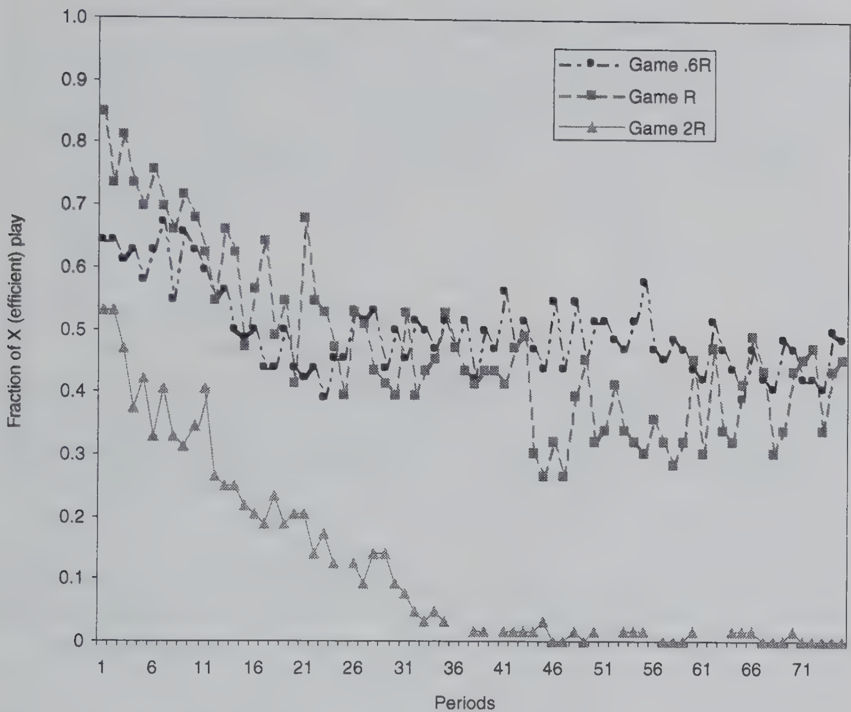


Figure 6.3. Percentage of efficient (X) play in stag hunt games with different response gradients. Source: Based on data from Battalio, Samuelson, and Van Huyck (2001).

games have the same Nash equilibria—an efficient equilibrium at (X,X), an inefficient equilibrium at (Y,Y), and a mixed-strategy equilibrium in which players choose X with probability .8. However, the payoff *differences* between strategies X and Y, for any belief q about an opponent's probability of choosing X, are $r(q) = 50q - 40$ in game 2R, $25q - 20$ in game R, and $15q - 12$ in game .6R. This payoff differential is twice as large in game 2R and .6 times as large in game .6R, compared with the differential in game R. Intuitively, it seems likely that convergence will be swifter when the payoff difference $r(q)$ is larger.

Figure 6.3 summarizes the results. The figures show the percentage of players choosing X across the seventy-five periods (in five-period blocks). Most of the groups began with fewer than 80 percent of the subjects playing X. Convergence is indeed more rapid when the payoff gradient is higher. In two of the .6R sessions and one R session, players hop across the "separatrix" at .80 and converge toward the efficient equilibrium despite starting out with a percentage of Y choices high enough to draw them toward (Y,Y).

Table 6.18. Learning model coefficients

Model	α	Received payoff r_{it}	Expected payoff $r(y_{it})$	2R dummy	.6R dummy	Log-likelihood
Reinforcement	1.031	.173	—	−1.322	n/i	−1783
	(.07)	(.01)		(.12)		
Beliefs	1.856	—	.348	n/i	n/i	−1510
	(.09)		(.02)			
Both	2.963	.048	.422	−1.742	−1.810	−1433
	(.19)	(.01)	(.02)	(.26)	(.19)	

Source: Battalio, Samuelson, and Van Huyck (1999).
Note: Standard errors in parentheses. Player-specific intercept and response-sensitivity parameters are defined as deviations from some common term; the terms reported above are common terms. The table reports only variables that enter significantly in a stepwise procedure. Variables that are not included are denoted "n/i."

In an earlier version of their paper (1999), Battalio et al. fitted three learning models. Fictitious play is used to compute an expected earnings difference $r(y_{it})$ and then players choose strategy X with a probability

$$P_{it}(X) = e^{\alpha_i+\lambda_i r(y_{it})}/(1 + e^{\alpha_i+\lambda_i r(y_{it})}). \tag{6.5.1}$$

The player-specific constant α_i expresses an inherent preference (or dispreference if α_i is negative) for strategy X. They also fit an average-reinforcement model in which r_{it} is the average of received payoffs. Table 6.18 summarizes fits of three models on periods 1–40, using expected payoffs $r(y_{it})$, average earnings r_{it}, and both terms together. The belief model r_{it} fits better than the average-payoff reinforcement model, and a model that combines both terms fits even better. But the combination is a lopsided mixture—like horse and rabbit stew, made with one horse and one rabbit—because the expected payoff term has an estimated coefficient almost ten times as large as the reinforcement term.

Feltovich (1999, 2000) studied two-stage games of asymmetric information. His game is interesting because a player's move in the first of the two stages both earns a payoff and (potentially) reveals information about what game is being played. Table 6.19 shows the payoffs. Nature moves first and determines a matrix, either left or right with probabilities p and $1-p$. The row player is informed of the matrix choice but the column player is not. Players move simultaneously and play twice with the same matrix. Both players are told only their opponent's choice in the first stage, but not their payoff (to prevent the uninformed column player from learning which matrix is being used). If the row player always chooses her dominant strategies (A if left, B if right) she reveals which matrix has been chosen; if the column

Table 6.19. *Two-stage asymmetric information game studied by Feltovich*

	Uninformed (column) player strategies			
	Left matrix (p)		Right matrix ($1-p$)	
Informed (row) player	A	B	A	B
A	1,0	0,1	0,1	0,1
B	0,1	0,1	0,1	1,0

Source: Feltovich (1999, 2000).

player figures this out she will choose B after observing A and A after observing B, and the row player will get 0 in the second round. So it is better to randomize in the first stage, then choose the dominant strategy in the second stage.

In equilibrium, when the prior $p = .50$, informed players should randomize equally in the first stage to hide what they know. When $p = .34$, the informed player should choose A with probability .971 when the matrix is left and randomize equally over A and B when the matrix is right.

When $p = .50$, the uninformed player should choose B in response to a first-stage choice by the informed player of A with probability β between .5 and 1, and choose A with probability $1 - \beta$. When $p = .34$ the uninformed player should randomize equally after an observed A choice and should always play A after a B choice.

Call the choice of A (or B) by player 1 after observing L (or R) the "stage-dominant action" (the choice that is a best response if the game ended right away rather than continuing to a second round). Call the choice of B (A) by player 2 after observing a player 1 choice of A (B) a "best response." In the $p = .50$ game, player 1 played her myopic choice (A if she saw L, B if she saw R) 85 percent and 77 percent of the time in the first and last twenty rounds, showing very slow convergence to the MSE prediction of 50 percent. Player 2 best-responded to the observed choice (picking A after observing B or B after A) 80–90 percent of the time. In the $p = .34$ game the corresponding fractions are 85 percent and 79 percent for player 1s (converging slowly toward the MSE of 66 percent) and 75 percent for player 2s.

Belief learning and a version of reinforcement that includes sophistication (see below) fit about equally well,[23] depending on the test statistic used. Sophisticated reinforcement fits individual choices a little better than belief learning (an 80 percent hit rate, about as accurate as predicting the most

[23] The reinforcement model builds in sophistication because players' first-stage strategies are reinforced by the *sum* of the payoffs from *both* stages. This means that an anticipatory strategy that anticipates how revealing information in the first stage will hurt the player's second-stage payoff gets strongly reinforced.

common choice); but belief learning generates simulated paths that are a little more accurate. These results show that when a range of models are about equally accurate—especially when equilibria are mixed, so no model can predict too accurately—the best fit is sensitive to the fit criterion.

In other studies, Nagel and Tang (1998) compared belief and reinforcement models in centipede games. They concluded that reinforcement fits best using the MSD criterion but their implementation of belief learning is odd. Ho, Wang, and Camerer (2002) showed that the EWA model fits slightly better than reinforcement on the same data, and reveals detectable individual differences. Blume et al. (2001) fitted simple reinforcement and belief models to their data on sender–receiver games (see Chapter 7). They found that reinforcement fits better.

Summary: Several studies have compared reinforcement and belief models. In games with mixed-strategy equilibria (Erev and Roth, Mookerjhee and Sopher), reinforcement models generally fit slightly better than belief models (although none of the learning models do much better than QRE equilibrium). In the coordination games studied by Ho and Weigelt and Battalio et al., belief learning does better. Three studies estimating the relative contribution of different models found that belief terms were almost ten times as important as reinforcement.[24]

Nonetheless, it is difficult to draw firm conclusions across studies because the games and details of model implementation differ. Different studies add or subtract parameters to reinforcement models and change how attractions are reinforced. Belief models are also implemented in different ways (for example, how initial beliefs or attractions are defined, and whether weighted or standard fictitious play is used). Test statistics differ. Some studies use log likelihood of fit or predicted choices, which permit statistical inferences, whereas others use squared deviations, which do not permit inference. The obvious solution is to use a wider variety of games and statistics, and to use general model specifications that include as many earlier ones as possible. This is the approach of Camerer and Ho (1999a,b) and Stahl (2000a,b), discussed next.

6.6 Experience-Weighted Attraction (EWA) Learning

Reinforcement learning assumes players ignore information about forgone payoffs. Belief learning assumes players ignore information about what they chose in the past. But players seem to use both types of information when they are available.

[24] See Erev and Roth (1998), Battalio, Samuelson, and Van Huyck (2001), and Munro (1999), reported in Chapter 7.

Teck Ho and I (Camerer and Ho, 1999a,b) therefore created a hybrid of reinforcement and belief models which also uses both types of information. The experience-weighted attraction (EWA) model has two variables, attractions $A_i^j(t)$ and an experience weight $N(t)$, which are updated after every period of experience.

The experience weight starts at an initial value $N(0)$ and is updated according to $N(t) = \phi(1-\kappa) \cdot N(t-1) + 1$, with the restriction $N(t) \leq 1/[1-\phi(1-\kappa)]$ so that $N(t)$ is weakly increasing.[25] Attractions start at $A_i^j(0)$ and are updated according to

$$A_i^j(t) = \frac{\phi \cdot N(t-1) \cdot A_i^j(t-1) + [\delta + (1-\delta) \cdot I(s_i^j, s_i(t))] \cdot \pi_i(s_i^j, s_{-i}(t))}{N(t)}. \tag{6.6.1}$$

Attractions determine probabilities using the logit form $P_i^j(t+1) = e^{\lambda \cdot A_i^j(t)} / \sum_{k=1}^{m_i} e^{\lambda \cdot A_i^k(t)}$ (but a power form fits about equally well; see Camerer and Ho, 1999b).

The weighted payoff term $[\delta + (1-\delta) \cdot I(s_i^j, s_i(t))] \cdot \pi_i(s_i^j, s_{-i}(t))$ is crucial. The attractions of *all* strategies are updated by δ times the payoff that strategy would have yielded, even if it was not chosen. The chosen strategy $s_i(t)$ is updated by an *additional* fraction $1-\delta$ of its payoff. Reinforcement is often justified by appeal to the "law of effect"—the regularity, discovered by behavioral psychologists, that animals tend to repeat successful strategies. Since behaviorists studied mostly animal learning, they never thought about a parallel "law of *simulated* effect"—strategies that *would* have been successful would be chosen more often. EWA allows for both effects.

Under different parameter restrictions, EWA reduces to reinforcement and weighted fictitious play. When $\delta = 0, \kappa = 1$, and $N(0) = 1$ (so $N(t) = 1$), attractions are updated by $A_i^j(t) = \phi \cdot A_i^j(t-1) + I(s_i^j, s_i(t)) \cdot \pi_i(s_i^j, s_{-i}(t))$, which is a simple form of cumulative reinforcement model. When κ is 0 instead of 1, the attractions are weighted averages instead of cumulations, with weights $\phi/(\phi+1)$ and $1/(\phi+1)$.

When $\delta = 1$ and $\kappa = 0$, the updating equation is

$$A_i^j(t) = \frac{\phi \cdot N(t-1) \cdot A_i^j(t-1) + \pi_i(s_i^j, s_{-i}(t))}{\phi \cdot N(t-1) + 1}. \tag{6.6.2}$$

[25] In our earlier work we used $\rho = \phi(1-\kappa)$, but recently switched notation to make the model more transparent.

A little algebra shows that this updating equation is *exactly* the same as weighted fictitious play.[26] That is, a person who learns according to weighted fictitious play behaves just like an EWA learner who starts with initial attractions based on expected payoffs, reinforces each strategy equally strongly according to what it would have earned (or did earn), and takes a weighted average of previous attractions and current reinforcements.

Thus, reinforcement and belief learning are elements of a family of learning rules which are surprisingly related, like siblings raised apart or rivers that turn out to have a common source. The kinship is important because it was often suggested that the two approaches are fundamentally incompatible. For example, Selten (1991, p. 14) wrote: "In rote [reinforcement] learning success and failure directly influence the choice probabilities. . . . Belief learning is very different. Here experience strengthens or weakens beliefs. Belief learning has only an indirect influence on behavior."[27]

The parameters of EWA have psychological interpretations. The parameter δ is the weight placed on forgone payoffs (opportunity costs, in economic terms, or counterfactuals or regret in psychological terms). The weight δ is presumably affected by imagination and the reliability of information about forgone payoffs (Heller and Sarin, 2000). It may also reflect the difference between received gains and "opportunity gains" (people may underweigh lost opportunities).

The parameter ϕ reflects the decay of previous attractions owing to forgetting or to deliberate shedding of old experience when the learning environment is changing. The parameter κ controls the rate at which attractions grow. When $\kappa = 0$ attractions are weighted averages of reinforcements and decayed lagged attractions; when $\kappa = 1$ attractions cumulate. The growth rate of attractions is important because in the logit model the difference in attractions determines the spread of choice probabilities; fixing λ, players can lock in more sharply to playing one strategy frequently if κ is larger.

The initial weight $N(0)$ calibrates the strength of initial attractions in units of "experience-equivalence". When players have a Dirichlet prior, and use fictitious play, their beliefs are Bayesian and $N(0)$ is the strength of prior beliefs. Frankly, $N(t)$ was included precisely to link the model to Bayesian ideas. It usually does not contribute much to empirical fit so in recent work we usually set $N(0) = 1$.

[26] The trick is to first write beliefs in period t as a function of period $t-1$ beliefs. When these beliefs are used to compute expected payoffs, and expected payoffs in period t are written as a function of period $t-1$ expected payoffs, the belief term disappears. That is, all of the mathematical effect of belief updating is encompassed by reinforcing strategies according to forgone or received payoffs, as in equation (6.6.2).

[27] The connection was also noticed by Fudenberg and Levine (1998) and Cheung and Friedman (1997). Hopkins (in press) showed that if actual payoffs are scaled by the probability with which a strategy is chosen, so that rare strategies get a boost in cumulative attraction, then the two are roughly equivalent.

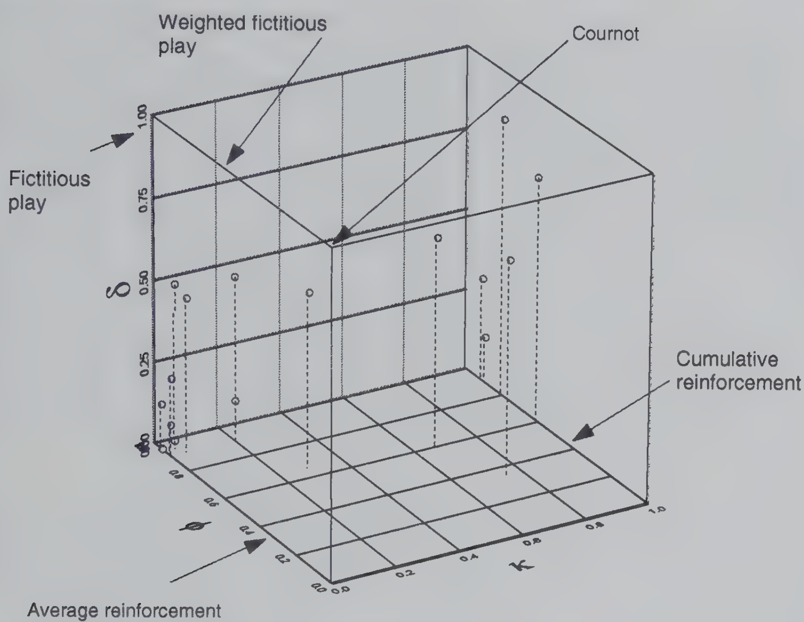


Figure 6.4. Cube of EWA parameter configurations.

Different learning rules can be represented as points in a cube of the three key EWA parameters (see Figure 6.4). Each point in the cube is a triple of parameter values which specifies a precise updating equation (leaving aside λ and initial conditions). The cube shows the EWA family of learning rules. Corners and vertices of the EWA cube correspond to boundary special cases. The corner of the cube with $\phi = \kappa = 0, \delta = 1$, is Cournot best-response dynamics. The corner $\kappa = 0, \phi = \delta = 1$, is standard fictitious play. The vertex connecting these corners, $\delta = 1, \kappa = 0$, is weighted fictitious play. Vertices with $\delta = 0$ and κ equal to 0 or 1 are averaging and cumulative choice reinforcement rules.

The EWA cube is a visual aid to show the relations and differences among theories. But the construction of EWA is also a bet that psychologically plausible learning rules have parameter values that might be in the interior of the cube rather than clustered on vertices and corners.

In all of our empirical estimation, the first 70 percent of each player's time series is used to estimate model parameters using maximum-likelihood estimation.[28] Initial conditions $A_i^j(0)$ are either estimated as free parameters

[28] In recent work we fit data from a sample of 70 percent of players, using *all* those players' choices, then forecast the entire learning path for the remaining holdout sample of 30 percent of the players.

or "burned in" by choosing attractions to fit the relative frequencies in the first period as closely as possible.

Studies with about thirty-one data sets (summarized in Camerer, Ho, and Chong, 2002a) show that EWA generally fits and predicts *out of sample* more accurately than the reinforcement and weighted fictitious play, except in games with mixed-strategy equilibrium (where no models improve much on QRE). Figure 6.4 shows twenty triples of estimates resulting when different treatments in the thirty-one data sets are collapsed together. Most points are sprinkled around the cube. Several points cluster in the average reinforcement corner where $\delta = \kappa = 0$ and ϕ is close to 1 (these are from games with mixed equilibria). Except for that cluster, there is no strong tendency for points to cluster on any special corners or vertices. This means that focusing attention on a single learning rule, because it is presumed to predict more accurately than rules with other parameter configurations, is a mistake.

Keep in mind that *all* the studies reviewed previously in this chapter compared either one point or vertex in the cube (one learning rule) with equilibrium, or two or more points in the cube with each other. These early comparisons tell us very little about which rules fit best because so few rules are being compared. Estimating best-fitting parameter values in EWA essentially compares a huge number of possible rules in one fell swoop (including all the familiar ones, and Bayesian learning).

It is tempting to conclude that any empirical improvement in EWA just shows that adding parameters always improves predictions. This claim is doubly wrong. First, forcing models to predict out of sample after parameters are estimated in-sample means that if a model is unnecessarily complex, it will fit better but *predict more poorly*. That is, adding parameters *does not* always improve predictions when predictive accuracy is judged correctly. Second, the hybrid model doesn't really add parameters because the new parameters (δ and κ) were implicit in earlier models; those parameters simply express the distinctions between models more clearly and allow new combinations.

Two games will help illustrate the strengths and weaknesses of different models.

6.6.1 Example: Continental Divide

The "continental divide game" (discussed in Chapters 1 and 7) is an order-statistic game in which each of seven players chooses integers from 1 to 14. A player's payoff depends on her number and the median number chosen by the players in her group, as shown in Table 6.20. There are two pure-strategy Nash equilibria, at 3 and 12.

Figure 6.5a shows the pooled frequencies of choices of ten groups across fifteen periods, from Van Huyck, Battalio, and Cook (1997). The data have

Table 6.20. *Payoffs in a "continental divide" experiment*

	Median choice													
Choice	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1	45	49	52	55	56	55	46	−59	−88	−105	−117	−127	−135	−142
2	48	53	58	62	65	66	61	−27	−52	−67	−77	−86	−92	−98
3	48	54	*60*	66	70	74	72	1	−20	−32	−41	−48	−53	−58
4	43	51	58	65	71	77	80	26	8	−2	−9	−14	−19	−22
5	35	44	52	60	69	77	83	46	32	25	19	15	12	10
6	23	33	42	52	62	72	82	62	53	47	43	41	39	38
7	7	18	28	40	51	64	78	75	69	66	64	63	62	62
8	−13	−1	11	23	37	51	69	83	81	80	80	80	81	82
9	−37	−24	−11	3	18	35	57	88	89	91	92	94	96	98
10	−65	−51	−37	−21	−4	15	40	89	94	98	101	104	107	110
11	−97	−82	−66	−49	−31	−9	20	85	94	100	105	110	114	119
12	−133	−117	−100	−82	−61	−37	−5	78	91	99	106	*112*	118	123
13	−173	−156	−137	−118	−96	−69	−33	67	83	94	103	110	117	123
14	−217	−198	−179	−158	−134	−105	−65	52	72	85	95	104	112	120

Source: Van Huyck, Cook, and Battalio (1997).

two key features: (1) Behavior bifurcates from initial choices in the range 4–8 toward the equilibria at 3 and 12; and (2) convergence is asymmetric—it is much sharper (taller spikes in the figure) at the equilibrium of 12 than in the neighborhood of 3. In addition, the learning process "brakes" and "accelerates" quickly, in the sense that common early choices from the range 5–8 are completely extinguished by period 15, whereas choices below 4 and above 10 are rare in early periods but quickly pile up in later periods.

Table 6.21 gives parameter estimates. Restricting parameters to belief and reinforcement values gives log-likelihood (LL) values that are 180 and 249 points worse, respectively, so those simpler models fit much worse. Figure 6.5e shows predictions based on maximum-likelihood estimates (MLE) of a reinforcement model (the restriction of EWA with $\delta = \kappa = 0$, $\phi = 1$, and reinforcements divided by variability of payoffs; see Roth et al., 1999).[29] Reinforcement fits reasonably well, although it does not brake early choices or accelerate later choices as quickly as is observed in the data.

[29] The predicted probability for each player and period, given the parameter estimates that maximize likelihood from the first ten periods, are averaged across players to produce the figures in the plot. Note that the model does make predictions about all individuals and periods, but they are simply averaged to give an overall glimpse of model fit.

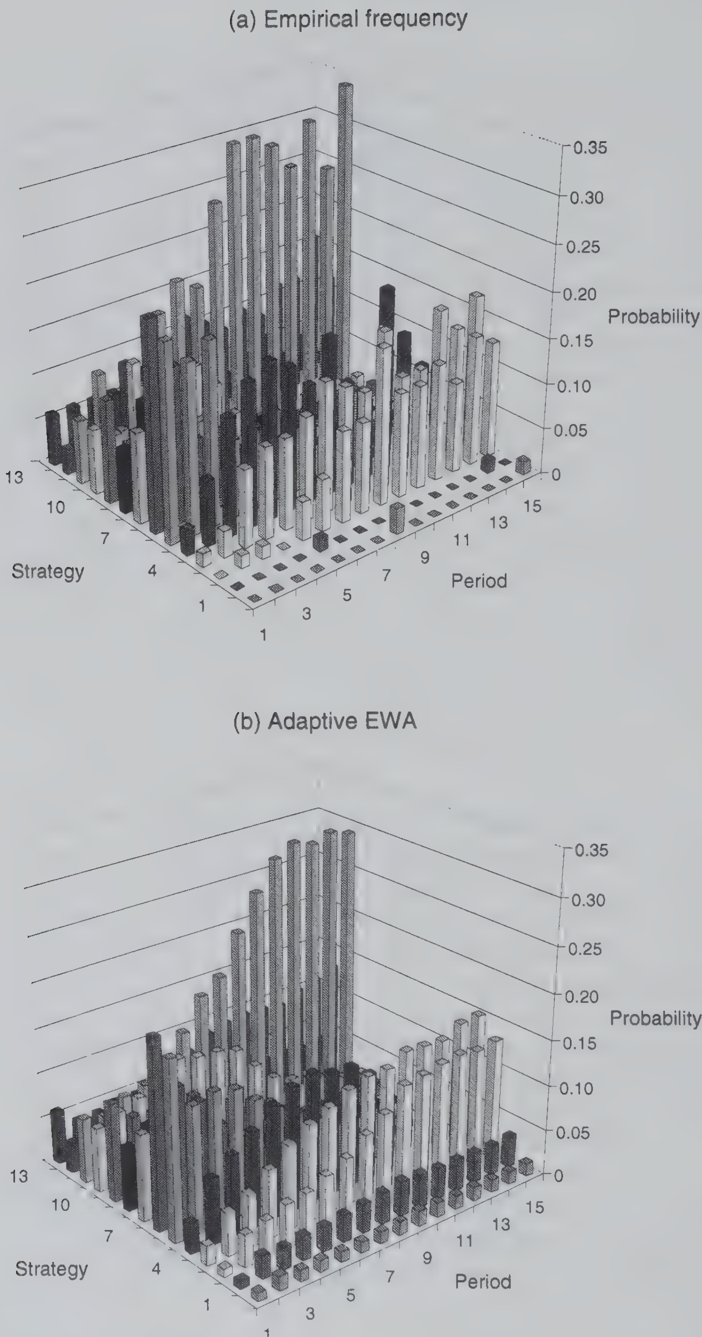
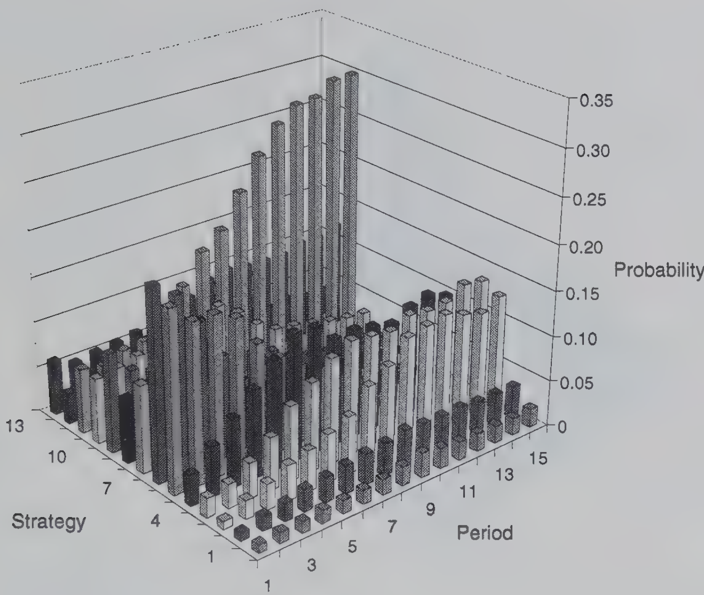


Figure 6.5. *Data and learning model fits, continental divide game. Source: Ho, Camerer, and Chong (2002).*

(c) EWA Lite



(d) Belief-based

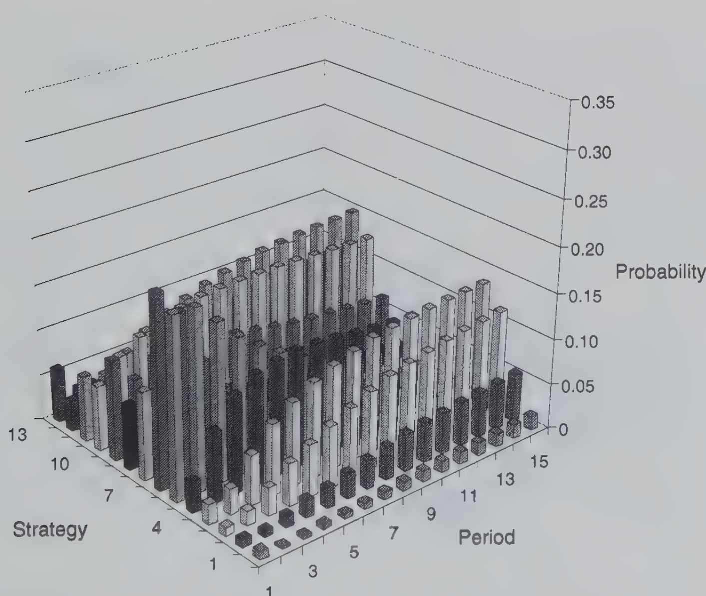
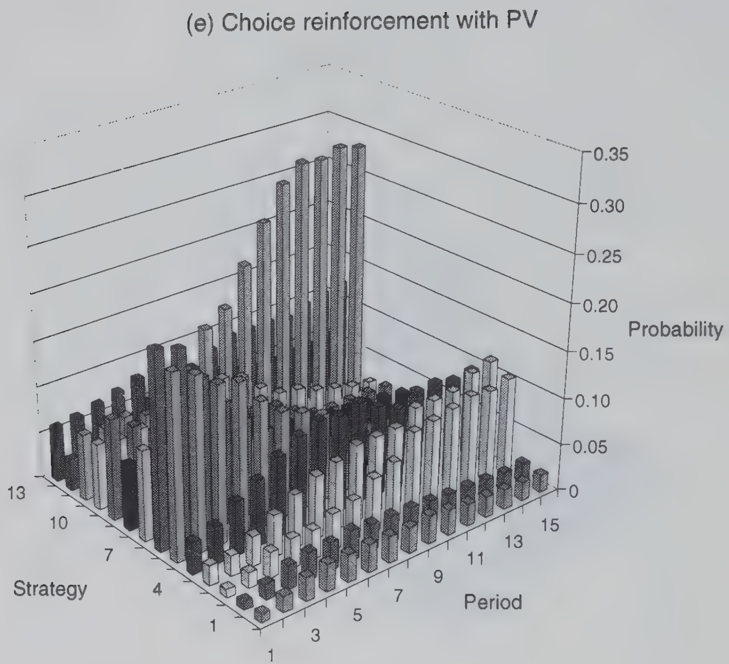


Figure 6.5 (continued)



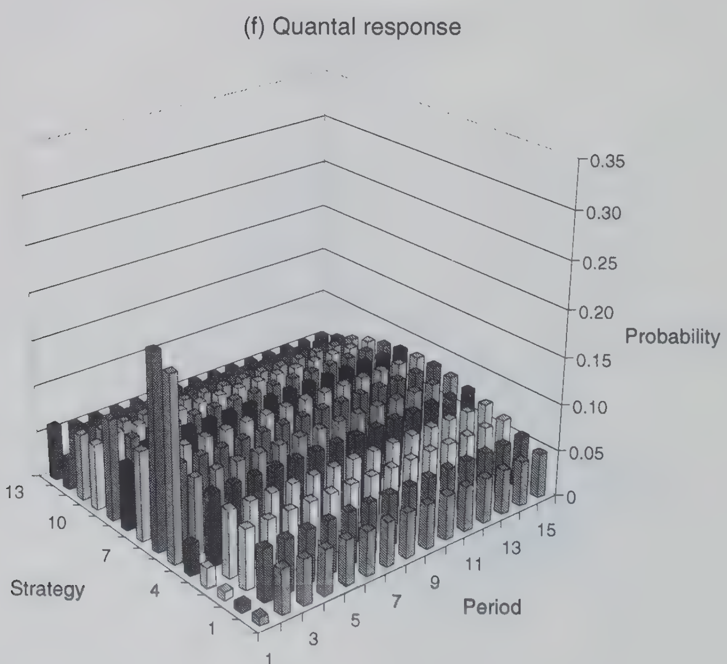


Figure 6.5 (continued)

Table 6.21. *Parameter estimates for continental divide and p-beauty contest games*

Parameter	Continental divide game	*p*-Beauty contest game			
		Inexperienced		Experienced	
		Sophisticated EWA	EWA	Sophisticated EWA	EWA
ϕ	0.61	0.44	0.00	0.29	0.22
δ	0.75	0.78	0.90	0.67	0.99
κ	1.00	0.00	0.00	0.04	0.00
α	—	0.24	0.00	0.77	0.00
α'	—	0.00	0.00	0.41	0.00
d	—	0.16	0.13	0.15	0.11
LL (in)		−2095.32	−2155.09	−1908.48	−2128.88
LL (out)		−968.24	−992.47	−710.28	−925.09
$\bar{p}$ (in)		0.06	0.05	0.07	0.05
$\bar{p}$ (out)		0.07	0.07	0.13	0.09

Source: Camerer, Hsia, and Ho (2002); Camerer, Ho, and Chong (2002a).
Note: QRE log likelihood in and out of sample are −2471 and −1129 (inexperienced) and −2141 and −851 (experienced).

Figure 6.5d shows predictions from the weighted fictitious play model. It captures the bifurcation toward low and high numbers, but it does not predict that convergence around 12 is sharper than around 3. It cannot explain the asymmetry because the payoff gradients around the two equilibria are *exactly* the same. In the belief model, the distribution of choices is entirely determined by the payoff gradient.[30] EWA can account for the asymmetry because sharpness of convergence is determined by the difference between received payoffs and δ-weighted forgone payoffs, and this difference is larger at the higher payoffs around the 12 equilibrium.

Figure 6.5f shows, as a static benchmark, the fit of a quantal response equilibrium model (which, by definition, makes the same prediction in each period). The model fits quite poorly because the data are roughly a mixture of high or low numbers, but the best response to such a mixture is a middle number (which are rarely chosen after the first couple of periods). The requirement that players' beliefs always be in equilibrium makes it impossible to explain these data when all the periods are pooled.

[30] If others are choosing 3 or 12, then choosing 1 or 2 units too high or low costs players only 2 cents or 8 cents, respectively. That is, deviations are equally costly at the two equilibria, though absolute payoffs are higher for the equilibrium at 12 (see Table 6.20).

Figures 6.5b–c show predictions of EWA. (The parametric model is the one being discussed now; the "fEWA" or "Lite" version is a one-parameter variant described below.) EWA fits only a little better than reinforcement learning, but it does predict both bifurcation and convergence asymmetry, and also brakes and accelerates quickly. Parameter estimates are summarized in Table 6.21.

The continental divide game can also be used to illustrate differences in how models are estimated and used. The maximum-likelihood estimates (MLE) method uses the actual observed history in period t to update attractions for period $t+1$. An alternative approach is to simulate paths, essentially using artificial history generated by the model itself from period t to update attractions and predict period $t+1$ choices. Knowing whether a model can simulate an entire path of play, without any data to work with, is important for using the model to forecast behavior in brand-new games or economic institutions.

Note, first, that if the accuracy of a model is judged by comparing the *conditional* frequencies of simulated play (i.e., frequencies after observed histories) with actual conditional frequencies, then simulating paths and forecasting period $t+1$ from data in periods $1-t$ produce precisely the same results.[31] Since conditional frequencies are often of interest (e.g., when does strategy switching occur?), forcing a model to predict conditional frequencies accurately is an important and tough test. Nonetheless, it is often useful to see whether models can pass an easier test of predicting unconditional frequencies.

Comparisons of fits and predictions derived from MLE, and simulated paths based on those MLE estimates, have been done by Crawford (1995), Camerer and Ho (1999b), Camerer, Hsia, and Ho (2002), and Erev and Haruvy (2001). The first three papers found no interesting differences between the two methods. The last found that models that include inertia—the frequency of a choice depends on how often it was chosen before—can be particularly bad at simulating paths because inertial models are extremely sensitive to starting points.

Teck Ho, Xin Wang, and I compared path simulation and period $t+1$ estimation further using the continental divide game. We simulated 1,500 groups of seven subjects using five different models—EWA, a functional

[31] In the continental divide game, for example, suppose a particular player's choice history in the first two periods is 6 and 8, and the medians of others in her group were 7 and 9. Suppose we simulate paths and compare the simulated frequency with which the simulated players make choices 1–14 conditional on the actual history (i.e., after choosing 6 and 8, with group medians of 7 and 9) with the actual conditional frequencies of those choices. The result will be exactly the same as if attractions were updated using the actual historical data—i.e., as if we were predicting period $t+1$ from periods 1 to t. Forecasted and (filtered) simulated results are the same because filtering out only those simulated paths with histories that match the actual history is exactly the same as using the actual history directly for updating.

Table 6.22. *Results from different model-fitting methods, continental divide game*

	Within-game							
	In-sample		Out-of-sample		Cross-game		Simulated path	
Model	LL	Rank	LL	Rank	LL	Rank	MSD(×100)	Rank
EWA	−1062	1	−460	1	−1635	1	0.2416	1
fEWA	−1081	2	−470	2	−1741	2	0.2434	2
Beliefs	−1288	3	−564	3	−2147	3	0.2506	3
Reinforcement−PV	−1293	4	−573	4	−2403	4	0.5300	4
QRE	−1890	5	−808	5	−2695	5	0.5300	5

Source: Ho, Camerer, and Chong (2002); Camerer, Ho, and Wang (unpublished).

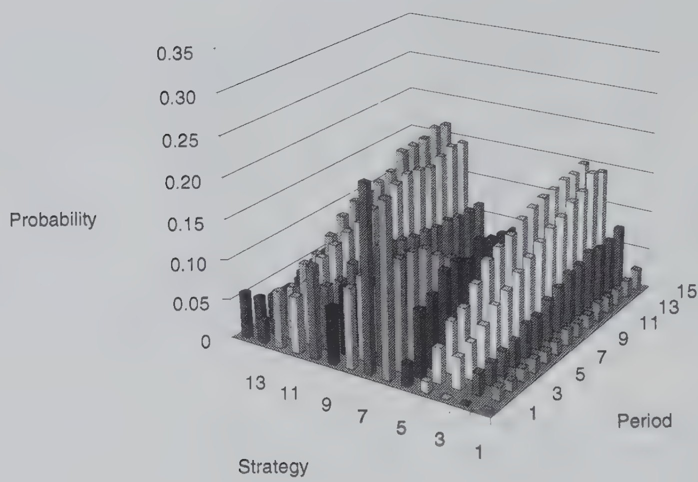
form of EWA (discussed below), belief learning, reinforcement with payoff variability, and QRE as a no-learning benchmark. Parameter values were chosen to minimize the sum of squared deviations between simulated unconditional frequencies (pooling all 10,500 simulated subjects together) and actual unconditional frequencies, summed across all strategies and periods.

Table 6.22 summarizes the statistical fits (mean squared deviations multiplied by 100) and compares them with log likelihoods (LL) derived three different ways: estimating parameters within a game; first fitting in sample then forecasting out of sample; and estimating parameters on six other games and using them to forecast fresh data from the continental divide game (see Ho, Camerer, and Chong, 2002). As it turns out, the models are ranked by the four estimation criteria in the same way. Although the ranks do not depend on whether simulated or actual data are used to update attractions, there are some interesting differences in the simulation results.

Figures 6.6a–c show the average simulated frequencies for the EWA, belief, and reinforcement models. First note that the simulated data do not match the actual frequencies as closely as when the periods $1 - t$ data are used to forecast $t + 1$. This is no surprise, of course, since in the simulations the models' errors are compounded rather than corrected by using the actual history. Belief learning also simulates better than it fits.

Reinforcement suffers because, as learning occurs, human players actually choose numbers further and further from the center because they are responsive to forgone payoffs. But in the reinforcement model, simulated players are not sensitive to forgone payoffs so they do not move quickly enough away from the middle. When the actual history is used, the sluggish tendency in the reinforcement model is corrected: The chosen strategies are not those the model predicts would be made, but they receive more

(a) EWA



(b) Belief learning

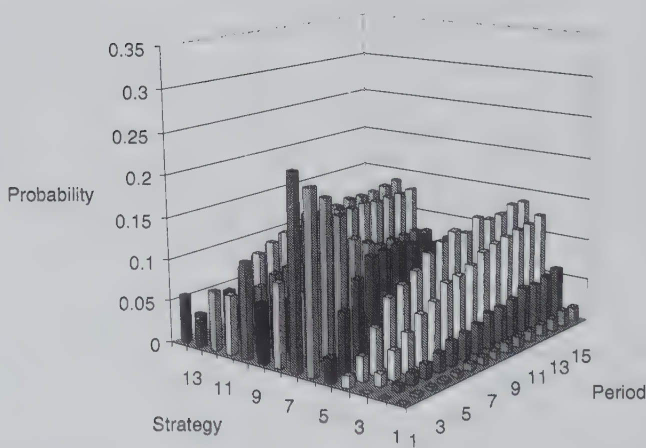


Figure 6.6. *Average simulated paths (parameters chosen to minimize mean squared deviations), continental divide game: (a) EWA; (b) belief learning; (c) reinforcement. Source: Camerer, Ho, and Chong (unpublished).*

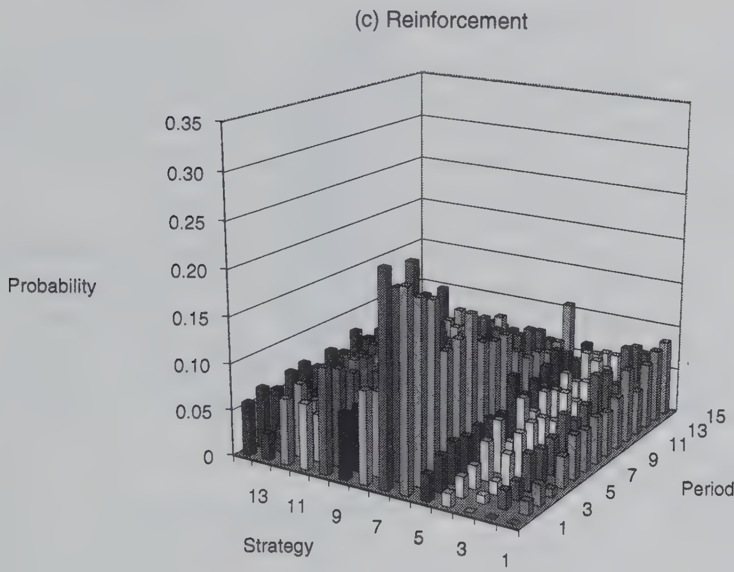


Figure 6.6 (continued)

reinforcement and constantly correct the model's mistakes. (By analogy, imagine a robot that simulates how quickly a person walks, but takes steps that are only half as long as the steps people actually take. The robot would be far behind the person it is simulating after 100 yards. But if the robot's mistake were corrected after each step—by letting the robot start afresh, where the person stood after each step—it would be only a half-step behind after 100 yards.) When the model is used to simulate paths, its sluggishness is never corrected and the resulting simulations do not create much convergence toward the high and low equilibria at 3 and 12.

Of course, *both* ways of evaluating models—predicting new history from actual history, or simulating an entire path—are useful criteria depending on one's purpose. It is even conceivable that some models perform one task well and another badly. However, I doubt that, in the domain of learning rules, some models are uniformly better than others at different types of forecasting (with the noted exception of inertial models simulating poorly). Good models will fit well both ways. And, when a promising theory does fail to predict accurately in a certain way, that failure usually contains a clue about how to improve the theory.

6.6.2 Example: *p*-Beauty Contest, and Sophistication

A second example is the p-beauty contest (pBC) described in Chapter 5 to study the number of steps of iterated thinking players seem to do. In the pBC games reported here, groups of seven players choose numbers from 0 to 100. The player in each group whose number is closest to p times the average number (for $p = .7, .9$) wins a fixed prize.

Figure 6.7a shows relative frequencies of choices by experienced subjects, pooled across several groups. Figures 6.7b–e show predicted frequencies of belief, reinforcement, EWA Lite (fEWA), and sophisticated EWA learning. (Parameter estimates are reported in Table 6.21.) Belief learning fits and predicts reasonably well, as does EWA learning. Reinforcement learning is terrible. The problem is that six of seven players earn nothing in each period, get no reinforcement, and therefore learn nothing. Something clearly has to be added to reinforcement to explain why losers are learning. EWA adds reinforcement of forgone payoffs.

The pBC also illustrates the benefits of adding "sophistication." Sophisticated players are aware that others are learning. Camerer, Ho, and Chong (2002a) include sophistication in a recursive way. In our model, a fraction α of players believe that α' of the players are sophisticated and the remaining $1-\alpha'$ are adaptive and learn according to EWA. Sophisticated players have correct guesses about the EWA parameters but may be socially miscalibrated about how sophisticated others are. If sophisticated players are correctly calibrated, $\alpha = \alpha'$. If they overestimate how sophisticated they are relative to others (owing to overconfidence about relative skill, for example), then $\alpha > \alpha'$. If they think others are more like themselves than they really are ("false consensus"), then $\alpha < \alpha'$. The sophisticated players do not update attractions per se. Instead, they update the perceived attractions of EWA players and use them to compute choice probabilities, and calculate expected payoffs $E_i^j(t)$ according to

$$E_i^j(t) = \sum_{k=1}^{m_{-i}} [(1-\alpha') \cdot P_{-i}^k(a, t+1) + \alpha' P_{-i}^k(s, t+1)] \cdot \pi_i(s_i^j, s_{-i}^k), \quad (6.6.3)$$

$$P_i^j(s, t+1) = \frac{e^{\lambda \cdot E_i^j(t)}}{\sum_{k=1}^{m_i} e^{\lambda \cdot E_i^k(t)}}, \quad (6.6.4)$$

where $P_{-i}^k(s, t+1)$ and $P_{-i}^k(a, t+1)$ are choice probabilities by sophisticated and adaptive (EWA) players, respectively. Note that sophistication is recursive because $P_{-i}^k(s, t+1)$ determines the expected payoff-$E_i^j(t)$, which in turn determines those probabilities through the logit response function.

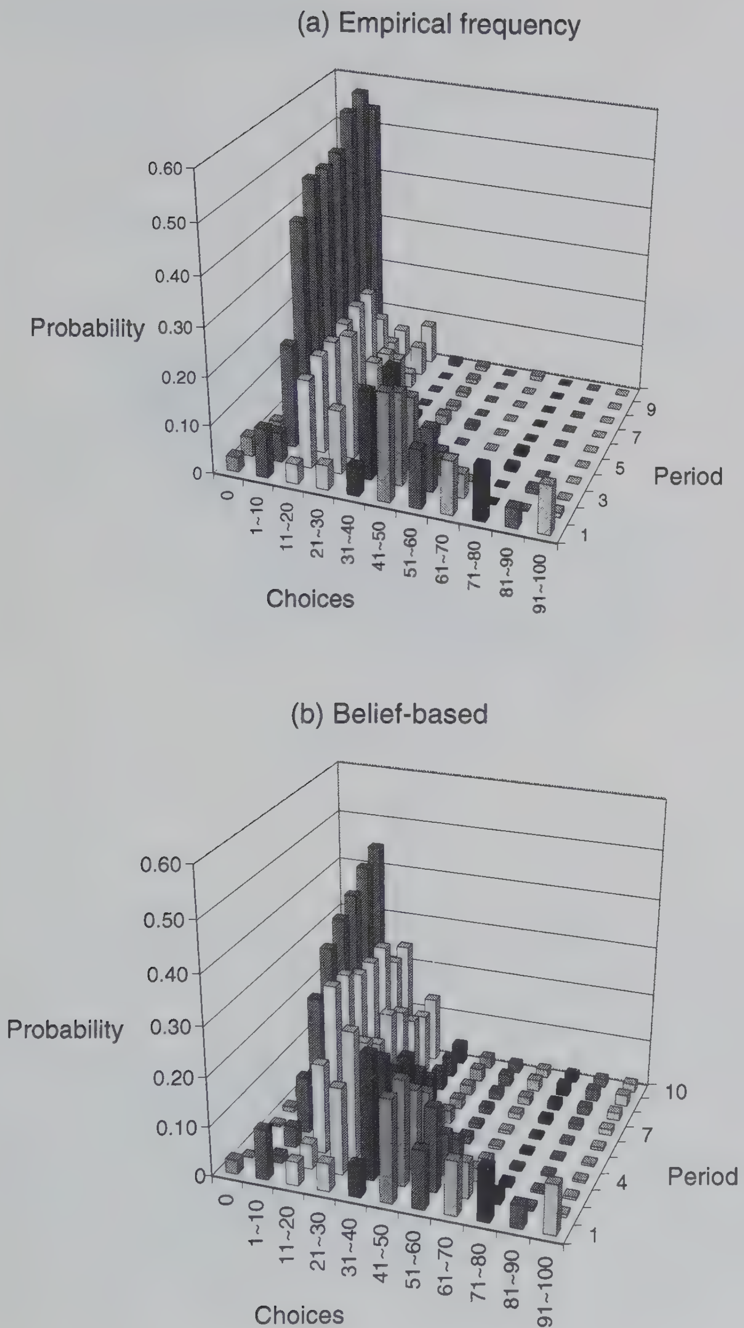
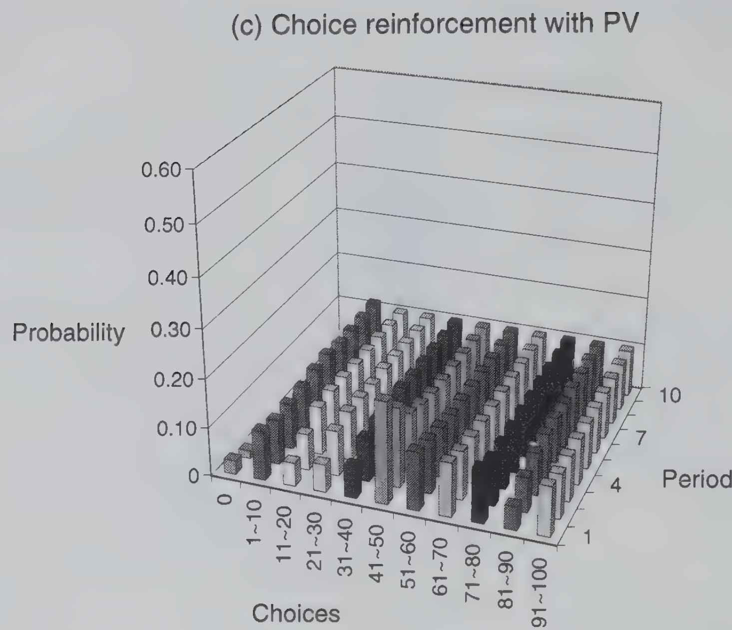


Figure 6.7. *Results of pBC beauty contest game, experienced subjects. Source: Camerer, Ho, and Chong (2002a); reproduced from* Journal of Economic Theory *with permission of Academic Press.*



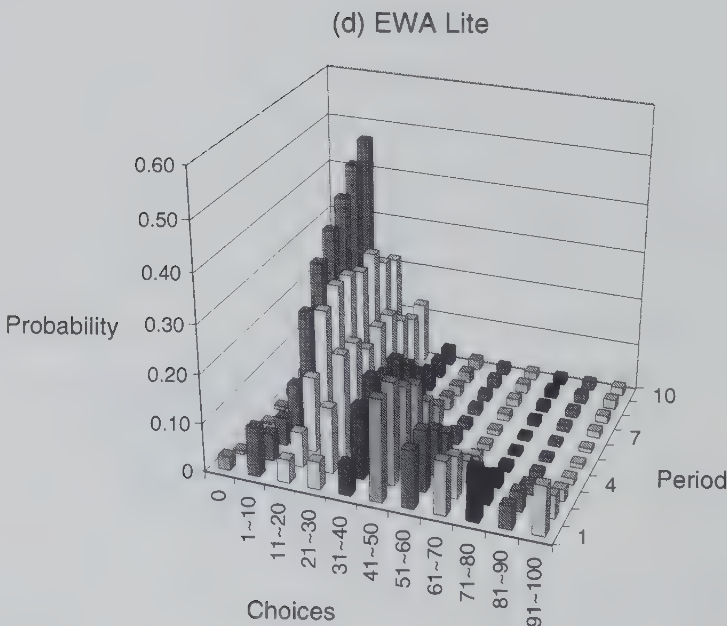


Figure 6.7 (continued)

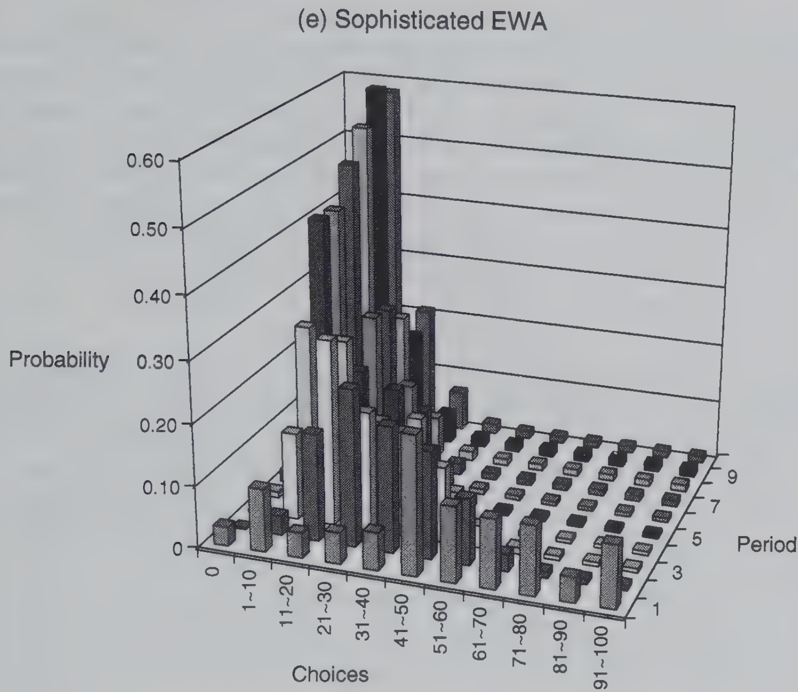


Figure 6.7 (continued)

Including sophistication in this way nests *many* special cases with only three new parameters (α, α', and a λ for sophisticates).[32] QRE is the conjunction of social calibration ($\alpha = \alpha'$) and complete sophistication ($\alpha = 1$). Adding hyperresponsiveness (large λ) is Nash equilibrium. Stahl's model of sophistication imposes $\alpha' = 0$. Fixing $\alpha = 0$ reduces to EWA and its many special cases. Estimating the full model therefore provides information on many quite different models in one fell swoop.

Table 6.21 gives parameter estimates for the sophisticated model, for both inexperienced subjects (playing a pBC for the first time) and experienced subjects (playing the second of two games). In the adaptive EWA model (with $\alpha = 0$) the estimates of δ and κ are close to 1 and 0, so the belief model restriction is reasonable but reinforcement is strongly rejected

[32] The three added parameters could easily be reduced to one by imposing $\alpha = \alpha'$ and using the same λ for all players, suitably rescaled.

(as the earlier figures suggested). When sophistication is included, the estimated proportion of sophisticated players, $\hat{\alpha}$, is 24 percent and 77 percent for inexperienced and experienced subjects. The increase in this percentage with experience suggests a kind of "learning about learning" or increase in sophistication over time. Furthermore, in both groups *perceived* sophistication α' is less than α, so players appear to underestimate how many others are sophisticated. The learning models also improve over QRE, although the improvement is modest when subjects are experienced.

6.6.3 Functional EWA (fEWA)

Ho, Camerer, and Chong (2002) developed a new variant, which we call fEWA, for two reasons. The first reason is that, because EWA has several parameters, many people have wondered whether it overfits, and researchers who are used to simpler models have found it hard to use in their own work.[33] So we tried to create a good theory with only one free parameter (a response sensitivity λ, which can be jettisoned in favor of a best-response rule if one's goal is simply to maximize the hit rate).

A second reason for developing fEWA is that estimated parameter values of EWA vary across games (although this is true of *all* learning models, e.g., Crawford, 1995; Cheung and Friedman, 1997; Erev and Roth, 1998). This means for cross-game prediction it is necessary to have some mapping from games to parameters.

The key ingredients in fEWA are a change-detector function $\phi_i(t)$ and an imagination weight $\delta_i(t)$ which is linked to $\phi_i(t)$. The change-detector simply compares the vector of historical frequencies of all previous choices by other players with the frequencies in the last W periods (where W is the (lowest) number of strategies that are predicted to be played in equilibrium; this feature just smoothes out fluctuations in the recent-history vector which are expected if other players are mixing).[34] The differences in these frequencies

[33] Of course, the too-many-parameters criticism is naive and unconstructive unless accompanied by a precise way to judge how many parameters is "too many." Fortunately, whether a theory has too many parameters is easily judged by restricting some parameters to plausible values and seeing whether the fit is much worse or not. If fit does not suffer, the theory *does* have too many parameters; otherwise, its parameters are justified statistically.

[34] The formal definition is

$$\phi_i(t) = 1 - .5\left(\sum_{j=1}^{m_{-i}}\left[\frac{\sum_{\tau=t-W+1}^{t} I(s_{-i}^{j}, s_{-i}(\tau))}{W} - \frac{\sum_{\tau=1}^{t} I(s_{-i}^{j}, s_{-i}(\tau))}{t}\right]^2\right).$$

The term $\sum_{\tau=t-W+1}^{t} I(s_{-i}^{j}, s_{-i}(\tau))/W$ is the jth element of a vector that simply counts how often strategy j was played by the others in periods $t-W+1$ to t, and divides by W. The term $\sum_{\tau=1}^{t} I(s_{-i}^{j}, s_{-i}(\tau))/t$ is the relative frequency count of the jth strategy over all t periods. Note that, in games with multiple players, a frequency count of the relevant aggregate statistics can be used. For example, in median action game, frequency count are squared and summed up across all the strategies, divided by two, then subtracted from 1. If the recent history is like all previous history, the frequency differences will be small so $\phi_i(t)$ will be close to 1. But when the recent history is quite different, the squared differences will be large, which drives $\phi_i(t)$ toward 0—i.e., the person has "detected change" and lowered the weight on old history, in effect increasing the responsiveness to the new history.

The imagination weight $\delta_i(t)$ is simply set equal to $\phi_i(t)/W$. This fits well because the parametric estimates of δ are close to 0 in games with mixed equilibria (when $W > 1$), so dividing by W pushes δ down toward 0 in those games.

The $\kappa_i(t)$ function controls the degree to which players explore different strategies (low κ) or exploit strategies that yield high payoffs (high κ). $\kappa_i(t)$ is the Gini coefficient (often used to measure income inequality) of previous choice frequencies. It is not as important empirically as the other functions and could be set to 0 or 1 without sacrificing much accuracy or insight.

These function values are then used in the EWA updating rule to update attractions after observations. Note that the functional values can differ across players and across time, so in principle they can capture individual differences and also endogenous changes in learning rules across games. For example, if ϕ starts low and grows toward 1 as behavior equilibrates, players are in effect switching from Cournot-like reinforcement rules toward fictitious play.

The model was estimated on seven games and compared with several other models. (Table 6.22 summarizes some fit results from the continental divide game.) It generally forecasts better than belief and reinforcement models which have *more* free parameters (except in mixed games where the models are equally accurate). The average values of the $\phi_i(t)$ and $\delta_i(t)$ functions also correspond rather closely, across games, to the values when their counterpart parameters are fixed and estimated in the EWA model.[35]

The dictionary definition of the phrase "ad hoc" is "for the specific purpose, case, or situation at hand, and for no other" (as in an "ad hoc committee"). Economists often misuse the term ad hoc to ridicule a new idea they didn't learn in graduate school or which does not arise from optimization. The fEWA functions are *unconventional*, but they are not ad hoc because the model is meant to apply to *all* games, not just the few it was applied to in our paper. In fact, after our paper was circulated, several readers wondered whether we might have tried so many different specifications in a search for

of the median strategy by all other players in each period is used. If each individual players' strategy is payoff-relevant then the vectors become matrices and an alternative form might be more parsimonious.

[35] For example, in the continental divide game the fEWA average values of ϕ, δ, and κ are 0.69, 0.69, and 0.77, compared to EWA MLE estimates of 0.61, 0.75, 1.00, and MSD-minimizing estimates from the simulations of 0.68, 0.68, 0.87.

good-fitting functions, that perhaps we overfitted the model to the sample of seven games we reported. So we invited people to submit more data sets and ended up with three more games (all dominance-solvable). fEWA fits about as well as the more complex theories on those new games too. fEWA has also been applied to learning repeated trust and entry games (see Chapter 8) with asymmetric players (where economizing on free parameters is especially desirable). It fits worse than parametric EWA, but substantially better than an equilibrium model (Camerer, Ho, and Chong, 2002b).

Summary: The EWA model is a hybrid of the key features of reinforcement and belief learning; namely, weighting received payoffs more strongly (reinforcement) and weighting forgone payoffs (belief models do this implicitly). If the simpler models were good approximations—and in mixed-equilibrium games they are—then adding parameters would not improve out-of-sample predictive accuracy, but EWA generally improves accuracy in about thirty-five games (except for mixed ones). Substituting functions of the data for the key parameters (fEWA) reduces the number of parameters that must be estimated to 1—or 0 if the criterion is to maximize hit rate—and fits as well as more complex models, or better, on a sample of ten games.

6.7 Rule Learning

Learning rules are functions from previous history and payoffs into a specific future choice. The papers discussed previously in this chapter all posit one learning rule and assume that a player sticks with the same rule throughout an experimental session (perhaps comparing models that posit different rules). Stahl (1996, 1999a,b, 2000a,b) discusses a more general approach in which players learn how various rules do, and switch among them (cf. Tang, 1996a; Erev and Barron, 2001). In rule learning, what one learns is not how well various *strategies* work, but how various *decision rules* perform (based on the performance of the strategies the rule recommends each period).

In Stahl (1999a,b), a rule is a set of weights given to various kinds of "evidence" or scores. The scores y_0, y_1, y_2, y_3 correspond, respectively, to a weighted average of previous play by others ("imitation," y_0), expected payoffs given updated beliefs (y_1), iterated expected payoffs y_2 (given the beliefs that others are best-responding to updated beliefs), and Nash payoffs y_3. A parameter θ measures the responsiveness to recent history in belief updating ($\theta = 1$ is Cournot). Weights on the four types of evidence are ν_i, $i = 0, 1, 2, 3$. Rules are reinforced by the expected payoff they would have generated in period t (taking expectations over all the possible strategies, and the actual distribution of population play) and chosen according to a logit rule.

Intuitively, think of a rule as the way a particular expert weighs different attributes or evidence (like jurors in a legal case, or critics judging films). Each expert weights evidence using their weights ν_i and recommends a strategy. The total probability that the strategy is played is the probability recommended by each expert times the tendency of the player to follow each expert's recommendation. As the player learns, the experts get "reinforced" according to their performance, so the player learns to pay more and more attention to the experts whose recommendations have performed well.

Stahl runs experimental sessions in which a pair of games are each played for fifteen periods. The initial reinforcement for a rule in the second game is an average of the rule's initial reinforcement at the start of the *first* game (with weight $1 - \tau$) and the ending reinforcement at the end of the first game (weight τ). The value of τ parameterizes the strength of *transfer* of learning—about rules, not strategies—from one game to another. Transfer of learning is crucial because, taken seriously, rule learning implies that, as people go through life, they learn which rules work best across all the strategic interactions they encounter. Generally this will imply a gradual shift of weight toward more sophisticated rules. The parameter τ gives a way of resetting rule propensities or, interpreted cognitively, incorporating how well rules transfer across games that may seem different.

The econometric estimation is complicated (conquering such challenges is Stahl's great contribution to empirical analysis of experimental data from games). Initial propensities are assumed to have a common distribution in the population but evidence weights are different for each player.

An impressive feature of Stahl's experimental approach is that he sampled billions of 5×5 symmetric normal-form games to find those which were best for distinguishing among rules (see Chapter 1 appendix), and chose four "optimally informative" games (see Table 6.23). Experiments were conducted with a mean-matching protocol—players earned the average of their payoffs playing against every other player—and players learned the population history after each trial. The right-hand columns of Table 6.23 show overall choice frequencies from periods 1–8 and 9–15. Nash equilibrium play always grows over time and is modal in later periods (except for game 2).

The overall log likelihood (LL) for the model, with nine parameters for each subject, is -1896. In Stahl's parameter-rich approach, many obvious models are special cases, so the restrictions they imply can be easily tested with standard χ^2 statistics. Models in which all the players are the same, all choose Nash equilibrium with error, or all learn only from Nash evidence, all have LL worse than -3000. The hypothesis of no rule learning ($\beta_0 = 1, \beta_1 = 0$) is strongly rejected. Each parameter helps explain how players learn because restricting parameters to be zero, one at a time, always harms fit significantly.

Table 6.23. Symmetric games used by Stahl (row payoffs only)

Decision rule	Row player payoffs					Frequency in periods 1–8	Frequency in periods 9–15
Game 1							
b1	19	43	96	85	85	0.46	0.27
Nash	28	62	88	74	24	0.18	0.52
b2	67	21	38	48	38	0.17	0.14
Worldly	40	58	0	15	92	0.05	0.02
Maximax	16	15	86	99	79	0.13	0.05
Game 2							
Worldly	68	10	76	33	75	0.51	0.60
Strictly dominated	73	4	59	0	8	0.03	0.01
b2	3	92	16	15	99	0.04	0.02
Nash	86	54	25	41	6	0.06	0.14
b1	72	98	92	8	52	0.36	0.22
Game 3							
Maximax	2	31	0	99	6	0.09	0.04
b2	6	10	97	40	24	0.07	0.02
b1	98	96	38	48	19	0.23	0.07
Worldly	42	40	80	51	48	0.36	0.24
Nash	97	46	5	68	49	0.24	0.64
Game 4							
Worldly	22	79	35	56	75	0.35	0.36
b1	22	38	78	55	99	0.25	0.12
Strictly dominated	27	58	1	11	0	0.01	0.01
Nash	70	1	34	59	37	0.26	0.36
b2	56	84	60	23	2	0.13	0.14

Source: Stahl (1999a).

Table 6.24 summarizes estimated means for each of the parameters, along with the fraction of subjects whose parameter estimates are significantly different from 0 at the 5 percent level. The largest evidence weight is put on expected payoff. The weight on past history in updating beliefs, θ, and the transfer weight, τ, are around a half. A few clusters emerge.

Table 6.24. *Mean coefficient estimates for rule-learning models*

Weighting parameter	Coefficient	Stahl (1999a)	Stahl (2001)	Stahl and Haruvy (in press)
Imitation	$\bar{\nu_0}$	1.12	—	—
Expected payoff	$\bar{\nu_1}$	1.35*	0.80	0.38
Iterated expected payoff	$\bar{\nu_2}$	0.19	0:07	0.00
Nash payoff	$\bar{\nu_3}$	0.26	0.00	0.00
Recent history	$\bar{\theta}$	0.43	0.65	0.94
Evidence weight dispersion	σ	1.15	0.77	0.80
Probability of imitation	δ_h	—	0.53	0.44
Experimentation probability	ϵ	—	0.09	0.12
Lagged propensity	β_0	1.26	1.00	1.00
Reinforcement	β_1	1.53*	0.0079	0.0047
Transfer	τ	0.47	1.00	0.31

Note: * denotes weights significant (at .05) for more than half of subjects. Significance of σ and β_0 was not tested in Stahl (1999a).

About one-third of the subjects have a particularly high value of ν_1 (corresponding to belief learning). Another sixth have a flat profile with even, low evidence weights. Another sixth have a high weight on imitation (high ν^0). The remaining third of the subjects show a variety of profiles that do not particularly fall into clusters.

Stahl (2000b) compares a wide variety of models using population data. Some of the models reinforce actions (strategies) and others reinforce rules, so his paper represents the largest "horse race" to date. Because the data used are relative frequencies of choices in the population, Stahl develops population-level analogues of theories previously applied to individuals (see Stahl, 2000a).

The first part of his ambitious paper compares replicator dynamics, a six-parameter aspiration-based reinforcement model from Roth and Erev (1995), and the Camerer–Ho (1999a) EWA model. He also defines a logit best-reply with inertia (LBRI) model in which choices are a mixture of inertia or habit (with weight δ_h) and logit best-reply to the population frequencies $p(t-1)$ from the last period (Cournot). Allowing best-response to θ-weighted adaptive expectations instead, $q(t) = \theta q(t-1) + (1-\theta)p(t-1)$ gives a model called LBRIAE. He also adds various sorts of trembles and mutations to these models.

Table 6.25. Out-of-sample performance of population learning models

Model	Measure of forecast accuracy LL	MSD	GOP
Random	−7086		
Logit Nash	−6660		
AR(1)	−5095	0.125	−356
Replicator	−5024	0.120	−312
Reinforcement	−4868	0.099	−335
LBRI	−4834	0.091	−305
EWA	−4803	0.091	−301
LBRIAE	−4794	0.088	−300
Best possible	−4296	0.000	

Source: Stahl (2001).

Stahl tries a wide variety of criteria for evaluating goodness of fit both in and out of sample. Stahl contributes to the debate about methods for fitting models by introducing a measure GOP (goodness of prediction). GOP uses parameters estimated by MLE, simulates the entire path of fifteen-period play, and computes the log likelihood of average simulated behavior in period 15. A bad model that wanders away from the data using its own simulated history will produce a poor GOP measure (even if its conventional LL and MSD are good).

The data consist of sessions with two runs of fifteen periods on separate 5×5 and 3×3 games (see Haruvy and Stahl, 1998). Summary statistics derived from estimating parameters on part of the sample and forecasting a fresh sample of subjects and sessions are shown in Table 6.25. The random, logit Nash, and AR(1)[36] models are simply naive benchmarks to see how much the learning models are really adding.

Since these models predict population frequencies rather than individuals, it is impossible to be perfectly accurate; the model called "best possible" simply guesses the population frequency correctly but still has a substantial log likelihood (although its MSD is 0).

The replicator model is a big loser. Reinforcement does modestly worse than the LBR* and EWA models. Note also that reinforcement is particularly bad at forecasting simulated paths (the GOP measure is high), contrary to

[36] The AR(1) model simply forecasts that the population frequencies in t will be the same as in $t-1$.

the claim of Erev and Haruvy (2001) based on simple pairwise choices. EWA is a little more accurate than LBRI but worse when adaptive expectations are added (LBRIAE).

In the second part of the paper, Stahl revises his earlier (2000a) population model in a way that encompasses the race-winning LBRIAE model. As in the earlier model, rules are combinations of evidence. Three kinds of evidence are expected payoffs, iterated expected payoffs, and Nash payoffs. Herding occurs with probability δ and is defined as following the past with probability θ and imitating the population with probability $1-\theta$ (akin to the LBRIAE rule).[37]

Table 6.24 shows parameter estimates. There is a large herd component (δ_h is around .50). Restricting $\beta_1 = 0$ means there are rules but no learning, and degrades LL by 10 points. Eliminating level-2 and Nash evidence ($\bar{\nu_2} = \bar{\nu_3} = 0$) gives the LBRIAE model, which is another 42 LL points worse. However, these special cases of the rule-learning model forecast almost as well out of sample (no-learning actually forecasts better by MSD and GOP measures) so it is hard to declare a clear winner. The main feature of the data that rule learning picks up, which LBRIAE does not, is the decline of herd behavior over time and the increasing propensity to use rules that choose according to expected payoffs or iterated expected payoffs.

Stahl and Haruvy (in press) add two twists to the rule-learning model (using the Haruvy and Stahl, 1998, data): aspiration-based experimentation, and cooperation. They generate aspiration-based experimentation by reinforcing an experimentation rule by the pseudo-payoff equal to the aspiration level,[38] and generate cooperation by reinforcing a cooperative rule with the largest *total* payoff.

Results of the estimation are summarized in the right column of Table 6.24. The overall LL is -8550. Setting $\beta_1 = 0$ in the basic model chokes off rule learning (there are rules, but their propensities do not change) and reduces LL only slightly, to -8569, which means switching among rules is rare. There is little evidence for aspiration-based experimentation, but including a cooperative rule improves LL by 30 points, to -8522 (roughly as important a factor as allowing rule learning). The initial probabilistic propensity to cooperate is estimated to be 3.7 percent, within an order of magnitude of estimates of "homemade" priors by Camerer and Weigelt (1988, Chapter 8) and McKelvey and Palfrey (1992, Chapter 5).

[37] The key difference from his earlier paper is that herding is treated as a separate rule rather than using $\ln(q(t))$ as "evidence" which generates imitation.

[38] Then, if aspirations are high but received payoffs are low, the experimentation rule will be more strongly reinforced than other payoff-based rules, and players will experiment. Conversely, when payoffs exceed aspirations, experimentation gets less reinforcement than other rules. The tendency to experiment will be low (representing satisfaction, or "if it ain't broke don't fix it").

6.8 Econometric Studies of Estimation Properties

Most estimation of learning models described in this chapter took place *before* the econometric properties of estimators were thoroughly investigated. Thus, it is conceivable that, for a particular game and number of experimental trials, a particular econometric method may not produce accurate results, or may not be able to distinguish different models. Three recent studies have tackled this problem in the obvious way: Try to prove as much as possible about the econometric properties of different techniques, for different games and experiment lengths, then use Monte Carlo simulation when theoretical proof is not possible.

The results from the three studies range from disappointing to encouraging. The most pessimistic conclusions have come from games with mixed equilibria and small numbers of strategies, which suggests that more complex games with pure equilibria are the right ones to study (if the goal is to estimate parameters precisely). Exercises of this kind can also be used to figure out, before running experiments, which games and experimental designs are best for model identification.

Salmon (2001) compares the Mookerhjee and Sopher (1994, 1997), Cheung and Friedman (1997), and Camerer and Ho (1999a) learning rules using several data sets. Specific rules are used to create simulated data sets—reinforcement, belief learning, and population mixture models. Econometric models are then fitted to data. Good identification occurs when the right model recovers the actual rule, and does not falsely recover the wrong rule.

The two Mookerjhee–Sopher studies use 2×2, 4×4, and 6×6 games with mixed equilibria. Identification is bad. When reinforcement rules are used to generate data, the reinforcement models fit better than belief models only in the 6×6 games, and incorrect mixture models fit as well as the correct reinforcement model.

In the Cheung–Friedman games, when ϕ weights are close to 1 (fictitious play), the FP and EWA rule with fictitious play restrictions correctly show that weights are close to 1. However, when weights are 0 (Cournot) the model correctly recovers a 0 weight half the time and mysteriously misestimates a very large weight (two or more!?) half the time. When data are generated by reinforcement or mixed-population models, the estimated fictitious play weight varies a lot.

When the EWA model is fitted to data generated by either reinforcement ($\delta = 0$) or belief learning ($\delta = 1$) rules, recovery of the proper δ is excellent in the sense that the correct restriction is rejected infrequently (about 20 percent of the time) and the wrong restriction is almost always rejected. Recovery of the cumulation parameter κ is poor, however. When the EWA model is used to simulate data, the EWA model recovers correct parameters significantly about half the time. The likelihood functions for parameter *combinations* work best and almost always pick out the right model in a direct comparison.

Salmon concludes that model identification is poor on many games that have been previously studied, particularly those with mixed equilibria and a small number of strategies. Other games have better identification. Furthermore, the EWA model usually identifies the correct value of δ, the parameter that most sharply distinguishes reinforcement and belief models. Salmon concludes by noting that other types of data besides choices (such as attentional data; see Salmon 1999; Costa-Gomes, Crawford, and Broseta, 2001; Johnson et al., 2002) may help in identifying learning rules.

Cabrales and Garcia-Fontes (2000) study the EWA model of Camerer and Ho (1999). They first prove that, if ϕ is bounded below 1, estimators that either maximize log likelihood or minimize squared deviations are consistent and asymptotically normal. Of course, asymptotic normality says little about small-sample properties of these estimators; so Cabrales and Garcia-Fontes did Monte Carlo simulations. They simulated different configurations of ϕ, δ, and λ parameters, for $T = 30$ or 1,000 trials, for 2×2 coordination (stag hunt) and prisoners' dilemma games, then saw how well estimation recovers the true values. When $T = 30$, recovered estimates of ϕ are biased downward and often inaccurate; mean δ values are quite accurate but the range of values is too highly dispersed. However, when $T = 1000$ the correct values of both parameters are recovered (within .01) and the dispersion around the true mean is close to that expected by random sampling error. These results are encouraging for identifying δ, but also a reminder that good estimation is sensitive to the length of the experiment.

Blume et al. (1999) also compare belief, reinforcement, and a hybrid model on their sender–receiver data (see Chapter 7). They find poor identification in small samples, but identification improves a lot with an increased sample of subjects and span (number of periods).

Summary: Three studies show that identification of models is sometimes poor; a wrong rule can fit as well as the actual rule that generated the data. Identification is particularly poor in games with few strategies (e.g., 2 and 4) and mixed equilibria. EWA identification of the δ parameter is good in the studies by Salmon (2001) and Cabrales and Garcia-Fontes (2000) when the experiment length is long enough. The more general lesson is that exercises such as these are not difficult to do, and are worthwhile. They should also prove valuable in the future in guiding us to choose games and experimental design features (e.g., number of periods) that *do* permit more accurate model discrimination.

6.9 Conclusions

Fitting learning models to experimental data has been very informative in the past five years. The first wave of studies focused on only one class of models (typically reinforcement, belief learning, or learning direction theory). All these approaches pass an important basic test by improving on equilibrium predictions and capturing the direction of movement in data (even though the *magnitude* of predicted changes is often far off the mark). Thus, there is no doubt that simple learning rules can approximate learning better than equilibrium (which assumes none); nor is this conclusion surprising any longer.

Since many different theories all predict better than equilibrium concepts, comparative studies become crucial if the goal is to make further progress. Most comparative studies establish two results. Reinforcement models tend to predict slightly better than belief learning in games with only mixed-strategy equilibria (although no models do much better than QRE or a static observed-frequency benchmark in those games).

But belief learning generally fits better than reinforcement in coordination games and some other classes of games (e.g., market games, dominance-solvable games). The relatively good performance of choice reinforcement on some games is surprising because these models assume players learn only from the payoffs they received, but studies show that players do learn faster when they have information besides received payoffs. Therefore, it is surprising that reinforcement reproduces some features of some games, but it also means reinforcement models are incomplete in a way that can be easily remedied.

My work with Teck Ho on EWA learning is one way of using more information to avoid the empirical sluggishness of reinforcement learning. EWA is a family of learning rules that vary how strongly forgone payoffs are weighted and the degree to which payoffs are averaged or cumulated. Since EWA includes some reinforcement and belief theories as special cases (by construction), it obviously fits better within a sample of data than those special cases. It is therefore important both to penalize the more complex EWA model for its extra parameters when judging in-sample fit, and to use holdout samples to evaluate how well the model predicts fresh data (new periods, or new subjects) once the parameters are estimated. EWA predicts better in 80–95 percent of the studies in which it has been studied. It also generates reasonably accurate simulated paths (where that has been tried) and can forecast new games with parameters estimated from other games. The major exception is games with mixed equilibria, in which all the basic models predict about equally accurately (and probably only a little better than QRE).

The point of EWA is to create an approach that is more robust across many games than simpler approaches. It is easy to construct games in which simple reinforcement does poorly (the market games in Roth and Erev, 1995, are an early example). Belief learning often predicts poorly in dominance-solvable games, where convergence is predicted by belief learning and is not always observed (e.g., Nagel and Tang, 1998; Capra et al., 1999). By building in some degree of responsiveness, EWA can improve on reinforcement when that model does badly, and also improve on belief learning by not predicting sharp convergence in dominance-solvable games as belief learning does.

EWA model parameters vary across games, but parameters from *all* other models vary across games too (e.g., Cheung and Friedman, 1997; Crawford, 1995). Parametric variation presents a challenge in predicting the behavior of new games. A one-parameter theory called "functional EWA," or fEWA for short (Ho, Camerer, and Chong, 2002), makes parameters functions of players' experience rather than free parameters, and can explain why different parameter values arise in different games.

Because EWA predicts better than simpler theories (adjusting for extra parameters in several ways), there is no compelling reason, other than historical convention, to focus attention solely on reinforcement or belief models, if one's purpose is predicting behavior as accurately as possible. fEWA is easy to estimate (it has only one free parameter) and fits better than models with more free parameters.

EWA is a hybrid that "gene-splices" features of learning processes. A different kind of hybrid is rule learning. In rule learning, rules recommend strategies and are reinforced by the payoffs of the strategies they recommend. Rule learning fits population-level data a little better than a model mixing inertia and best response, which in turn fits a little better than EWA. Rule learning is an obvious way to explain "learning to learn" and certainly deserves further exploration.

An important issue is how evidence about the statistical fit of models should interact with theoretical exploration of different models. Virtually all of the theory literature focusses on models—typically evolutionary dynamics and fictitious play—that repeatedly fit experimental data much worse than other types of models. At the same time, the statistical models look too complex, and perhaps even intimidating, to theorists. (I had lunch recently with a smart young theorist who said he was embarrassed to be working on fictitious play dynamics in the light of the experimental results showing that other theories fit data better. He said he had looked at the EWA model and found it "too complicated"—this from a theorist working on deep mathematical questions involving supermodularity, stability, and so forth! What he meant, of course, was that no ready-made tools he had learned in graduate school equipped him to start working on proving theorems about the behavior of models fitted to data. But isn't rising to that challenge what innovative science is all about?) The right compromise seems to be for theorists to begin thinking about good-fitting theories, and for those of us sharpening such theories to think about which simplifications are likely to make theorizing easier.

There are two loose ends and three challenges in crafting better theories of how people actually learn. The loose ends are learning direction theory and imitation dynamics.

In learning direction theory, players know their ex post best response and move toward it (or have some intuition about the direction in which they should move). Selten suggests that direction learning is a good place to start because, in many domains, players know the direction in which to move and know little else. But learning direction theory has never been fully specified (e.g., it applies only when strategies are ordered) and its core prediction is not surprising. My hunch is that, when players know which direction to move, they typically *also* know something more about forgone payoffs from different strategies. Then a theory such as EWA could be applied.

Another loose end is imitation. Imitation dynamics are evident in the oligopoly study of Hück, Normann, and Oechssler (1999) (less so in Bosch-Domenech and Vriend, in press) and in "herd behavior" in Stahl (1999a,b, 2000a,b). A good theory could incorporate imitation indirectly. After all, imitation is most compelling when players are symmetric, and not at all sensible when players are asymmetric. (For example, children imitate the actions of adult humans, but do not instinctively imitate their pets.) This difference is an important clue: Imitation of successful players who are similar is probably a heuristic way of moving toward strategies with high forgone payoffs. Symmetric imitation could be gracefully incorporated into a theory such as EWA, where actions of similar players are proxies for forgone payoffs.

The learning models described in this chapter will have to conquer three modeling challenges before they are applicable to all situations: sophistication, imperfect payoff information, and strategy specification.

- *Sophistication* means some players understand how others are learning. Camerer, Ho, and Chong (2002a,b) and Stahl (2000a) show the advantages of modeling sophistication in a simple way, and how "teaching" in repeated games follows naturally.
- Models that require perfect information about forgone payoffs will have to adjust to environments with *imperfect information* about payoffs (Vriend, 1997). For example, EWA can be applied in low-information settings by fixing $\delta = 0$. Richer approaches use information about possible payoffs (in extensive-form games; see Ho, Wang, and Camerer,

2002) or historical payoffs (Anderson and Camerer, 2000) as proxies for forgone payoffs (Chen and Khoroshilov, in press).

- Most learning models use stage-game strategies. Broader *strategy specifications* are often sensible, in extensive-form games, and when there is pattern-recognition (e.g., the rule learning of Stahl, 1999a; and see Duffy and Engle-Warnick, 2000; and Engle-Warnick and Slonim, 2000). However, expanding the set of strategies from stage-game strategies typically generates combinatorial explosion. The trick is to find efficient methods for winnowing a large set of feasible strategies to a few psychologically plausible ones as rapidly as people seem to (e.g., genetic algorithms). Connecting this research to psychological research on learning, particularly connectionist neural networks (e.g., Sgroi and Zizzo, 2002), may prove useful too.

7
Coordination

GAMES WITH MULTIPLE EQUILIBRIA require coordination. Even if players have a common incentive to follow a convention or commonly understood pattern of behavior, agreeing on how to behave is not simple if there are many self-enforcing conventions, or if different players prefer different conventions. Language is a ubiquitous example: To communicate, players must choose words that are understood. Another example is economic life with "participation externalities," such as trading in markets (traders want to be where other traders are to increase liquidity) or geographic concentration (Internet whizkids move to Silicon Valley because employers expect to find the most talent there, and do). Predicting which of many equilibria will be selected is perhaps the most difficult problem in game theory. This "selection" problem is essentially unsolved by analytical theory and will probably be solved only with a healthy dose of observation.

Many approaches to the selection problem have been used. One approach is to look at features of equilibria and choose those that are desirable. For example, everyone is happier in a "payoff-dominant" equilibrium and, hence, players might know to choose it. Other equilibria are less risky and are appealing when uncertain players hedge their bets. Another approach is to ask which equilibria are more likely to be reached by adaptation or evolution. Careful treatment of this important approach is beyond the scope of this book (but see Weibull, 1995, and Fudenberg and Levine, 1998).

A third approach—the subject of this chapter—is fundamentally empirical. The empirical approach tries to infer what selection principles players are using by putting them in experiments and observing what they do.

Suppose a game is played repeatedly. An obvious selection principle is "precedent": Play the equilibrium that was played previously. Sometimes equilibria are "focal" or psychologically prominent—some strategies are culturally understood to be the ones people are likely to choose (such as equal splits in bargaining). The empirical research can be summarized as a catalog of which selection principles are used in which sorts of situations.

Coordination games illustrate why observation is useful. It is unlikely that a purely mathematical theory of rational play will ever fully identify which of many equilibria are likely to emerge because history, shared background, and the way strategies are described or made psychologically prominent surely matter. As a result, experiments and observation of the sort that naturalists do in biology can potentially do what mathematical analysis cannot—predict what will happen. As Schelling (1960, p. 164) put it, "One cannot, without empirical evidence, deduce what understandings can be perceived in a nonzero-sum game of maneuver any more than one can prove, by purely formal deduction, that a particular joke is bound to be funny."

Furthermore, precisely what people know about each other and can see are likely to affect coordination in subtle ways. Chwe (2001) is a fascinating account of how delicate details of how groups are physically organized can influence whether uprisings take place (in union organizing) or don't (in prisons).

Given the large relative payoff to experimental observation, it is surprising that coordination games have been relatively understudied compared with, say, public goods games or prisoners' dilemmas. Perhaps a bias against empirical observation arose because coordination games might be easily "solved" by communication, in principle. This prejudice is wrong in practice and in theory. In practice (at least in experiments), communication usually improves coordination but it does not *always* help, and communication rarely leads to full efficiency. In theory, communication is not really a solution because simple coordination games with few players who do not talk are really meant as micro-scale reduced-form models of large social processes in which players cannot all talk at the same time (and large public announcements may not be believed). Examples include coordinating activity in the macroeconomy, commuters responding to an unexpected closing of the Bay Bridge in San Francisco, and the use of speed limits as a coordinating device.[1]

Miscoordination can lead to inefficiencies that are hard to reverse. Language is again a good example. English has many odd features that make

[1] Lave (1985, p. 1159) argues that "For peculiar historical reasons, speed laws evolved as limits on driver behavior, rather than as signaling devices meant to coordinate it. . . . This paper tests these differing views of the law by examining the current effects of the 55 mph NMSL [national maximum speed limit]—should it be viewed as a coordinating mechanism or a limiting mechanism?" Lave finds that "variance kills, not speed" and concludes that speed limit laws are useful because they reduce variance (i.e., enhance coordination).

learning the language difficult. But convention rules by tyranny—if English is spoken in an odd way, and has a large "installed base" of speakers who understand its peccadillos, then new speakers must learn the peccadillos too.[2] Nowadays, English seems to be growing as the international language because of the advantage English speakers have in using the Internet. These users inherit the quirky features of English like bad genes.

Here are some examples of where coordination matters and how it arose:

- Why is the standard width (gauge) of U.S. railroad tracks 4 feet and 8.5 inches?[3] The answer is that English wagons built to carry goods and people were made about 5 feet wide, the width of two horses. Tramway builders used the same jigs and tools as the wagon makers, so they made tram lines 5 feet wide less 4 inches for track, plus a half inch for some subtle technical reason. English railroads were built by the tram builders, who used the width they knew best. English expatriates built the U.S. railroads and copied the English tracks they were familiar with. At many points along the way of this two-millennium journey, a coordinated effort to switch to a different standard could have been achieved, but wasn't. The result was that the NASA Space Shuttle is powered by two big booster rockets that are smaller than engineers would have liked them to be. Why? The rockets had to be shipped by train from a factory in Utah to the launch site. Since the train track width was determined by the construction of wagons, pulled by teams that were two horses wide, the Space Shuttle rockets were two horses wide as well. (In a sense, the shuttle was designed by a horse's ass!) An interesting twist: Russia deliberately chose a different track width—deliberately to *mis*coordinate—either to make invasions harder or to signal non-offensive intentions.
- Economic geographers are interested in why some industries are so heavily concentrated in small areas. The answer is that a small historical accident can determine a town's long-run concentration. For example, a woman in Smyrna, Georgia injured her hand and had to learn to stitch quilts in a different way. She developed a new technique which turned

[2] The attempt to replace different languages by the more efficient hybrid of Esperanto—which is spoken by game theorists Roger Myerson and Reinhard Selten, among others—hasn't caught on.

[3] Thanks to my sister Doreen and Giovanna Devetag for this example. A variant of this story traces the track gauge further back to Roman chariots, which supposedly carved ruts all over Europe, so the English wagon-makers were then forced to make their wagons the same width so axles wouldn't break as wheels dipped in and out of the Roman ruts. Apparently the Roman-chariot link is largely myth because the Roman chariots were not of uniform width, and there is no evidence that the English tram makers were influenced by the Romans. Furthermore, the rocket boosters were not entirely constrained by the track width (the constraint was passing through tunnels, which are typically wider than track) but the point that horse booty influenced rocket design through a long historical chain still holds. See http://www.straightdope.com/columns/000218.html for more.

out to be more efficient than the old way, and taught it to several friends. Decades later, Smyrna emerged as one of the rug-stitching capitals of the world because her technique was best taught from friend to friend (e.g., Krugman, 1992). Another famous example is Silicon Valley. Silicon Valley emerged because of the influence of local universities and some historical accidents. Being far away from the stuffy bankers in New York enabled the creation of a culture of proud scientific innovation—every "us" needs a "them"—in which cleverness, workaholic zeal, and engineering feats were valued more highly than money, fancy suits, and Ivy League pedigree. (Many early Silicon Valley pioneers were midwestern boys who had learned to cobble together inventions from scratch on the farm, out of necessity. Their do-it-yourself skills and philosophy were ideally suited to the technology of computing and new economy.) Making movies coalesced in Hollywood because the reliable, dry climate (ten days of rain a year) lowered the weather risk of shooting movies outside. Like a speck of dirt which becomes a pearl in an oyster, when there are increasing returns to scale or geographical externalities a little accident can lead to an entire industry.

- Urban gentrification often occurs in a small city (Manhattan, San Francisco), where expensive housing drives wealthy yuppies to areas where prices are cheaper because some kind of physical risk or undesirable feature drives down prices. Then a new gentry want to live where the yuppies are. Gentrification usually happens rapidly. A small critical mass can create a Schellingesque (1978) "tipping point," a point at which the percentage of people who want to do something changes rapidly from none to all. Examples include Soho in the 1970s in Manhattan, then Brooklyn; South of Market (SoMa) in San Francisco in the 1990s; Los Feliz in Los Angeles in the 1990s; Brixton in London in the late 1990s; and so on.
- A classic example of pure coordination is the emergence of conventions for driving on the left- and right-hand side of roads. (Supposedly the American convention of driving on the right, contrary to the British, emerged from having to hold a whip in the right hand to drive a wagon to market and wanting to avoid hitting a passerby if you drove on the left.) A wonderful twist on this example comes from Bolivia.[4] Ascending from the cities to mountainous areas, the roads become narrow and treacherous, with steep cliffs and frequent accidents. (When a bus driver arrives home in his village from the city, his family gathers around and celebrates the fact that he made it back alive.) In the cities drivers stay to the right (and sit in the left side of a car). But on mountain roads this convention is risky because the driver cannot see very clearly where

[4] Mónica Capra told me this great story.

the cliffside is. (Imagine sitting in the left side of the car, American style, and peering across the passenger side to emptiness beyond.) So the convention (which is enforced by signs) *switches* to driving on the left on mountain roads! This way both drivers can see the cliffside more clearly. Another driving example: Pittsburgh has many two-way streets with one lane in each direction. Normally drivers who are making left-hand turns must wait for oncoming traffic to pass before turning, but this rule creates long delays for those who are waiting behind the left-turner because streets have just one lane. So a convention developed—the "Pittsburgh left"—which unofficially permits drivers turning left to turn *first* (contrary to state law).

- Categorizing products is a coordination problem. Is the movie *Casino* in the "action" category at Blockbuster or in "drama"? Buyer and seller have a common interest in getting customers in and out of the store rapidly, so nobody cares how the movie is categorized as long as it's easy to find. The two sides play a coordination game in which movies must be assigned to a small number of categories, and both sides benefit if they can guess which category the other side chose.
- Usually the desire to communicate produces a preference for a common language. For example, the rise of the internet may increase widespread English speaking across the world. (Unintended consequence: Young Koreans eager to learn English are actually having their tongues loosened surgically—as funnyman Dave Barry would say, "I'm not making this up!"—hoping it will improve their pronunciation.) At other times one group of people may want to disguise what they are saying in front of others; then having an obscure, impenetrable language is better than a widely spoken one. In World War II, twenty-nine Navajo Indians were recruited as "code talkers" in the Pacific to communicate front-line commands, because their native language, Dineé, had such complex syntax and tonal subtleties that it was unusually difficult for expert Japanese code breakers to break.

In these examples, agreeing on *some choice* is often more important than what is agreed upon, but some coordinated choices are better than others. Actors and software engineers want to be where demand for their skills is (and firms will locate where they expect those skilled people to congregate); yuppies want to live in yuppie neighborhoods; and video store customers with good taste want to quickly find *Jackie Brown* (action? or blaxploitation?) and *Crouching Tiger, Hidden Dragon* (Hong Kong–style action, wu-xia flavor? or romance?); debt-heavy nations want patient investors. They all need to coordinate.

In this chapter, coordination games are divided into three categories: matching games; games with asymmetric payoffs; and games with asymmetric equilibria.

In matching games, all equilibria have the same payoffs for each player.[5] These games are useful for studying how "psychologically prominent" focal points emerge from psychophysical, semantic, and cultural considerations. In some coordination games, payoffs are asymmetric so players disagree about which equilibrium is best. The battle-of-the-sexes (BOS) game is a canonical example. In BOS, variables that distinguish one player from another—such as communication, preplay options, timing (first-mover advantages or disadvantages), or cues to bargaining power (such as past reputations)—determine which equilibrium is selected. In other games, players are symmetric but equilibria are not. A game with this feature is "stag hunt" (also called the "assurance game"). In stag hunt, two players choose risky or safe actions. If both choose the risky action they earn *more* than if they had played it safe, but if the other person doesn't choose the risky action the risk-taking player earns *less*. Selection principles that distinguish different kinds of equilibria, such as payoff-dominance and risk-dominance, are tested by these games.

Each class of games has a different source of strategic uncertainty about what others will do, which makes coordination difficult. In matching games, strategic uncertainty comes from the lack of structural ways to distinguish among equilibria. In BOS, strategic uncertainty comes from doubt about which player deserved the better outcome. In stag hunt, strategic uncertainty comes from the conflict between the shared motive for a higher payoff (expressed by payoff-dominance) and the individual motive to avoid risk (expressed by risk-dominance). Excellent overviews of work on coordination are given by Ochs (1995a) and Crawford (1997).

7.1 Matching Games

In the fall of 1988 *GAMES* magazine (1989) conducted a contest. Players could submit mock votes for each of nine celebrities, one for president and another for vice-president. From the pool of contestants who "voted" for the most popular presidential candidate, one contestant was drawn and

[5] Matching games as defined here are different from the kind of matching studied experimentally by Harrison and McCabe (1996a), Kagel and Roth (2000), and Haruvy, Roth, and Unver (2001), and the mathematical study of matching algorithms stretching back to the 1950s. In these matching games players seek to consummate one-to-one or many-to-one matches, but players have idiosyncratic preferences about whom to match with. (In the early game theory literature this was often called the "marriage problem" for obvious reasons, and has also been applied to matching of medical residents to hospitals, sorority "rush" matching students to sororities, College Bowl games, and so forth.) In those matching problems, the key is to find workable algorithms which guarantee some degree of Pareto-efficiency in matches. In the Schelling-type "pure matching games," *which* "match" occurs is irrelevant and so the focus is entirely on how shared focal points can guide equilibration.

Table 7.1. Results of GAMES magazine matching contest

President	Vice-president									Vice-president total
	Oprah	Pete	Bruce	Lee	Ann	Bill	Sly	Pee-Wee	Shirley	
Oprah Winfrey	—	35	63	218	35	247	41	110	29	778
Pete Rose	36	—	33	74	25	67	32	36	20	323
Bruce Springsteen	45	36	—	71	26	139	49	65	23	454
Lee Iaccoca	56	36	41	—	31	155	34	39	22	414
Ann Landers	48	30	37	149	—	365	29	58	28	744
Bill Cosby	122	41	83	435	41	—	53	147	21	943
Sly Stallone	36	27	66	58	27	117	—	145	20	496
Pee-Wee Herman	61	32	75	84	76	343	90	—	33	794
Shirley MacLaine	33	30	37	66	30	56	29	56	—	337
President total	437	267	435	1,155	291	1,489	357	656	196	5,283

Source: GAMES magazine (1989).

paid a dollar sum which was independent of their choice. Each contestant was trying to guess what each other contestant would guess, precisely as in Keynes's description of the stock market as a "beauty contest." The results are shown in Table 7.1. The celebrities getting the largest number of the 5,283 presidential votes were Bill Cosby (1489 votes), followed by Lee Iacocca and Pee-Wee Herman. Actress and self-proclaimed reincarnate Shirley MacLaine finished last.

The *GAMES* contest is a pure matching game because the amount earned by matching the most people was independent of what one chose. Good matching requires figuring out which of the equally profitable strategies the most people would pick (knowing others have the same goal). At the time of the game, the popular Cosby had a successful television show, which may have served as a focal principle; but so did Pee-Wee Herman and Oprah Winfrey, who finished fourth. Lee Iacocca had actually been mentioned as a possible candidate for President (of the United States), which may have directed attention toward him.

Table 7.2 shows a simple pure matching game, where strategies are labeled A and B. Choosing A or choosing B are both equilibria, and no sophisticated selection principles apply (i.e., refinements of Nash equilibrium). However, some equilibria might be "focal" or "psychologically prominent" (as Schelling pointed out in 1960) because of precedent[6] or physical or

[6] Pascal wrote, "Why do we follow old laws and old opinions? Because they are better? No, but they are unique, and remove sources of diversity."

Table 7.2. A matching game

	A	B
A	1,1	0,0
B	0,0	1,1

semantic distinctions in the way strategies are labeled—e.g., middleness, upper-leftness, choose the strategy with the longest number of vowels, etc.

Despite the charm of these games, and Schelling's compelling examples, amazingly little hard evidence has been collected about how people play them. Mehta, Starmer, and Sugden (1994a,b) ran the most thorough experiments. The percentage of coordinated responses ranged from 10 percent and up in games with many strategies. For example, 29 percent picked the number 1 when asked to choose a number, 28 percent picked the time "noon" from the set of times of day, and 38 percent picked Trafalgar Square from the set of meeting places in London. Sometimes a sharp focal point sticks out—87 percent chose heads out of the set {heads,tails}, 89 percent chose Mt. Everest from the (large) set of mountains, and 89 percent chose Ford from the set of car makes.

In their 1994a paper, Mehta et al. were interested in where strategic salience, or focality, came from. They contrasted a pure picking condition, in which players simply picked (P) strategies, with a coordinating (C) condition in which subjects earned £1 for each match with another randomly chosen subject. Comparing choices by the P group and the C group enabled a test of whether some strategies are simply preferred (and hence chosen by Ps and Cs alike), or whether players know certain strategies are focal and pick them more often in the C condition.

Results from several games are shown in Table 7.3. The table shows the frequency of the most common choices, the total number of different responses given (r), and a coordination index c (the fraction of matches if responses were randomly matched). The frequency of matching is impressive. Asked to name a year, the pickers chose forty-three different years—they picked 1971 (probably the most frequent birth year among the young students) 8.0 percent of the time, and 1990 (the year of the experiment) 6.8 percent of the time. However, they managed to coordinate by picking the year 1990 61.1 percent of the time. The eighty-eight pickers chose a total of seventy-five different dates (presumably their privately known birthdays) but in the matching condition nearly half chose December 25, Christmas Day.

Slim popularity differences in the picking condition blossomed into bigger advantages in matching. Pickers chose the color blue slightly more

Table 7.3. Results of picking (P) and coordinating (C)

	Group P ($n = 88$)		Group C ($n = 90$)	
	Response	Proportion	Response	Proportion
Years	1971	8.0	1990	61.1
	1990	6.8	2000	11.1
	2000	6.8	1969	5.6
	1968	5.7		
	$r = 43$	$c = 0.026$	$r = 15$	$c = 0.383$
Flowers	Rose	35.2	Rose	66.7
	Daffodil	13.6	Daisy	13.3
	Daisy	10.2	Daffodil	6.7
	$r = 26$	$c = 0.184$	$r = 11$	$c = 0.447$
Dates	25 December	5.7	25 December	44.4
	10 December	1.1	10 December	18.9
	1 January	1.1	1 January	8.9
	$r = 75$	$c = 0.005$	$r = 19$	$c = 0.238$
Towns	London	15.9	London	55.6
	Norwich	12.5	Norwich	34.4
	Birmingham	8.0		
	$r = 36$	$c = 0.054$	$r = 8$	$c = 0.238$
Numbers	7	11.4	1	40.0
	2	10.2	7	14.4
	10	5.7	10	13.3
	1	4.5	2	11.1
	$r = 28$	$c = 0.052$	$r = 17$	$c = 0.206$
Colors	Blue	38.6	Red	58.9
	Red	33.0	Blue	27.8
	Green	12.5		
	$r = 12$	$c = 0.269$	$r = 6$	$c = 0.422$
Boys' names	John	9.1	John	50.0
	Fred	6.8	Peter	8.9
	David	5.7	Paul	6.7
	$r = 50$	$c = 0.002$	$r = 19$	$c = 0.264$
Coin toss	Heads	76.1	Heads	86.7
	$r = 5$	$c = 0.618$	$r = 3$	$c = 0.764$
Gender	Him	53.4	Him	84.4
	$r = 6$	$c = 0.447$	$r = 2$	$c = 0.734$

Source: Mehta et al. (1994a).

often than red but they chose red twice as often when matching (58.9 percent versus 27.8 percent). "Him" beats "her" by a small margin in picking but wins by 84.4 percent to 15.6 percent in matching.

Bardsley et al. (2001) dissected the difference between picking and matching more carefully. They used choice sets in which one of four choices was distinctive. For example, the city Bern is distinctive in the set {Bern, Barbados, Honolulu, Florida} because the last three locations are warm vacation spots and Bern is not. They compared choices in a picking condition, in a guessing condition in which subjects guessed what most people picked, and in a matching condition. (Their design adds guessing to the P and C conditions above.) If salience in matching arises from knowledge of what people like, what people believe people like, and so forth, then the modes in all three conditions will be the same; they call this condition "derivative salience." In contrast, "Schelling salience" (or nonderivative salience) is present when the mode in the matching (C) condition is different than in picking and guessing (e.g., if subjects choose Bern when matching but pick Florida and guess that others pick Florida). In fact,[7] Schelling salience predicts well. In twelve of fourteen games, the distinctive choice is the modal one, chosen about 60 percent of the time, even though distinctive choices are usually picked and guessed less often.

7.1.1 Assignment Games and Visual Selection

Some matching games are "assignment games." In an assignment game, a strategy assigns each object in a set to one category or another. Economic examples include dividing people into teams, firms choosing locations in a physical space or attribute space (e.g., Ochs, 1990), assigning objects in an estate to heirs, categorizing movies into genres in a video store, dividing assets of a bankrupt firm among creditors, and dividing community property in a divorce.

In one class of assignment games, several circles must each be assigned to a left or right square.

Some focal principles that might be used in assignment are anticipated by Hume's (1978 [1740]) analysis of justice and property. As Mehta et al. (1994b, p. 170) wrote,

> Hume argues that people recognize the advantages to individual self-interest gained from interaction in society. But they also recognize that the major source of conflict in society emanates from goods with the characteristic that they can easily be transferred from person to person.

[7] In their experiments, focality of physical location (such as first on the list or upper left) was cleverly disabled by having stimuli "swim" around a computer screen.

> He suggests convention has emerged as the means by which such conflicts may be averted: convention enables each person to recognize what others will accept as being his or her property.

Hume suggested that property conventions arise from analogy and metaphor. Since "property of" is a relation between people and objects, natural analogies for property relations might be found in other kinds of people–object relations, such as spatial or temporal proximity, physical resemblance, cultural attachment, and so forth. Spatial proximity is often used to decide which countries or people have rights to catch fish or mine minerals. Temporal proximity is embodied in the principle of "finder's keepers" or "first possession," which awards an object to the person who found it first. [8]

Following Hume, Mehta et al. describe three focal principles for assigning objects to "owner" A or B: *Closeness*—Assign an object to whichever of A or B is closer; *equality*—assign half to A and half to B; and *accession*—if a subset of objects are closely related to one another, the subset should not be broken up.

These rules are evident in everyday bargaining and folk law assigning assets, costs, debts, and damages to various parties. Suppose apples fall from two trees owned by neighbors, Mr. Left and Ms. Right. The trees' branches are spread widely and it is impossible to tell which apple fell from which tree. Who owns which apples? The rule of closeness gives an apple to the person whose tree it fell closest to. The rule of equality divides the apples equally. The rule of accession insists that a cluster of apples that fell close together not be separated. (Keeping nearby apples together is silly, but accession has much appeal for awarding custody of siblings in a divorce or adoption, or assigning assets that are productive complements to creditors.)

A set of questions from Mehta et al. (1994b) designed to test these principles are shown in Figure 7.1. In each question, each of several circles (numbered 1–5) is assigned to a square. If a subject's assignment exactly matches the assignment of another subject (for all circles), she earns money from a pool. Table 7.4 shows the assignments of circles predicted by each of the three selection rules defined above, in two studies by Mehta et al. (1994a,b). The percentages of subjects choosing consistently with each rule are shown in parentheses. For example, in question 20 of Mehta et al. (1994b), closeness assigns circles one and two to the left and the other three to the right (32 percent do this), accession assigns circles four and five to the right (43 percent do this), and equality is mute.

[8] Mehta et al. (1994b) quote a Humean example in which cultural "ownership" matters (cf. Kreps, 1990, pp. 424–25): "a German, a Frenchman, and a Spaniard enter a room in which there are three bottles of wine—Rhenish, Burgundy, and port. If they fall into a dispute about who should have which bottle, Hume says, the obvious solution is that each should take the product of his own country" (1994b, p. 170).

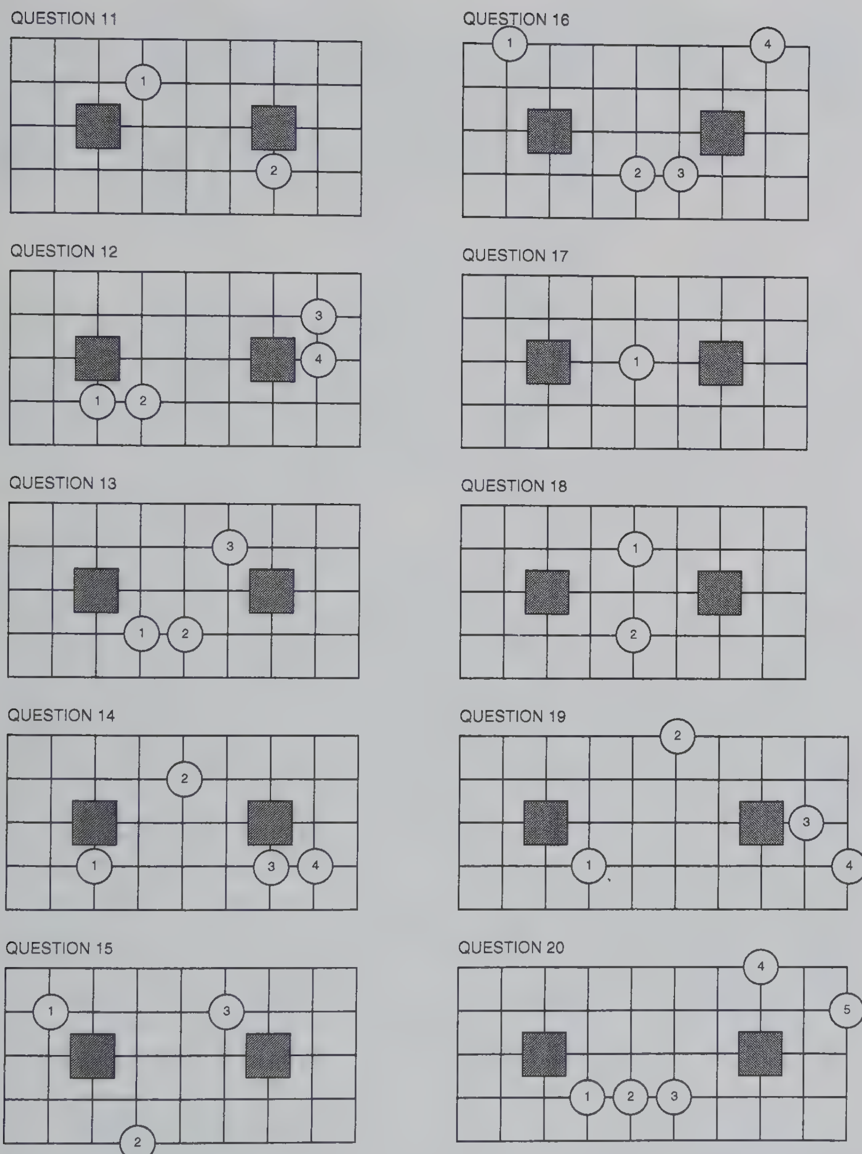


Figure 7.1. *Circle-to-square assignment games. Source: Based on Mehta, Starmer, and Sugden (1994b).*

Table 7.4. Choices consistent with various assignment rules

Question	Closeness	Accession	Equality
11	LR (74)	LR (74)	LR (74)
12	LLRR (68)	LLRR (68)	LLRR (68)
13	L*R (70+)	LLR (70)	None
14	L*RR (76)	**RR (76+)	LLRR (68)
15	LLR (71)	LLR (71)	None
16	L*RR (73)	LRRR (5)	LLRR (68)
19	LRRR (29)	LRRR (29)	LLRR (45)
20	LLRRR (32)	LLLRR (43)	None

Source: Mehta et al. (1994b).
Note: Figures in parentheses show the percentage of subjects choosing consistently with each rule. * Denotes *either* L or R permitted.

When all three rules agree, about 70 percent of the subjects chose the assignment the rules imply. Some of the questions are designed to pit principles against one another. In question 19, closeness and accession predict a lopsided split, assigning the middle circle 2 to the right (because it's closer to the right square). About a third of the subjects made that assignment, but nearly half enforced equality and assigned circle 2 to the left. Question 20 is a straight choice between closeness, which assigns the middle circle 3 to its nearest square (on the right), and accession, which keeps 3 with its "siblings" and assigns it left. Accession beat closeness by a small margin, 43 percent versus 32 percent. Question 16 pits accession against equality: Does circle 2 stay with its sibling 3, and get assigned to the right, or go left to create equality? Most subjects voted for equality and only a few for accession. Looking across all the questions, it appears that equality is a primary principle that is always respected, and accession and closeness considerations are applied about equally strongly when equality is satisfied.

There are many interesting experiments on matching that could be done. Matching games could be used to measure cultural "strength": Ask subjects questions about shared values and see how well answers correlate across subjects.[9] The degree of shared understanding might also predict

[9] For example, a company's employees could be asked questions such as, "Is it appropriate to bend the rules for an angry customer even if it means costing the company a small sum?" Some cultures clearly allow this indulgence (some luxury hotel chains have rules such as "Spend up to $1,000 to solve a customer's problem"); others do not. But this measure of one employee's answer does not get at truly *shared* values, which players know that others know (and others know they know, and so on). To measure shared values,

success in relationships, such as marriages. On the *Newlywed Game* television show, two newlyweds are separated and asked questions, usually with bawdy overtones designed to draw snickers like, "In what room of your house would your wife say you two have the most fun?" If the husband's answer matches the wife's answer they win a prize. Perhaps matching tasks such as this helps predict the length of a marriage and the chance of divorce.

7.1.2 Unpacking Focality

Bacharach and Bernasconi (1997) unpack what it means for a strategy to be focal (see also theory by Gauthier, 1975; Crawford and Haller, 1990; Bacharach, 1993; Sugden, 1995). The basic elements are evident from the matching games discussed above, and from Schelling's earlier work: People make perceptual and other distinctions among strategies—labels matter—then "use" distinctions which are most likely to produce a match.

The elements in the Bacharach framework are *actions* Ω; *attributes* which partition Ω into subsets with and without the attribute; and *subtlety*, the probability that an attribute is recognized by other players. Players are assumed to randomize within a subset of options with the same available attributes, and then apply payoff-dominance. Attributes that are most distinctive are preferred as selection principles because they yield a higher chance of a match, and hence higher expected payoffs. However, since distinctive attributes are usually less recognizable, a tradeoff between the distinctiveness of an attribute and its subtlety can arise. Bacharach and Bernasconi ran experiments to test some implications of this framework. Because subjects are eager to coordinate, "nuisance" attributes which have not been controlled by the experimenter—such as the position of an action on the page—loom large. Bacharach and Bernasconi try to disable these nuisance attributes by inserting subjects' pages in their packet of experimental materials in different orientations. Then there is no natural top/bottom or left/right because each subject has privatized values of these orientations. (Computerized displays would be useful for this purpose too.)

Their results can be organized by the principles they test.

Tests of Rarity Preference

Rarity preference means subjects should choose objects that are rarer—fewer of them share the same attribute. Figure 7.2 displays a typical picture subjects saw. The display shows six flowers and eight triangles, drawn to look

players should be asked how *others* would answer questions. Why stop there? Players should also wonder how others think *they* will answer questions. Schelling-type matching games provide an ideal—and simple—tool for measuring shared cultural values.

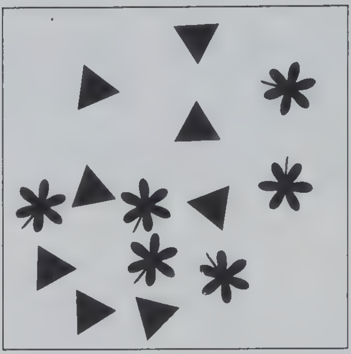


Figure 7.2. *Example of a picture used to test "rarity preference" in matching games. Source: Based on Bacharach and Bernasconi (1997).*

as similar as possible. Table 7.5 reports the percentage of times that subjects chose relatively rare or frequent options in four games with varying degrees of rarity. In games such as the one pictured in Figure 7.2, strength is an increasing function of the gap between the number of rare and frequent actions. When the rare action is an "oddity," the only action with a particular attribute, the oddity is chosen 94 percent of the time.

Tests of Tradeoff

Suppose a game has an oddity—a singleton option which is the only action that has feature F—but the feature F is subtle and is recognized with probability $p(F)$. And suppose some of the other actions can be classed into a group that is relatively rare, with n elements. Then the tradeoff principle says that people decide to choose the oddity, rather than take their chances matching on one of the members of the rare set, if $p(F) > 1/n$.

Table 7.5. *Frequency of choices of rare versus frequent actions (percent)*

	Number of rare/frequent options			
	6/8	2/3	6/18	1/15
Rare	65	76	77	94
Frequent	35	24	23	6

Source: Bacharach and Bernasconi (1997).

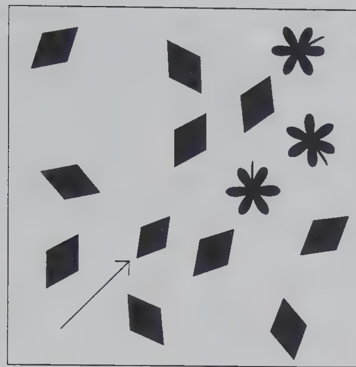


Figure 7.3. Example of a picture with object size differences used to test "tradeoff." Source: Based on Bacharach and Bernasconi (1997).

Figure 7.3 shows a stimulus used to test for tradeoff. The actions are three flowers and eleven diamonds. One of the diamonds, just to the lower left of center, is slightly smaller than the rest (marked by an arrow, which subjects did not see). This size difference is a subtle feature that creates a distinctive oddity diamond if subjects notice it (and think others do too). To test tradeoff one must induce a degree of availability $p(F)$, then measure it. Bacharach and Bernasconi did so by manipulating two dimensions of the oddity objects—their size and their intensity (boldness of color)—and asking subjects whether they noticed the oddity. In the obvious conditions, 94 percent of the subjects noticed the oddity, so $p(F)$ is estimated to be .94. In the subtle conditions, 40 percent of the subjects noticed the oddity. Both sessions of each condition are pooled and reported in Table 7.6.

The table shows the number of choices of the oddity and of one of the rare options, ranked by the number of options in the rare set r (from 2–6) and by the subtlety of the oddity $p(F)$. The tradeoff principle predicts that if the percentage noticing the oddity, $p(F)$, is above $1/r$, people should pick the oddity; otherwise they should pick a rare option. Because $p(F)$ happens to be always above $1/r$, people should always choose the oddity.

The oddity is usually chosen when it is obviously unique (i.e., $p(F)$ is high; the left half of Table 7.6). However, when an oddity is only subtly unique ($p(F)$ is low; the right half of Table 7.6) it is chosen less than half the time. For example, Figure 7.3 shows a subtle size oddity (only 40 percent noticed that one diamond was smaller) and three options in the rare class (flowers). In this case, 36 percent picked the small diamond and 64 percent chose one of the flowers. As the gap between $p(F)$ and $1/r$ increases, the

Table 7.6. *Frequency of choices in tests of tradeoff, fourteen actions in total (percent)*

	Subtlety of oddity attribute								
	Number of rare options r								
	Obvious				Subtle				
	2	3	4	5	2	3	4	5	6
Rare	14	19	9	7	77	55	45	69	55
Oddity	83	79	91	88	23	31	45	19	20
Other	2	2	0	5	0	14	10	12	25
$p(F)$	0.95	0.91	0.95	0.93	0.55	0.40	0.62	0.25	0.25

Source: Bacharach and Bernasconi (1997).

oddity should be chosen more often but there is not enough variation in the size of that gap to test this prediction.

Bacharach also conjectured a property called "symmetry disqualification" but it does not appear to be widely followed.[10]

Munro (1999) reports on an unusual field study of coordination in physical matching games. He watched ten samples of bicyclists approaching from opposite directions on bike paths around a college campus in Japan. (He chose paths that did not have lines painted down the middle; on these paths, bicyclists tend to ride in the middle and have to coordinate on which direction to swerve.) When two bicyclists approached each other, did they both go left, or both go right? Or did they fail to coordinate and collide? Munro found that collisions were very rare, and bicyclists went left 80 percent of the time. People drive cars on the left in Japan (though it is a little surprising that a more uniform convention did not emerge).[11]

[10] Suppose there are two circles and one each of a diamond, square, and triangle. Since there is only one diamond, for example, you might think that choosing it is a good idea. But there is nothing special which distinguishes the partitions (diamond, non-diamond), (square, non-square), and (triangle, non-triangle). Symmetry disqualification says that if no feature favors any of these three partitions then even though each partition has a unique "singleton" element in it, there is only a 1/3 chance that you will coordinate on the same singleton as another player. You should "disqualify" the three singleton partitions and pick one of the two circles, where you have a 1/2 chance of matching. This principle does not predict well when it disqualifies partitions with singletons and predicts choice of one of two or more objects (e.g., only 12 percent chose one of the two circles, and more than half chose the triangle). Symmetry disqualification does predict well when it favors one of two or more singleton choices over multiple-object choices, but these patterns can be explained by other simple theories.

[11] Munro points out that virtually any learning theory predicts an increasing tendency toward one convention or the other over time, which is not evident in the data. When traffic was heavy, a strong left convention emerged spontaneously, but when the flow slowed down cyclists returned to negotiating a direction separately for each encounter. Munro also compared learning models, regressing a cyclist's

Summary: In pure matching games players choose an object from a set and win a fixed prize if they match. These games are extreme illustrations of what happens when players care *only* about coordinating, and don't care what they coordinate on. Mehta, Starmer, and Sugden found that players use shared knowledge of which choices are "focal" or "psychologically prominent" (in Schelling's terms) to match much better than chance, though matching is not perfect. They also found that coordination does not derive solely from preferences or guesses about preferences; it seems to depend on what choices are most distinctive and are noticed. Bacharach and Bernasconi also show a strong effect of labeling actions and a preference for actions with more distinctive features, and modest evidence that subjects trade off subtlety with distinctiveness.

In assignment games where different focal principles can be applied, there is a rough ranking in which equality is respected first, followed by accession and then closeness. It would also be interesting to study matching games embedded in economic contexts of special interest—division of assets or debts, division of workers into categories, or focal principles that are part of "organizational culture" (see Camerer and Vepsalainen, 1988; and Kreps, 1990). Also, it is possible that persistent failure to match one's peers may be an index of poor social acuity, perhaps linked to deficits in "theory of mind" or autism.

7.2 Asymmetric Players: Battle of the Sexes

Table 7.7 shows a "battle of the sexes," a canonical game with asymmetric player equilibria. In this modern version of BOS, two people are interested in coordinating their choice of a movie to see. Chris (playing as Column) wants to see *Waiting to Exhale* whereas Randy (Row) prefers *The Usual Suspects.* Both would like to match, but each prefers a different movie. The pure-strategy equilibria are (Exhale, Exhale), which Chris likes best, and (Suspects, Suspects), which Randy prefers. There is a third mixed-strategy equilibrium in which players mix by choosing their preferred movie with probability 3/4. In that equilibrium, the expected payoffs are 0.75, which are worse for both players than either of the pure-strategy equilibrium payoffs.

The two equilibria are symmetric, up to the identity of the players, because both yield 1 to one player and 2 to another. A dispassionate arbitrator who cared equally about the two players would be indifferent between the

reinforced strategy (i.e., successful left or right movement) with the fraction of observed left movements, to compare reinforcement and belief learning. Like some other studies which combine the two terms, he found that the belief term was six times larger than the reinforcement term; see Chapter 6).

Table 7.7. Battle of the sexes

		Chris	
		Waiting to Exhale	*Usual Suspects*
Randy	*Exhale*	1,3	0,0
	Suspects	0,0	3,1

Table 7.8. Battle of the sexes (Cooper et al.)

		Column	
		1	2
Row	1	0,0	200,600
	2	600,200	0,0

Source: Cooper et al. (1994)

two pure equilibria. Hence, only selection principles that distinguish one player from the other will help them coordinate. BOS captures an important "mixed motive" social situation—both players want to coordinate (a shared or social motive), but they disagree on which strategy to coordinate on (because of individual motives).

An elegant experimental investigation of BOS was conducted by Cooper, DeJong, Forsythe, and Ross (1990, 1994). In the 1990s the well-coordinated Cooper et al. group, based in Iowa, produced a long series of beautiful studies of coordination games. The BOS game used by Cooper et al. is shown in Table 7.8. Both players' preferred equilibrium strategy is 2. The payoffs 600 and 200 are numbers of lottery tickets.[12]

The results from several treatments are shown in Table 7.9. The table shows the numbers and percentages of pairs choosing each of the two equilibrium outcomes and either of the disequilibrium outcomes, from the last half of a twenty-two-period experiment.

Coordination failure is common. The players mismatch 59 percent of the time in BOS, very close to the 62.5 percent mismatch rate predicted by the mixed-strategy equilibrium. Even if the subjects are not deliberately

[12] After each round, a lottery for \$1 or \$2 was conducted (depending on the session conducted). In the lotteries, X tickets give the player an $X/1000$ chance of winning; the 600 payoff means a 60 percent chance of winning.

Table 7.9. *Battle of the sexes game: Last eleven periods*

Game	Outside option	(1,2)	(2,1)	(1,1) or (2,2)	Total
BOS	—	37	31	97	165
		(22%)	(19%)	(59%)	
STRAUB	—	24	13	53	90
		(26%)	(14%)	(60%)	
BOS-300	33	0	119	13	165
		(0%)	(90%)	(10%)	
BOS-100	3	5	102	55	165
		(3%)	(63%)	(34%)	
BOS-1W	—	1	158	6	165
		(1%)	(96%)	(4%)	
BOS-2W	—	49	47	69	165
		(30%)	(28%)	(42%)	
BOS-SEQ	—	6	103	56	165
		(4%)	(62%)	(34%)	

Sources: Cooper et al. (1994); Straub (1995).
Note: Numbers in parentheses refer to proportions of play of each outcome.

randomizing, the data are consistent with the idea that, as a population, they are mixing in the equilibrium proportions (as we often saw in the Chapter 2 studies of mixed games). The next row, STRAUB, reports results collected by Straub (1995) with the same payoffs, which replicate Cooper et al. very closely.

7.2.1 *Outside Options*

In treatment "BOS-300," the row player can take an outside option that pays 300 tickets to both players, rather than playing the BOS. Outside options are interesting because a refinement called "forward induction" predicts that the very presence of an outside option could matter for coordination, in a way that depends delicately on the size of the option. If the option is 300, then Column should reason that the Row player would reject the option only if she expected to get 600 in the BOS game. (Rejecting 300, then playing for the inferior 200 outcome, would violate the conjunction of dominance, self-interest, and dynamic consistency.) Thus, Column should confidently conclude that Row intends to play 2, and Column should best-respond by choosing 1.

Forward induction is intuitive to game theorists, but a lot can go wrong with it empirically. Row players could appreciate the argument but lack faith that Column players will think it through, and take the sure 300 rather than play BOS and risk getting 200. Columns could play 1 because they think it is unfair that Row should both have the option and, having rejected it, get the better payoff as well. Row players could violate dynamic consistency, rejecting the option then "forgetting" what they had in mind by doing so (or changing their minds about whether Columns will have reasoned it through). The third row of Table 7.9 shows that, despite these cognitive obstacles, the forward induction argument works well: Only 20 percent of the Rows chose not to play, and 90 percent of those who did got their preferred payoff (from the (2,1) equilibrium).

Of course, players in a BOS crave any tie-breaking feature that distinguishes one player from another, to break the stalemate between (2,1) and (1,2). Cooper et al. wondered whether having *any* option distinguishes the Row player, leading both players to the (2,1) equilibrium that favors Row. To test this possibility, they ran sessions in which the option pays 100 to both players. When the option value is only 100, forward induction does not apply because rejecting the option does not indicate anything about Row's beliefs about what will happen in the BOS subgame.

Row "BOS-100" in Table 7.9 shows what happened when the option is 100. Only three players picked the option. When the other 98 percent chose to play BOS, 63 percent of the pairs gravitated toward Row's preferred equilibrium, and 34 percent mismatched. These percentages are about halfway between the original BOS results and the BOS-300 results. We can conclude that roughly half of the effect of the 300 option is due to forward induction, and half to the presence of an option being used as a coordinating device that favors the Row player.

7.2.2 Communication

Communication is obviously important in coordination games. To study communication, Cooper et al. allowed either one or both players to make nonbinding preplay announcements ("cheaptalk").

The effects of communication are summarized in the rows labeled BOS-1W (one-way) and BOS-2W (two-way) in Table 7.9. One-way communication worked like a charm: 95 percent of announcements by Row were intended play of 2, and all but one actually did play 2. All but two of the Columns went along and played 1.

If a little (one-way) communication helps a lot, two-way communication must do even better, right? Wrong. Table 7.9 shows that with two-way communication the mismatch rate was 42 percent, not quite as high as the communication-free 59 percent in the baseline BOS treatment, but far short of the low 4 percent rate with one-way communication. The problem was that both players tended to announce their preferred strategies, which did not form an equilibrium and therefore left them in roughly the same position as if they'd said nothing.

7.2.3 *Evolution of Meaning*

Communication creates a meta-coordination problem in which players must coordinate their beliefs on what various messages mean before they can use messages to coordinate on what to do (see Farrell and Rabin, 1996). This problem is illustrated by the following story.[13] There is a dialect of Portuguese that is widely spoken in Brazil, and substantially different from the dialect spoken in Portugal. Although the Portugese were first to use their language, the Brazilian-dialect speakers outnumber the Portugese natives, so there is a conflict between "precedent" (temporal priority) and sheer numbers as principles for determining which dialect to use. So a conference was convened to agree, once and for all, on a common dialect. But what language should they speak at the conference?!

The point is that creating shared meaning from scratch is deceivingly difficult and may take a long time. Where does meaning come from? "Common sense" is not a satisfactory answer for theorizing, since we would like to know where common sense comes from and how it is sustained (i.e., why is it common?).[14]

Because pure theory seems unable to give conditions under which specific meanings arise, it is useful to put people in a situation with no meaning and see how they create it. Blume et al. (1998) did such an experiment (see also Blume et al. 2001). They were interested in whether senders in sender–receiver games could learn to create a homemade language, whose meaning would be understood by receivers.

Start with game 1 of Blume et al, shown in Table 7.10. The game shows the payoffs to senders and receivers when the sender's type is t_i and the receiver's action is a_j. (Payoffs are in units of probability of winning $1, since they used the lottery ticket procedure.) In the experiment, senders

[13] Thanks to Sam Bowles for this story.

[14] There are also many examples in everyday language where speakers and listeners know that sentences mean the opposite of their literal meaning. As I write this, I am staring at an ad which says, "When we ask, 'Is that what you're wearing?' we're actually hoping it's not." The ad is part of a Virginia Slims campaign with the slogan "It's a *woman* thing" which tries to explain nonliteral female meanings to clueless men. For example, most women know that the utterance "Is that what you're wearing?"—which appears to be a question requiring a yes or no answer—is in fact a thinly veiled command: "Take those clothes off and put on something different that I would approve of." A "take it literally" assumption of meaning creation must bend to accommodate these exceptions. See Sally (2002a) for more.

Table 7.10. Payoffs in sender–receiver game 1 of Blume et al.

	Receiver actions	
Sender types	a_1	a_2
t_1	0,0	0.7, 0.7
t_2	0.7, 0.7	0,0

Source: Blume et al. (1998).

observed their type and chose a preplay message from the set A,B. The receiver observed the message, but not the sender's type, and chose an action.

Game 1 is a game of common interest because both senders and receivers would prefer to have their types revealed to the receivers, so the receivers can match actions with types. Their paper describes an adaptive process which leads to an efficient separating equilibrium when there are two messages. That is, either type 1s send A and type 2s send B, or vice versa, and receivers come to know which message connotes which types, and choose action a_2 after the type 1 message and a_1 after the type 2 message. Converging to this kind of separation requires players to decide endogenously on a homemade language—either A will "mean" you are type 1 (and B means type 2) or B means you are type 1 and A means type 2.

Game 2 of Blume et al., shown in Table 7.11, complicates the situation by adding a secure action for receivers (a_3). Game 2 is still a common interest game in which players would prefer to find a meaningful language that indicates types and enables separation. Blume et al.'s adaptive process still predicts separation, as do most evolutionary theories.

In game 3 (see Table 7.12), senders would prefer to disguise their types so that receivers could not guess which type sent a message, and would choose a_3 (giving the senders .4) rather than gambling on guessing the type

Table 7.11. Payoffs in sender–receiver game 2 of Blume et al.

	Receiver actions		
Sender types	a_1	a_2	a_3
t_1	0,0	0.7,0.7	0.4,0.4
t_2	0.7,0.7	0,0	0.4,0.4

Source: Blume et al. (1998).

Table 7.12. *Payoffs in sender–receiver game 3 of Blume et al.*

	Receiver actions		
Sender types	a_1	a_2	a_3
t_1	0,0	0.2,0.7	0.4,0.4
t_2	0.2,0.7	0,0	0.4,0.4

Source: Blume et al. (1998).

correctly and giving the senders only .2. The adaptive process still picks out the separating equilibrium when there are only two messages. A pooling equilibrium at a_3 cannot be sustained because it depends on both types of sender picking the same message (say, A). When one type trembles and chooses B, history-watching receivers who are active will detect that shred of revelation and respond to it (since guessing types correctly gives them a higher payoff), breaking the pooling equilibrium. Thus, when there are two messages the shrewd receivers can always use them to sort the types and separate.

Blume et al.'s experimental design required some ingenuity. Most subjects participated in a first session in which they played game 1 with messages A and B. The same subjects then participated in a second session in either game 2 with two messages or game 3 with either two or three messages. To disable any focal influence of their first-session experience, in the second session players chose messages from the set $*, \#$, which were privately translated into A and B.

Table 7.13 shows results in periods 1, 5, 10, 15, and 20, pooled across sessions.[15] The table reports the percentages of plays consistent with separation, and the proportion of a_3 messages as an index of pooling. The results show a steady, sharp convergence toward separating in game 1 in the first session—players *are* able to create a meaningful language for expressing their types. Game 1NH is game 1 played with an own-history protocol in which players know only the history of their own matches. Without population history, convergence in 1NH to separation is sluggish; after twenty periods players separate only 72 percent of the time, about the same rate as players achieved in five periods with history. Game 2 play begins with about

[15] The results are standardized according to whichever message configuration emerged as the predominant one in period 20. Although this reporting convention understates the amount of temporary separation, it is the best way to show the time path of convergence to an equilibrium with a particular "meaning" that emerges later.

Table 7.13. Percentage of plays consistent with separating in sender–receiver games 1 and 2 of Blume et al.

	Period				
Game	1	5	10	15	20
First session					
Game 1	48	65	74	88	95
	(.14)	(.12)	(.17)	(.13)	(.07)
Second session					
Game 1	49	72	61	89	100
	(.28)	(.09)	(.19)	(.09)	(.00)
Game 1NH	55	55	28	55	72
	(.25)	(.09)	(.19)	(.09)	(.19)
Game 2					
Separating	44	88	88	88	94
	(.19)	(.09)	(.09)	(.09)	(.09)
Pooling	39	05	00	05	05
	(.25)	(.09)	(.00)	(.09)	(.09)

Source: Blume et al. (1998).
Note: Standard deviations are in parentheses.

equal amounts of separating and pooling, and moves toward separating, more swiftly than in game 1.

Table 7.14 reports results from game 3 with two and three messages. There is certainly a stronger tendency to pool (more than half) in the divergent interest game 3 than in games 1 and 2. However, there is more heterogeneity across sessions (masked by the averages shown in Table 7.14) and there is greater separation in the session that followed game 1, which suggests that the separation subjects were able to achieve in that game spilled over to game 3.

A similar experiment on cheaptalk in sender-receiver games was done by Kawagoe and Takizawa (1999). In their games a sender observes her type, either 1 or 2 (equally likely), and sends one of the two messages "I am type 1" or "I am type 2." A receiver hears the message, but does not observe the sender's type, and chooses an action A, B, or C. Payoffs for both sender and receiver depend only on the sender's type and the receiver's action. The messages are cheaptalk because they do not affect payoffs and are not binding.

Table 7.14. *Results in sender–receiver game 3 of Blume et al.*

Number of messages	Behavior	Periods 1–10	11–20	21–30	31–40	41–50	51–60
Second session (after game 1)							
2	Separating	43	53	38	39		
	Pooling	33	34	41	43		
3	Separating	43	38	33	24		
	Pooling	33	37	42	60		
First session							
2	Separating	39	27	23	24	24	23
	Pooling	39	48	51	60	63	61
3	Separating	23	22	23	25	22	24
	Pooling	55	61	58	56	57	61

Source: Blume et al. (1999).

Kawagoe and Takizawa's games are designed to vary the degree of common interest and compare likely equilibria. Payoffs in three games are shown in Table 7.15. In game 1, senders and receivers have a common interest in revealing types, since senders will then choose A for type 1 and B for type 2, earning (4,4) rather than the pooling equilibrium outcome (3,3). The separating equilibrium outcomes and results are shown in bold.

Table 7.15. *Sender–receiver games with cheaptalk*

Sender's type	Receiver's action A	B	C	Frequencies in sessions 1–3 (percent) Separating	Babbling	Other
Game 1						
1	**4,4**	1,1	*3,3*	**86**	*11*	4
2	1,1	**4,4**	*3,3*			
Game 2						
1	**3,4**	2,1	*4,3*	**56**	*35*	9
2	2,1	**3,4**	*4,3*			
Game 3						
1	4,4	1,1	*2,3*	—	*40*	.60
2	3,1	2,4	*4,3*			

Source: Kawagoe and Takizawa (1999).

However, there is always a "babbling" equilibrium in which receivers do not think they have learned anything from the senders' messages, and choose C in response to either message; these pooling outcomes and results are marked in italics. In games where interests coincide (as in game 1), evolutionary dynamics typically lead to the separating equilibrium.

Game 2 reduces the degree of common interest, creating something akin to BOS. There is still a separating equilibrium in which types are revealed, yielding (3,4). Senders who think their message reveals their type do not want to be mislabeled (leading to a payoff of 2 rather than 3) so they should reluctantly reveal. However, the babbling equilibrium in which messages do not reveal types, and receivers choose C, gives a higher payoff to the senders (4) than the separating equilibrium does. In game 3, there is no separating equilibrium. The interesting question is whether the babbling equilibrium emerges strongly and, if not, what happens.

Message-action pairs for each subject pair were coded as separating equilibria if a type 1 (2) message led to action A (B). They were coded as babbling equilibria if receivers chose the action with the highest expected payoff given the prior type probabilities (i.e., C). The relative frequences are shown in the right-hand columns of Table 7.15.[16] There was very sharp separation in the common interest game 1, around 80–90 percent. In game 2, there was substantial separation in the first session (66 percent) but it was reduced a lot, and babbling emerged, when the action labels were scrambled in the second session. In game 3, babbling did not emerge strongly, since only 30–55 percent of the choices were the predicted action C.

As in Blume et al. (1999), separating emerged strongly when sender and receiver interests coincided, and less strongly otherwise. Babbling did not emerge strongly when there was no separating equilibrium in game 3, but this fact can be explained by quantal response equilibrium.[17]

7.2.4 External Assignment

One interpretation of an equilibrium is a set of strategies that players would use, self-enforcingly, if an external arbiter recommended them. This definition suggests a straightforward experimental: Recommend or "assign" them in an experiment, and see what subjects do.

[16] No separating equilibrium statistics are reported for game 3 because there is no such equilibrium in that game.

[17] When the QRE response sensitivity λ is low, types 1 and 2 do not have an incentive to separate their messages because receivers respond so noisily that it doesn't matter which messages are chosen. When λ reaches a critical value, around 1, a noisy separating QRE emerges, in which type 1s (2s) choose A (B) about 90 percent of the time. Thus, QRE is able to suggest an explanation why Kawagoe and Takizawa *do not* find strong emergence of a babbling equilibrium. In this equilibrium, receivers play the action C only around 35–40 percent of the time, which fits the data (41 percent C) reasonably well.

Table 7.16. *Results of assignments in games A, B, and C*

Game	Assignment	Choices (percent)		
		1	2	3
A payoffs		(5,5)	(5,5)	(5,5)
	None	30	51	19
	1	99	0	1
	2	1	99	0
	3	0	0	100
B payoffs		(9,9)	(5,5)	(1,1)
	None	97	2	1
	1	99	1	0
	2	51	48	1
	3	62	0	38
C payoffs		(7,3)	(5,5)	(3,7)
	None	10	82	8
	3 (row play)	1	63	35
	3 (column play)	0	54	46

Source: Van Huyck, Gillette, and Battalio (1992).

This is precisely what Van Huyck, Gillette, and Battalio (1992) did (see also Brandts and MacLeod, 1995). The games they used are shown in the top lines of each panel of Table 7.16. In all games, players chose strategies 1, 2, or 3 and earned nothing if they mismatched. Game A is a pure matching game where all payoffs (in multiples of \$0.10) are (5,5). Game B has three (strict) equilibria which are Pareto ranked. Game C is like a BOS, with two asymmetric equilibria yielding (7,3) and (3,7), and an equilibrium with equal payoffs (5,5).

In periods with assignments, the experimenter chose one of the three strategies to assign. For strategy 1, for example, they said "Row choose row 1 and Column choose column 1. If the Row participant chooses row 1 then the best the Column participant can do is to choose column 1. If the Column participant chooses column 1 then the best the Row participant can do is to choose row 1."

Table 7.16 summarizes the results, aggregated across periods and sessions. In game A, with no assignment, subjects tended to choose the middle strategy 2 about half the time and split choices between 1 and 3 roughly equally. Hungry for a device to coordinate their choices, when one strategy is assigned they play it almost all the time (99–100 percent). Game B is different because the equilibria are Pareto ranked. When no assignment was made, subjects overwhelmingly chose the payoff-dominant strategy 1, which yielded (9,9) if both players chose it. When that strategy was assigned, they played it. When one of the Pareto-inferior strategies was assigned—2 or 3—they played the assigned strategy less than half the time.

In game C, when no assignment was made subjects tended to choose the (2,2) equilibrium yielding equal payoffs (5,5). When the strategy yielding unequal payoffs was assigned, slightly more than half continued to play the equal-payoff strategy and less than half played the assigned strategy. There was also a small self-serving asymmetry in row and column responses to the suggestion to play the strategy that yielded unequal payoffs: The column player, who benefited from the inequality, getting 7, followed the assignment 46 percent of the time, whereas the row player, who suffered by getting only 3, played it only 35 percent of the time.

Thus, assignment influences behavior strongly when it does not compete with another focal principle (in game A). But, when an assigned strategy conflicts with another focal principle—payoff-dominance (in game B) or equality of payoffs (in game C)—assignments are followed only half the time.

Suggestions by an outside authority of how to play coordination games often occur in economic and political life. For example, in 1966 Mao Tse-tung launched the Great Proletarian Cultural Revolution in Communist China. As part of the revolution (Kristof and WuDunn, 1994, p. 70), "For a time, cars were instructed to go forward at red lights and to stop at green, because red was a revolutionary color signifying action. That plan was dropped when not enough drivers got the message and pileups occurred at major intersections." As in experiments, an assignment that competes with another focal principle (in this example, historical precedent and automatic habit) does not necessarily work, even when recommended by one of the most authoritarian governments in the history of the world.

Traffic lights and communication are familiar examples of a "correlating device"—a publicly observable signal on which players can condition their strategies.[18] Correlating devices are useful in triggering collective action when tacit communication is cumbersome or even illegal. For example, in recent years there have been large-scale riots (often causing widespread injury and destruction) after both sports losses and *victories* (e.g., after the 2000 Los Angeles Lakers world championship win). One interpretion of these events is that some groups of people would like to riot, but don't want

[18] Conditioning strategy choices on correlating devices leads to "correlated equilibrium," an interesting generalization of a Nash equilibrium that captures the essence of many coordination problems.

to get caught. The end of a sport game—regardless of whether the outcome makes the hometown crowd happy or sad—is a good correlating device because it has sharp timing and is widely heard, like a pistol shot starting a race.

Another example: During the 2001 power crisis in California, state investigators claimed that a cartel of power companies created "artificial shortages" by taking plants off-line (allegedly for routine maintenance and so forth) when the state issued emergency alerts that more power was needed (*Los Angeles Times*, 2001). To a game theorist, it is obvious that the emergency alerts might have served as a correlating device that enabled firms to conspire by removing capacity, spiking prices sharply upward. Like sports outcomes, the emergency alerts were widely publicized and sharply timed—ideal properties in a correlating device.

7.2.5 *Timing*

Early in the history of game theory, von Neumann and Morgenstern made a deliberate choice to emphasize only information known at decision nodes. They reasoned that, if you did not know what another player did, you should not care whether that player had already moved, was moving just when you did, or was moving later.

More recently, several game theorists have wondered whether timing, per se, might act as a selection principle in games such as BOS, even without affecting the information players have. The first to take this idea seriously were Amershi, Sadanand, and Sadanand (1989, 1992); they introduced a generalization of Nash equilibrium that incorporated the principle that early-moving players would make choices assuming later-moving players would realize they were moving later. They also reported an informal pilot experiment showing this effect in BOS. Kreps (1990, p. 112) conjectured this effect as well and several experiments have replicated its existence and studied its cause (e.g., Rapoport, 1997; Colman and Stirk, 1998; Weber and Camerer, 2001).

Cooper et al. looked for the effect of timing in BOS. In their treatment, it was common knowledge that Row players went first, but also that Column players didn't know what Row did. The results are shown in row BOS-SEQ in Table 7.9. The mismatch rate was 34 percent, about half as large as in the timing-free BOS baseline, and 62 percent of the time the players together chose (1,2). Simply knowing that Row was moving first led pairs to move about halfway to Row's preferred equilibrium. This remarkable effect of timing overturns Von Neumann and Morgenstern's presumption.

Rapoport (1997) explored the effect of timing on perceived first-mover advantage. He studied three-player BOS games. Half the players used the

Table 7.17. Mean requests in sequential resource dilemmas

	Position of player				
Previous moves	1	2	3	4	5
Known (sequential)	172	135	125	104	102
Unknown (positional)	139	122	116	103	102

Source: Rapoport (1997).

position in which people had played—though they did not observe earlier moves—to coordinate behavior on the equilibrium the first-mover preferred. Rapoport also experimented with resource dilemmas with uncertain resource totals. In these games, five players sequentially requested resources from some pool uniformly distributed between $[a, b]$. Players did not know the precise amount of resources available, but the distribution $U(a, b)$ was commonly known. If the total of their requests was larger than the amount of resource, nobody got anything; otherwise they got what they requested.[19] To test for timing, some subjects participated in a "sequential protocol" in which each player's request was known to players who made later requests. Others played under a "positional protocol" in which they moved in a specified order (their positions were known) but later movers did *not* know what earlier movers had done.

Table 7.17 summarizes the average requests in the two conditions, by players' positions in the sequence. Timing played a strong role in the sequential protocol (as predicted by subgame perfection) and a weaker, but highly significant role in the positional protocol. The gap between the first and last players' requests was roughly half as large in the positional case as in the sequential case.[20] Timing, per se, matters.

Rapoport (1997) also reports effects of pure timing in "threshold public goods games" in which a public good was provided if three out of seven players contributed. This game is like BOS because there are many equilibria—every combination of any three players contributing is an equilibrium. Rapoport tested whether players used order of moves as a correlating device to coordinate on equilibria in which the first-movers got to free ride,

[19] This game, which has been widely studied, is a player asymmetric coordination game like BOS, but with many more equilibria and nonzero payoffs for "mismatched" nonequilibrium strategy vectors. It is also called the "Nash demand game" (when $a = b$) and has been studied in bargaining.

[20] Rapoport also asked players to estimate how much the players playing *previously* had given, and how much players playing *subsequently* would give. Estimates of previous requests did show an effect of position, but estimates of subsequent requests did not. This asymmetry suggests that players are able to reason backward in time better than they can forecast ahead in time.

and later movers picked up the slack by contributing (even though earlier moves were not known for sure). The fractions of players contributing across the five positions were 0.15, 0.11, 0.29, 0.44, 0.32, and 0.40, so there was a strong positional effect (although it took a couple of trials to emerge).

Summary: Battle-of-the-sexes (BOS) games are the consummate mixed-motive games: Both players desire to coordinate on *some* joint outcome, but each prefers a different coordinated outcome. (Imagine choosing a movie or restaurant with your spouse.) Players tend to choose somewhere between random mixing and the mixed-strategy equilibrium (which yields many mismatches, and is less efficient than agreeing on *either* of the coordinated outcomes). Having one player announce in advance their intended choice helps a lot (the announcing player gets their preferred outcome); simultaneous announcements are not much help. Other changes in the game can improve coordination. It helps if one player has an outside option that is preferable to their inferior BOS equilibrium (then, rejecting the option and playing the BOS signals their intent to choose the preferable equilibrium, a logic known as "forward induction"). Most surprisingly, if one player moves first, *even if the second-mover does not know what the first-mover has done*, the two players tend to coordinate on the first-mover's preferred outcome, so there is a tacit—almost telepathic—first-mover advantage. Having an external "assignment" (e.g., by an experimenter, a laboratory "body double" for a government, regulatory influence, or media announcement) can also improve coordination.

Finally, some experiments have explored endogeneous development of meaning. Players with different types choose announcements from a random language. When it pays for players to reveal their types they find a language that does so (i.e., one type announces one message and the other type announces a different message), but when sender and receiver players have different incentives the results are mixed.

7.3 Market Entry Games

An important class of coordination games involves entry into markets and competitions. In a typical model, there are n players and a market with capacity c. (It is easy to generalize to more than one market, and a few experiments have explored multiple markets.) Entrants earn a return which declines with the number of entrants, and is negative if more than c enter. How firms (and workers) coordinate their entry decisions in domains such as this is important for the economy. If there is too little entry, prices are too high and consumers suffer; if there is too much entry, some firms lose money and waste resources if fixed costs are unsalvageable. Public announcements of planned entry could, in principle, coordinate the right amount of entry, but announcements may not be credible because firms that *may* enter always have an incentive to announce that they surely will do so, to ward off competition. Government planning may help but is vulnerable to regulatory capture by prospective entrants seeking to limit competition.

A dramatic example in which tacit coordination failed miserably is the production of high-capacity optic cable. Too many firms laid too much cable, which is so powerful that it would take a huge expansion in use to soak up all the capacity. As the *Los Angeles Times* reported (2002):

> The cables were laid by a band of upstart companies that spent $50 billion or more in the last few years to wire the planet. . . . These upstarts bet that if they built communications networks with far more capacity, or bandwidth, than had ever been available before customers would rush to use them. . . . The problem was that too many companies had the same dream, and they built too many digital toll roads to the same destination. . . . "People have laid huge amounts of fiber in the ground," said Internet analyst Tony Marson of Probe Research Inc., "and there is a distinct possibility that quite a lot of that will never actually see any traffic."

The optic cable industry is a failure of independent firms to coordinate the right amount of entry (or perhaps a collective forecasting mistake), but other evidence suggests that errors in planning entry are common. Field studies of business entry and exit find that *most* new businesses (and plant openings by established businesses) fail, and usually fail rapidly. (For example, about 80 percent of new restaurants fail within a year.) This stylized fact suggests there is too much entry. However, since the relatively few successful entrants are often hugely successful, firm profits have a long positive "right tail" or skew. So entrants may be maximizing *expected* profits even if the failure rate is high.

Since the process by which firms coordinate entry is not well understood, and field evidence tentatively suggests too much entry, experiments are useful (see Ochs, 1999, for a review).

The first experiments on single-market games were conducted by Kahneman (1988). He was surprised that the number of players who chose to enter was very close to the number predicted by theory (i.e., around c entrants), even though the players all made their choices at the same time and could not communicate or learn from feedback. "To a psychologist," he wrote, "it looks like magic."

Rapoport (1995) created a simple single-market design and did experiments with Ph.D. students playing over several weeks. He has subsequently explored the design with various colleagues under more controlled laboratory conditions.

Denote player i's entry decision by e_i, 1 for entry and 0 for non-entry. Payoffs are v if a player stays out and $k + r(c - m)$ for entrants, where $m = \sum_{j=1}^{n} e_j$. Sundali, Rapoport, and Seale (1995) did experiments in which $v = k = 1$ and $r = 2$. Since capacity is c, the pure-strategy equilibria are for c or $c - 1$ players to enter (the marginal cth entrant is indifferent). There is also a symmetric mixed equilibrium in which players enter with probability $[r(c - 1) + k - v]/r(n - 1)$.

In their experiment, groups of twenty players made a series of simultaneous one-shot entry decisions with ten different capacity values c—the odd integers 1, 3, . . . , 19. In most experiments subjects were told how many others decided to enter after each decision. In each block they played once with each of the ten capacities, and to allow learning they played six such blocks.

Table 7.18 shows the results from the Sundali et al. experiment and from several others. In the first block, the number of entries rose erratically with capacity c, and there were too many entries at low c and too few at high c. But subjects learn fast: Pooling across blocks, the number of entries was never more than two different than the number predicted by the mixed equilibrium for all capacities c.

Rapoport, Seale, Erev, and Sundali (1998) varied the nonentry payoff v (keeping the other payoff parameters constant). When $v = 6$ many fewer players should enter than when $v = 1$ (fixing capacity c) and when $v = -6$ more should enter. Table 7.18 shows that players did respond to changes in v but, as in the earlier experiments, there was some over-entry at low c and under-entry at high c.

Seale and Rapoport (2000) used the strategy method, eliciting entry decisions for each value of c in one fell swoop. The overall entry rates were similar to those observed from asking for entry decisions for each c separately. Furthermore, the c-dependent strategies show little regularity or stability. One hypothesis about how effective coordination comes about is that players use cutoff rules, entering for some threshold capacity c^*, but variation in cutoffs generates a population profile that roughly matches the equilibrium prediction. However, the direct measurement of strategies shows that few are sharp cutoff rules.

Rapoport, Seale, and Winter (2002) experimented with an asymmetric game in which players paid different costs to enter. There were five types of players, numbered 1 through 5, and players of type (number) i paid a cost i to enter. Different costs are interesting because they imply asymmetric equilibria in which high-cost players should enter less often than low-cost players. For example, in their design low-cost players should *always* enter when $c = 9$ or above and high-cost players should *never* enter until $c = 19$. Overall entry was close to the rate predicted by equilibrium but players were remarkably insensitive to costs. Low-cost players did not enter often enough and high-cost players entered too often.

Table 7.18. Number of entries in market entry games

Study	Data	Market capacity c									
		1	3	5	7	9	11	13	15	17	19
	MSE equilibrium	0	2.1	4.2	6.3	8.4	10.5	12.6	14.7	16.8	18.9
Sundali, Rapoport, and Seale (1995), $v = 1$											
Experiment 2	First block	1.3	5.7	9.7	6.7	3.7	14.0	11.3	11.3	16.0	18.0
	All blocks	1.0	3.7	5.1	7.4	8.7	11.2	12.1	14.1	16.5	18.2
Seale and Rapoport (2001), $v = 1$											
(Strategy)	First block	3.7	5.7	6.2	7.0	7.7	10.2	11.0	12.2	12.7	13.9
method)	All blocks	1.9	4.1	5.1	6.8	7.6	10.9	12.5	13.5	15.1	16.0
Rapoport, Seale, and Winter (2002), $v = 1$, asymmetric entry cost											
	Pure equilibrium	0	2	4	5–6	7–8	9	11	12–13	14–15	16
	Total	0.6	2.1	4.0	6.0	8.0	9.6	11.0	13.0	14.3	16.1
	Low cost = 1 (max. 4)	0	0.3	0.8	1.2	1.5	2.0	2.6	3.3	3.4	3.4
	High cost = 5 (max. 4)	0.1	0.2	0.1	0.7	1.1	1.1	1.5	1.7	1.6	2.1
Rapoport et al. (1998), $v = -6$											
	Capacity	1	3	5	7	8	9	10	12	14	16
	MSE equilibrium	3.7	5.8	7.9	10.0	11.1	12.1	13.2	15.3	17.4	19.5
	First block	4.0	9.0	9.0	9.0	9.0	15.0	9.0	17.0	17.0	18.0
	All blocks	5.2	9.0	8.6	10.2	10.8	12.8	12.2	14.6	16.0	18.4
Rapoport et al. (1998), $v = 6$											
	Capacity	4	6	8	10	11	12	14	16	18	20
	MSE equilibrium	0.5	2.6	4.8	6.8	7.9	8.9	11.1	13.2	15.3	17.4
	First block	4.0	5.0	6.0	4.0	9.0	11.0	10.0	14.0	16.0	15.0
	All blocks	2.2	4.2	7.8	7.0	10.0	10.2	11.8	13.4	15.0	16.8

In Rapoport, Seale, and Ordóñez (2002), the nonentry payoff was a lottery (see also Rapoport, Lo, and Zwick, in press). Results were like those in earlier studies: Entry rates were close to those predicted, with over-entry at low c and under-entry at high c, and can be fit by a nonlinear transformation of probability (as is common in nonexpected utility theories; e.g., Prelec, 1998).

Rapoport and Erev (1998) ran experiments with $c = 4$ and 8 and varied information conditions, in order to compare learning models. When subjects did not know the payoff function, but got feedback about either their own payoff or the payoffs from both entering and not entering, learning was slow: Even after twenty periods with the same value of c, the number of entries was one or two away from equilibrium. Knowing the payoff function improved the speed and accuracy of convergence. The differences in speed of convergence with information show that models which assume players respond only to payoffs from chosen strategies can be improved by adding information about forgone payoffs from unchosen strategies (cf. Chapter 6).

7.3.1 Multiple Markets

Meyer et al. (1992) studied entry in two markets. Each of six subjects supplied a single unit to either market A or market B. Prices in each market were equal to \$1.05 divided by the number of units of supply. In a pure equilibrium, players would coordinate to supply three units in each market; in the symmetric mixed equilibrium they should randomize equally over the two markets. Meyer et al. were motivated by concerns about how well decentralized allocations works. (Their example corresponds to a famous example of Marshall's in which fishermen deliver their catch to one of two islands; do quantities adjust to satisfy the law of one price?)

The total supply to market A fluctuated around three units, and did not converge in fifteen or even sixty periods. Even when subjects hit upon the even (3–3) allocation of units in one of the first few periods, they did not stick to that allocation. Why not? Subjects appeared to be roughly randomizing each period (though they repeated their previous choice more often than is predicted by random mixing). However, sharp convergence to the equal-allocation equilibrium did occur under two conditions: when subjects were experienced (i.e., they returned for a second experimental session); and when they announced their entry decisions sequentially and were allowed to change their choices.

Rapoport, Seale, and Winter (2000) ran experiments in which subjects could enter either of two markets, or sit out and earn a small return. When the two markets had the same capacity, entry decisions were close to equilibrium (except for the familiar bias of over entry at low c). When the markets had different capacities, however, initial decisions were wildly far from equilibrium at first, but converged rather closely over ten blocks.

Samuelson (1996) studied entry in market games with asymmetric information about costs. In his experiments, if there are x entrants and n players, then entrant i earns $\pi(x) - c_i = a - bx - c_i$, where c_i is entrant i's cost, drawn independently from some commonly known interval. The profit function and the number of players vary, to test whether actual entry is sensitive to these factors as predicted by equilibrium. In each case there is a symmetric cutoff equilibrium in which players with costs below a cutoff c^* enter and players with costs above the threshold stay out. For example, when the cost interval is [1,2] and $\pi(x) = 2.8 - .08x$, and $n = 2$ players, the cutoff $c^* = 1.56$ (assuming risk-neutrality).[21] In the experiment, cutoff strategies were elicited explicitly. Results for several one-shot games with different numbers of players and profit functions, and hence different equilibrium values of c^*, are reported in Table 7.19.

The average cutoff values were remarkably close to those predicted by theory, although always slightly below (usually significantly so). Mean cutoff values tracked changes across treatments. An interesting case is the three middle data columns of Table 7.19, showing cutoff values from a two-period game with $n = 2$. In the two-period game the equilibrium cutoff in the first period is $c^* = 1.61$. In the second period, the equilibrium cutoff changes because subjects should learn something about other players' costs from their first-period behavior. When both subjects in a pair enter in the first period ("after both"), each should think the other's cost is low. Since the other player, if rational, will be likely to enter again in the second period, players should be hesitant to enter again unless their costs are quite low (below 1.43, a lower cutoff than the original 1.61). In fact, entry cutoffs did drop substantially in the second period after both entered (to a mean of 1.28). When neither subject enters in the first period ("after none"), subjects should be more likely to enter in the second round, raising the equilibrium cutoff to 1.74. However, the cutoff distribution after neither entered was very similar to the first-period distribution. As Samuelson notes, the failure of the other player to enter after nonentry is like the "dog that didn't bark" in a famous Sherlock Holmes story (the dog's *failure* to bark suggested an intruder knew the dog). Subjects appeared to learn less from observed nonentry than they did from observed double entry.[22]

7.3.2 Skill

The experiments described above all exclude an important feature of entry in many markets: skill. In most naturally occurring markets, entrants do not earn the same amount of money. Profits differ because of differences in costs, the ability to anticipate customer demand, and so forth. Adding skill is also important because efficient entry requires firms to anticipate their *relative* skill accurately. But many surveys and experiments indicate that av-

[21] At that cutoff, a player can expect no other entrant with probability .44, and earns $2.0 - c_i$ if she enters, and expects one other entrant with probability .56, and earns $1.2 - c_i$ if she enters. Since expected profits are $.56(1.2) + .44(2.0) - c_i$, or $1.56 - c_i$, she earns a tiny positive expected profit if she enters when costs are 1.56 or below.

[22] This asymmetry in learning from entry and failure-to-enter seems related to a principle in mental representations of logic problems, that people fail to explicitly represent what is false, which can lead to dramatic logical errors (see Johnson-Laird, 1994).

Table 7.19. *Cumulative frequency of entry at each cost or below in games with private costs and n players*

	$\pi(x) = 2.8 - .8x$					$\pi(x) = 2.1 - 0.2x$	$\pi(x) = 3.3 - 0.2x$
			$n = 2$, two-period games				
Cutoff cost				Period 2			
interval	$n = 2$	$n = 4$	Period 1	After both	After none	$n = 11$	
1.1	0.98	0.82	0.98	0.97	0.97	0.84	0.97
1.2	0.98	0.78	0.94	0.97	0.96	0.74	0.97
1.3	0.90	**0.42**	0.88	0.48	0.92	**0.53**	0.94
1.4	0.88	0.18	0.82	**0.26**	0.86	0.19	0.91
1.5	**0.74**	0.12	0.74	0.20	0.78	0.11	0.86
1.6	0.44	0.04	**0.40**	0.10	0.53	0.08	0.73
1.7	0.00	0.04	0.00	0.04	**0.19**	0.00	**0.52**
1.8–2.0	0.00	0.00	0.00	0.00	0.04	0.03	0.31
Equilibrium	1.56	1.29	1.61	1.43	1.74	1.30	1.70
Mean	1.49	1.21	1.50	1.28	1.53	1.25	1.68
Standard deviation	0.135	0.143	0.139	0.136	0.161	0.178	0.200
t-test	−3.59	−4.04	−5.74	−7.76	−8.77	−1.61	−0.63

Source: Samuelson (1996).
Note: Entries are cumulative frequencies of players choosing to enter at each cost level or below. *t*-test tests whether the mean is significantly different from the equilibrium prediction. If players are risk neutral and choose equilibrium strategies, entries in bold should be 1.0.

erage people are persistently overconfident about their relative abilities and life prospects. If firms overestimate their ability to deliver a good product, and confident firms choose to enter, there may be too much entry.

Camerer and Lovallo (1999) modified the standard paradigm to allow skill and overconfidence about skill. In their design the top c entrants shared a portion of \$50 according to their *rank*.[23] Entrants below the top c lost \$10. To mimic the usual design in which skill doesn't matter, in a random-rank condition ranks were determined randomly. In a skill-rank condition ranks were determined by how well subjects did in a trivia quiz. It is crucial to note that the random ranks and the trivia ranks were determined *after* subjects made all their entry decisions.

On average, about two more subjects (out of fourteen) entered when their payoff depended on relative skill than when it was random. Subjects also forecast how many others would enter (and were rewarded for accuracy), and their forecasts were quite accurate. This fact is important because it means subjects entered knowing full well that many others would enter too.

Lovallo and I also studied a phenomenon called "reference group neglect." This is illustrated by a quotation from Joe Roth, the former chairman of Disney movie studios. When asked why so many big-budget movies are released at the same time (rather than avoiding direct competition), Roth said (*Los Angeles Times*, 1996, p. F8), "Hubris. If you only think about your own business, you think, 'I've got a good story department. I've got a good marketing department, we're going to go out and do this.' And you don't think that everybody else is thinking the same thing." To study this phenomenon, when recruiting for the experiment we told subjects that their earnings would depend on how skilled they were at trivia. When the subjects arrived in the lab we reminded them of what they were told when they were recruited. We hypothesized that subjects would not adjust for the fact that other subjects self-selected to participate because they thought they were good at trivia too. In these sessions, there was far too much entry in the skill-rank condition. In 70 percent of the periods, so many subjects entered that, as a group, they *guaranteed* themselves a collective loss.[24]

This experiment shows that overconfidence about relative skill may influence entry decisions. Subjects also easily neglected the fact that their competition was self-selected based on perceptions of skill, and subjects in this condition entered far too often. A single experiment is no proof that businesses fail owing to overconfidence, but it is suggestive and, most importantly, offers an experimental paradigm in which to investigate further.

[23] The i-th ranked entrant earned $(c - i + 1)/(0.5c(c + 1))$ times \$50.

[24] Since the top c entrants earned \$50, and lower-ranked entrants lost \$10, if more than $c + 5$ entered then they would lose money as a group.

Summary: In market entry games, initial entries into a single market are often remarkably close to those predicted by the symmetric mixed equilibrium, and convergence is rather sharp after learning. Players tend to slightly over-enter at low capacities and under-enter at high capacities. (These stylized facts can be explained by QRE and some models of limited thinking; see Goeree and Holt, 2000b, and Camerer, Ho, and Chong, 2001.) In multiple markets, however, entry decisions are much further from equilibrium, and in one experiment (Meyer et al., 1992) did not converge at all unless subjects were experienced. When the payoffs of entrants depend on their relative skill (at trivia), overconfidence leads players to over-enter so dramatically that, in many periods, the players as a whole are sure to lose money.

7.4 Payoff-Asymmetric Order-Statistic Games

In payoff-asymmetric coordination games, players' outcomes are the same in equilibrium, but equilibria are different. An important set of games in this class, which have been well studied, are "order-statistic games." In order-statistic games, players choose numbers and their payoff depends on their own choice and on an order statistic (such as the minimum or the median) of all the numbers.

One well-known order-statistic game is called "stag hunt" (also called "Wolf's Dilemma" by Hofstadter, 1985; see Huettel and Lockhead, 2000, for a psychological perspective). Stag hunt is named after a story in Jean-Jacques Rousseau illustrating the benefits of coordination. Two hunters can hunt for rabbit, earning 1 each, or together hunt for stag and earn 2. But since a single hunter can't catch a stag, he earns 0 unless the other hunter hunts for stag too. The Lamalera whale hunters of Indonesia actually play this sort of game all the time (Alvard, 2000). Hunting whale requires a team—a captain, a navigator, a spotter, and daring men willing to stand on the prow of a boat and throw a harpoon. A team that sails out one person short has much less chance of success. But whale hunters can always stay ashore and hunt small game or socialize. Roughly speaking, whale hunters prefer hunting whale if enough others join the expedition, but prefer to stay home and hunt on their own if the crew is short-handed.

Payoffs in a stylized stag hunt game are shown in Table 7.20. In stag hunt, (stag,stag) is a pure-strategy Nash equilibrium but (rabbit,rabbit) is also. This is an order-statistic game because, if strategies are defined as numbers, stag $\equiv 1$ and rabbit $\equiv 0$, then the payoffs depend on the minimum of the two numbers.[25] Stag hunt is also called an "assurance game" because players will

[25] That is, payoffs can be written as $\pi_i(s_i^j, s_{-i}^k) = 1 + 2 \cdot \min(s_i^j, s_{-i}^k) - s_i^j$.

Table 7.20. *Stag hunt (assurance game)*

	Stag	Rabbit
Stag	2,2	0,1
Rabbit	1,0	1,1

choose stag only if they are assured others are likely to choose stag as well. In stag hunt, strategic uncertainty arises from the conflict between the players' common motive—to somehow coordinate on (stag,stag) and earn 2 each—and the private motive to avoid the risk of getting 0 if the other person plays rabbit.

Stag hunt illustrates basic selection principles in action. The (stag,stag) equilibrium is payoff dominant, or Pareto dominant, because it is better for everyone than (rabbit,rabbit). Playing rabbit is "secure," or maximin, because it has the highest possible guaranteed (minimum) payoff. A risk-dominant equilibrium is one that minimizes players' joint risk, measured by the product of the cost of deviations by other players to any one player who does not deviate (Harsanyi and Selten, 1988). For example, if players think the equilibrium is (stag,stag) and play stag, then when another player deviates, the cost to the player who sticks to the equilibrium is $(2-0)$ or 2, and the product of these costs is 4. But if players think the equilibrium is (rabbit,rabbit) and play rabbit, then it costs them nothing if others deviate. Therefore, (rabbit,rabbit) is risk dominant.

Another idea which can be used as a selection principle is a QRE or noisy Nash equilibrium. In n-player stag hunt games with continuous strategies—you can choose a "degree" of hunting for stag—Anderson, Goeree, and Holt (1996) showed that there is a unique QRE. Thus, QRE is a kind of selection principle. When the cost of exerting higher effort (hunting for stag) rises, or the number of players rises, then players will be less likely to hunt for stag. As we will see below, these predictions match the experimental results, so QRE shows some promise for explaining these data and others.

Stag hunt is important because many games that are thought to be prisoners' dilemmas (PD) are actually coordination games such as stag hunt. For example, suppose players contribute to a public good but they can be excluded from consuming a public good (or fined for imposing negative externalities on others), or there are synergies from producing joint output. Suppose the cost of contributing is $c < 1$, the public good is worth P if one person contributes and $P + s$ if two contribute, and a noncontributer gets only $(1 - e)$ of the public good. Then payoffs are those shown in Table 7.21. For this game, (contribute,contribute) is a Nash equilibrium if and only if $P + s - c > P(1 - e)$, or $c < s + Pe$. This condition holds if s and e are

Table 7.21. *Public good contribution with synergy and exclusion*

	Contribute	Don't contribute
Contribute	$P+s-c, P+s-c$	$P-c, P(1-e)$
Don't contribute	$P(1-e), P-c$	$0, 0$

large enough. Thus, if there is enough excludability (high e) and synergy in producing the public good (high s), the public good game is actually a game of stag hunt.

Similarly, if players in a PD have homemade reciprocal social values, as in the Rabin (1993) fairness equilibrium (see Chapter 2), then cooperating is an equilibrium because somebody cooperating with me is nice, and I repay their niceness by cooperating as well. Furthermore, it is a well-known consequence of the "folk theorem" about repeated games that, if a PD is repeated with a high enough discount factor, the repeated-PD game has multiple equilibria, which can "implement" or achieve the highest possible joint payoff. Seen this way, many situations in the world that are classified as PDs are really games of coordinating on repeated-game strategies with stag hunt properties. If a PD is likely enough to be repeated, evokes emotion, or has enough synergy or excludability, it is really a stag hunt game.

Stag hunt is also the building block of economic situations with "strategic complementarities." Strategic complementarities are present when the marginal productivity of one player's strategy choice rises with the level of another's strategy choice. An example is "spatial externalities" in which firms would like to locate near one another. Locating nearby often enables a common supplier to achieve scale economies (by building a larger mine or factory, to sell to many clients). Spatial proximity also creates liquid or "thick" markets, which are useful when products are differentiated. Then many merchants come to the market because they are more likely to find a buyer for their goods, which in turn draws more buyers and fulfills the market's forecast success. (Think Ebay.) Sometimes the crucial markets are labor markets for talented workers (as in Silicon Valley or Hollywood).

Coordination games of this sort have also been studied in macroeconomics as a basis for neo-Keynesian business cycles (e.g., Bryant, 1983; Romer, 1996, Chapter 6). In full-employment equilibria, workers are employed, so they spend, which creates demand for products and keeps them employed. But if workers fear a recession, they will cut back spending, causing layoffs and fulfilling their fearful prophecy. Summers (2000) explicitly describes a stag hunt game as a model of the economic fragility of borrowers in emerging markets such as Indonesia. He writes (p. 7):

> Imagine that everyone who has invested \$10 with me can expect to earn \$1, assuming that I stay solvent. Suppose that if I go bankrupt, investors who remain lose their whole \$10 investment, but that an investor who withdraws today neither gains nor loses. . . . Suppose, first, that my foreign reserves, ability to mobilize resources, and economic strengths are so limited that if any investor withdraws, I will go bankrupt. It would be a Nash equilibrium (indeed, a Pareto-dominant one) for everyone to remain, but (I expect) not an attainable one. Someone would reason that someone else would decide to be cautious and withdraw, or at least that someone would reason that someone would reason that someone would withdraw, and so forth . . . I think that this thought experiment captures something real. On the one hand, bank runs or their international analogues do happen. On the other hand, they are not driven by sunspots: their likelihood is driven and determined by extent of fundamental weakness.

A formal way to capture the effects of the strategic uncertainty Summers describes is called "global games" (Carlsson and Van Damme, 1993; Morris and Shin, 2000). In a global game, the precise payoffs in the game are drawn from some interval, so each player isn't sure exactly what the other player's payoffs are. If the set of possible games is connected in a precise way, and one set of possible payoffs yields a dominated strategy, iterated application of dominance can lead to the risk-dominant equilibrium in stag hunt. This gives a precise way to express why a little uncertainty about what others might do (in the form of uncertainty about their payoffs, and hence behavior) can lead players to an inefficient equilibrium even when, as in Summers' example, the result is a small-scale economic catastrophe. Cabrales, Nagel, and Armenter (2001) ran experiments that support the prediction of the global games view, though the iterated process of convergence happens slowly.

7.4.1 Experimental Evidence

An elegant series of experiments on stag hunt were done by Cooper et al. (1990). Their payoffs are shown in Table 7.22. (Points were units of probability, with 1,000 points guaranteeing a win of \$1.)

Data from the last eleven periods of their experiments are shown in line "CG" of Table 7.23. The inefficient equilibrium (1,1) exerts a strong pull on the subject population: 97 percent played (1,1) and *none* played the payoff-dominant equilibrium (2,2).

As with their BOS experiments, Cooper et al. experimented with two outside option treatments to test forward induction. In CG-900, the row player could opt out and award both players 900 instead of playing the stag

Table 7.22. *Stag hunt payoffs in Cooper et al.*

	1	2
1	800,800	800,0
2	0,800	1000,1000

Source: Cooper et al. (1990).

hunt game. Forward induction worked most of the time (77 percent) when players opted in; but players opted out almost half the time. CG-700 tests whether the mere presence of an outside option, even one that is dominated by a matrix strategy, has any effect (as it did in BOS). The option had two small effects: Strangely, 12 percent of row players chose the option (earning 700 instead of the 800 they could have guaranteed by opting in and playing strategy 1); and, when they rejected the option and played the matrix game, 82 percent went to the inefficient (1,1) equilibrium.

The effects of cheaptalk are explored in treatments CG-1W and CG-2W. With one-way communication by the row player, the level of efficient (2,2) play is increased from nothing to 53 percent. Communication fills the glass of efficiency half full, but also leaves it half empty. Communication

Table 7.23. *Stag hunt results: Last eleven periods*

Game	Outside option	(1,1)	(2,2)	(1,2) or (2,1)	Total
CG	—	160	0	5	165
		(97%)	(0%)	(3%)	
CG-900	65	2	77	21	165
		(2%)	(77%)	(21%)	
CG-700	20	119	0	26	165
		(82%)	(0%)	(18%)	
CG-1W	—	26	88	51	165
		(16%)	(53%)	(31%)	
CG-2W	—	0	150	15	165
		(0%)	(91%)	(9%)	

Source: Cooper et al. (1994).
Note: Numbers in parentheses refer to proportions of play in the subgame of the outside option treatments.

works only half-way because, after an announcement of intended play of 2, both players actually play 2 only about 80 percent of the time. [26] Two-way communication, however, worked like a charm. In the last eleven periods, *every* pair of players announced the intention to play (2,2), and in 91 percent of the cases both actually did.

Notice the contrast between the effects of one- and two-way communication in BOS and in stag hunt. In BOS the central problem is to resolve the asymmetry between players' preferences, which mixes their motives. One-way communication resolves this conflict (in favor of the speaker) and improves efficiency from 40 percent to almost 100 percent. Moving to two-way communication is a half-step backward because it often restores the conflict, lowering overall efficiency back down to 60 percent. In stag hunt, the problem is not resolution of the asymmetry, but providing both players with enough assurance that the other player will take the risky efficient action. One-way communication provides halfway assurance, raising efficiency to 50 percent, and two-way communication provides fuller assurance, raising efficiency further to 91 percent. Too much communication hurts in BOS, and too little hurts in stag hunt.

The stag hunt results suggest that efficiency is difficult for subjects to achieve without preplay options or explicit communication. How much does this result depend on payoffs? Straub (1995) collected data from several variants of stag hunt to answer this question. The payoffs change the number p, the probability that a player would have to attach to his partner playing the payoff-dominant action to be just indifferent to playing either action. This is an index of the "risk" inherent in playing the efficient action (and also corresponds to the mixed-strategy equilibrium proportion of playing the efficient action and the size of the "basin of attraction" from which an evolving population will be drawn to inefficiency).

When p is .8 or .75, the population converges to the inefficient outcome. But when the break-even probability p drops to .67, play converges to the *efficient* outcome. There appears to be a sharp flash point somewhere between $p = .67$ and $p = .75$, which determines which equilibrium results.

[26] Why do row players announce 2 then play 1? One possibility is that some row players prefer the outcome (800,0) to the outcome (1000,1000) because they like the status advantage of earning more than the player they are paired with, even though they earn less (and, hence, less than many other subjects in the same room) as a result. Such status-seeking row players might be suckering column players in, inducing the Columns to play 2 so they can achieve the (800,0) payoff they desire. Although this behavior may be part of the story (see also Weber and Camerer, 2001), the fact that we rarely see announcements of 2 and choices of 1 in the two-way communication treatment suggests that imperfections in one-way communication are due to failures of nerve or residual strategic uncertainty, rather than systematic status-seeking.

Table 7.24. *Weak-link game payoffs in Van Huyck et al.*

Your choice of X	Smallest value of X chosen (including own)						
	7	6	5	4	3	2	1
7	1.30	1.10	0.90	0.70	0.50	0.30	0.10
6	—	1.20	1.00	0.80	0.60	0.40	0.20
5	—	—	1.10	0.90	0.70	0.50	0.30
4	—	—	—	1.00	0.80	0.60	0.40
3	—	—	—	—	0.90	0.70	0.50
2	—	—	—	—	—	0.80	0.60
1	—	—	—	—	—	—	0.70

Source: Van Huyck, Battalio, and Beil (1990).

7.4.2 Weak-Link Games

The two-person stag hunt is an example of the more general "weak-link" game studied by Van Huyck, Battalio, and Beil (1990). In these games, each of n players chooses an integer x_i from a set (say, 1 to 7). Their payoff increases with the minimum of all the numbers chosen, and decreases with the deviation of their own choices from the minimum. In the payoff table used by Van Huyck et al., the payoff from choosing x_i is \$0.60 + $0.10 \cdot \min_i(x_1, x_2, \ldots, x_7) - 0.10 \cdot [x_i - \min_i(x_1, x_2, \ldots, x_7)]$ (see Table 7.24).

The weak-link game models situations in which a group's payoff is sensitive to the "weakest link" in some productive chain. Economists will recognize this property as a feature of Cobb–Douglas production functions with large exponents (or Leontief). A common example is production of a recipe (perhaps industrial chemicals) which is sensitive to the quality of the worst ingredient. Often the "ingredients" in the recipe are *times* at which project parts are done. Imagine a group of hungry people who meet at a restaurant which won't seat them until their entire group is present. Each person wants the latest person to be there early, but nobody wants to wait around. In professional organizations such as law firms, accounting firms, and investment banks, documents are often assembled in pieces, by various people or groups, and must meet a very strict deadline. If one part of the project is late or shoddy, the whole project is sunk or jeopardized.

In the airline business, a weak-link game is played every time workers prepare an airplane for departure. The plane cannot depart on time until it has been fueled, catered, and checked for safety; passengers have been boarded; and so on. For short-haul carriers, which may use a single aircraft

Table 7.25. Total frequencies of choices in large-group weak-link games of Van Huyck et al.

	Period									
Choice	1	2	3	4	5	6	7	8	9	10
7	33	13	9	4	4	4	6	3	3	8
6	10	11	7	0	1	2	0	0	0	0
5	34	24	10	12	2	2	4	1	0	1
4	17	23	24	18	15	5	3	3	2	2
3	5	18	25	25	17	9	8	3	4	2
2	5	13	17	23	31	35	39	27	26	17
1	2	5	15	25	37	50	47	70	72	77

Source: Van Huyck, Battalio, and Beil (1990).

on multiple flights in a day, each departure is also a link in the chain of multiple flights, which creates another weak-link game among different ground crews. A flight from San Jose to Las Vegas, and on to Tucson, cannot arrive in Tucson on time unless it is approximately on time in Las Vegas.

Data from seven sessions conducted by Van Huyck et al. are pooled and shown in Table 7.25. There is initial dispersion and gradual deterioration to 1 in all seven groups.

Van Huyck et al. also conducted sessions with large groups under two additional treatments. Eliminating the penalty for choosing a number above the minimum led most subjects (83 percent) to immediately choose 7; four of six groups reached a minimum of 7. Giving the subjects the entire distribution of choices people made, rather than just the minimum, *accelerated* convergence toward the inefficient minimum of 1.

Group Size

The weak-link game is unforgiving because it is sensitive to a single outlying (low) response. To see whether smaller groups might coordinate on higher (more efficient) numbers, Van Huyck, Battalio, and Beil (1990) ran one experimental session in which subjects were paired in groups of two (in a partner protocol) for seven periods. Their choices are shown in Table 7.26. The distribution of first-period choices is roughly similar to that in the large groups (shown in Table 7.25), but, after seven periods, ten of twelve pairs reached the efficient minimum of 7. A subject who chose 7 while her partner

Table 7.26. *Choices in weak-link games for two-person groups of Van Huyck et al.*

	Period						
Choice	1	2	3	4	5	6	7
7	9	13	13	17	19	19	21
6	0	1	4	2	1	1	0
5	4	1	1	1	0	0	0
4	0	1	2	0	1	1	0
3	1	2	1	1	0	0	0
2	1	2	0	0	0	0	1
1	8	4	3	3	3	3	2

Source: Van Huyck, Battalio, and Beil (1990).

chose a lower number is more likely to wait a couple of periods for the other player to adjust upward (and usually the other player did).

Van Huyck et al. ran other sessions in which subjects were paired in two-person groups, but with random rematching. As in the large groups, initial choices were dispersed and eventually converged near 1. Thus, a small group size per se does not guarantee efficiency; the stability and mutual adjustment in fixed pairings are also required. Clark and Sefton (1999) replicated this finding in a stag hunt game.

A more complete look at group size can be had by compiling data from various studies (and some informal experiments conducted during seminar presentations; see Weber et al., 2001). The top panel of Table 7.27 shows the frequency of first-period choices in groups ranging from two to fourteen–sixteen subjects (with medians in italics). The second panel shows the frequency of various minima in the first period, and the third panel shows the frequency of minima in the fifth period. There is a strong effect of group size on first-period minima, which is exacerbated by the fifth period. In groups of six or more, the first-period minima are never above 4, and usually deteriorate to 1 by the fifth period. In groups of two or three, however, minima of 5–7 are achieved with some frequency in the first period and two-person groups usually reach efficient minima of 7 by the fifth period.

Given the strong effect of group size, the distribution of first-period choices is surprisingly invariant to the number of subjects in the group. The percentage of subjects choosing 7 ranges is about as high in fourteen–sixteen person groups as in two–three-person groups, and initial medians

Table 7.27. *Weak-link games, various group sizes*

Group size	Number choice 1	2	3	4	5	6	7	Total sample size
Distribution of first-period choices (percent)								
2	28	3	3	7	*21*	0	36	28
3	8	5	8	17	*7*	2	41	60
6	18	7	13	*16*	7	7	39	114
9	0	11	28	*39*	5	0	17	18
12	25	4	13	*8*	16	4	29	24
14–16	2	5	5	17	*32*	9	31	104
Distribution of first-period group minima (percent)								
2	43	7	7	7	29	0	7	14
3	25	5	35	15	5	0	15	20
6	73	16	11	0	0	0	0	19
9	0	100	0	0	0	0	0	2
12	100	0	0	0	0	0	0	2
14–16	28	28	14	28	0	0	0	7
Distribution of fifth-period group minima								
2	14	0	0	0	0	0	86	14
3	30	15	20	15	0	0	20	20
6	80	10	10	0	0	0	0	10
9	100	0	0	0	0	0	0	2
14–16	100	0	0	0	0	0	0	7

(in italics) are 4–5 for all group sizes. Subjects in larger groups should realize that the minimum in a large group of choices is likely to be low, and should choose much lower numbers than in small groups.[27]

[27] I think subjects construe the game as a game between themselves and a single "representative" other player There is much related work on the cognitive psychology of judgment showing that people use "representativeness" as a heuristic to make judgments of this sort. For example, they think that exemplars that are highly representative of a category—as robins are of birds, or stylish Parisian women are of Parisian women in general—are also common, and that short sequences of Bernoulli trials that match the population proportion are more likely than sequences with too many runs. They extrapolate for group size by 'cloning' or reproducing the representative player but, unless their guess about the representative player's choice has some element of variance, they will ignore the downward bias that choosing a minimum from a large group exerts.

Table 7.28. Weak-link payoffs for row players in Berninghaus et al.

		Other player choices		
		Both X	One X, one Y	Both Y
Row player	X	80	60	60
	Y	10	10	90

Source: Berninghaus, Erhart, and Keser (2002).

Local Interaction

An important finding from theoretical work on social learning and evolution in games is that the nature of interaction among players can matter. This claim is verified by experiments by Berninghaus, Erhart, and Keser (2002). In their design, groups of three subjects played a weak-link or three-person stag hunt game with payoffs (shown in Table 7.28).[28] The strategy profile (X,X,X) is an (inefficient) Nash equilibrium (everyone earns 80), and (Y,Y,Y) is an efficient Nash equilibrium (everyone earns 90).

In a standard protocol, subjects are grouped into three-person groups and play for twenty periods. They learn the two choices by their partners at the end of each period (but not which partner made each choice). In a "local interaction" protocol, eight subjects are (hypothetically) arranged on a circle and play with their two nearest neighbors. Local interaction is different because a player A's two neighboring partners are influenced by the behavior of *their* distant neighbors, who do not directly affect A's payoff. Local interaction allows a kind of contagion or linkage, which permits equilibration to spread around the circle in a way that cannot happen in the standard protocol.

Equilibration in the two protocols is completely different. In the standard three-person groups, players initially play Y about three-quarters of the time, and seven out of eight groups coordinate on the Pareto-dominant all-Y equilibrium. In the local interaction groups, players start by playing Y only half the time, and play of Y falls steadily, to almost none in period 20. Players respond to their neighbors by playing X 64 percent of the time when one neighbor chose X. Like a disease that spreads through a population by close

[28] They report some other effects of different group sizes, and players arranged on a lattice who play the neighbors around them, which allow segregated equilibria in which "rectangles" of neighbors play the same way. Messick, Moore, and Bazerman (1999) report a very similar finding in "committee" with ultimatum bargaining.

contact—fear—the incidence of X play spreads from neighbor to neighbor, eventually infecting the entire group.

7.4.3 Mergers, Bonus Announcements, and "Leadership"

Camerer and Knez (1994) argued that weak-link games model many kinds of organizational processes. Motivated by this analogy, they studied three treatments of special interest to business researchers.

One treatment was the "merger" of two small groups into one large one. The group size effect documented above suggests that, everything else held equal, larger groups will do worse (so a merger will harm efficiency). On the other hand, players in small groups stuck in inefficient equilibria might use the merger as an opportunity to "restart" and coordinate on a better equilibrium.

In fact, mergers fail. Table 7.29 shows results. Each line shows the two minima achieved by each of two three-person groups after five periods, followed by the minima in the first and fifth periods for their merged six-person group.[29] When there was public information about previous group performance (top panel), groups usually continued to do what they had been doing after being combined, leading to low minima. When there was no information, groups picked lower numbers when they were combined. Eight of ten groups converge to the minimum of 1.

A second treatment is the public announcement of a "bonus" (by the experimenters) of $0.20 or $0.50 if everyone picks 7. Notice that, if a group has converged to a number below 7 (such as 1), there is *already* an implicit bonus, because everyone can profit if a tacit switch to 7 can be coordinated. The bonus announcement combines this added incentive with a public announcement that draws attention to 7 (much as communication did in the BOS and stag hunt experiments). Bonus announcements turn groups on a dime in one period, from 85 percent choosing 1–2 to 90 percent choosing 7. This regularity may explain why group-level bonuses in firms work surprisingly well (Knez and Simester, 2001).

A third treatment is "leadership." Much research extrapolates from case studies and other kinds of evidence to identify good and bad leaders and tries to determine their different traits. Evidence in social psychology on "attribution errors" suggests that searching for leadership qualities may be misguided. When both situational factors and personal traits contribute to

[29] For example, the first row shows a merger in the public information condition in which each three-person group knew the other group's period 5 minimum just before the merger. The two small groups had reached minima of 1 and 2; when combined, the six-person group had a minimum of 1 in period 1 and 1 in period 5. (The separate minima for the players in the two small groups in the first period after their combination are reported in parentheses; they were (1,3).)

Table 7.29. *Minima (MIN) after "mergers" of groups in Camerer and Knez*

Three-player groups	"Merged" six-player group	
Period 5 MINs	Period 1 MIN	Period 5 MIN
Public information about other group's minimum		
1,2	(1,3) 1	1
1,4	(1,1) 1	1
1,1	(1,2) 1	1
4,1	(4,1) 1	1
1,7	(1,7) 1	1
No information about other group's minimum		
2,4	(1,2) 1	1
7,3	(7,1) 1	1
3,2	(3,1) 1	2
7,3	(7,3) 3	3
7,3	(7,2) 2	1

Source: Camerer and Knez (1994).

a behavior, people who are trying to explain the behavior tend to attribute too much cause to personal traits and not enough cause to situational traits (e.g., Nisbett and Ross, 1991). For example, when a colleague in San Jose arrives 20 minutes late for a meeting and claims the traffic was unusually bad, people are too likely to jump to the conclusion that the colleague is chronically late, rather than blame traffic (the situation).

Weber et al. (2001) applied this idea to study misattributions of leadership ability in a weak-link game. Subjects played weak-link games with small groups of two people or large groups of eight–ten people. After each group had played two rounds, a "leader" was randomly selected to make a short speech exhorting the group to choose higher numbers so everyone could benefit. The earlier research on group size effects showed that two-person groups coordinate in weak-link games efficiently but larger groups cannot. Weber et al. guessed that subjects would mistakenly infer that their good or bad outcomes were due to the different leadership skills of the people chosen to be leaders, rather than to the difficulty of leading large groups and the ease of leading small groups.

The game had a weak-link structure in which players chose integers from 0 to 3, and earned $2.50 + 1.25 \cdot [\min_k(s_k^j) - 1] - s_i^j$ from choosing S_i^j (minus an additional 0.25 if the minimum was 0). Higher numbers lead to

Table 7.30. *Choices and leadership ratings in weak-link game of Weber et al.*

	Large group				Small group			
	0	1	2	3	0	1	2	3
Rounds 1–2								
Frequency of choices (percent)	25	24	20	32	5	24	26	45
Leadership rating (before)		5.88				5.80		
Rounds 3–8								
Frequency of choices (percent)	47	4	0	49	6	6	6	83
Leadership rating (after)		4.53				6.17		

Source: Weber et al. (2001).

better equilibria. The results are shown in Table 7.30. In the first two rounds (top panel), the large group chose slightly lower numbers and, immediately after the leaders' short speeches, the leaders were given ratings of 5.88 (large groups) and 5.80 (small groups) on a nine-point scale where 9 is high. When they played six more times, the large groups began to choose lower numbers and the small groups chose larger numbers, consistent with the group size effect observed by Van Huyck, Battalio, and Beil (1990). Restrospective ratings of leadership ability *after* the last period differed significantly—leaders of large groups had been marked down from the post-speech rating of 5.88 to 4.53, whereas leaders of small groups got a little bit of extra credit, with ratings rising from 5.80 to 6.17.[30]

7.4.4 Median-Action Games

Van Huyck, Battalio, and Beil (1991) studied order-statistic games in which a player's payoffs depend on the *median* number picked by each member of the group. Table 7.31 shows dollar payoffs, using the payoff function $\pi_i(x_i, M) = \$0.70 + \$0.10 \cdot (M - 1) - \$0.05 \cdot (x_i - M)^2$. Every point increase in the median adds a dime to each person's earnings, and each person is penalized a nickel times the squared difference between their choice and the median. Like stag hunt, median-action games pit selection principles against one another. Payoff-dominance points to choosing 7, but "security" (or maximin) points to 3, which guarantees earnings of $0.50.

[30] In another study, the subjects played the eight-period game and rated the leaders. Then they were unexpectedly told they would play another eight periods and could keep the same leader or "fire" him. Subjects voted to switch leaders 32 percent of the time in the large groups and 16 percent of the time in the small group, so their ratings affected their behavior.

Table 7.31. *Payoffs in median-action game of Van Huyck et al.*

	Median value of X chosen						
Your choice of X	7	6	5	4	3	2	1
7	1.30	1.15	0.90	0.55	0.10	−0.45	−1.10
6	1.25	1.20	1.05	0.80	0.45	0.00	−0.55
5	1.10	1.15	1.10	0.95	0.70	0.35	−0.10
4	0.85	1.00	1.05	1.00	0.85	0.60	0.25
3	0.50	0.75	0.90	0.95	0.90	0.75	0.50
2	0.05	0.40	0.65	0.80	0.85	0.80	0.65
1	−0.50	−0.05	0.30	0.55	0.70	0.75	0.70

Source: Van Huyck, Battalio, and Beil (1991).

Median-action games capture economic situations in which players prefer to conform. An example is a group production process that depends on the median person's effort and people prefer to not work too hard, or too little. Subjects played in groups of nine for several periods. At the end of each period they learned only the group median. Choices from six sessions are shown in Table 7.32. Two patterns are regular. First, there was substantial dispersion in the first period, which *always* leads to first-period medians of 4 or 5. Second, there is perfect lock-in: In *every* session the tenth-period median is exactly the same as the first-period median.

Table 7.32. *Results in median-action games (six sessions pooled)*

	Period									
Choice	1	2	3	4	5	6	7	8	9	10
7	8	2	2	0	0	1	1	0	0	0
6	4	6	6	6	3	3	4	1	3	1
5	15	15	22	19	22	20	20	24^{1}	23^{1}	26^{2}
4	19	26	22	29^{1}	27^{1}	30^{2}	30^{2}	28^{2}	28^{3}	27^{3}
3	8	3	2	0	0	0	0	1	0	0
2	0	1	0	0	1	0	0	0	0	0
1	0	1	0	0	0	0	0	0	0	0

Source: Van Huyck, Battalio, and Beil (1991).
Note: Superscripts denote the number of groups fully equilibrated (all choosing the same number).

Table 7.33. First-period choices in three median-action games

	Game					
	γ		ω		ϕ	
Number	Principle	%	Principle	%	Principle	%
7	Payoff dominance	15	Payoff dominance	52		8
6		7		4		11
5		28		33		33
4		35		11	Maximin	41
3	Maximin	15		0		8
2		0		0		0
1		0		0		0

Source: Van Huyck, Battalio, and Beil (1993).

Van Huyck et al. used two other games to study the effects of payoff dominance and maximin separately. One game (ω) is like the game shown in Table 7.31 except payoffs to actions that are not best responses are 0 (i.e., the penalty to missing the median is to earn 0). In this game, the minimum earnings for all strategies are 0, so maximin no longer selects strategy 3. Table 7.33 contrasts first-period choices in this game with those in the original game γ. Equalizing the penalty for mismatching the median shifts the distribution of choices upward. Half the subjects chose 7, compared with only 15 percent in the original game γ.

In another game ϕ, the gain from an increasing median, $\$0.10 \cdot (M - 1)$, is removed. Then all strategies pay a maximum of \$0.70 (i.e., the diagonal terms in Table 7.31 are all \$0.70), so payoff-dominance no longer favors any strategy. Results from this game are also shown in Table 7.33. The initial choices look a lot like those from the original game γ.

The three games show that a combination of selection principles guides choices. When a single selection principle points to a choice, most people pick it but there is still substantial dispersion. When selection principles point to different choices, people often pick those or—more typically—pick something in between. No single principle universally trumps the others.

7.4.5 Preplay Auctions and Entry Fees

Van Huyck, Battlio, and Beil (1992) experimented with a special form of communication—a preplay auction for the right to play a coordination game. In these experiments, eighteen subjects participated in an "English clock auction." In the clock auction, prices started at a low level, \$0.05, and increased slowly. When the price rose to a price a player did not want to pay, that player dropped out of the auction and earned nothing. When nine players remained, at a price P^*, they then played a nine-person median-action game and P^* was deducted from their earnings.[31]

As players sit in the auction and watch the price rise, they should think to themselves:[32] "At the current price P, anybody who chooses to play expects to earn more than P. Then the median will be a number that enables me to earn P if I just choose that number too. So I should stay in the auction." The logical, surprising consequence of this reasoning is that prices will rise to \$1.29 and all players will stay in. Nine of them will play the game, choose 7, and earn \$1.30 for a tiny \$0.01 net return. This is an application of "forward induction," by which players look back at forgone choices others could have made, to make educated guesses about what those players can be expected to do.

Table 7.34 shows results from the first period, in five different sessions. Each column shows the price at which only nine players were willing to play, and the distribution of numbers those players chose. Forward induction has some force: The preplay auction increased the number of high-number choices 5–7, from 50 percent in the no-auction baseline experiments reported above, to more than 80 percent with auctions.[33] On the other hand, forward induction did not drive subjects all the way to the payoff-dominant equilibrium right away. However, across trials prices rose and a median of 7 was reached in the third period, on average.

After seeing the Van Huyck et al. auction results, Gerard Cachon and I (1996) wondered whether the effect of preplay auctions was the result of forward induction reasoning, or of the fact that all players had to pay a fee to play the game. This distinction was important because psychological research shows that people do not always ignore sunk costs in making decisions. In one of the most clever studies, Arkes and Blumer (1985) randomly gave surprise discounts to people who had come to a theater box

[31] It is crucial that players all know that the other eight people they are playing the game with have all agreed to pay P^* as well.

[32] More formally, the application of three principles implies that subjects can use the auction to reach the payoff-dominant equilibrium where everyone plays 7 and earns \$1.30. The first principle is dominance—never lose money if you can avoid it. The second principle is dynamic consistency—if nothing has changed, do what you planned to do. A player who obeys these two principles will never pay P then choose an action in the game that gives an expected payoff less than P. The third principle is iterated dominance—players believe others obey the first two principles. This implies that players can expect others always to choose numbers that give higher payoffs than P^*.

[33] Notice, however, that these results could be the result of players having dispersed beliefs about what would happen in the median-action games, and the auction mechanism screened players who had the most optimistic beliefs (and who then chose the highest numbers).

Table 7.34. First-period prices and choices in median-action games with preplay auctions

	Session and prices					
	10	11	12	13	14	Overall
Choice	\$1.24	\$1.00	\$0.95	\$1.05	\$1.05	percent
7	7	4	1	2	0	31
6	2	1	0	1	7	24
5	0*d*	2	6	3	1	27
4	0*d*	2	1	3	0	13
3	0*d*	0*d*	1	0*d*	1*d*	4
2	0*d*	0*d*	0*d*	0*d*	0*d*	0
1	0*d*	0*d*	0*d*	0*d*	0*d*	0

Source: Van Huyck, Battalio, and Beil (1991).
Note: d denotes a dominated action.

office to buy six tickets to a series of plays. If the theatergoers exhibited a "sunk cost fallacy," those who paid the full price might feel they "had to go" to "get their money's worth" even though the ticket cost was sunk.[34] In fact, the full-price group went to significantly more plays.

Applied to the auction game of Van Huyck et al., having to pay a fee might change players' behavior by making them eager to recoup the fee by choosing higher numbers. And if players think that others exhibit such behavior, they could use "avoid losses" (see Kahneman and Tversky, 1979) as a selection principle, which would rule out some equilibria.[35]

Forward induction can be easily separated from loss-avoidance—just make all the subjects pay the entry price. If players do not *choose* to pay a price, forward induction does not apply so the mandatory fee should make no difference.

[34] Economists will note that the cost is not sunk if the tickets can be resold, but there is no reason to think the resale price varies between those who got the discount and those who paid the full price, which is the crucial comparison.

[35] The loss-avoidance principle partitions the set of equilibria into those in which everyone loses money (after subtracting the fee) and those in which people might profit, and selects only equilibria in the profitable subset. Notice that payoff-dominance also partitions equilibria into "best for everyone" and "not best for everyone" and picks "best for everyone." Seen this way, loss-avoidance uses the same intuition that underlies payoff-dominance, but "satisfices" by allowing equilibria that are profitable but not best for everyone.

In fact, forcing subjects to pay the fee *did* lead them to choose higher numbers, almost as strongly as letting them choose whether to pay the fee. Thus, some of the efficiency-enhancing effect of preplay auctions that Van Huyck et al. observed is probably due to a sunk cost effect. Cachon and I conjectured that players use "avoid losses" as a selection principle which coordinates their beliefs on those equilibria that earn a positive profit after the fee is subtracted.[36]

7.4.6 *General Order-Statistic Games*

Ideally, experimental observations inspire theorizing, which in turn suggests interesting new experiments. A good example of this dialogue is the learning model of Crawford (1995), which was developed to explain the adaptive dynamics observed in the weak-link and median-action games. (See Chapter 6 for a fuller discussion of learning.) Crawford's model assumes players begin with different beliefs and strategy choices, which can be characterized statistically.[37] Van Huyck, Battalio, and Rankin (2001b) ran experiments to test the most striking fresh predictions of the Crawford model.

In their design, there are n players who choose integer numbers $x_i \in \{0, 1, 2, \ldots, 100\}$. To make the results comparable to earlier work, each choice is then transformed onto a 1 to 7 scale by taking $e_i = 1 + 0.06 \cdot x_i$. The jth order statistic is denoted by $m_{j:n}$. (The weak-link experiments use the minimum or first-order statistic, $m_{1:n}$, and the nine-person median-action games used $m_{5:9}$.) The payoff to player i is $\pi_i(e_i, m_{j:n}) = \$0.30 + \$0.10 \cdot m_{j:n} - \$0.05 \cdot (m_{j:n} - e_i)^2$.

In Crawford's model, players choose a weighted mixture of their own previous choice and the previously observed order statistic, plus an idiosyncratic zero-mean term and a period-specific common drift term. Under general conditions, Crawford proves that: increasing j—that is, taking a higher number as the order statistic from which deviations are penalized—will *increase* choices; and when the order statistic is the median there is no expected change in the order statistic across periods. The median is $j = (n + 1)/2$ (for n odd). When j is below (above) that threshold, the order statistic is predicted to fall (rise) over time.

[36] A key part of this interpretation is that the fee is common knowledge. In some experimental sessions, subjects knew only their own prices, so they could not tell whether other people were losing money at particular equilibria. In those sessions, raising the price did *not* affect behavior significantly. This means common knowledge of losses is crucial: Loss-avoidance is a selection principle applied to infer what others will do (and what others will expect you to do, and so on), not a principle of individual choice.

[37] His model "reflects the conviction . . . that it is impossible to predict from rationality alone how people will respond to coordination problems" (Crawford, 1995, p. 105).

Table 7.35. Order statistics in all sessions

	Position of game in experimental session			
	First		Second	
n, j	First period	Last period	First period	Last period
5,4	78	100*	53	53
	69	80*	60	100*
	97	100*	67	100*
	95	100*	100	100*
5,2	42	43	60	100
	35	40*	48	80*
	50	100*	100*	100*
	36	36*	100	100*
7,4	80	100*	50	50
	50	50	50	90
	100	100*	96	100*
	73	73	59	59*
	50	100	—	—
	70	100	—	—
7,2	33	27	49	49*
	39	36	15	15*
	6	0	18	46
	32	33*	35	73*

Source: Van Huyck, Battalio, and Rankin (2001b).
Note: * denotes full equilibration (all subjects choosing the same number).

These predictions motivate the design of Van Huyck et al., which varies the order statistic j, either 2 or 4, and the group size n, either 5 or 7. These variations test whether choices rise with group size and fall with j, and test whether choices change over time in the predicted directions. Table 7.35 summarizes the order statistics derived from each session, in the first and last periods of the first and second game. Each line is data from one session. The games are organized from top to bottom to provide a quick visual test of the prediction that higher n and smaller j lead to lower numbers.

The order statistics differ across (n, j) conditions in ways that are consistent with the prediction of the Crawford model. Lowering j or raising n (fixing the other factor) lowers choices. Choices should also drift upward over time in the (5,4) condition, remain stable in the median-action game (7,4), and fall in the conditions (5,2) and (7,2). In fact, choices generally rise or stay constant in most sessions. Thus, Crawford's model is accurate about the *relative* tendency to drift upward, across games, but underestimates the amount of upward movement.

The contrast between the (7,4) results and the earlier results in Van Huyck, Battlio, and Rankin's (1991) nine-person median games is worth noting. In those games, the median choice (transformed to a 0–100 scale) was 58.3 whereas here the initial (7,4) mean is 69. These groups sometimes escape the irresistible path-dependence that occurred in the (9,5) games with only seven actions (causing the final median *always* to equal the first-period median). Many groups increased rising order statistics over time. Typically, once the second-period order statistic was larger than the first period, this created a common belief that the third-period statistic would be larger still, creating a precedent of (profitable!) change which often resulted in an equilibrium of 100. Van Huyck, Battlio, and Rankin speculate that the fine-grained strategy space might play a role in these dynamics. Deviating from the order statistic by even 5 units produces a tiny loss of only $\$.005 \cdot (0.3)^2 = \$0.0045$. Subjects can therefore experiment more cheaply and, in smaller groups, can nudge the order statistic upward.

Summary: In order-statistic games (Van Huyck, Battalio, et al.), players' payoffs depend on their numerical choices and how far they deviate from some order statistic of others' choices (such as the minimum or median). In minimum ("weak-link") games, large groups generally collapse to inefficient outcomes. In median games, players gravitate toward middle numbers and there is near-perfect path-dependence (medians after several periods of play are precisely the median in the initial period). Conducting a preplay auction for the right to play improves efficiency, since players know that those who have paid a high price will play efficiently (or perhaps are just more optimistic). Further experiments showed that simply *imposing* a high price on all players to play also improves efficiency, which is consistent with players using avoidance of losses as a principle to rule out some equilibria and move toward others. In a good example of the potential of two-way dialogue between data and theory, Crawford (1995) showed how these patterns could be explained by a model in which players have initial beliefs that are spread out, but respond to what they have seen. His model also predicts interesting effects in other order-statistic games, which are largely confirmed by new experiments (although players converge to more efficient outcomes than Crawford would predict).

7.5 Selecting Selection Principles

7.5.1 Simplicity

Early in the history of game theory, a small literature investigated the connection between games with extensive forms and games with normal forms. Dalkey (1953) showed that all extensive-form games with the same normal-form representation could be derived from one another with one or more "inessential transformations." The applied mathematicians who did this work were interested in purely formal relations among different extensive-form games which would render them equivalent (through their shared normal form). The term "inessential" conveys an implicit prediction that games which are transformations of one another will be played in the same way.

However, some games that are formally equivalent (through some inessential transformation) *are* played differently (e.g., the effects of timing described in Section 7.2.5 and Cooper and Van Huyck, 2001). The fact that representation sometimes matters raises the possibility that, in games with multiple equilibria, features of the representation of the game are used as selection principles.

Motivated by this intuition, Ho and Weigelt (1996) studied whether players use representational simplicity as a selection principle. They measure simplicity of an equilibrium in three ways: (1) the number of outcomes (terminal nodes) following from the last information set on an equilibrium's path; (2) the number of levels of iterated rationality required to make an equilibrium subgame perfect; and (3) the number of levels of knowledge of strategies required to reach an equilibrium.

Figure 7.4 illustrates their second simplicity measure, using their game G5. In game G5, there are two subgame perfect equilibria in pure strategies: outcomes (8,4) and (3,9). The equilibrium outcome (8,4) is reached if player I chooses top and II chooses top also. To make player I's top choice a best response, all she needs to think is that II will choose top (with high enough probability). Whether II is doing so rationally (forming beliefs and choosing the strategy with the highest expected utility given those beliefs) or not does not matter, so I does not need to know that II is rational. The equilibrium outcome (3,9) requires more: If both choose bottom, and play proceeds to the subgame where player I gets to move again, then player I must believe that II will move across to reach (3,9) if she gets the chance, rather than foolishly move down to (0,2). Thus, to reach (3,9) requires I to believe II is rational. Since reaching the (3,9) equilibrium requires I to think II is rational, but reaching (8,4) does not, the (8,4) equilibrium is simpler. Players will be more likely to reach the (8,4) equilibrium if they either prefer simpler equilibria, or use simplicity as a focal principle.

Ho and Weigelt's experiments used two-player games with two or three pure-strategy equilibria. In all cases the players' equilibrium payoffs add

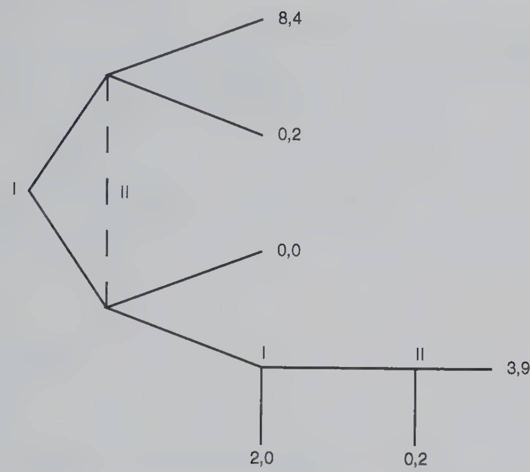


Figure 7.4. *Game G5, illustrating simplicity as a selection principle. Source: Based on Ho and Weigelt (1996).*

to 12, but they varied the payoff disparity—payoffs were (3,9), (8,4), or (7,5)—to see whether "minimize payoff-inequality" also acted as a focal principle. Results from periods 1–5 are summarized in Table 7.36. The table shows the percentage of players choosing strategies that lead to the

Table 7.36. *Fraction of players choosing simple equilibrium in Ho and Weigelt's games 4–6*

			Measure of simplicity			
	Equilibrium payoffs		Number of outcomes		Level of iterated rationality	
Game	Simple	Complex	Player s	Player c	Player s	Player c
G4	(3,9)	(7,5)	78	74	46	32
G5′	(3,9)	(8,4)	92	83	71	41
G6′	(8,4)	(7,5)	56	71	69	77
G5	(8,4)	(3,9)			89	84
G6	(7,5)	(8,4)			86	80
G4′	(7,5)	(3,9)	69	59	68	63

Source: Ho and Weigelt (1996).
Notes: Player s denotes player who prefers the simple equilibrium (player II in the first two games, player I in the last four games); player c denotes player who prefers the complex equilibrium. Data are fractions of choices in rounds 1–5.

simple equilibrium, reported separately for the type of player who prefers the simple equilibrium (player s) and the type of player who prefers the complex equilibrium (player c). Most fractions are above 50 percent, so there is a general tendency to choose the simpler equilibrium by either measure. Payoff-inequality is also used as a focal principle because the equilibrium with the more-equal payoffs is generally chosen more often (holding simplicity constant). There is also a slight "self-serving bias"—the players s, who get more from the simple equilibrium, choose it about 5 percent more often than players c, who get less from the simple equilibrium.

7.5.2 Empirical Comparison of Selection Principles

In the first draft of this chapter, I wrote that no single selection principle seemed to dominate others in determining what subjects chose in the first period or converged to. Consequently, studies should be designed to measure the frequency with which various principles are used. Then a paper by Haruvy and Stahl (1998) arrived in the mail, reporting exactly the kind of comparison I had in mind.

Their experiment used twenty symmetric games with three strategies in each. In each game different strategies are chosen by various selection principles. Comparing across games measures how often the various principles are used. Table 7.37 summarizes the fraction of choices consistent with each of four selection principles: payoff-dominance, risk-dominance, maximin (security), and "level-1 bounded rationality" (choose strategies with the highest expected payoff given a diffuse prior about opponent behavior). The table shows the minimum, maximum, and average frequencies of choices corresponding to each principle, across the twenty games. The level-1 principle *always* accounts for at least 62 percent of the choices, and accounts for 83 percent on average, which is twice as many as risk-dominance and maximin. Payoff-dominance predicts worst of all.

Haruvy and Stahl use these choices to fit an "evidence-based" model of first-period choices. Selection principles produce numerical evidence for different strategies. Players weight evidence and combine it to produce an overall score for a strategy, then choose strategies with probabilities given by a multinomial logit function of their scores. Estimation of the model confirms the Table 7.37 impression that level-1 is a good predictor of one-shot play. One-third of the players are estimated to use level-1 evidence exclusively, and half are "wordly" types who combine level-1 with a small sprinkling of other evidence.

Summary: A comprehensive test by Haruvy and Stahl (1998) using twenty games, comparing different selection principles, shows strong and robust support for "level-1" or Laplacean reasoning, in which players act as if

Table 7.37. Frequency of choices consistent with selection principles in twenty games (percent)

	Selection principle			
Game	Payoff-dominance	Risk-dominance	Maximin	Level-1 bounded rationality
Average	28	44	53	83
Minimum	0	0	0	62
Maximum	98	98	94	98

Source: Haruvy and Stahl (1998).

others might do anything, and choose strategies that are best responses to that "diffuse prior" belief. However, there is evidence for other selection principles, such as simplicity, precedent, and assignment. It is unlikely that any theory of selection will be anything more (or less) than a statistical collage of when the different principles are used.

7.6 Applications: Path-Dependence, Market Adoption, and Corporate Culture

In this section I describe three related experiments inspired by observations made by experimenters and applied economists about the nature of coordination. The first is a creative attempt to explore the strength of path-dependence and "historical accident." The second is motivated by interest in the development of information technology for trading financial assets. The third is about development of culture, in the special form of language "codes" for rapid, memorable communication.

7.6.1 Path-Dependence: Creating a Laboratory "Continental Divide"

A clear lesson from the median-action coordination games is that the determination of medium-run equilibria can be extremely sensitive to small "historical accidents." Van Huyck, Battalio, and Cook (1997) studied this property further using a "continental divide" game. (The game was discussed in Chapter 1, and again in Chapter 6 on learning, so I'll only sketch the basic structure and results here.) Players choose integers 1–14 and their payoff depends on their choice and on the *median* choice of the seven people in their group. If the median starts at 7 or less and subjects best-respond, they will eventually work their way to a pure-strategy equilibrium at 3. If the median starts at 8 or above, however, best-responding will eventually converge to an equilibrium of 12. The payoff at 3 is about half as much as at 12. This game captures the possibility of extreme sensitivity to initial conditions (or path-dependence), which has fascinated scientists interested in chaotic dynamics and complex systems in the past fifteen years or so.

Path-dependence is often evident in physical systems. I once went mountain climbing in Alaska with a friend. We stood on the continental divide, which is the imaginary line (marked on a map) that marks the point at which the direction of water flow changes from one direction to the opposite. We poured water from a canteen right at the divide; some water trickled south and some trickled north. Eventually, the north-flowing water made it to the Arctic Ocean, and the south-flowing water to the Pacific. Molecules that began imperceptibly close together ended up a thousand miles apart.

In the experiments of Van Huyck et al., the invisible "separatrix" between choices 7 and 8 acts precisely like the continental divide. Half the groups start high and are inexorably drawn to 12–13; the other half start low and converge to 3–6. Since the low groups, in equilibrium, earn about half as much as the high groups do, tiny historical accidents that determine the initial conditions have large, persistent earnings consequences. One cannot help but wonder whether the continental divide property is related to the fact that Thailand and Vietnam prosper much more than neighboring Laos does, or the fact that in the late 1990s housing prices in Palo Alto went sky-high while across a creek, in East Palo Alto, more people murdered each other (per capita) than anywhere else in America.

7.6.2 Market Adoption

In large centralized markets for commodities, such as financial assets, most traders value the ability to make large trades cheaply (liquidity) and quickly (immediacy). To find the best price quickly, traders want to use the market that most other traders use. These "participation externalities" tend to lead to dominance of one market over another (and perhaps natural monopoly). However, changes in technology also affect liquidity and immediacy by lowering the per-trade cost. Can a new market that offers lower trading costs displace a market that has been historically active?

This question was investigated experimentally by Clemons and Weber (1996).[38] Their experiments have four buyers or four sellers. Each subject

[38] Indeed, their experiments are a nice example of how policy considerations lead to special features of experimental design. They were motivated by changes in information technology that make it possible for investment institutions to trade more cheaply in non-intermediated electronic off-exchange market systems such as Instinet or Posit than on the established New York Stock Exchange or London Stock Exchange