u>	<u>0</u>	<u>22</u>			<u>4</u>	<u>35</u>	<u>1</u>
	T	10	100	20		T	30	0	100
5	M	0	70	30	6	M	40	40	80
	B	20	50	40		B	50	20	40
		<u>6</u>	<u>31</u>	<u>3</u>			<u>8</u>	<u>13</u>	<u>19</u>
	T	40	20	0		T	10	100	0
7	M	30	20	100	8	M	0	60	70
	B	20	10	100		B	90	40	20
		<u>26</u>	<u>0</u>	<u>14</u>			<u>5</u>	<u>0</u>	<u>35</u>
	T	30	100	40		T	40	100	10
9	M	20	60	40	10	M	60	50	30
	B	80	0	40		B	50	80	40

distribution of choices made by all other participants. [5] The experiment was conducted in sessions of one to ten participants over a two-week period, and actual payments were delayed until all the experimental data was in. This method of computing payoffs and random money winnings was explained fully to all participants (complete experiment protocol available from the authors upon request).

The participants consisted of 40 business or economic seniors and graduate students at the University of Texas who had not previously studied game theory. Each player was given an information sheet to study and some exercises to perform one day prior to the experiment. At the beginning of a session, each participant was given a 10 minute screening test (similar to the exercises). This screening test was designed to eliminate potential participants who did not understand the basics of the games and to instill common knowledge among all participants that everyone did understand these basics. While everyone who took the screening test passed, only 20% of the invitees showed up to take the test, so there was considerable self-selection.

Each participant was given up to 40 minutes to complete their choices for the ten games. Actual time used ranged from 10 to 30 minutes. Monetary payoffs ranged from $2.50 to $17.50, with an average of $8.56.

4. The experimental data

The aggregate data are given in Table 1; the total number of participants making a particular choice are the underlined numbers above the respective payoff matrix. The disaggregate data are presented in Table 2. The participants have been ordered from the highest point score to the lowest. The average point score was 37.25 (s.d. = 4.24). Remarkably, eight participants (#24–31) behaved identically in all ten games. Also, quite remarkably, except for three participants in Game 7, no participant chose a (strictly or weakly) dominated strategy. [6]

On the other hand, participants did not solve games by iterative elimination of strictly dominated pure strategies. In games 1 and 3, this procedure leads to a unique choice (T and M respectively), but 29 and 14 participants (respectively) did not choose the iteratively undominated strategy. In game 5, M is dominated by a mixture of T and B, and after eliminating M, B dominates T; while no participant chose M, 18 participants chose T instead of B.

[5] While pure theory would hold that playing against a single opponent randomly selected from the population is equivalent to playing against the field, we felt that the latter protocol would reinforce the inappropriateness of asymmetric equilibria. Friedman (1993) finds very little difference between the two protocols in learning-by-doing experiments; if anything the limiting behavior is slightly more Nash-like when playing against the field.

[6] In the exceptional Game 7, B is weakly dominated, but is also the Pareto superior NE.

Table 2
Experiment data

h	1	2	3	4	5	6	7	8	9	10	Points	#$b(1)$ Dev.	#$b^2(1)$ Dev.	#NE Dev.
1	T	M	M	M	B	M	M	M	B	B	44.30	6	1	0
2	T	M	M	B	B	M	T	M	B	B	43.78	8	1	0
3	T	M	M	B	B	M	T	M	B	B	43.78	8	1	0
4	T	T	M	M	B	M	M	M	B	B	43.10	5	2	0
5	T	T	M	B	B	M	T	M	B	B	42.58	7	2	0
6	T	M	M	B	B	M	M	T	B	B	42.55	7	1	0
7	B	M	M	M	B	M	B	M	B	B	42.38	6	3	1
8	T	T	M	B	B	M	B	M	B	B	42.25	7	2	0
9	B	T	M	M	B	M	M	M	B	B	42.10	4	3	1
10	B	T	M	M	B	M	M	M	B	B	42.10	4	3	1
11	T	T	M	B	B	M	T	B	B	B	41.35	6	3	0
12	T	M	M	B	B	M	M	B	T	B	39.90	5	2	1
13	B	T	M	M	T	M	M	B	B	B	39.33	2	5	2
14	B	T	M	M	B	M	M	M	T	B	38.85	3	4	2
15	B	T	M	M	B	M	M	M	T	B	38.85	3	4	2
16	T	B	M	M	B	M	M	M	T	B	38.55	5	3	1
17	T	B	M	B	B	M	T	M	T	B	38.03	7	3	1
18	B	T	M	B	B	M	M	B	T	B	37.70	3	4	2
19	B	T	M	B	B	M	M	B	T	B	37.70	3	4	2
20	B	T	M	M	B	M	M	T	T	B	37.03	3	5	2
21	B	B	M	B	B	M	M	B	T	B	36.40	4	4	2
22	B	B	M	M	B	M	M	B	T	B	36.33	3	5	2
23	B	B	M	M	T	M	M	B	T	B	34.78	2	6	3
24	B	T	T	M	T	M	M	B	T	B	34.73	0	7	4
25	B	T	T	M	T	M	M	B	T	B	34.73	0	7	4
26	B	T	T	M	T	M	M	B	T	B	34.73	0	7	4
27	B	T	T	M	T	M	M	B	T	B	34.73	0	7	4
28	B	T	T	M	T	M	M	B	T	B	34.73	0	7	4
29	B	T	T	M	T	M	M	B	T	B	34.73	0	7	4
30	B	T	T	M	T	M	M	B	T	B	34.73	0	7	4
31	B	T	T	M	T	M	M	B	T	B	34.73	0	7	4
32	B	T	M	B	T	B	M	T	B	T	34.30	6	6	4
33	B	T	T	M	T	M	M	T	T	B	34.13	1	7	4
34	B	T	T	M	T	M	T	T	B	T	34.03	4	8	4
35	B	B	T	M	T	M	M	B	T	B	33.43	1	7	4
36	B	B	T	M	T	M	M	B	T	B	33.43	1	7	4
37	B	T	M	B	B	T	B	B	T	B	33.23	5	6	3
38	B	M	M	M	T	T	M	T	T	T	30.38	5	7	5
39	B	T	T	M	T	T	M	T	T	T	27.83	3	9	6
40	B	T	T	M	T	T	M	T	T	T	27.83	3	9	6
$b(1)$	B	T	T	M	T	M	M	B	T	B	34.73		7	4
$b^2(1)$	T	M	M	B	B	M	M	M	B	B	44.38	7		0
NE	T	TMB	M	MB	B	M	TMB	TMB	B	B	41.92	4	0	

Similarly, participants did not solve games by iterative elimination of weakly dominated strategies. In addition to the above games, games 7 and 9 possess a unique 'iteratively admissible' strategy (T and B respectively); however, 34 and 26 participants (respectively) failed to chose the iteratively admissible strategy.

This is conclusive evidence that while participants avoided strategies dominated with respect to all three strategies, their model of other players did not incorporate this behavior.

In the six games with a unique NE, 40.4% of the responses differed from the unique NE. In the two games with a unique mixed-strategy NE (2 and 8), the empirical distribution differs from the NE distribution at the 5% significance level. In game 4 with a weakly dominated pure-strategy NE (i.e., T) and a mixed-strategy NE (2/3M + 1/3B), the empirical distribution is insignificantly different from the mixed NE at the 5% level. The remaining game (7) has three pure-strategy NE, so all observed behavior is consistent with some NE. On the other hand, it is interesting that 31 participants chose the Pareto-inferior NE, only 6 participants chose the iteratively admissible NE, and 3 participants chose the weakly dominated (albeit Pareto-superior) NE.

At the bottom of Table 2 we list $b(\underline{1})$, $b^2(\underline{1})$, as well as the support of the NE. The last three columns give the numbers of deviations from $b(\underline{1})$, $b^2(\underline{1})$, and the support of the NE respectively. It is interesting that the lowest performing participants had the fewest deviations from $b(\underline{1})$, while the highest performing participants had the fewest deviations from $b^2(\underline{1})$ and NE.

5. Statistical analysis

We first analyze the modified NE model and then the aggregate logit model with level-0, level-1 and level-2 types. Log-likelihood functions were maximized using the simplex method of Nelder and Mead (1965). The method requires only function evaluations; although it is not very efficient in terms of the number of function evaluations required, the computational burden was small given the number of observations in our sample; the method is much easier to implement than other algorithms such as the EM-algorithm.

Nonparametric confidence intervals for parameter estimates were estimated using the bootstrap percentile method described by Efron (1982, chapter 10). Initially popularized by Efron (1979), the bootstrap is a nonparametric method of computing estimated parameter covariances and confidence intervals by repeatedly applying the original estimator to randomly selected pseudo data sets constructed by simulating the error process. In models of the form $y_i = f(x_i \mid \beta) + \xi_i$, where x_i represents a vector of covariates for the ith observation, β is an appropriately dimensioned parameter vector, and ξ_i is a random error term, the pseudo data sets are generated using the empirical distribution of the fitted residuals, which approximates the theoretical distribution of the true residuals. The bootstrap has been discussed by Efron (1979), Efron (1982), Wu (1986), Liu and Singh (1992), Atkinson and Wilson (1992), and others. The bootstrap procedure is particularly advantageous in our setting because it allows us to estimate the posterior probabilities that each player is of a particular type using a semi-parametric Bayesian procedure as discussed below.

Since the modified NE model is a discrete choice model, and the aggregate logit model is a finite mixture of discrete choice models, residual terms are not explicitly estimated; consequently, the simulation of the pseudo data required for the bootstrap requires some modification. In the case of the modified NE model, we simulate pseudo data s_*^h by first estimating the log-likelihood to obtain (a vector of) parameter estimates $\hat{\beta}$. Next, uniform [0,1] pseudo random deviates are generated to simulate a choice by each player on each of the 10 games in the experiment using the transformation method. For example, if ρ_1, ρ_2, ρ_3 denote the probabilities of choosing strategies 1, 2, and 3, respectively, computed using $\hat{\beta}$, then a pseudo random deviate υ is used to simulate a choice of strategy 1 if $\upsilon \leqslant \rho_1$, a choice of strategy 2 if $\rho_1 < \upsilon \leqslant (\rho_1 + \rho_2)$, or a choice of strategy 3 if $(\rho_1 + \rho_2) < \upsilon \leqslant 1$.

For the aggregate logit model, the pseudo-data s_*^h are simulated similarly. For each player, a uniform [0,1] pseudo random deviate is first generated and compared to the estimates of α_0 and α_1 to determine player type for the simulated data. Next, uniform [0,1] pseudo random deviates are generated to determine a choice by each player on each of the 10 games in the experiment, using the estimated parameter values for each component of the mixture model as described above.

Once a complete set of choices s_*^h have been simulated, the model is reestimated using these pseudo data to obtain a bootstrap estimate $\hat{\beta}^*$. Then the process is repeated a large number of times to produce M estimates: $\{\hat{\beta}^*(m)\}_{m=1}^{M}$. The bootstrap estimates $\hat{\beta}^*$ then approximate the sampling distribution of the original estimator, $\hat{\beta}$. Let $\hat{\beta}_j$ and $\hat{\beta}_j^*(m)$ denote the jth elements of $\hat{\beta}$ and $\hat{\beta}^*(m)$, respectively. Nonparametric confidence intervals for $\hat{\beta}_j$ are obtained by sorting $\{\hat{\beta}_j^*(m)\}_{m=1}^{M}$ by algebraic value and then deleting the appropriate number of values from each end of the resulting sorted array. If 95 percent confidence intervals are desired, then $0.025 \times M$ values would be deleted from each end of the sorted array; the new endpoints give the confidence interval. In the results reported below, we choose $M = 1000$ to ensure adequate coverage. Throughout, we use the multiplicative congruential method for generating uniform pseudo random deviates, with modulus $(2^{31} - 1)$ and multiplier 7^5.

Of course, one could compute the Fisher information matrix for the models estimated below to obtain variance estimates of the parameters. However, interpretation of t-ratios obtained from these estimates is problematic due to the proximity of the parameter estimates to parameter-space boundaries. Also, as discussed below, the bootstrap is necessary for the semi-parametric Bayesian estimation of the posterior probabilities that each player is of a particular type.

5.1. Estimation of the modified NE model

Before we do any estimation, we need to specify what NE theory should predict about the games with multiple NE. In Game 4, there are two NE: T and a mixed-strategy (2/3M + 1/3B), but T is weakly dominated, so it is natural to predict the mixed-strategy. Indeed, no participant chose T; we, henceforth, specify (2/3M + 1/3B) as the NE prediction for Game 4.

In Game 7, there are three pure-strategy NE, but B is weakly dominated, and deleting the third strategy then M is weakly dominated, so it is natural to predict T. On the other hand, while B is weakly dominated, it is also the Pareto superior NE, so predicting T is not as compelling. We opt to let the data tell us how the participants spread themselves across the three strategies.

Let a denote the probability that the participant believes T, let b denote the probability that the participant believes M, and let $(1-a-b)$ denote the probability that the participant believes B. Then, the predicted probability of T being chosen is $a(1-2\epsilon/3)+b(\epsilon/3)+(1-a-b)(\epsilon/3)$, etc. [7]

Maximizing the log-likelihood corresponding to the NE model using the entire sample of 40 participants produces the parameter estimates shown in column *I* of Table 3. The confidence intervals were computed at the 95 percent level using the bootstrap procedure outlined above. Since over 50% weight is put on the 'error' term ϵ, we interpret this result as a strong rejection of the modified NE theory. [8]

5.2. Estimation of the aggregate logit model

The finite mixture model in (7) was estimated by maximizing the corresponding log-likelihood function with the entire sample of 40 participants; the results are shown in column *II* of Table 3. The mixture parameters α_0 and α_1 were restricted to lie between 0 and 1. While α_0 is estimated at zero, the 95 percent bootstrap confidence interval suggests α_1 is significantly different from either 0 or 1.

Restricting $\alpha_1 = 0$ and re-estimating the model yields a log-likelihood of -309.367; computing the likelihood-ratio statistic $\hat{\lambda} = -2(L_R - L_F)$, where L_R and L_F denote the maximized values of the restricted and unrestricted log-likelihoods, respectively, yields $\hat{\lambda} = 68.572$. However, this statistic has unknown distribution under the null hypothesis since the null value of α_1 is on the boundary of the parameter space (see Everitt and Hand, 1981 and Titterington et al., 1985 for discussions of this problem in the context of finite mixture models).

[7] Of course, since the number of parameters equals the degrees of freedom for Game 7, we will never be able to reject NE behavior in this game alone. It is only by hypothesizing identical behavioral rules for all games that we have the possibility of rejecting NE behavior.

[8] While restricting the noise to undominated strategies obviously improves the loglikelihood, it actually causes the estimate of ϵ to increase, thereby making the NE theory appear worse.

Table 3
Results of estimation
(Bootstrapped 95 percent confidence intervals in brackets)

	I NE	II 0, 1, 2	III 0, 1, 2, NE
ϵ	0.530		0.0603
	[0.451,0.612]		[0.000,0.143]
a	0.0		0.473
	[0.000,0.000]		[0.163,0.815]
b	1.0		0.415
	[1.000,1.000]		[0.0863,0.732]
γ_1		0.199	0.538
		[0.163,0.246]	[0.340,5.000]
μ		0.112	0.0216
		[0.0742,0.181]	[0.0092,0.0343]
γ_2		0.0819	0.142
		[0.0600,0.117]	[0.110,0.181]
α_0		0.000	0.000
		[0.000,0.00284]	[0.000,0.000203]
α_1		0.601	0.242
		[0.438,0.745]	[0.105,0.425]
α_2		0.489	
			[0.298,0.661]
L	−379.506	−275.081	−245.396

Conventional Wald and Lagrange multiplier tests also break down at the edge of the parameter space. Consequently, we use a bootstrap procedure to approximate the sampling distribution of our likelihood-ratio statistic and hence determine a significance level.

Restricting $\alpha_1 = 0$ and reestimating the model yields a vector $\hat{\beta}_R$ of restricted parameter estimates and our likelihood-ratio statistic $\hat{\lambda}$. The choice data are then simulated as outlined for the bootstrap procedure above, except we now use $\hat{\beta}_R$ in the simulation so that the data are generated under the null hypothesis. Next, both the restricted and unrestricted models are estimated on these pseudo data, yielding log-likelihood values L_R^* and L_F^*, respectively. These values are then used to compute a bootstrap estimate $\hat{\lambda}^*$ of the likelihood-ratio statistic. This process was repeated 1000 times to produce bootstrap estimates $\{\hat{\lambda}^*(m)\}_{m=1}^{1000}$. Since these values approximate the sampling distribution of $\hat{\lambda}$, it is straightforward to determine the significance of the original likelihood-ratio statistic by first sorting the bootstrap estimates by algebraic value and then determining the percentile of the original statistic. The value 68.572 turns out to be significant at greater than 99.9 percent. Thus, we reject the null hypothesis $\alpha_1 = 0$ and conclude that level-1 players are indeed present.

Similarly, we restrict $\alpha_0 + \alpha_1 = 1$ to test the null hypothesis that no level-2 players are present in the data. Re-estimating the model with this restriction yields a value for the likelihood-ratio statistic of 86.514. Applying the above bootstrap procedure to approximate the sampling distribution of the likelihood-ratio statistic indicates this value is significant at greater than 99.9 percent. Hence, the results suggest that level-1 and level-2 players, but not level-0 players, are represented in our sample.

Given the sufficiency of the parameter estimates $\hat{\alpha}_0, \hat{\alpha}_1, \hat{\alpha}_1, \hat{\mu}$, and $\hat{\gamma}_2$, Bayes' theorem can be used to derive the posterior probability that any participant h is level-k ($k = 0,1,2$), which we denote as α_k^h. For notational convenience, let $s = \{s^h,\ h = 1,\ldots,40\}$, the vector of all observations, and $\beta = [\hat{\alpha}_0,\ \hat{\alpha}_1,\ \hat{\gamma}_1,\ \hat{\mu},\ \hat{\gamma}_2]$, the vector of model parameters. Let $L(s \mid \beta)$ denote the likelihood of the observed data s conditional on the model parameters β, and let $f(\beta)$ denote the joint density of β. Then $\alpha_k^h \equiv L(\text{player h is level-}k \mid s)$, and by Bayes' theorem,

$$\alpha_k^h = \frac{L(\text{player } h \text{ is level-}k \text{ and } s)}{L(s)}. \tag{8}$$

Conditional probabilities can always be integrated to yield unconditional probabilities, and hence

$$L(s) = \int \ldots \int L(s \mid \beta) f(\beta)\, \mathrm{d}\beta. \tag{9}$$

Recall that $L(s_h \mid \beta)$ denotes the joint probability of observations corresponding to player h. Then $L(s \mid \beta) = \prod_{h=1}^{40} L(s^h \mid \beta)$, and

$$\begin{aligned} L[(\text{player } h \text{ is level-}k \text{ and } s) \mid s] &= \alpha_k P_k^h \prod_{j \neq h} L(s^j \mid \beta) \\ &= \alpha_k P_k^h L(s \mid \beta) / L(s^h \mid \beta), \end{aligned} \tag{10}$$

and so

$$\alpha_k^h = \int \ldots \int \frac{\alpha_k P_k^h L(s \mid \beta)}{L(s^h \mid \beta) L(s)} \cdot f(\beta)\, \mathrm{d}\beta. \tag{11}$$

The bootstrap procedure used to obtain confidence intervals for the parameter estimates provides an approximation to the sampling distribution of β which is used to perform the integration in (9) and (11). The integral in (9) is approximated by computing $L(s \mid \beta)$ for each of the bootstrap replications and averaging; the integral in (11) is computed similarly, using the value for $L(s)$ obtained from (9). Consequently, the estimation of the α_k^h amounts to a semi-parametric Bayesian procedure; the procedure is semi-parametric since the functional form of $f(\beta)$ is not specified. The procedure is more general than a fully parametric procedure that would require specification of $f(\beta)$.

The estimated posterior probabilities α_k^h are presented in Table 4. In this table, we also present the predicted log-likelihood of each participant's actual choices. (Note that the log-likelihood of level-0 types is -10.986, independent of the

Table 4
Predictions of aggregate logit model

h	$\log(P_1^h)$	$\log(P_2^h)$	α_1^h	α_2^h
1	−16.16	−5.70	0.000	1.000
2	−24.79	−7.20	0.000	1.000
3	−24.79	−7.20	0.000	1.000
4	−13.50	−5.56	0.001	0.999
5	−22.13	−7.06	0.000	1.000
6	−20.14	−7.00	0.000	1.000
7	−12.84	−6.82	0.004	0.996
8	−17.49	−5.07	0.000	1.000
9	−8.86	−6.12	0.096	0.904
10	−8.86	−6.12	0.096	0.904
11	−20.80	−8.96	0.000	0.999
12	−14.17	−8.71	0.009	0.991
13	−6.20	−8.16	0.901	0.099
14	−5.54	−8.29	0.950	0.050
15	−5.54	−8.29	0.950	0.050
16	−11.51	−9.41	0.185	0.814
17	−20.14	−10.91	0.000	0.996
18	−6.87	−9.13	0.924	0.076
19	−6.87	−9.13	0.924	0.076
20	−6.87	−10.64	0.980	0.020
21	−8.19	−10.82	0.943	0.056
22	−5.54	−11.87	0.998	0.002
23	−4.21	−12.01	1.000	0.000
24	−2.22	−11.50	1.000	0.000
25	−2.22	−11.50	1.000	0.000
26	−2.22	−11.50	1.000	0.000
27	−2.22	−11.50	1.000	0.000
28	−2.22	−11.50	1.000	0.000
29	−2.22	−11.50	1.000	0.000
30	−2.22	−11.50	1.000	0.000
31	−2.22	−11.50	1.000	0.000
32	−16.16	−10.12	0.006	0.992
33	−4.88	−11.96	0.999	0.001
34	−15.49	−13.63	0.237	0.742
35	−3.55	−13.19	1.000	0.000
36	−3.55	−13.19	1.000	0.000
37	−10.19	−11.91	0.871	0.128
38	−11.51	−14.43	0.944	0.052
39	−8.20	−15.47	0.999	0.001
40	−8.20	−15.47	0.999	0.001

actual choices.) It is interesting that 37 of the 40 participants have a 90% or better probability of being one type. Of these, 14 are level-2 types and 23 are level-1 types. Since α_0 is virtually zero, the posteriors of level-0 types (α_0^h) are less than 0.01 (and hence not reported in Table 4), except $\alpha_0^{34} = 0.021$.

5.3. An alternative hybrid model

A casual comparison of the last and second from last columns of Table 2 suggests that the top performing participants might be better described as 'NE types' rather that level-2 types. We can address this hypothesis by adding NE types to the previous aggregate model. Let α_2 denote the proportion of the population that is level-2 types, so $(1-\alpha_0-\alpha_1-\alpha_2)$ is the proportion of the population that is NE types, and the joint probability of the choices of a NE type $P_N^h(\epsilon,a,b)$ is defined analogously to (6). Then the probability of participant h's joint choices is given by

$$Pr(s^h \mid \alpha,\gamma,\mu,\epsilon,a,b) \equiv \alpha_0\underline{1} + \alpha_1 P_1^h + \alpha_2 P_2^h + (1-\alpha_0-\alpha_1-\alpha_2)P_N^h. \tag{12}$$

Maximizing the corresponding log-likelihood function for the 40 participants in the sample yields the parameter estimates shown in column *III* of Table 3. The model in column *II* represents a restricted version of this model in which no players are NE types. Computing the corresponding likelihood-ratio statistic produces $\hat{\lambda}=59.370$; the bootstrap approximation of the sampling distribution of $\hat{\lambda}$ for this restriction suggests that the likelihood-ratio statistic is significant at greater than 99.9 percent. Hence, the restriction is easily rejected. Alternatively, restricting $\alpha_2=0$ yields $\hat{\lambda}=21.804$. Applying the bootstrap procedure indicates this value is significant at greater than 99.9 percent, and hence the null hypothesis that no players are level-2 types is rejected. Restricting $\alpha_1=0$ yields $\hat{\lambda}=20.188$; again, applying the bootstrap procedure indicates this value is significant at greater than 99.9 percent, and the null hypothesis that no players are level-1 types is also soundly rejected.

The bootstrapped confidence interval for $\hat{\epsilon}$ in Table 3 includes 0. To further test the significance of $\hat{\epsilon}$, we estimated the model while imposing the restriction $\epsilon=0$ to obtain a likelihood-ratio statistic $\hat{\lambda}=8.876$. Bootstrapping this statistic along the lines employed in testing the significance of the mixture parameters, we find that $\hat{\lambda}$ is significant at greater than 99 percent.

The addition of NE types to the aggregate logit model causes the estimated parameters for level-1 and level-2 types to change dramatically. The precision of level-1 types increases substantially (from 0.199 to 0.538), predicting that level-1 types choose $b(\underline{1})$ with very little error. The level-2 parameter for a level-2 type's belief about level-1's precision has decreased substantially (from 0.112 to 0.022) and is 25 times less than the value of γ_1. One interpretation is that level-2 types believe that a large proportion of the population are level-0 types, which is captured by a low precision parameter μ. We also find that the precision of level-2 types (γ_2) increases (from 0.082 to 0.142), but is substantially less than γ_1. One interpretation is that, since level-2 thinking involves two levels of computation, errors can be compounded resulting in an overall lower precision.

Given the true values of (α,γ,μ), Bayes' rule can again be used to derive the posterior probability that any participant h is level-k ($k=0,1,2,N$), which we

Table 5
Predictions of hybrid model

h	$\log(P_1^h)$	$\log(P_2^h)$	$\log(P_{NE}^h)$	α_1^h	α_2^h	α_N^h
1	−37.96	−9.64	−3.40	0.000	0.005	0.995
2	−61.27	−15.76	−3.94	0.000	0.000	1.000
3	−61.27	−15.76	−3.94	0.000	0.000	1.000
4	−30.79	−7.92	−3.32	0.000	0.022	0.978
5	−54.10	−14.04	−3.86	0.000	0.000	1.000
6	−48.72	−12.43	−4.57	0.000	0.001	0.999
7	−29.00	−8.47	−8.46	0.000	0.636	0.364
8	−41.55	−10.10	−5.17	0.000	0.027	0.973
9	−18.24	−5.85	−7.19	0.000	0.856	0.144
10	−18.24	−5.85	−7.19	0.000	0.856	0.144
11	−50.51	−14.03	−4.26	0.000	0.000	1.000
12	−32.58	−9.72	−8.33	0.000	0.325	0.675
13	−11.06	−5.15	−11.45	0.004	0.994	0.001
14	−9.27	−4.67	−11.05	0.011	0.988	0.001
15	−9.27	−4.67	−11.05	0.011	0.988	0.001
16	−25.41	−8.23	−7.69	0.000	0.516	0.484
17	−48.72	−14.33	−8.23	0.000	0.007	0.993
18	−12.86	−5.93	−12.12	0.002	0.996	0.002
19	−12.86	−5.93	−12.12	0.002	0.996	0.002
20	−12.86	−6.19	−11.56	0.003	0.993	0.004
21	−16.44	−7.40	−12.62	0.001	0.995	0.004
22	−9.27	−6.13	−11.96	0.043	0.955	0.002
23	−5.69	−5.44	−15.82	0.239	0.761	0.000
24	−0.31	−4.36	−19.19	0.960	0.040	0.000
25	−0.31	−4.36	−19.19	0.960	0.040	0.000
26	−0.31	−4.36	−19.19	0.960	0.040	0.000
27	−0.31	−4.36	−19.19	0.960	0.040	0.000
28	−0.31	−4.36	−19.19	0.960	0.040	0.000
29	−0.31	−4.36	−19.19	0.960	0.040	0.000
30	−0.31	−4.36	−19.19	0.960	0.040	0.000
31	−0.31	−4.36	−19.19	0.960	0.040	0.000
32	−37.96	−11.41	−19.95	0.000	0.997	0.000
33	−7.48	−5.89	−19.29	0.114	0.886	0.000
34	−36.17	−13.12	−19.17	0.000	0.984	0.005
35	−3.89	−5.83	−19.69	0.573	0.427	0.000
36	−3.89	−5.83	−19.69	0.573	0.427	0.000
37	−21.82	−8.78	−17.17	0.000	0.999	0.000
38	−25.41	−10.36	−23.24	0.000	0.999	0.000
39	−16.44	−9.03	−27.02	0.005	0.995	0.000
40	−16.44	−9.03	−27.02	0.005	0.995	0.000

continue to denote as α_k^h. The estimated posterior probabilities were estimated as in the previous section and are presented in Table 5.

It is interesting that 35 of the 40 participants have a 76% or better probability of being one type. Of these, 9 are NE types, 18 are level-2 types and 8 are level-1 types. Since α_0 is virtually zero, the posteriors of level-0 types (α_0^h) are less than 0.01 (and hence not reported in Table 4), except $\alpha_0^{34} = 0.011$.

Table 5 reveals that indeed the level-1 model predicts the choices of the eight participants whose choices correspond exactly to $b(\underline{1})$ and two other participants with only one deviation each from $b(\underline{1})$. The NE model predicts the behavior of many of the top-performing participants (which the previous model classified as level-2 types). The level-2 model, therefore, captures the other players choices, but since these choices involve numerous deviations from $b^2(\underline{1})$, the model parameters are substantially different from those of the previous aggregate model.

6. Conclusions

We have posed a hierarchial model of strategic thinking. A level-0 type plays unpredictably, a level-1 type chooses a best-response to a uniform distribution (as if everyone else were level-0 types). A level-2 type models everyone else as level-0 and level-1 types and chooses a best-response.

We conducted an experiment to estimate the parameters of this hierarchal model and test the theory. We found that the participants avoided dominated strategies, but did not assume that everyone else would avoid dominated strategies. NE theory was a poor predictor of overall behavior.

The patterns of behavior across the ten games is suggestive of different modes of thinking. Remarkably, 20% of the participants chose precisely the best-response to the uniform distribution in all ten games. However, we rejected the hypothesis that all participants were level-1 types in favor of the alternative hypothesis of level-2 thinking. Estimating an aggregate logit model with level-k ($k = 0,1,2$) types only, we concluded that at most one of the participants was a level-0 type, 60% were level-1 types, and the remainder were level-2 types, conditional on our hierarchial logit model.

On the other hand casual observation of the data suggested that NE behavior might be a better predictor of the level-2 group than the level-2 model itself. Conditional on the population consisting of level-k ($k = 0,1,2,NE$), we concluded that an insignificant portion of the participants were level-0 types, 24% were level-1 types, 49% were level-2 types, and the remaining 27% were NE types. This alternative hybrid model was a much better predictor of the experimental data.

Despite the fact that as many as 27% of the participants played consistent with NE theory, that behavior was not best against the population distribution. The best-response behavior against the sample population turned out to coincide with the best-response to the best-response to the uniform distribution, $b^2(\underline{1})$, yielding a payoff score of 44.375. In contrast, the expected score from error-free NE behavior was 41.92, and the expected score from the estimated NE model was 41.345, almost a standard deviation below the best. The fact that 8 of the 9 participants in the ex-post NE group scored better than 41.345 suggests that NE thinking does not fully capture the thought processes of these participants.

It is also interesting that the spread between the best response and the level-1 average score amounted to only $2.40 in expected payoff. Thus, the actual penalty for level-1 behavior was small, even though each game had the potential for a 0% to a 100% lottery. In a subsequent experiment (Stahl and Wilson, 1994), we forced all participants to use the same amount of time in order to control for the opportunity cost of time, and we found similar results. Therefore, we do not believe that the level-1 behavior we observed was due to the participants rationally deciding that the opportunity cost of their time exceeded the expected gain from higher-level thinking.

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