Table 7.38. Percentage of shares traded in market Y

	Part I periods	Part II (market Y more profitable) periods			
Sessions	1–12	1–3	4–6	7–9	10–12
1–4	48.3	62.9	67.6	71.2	73.7
5–7	45.5	52.9	56.4	60.4	57.5
T	41.2	57.1	59.6		

Source: Clemons and Weber (1996).

has ten shares to trade and can divide the ten, in any (integer) combination, into separate orders which are sent to either market X or market Y. Buyers and sellers earn a (reduced-form) profit if they trade in the market where the order imbalance benefits their side (e.g., buyers benefit if there are more sellers in the market they trade in). To simulate risks from not having orders executed in a new market, after subjects submitted orders to markets X and Y in each period a coin flip would determine whether market Y would actually open. If it did not open, orders were not executed and subjects earned no profit from these orders.

Table 7.38 shows the percentage of orders submitted to market Y—its "market share"—averaged across groups of sessions. The market share of Y in the first twelve periods of part I averaged less than half, so Clemons and Weber were able to induce subjects to trade more often in market X. After the twelfth period, incentives were changed to favor market Y. The execution risk was removed and profits were higher in market Y (with different bonuses in sessions 1–4 and 5–7).

Table 7.38 shows what happened in the next twelve periods, after this "technological advantage" was added to market Y. There is a steady movement toward market Y but the movement is not very rapid (especially in sessions 5–7, where the premium for switching is lower).[39]

Session T was an unusual session motivated by concern that student subjects behave differently than the market professionals whose behavior is ultimately of interest. Subjects in session T were eight New York Stock Exchange floor traders. They participated in only two periods in part I, and

markets, which hold a dominant share of trading volume but impose higher fees. Policy or field-motivated experimentation has been rare in game theory experiments, except for some auctions and some kinds of bargaining experiments (e.g., final-offer arbitration).

[39] An exponential model used to guess how many periods would elapse before market Y achieved an 85 percent market share forecast ten periods in sessions 1–4 and thirty-three periods in sessions 5–7.

six periods in part II. Table 7.38 shows that the traders behaved much like the students did. The estimated time for market Y to reach 85 percent share was fifteen periods, comparable to the ten periods forecast for student subjects. Their finding is one of several experiments showing that regularities established with student subjects are usually replicated when professional subjects are used (see Ball and Cech, 1996).

7.6.3 Culture

Camerer and Weber (in press) used experiments to study the development of organizational culture. In our experiments, subjects saw sixteen photographs depicting workers in offices. The pictures had similar features (office furniture, people talking) but each picture had some distinctive features. In each pair of subjects, one subject was given a list of which eight of the sixteen pictures were the targets in a particular trial. That subject had to describe the target pictures to another subject accurately enough that the listener subject could pick them out, with a penalty for being too slow. Over several trials, subjects developed a pithy homemade language to describe the pictures—a kind of jargon or slang, a feature of culture. For example, one picture showed a meeting in which a man was gesturing with his hands. In the first trial, it took about 30 seconds of description to get the listener to understand what picture was being described. After several trials, however, the talker just said "Macarena" (the name of a then-popular dance which used hand gestures similar to those in the picture).

To study cultural conflict, the talker in one group B was "fired" and the listener was combined with the group A players. The time it took to complete the task shot up because the group B listener didn't understand the language the A pair had developed. Furthermore, before the "merger" took place subjects forecast how long it would take them to complete the task. Their forecasts were too optimistic. Just as a fish swimming in water is oblivious to its liquid environment, the players were unaware of how special was the culture (language) they created, and how hard for new players to grasp. Camerer and Weber speculate that this phenomenon is a very stylized representation of the kind of surprising cultural clash that undermines many corporate mergers.

Summary: Three applications show that: small historical accidents can lead to equilibria with large payoff differentials; biasing traders toward one of two markets can lead to a handicap that is modestly, and slowly, overcome when the "bad" market suddenly becomes efficient because of technological change; and experiments can be used to create endogeneous culture and explore how merging different cultures creates surprising conflict.

7.7 Conclusion

Theory has not generally provided clear, convincing guesses about which of the multiple equilibria will occur in a coordination game with many equilibria. A high ratio of observation to theorizing is therefore necessary to understand which equilibria are selected. As a result, experimental research on coordination games represents an influential body of empirical findings. Energetic research by Cooper, De Jong, Forsythe, and Ross at Iowa; Van Huyck, Battalio, Beil, and other colleagues at Texas A&M; and many others, has provided a wealth of data on coordination games, which should keep theorists busy for some time.

Several basic conclusions have emerged from this research: Coordination failure is common—play does not usually converge reliably to a Pareto-efficient equilibrium. Risk-dominant and secure outcomes are typical in games where the risk inherent in playing the payoff-dominant equilibrium is high. In stag hunt games with a Pareto-efficient equilibrium and a risk-dominant equilibrium that is efficient, whether play converges to the efficient equilibrium seems to depend sensitively on the degree of assurance that others will play efficiently.

Adaptive dynamics generally do guide players to an equilibrium, often swiftly. The standard pattern, as Crawford (1995) noted and theorized about, is that initial choices are dispersed but players best-respond sluggishly, perhaps optimistically and noisily, to what they have observed. In large-group median-action games, for example, these dynamics imply that the group converges to a strategy choice in the middle of the strategy space; and they do. In weak-link games, where payoffs depend on the minimum number picked in a group, these dynamics imply that small groups can achieve efficiency but large groups will erode to the inefficient equilibrium; and they do.

Convergence can depend on the matching protocol and information available to players. In stag hunt games with "local interaction," in which players are located on a circle and play their two nearest neighbors, inefficiency can spread through a population like a disease.

A strong intuition is that, in coordination games with common interests in reaching some equilibrium, a little communication among players would go a long way. Although communication can enhance coordination effectively, its effects are often weaker than one might expect and depend in subtle ways on game structure. Communication works when it (1) selects a unique equilibrium and (2) provides enough assurance that players will believe the communication and act on it. The combination of these conditions means that in battle of the sexes, for example, one-way communication works well because it points to a single equilibrium (the one that the communicator prefers). But two-way communication creates almost as much conflict as no communication, so it does not help. In stag hunt games the effects of communication are opposite: One-way announcements help only partly because they do not provide full assurance; two-way announcements work beautifully because they provide enough assurance and point to a consistent outcome.

Special forms of communication have mixed effects on efficiency. Preplay speeches by a single player in a weak-link game (a "leader") with many players do not help avoid coordination failure. (The leaders are also unfairly blamed by other subjects for the failure of their large group to coordinate, an effect predicted by research in social psychology on attribution of cause.) "Assignment" or recommendation of equilibrium by an outside authority (e.g., the experimenter) guides players to an equilibrium when the recommendation does not compete with another focal principle. When it competes with another principle, such as payoff-dominance or payoff-equality, players are usually torn between following the recommendation and following the other principle. Preplay auctions for the right to play a coordination game do enhance efficiency, but it is not clear whether the effect is the result of self-selection of players with optimistic beliefs (and, hence, optimistic actions) or of forward induction by players about the intentions of others who self-selected by paying the auction price. Efficiency is also enhanced by simply charging all players a fee to pay (with no option to quit, so forward induction does not apply). The fact that charging a fee matters suggests that "loss-avoidance" is used as a focal principle to exclude equilibria in which all players lose. (In turn, this principle implies that the way losses are "mentally accounted" for can affect equilibrium selection.)

Work on evolution of meaning shows that, even when a natural assignment of messages to behavior is not supplied to subjects, they can create a homemade language within ten–twenty experimental periods, if it is in their interest to do so.

Experiments on matching games, in which players earn a fixed prize if their choices match, so there are no payoff differences among equilibria, show the importance of understanding the cultural and cognitive bases of "common ground." In these games, thinking strategically means guessing how others will distinguish objects in a choice set, and how they think you will distinguish them, and so forth. Experiments suggest subjects are rather inventive and consistent in finding shared distinctions among objects, or principles that select focal points. Theories that pit the distinctiveness of strategy features against the jointly understood likelihood that players recognize those features (if they are subtle) promise to unpack elements of focality.

Finally, the standard approach to refinement of equilibrium concepts in coordination games is to posit selection principles that pick one equilibrium out of many. A list of selection principles includes: precedence (choose the last equilibrium played); risk-dominance (choose the equilibrium with the lowest "risk," i.e., product of deviation costs); payoff-dominance (choose the equilibrium that pays us all more than any other does); loss-avoidance (choose only equilibria that pay everyone positive profits); maximin or security (choose the equilibrium in which everyone maximizes their minimum possible payoff from that strategy); level-1 expected payoffs (choose the equilibrium that gives the highest expected payoff if others choose randomly); first-mover advantage (if there is a known first-mover, she gets her preferred equilibrium); complexity (pick "simpler" strategies).

There is evidence for each of these focal principles in various games. The paper by Haruvy and Stahl (1998) is an excellent comparison of a subset of these rules. They find that the level-1 rule does extremely well when it competes with payoff-dominance, risk-dominance, and maximin. Indeed, there is evidence from a wide range of games that people often choose strategies that are good responses to uncertain beliefs about what others will do, at least in the first period of a game (cf. Chapter 5). This can explain initial choices of preferred strategies in battle of the sexes, of 4–5 in both the median and weak-link order-statistic games, average choices distributed around 50 in the general order-statistic games, and phenomena described in other chapters—such as initial behavior in dominance-solvable *p*-beauty contests (see Chapter 4) and signaling games (see Chapter 8). The level-1 rule is appealing because it uses a lot of information about possible payoffs (indeed, every possible payoff is weighted equally). It can also be seen as a compromise between the pessimistic and optimistic intuitions of maximin and maximax, but it is more robust than those criteria because it does not depend on only one possible payoff.

Appendix: Psycholinguistics

In psycholinguistics, Herbert Clark (1996) and others take an action-oriented approach that emphasizes the idea that language is often used to coordinate joint action. The recognized precursor to Clark's action-oriented approach seems to be Mead (1934), who emphasized the fact that speakers must take the perspective of their listeners ("audience design") in choosing words.

To do so, speakers and listeners must use language that is understandable in terms of a shared "common ground." Clark argues that searching for common ground is a heuristic that halts the infinite regress of wondering whether a listener knows that the speaker know that . . . (ad infinitum), making effective speech possible. (Note the analogy to behavior in the "email game" in Chapter 5, in which players naturally coordinate on a convention that one or two steps of message-passing are enough to create "mutual-enough" knowledge for coordinated action to proceed.)

An important set of examples for investigating common ground are "indefinite references," in which a speaker refers to one object in a class without being specific about which object she is referring to. For example, Clark, Schreuder, and Buttrick (1983) showed subjects' pictures depicting four types of flowers growing next to a fence and asked them, "How would you describe the color of *this* flower?" The phrase "this flower" makes the reference indefinite because the experimenters refer to a flower, but don't say which of the four types they mean. Nonetheless, most subjects were able to induce a definite reference by using some cue about which of the four flowers was being referred to. For example, when daffodils were substantially more prominent in the picture than the other three types, half the subjects were confident that daffodils were the flowers that were being referred to. Clark and others suggest that several general heuristics are used to establish common ground.

One is "physical copresence"—two communicators assume objects that are physically observable to both people are likely to be the objects which are being referred to (see Chwe, 2001). For example, if a person at a party exclaims, "What a dress!" it probably refers to the nearest dress within looking distance of both communicators (rather than a dress that both saw earlier but has disappeared from view). "Perceptual copresence" and "linguistic copresence" are similar heuristics. "Category membership" is a way of inferring common ground, by assuming that people in the same category have a common database. This is beautifully illustrated in experiments by Kingsbury (1968). He approached people on the streets of Boston and asked for directions, speaking in either a Boston or a Missouri accent. The instructions he received were more detailed when he asked in a Missouri accent, and less detailed when he asked in a Boston accent. Bostonians inferred membership in the category "people who know their way around Boston" from the Boston accent, and were able to draw on perceived common ground to give pithier instructions.

The psycholinguistic work on common ground tends to emphasize how nimble speakers and listeners are at creating shared understanding. However, it would be useful to study two categories of lapses or mistakes. The first category is systematic mistakes in the guesses of speakers about what listeners know. For example, speakers probably take their own knowledge as prototypical and may insufficiently adjust for differences between what they know and what others know. Indeed, there tends to be a positive correlation between subjects' estimates of the percentage of people who recognize a person or object, and their forecast errors (their estimate minus the actual percentage). This correlation occurs because people who are familiar with a person, for example, don't adjust for the fact that if they are familiar then their familiarity is likely to be an upward-biased estimate of the population proportion (and oppositely if they are unfamiliar). In rational expectations terms, if forecast errors are correlated with any available information, then errors can be reduced by using the information to eliminate the correlation. In this case, their own familiarity is information they could use to give a clue about population familiarity—they should self-consciously recognize that on average, if a person is familiar to them, their recognition is to some extent a fluke and won't be commonly held. This "false consensus" bias may lead to a "curse of knowledge" in which well-informed speakers tend to confuse an audience because the speakers do not appreciate how little the listeners know. (Piaget mentioned teaching as an example of the curse of knowledge in action; and see Camerer, Loewenstein, and Weber, 1989, for more discussion.)

The language-for-coordinated-action view emphasizes coordination in which speaker and listener strive to achieve the same goal. A second kind of "mistake" in conversational matching can arise in cases, such as bargaining or confidence games, in which speakers try to exploit common ground to trick listeners into drawing the wrong inference about what they actually mean or intend to do. Some advertising works this way. In one famous case, ads for Listerine mouthwash reminded listeners that winter is a time when it is easy to get colds. Another ad explained that Carnation Instant Breakfast provides as much mineral nutrition as strips of bacon. Filling in the gap, listeners are implicitly expected to infer that Listerine prevents colds (which it doesn't) and both Carnation Instant Breakfast and bacon have healthy mineral nutrition (which they don't). Exploiting common-ground rules to establish false claims has not been explored much by the psycholinguists.

8
Signaling and Reputation

IN 1974 MICHAEL SPENCE published one of the most influential dissertations ever in economics, which eventually (in 2001) earned him a Nobel Prize (shared with George Akerlof and Joseph Stiglitz, for related work in information economics). Inspired by ideas in international relations, Spence formalized and demonstrated the usefulness of the idea of "signaling." Signals are actions players take that convey unobservable information about their "types" to other people, who can observe the signals but not the types. Spence had in mind actions that were too costly for people or countries to take unless they were serious, or *might be* serious. His central example was investing in education to signal intellectual ability (or something). In labor markets, for example, education may signal intelligence (or obedience) to prospective employers. A cheap warranty signals to consumers that a product isn't likely to break down too often. Giving flowers and small gifts (without the reminder of a splashy holiday such as Valentine's Day) means you think about her (or him). In organized crime gangs, a person's willingness to kill or maim a relative who broke the rules signals that they are more loyal to the gang than to their own kin.

Signals are credible if they satisfy two properties. First, signals must be *affordable* by certain types of people, for whom the cost of the signal is less than the benefits that result if the "receiver" decodes the signal. Education pays because it gets students jobs; warranties get firms eager customers; flowers are well worth it if she knows what they mean; and loyalty tests give killers security in their gangs. And, secondly, signals must be *too expensive* for players of the wrong type to afford. Students who really hate schoolwork cannot stand the pressure and boredom of a very academic university. People selling fake Rolexes from briefcases on the streets of New

York cannot afford to offer warranties. Regular gifts of gorgeous flowers are too high a price for a casual relationship. Some gangsters quit rather than kill their brothers.

Combining the two affordability properties, a logical observer can conclude that, if one person buys the signal and another does not, then the two people are of different types (i.e., there is a "separating" equilibrium). This gives the assurance an employer, customer, girlfriend, or gang leader might need to take action. Returning full circle to the signal buyers, the actions by observers who decode the signal fulfill the prophecy made by the signal buyers, which makes the signal worth buying in the first place. A psychologist to whom I once explained signaling equilibrium said, ''But it's . . . circular!" Indeed it is. Everything fits together: the sender *should* take the action the receiver expects (that's why she expects it); the receiver's anticipated response is what makes the sender's action worthwhile. By "circular," we usually mean nothing could be ruled out. But the mutual consistency requirement actually rules out a lot of possible behaviors (as the examples below illustrate). Put differently, without the "circular" equilibrium requirement then *some* player is making a mistake—such mistakes surely *do* happen (we'll see some in the experimental data), so it makes sense to think of equilibrium as an idealized steady state reached by learning.[1]

Spence's work used tools developed by John Harsanyi (1967–68). Harsanyi proposed that a simple way to include asymmetries in information in game theory was to assume that one player observed a move by "nature," and all players knew the possible moves but only the privately informed player knew what nature had done. ("Nature" is just a metaphor for an exogenous force or player who is not an active player in the game, and is the source of the asymmetric information.) Harsanyi's idea gave theorists a powerful tool to analyze the influence of private information and is probably the most important development in noncooperative game theory after the seminal contributions of von Neumann and Morgenstern and Nash. The types-based approach also has the right degree of difficulty for challenging theorists—not too easy, not too hard.

Introducing types also provides a generic way to try to explain apparently irrational actions. Why would a gangster kill his brother? Perhaps he is trying to convey information about himself. Why would autoworkers who would genuinely prefer to work stand on a picket line, shivering, to strike

[1] In technical terms, signaling models are those in which the privately informed player moves first ("buying" a signal). When the uninformed player moves first, the analogous model is called a "screening" model (e.g., insurance firms offering a menu of contracts to get consumers to self-sort according to their riskiness, to limit adverse selection). I will generally ignore these distinctions in model structure and use the term "signaling" broadly.

against a firm? Perhaps the workers are trying to convey how much they feel they deserve a higher wage.

Applications of signaling models exploded in social science in the twenty-five years since Spence's book appeared (e.g., see Gibbons, 1992). Signaling is used in economics to explain activities that might seem inefficient, such as advertising (which certifies product quality, even if it does not convey information), strikes (which convey each side's seriousness or patience), education, and more. Applications in political science include a Congressional committee signaling a bill's appeal to the entire House. Signaling models have also begun to influence biology and anthropology. The theory of "costly signaling" in biology (e.g., Zahavi, 1975) explains apparently maladaptive (fitness-reducing) activities or mutations by asking what type of information they convey. Male animals often develop physical features that seem to be handicaps, such as a peacock's lush tail-feathers, or an elk's large, heavy antlers. A handicap shows females that a particular male is *so* strong or safe from predators that it can afford to bear an extra burden—the peacock's showy feathers may attract predators, the elk's heavy antlers slow it down. The equivalent for human animals is flashy Hong Kong gangsters lighting cigarettes with $100 bills in John Woo movies (showing they literally have money to burn).

Despite the ubiquity of signaling models in theoretical social science, there are relatively few direct empirical studies of them. Some studies relate predictions of models to evidence from labor strikes (e.g., Kennan and Wilson, 1990), measures of advertising, and product quality for cars (Thomas and Weigelt, 1998). There are also a small number of experimental studies. This chapter describes those experiments and what is learned from them (see Van Winden, 1998, for an earlier review).

The signaling games I shall discuss are the most theoretically difficult games described in this book. The results of these studies are therefore important for judging the descriptive accuracy of game theory when it demands the most cognition or delicate equilibration. If subjects do not behave as theory predicts in simple laboratory games, with ample feedback, one must surely question whether actual players behave that way in much more complicated naturally occurring games.

In fact, most experiments show that players do not fully reason their way toward signaling equilibria as theory conventionally assumes. However, most studies indicate *some* strategic reasoning like that assumed in the models. Players seem to realize that the signals they choose convey information, and receivers make inferences from signals. And when repeatedly playing the same game, subjects usually adapt toward equilibria.

Signaling games illustrate an important methodological point about the interplay of theory and data. As theorists began using these games to explain phenomena such as educational investment, warranties, and strikes, they quickly realized that games often had multiple equilibria. Some equilibria seemed patently implausible, although they were mathematically consistent with established equilibrium concepts (such as even sequential equilibrium). Looking back, it is easy to see that concepts such as Nash equilibrium were just too mathematically weak to pick out the likely equilibria. Refinements of the established concepts were needed to codify what "implausible" meant.

The search for refinements obsessed many game theorists during the 1980s. The papers proposing these ideas are filled with discussions of intuition and plausibility, but no data. It is strange for mathematically gifted theorists to spend years debating with each other which kinds of behavior are most plausible in different games, without occasionally putting people in those games and defining "plausible" as what most people do. The experiments reported in this chapter do just that.

Most tests have yielded pessimistic results about the ability of strong refinements to predict which equilibria players converge to. One important set of experiments (Brandts and Holt, 1993, 1994) even established that players can be systematically led to the "wrong" equilibrium (the *less* refined one). However, theories of learning provide an empirically based way to restrict out-of-equilibrium beliefs, substituting empirical arguments for the logic of mathematical refinement.

8.1 Simple Signaling Games and Adaptive Dynamics

The first thorough experimental investigations of sender–receiver games were conducted by Brandts and Holt (1992) and Banks, Camerer, and Porter (1994). Brandts and Holt (1992) were interested in the Cho–Kreps "intuitive criterion." This can be illustrated by their game 1, shown in Table 8.1.

Think of the payoffs as reflecting labor market returns to education, in the following way: A worker draws her type, either L (low intelligence) or H (high intelligence). There is a common prior probability that her type is L with probability 1/3, and H with probability 2/3. (That means everyone

Table 8.1. *Payoffs in Brandts and Holt's sender–receiver game 1*

	Action after message S(kip)		Action after message I(nvest)	
	C^S	D^I	C^I	D^S
Type L(ow)	140,75	60,125	100,75	20,125
Type H(igh)	120,25	20,75	140,125	60,75

Source: Brandts and Holt (1992).

knows the distribution of Ls and Hs but also knows that the worker knows her own type.) After observing her type, the worker can either skip education (S) or invest in education (I). A prospective employer does not observe the worker's type (L or H), but *does* observe whether she invested in education (S or I). Then the employer assigns the worker to either a dull (D) job that requires little skill, or a challenging (C) job that requires skill.

The employer's payoffs are simple. She wants to assign L employees to the D job and H employees to the C job. Those assignments produce a payoff for employers of 125; the opposite assignments are mismatches and produce a payoff of 75.

The worker's payoffs create a different incentive. Workers get payoffs from both wages and "psychic income." Both types earn 100 from the challenging job C and only 20 from the dull job D. In addition, L types get an added payoff of 40 if they skip college (S) and H types get an added payoff of 40 if they invest in education and go to college (I). (In addition, to match the payoffs in Brandts and Holt's game, suppose the H types who skip college get an extra payoff of 20 from the challenging job C, perhaps reflecting on-the-job learning from the challenging job which is a substitute for what they would have learned in school.) Adding up these payoffs gives those in Table 8.1.

There is a conflict of interest between senders (workers) and receivers (employers) in this game. Employers would like to know the workers' types so they can assign the Ls to job D and the Hs to job C. Since the probability is 2/3 that the worker's type is H, if the employer doesn't learn anything about the worker's type from her choice of S or I, she will assign the worker to job C.[2] Both types of workers prefer job C. Therefore, since the employer will assign a worker to job C unless she becomes fairly convinced (more than 0.5 probability) that the worker is type L, the type L workers have an incentive to "pool" with the type H workers and mimic whatever they do, so they can get the lucrative C job assignment.

There are two equilibria in which both types send the same message (called "pooling equilibria"). In one sequential equilibrium, both types choose S and employers respond with C. In addition, a Bayesian–Nash equilibrium must specify how employers respond to unexpected messages. In this sequential equilibrium, to keep H types from breaking out of the pool and choosing I, it must be that employers think that an unexpected choice of I is evidence that the worker is type L, and assigns a worker who sends message I to the dull job D. Since the payoffs to D after choosing I are lower for both types than the payoffs from C after choosing S, neither type will deviate and so the pattern of choosing S and getting job C is an

[2] Assigning to job C gives the employer an expected payoff of (2/3)125 + (1/3)75, or 108.3. Assigning to job D gives an expected payoff of (2/3)75 + (1/3)125, or 91.7. Essentially, since H is more likely than L, it pays for the employer to take a chance and assign everyone to the more challenging job C.

equilibrium. In Table 8.1 the equilibrium is denoted by superscripting the equilibrium employer choices after each message by S; thus, if the worker chooses S, the employer's equilibrium action is C^S, and if the worker deviates and chooses I, the employer's equilibrium action is D^S.

There is something fishy about this equilibrium pattern. Imagine a country in which nobody gets advanced education for decades. Then one person *does* choose to get educated—travelling abroad to college and then returning. Everybody knows that L types don't like education—they are bored and frustrated by school—but H types love it (they earn a higher psychic payoff, reflected by the bonus of 40 in the game). Then why would an employer think that the person who went abroad to college is an L type? The L types are earning their highest possible payoff of 140 by choosing S and getting job C, so they cannot possibly do better by deviating (going abroad to college) and choosing I. The H types, on the other hand, earn a payoff of 120 in the equilibrium but by choosing I they can conceivably earn 140. The Cho–Kreps (1987) "intuitive criterion" (also called "equilibrium dominance") says that when one type cannot possibly improve her payoff by deviating from an equilibrium, and another type might improve her payoff, then the only sensible belief about which type deviated is to assume that the deviator was the type who might benefit. (You should assume that the nonconformist who broke the rules and went to college is an H type, who might have liked it, rather than an L type who wouldn't.) In algebraic terms, the employer should have the belief $P(H \mid I) = 1$. This belief leads her to place a worker who went abroad to college, choosing I, in the job C. Anticipating this, H-type workers will deviate from the S equilibrium by choosing I, thus "breaking" the equilibrium. Because L-type workers will get placed in the low-payoff D job if they are the only ones who choose S, they too will be forced to invest in college by choosing I.

Thus, there is a second sequential equilibrium in which players pool by choosing I, employers assign everybody who chooses I to job C, and a worker who deviates by choosing S is assigned job D. This equilibrium *does* satisfy the intuitive criterion because the H types cannot possibly earn a higher payoff by deviating (by choosing I and getting job C they earn their dream payoff of 140), but the L types might earn a higher payoff. Assigning deviators who choose S to the bad job D is consistent with employers believing that anybody who deviates is an L type; and, in fact, L types are the only ones with an incentive to deviate.

Refinement concepts such as the intuitive criterion were developed to reflect intuitions about what sort of receiver beliefs were sensible after unexpected behavior. We have no idea if these intuitions are right, so the stage is set for an informative experiment.

The results from game 1 are summarized in Table 8.2 in four-period blocks, along with equilibrium predictions. In the top panel, the relative

Table 8.2. Results in Brandts and Holt's sender–receiver game 1

	Message given type		Action given message		Equilibrium predictions	
Periods	$I \mid H$	$I \mid L$	$C \mid I$	$D \mid S$	Intuitive	Sequential (unintuitive)
1–4	100	25	100	74	100	0
5–8	100	58	100	100	100	0
9–12	100	75	98	60	100	0
With suggested message S, suggested actions $C \mid S$, $D \mid I$						
1–4	50	13	60	46	100	0
5–8	75	33	33	67	100	0

Source: Brandts and Holt (1992).

frequencies of I messages and intuitive equilibrium actions tend to support the intuitive equilibrium. The strongest evidence against it is a low frequency of I choices by L types in the early periods 1–4, but they quickly learn to pool with the Hs (who *always* choose I) by choosing I most of the time in later trials.

The bottom panel of Table 8.2 shows results from a treatment in which the experimenters read an announcement in which they "suggested" that players choose message S, and respond to S with C and to I with D. This treatment was intended to test an interpretation of game theory in which equilibria result from self-enforcing "assignments" by an outside authority (or perhaps history; see Chapter 7). The suggestion of playing the unintuitive equilibrium stress-tests the fragility of the intuitive equilibrium. Conformity with the intuitive equilibrium *is* shaken by the announcements, but I choices do rise over time. The substantial announcement effect hints that the intuitive equilibrium could be vulnerable to social forces that nudge people away from it.

In Banks, Camerer, and Porter (1994) we had an ambitious goal. Because expanding the concept of Nash equilibrium to "Bayesian–Nash equilibrium"—essentially, insisting that players have *some* belief about other players' types which justifies their moves at each information set—created many equilibria that seemed nonsensical, theorists proposed many refinements to specify precisely what kinds of beliefs were nonsensical. Banks, Porter, and I thought the refinement question begged for an empirical answer. The theorists' intuitions were mathematically clear, but based on no data, and examples used in the literature sprang ready-made into experimental designs. Banks cooked up a set of seven games, which each had two (pooling) equilibria and which tested levels of refinements—constraints on beliefs—that were mathematically nested. The games are shown in Table 8.3.

Table 8.3. *Signaling games and equilibria in Banks et al.*

Types	Message m_1			Message m_2			Message m_3		
Game 1: unique Nash pooling (m_1)									
	a_1^N	a_2	a_3	a_1	a_2	a_3^N	a_1^N	a_2	a_3
t_1	2,1	2,0	0,2	3,1	1,0	0,0	1,2	1,1	3,0
t_2	1,2	2,0	2,1	2,1	0,0	0,6	0,2	3,1	1,1
Game 2: Nash (m_1) vs. sequential (m_3)									
	a_1	$a_2^{N,S}$	a_3	a_1	$a_2^{N,S}$	a_3	a_1^S	a_2^N	a_3
t_1	1,2	2,2	0,3	1,2	1,1	2,1	3,1	0,0	2,1
t_2	2,2	1,4	3,2	2,2	0,4	3,1	2,2	0,0	2,1
Game 3: sequential (m_2) vs. intuitive (m_1)									
	a_1^S	a_2^I	a_3	a_1^I	a_2	a_3^S	$a_1^{S,I}$	a_2	a_3
t_1	0,3	2,2	2,1	1,2	2,1	3,0	1,6	4,1	2,0
t_2	1,0	3,2	2,1	0,1	3,1	2,6	0,0	4,1	0,6
Game 4: intuitive (m_2) vs. divine (m_3)									
	a_1	a_2	$a_3^{I,D}$	a_1	a_2^D	a_3^I	a_1^D	a_2^I	a_3
t_1	4,0	0,3	0,4	2,0	0,3	3,2	2,3	1,0	1,2
t_2	3,4	3,3	1,0	0,3	0,0	2,2	4,3	0,4	3,0
Game 5: divine (m_3) vs. universally divine (m_3)									
	a_1^U	a_2^D	a_3	a_1^D	a_2	a_3^U	a_1^D	a_2^U	a_3
t_1	4,1	2,4	1,5	1,3	3,1	4,2	3,3	2,0	1,4
t_2	5,6	2,5	2,2	1,3	1,4	3,3	3,4	1,5	0,1
Game 6: universal divinity (m_3) vs. never-a-weak-best-response (NWBR) (m_2)									
	a_1^U	a_2^N	a_3	a_1^N	a_2	a_3^U	a_1	a_2^U	a_3^N
t_1	2,2	0,3	5,2	1,5	5,3	1,0	2,1	3,3	0,4
t_2	0,2	2,0	5,1	4,0	4,1	0,2	1,4	3,3	2,1
Game 7: NWBR (m_1) vs. stable (m_2)									
	a_1	$a_2^{N,S}$	a_3	a_1^N	a_2^S	a_3	a_1	$a_2^{N,S}$	a_3
t_1	1,6	2,5	2,0	0,5	3,4	1,2	4,2	1,1	0,3
t_2	2,0	2,5	0,6	1,2	3,4	0,5	1,2	0,4	3,3

Source: Banks, Camerer, and Porter (1994).

I will mention only a few basic refinements here. (The subtler ones require some serious hair-splitting.) Motivated readers can look at our paper and the original theory papers for more depth.

I have already discussed the intuitive criterion, which ties the plausibility of a receiver's beliefs about which type is likely to deviate to the possibility that the deviator's out-of-equilibrium payoff is above the equilibrium payoff. But the intuitive criterion does not go far enough. Two types may both conceivably do better after deviating, but one type's deviation seems more likely, because the set of receiver responses that make a deviation optimal is larger for that type.

This property is called "divinity"; game 4 illustrates it. In game 4 there is an intuitive (I) equilibrium in which both types send message m_2. Receivers respond with action a_3, and respond to a deviation m_1 with a_3 (because they believe the chance that any such deviation came from a type 1, denoted $\mu(t_1 \mid m_1)$, is above 0.75). This belief satisfies the intuitive criterion because both types *might* benefit from deviating to m_1.[3] Because both types could benefit from deviating, the intuitive criterion is silent about which type is more likely to deviate. But a little calculation shows that the set of responses by receivers that make a deviation profitable (including mixtures) is strictly larger for type 2 than for type 1.[4] Intuitively, imagine that types 1 and 2 are identical twins who have the same guess about what receivers will do in response to a deviation to m_1. Whenever type 1 deviates, type 2 does too. But if "their" shared belief puts less than 0.75 weight on an action a_1, but more than 0.50 on a_1 and a_2 together, then type 2 will want to deviate while type 1 will not want to deviate. In this sense, a type 2 deviation is more likely. Divinity requires that when the sets of possible action responses that support deviation are larger for one type than another, then the receiver's beliefs about which type deviated should put more weight (relative to the prior) on the type whose deviation-supporting belief set is larger. Since the intuitive equilibrium belief $\mu(t_1 \mid m_1) > 0.75$ puts too much weight on the wrong type (t_2 is more likely to deviate than t_1), it does not satisfy divinity.

Put more simply, the intuitive criterion just divides the set of types into those who would never deviate and those who might. Divinity divides those who might deviate into types who deviate "more often" than others. Divinity therefore requires more reasoning (or a learning process that leads you to that conclusion). A step farther—a step too far, in my view—is "universal divinity." Universal divinity requires that, if the set of receiver actions that make a deviation profitable is higher for one type than another, receivers

[3] Type 1 gets 3 in equilibrium and might get 4 after choosing m_1; type 2 gets 2 in equilibrium and might get 3 after choosing m_1.

[4] Type 1 benefits from deviating only if the receiver chooses a mixture that at least places 0.75 probability on action a_1. Type 2, in contrast, benefits from deviating for a wider set of mixtures—namely, any mixture with 0.5 probability or more on the combination of a_1 and a_2.

must believe *for sure* that a deviation came from the more likely type. (Game 5 illustrates an equilibrium that is divine but not universally divine.)

The next two refinements, "never-a-weak-best-response" (NWBR) and stability, are *extremely* subtle.[5] Divinity is as far as intuitive strategic reasoning is likely to go because going farther requires receivers to have complete faith in their ability to figure out the senders' sense of how likely it is that a deviation is profitable, which in turn affects the propensity of different types of senders to deviate. We know from experiments on dominance-solvable games (Chapter 5) that people seem instinctively to do just one or two steps of strategic thinking about others. It is therefore unlikely that senders will reason as thoughtfully about what receivers will think about—what their own signal choices imply as subtle refinements predict. Nonetheless, these refinements were taken very seriously for a while (and still are used in many applications), so seeing what happens in the lab is important. Table 8.4 summarizes the relative frequencies of more refined, less refined, and non-Nash outcomes in two five-period blocks, for all games.

Look first at the frequencies with which message-action pairs match the more and less refined equilibrium predictions in periods 1–5 and 6–10. The more refined message–action pair is played more than half the time in games 1–3, and there is substantial increase in that frequency between periods 1–5 and 6–10. The conformity with the more refined equilibrium drops sharply in game 4. At one point we thought, alliteratively, that subjects acted as if they used simple refinements but not subtler ones ("refine till divine"). That rhythmic conclusion is too simplistic. The reason is that the coarser refinements of the lower-numbered games are also satisfied by both refinements in higher-numbered games (but not vice versa). For example, the intuitive equilibrium is reached 68 percent of the time in later periods of game 3. But in games 4–7, both equilibria are intuitive, yet they are played only a total of 46–63 percent of the time; hence, there is less overall intuitive play in higher-numbered games than in game 3. With that small qualification, there is certainly not strong support for very

[5] The idea behind the NWBR criterion is that even when universal divinity fails to apply, because the set of receiver actions making a deviation profitable is not strictly larger for one type than another, it is sometimes the case that one type will have a receiver action response that makes a deviation just as profitable as sticking to the equilibrium message (i.e., defecting is a "weak best response") while the other type prefers the equilibrium payoff for that same action response. Note that this definition nests universal divinity because, if the two action response sets that elicit profitable deviation are strictly nested, then there will be at least one response that makes deviation by the larger-set (more likely) type a weak best response while the other type will prefer the equilibrium payoff.

The last refinement is stability. Banks, Camerer, and Porter wrote (1994, p. 11) that, "Universal divinity and NWBR were initially attempts to characterize the restrictiveness of the concept of stable equilibrium . . . The stable equilibrium concept requires that every possible 'tremble' of strategies have an equilibrium 'close to' the candidate equilibrium." The appeal of stability was that the existence of a stable equilibrium can be guaranteed, yet it is the finest-grained refinement. It is the closest theorists came to finding a Holy Grail theory that guarantees the existence of a kind of equilibrium, with as few of those equilibria as possible.

Table 8.4. Results in signaling games (percent)

	More refined equilibrium				Less refined equilibrium			
	Message–action pairs in periods				Message–action pairs in periods			
Game	1–5	6–10	Messages only	Actions only	1–5	6–10	Messages only	Actions only
	Unique Nash							
1	56	76	86	84	—	—	—	—
	Sequential				Nash			
2	61	71	72	87	13	24	28	22
	Intuitive				Sequential			
3	53	68	72	91	13	4	20	9
	Divine				Intuitive			
4	28	38	42	47	16	8	15	18
	Universally divine				Divine			
5	31	27	39	44	36	36	42	75
	NWBR				Universally divine			
6	30	15	53	17	30	33	41	72
	Stable				NWBR			
7	59	56	78	64	13	7	7	23

Source: Banks, Camerer, and Porter (1994).

subtle refinements beyond the intuitive criterion. When one splits hairs by distinguishing divine from intuitive, universally divine from divine, and so forth, the tendency to play the more refined equilibrium falls, the tendency for more refined play to increase over time falls, and the fraction of non-Nash play rises. Subjects do not act as if they appreciate subtle refinements.

When the frequency of non-Nash message–action pairs is high, it is useful to ask whether it is the senders or receivers who make more non-Nash choices. Senders may choose divine messages in game 4, for example, but receivers respond to the divine message with an intuitive action (this would be scored as a non-Nash outcome). Columns 4–5 and 8–9 of Table 8.4 enable us to tell whether this is common by reporting the frequency of equilibrium messages of each type and actions of each type separately.[6] More refined

[6] Note that although the games are constructed so that the more and less refined messages are never the same (so their percentages can never add to more than 100), more and less refined action responses to messages sometimes overlap, so their percentages can add to more than 100.

Table 8.5. *Brandts and Holt's games 3 and 5*

Types	Message I			Message S		
Game 3	C^I	D^S	E	C^S	D^I	E
A	45,30	15,0	30,15	30,90	0,15	45,15
B	30,30	0,45	30,15	45,0	15,30	30,15
Game 5	C^I	D^S	E	C^S	D^I	E
A	45,30	0,0	0,15	30,90	30,15	60,60
B	30,30	30,45	30,0	45,0	0,30	0,15

Source: Brandts and Holt (1993).

message and action choices are quite common in games 1–3 (70–90 percent) but these frequencies drop sharply in games 4–7.

Another important finding (which is not apparent in the table) is that senders do not really pool because type 1s and 2s tend to choose different messages in games 2–6. We tried to figure out whether subjects were using different decision rules, such as maximin or the principle of sufficient reason (level-1 reasoning in Chapter 5 jargon), but no single decision rule could account for most deviations. However, it is notable that hardly any receivers rarely violated weak dominance, and senders rarely chose messages that could be eliminated by one round of iterated dominance. Thus, there is strong conformity with a simple principle of strategic thinking—deletion of another player's dominated strategies and strong conformity to Nash equilibrium in games 1–3.

The way learning dynamics can lead to unrefined equilibria can be illustrated by two games taken from Brandts and Holt (1993), extending their earlier work. Table 8.5 shows their games 3 and 5.

In game 3, types are equally likely ($P(A) = P(B) = 0.5$) and there are two equilibria. In the sequential (S) equilibrium, both types of senders (A and B) send message S (denoted by superscripting by S). Since both types choose S, receivers infer or learn this, Bayesian-update and infer $P(A \mid S) = P(A) = 0.5$ and $P(B \mid S) = P(B) = 0.5$. Then they should choose C, which gives payoffs of 90 or 0 and hence maximizes expected utility given the Bayesian posterior beliefs. Note that, in this equilibrium, type As earn 30 and type Bs earn 45. What prevents the type As from defecting to I and earning 45 if their message is met with a response of C? The sequential equilibrium coheres only if receivers choose D in response to message I.[7] But then why would

[7] Note that E in response to I is strictly dominated and should never be chosen; in the experiments, it rarely is.

receivers choose D in response to I? Receivers must believe that D choices are more likely to be made by B types (more specifically, $P(I \mid B) > 2/3$ to justify a choice of D by receivers). The intuitive criterion insists that belief is wrong. Type Bs earn 45 in equilibrium (from choosing S and getting response C) and could not possibly benefit from switching to I. Type As earn 30 in equilibrium and could conceivably benefit if they choose I and meet response C, earning 45. Hence, the off-path belief $P(I \mid B) > 2/3$ is not intuitive because it shifts belief toward B types, who are least likely to benefit from having picked I.

The argument underlying the intuitive criterion is purely theoretical. It deduces from sheer payoffs the implausibility of type Bs having switched from S to I. From a learning point of view, this is sensible only if subjects have been choosing S for a long time and any history of I choices is forgotten or dismissed. But learning, by definition, allows the possibility of a pre-equilibrium convergence process that leaves an empirical trace of previous I choices. What if, during the learning process, most players who chose I happened to be type Bs? Then the intuitive criterion competes with an empirical construction of off-path beliefs, recalled from the equilibration phase (before such moves *were* out of equilibrium). It is hard to think of a compelling empirical reason why players should ignore, in the medium run, what they previously observed in favor of a purely deductive argument that flagrantly ignores history.

In game 3 there is another equilibrium in which both types choose I and are met with the response C, yielding them 45 and 30. Since type Bs could conceivably earn more (45) by choosing S instead, the equilibrium sticks only if defections to S are met with responses of D. Note that a D response to S is justified by the belief that S defections are more likely to have come from B types (i.e., D is optimal for receivers if $P(B \mid S) > 5/7$). This inference *does* satisfy the intuitive criterion because, indeed, B types might benefit by defecting from I to S whereas A types would never benefit. Hence, the equilibrium in which both types choose I does satisfy the intuitive criterion.

From an equilibrium point of view, game 5 is identical to game 3. In the game 5 sequential equilibrium, both types pick S and are met with response C, earning 30 and 45 respectively. The A types are prevented from defecting to I only if a defection is met with response D, which is justified if receivers think a defection probably came from a B type ($P(B \mid I) > 2/3$). But in the S equilibrium, B types never do better defecting whereas A types might; so the presumption that defections were probably from Bs is unintuitive.

And, as in game 3, in game 5 there is an intuitive equilibrium in which both types choose I and are met with response C. A defection to

S makes receivers sensibly think the defectors are B's who might do better ($P(B \mid S) > 5/7$), so they choose response D, which in turn keeps both types from defecting.

Game 5 nicely illustrates how a plausible convergence path might leave "footprints" of empirical history that contradict the intuitive criterion. In the account due to Brandts and Holt (the BH dynamic), both types of players start out with (roughly) diffuse priors about the behavior of others. In game 5, suppose senders have diffuse priors about what receivers will do. Type A senders will compute expected payoffs of $(45 + 0 + 0)/3 = 15$ and $(30 + 30 + 60)/3 = 40$ for strategies I and S, and are more likely to choose S. Type B senders compute expected payoffs of $(30 + 30 + 30)/3 = 30$ and $(45 + 0 + 0)/3 = 15$, and are more likely to choose I initially. Thus, if senders have sufficiently diffuse priors they are likely to separate initially—As choose S and Bs choose I.

This first step illustrates the way in which a sensible start to the convergence process can yield empirically based off-path beliefs that are unintuitive. Recall that in the sequential equilibrium, both types pick S. This unintuitive equilibrium is held together by the odd belief that B types who would never want to revert to I, once they end up getting 45 by picking S, are more likely to have picked I. But in fact, the B types pick I in the first place. They begin doing so because the diffuse prior does not concentrate their belief on the possibility of getting 45 from S, which later becomes an equilibrium. Groping around, they begin doing something they later regret. But their early groping leaves an empirical trace that can sustain the belief that any player who picks I might be a B type.

Suppose receivers, meanwhile, have diffuse priors over the types of senders who are likely to have chosen each message. Receivers who observe I compute expected payoffs of 30, 22.5, and 7.5 for strategies C–E and, hence, choose C most often. Receivers who observe S compute expected payoffs of 45, 22.5, and 37.5 and choose C most often. Hence, in this equilibration phase A's choices of S are mostly met with C (or E) and B's choices of I are mostly met with C or D.

In the second phase, equilibration occurs as players build up empirical experience by observing what others do, and move in the direction of ex post best responses. Two things happen, probably more or less simultaneously. Receivers learn that, when they observe message S and choose C, they earn 90 because S choices come from A types. So their C choices are reinforced and become more frequent. When receivers observe message I, they earn 30 from C and 45 from D, and gradually switch to D. Meanwhile, type A senders earn 30 (or 60) from choosing S, and learn that, if they had picked I, they would have earned 45 or 0. As receivers adapt, picking D more frequently in response to I, the A types realize that a defection to I would yield 0, which

Table 8.6. Results in games 3 and 5: Brandts–Holt and Partow–Schotter (percent)

		Message given type		Action given message		Equilibrium predictions	
Game	Periods	$I \mid A$	$I \mid B$	$C \mid I$	$D \mid S$	Intuitive	Sequential
3 (BH)	1–4	77	26	91	60	100	0
	5–8	82	8	93	62	100	0
	6–12	89	57	91	82	100	0
3 (PS)	1–6	78	10	62	27	100	0
		$S \mid A$	$S \mid B$	$C \mid S$	$D \mid I$		
5 (BH)	1–4	87	8	74	61	0	100
	5–8	89	49	79	76	0	100
	6–12	96	59	68	74	0	100
5 (PS)	1–6	87	39	68	51	0	100

Source: Brandts and Holt (1996); Partow and Schotter (1996).

keeps them from switching. On average, then, their S choices are (relatively) reinforced and they keep choosing S. Type B senders, meanwhile, earn 30 from picking I, but realize they might have earned 45 from picking S since action C is often chosen in response. And indeed, as senders learn that S messages are chosen by A types, they learn to choose C more often in response, which intensifies reinforcement of the unchosen strategy S for B types.

Gradually, then, As stick with S and Bs switch to S as well. Eventually a pooling equilibrium results in which both types pick S and receivers, having learned that an S choice is likely to come from either type, maximize expected payoff by choosing C most of the time. The equilibrium is sustained by the fact that a sender who learned from experience would respond to an unusual I choice by presuming—based on history—that a type B picked it, and would choose D in response, which keeps both types from picking I and sustains the choice of S.

Table 8.6 shows the time path of intuitive equilibrium messages for each type ($I \mid A$, $I \mid B$) and receiver actions. In game 3 senders start by initially separating by type, so that As mostly choose I and Bs rarely choose I. Anticipating this separation, or quickly learning it, receivers respond to I with C, and to S with D. That pattern of responses quickly teaches B types that choosing S yields only 15, whereas choosing I would have yielded 30, causing them to switch to I and pool with the type As. That is roughly what happens. By periods 9–12, type Bs choose I 57 percent of the time.

In game 5, the initial type-dependence is reversed, so that As strongly prefer S (which guarantees 30, and may pay 60) whereas Bs rarely choose

S because I guarantees a payoff of 30. Receivers again anticipate this separation or learn it quickly, and usually respond to S with C (which implies that type Bs would have earned 45) and to I with D (which implies that type As would have earned 0). Senders then learn these action responses. Since there is no incentive for type As to switch to I, and a stronger incentive for Bs to switch to S, there is a gradual shift toward pooling on S.

If the process fully converged, the receivers would end up responding C to S and D to the occasional I, because they would have remembered that the only types who chose I in the past were type Bs. However, once the equilibrium crystallizes, the only types with an incentive to deviate are type As. Thus, there is a sharp conflict between the force of history, which shaped beliefs about which types might deviate based on which types *did* "deviate," and the logic of the intuitive criterion, which asks which types would prefer to deviate, given the *current* equilibrium. The logic of the intuitive equilibrium is impeccable, but is no match in the short run for the force of historical observation.

Partow and Schotter (1996) noted that, in the adjustment dynamic posited by Brandts and Holt, players do not use information about other players' payoffs to sharpen their beliefs, or about what receivers will do or what types of senders might have chosen different messages. So they replicated the Brandts–Holt (BH) games 3 and 5 by telling players only their own payoffs (and fixing roles within each game). If the Brandts–Holt dynamic explains what is happening, these own-payoff games should look like the other games. Results are shown in Table 8.6. The type-dependence observed by Brandts and Holt is replicated in Partow and Schotter (PS). An important difference is that the convergence toward pooling on the intuitive equilibrium is not evident in game 3; only 10 percent of the type Bs choose I. This is driven by the fact that receivers respond to a choice of S by choosing D only 27 percent of the time, and C otherwise (which gives the type Bs their highest payoff, 45, rather than the lowest payoff of 0, so they have little incentive to switch from choosing S to choosing I).

The difference due to knowing others' payoffs suggests that the receivers are doing some strategic thinking. If they were simply keeping track of which types of senders chose I and S (which does not require them to know anything about the payoffs of the senders), they should choose D in response to S with similar frequency in BH periods 1–4 and PS periods 1–6. But even in those early periods receivers choose D much more often, which suggests that they are able to figure out, from studying the senders' payoffs, which types of senders are likely to be choosing S rather than I. They are being sophisticated rather than purely adaptive.

Anderson and I (2000) were curious whether the belief learning story of Brandts and Holt would track the equilibration path as well as other learning models described in Chapter 6, which have been applied to many other games. In experience-weighted attraction (EWA) learning, players are assumed to learn by updating a numerical measure of a strategy's attractiveness based on the payoffs they received, or the forgone payoff the strategy *would* have earned if it had been played, multiplied by an "imagination" weight δ. (Belief learning is a kind of EWA earning in which forgone payoffs are weighted as heavily as the payoffs players actually get, so it assumes "perfect imagination.") Extensive-form games present a challenge because players do not know what exactly would have happened if they had chosen a different message. However, they know the set of *possible* payoffs for an unchosen message. And if message I was chosen in the past, for example, then its historical payoff is known, so players could use this as a guess about what they would have gotten in the current round if they had chosen I. Anderson and I implemented EWA by using an agent-based model in which strategy attractions are updated separately for each type (for senders), and for each possible message (for receivers), and a proxy for forgone payoffs is used based on various models, such as the last received payoff.[8]

We replicated Brandts and Holt's experiments on games 3 and 5 for thirty-two periods, to have a longer span of data and see more learning. The thick lines in Figures 8.1a–8.1d show the results from game 5 in four-period blocks. The figures show the type-dependent relative frequencies of choosing m_1 given t_1 and choosing m_1 given t_2 (Figures 8.1a and 8.1b), and the message-dependent action frequencies (Figures 8.1c and 8.1d). Replicating Brandts and Holt's results, there is movement away from the intuitive message choices of m_1 (although the t_2 frequency time series turns sharply upward in the last four periods). Receiver action choices drift toward the unintuitive choice over time.

The figures also show the fit of three learning models which are fitted to the first twenty-four periods and used to forecast the last eight periods. The learning models are EWA, choice reinforcement (CR), and weighted fictitous-play belief learning (BB) (see Chapter 6 for details). The three models track the data about equally well, but there are small differences among them. Reinforcement does not pick up the drift over time in $m_1|t_2$ and $a_2|m_1$. Belief learning is too flat for $a_2|m_1$ and overshoots on $a_1|m_2$. EWA avoids these small mistakes and therefore fits and predicts a little better than the special case models, adjusting for its extra degrees of freedom in several ways.[9] The EWA imagination parameter δ is estimated to be 0.54 with

[8] We also realized that belief learning requires the strategy attraction for message choices by an "undrawn" type to be updated by the forgone payoff that type would have earned if they had chosen the same message that was chosen by the type that was actually drawn. Cross-type updating is equivalent to belief learning.

[9] The per period out-of-sample log likelihoods for EWA, reinforcement, and belief learning are −17.44, −19.20, and −20.40. (Note that the more complex model does not generally do better out of sample and, in fact, does worse if it is overfitting.) Out-of-sample hit rates are 81.6, 80.1, and 77.3. The corresponding figures for game 3 are −9.02, −9.83, and −10.28; and 91.0, 87.5, 91.0.

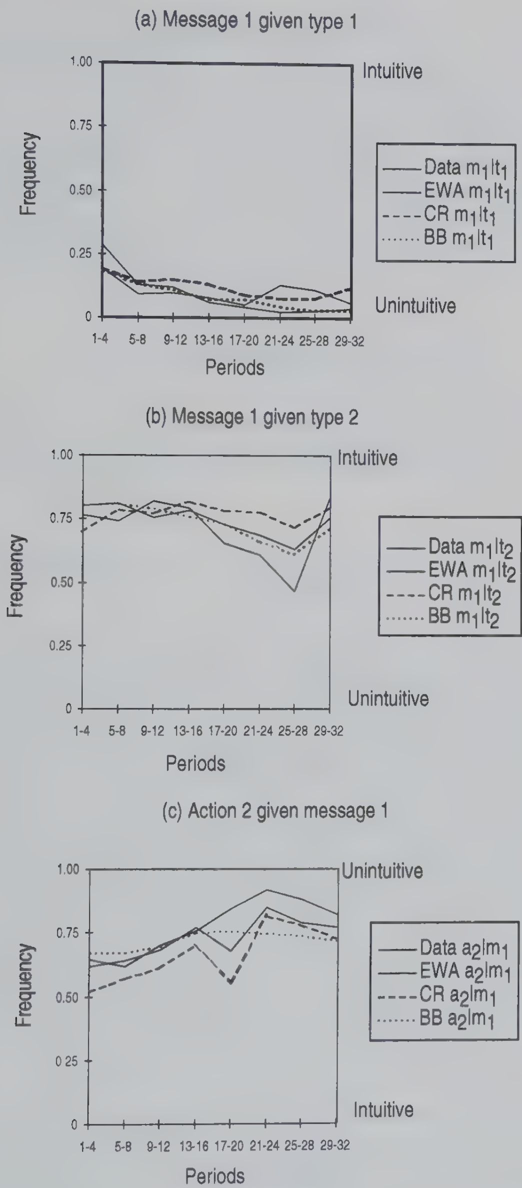


Figure 8.1. *Frequencies of messages and conditional actions (and intuitive/unintuitive equilibrium predictions) in Brandts and Holt's sender–receiver signaling game 5. Source: Anderson and Camerer (2000), p. 710, Figure 3. Reproduced with permission of Springer-Verlag.*

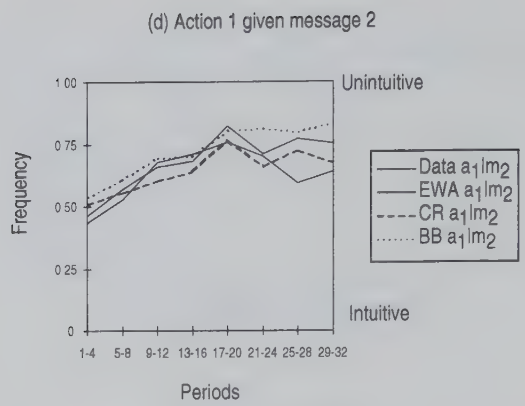


Figure 8.1 (continued)

a standard error of .05, so we can confidently reject the hypotheses that $\delta = 0$ (reinforcement) or $\delta = 1$ (belief learning).

Summary: Tests of basic signaling games yield mixed evidence about the adequacy of equilibrium refinements to predict what players will do. There is certainly much evidence of convergence toward Nash equilibria over time. However, refinements that are subtler than sequentiality do not predict reliably well in Banks, Camerer, and Porter (1994). The results very roughly match the cognitive difficulty underlying subtler refinements.[10] Brandts and Holt also showed that, if equilibration occurs through adaptive dynamics, a game can be constructed in which players are led toward the sequential equilibrium, which is not intuitive. Beliefs about likely deviations emerge from looking at history, rather than from logic applied at equilibrium to deduce which types are likely to deviate. Anderson and I showed how a precise learning model could explain this process.

[10] Define the difficulty of a concept by how hard it is to implement computationally. To execute the intuitive criterion, for example, a receiver must simply compare all possible payoffs from a deviation to a type's equilibrium payoff, for each type. If one type has no deviation payoffs that are larger, and another type has some, then the intuitive receiver decides the latter type must have deviated. Implementing this idea requires a receiver to classify types into "never will" or "might." Divinity is more difficult, since it requires the receiver either to construct sets of her own possible reactions that would make a deviation profitable, for each type, then test for embeddedness of the sets, or to enumerate all possible deviation responses on a checklist, and ask whether one type says "Yes, I'd deviate" every time another does and sometimes says "Yes" when the other says "No." This procedure is more complex computationally. These computational stories are metaphors for what subjects may be thinking, or rough yardsticks to calibrate difficulty—and hence likelihood of actual play. But they suggest a precise way to measure plausibility of refinements by linking them with cognitive complexity.

Table 8.7. *Payoffs in Potters and van Winden's lobbying signaling game*

	No signal		Costly signal	
Type (probability)	x_1	x_2	x_1	x_2
$t_1\ (1-p)$	$0,b_1$	$a_1,0$	$-c,b_1$	$a_1-c,0$
$t_2(p)$	$0,0$	a_2,b_2	$-c,0$	a_2-c,b_2

Source: Potters and van Winden (1996).

8.2 Specialized Signaling Games

This section describes games designed to model very specific economic environments: lobbying, corporate finance, ratchet effects in multiperiod production, and limit pricing.

8.2.1 Lobbying

Potters and van Winden (1996) studied a costly signaling game inspired by theories of lobbying in politics. Senders observe their types t_1 or t_2 (which occur with probabilities $1-p$ and p). They can then choose to send a signal which costs c. Receivers observe the signal (but not the sender's type) and choose actions x_1 and x_2. Payoffs are shown in Table 8.7 (assume $0 < c < a_1 < a_2$ and b_1, $b_2 > 0$).

The game models a lobbying group (the sender) that wants a politician (receiver) to take the action x_2. The politician wants to figure out whether the lobbying group has power or not. The politician prefers to take action x_i if the group is type i.

Define $\beta = pb_2/(1-p)b_1$, the relative expected payoff from choosing x_2 instead of x_1 if the receiver knows only the prior probabilities of types. Assume $\beta < 1$. There are two sequential equilibria. In one, senders think the costly signal will be ignored if it is sent and, since $\beta < 1$, the receiver will choose x_1. Although the senders are not happy (getting 0), it doesn't pay to signal if their signals are ignored, so there is nothing they can do. This equilibrium is intuitive (since both types could conceivably benefit from defecting) and divine, but not universally divine.[11] In the other equilibrium, type 2s always buy the costly signal. Type 1s will attempt to pool, since

[11] The set of possible receiver actions that justifies a defection by type i is $p(x_2|\text{signal}) > c/a_i$, which is a lower threshold for type 2 than for type 1. Divinity therefore requires that receivers believe a defection to "signal" is more likely to have come from a type 2. This is not enough to guarantee a choice of x_2 in response to a signal, however, unless the stronger requirement of universal divinity is imposed, which requires $p(t_2|\text{signal}) = 1$.

Table 8.8. Results in Potters and van Winden's lobbying signaling game (percent)

			Frequency of signals by t_1, t_2		Frequency of x_2 after no signal, signal	
Treatment	β	c/a_1	Actual	Prediction	Actual	Prediction
1	0.25	0.25	38, 76	25, 100	2, 5	0, 25
2 ($a_2 = 2c$)	0.75	0.25	46, 100	75, 100	3, 79	0, 25
2a ($a_2 = 6c$)	0.75	0.25	83, 93	75, 100	11, 54	0, 25
3	0.25	0.75	16, 85	25, 100	0, 53	0, 75
4	0.75	0.75	22, 83	25, 100	5, 80	0, 75
Averages across β		$c/a_1 = 25$			5, 46	0, 25
		$c/a_1 = .75$			2, 66	0, 75
Averages across c/a_1	$\beta = 0.25$		27, 81	25, 100		
	$\beta = 0.75$		50, 92	75, 100		

Source: Potters and van Winden (1996).

otherwise their type is revealed and they get 0. In equilibrium, receivers play x_1 after no signal and mix after receiving a signal, playing x_1 with probability $(a_1 - c)/a_1$ and x_2 with probability c/a_1. This mixture gives t_1 an expected payoff from signaling of 0, so type 1s mix by sending the signal with probability β.[12]

Potters and van Winden argue that for giving policy advice it is often sufficient to be confident that a change in a policy variable will affect behavior in the correct direction. To test the comparative static predictions, they vary parameters so that β and c/a_1 change, to see if the probability of type 1s signaling and receivers choosing x_2 after a signal respond to these changes as predicted. Table 8.8 summarizes the relative frequencies of signaling by the two types, and the choice of x_2 after no signal and a signal.

Data are pooled across all twenty periods, although there is some trend across trials (generally in the direction of equilibrium). Look at the signaling frequencies first. The universally divine equilibrium predicts that type 1s should signal β of the time, and type 2s should always signal. The type 2s *do* signal most of the time. Type 1s signal an average of 27 percent of the time when $\beta = 0.25$ and 50 percent of the time when $\beta = 0.75$, so the predictions

[12] Such a mixture leads to a posterior belief after observing a signal that makes receivers indifferent between x_1 and x_2. The posterior $p(t_2|\text{signal}) = 1 \cdot p/[1 \cdot p + \beta(1-p)]$ and the expected payoffs to x_1 and x_2 are therefore equal.

do change with β as predicted, although not as sharply.[13] As predicted, receivers rarely choose x_2 if there is no signal. After a signal, they choose x_2 46 percent and 66 percent of the time, respectively, when the predicted percentages were 25 percent and 75 percent. The results again respond to $c \mid a_1$ in the correct direction, but the change is too small.

Treatments 2 and 2a vary the payoff to type 2s as a function of the signal cost. Since type 2s should always signal (for these parameter values), changing their payoff a_2 should not matter, but it does, increasing t_2 signaling from 46 percent to 83 percent. This unpredicted response is supportive of a decision-theoretic approach (à la Brandts and Holt) in which one's own payoffs matter.

Potters and van Winden note that players respond to past history. Denote the past frequency of x_2 response to signals (across both types) in periods 1 to t_1 by $r(m)_{t-1}$. Type 1 senders learning by fictitious play should then signal in period t if $r(m)_{t-1}a_1 - c > 0$. When this expected payoff was positive (negative) they did signal 46 percent (28 percent) of the time. Fictitious play learning by receivers is even stronger: they chose x_2 when its expected payoff was positive (negative) 77 percent (37 percent) of the time.

Potters and van Winden (2000) extended their study to compare the behavior of students and professionals. There have been only a few serious studies comparing the behavior of college students (the typical subject pool) and professionals (see Ball and Cech, 1996). Most studies show that students and professionals behave similarly, which is extremely important for generalizability, but it is important to know more.

The payoffs in their game are shown in Table 8.9 (payoffs were four times higher for professionals). As in their earlier study, there is an equilibrium in which neither type signals, since the receiver believes a signal came from t_1, but this equilibrium does not pass universal divinity. There is also a universally divine equilibrium in which t_1 (t_2) signal with probability 0.25 (1.00), receivers never choose x_2 when there is no signal, and choose x_2 after a signal with probabilities 0.25 and 0.75 in the low- and high-cost conditions.

The results, shown in Table 8.10, are similar to their earlier findings and show little evidence that professionals are more inclined to equilibrium play. The degree of type separation (the difference in signaling rates by t_1 and t_2) is similar in the two groups, and is about half as wide as predicted. Learning occurs because players responded to history as in their earlier study. (Students were actually twice as responsive as professionals, so they learned better.)

[13] Note also that the equilibrium in which senders never signal is badly rejected, which is evidence for the refinement of universal divinity relative to the weaker refinements of divinity and intuitiveness.

Table 8.9. Payoffs in Potters and van Winden's lobbying signaling game

	No signal		Costly signal	
Type (probability)	x_1	x_2	x_1	x_2
Low-cost signals				
t_1 (2/3)	2,3	4,1	1.5,3	3.5,1
t_2 (1/3)	1,0	7,1	0.5,0	6.5,1
High-cost signals				
t_1 (2/3)	2,3	4,1	0.5,3	2.5,1
t_2 (1/3)	1,0	7,1	−0.5,0	5.5,1

Source: Potters and van Winden (2000).

Table 8.10. Results in Potters and van Winden's lobbying signaling game (percent)

		Frequency of signals by t_1, t_2		Frequency of x_2 after no signal, signal	
Subject pool	Signal cost	Actual	Prediction	Actual	Prediction
Students	Low	55, 69	25, 100	6, 27	0, 25
Professionals	Low	52, 83	25, 100	8, 27	0, 25
Students	High	34, 93	25, 100	4, 50	0, 75
Professionals	High	37, 71	25, 100	25, 65	0, 75
Students	Overall	46, 70	25, 100	5, 39	0, 50
Professionals	Overall	46, 87	25, 100	13, 40	0, 50

Source: Potters and van Winden (2000).

8.2.2 *Corporate Finance*

Distant investors and a company's managers have very different information about the company's prospects. Theorists in corporate finance have recognized this information gap and seized on it as an explanation for why firms engage in activities to convey good news to investors (and hide bad news) even if those activities don't otherwise benefit the company. For example, guaranteeing payment of dividends from current cash flows is a way for managers to signal that cash flows are healthy (cutting the dividend is a sign of very poor financial health), even though dividend payments make little sense otherwise. (They simply transfer cash from the stockholder's share of the cash in the firm's bank account to the stockholder's own bank account, incurring a tax debt in the process.)

An influential signaling model of corporate external financing of new projects is due to Myers and Majluf (1984). They asked what happens if firms have great investment opportunities but must raise capital by issuing shares ("diluting" the firm's value to current shareholders). There are inefficient separating equilibria in which firms with good projects will fear that investors will demand too much of the firm to supply capital (or, equivalently, will pay too little for new shares) and, hence, will not offer their best projects. Then only the most mediocre projects will be offered and investors are justified in demanding a large share of the firm because the firm is not so valuable after the good project is undertaken. Note that this equilibrium is inefficient because the best projects go unfinanced.

Cadsby, Frank, and Maksimovic (1990) did the first experiments to test this theory. In their design, firms are equally likely to be high (H) or low (L) value, and need capital I to finance a new project. H and L firms are worth A_{H} and A_{L} if they pass up the project, and an additional gain (net of I) of B_{H} and B_{L} if they do the project. Investors cannot determine whether the firm is H or L and can agree to put up I in financing in exchange for a fraction S of the firm's ex post value.

Cadsby et al. used a variety of parameter values, so that unique pooling, unique separating, and both types of equilibria are possible in different sessions.[14] An example will help illustrate. In session E, L firms are worth 50 without the project and 375 with it. H firms are worth 200 and 625. The investment is 300. There is a separating equilibrium in which $S^* = 0.80$. L firms offer shares, since selling 80 percent of the firm is worthwhile (because $(1 - 0.8)375 > 50$). H firms don't offer shares because accepting only 20 percent of shares worth 625 is worth less than the no-project firm value of 200. However, if investors demand only $S^* = 0.60$ of the firm, then Ls will offer because $0.4(375) > 50$. Hs also offer since $0.4(625) > 200$, and investors get an expected value of $0.6(0.5(0.25) + 0.5(375)) = 300$, equal to their investment. Therefore, there is a pooling equilibrium in which $S^* = 0.60$

[14] First note that $S_{\mathrm{L}} = I/V_{\mathrm{L}}$ is the fraction of shares shareholders would want if they knew that the firm was L. $S_{\mathrm{P}} = I/(0.5V - H + 0.5V_{\mathrm{L}})$, which is lower than S_{L}, is the fraction investors want in a pooling equilibrium. H firms will sell S shares if $S < (B_{\mathrm{H}} + I)/V_{\mathrm{H}}$ (H's will sell if $V_{\mathrm{H}} - B_{\mathrm{H}} = I$, the no-project firm value, is less than $(I - S)V_{\mathrm{H}}$, giving the inequality in the text), and investors will finance even L firms at a share of S_{L}. If $(I/V_{\mathrm{L}}) < (B_{\mathrm{H}} + I)/V_{\mathrm{H}}$ then there is a unique pooling equilibrium in which H and L both offer projects and the shares demanded, S^*, is S_{P}. If $(B_{\mathrm{H}} + I)/V_{\mathrm{H}} < S_{\mathrm{P}}$ then there is a unique separating equilibrium in which the shares that must be offered dilute the H firm too much, so only Ls offer and the share is $S^* = I/V_{\mathrm{L}}$. If both conditions are met, $I/(0.5V_{\mathrm{L}} + 0.5V_{\mathrm{H}}) < (B_{\mathrm{H}} + I)/V_{\mathrm{H}}) < I/V_{\mathrm{L}}$, then there is also a semi-separating equilibrium in which a fraction $P = I/B_{\mathrm{H}} - V_{\mathrm{L}}(I + B_H)/B_{\mathrm{H}}V_{\mathrm{H}}$ of H firms offer projects, all Ls offer projects, and investors demand shares which are worth I in expected value.

and both types offer projects. There is also a semi-separating equilibrium with $S^* = 0.68$, Ls always offer, and Hs offer with probability 0.36.

Cadsby et al. first ran two baseline sessions (A and B) in which firm types *were* known to investors, as a kind of comprehension check (and to see how competitive investors were during the share auction). All firms offered and shares went to within 1 percent of predicted levels within one period. Later sessions used two distinct groups to look for effects of experience across different parametric environments. One group participated in sessions C, E, and G and a different group participated in D, F, and H. Subjects were then mixed for three more sessions, I–K. Results are summarized in Table 8.11. Each panel shows an experimental session. Since the data are extremely regular, they can be summarized as the fractions of H and L firms offering projects in periods 1–5 and 6–10, and the mean share demanded by investors (in percentage terms) in the last two periods.

The results are very supportive of equilibrium predictions, and of pooling when there are multiple equilibria. When pooling is predicted uniquely (session C), all firms offer in all periods. When separating is predicted uniquely (sessions D, G, I), type H firms offer projects in periods 1–5, but they gradually learn the shares they will get are too low, and offer few projects in periods 6–10. In the sessions with multiple equilibria (E, F, H, J), there is reliable convergence to the pooling equilibrium. The pooling happens very rapidly, generally in the very first period. The willingness of investors to bid down to the lower pooled-equilibrium share right away assures H firms that it pays to offer projects, and the equilibrium quickly crystallizes. Of course, the result may be very sensitive to the institutional structure that determines investor shares.[15]

Cadsby, Frank, and Maksimovic (1998) extended their earlier experiments to include signals. Often signals are available to the firms that are socially inefficient, but which H firms can afford while L firms cannot. (Signals include costly activities such as paying dividends, share repurchases, "road shows" in which executives tell security analysts about all their firms' wonderful prospects, and so forth.) Do H firms buy these signals? In some cases there are only pooling equilibria in which either both types of firms purchase the signal, or neither type does. In these cases, the signaling equilibrium is Pareto dominated by the no-signaling equilibrium because (by definition) the signals are wasteful, but the no-signaling equilibria do not

[15] The auction is *descending* in the share S, and the pooled equilibrium share S^* is always below the other equilibrium shares. An auction in which shares were determined in a way that put less downward pressure on shares, such as a sealed-bid second-price auction or bilateral bargaining, might produce less evidence for pooling. Alternatively, investors and firms may pool because doing so is efficient and increases their collective take from the experiment. Maybe they are using efficiency as an equilibrium selection principle.

Table 8.11. *Results in project financing experiments of Cadsby et al.*

Session		Percentage of firms offering new projects in periods (1–5), (6–10) L	H	Mean share S (last two periods)
C	Results	100,100	100,100	28.3
	Prediction—pooling	100	100	30.0
D	Results	100,100	50,21	57.5
	Prediction—separating	100	0	75.0
G	Results	100,100	21,6	61.7
	Prediction—separating	100	0	62.5
I	Results	82,83	36,00	38.5
	Prediction—separating	100	0	40.0
E	Results	100,100	100,100	58.3
	Prediction—pooling	100	100	60.0
	separating	100	0	80.0
	semi-separating	100	36	68.0
F	Results	93,92	80,100	29.3
	Prediction—pooling	100	100	30.0
	separating	100	0	40.0
	semi-separating	100	20	36.0
H	Results	100,100	100,100	37.8
	Prediction—pooling	100	100	39.2
	separating	100	0	62.5
	semi-separating	100	9	57.1
J	Results	100,100	88,100	28.8
	Prediction—pooling	100	100	30.0
	separating	100	0	50.0
	semi-separating	100	35	37.1

Source: Cadsby, Frank, and Maksimovic (1990).

satisfy the Cho–Kreps intuitive criterion. These games pit Pareto-dominance against the intuitive criterion as a selection principle.

Cadsby et al. ran twenty-six experimental sessions using a very wide range of parameter configurations. In each round firms learned their types and chose whether to raise equity and, if so, whether to buy a costly advertisement. Then a descending-price auction was conducted in which many investors bid for the right to own a share of the firm's equity in exchange for some fixed capital.

The results from the last two periods of each session are summarized in Table 8.12. The left-hand columns show the percentage of L and H firms choosing to advertise (i.e., to signal), and the mean equity share raised in the market for firms that advertised ("if A") and did not advertise ("if no A"). The right-hand columns show the predictions about the fractions of firms of the respective types that should advertise, and the predicted equity shares (in percent). The "intuitive" column gives the intuitive equilibrium. The "other" column gives other equilibria that are not intuitive.

Take game G4 as an example. The intuitive equilibrium is a separating equilibrium in which L firms are predicted never to advertise, H firms are predicted always to advertise, and equity shares of 40 percent and 60 percent are predicted for firms that advertise and don't advertise, respectively (denoted (0, 100, 40/60)). There are two other equilibria, one in which neither firm advertises and the equity share is 48 percent, and another in which only L firms enter the equity market (H firms stay out) and the equity share is 60 percent.

There are three types of games. G1–2, BC1, GW2, and GS1 are the simplest: Firms could not advertise (replicating their earlier study). In these games, virtually all firms enter the equity market, and the mean equity share is within a few percentage points of the prediction.

In games G4–G6, GW1, and GW3, the intuitive equilibria are separating equilibria in which only H firms advertise (although there are also no-signaling pooling equilibria that are not intuitive). H firms do not advertise quite as often as predicted, but there is partial separation because the L types rarely advertise and the H types advertise about half the time.

In all other games, there are two pooling equilibria; the intuitive equilibrium requires both types to advertise. In roughly half the games there is a lot of advertisement and equity shares are fairly close to those predicted. In the other half, however, there is very little advertisement (e.g., BC2–BC4 and GW4–GW5).

To explore why players in some sessions pool on advertising and in other sessions pool on no-advertisement, Cadsby et al. computed the short-term gain for an H firm defecting from a no-advertising pooling equilibrium, assuming their advertisement is taken as a signal of high quality. This number is the dollar pressure toward convergence (by H types) *away* from the

Table 8.12. *Corporate finance games with advertising of Cadsby et al.*

	Results (last two periods)				Equilibrium predictions	
	Percent advertising (A)		Mean share (percent)		Intuitive	Other
Game	L	H	If A	If no A	(%L, %H, shares)	(%L, %H, shares)
No advertising allowed (data are entry rates)						
G1*	100	100	—	39.7	(100, 100, 40)	—
G2*	100	0	—	59.4	(1, 0, 62.5)	—
BC1*	100	100	—	28.0	(1, 1, 32)	
GW2*	100	83	—	26.3	(1, 1, 30)	
GS1*	100	100	—	59.8	(1, 1, 60)/(1, 0, 80)	
Separating equilibria are intuitive						
G4	0	44	42.5	48.0	(0, 100, 40/60)/ (0, —, 60)	(0, 0, 48)/ (0, —, 60)
G5	0	50	30.0	56.6	(0, 1, 30/60)/ (0, —, 70)	(0, 0, 40)/ (0, —, 75)
G6	0	75	50.0	73.8	(0, 1, 50/75)	(0, 0, 60)
GW1	14	20	31.0	32.3	(0, 1, 30/50)	(0, 0, 37.5)
GW3	0	100	51.0	74.9	(0, 1, 50/75)	(0, 0, 60)
All-advertise pooling equilibrium is intuitive						
G3	40	14	22.0	23.6	(100, 100, 24)	(0, 0, 24)
G7	75	100	23.7	58.0	(1, 1, 30)/(0, —, 80)	(0, 0, 30)
G8	33	100	50.5	76.2	(1, 1, 60)	(0, 0, 60)
BC2	0	25	17.0	24.0	(1, 1, 24)	(0, 0, 24)
BC3	0	14	48.0	51.5	(1, 1, 60)	(0, 0, 60)
BC4	0	17	60.0	60.2	(1, 1, 60)/(0, —, 75)	(0, 0, 60)
GW4	0	0	—	54.0	(1, 1, 60)	(0, 0, 60)
GW5	0	0	—	45.2	(1, 1, 48)	(0, 0, 48)
GW6	100	100	58.0	—	(1, 1, 60)	(0, 0, 60)
GW7	80	100	55.6	69.0	(1, 1, 60)	(0, 0, 60)
GW8	14	100	45.6	59.0	(1, 1, 50)	(0, 0, 50)
GS2	0	33	19.0	24.9	(1, 1, 30)	(0, 0, 30)
GS3	60	100	30.9	41.5	(1, 1, 24)	(0, 0, 24)
GS4	80	100	44.9	70.0	(1, 1, 52.2)	(0, 0, 52.2)
GS5	0	50	53.0	59.2	(1, 1, 60)	(0, 0, 60)

Source: Cadsby, Frank, and Maksimovic (1998).

no-signaling equilibrium. A regression of the fraction of H-type firms advertising against this convergence-pressure figure (where each session is a data point) yielded a coefficient significant at $p = .04$, so they have a statistical explanation for why some sessions converge to advertising whereas others don't. They also fitted an adaptive fictitious-play type model, which shows significant learning by investors in about half the sessions.

8.2.3 Games with Ratchet Effects

Chaudhuri (1998) studied ratchet effects in a principal–agent game with two-period output and dynamic quota-setting. His experiment is difficult to explain briefly so I'll just sketch the design and results. In each of two periods the principal sets a quota. Agents have high or low costs and produce an output in each period. A clever principal should set a low quota in the first period, since the low-cost agent will prefer to produce a lot to meet the quota and earn a big bonus, even though it means revealing their type. When their type is revealed, the principal "ratchets" up the quota in the second period and extracts more surplus from the low-cost principal.

Berliner (1957) coined the term "ratchet effects" and pointed out that in centrally-planned economies, quota schemes are often used to induce managers to produce higher output (in the absence of profit-sharing, reputational, and high-powered promotional incentives). Wise managers often create inefficiency by deliberately underproducing to avoid being ratcheted. Even in capitalist economies, quota-setting is commonly used in regulated industries. Quotas are also used inside firms in contracts between managers and individual workers whose effort is hard to observe (e.g., salespeople who work in the field). For the purposes of this book, ratcheting is interesting for another reason: It isolates two simple steps of strategic reasoning that are central in all signaling games. Agents should worry about revealing their types in early periods. In turn, principals should take actions that force agents to reveal their types and enable them to ratchet.

As in many signaling games, Chaudhuri's subjects show *some* strategic sophistication, but not as much as is generally assumed by most equilibrium concepts. The main deviation is that low-cost agents unwittingly reveal their types by producing a high output in the first period even when the quota is high (when they should be hiding their skill by underproducing). As a result, principals do not need to choose the type-revealing low quota in the first period to learn what they need to ratchet later. So ratcheting occurs, but not because the principals force revelation of information initially; it occurs because the low-cost agents just give themselves away.

Cooper, Kagel, Lo, and Gu (1999) also studied ratchet effects. Their model is a simplified form of Freixas, Guesnerie, and Tirole (1985). It models target and output decisions in a "dual-track" economy, in which firms sell all output up to a target to the state at a fixed price, and can sell additional output in an open market. Planners prefer to maximize social surplus (output up to the target minus costs), subject to a penalty for profits from output beyond the target.[16]

In their game, firms first learn their productivity type, high (H) or low (L), which are equally likely. Then firms choose an output level from 1 to 7. A planner observes the firm's output choice, but not its type, and chooses a target, easy or tough. Compared with Chaudhuri's two-period model, Cooper et al. essentially trimmed away the planner's choice of a first-period quota (it's exogeneous) and the firm's second-period output (it's assumed to be optimal) from Chaudhuri's two-period model. Their reduced form focuses attention on what inferences about the firm's type planners can draw from observed output, and whether firms anticipate these inferences and try to hide their types to avoid being ratcheted.

Payoffs are shown in Table 8.13. With these payoffs, it pays for planners to set an easy target if $P(L|\text{output})$ is above 0.325. In equilibrium, L firms will choose low outputs, either 1 or 2, and H firms should pool by choosing what L firms do most of the time. (The pooling equilibria at 1 and 2 satisfy the intuitive criterion.) Myopic K firms that ignore the inferences planners draw will choose 5, hoping for the payoff of 1328 (but receiving 1035, less than the pooling payoffs of 1108–1145, if planners set the tough target offer observing the firm's choice of 5).

Cooper et al. were interested in the effects of unusual treatments they were uniquely suited to apply. Subjects were students and factory managers in China. The factory managers were generally in their forties or fifties and worked for state-run enterprises or in joint ventures with foreign investors. Most had some higher education (typically equivalent to a trade school or community college). Because wages are low in China (relative to American research budgets!) they were able to run high-stakes treatments very cheaply. They also explored the interaction of contextual labels—compared with abstract labels, the default standard in economic experimentation—with managerial experience.

Some results are summarized in Table 8.14. Since 70–90 percent of the L firms chose the output of 2, and most H firms chose either 2 or 5, the most interesting feature of the data is the relative frequencies of H firms choosing either 2 or 5 over time. They started in periods 1–12 choosing the myopic output of 5 between 54 and 76 percent of the time, and choosing the pooling output 2 only about 10 percent of the time. Planners reacted

[16] The penalty from profits presumably reflects something like a social distaste for private profits in a communist society, akin to a personal distaste for inequality writ large.

Table 8.13. Payoffs in ratchet game of Cooper et al.

	Firm payoff			
	Low-productivity firm production target		High-productivity firm production target	
Output	Easy	Tough	Easy	Tough
1	710	542	1108	815
2	730	561	1145	852
3	697	528	1230	937
4	527	357	1308	1015
5	273	103	1328	1035
6	220	48	1298	1005
7	190	15	1250	966

	Planner payoff	
Production target	Facing low-productivity firm	Facing high-productivity firm
Easy	645	764
Tough	528	820

Source: Cooper et al. (1999).

sensibly, albeit noisily, by choosing tough targets in response to outputs of 2 and 5 about 20 percent and 80 percent of the time, respectively. Over trials 13–36 there was gradual convergence away from H choices of 5 and toward the pooling prediction of 2, but the convergence was not sharp.[17]

Students in H firms started out more strategically than older managers; they chose the pooling output of 2 more often in early periods, and they learned faster. Probit regressions of H firm choices and planner responses confirm that players learned: H firms responded to the differential rate of tough target-setting they experienced, and planners reacted to the previous distribution of H firm outputs.

Stakes had an effect because H firms learned more when stakes were higher (although planners did not). Cooper et al. suggest that the planners' reasoning problem was easier, since it required an inference only about which type of firm chose which output, whereas the firms had to anticipate

[17] Cooper et al. comment that learning is too fast to be explained by reinforcement models, but also too slow to be explained by fictitious play. This suggests that the hybrid EWA model will fit better than those extreme cases (see Chapter 6).

Table 8.14. *Frequency of H firm outputs 2 and 5, and tough responses (percent)*

	Periods						Pooling prediction	
	1–12		13–24		25–36			
Outputs	2	5	2	5	2	5	2	5
Older managers, generic labels								
H output of 2, 5	7	76	22	60	37	39	100	0
Frequency of tough targets	30	73	18	98	21	92	0	100
Older managers, contextual labels								
H output of 2, 5	1	70	27	46	45	38	100	0
Frequency of tough targets	19	79	25	91	18	100	0	100
Students, generic labels								
H output of 2, 5	14	54	32	40	43	37	100	0
Frequency of tough targets	20	88	21	98	30	98	0	100
Students, context labels								
H output of 2, 5	22	62	52	31	57	30	100	0
Frequency of tough targets	11	92	18	93	20	95	0	100

Source: Cooper et al. (1999).

the thinking of the planners. Incentives therefore affected the kind of thinking that is more difficult (the firms').

Probit regressions showed that context effects were weak for students and stronger for managers. The context effects virtually disappeared when interacted with prior behavior of other subjects, however, which suggests that context was simply providing a language or vivid framework that facilitated learning from experience. The context effects on managers' choices were also stronger in their role as planners than in their role as firms. Cooper et al. speculates that this difference was because managers were less familiar with planner thinking (so context helps), but already knew from the field how to manage earnings and hide productivity. For example, many Chinese firms keep two sets of accounting books. They also maintain slush funds (*xiaojin ku*) built up from selling excess production off the books, to spend on expense categories that are carefully regulated, such as employee housing and other in-kind benefits.

8.2.4 Belief Learning in Limit Pricing Signaling Games

Cooper, Garvin, and Kagel (1997a,b) studied limit pricing in a well-known game in which monopolists have unobserved types (e.g., product costs) and choose prices or quantities to prevent entry. Table 8.15 shows the payoffs in

Table 8.15. Payoffs in a limit pricing game of Cooper et al.

	Player A's (monopolist) payoffs			
	High cost, M_H		Low cost, M_L	
A's choice	X (IN)	Y (OUT)	X (IN)	Y (OUT)
1	150	426	250	542
2	168	444	276	568
3	150	426	330	606
4	132	408	352	628
5	56	182	334	610
6	−188 (38)	−38 (162)	316	592
7	−292 (20)	−126 (144)	213	486

	Player B's (entrant) payoff		
B's action choice	High cost, M_H B's payoff	Low cost, M_L B's payoff	Expected value
Treatment I			
X(IN)	300	74	187
Y(OUT)	250	250	250
Treatment II			
X(IN)	500	200	350
Y(OUT)	250	250	250

Source: Cooper, Garvin, and Kagel (1997a).
Notes: Expected values were not included in experimental instructions. Payoffs in parentheses (.) are those in Cooper, Garvin, and Kagel (1997b). Payoffs are in "francs," 1 franc = $0.001.

their game (adapted from Milgrom and Roberts, 1982). A monopolist (A) learns its own type, equally likely to be either high or low (denoted M_{H} and M_{L}), and chooses a quantity from 1 to 7. After observing the quantity choice, but not the monopolist's type, an entrant B decides whether to move IN and enter, or stay OUT. In their treatment I, B (entrant) players earn 250 from staying out and either 300 from entering, if the monopolist is M_{H}, or 74 from entering, if the monopolist is M_{L}. Given these payoffs, entrants are reluctant to enter unless they are fairly certain that the monopolist is a high type. If they are risk-neutral, they must think $P(M_{\mathrm{H}}) \geq 0.78$ to enter.

With the treatment I payoffs, there are a variety of equilibria. There are two pure-strategy separating equilibria in which the high type M_{H} chooses quantity 2, succumbing to entry, and the low type M_{L} chooses either 6 or

7, deterring entry. In these equilibria, the monopolist's choice reveals its type. There are also several pooling equilibria. In each of these equilibria, high and low types choose exactly the same quantity (any level from 1 to 5). Because the entrant doesn't learn anything about the monopolist's type from the observed quantity, it uses the prior probability of types, calculates an expected value of 187, and prefers to stay out and earn 250. However, these are equilibria only, if a surprise defection to a different quantity is perceived to be an indication that the monopolist is M_H; then the entrant will choose IN. Since that's worse for both types, the monopolists will stick to the pooling quantity. Refinements of sequential equilibrium allow various equilibria and exclude others.[18]

The top panel of Table 8.16 shows frequencies of choices in each of the three twelve-period cycles for inexperienced subjects. Both types of monopolists started at their "myopic maxima"—the choices that give the highest expected payoffs if the entrant is equally likely to choose in or out. Low types M_L chose 4 and high types M_H chose 2. However, Es were more likely to enter when the quantity they observed was 1–3. As a result, a choice of 2 was often met with entry and gradually M_H monopolists moved upward toward 4, trying to pool with M_L types. The learning process started up again with experienced subjects playing the same game. After three cycles, there was sharp convergence to a pooling equilibrium at the quantity 4: About two-thirds of the high types M_H chose 4, almost all the low types did, and only 6 percent of the entrants entered.

In treatment II, the payoffs are changed so that entrants should enter if the monopolist is equally likely to be either high or low. This parameter change breaks the pure pooling equilibria present in the treatment I, since any pooling equilibrium in which both types enter all the time then leads to entry. Low-type monopolists can always do better by choosing 6 or 7 and separating themselves from the high types.[19] This treatment is good for checking whether there is a natural tendency for the monopolist types to pool that is immune to experience, or whether pooling is a natural state for players to pass through while converging to a separating equilibrium.

Table 8.17 shows results from treatment II. For inexperienced subjects, the cycle 1 results look very much like those from treatment I, beginning at the monopolists' myopic maxima. This feature is important because it

[18] Of the separating equilibria in which M_L types choose 6 or 7, the Riley equilibrium, a single round of deletion of dominated strategies, and the Cho–Kreps intuitive criterion all exclude the possibility that M_L types choose 7. Among the pooling equilibria, the intuitive criterion allows pooling at 4–5 (as does divinity), undefeated equilibrium allows 2–4, and both perfect sequential equilibrium (Grossman and Perry, 1986) and the evolutionary model of Noldeke and Samuelson (1993), which is uniquely H-dominant, predict pooling only at 4.

[19] There are also several partial pooling equilibria in which the two monopolist types do not make exactly the same choices but the sets of choices they sometimes make overlap.

Table 8.16. *Quantity choices and entry rates: Treatment I (percent)*

	M_H			M_L			IN%		
	Cycle			Cycle			Cycle		
Quantity	1	2	3	1	2	3	1	2	3
Inexperienced subjects (sessions 1–2)									
1	2	3	6	1	0	0	33	67	33
2	**69**	**50**	38	4	0	0	57	64	64
3	6	10	**10**	5	2	1	30	74	30
4	21	36	47	**76**	**86**	**91**	13	10	9
5	2	1	0	6	8	6	0	15	25
6	0	0	0	3	2	1	33	50	0
7	0	0	0	3	2	1	0	0	0
Experienced subjects (session 3)									
1	2	2	3	0	0	0	100	0	100
2	41	28	23	0	0	2	59	91	70
3	2	2	5	0	2	0	100	50	50
4	**55**	**68**	**69**	**100**	**98**	**98**	3	6	6
5	0	0	0	0	0	0	—	—	—
6	0	0	0	0	0	0	—	—	—
7	0	0	0	0	0	0	—	—	—

Notes: Numbers are estimated from Cooper, Garvin, and Kagel (1997a, Figure 1), assuming equal sample sizes in sessions 1–2. Median choices are in **boldface.**

implies that (as in treatment I) the monopolists were *not* making a sophisticated guess about how entrants would react to different quantity choices. By the third cycle, however, a small spike of data grows at 6 (15 percent), where low types M_L were beginning to separate themselves from the pack, who pooled at 4 and were entered upon about half the time. By the third cycle of experienced subjects the data have modestly converged to the separating equilibrium in which high types chose 2 and low types chose 6, with a residue of both types pooling at 4, and entrants chose IN frequently for quantities of 5 or less.[20]

[20] In further sessions, Cooper et al. put some subjects in the treatment II condition first, and observed fairly strong convergence to separating equilibria (M_H chose 2, M_L chose 5–6). Then they reverted to the treatment I payoffs, in which pooling can be an equilibrium. They observed some movement toward the pooling equilibria (a lot more M_Hs chose 4), but not much.

Table 8.17. *Quantity choices and entry rates: Treatment II (percent)*

	M_H			M_L			IN%		
	Cycle			Cycle			Cycle		
Quantity	1	2	3	1	2	3	1	2	3
Inexperienced subjects (sessions 4–5)									
1	6	6	0	1	0	0	80	100	—
2	**71**	39	33	7	4	12	88	91	94
3	12	**6**	13	3	8	6	60	83	100
4	11	48	**54**	**72**	**67**	**67**	53	52	63
5	0	0	0	9	15	0	40	44	—
6	0	1	0	6	6	15	50	33	33
7	0	0	0	2	0	0	0	—	—
Experienced subjects (session 6)									
1	3	5	8	0	0	0	100	100	100
2	43	40	**49**	4	0	0	95	100	100
3	**13**	**5**	4	2	5	3	100	100	100
4	41	**40**	32	37	22	14	79	85	80
5	0	10	6	**9**	7	3	0	57	100
6	0	0	0	48	**66**	**80**	14	7	12
7	0	0	0	0	0	0	—	—	—

Notes: Numbers are estimated from Cooper, Garvin, and Kagel (1997a, Figure 1), assuming equal sample sizes in sessions 1–2. Median choices are in **boldface.**

In Cooper et al. (1997b) they extended their earlier experiments in two directions, using variants of treatment II in which pure pooling is not an equilibrium and subjects tend to separate. First, note that choices of 6 and 7 are dominated for high-type monopolists (and are rarely chosen). From a learning point of view, the speed with which low types separate by choosing high quantities (6 or 7) might depend on how frequently entrants stay out after observing such a high choice, which in turn depends on whether entrants stay out after observing such a high choice, which in turn depends on whether entrants realize that the high quantities are dominated for high types, so only low types will make those choices. Therefore, variables that affect the entrants' beliefs about which type chooses high quantities (and the low-type monopolists' beliefs about those beliefs) could affect the rate of convergence.

To test this prediction, Cooper et al. changed the high-type payoffs to the larger positive numbers shown at the bottom of Table 8.15 ("Treatment

II"). In the original treatment, the high-type payoffs to 6–7 were negative, so entrants could guess more easily that high types would never choose those quantities.[21] The positive-payoff treatment is called the "0 percent anticipation" treatment because entrants are not *told* that high-cost monopolists won't choose 6 or 7 (but they should be able to infer it). In the "100 percent anticipation" treatment, the instructions specifically forbid high-type monopolists from choosing 6 or 7, and this instruction is public knowledge. The conjecture is that since low-type monopolists know that entrants know this, they might be more willing to separate because their high-quantity choices will be taken as a signal of their low type and will deter entry.

Table 8.18 shows the results from experienced subjects only. As expected, in the 100 percent anticipation treatment subjects converged rather swiftly and sharply to the separating equilibrium in which M_{H} chooses 2 and M_{L} chooses 6 (as predicted by most refinement concepts). The swift emergence of this equilibrium, compared with the earlier results, shows that even simple steps of iterated dominance (e.g., entrants learning that high-type monopolists will obey dominance) are apparently learned only over time, because when those steps are explained to subjects overall equilibration is faster.

Summary: This section reviewed signaling games about lobbying, equity finance, ratchet effects, and limit pricing. These games are very simple models of complex processes in these economic domains which assume signaling. The results are encouraging for the combination of simple game-theoretic predictions and adaptive processes. In the lobbying games, predictions of an equilibrium that survives universal divinity are roughly accurate and behavior responds to changes in payoff parameters, although less sharply than predicted. In the equity finance games without advertising, and in games with advertising and intuitive separating equilibria, behavior in later periods is very close to predicted. In finance games with two pooling equilibria, there tends to be bifurcation to either the both-advertise equilibrium or the none-advertise equilibrium. In the ratchet effect games, players often unwittingly reveal their types, subjecting themselves to ratchet effects; and other players applying ratchets (raising production quotas for productive firms) quickly notice the separation and exploit it. Chinese managers who are used to operating in planned economies with ratchet incentives do better when contextual labels remind them the game is familiar.

In limit pricing games, players in the role of low- and high-cost firms often unwittingly separate. Learning to (partially) pool is retarded by the noisy interpretation by entrants of what they can learn from the firms'

[21] The implicit assumption here is that attention is drawn to negative numbers, or that entrants believe players are more motivated to avoid out-of-pocket losses than opportunity losses, compared with the case in which 6 and 7 are dominated but yield positive payoffs.

Table 8.18. Quantity choices and entry rates: experienced subjects (percent)

	M_H			M_L			IN%		
	Cycle			Cycle			Cycle		
Quantity	1	2	3	1	2	3	1	2	3
0% anticipation of M_H not playing 6–7									
1	2	2	2	5	0	0	100	100	100
2	38	26	38	5	2	0	95	92	94
3	**11**	18	**23**	22	9	8	67	56	86
4	49	**51**	33	**68**	**33**	**52**	42	69	72
5	0	3	4	3	28	30	100	17	47
6	0	1	0	0	6	0	—	50	—
7	0	0	0	4	9	9		33	50
100% anticipation of M_H not playing 6–7									
1	0	9	2	0	0	0	—	100	0
2	**56**	**76**	**78**	0	2	0	96	100	100
3	2	4	7	0	0	3	100	100	100
4	38	12	15	26	13	12	63	92	100
5	3	0	0	0	0	0	50	—	—
6	0	0	0	**75**	**84**	**88**	8	0	5
7	0	0	0	0	0	0	—	—	—

Notes: Numbers are estimated from Cooper Garvin, and Kagel (1997b, Figure 4). Median choices are in **boldface.**

output choices. Convergence to pure separation is sharper when payoffs are changed so that dominance violation for high-cost firms is easier to infer (exploiting vividness of losses), or when dominance violations are explicitly ruled out by forbidding high-cost firms to violate dominance.

In most of the games, simple learning notions such as fictitious play, or study of subjects' incentives to break a separating equilibrium, do a good job of explaining how equilibration occurs or where it fails.

8.3 Reputation Formation

In modern game theory, a player's reputation is crisply defined as the probability that she has a certain privately observed type or will take a certain action. The precision of these reputation models comes from sequential equilibrium assumptions, which tie together a player's reputation with the actions an optimizing player is likely to take. This precision also raises the empirical question of whether such complex equilibria actually occur when people play these games. Equilibria require a delicate trapeze act linking actions of the agents cultivating reputations with Bayesian updating by players who perceive reputations of others (knowing what the agents are likely to do).

8.3.1 Trust

The first paper to explore these reputation models carefully was by Weigelt and me (Camerer and Weigelt, 1988). We used a "trust" or borrower–lender game. In each eight-period repeated game, a single borrower drew a random type, either normal (X) or "nice" (Y). The same borrower played a series of eight stage games, with a different lender player each time. The lender could either lend or not lend; if she chose to lend, the borrower then decided whether to default or repay. Payoffs are shown in Table 8.19. Not lending gives the lender 10, and lending pays 40 if the loan is repaid or −100 if the borrower defaults. A normal borrower prefers to default, since she gets 150 rather than getting 60 from repayment. A nice borrower gets 0 from defaulting and thus prefers to repay.

Sequential equilibria for the eight-period game have the following properties: Lenders and borrowers must choose strategies with the highest expected payoffs given their beliefs; lenders update their beliefs about the borrower's type (X or Y) using Bayes' rule when possible; and, loosely speaking, lenders have *some* belief about the borrower's type after out-of-equilibrium events (when Bayes' rule cannot be applied).

With these requirements in mind, the equilibrium analysis proceeds as follows. Start from period 8. Since this is the last period, lenders know the borrower will default if she is type X and gets a loan, and will repay if she is type Y. Therefore, the only question is the probability that the incumbent is a nice type, $P_8(nice)$. Simple algebra shows that, if entrants are risk-neutral, they should enter if $P_8(nice)$ is above 0.79. Define this important

Table 8.19. *Payoffs in Camerer and Weigelt's borrower–lender (trust) game*

Lender strategy	Borrower strategy	Payoffs to lender	Payoffs to borrower	
			Normal (X)	Nice (Y)
Lend	Default	−100	150	0
	Repay	40	60	60
Don't lend	No choice	10	10	10

Source: Camerer and Weigelt (1988).

threshold to be τ. In the second-to-last period, period 7, normal borrowers are torn between two forces: Conditional on getting a loan, they would like to default to get the highest payoff; but if they do so they reveal their type and will not get a loan in period 8. Suppose the borrower's reputation (the perceived $P_7(nice)$ in period 7) is below τ. If the normal-type borrower always repays, Bayesian updating leads lenders to have the same perception $P(nice)$ in period 8 as in period 7. But since (by assumption) this $P(nice)$ $\leq \tau$, if borrowers always repay in period 7 they will not get a loan in period 8. The trick is for borrowers to play a mixed strategy, repaying frequently enough that if they *do* repay, the updated $P(nice)$ will be equal to τ, so that lenders will be willing to lend (actually, are indifferent) in the last period. Given a particular $P(nice)$ in period 7, the normal borrower should choose a repayment probability that keeps the lender indifferent to lending and not lending in period 7, and allows Bayesian updating of his reputation to the threshold τ in period 8. Combining these two conditions gives a threshold of perceived $P_7(nice)$ that happens to be τ^2, and a mixed strategy probability of default in period 7 of 0.560.

The same argument works by induction back to period 1. In each period the lender has a threshold of perceived $P(nice)$ which makes her indifferent to lending and not lending. The path of these $P(nice)$ values is simply τ^n. Figure 8.2 shows this path (using language for entry-deterrence in

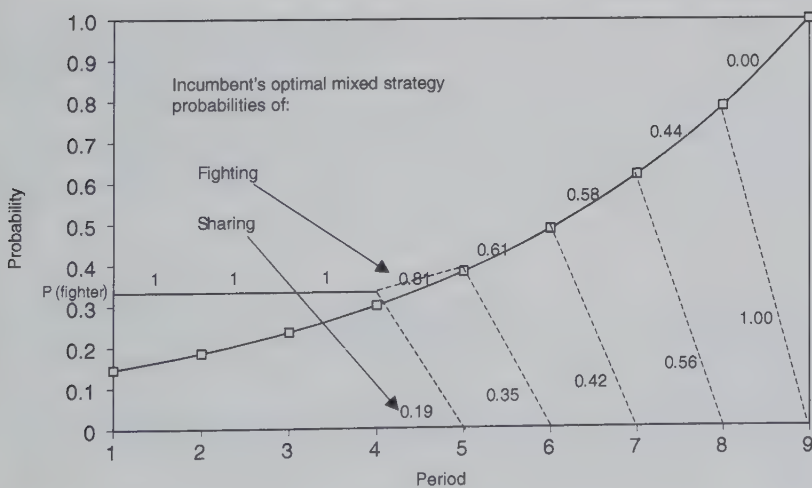


Figure 8.2. *Incumbent's optimal mixed strategy of fighting as predicted by sequential equilibrium. Source: Based on Camerer and Weigelt (1998). Note: Entrant's entering threshold =* $(.789)^{9-t}$.

which "fighter" incumbents correspond to nice borrowers and entrants correspond to lenders). The y-axis shows the entrant's reputation ($P(\mathit{fighter})$) and the x-axis is the eight periods in the game (plus "period 9," which, like the 19th hole in golf, is a dummy period denoting perceptions after period 8 is over). The probabilities on the graph are the mixed-strategy probabilities of fighting by normal incumbents which keep the entrant's perceptions along this path (for an initial prior $P(\mathit{fighter})$ of 1/3).

Figure 8.2 can be used to illustrate all the key properties of this equilibrium.[22] In the first three periods, the threshold $P(\mathit{nice})$ is below the prior of 1/3, so borrowers can "afford" to always repay. Anticipating this, lenders should always lend. Beginning in period 4, normal borrowers must mix in order to boost their reputation, conditional on repaying, to stay along the equilibrium path of $P(\mathit{nice})$ which increases. The probability of default rises as the last period draws near. Lenders also mix starting in period 4, lending with probability 0.643. If the borrower ever defaults, the entrant should never lend in subsequent periods.

Two patterns in the data are of primary interest. First, what is the rate of lending and default across periods (and how does it change across sequences)? And, second, do lending and repayment in each period of an eight-period sequence reflect prior history in that sequence as the equilibrium predicts?

Table 8.20 reports the conditional frequencies of lending and repayment (by normal types), from the last two-thirds of all the sequences, pooled together.[23] Actual frequencies significantly different than the prediction are marked by an asterisk.[24]

One key prediction was that lending should drop off sharply in period 5 (for experiments 3–5) or 4 (experiments 6–10). It did, but the drop was not as sharp as predicted. However, comparing the pooled periods predicted to have 100 percent lending together with those predicted to have a constant mixed rate of lending does show a sharp difference, which is close to the prediction. For example, in experiments 3–5 those pooled frequencies were

[22] Characteristically, there are other sequential equilibria. For example, the normal borrower might never repay, if she thinks that the entrant will perceive repayment as an indication of a normal type rather than a nice type. The intuitive criterion selects the equilibrium we discuss however, so we will casually refer to it as "the" equilibrium.

[23] The frequencies reported are conditional on within-sequence history. For lending, the frequencies are conditioned on no previous default and no failure to lend in the previous period (which, in theory, should lead lenders never to lend again in that sequence). For default, the frequencies are conditional on getting a loan, and on no previous default in the sequence.

[24] The statistical test assumes draws are independent across sequences, which is unlikely, but there is no accepted method for correcting for independence. Furthermore, dependence probably means the standard errors are too low, and hence the significance reported in the table is overstated.

Table 8.20. *Lending and repayment rates: Camerer–Weigelt (percent)*

Experiments		Round (1 = start, 8 = end)							
		1	2	3	4	5	6	7	8
Conditional frequency of lending									
3–5	Predicted	100	100	100	100	64	64	64	64
	Actual	94	96	96	91	72	59	38*	67
6–8	Predicted	100	100	100	64	64	64	64	64
	Actual	96	99	100	95*	85*	72	58	47
9–10	Predicted	100	100	100	64	64	64	64	64
	Actual	93	92	83	70	63	72	77	33
Conditional frequency of repayment by normal (X) types									
3–5	Predicted	100	100	100	81	65	59	44	0
	Actual	95	97	98	95*	86*	72	47	14
6–8	Predicted	100	100	73	68	58	53	40	0
	Actual	97	95	97*	92*	85*	70*	48	0
9–10	Predicted	100	100	73	67	63	56	42	0
	Actual	91	89	80	77	84*	79*	48	29

Source: Camerer and Weigelt (1988).
Note: * denotes significant difference (abs(z) > 2) between predicted and actual frequencies.

95 and 62 percent (predicted to be 100 and 64); in experiments 6–8 the corresponding figures were 98 and 80.

Repayment rates did fall, from close to 100 percent in the first couple of periods, to nearly zero in the last period, but repayment was generally more common than predicted. We suggest an explanation for this discrepancy: Perhaps subjects act as if some proportion of players play like nice Y types instead, even when they draw normal X-type payoffs (preferring to pay back, even in the last period).[25]

The proportion of endogeneous Y types necessary to explain discrepancies between the data and the predictions can be estimated from the data using a back-of-the-envelope calculation. We estimated the "homemade" $P(Y)$ to be 0.17 (that is, 17 percent of the subjects who drew the X type actually played as if they had Y-type payoffs). Based on that figure, we ran

[25] This is quite plausible given the evidence (see Chapter 3) that some players exhibit reciprocity, or social preferences that might make them reluctant to take an action that gives them 100 and another player −150, instead of a sacrificial action that gives them 60 and the other person 40.

experiments 9–10 in which there were *no* actual Y types at all. The predictions listed in Table 8.20 were derived from assuming that the homemade prior estimated from experiments 3–8 would then apply to experiments 9–10. This out-of-sample prediction was reasonably accurate: Predicted repayment rates were not far off (only erring significantly in periods 5–6) and predicted lending rates were quite close, averaging 90 and 67 in early and late periods when the predicted values were 100 and 64.

Weigelt and I concluded that "sequential equilibrium predicts reasonably well, given its complexity. However, formal statistical tests reject sequential equilibrium strongly for some periods of the game. Subjects failed to default as early in the game, or as often, as predicted" (Camerer and Weigelt, 1988, p. 26). The key phrase is "given its complexity." Although the equilibrium predictions are not perfectly accurate, they vary across periods in the correct direction and are not far off in magnitude. More importantly, the logic by which the predictions were derived was subtle and daring and used *no* free parameters! One can imagine many other stories about reputation-building, but they are often hard to test at all or lack the precision of the equilibrium prediction. At the time, it was hard for us to imagine a comparable theory that would deliver predictions that are just as precise and more accurate.

Neral and Ochs (1992) replicated our experiments with a critical eye on a key feature: In the later stages when players are assumed to mix, the probability $P(lend)$ should fall when the *borrower's* default payoff *falls*. (This kind of reverse payoff-dependence, in which A's mixture probabilities depend on B's payoffs, is a typical feature in games with mixed equilibria and is surely one of the more counterintuitive predictions of game theory; see Chapter 2.) Neral and Ochs tested for this perverse comparative static effect by varying the default payoff, from 150 to 100. Their design varied from ours in a few other small ways, designed to promote faster learning. They first compared the rates of four out-of-equilibrium events, which should never occur.[26] All four events are quite rare (1–13 percent), and substantially more rare than in our original study.

Table 8.21 reports the overall frequency of lending in each round of the six-round sequence and frequencies of repayment conditional on getting a loan.[27] Since Neral and Ochs's cell 1 uses the same parameters as Camerer and Weigelt (1988), the results should be the same unless the small change in design or subject pool matters. In fact, lending is a little higher in Neral and Ochs's data in middle rounds (marked by an * in CW cells), and repayment is significantly lower in one round.

[26] The events are not-lending in periods 1–2, loans following default, default in period 1, and final-round repayment.

[27] All frequencies are conditional on no previous defaults in the sequence and on no non-lending in the previous period.

Table 8.21. *Lending and repayment rates: Neral–Ochs (NO) and Camerer–Weigelt (CW)*

Condition	Round (1 = start, 6 = end)					
	1	2	3	4	5	6
Conditional frequency of lending						
Predicted (CW, NO-1)	100	100	64	64	64	64
Predicted (NO-2)	100	100	44	44	44	44
CW	96	89*	71*	51	32*	43
NO cell 1	100	99	88	60	70	88
NO cell 2	98	100	100	87	67	19
Cell 1–2 difference (p-value)	(.13)	(.81)	(.00)	(.00)	(.80)	(.00)
Conditional frequency of repayment by normal (X) types						
Predicted (all)	100	81	65	58	44	00
CW	98	92	85*	70	44	16
NO cell 1	98	97	64	51	55	00
NO cell 2	100	99	97	69	47	38
Cell 1–2 difference (p-value)	(.36)	(.21)	(.00)	(.09)	(.88)	(.21)

Sources: Neral and Ochs (1992); Camerer and Weigelt (1988).
Note: * denotes significant difference ($p < .02$) between CW and NO-1.

Table 8.21 also compares results from their cells 1 and 2 (with borrower default payoffs of 150 and 100, respectively). The table reminds you that lenders should lend *less* frequently in rounds 3–6 when the default payoff falls, only 44 percent of the time instead of 64 percent. The results go in the *opposite* direction of that prediction in rounds 3–5 (strongly so in two rounds, as indicated by reported p-values), and go significantly in the right direction in round 6. Sequential equilibrium also predicts that repayment rates should not depend on the borrower's default payoff, and this prediction is correct. Thus, the results are mixed to unfavorable for the sequential equilibrium prediction—one predicted effect fails to turn up (and often goes in the wrong direction) but the predicted non-effect on repayment is accurate.

Neral and Ochs estimated the apparent "homemade prior" of X types playing like Ys to be only 0.031, 0.019, and 0.091 in the three sessions of their cell 1, substantially lower than the 12–17 percent estimated by us. A test for pooling rejects homogeneity of these estimates at the .025 level. Thus, it appears unlikely that deviations from sequential equilibrium can be perfectly organized, across different experiments, by a single parameter.

Brandts and Figueras (1997) also studied repeated trust games. Their experiments extend our initial framework. After reviewing earlier results, they note that "it is not clear how all these experimental results fit together." They quote Selten's famous paper on the chain-store paradox:

> On the level of imagination a clear and detailed visualization of a sequence of two, three or four periods is possible—the exact number is not important. A similarly clear and detailed visualization of a sequence of 20 periods is not possible. For a small number of periods the conclusions of the induction argument can be obtained by the visualization of scenarios. For a large number of periods the scenarios will either be restricted to several periods, e.g., at the end of the game, or the visualization will be vague in the sense that the individual periods are not seen in detail. (1978, p. 153)

Brandts and Figueras suggest that shorter games provide a suitable test of sequential equilibrium because equilibria might be more visualizable or imaginable, as Selten suggests, in games with fewer rounds. Their game uses the basic Camerer–Weigelt structure except the lender's repayment payoff is 55 rather than 40 and the borrower plays either three or six periods. They vary the probability of nice type $P(Y)$ across $0, 0.25, 0.50, 0.75$. The sequential equilibria for each of these parameter values differ dramatically. Take the three-period games. When $P(Y)$ is 0 or 0.25 it is very likely that the borrower is normal and won't pay back the loan, and three periods are too few for a normal borrower to reap the rewards of paying back in the first period, so the sequential equilibria predict lenders will never lend and borrowers will never repay. When $P(Y)$ is 0.75, in contrast, most borrowers are nice and it pays for the bankers to lend even in the last period. As a result, it pays for normal borrowers to pay back in the first two periods and for bankers always to lend. The equilibrium for $P(Y) = 0.50$ is between the extreme cases.

Brandts and Figueras repeated the entire three six-period game seventy-two times for each pool of subjects. Tables 8.22 and 8.23 report results averaged across the last quarter of the sessions (eighteen sequences). Asterisks denote statistically significant deviations from the equilibrium predictions.

As they conjectured, behavior was fairly consistent with the sequential equilibria in the extreme cases where $P(Y) = 0$ (never lend) and $P(Y) = 0.75$ (always lend), for both three- and six-period games. When $P(Y) = 0.25$ or 0.50 the picture is mixed. Bankers lent too often and borrowers repaid too often. Brandts and Figueras estimated a homemade prior $P(Y)$ of 0.24–0.29. However, adjusting for this effect does not generally move the (adjusted) sequential equilibrium predictions much closer to the data.

Brandts and Figueras show that behavior *is* consistent with three basic effects of period number, game length, and $P(Y)$. Changing variables one at a time, there is more lending and more repayment in earlier periods, for longer games, and as $P(Y)$ rises. Table 8.24 illustrates these properties with statistics from the last eighteen sequences of the six-period games,

Table 8.22. *Repayment and lending rates in six-round games*

		Round (1 = start, 6 = end)					
P(nice)		1	2	3	4	5	6
Conditional frequency of lending							
0	Predicted	0	0	0	0	0	0
	Actual	61*	20	0	—	0	—
0.25	Predicted	100	100	64	64	64	64
	Actual	100	100	100*	86*	69	33
0.75	Predicted	100	100	100	100	100	100
	Actual	100	100	100	100	85	89
Conditional frequency of repayment by normal (X) types							
0	Predicted	0	0	0	0	0	0
	Actual	45*	37*	0	17	0	0
0.25	Predicted	100	98	61	55	41	0
	Actual	100	100	100*	80*	29	0
0.75	Predicted	100	100	100	100	100	0
	Actual	100	100	100	13	22	0

Source: Brandts and Figueras (1997).
Note: * denotes significant difference ($p < .02$) between predicted and actual.

smoothing the data by combining two-period pairs. Lending frequencies fell smoothly across rounds in each condition, and reneging frequencies rose. Furthermore, across the three values of $P(Y) = 0, 0.25$, and 0.75, the overall lending probabilities increased (0.37, 0.75, and 0.93) and repayment probabilities increased (0.31, 0.89, and 0.93). Brandts and Figueras concluded that behavior is consistent with basic principles of reputation formation even if it is not consistent with detailed predictions of sequential equilibrium.

8.3.2 Entry Deterrence

Jung, Kagel, and Levin (1994) ran experiments with a "chain-store" entry-deterrence game, which is a workhorse in industrial organization modeling. Chicago School economists (among others) are skeptical that monopolist firms deter entry by setting "predatory prices" or taking other unprofitable actions. The problem is that deterrence is a costly investment, and pays only if future entrants are dissuaded from deterring. But, if future entrants know that the incumbent monopolist can't lose money forever, they will enter,

Table 8.23. *Repayment and lending rates in three-round games (percent)*

$P(honesty)$		Round (1 = start, 3 = end)					
		Frequency of repayment			Frequency of lending		
		1	2	3	1	2	3
0	Predicted	0	0	0	0	0	0
	Actual	40*	0	0	17	0	0
0.25	Predicted	0	0	0	0	0	0
	Actual	91*	53*	17	85*	86*	53*
0.50	Predicted	99	41	0	100	64	64
	Actual	86	68*	0	52*	100*	64
0.75	Predicted	100	100	0	100	100	100
	Actual	100	86	0	100	100	100

Source: Brandts and Figueras (1997).
Note: * denotes significant difference ($p < .02$) between predicted and actual.

so it doesn't pay to deter in the short run. Early experiments by Isaac and Smith (1985) in a complete-information framework showed that predation was indeed rare.

Jung, Kagel, and Levin pointed out that incomplete-information reputational models were being developed around the same time to formalize Selten's (1978) intuition that deterrence would occur until the last few periods if the game was finitely repeated. And my earlier experimental results with Weigelt suggested that reputation-formation was possible, even without an exogeneous prior "type" that likes to deter. Jung, Kagel, and Levin were motivated by both the experimental results confirming some predictions of the new models and the fact that the likelihood of predatory pricing or en-

Table 8.24. *Repayment and lending rates in six-round games: Two-period pairs*

	$h = 0$			$h = 0.25$			$h = 0.75$		
Rounds	1–2	3–4	5–6	1–2	3–4	5–6	1–2	3–4	5–6
Lending frequency	46	36	29	100	93	32	100	1	78
Repayment frequency	42	27	19	100	91	25	100	93	37

Source: Brandts and Figueras (1997).

Table 8.25. *Payoffs in the chain-store game*

Entrant strategy	Incumbent strategy	Payoffs to entrant	Payoffs to incumbent	
			Normal (X)	Fighter (Y)
In	Fight	80	70	160
	Share	150	160	70
Out	No choice	95	300	300

Source: Jung, Kagel, and Levin (1994).

try deterrence is a long-standing problem in IO, which experimental data might shed light on.

Their design closely followed ours and the game-theoretic models. Table 8.25 shows payoffs. The entrants prefer to play IN if met by SHARE behavior, earning 150, but they earn the least if the incumbent fights (80) and earn an intermediate payoff of 95 from staying OUT. A "normal" incumbent earns 300 if the entrant is out, earns 160 if the entrant plays IN and she plays SHARE, and earns only 70 if she fights. Fighter-type incumbents have share and fight payoffs reversed. Payoffs were increased for entrants in two sessions.[28] With these parameters, the sequential equilibrium is very much like the one in the Camerer–Weigelt trust games: fighting for a couple of periods (and entrants staying out) followed by mixing, with an increasing tendency to share toward later periods. (The exact predictions are given in Table 8.26.)

The main treatment variables in the experiments by Jung et al. were experience (some subjects returned for a second session) and the prior $P(\mathit{fighter})$, which was 1/3 in six sessions and 0 in six sessions. Results are summarized in Tables 8.26 and 8.27, reported separately for early (1–30) and late (31–end) sequences within a session.[29]

Consider $P(\mathit{fighter}) = 1/3$ first. Inexperienced subjects generally entered more often than predicted, because entrants shared more often than predicted. The rates of entry and sharing were also close to equal across periods within a sequence (except that entrants always shared in period 8), and showed little evidence of the early fighting predicted by the reputation-formation equilibrium. However, experienced subjects did behave as the equilibrium predicted, in later sequences (31–end): They hardly ever entered in the first four periods, and entrants never shared (i.e., always fought)

[28] In sessions 5–6, entrant payoffs were 150 for in–fight, 475 for in–share, and 300 for out.

[29] The entry rates are conditional on no sharing previously in the sequence, and are reported separately for subsamples in which another entrant played in or out in the previous period. Sharing rates are conditional on no previous sharing in the sequence and on entry.

Table 8.26. *Entry and fighting rates: P(fighter) = 1/3 (percent)*

			Round (1 = start, 8 = end)							
Experience?	Sequences		1	2	3	4	5	6	7	8
Conditional frequency of entry after previous IN										
		Predicted	—	0	0	0	36	36	36	36
No	1–30	Actual	—	45*	37*	38*	41	25	16	28
No	31–end	Actual	—	30*	33*	26*	40	27	38	61*
Yes	1–30	Actual	—	28*	22	20	40	33	62	60*
Yes	31–end	Actual	—	0	0	0	0	33	75*	82*
Conditional frequency of entry after previous OUT										
		Predicted	—	0	0	0	100	100	100	100
No	1–30	Actual	—	77*	62*	31*	35*	45*	44*	31*
No	31–end	Actual	—	62*	40*	45*	35*	28*	45*	63*
Yes	1–30	Actual	—	37*	31*	18*	21*	24*	62*	70*
Yes	31–end	Actual	—	0	1	2	5*	16*	67*	100
Conditional frequency of SHARE after entry										
		Predicted	0	0	0	19	35	42	56	100
No	1–30	Actual	60*	18	17	17	10	18	20	75
No	31–end	Actual	17*	18*	7	15	5	14	33	98
Yes	1–30	Actual	15	0	0	0	11	8	27	100
Yes	31–end	Actual	0	0	0	0	0	18	43	93

Source: Jung, Kagel, and Levin (1994).
Note: * denotes significant difference (abs(z) > 2) between predicted and actual frequencies.

when there was entry. In periods 6–8, incumbents began to share with increasing frequency, roughly as the equilibrium predicted (though the exact sharing rates were not that close to those predicted). Entrants also began to enter more and more frequently, which was a pattern not predicted.

These data illustrate why it is always wise to run an experiment for many sequences—if you don't do so, you miss the opportunity to see what might happen if the subjects had more time to learn. In this case, the main conclusion from thirty sequences of play—that equilibrium predictions are badly rejected—was overturned by more experience.[30]

[30] This is *not* to say, of course, that the data from experienced subjects in later sequences are the right ones to generalize from to naturally occurring environments. Some economics is about inexperienced people making rare decisions with few learning opportunities (students choosing colleges, people getting married, deciding when to have children, buying houses, and so on). In other situations people have lots of experience

Table 8.27. Late-sequence entry and fighting rates: P (fighter) = 0 (percent)

Experience?	Sequences		Round (1 = start, 8 = end) 1	2	3	4	5	6	7	8
Conditional frequency of entry after previous IN										
		Predicted ($P = 0$)	—	100	100	100	100	100	100	100
		Predicted ($P = 0.2$)	—	0	36	36	36	36	36	36
No	31–end	Actual	—	75*	48	51	74*	58*	56	82*
Yes	31–end	Actual	—	19	21	20	47	67*	89*	100*
Conditional frequency of entry after previous OUT										
		Predicted ($P = 0$)	—	100	100	100	100	100	100	100
		Predicted ($P = 0.2$)	—	0	100	100	100	100	100	100
No	31–end	Actual	—	60*	37*	35*	37*	33*	76	78
Yes	31–end	Actual	—	27	31*	28*	35*	64*	82*	100
Conditional frequency of SHARE after entry										
		Predicted ($P = 0$)	100	100	100	100	100	100	100	100
		Predicted ($P = 0.2$)	0	19	28	31	35	42	56	100
No	31–end	Actual	59*	27*	8	21	29	27	50	79
Yes	31–end	Actual	7	30*	9	21	31	53	69	100

Source: Jung, Kagel, and Levin (1994).
Note: * denotes significant difference (abs(z) > 2) between predicted ($P = 0.2$) and actual frequencies.

Table 8.27 shows data from games where there were no fighter types. Following Camerer and Weigelt, Jung et al. reported predictions assuming that players think 20 percent of the subjects behave like fighter types, denoted $P = 0.2$. (The table also shows predictions from assuming no fighters, which leads to unravelling and chronic entry and sharing.) The complete unravelling idea is clearly refuted. For experienced subjects, entry rates were a little too flat across periods, and rose too much in later periods, compared with the rates predicted by the homemade prior ($P = 0.2$) theory, but they went in the right direction.

Jung, Kegel, and Levin concluded that "the data satisfy a number of qualitative implications of SE [sequential equilibrium] reputation-based arguments supporting predatory pricing"(1994, p. 90)—namely there was less entry and sharing in early periods and when the prior P(*fighter*) was

(learning food preferences, negotiating with business partners and long-term employees). Experiments should tell us about behavior in both kinds of situations, and only long experiments do so.

higher. But they gave "the Kreps and Wilson SE model lower marks than Camerer and Weigelt did" and suggested that different parameter values might explain the difference. Most importantly for industrial organization, they noted that one can find predatory behavior by incumbent firms in suitable experimental environments.

8.3.3 Learning in Repeated Games

Weigelt and I concluded our 1988 paper by suggesting that "The data could be fit to statistical learning models, though new experiments or new models might be needed to explain learning adequately. Indeed, even equilibrium behavior could conceivably be better explained by a heuristic model in which people adapt or learn to approximate sequential equilibrium, than by sequential equilibrium itself" (Camerer and Weigelt, 1988, p. 28).

Camerer, Ho, and Chong (2002a,b) were the first to look seriously at a precise heuristic learning model that could generate the approximate adaptation that we (and others) saw in signaling games. Their theory is an extension of the experience-weighted attraction (EWA) approach described in Chapter 6. It is extended to repeated games by assuming that incumbent firms know how entrants are learning, and take into account what their current actions "teach" the entrants to expect, which creates future gains (see Fudenberg and Levine, 1989; Watson, 1993; Watson and Battigali, 1997). Strategic teaching is measured by two parameters: The fraction of incumbent players who sophisticatedly think the entrants are learning (α) (the rest of the incumbents play adaptively), and the weight placed on future payoffs (see Camerer, Ho, and Chong for details).

In the Jung et al. entry games, 27 percent of the incumbents were estimated to be teachers when subjects were inexperienced and this estimate rises to 55 percent when subjects were experienced, which means players were learning to be more sophisticated (and teach) across sessions. In the Camerer–Weigel trust games, the estimated proportion of teachers was the same as for experienced entry-game subjects, 55 percent. The out-of-sample hit rate of the teaching model was around 70 percent in most sessions, which is not bad. (Even in equilibrium, the hit rate cannot be more than about 85 percent since there is mixing.) An agent form of QRE, a benchmark static model, predicts less accurately than the teaching model in every entry game session, and in eight of ten trust game sessions.

Figures 8.3a–8.3b shows histograms of the relative frequency of lending across periods 1–8 (Figure 8.3a), averaged across ten-sequence blocks (conditional on no default earlier in the sequence), and the conditional relative frequency of default given that a loan was made in a particular period (Figure 8.3b). Loans are made in early periods (i.e., "no loan" is rare) but are increasingly rare in later periods of a sequence; and default is rare in early periods but common in later periods. There is also a slight tendency for these patterns to emerge more strongly across sequences (in particular, no-loan and default in early periods disappear across sequences). Figures 8.4c–8.4d show the predicted frequencies from the teaching model. The model gets the basic within-sequence patterns right and captures the slight trend across sequences, but predicts too few no-loans and defaults in later periods of a sequence.

Although these fits are not too visually impressive, the model persistently outpredicts agent-QRE, which by construction will always outpredict standard game-theoretic concepts. Other parameter estimates show that the EWA teaching approaches improve on other adaptive models (reinforcement and belief learning) because the parameter restrictions those models impose are confidently rejected. So, although the model leaves room for improvement, it is demonstrably more accurate than existing models.

It is conceivable that learning models in which players start with complex repeated game strategies, but learn amongst them using simple learning heuristics, could fit these data even better. The danger is that the repeated game strategies that are selected by the modeler are tailor-made to fit a particular data set. The teaching model creates such repeated-game strategies "automatically" if they are profitable for teachers and builds in sensitivity to the time horizon and other structural parameters. (For example, teachers will behave nicely in trust games, to teach lenders that lending pays, but they will also behave meanly in entry games, to teach entrants that entry does not pay.) The model also has more intuitive properties than type-based equilibrium models.[31]

Summary: Reputation-formation in repeated games is popular in applied economics, and to a lesser extent in political science, but there are amazingly few experiments compared with the large amount of theorizing. Experiments on reputation-formation in trust and entry-deterrence games are remarkably supportive of sequential equilibrium, given how complex the equilibria are. The main effects go in the right direction, but many features of the equilibria (e.g., constant entry rates within blocks of adjacent periods) are clearly wrong. Those who are skeptical of these theories as predictions of what will happen should be impressed that they work at all, in

[31] For example, the type-based model predicts "missed opportunity." In later periods of a sequence, if the reputation-building player does not get a chance to move—if an entrant doesn't enter, or a lender doesn't lend—then in every period after that the entrants should enter (and the incumbents share), and the lenders won't lend. Missing an opportunity to build reputation ruins one's reputation. In the teaching model, a missed opportunity means entrants learn only a little about the relative payoffs of strategies so there is not a sharp decline in the predicted incidence of not lending or not entering.

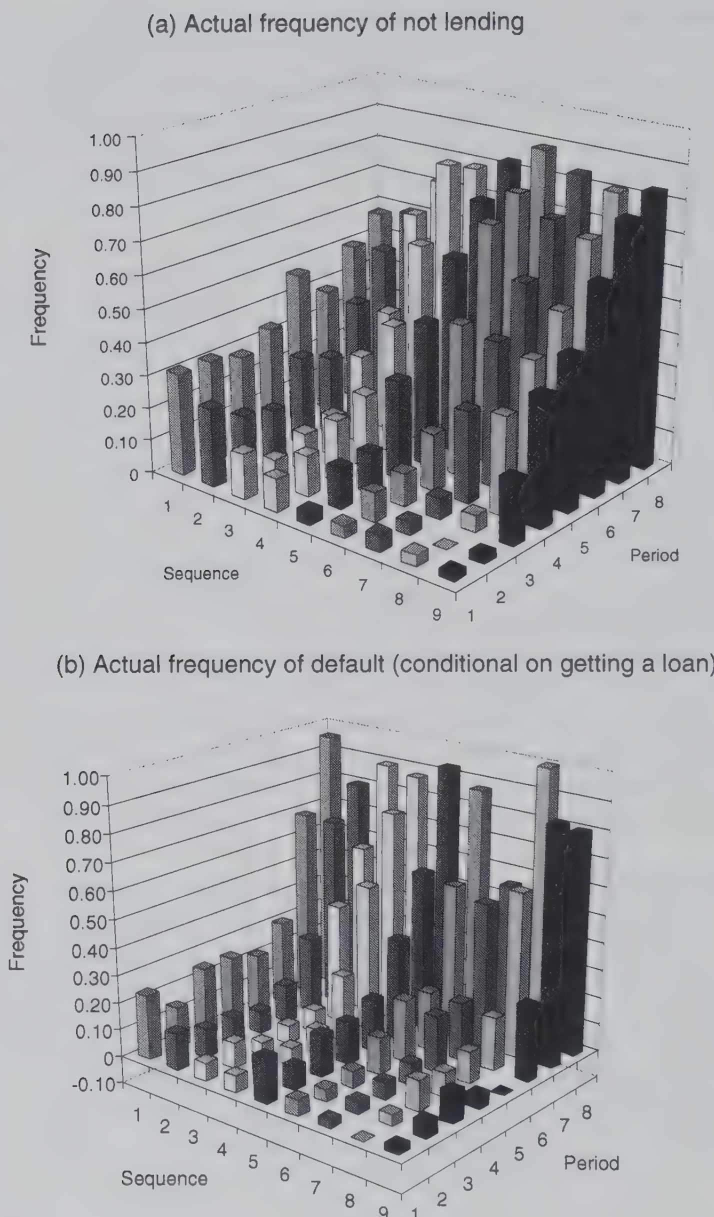
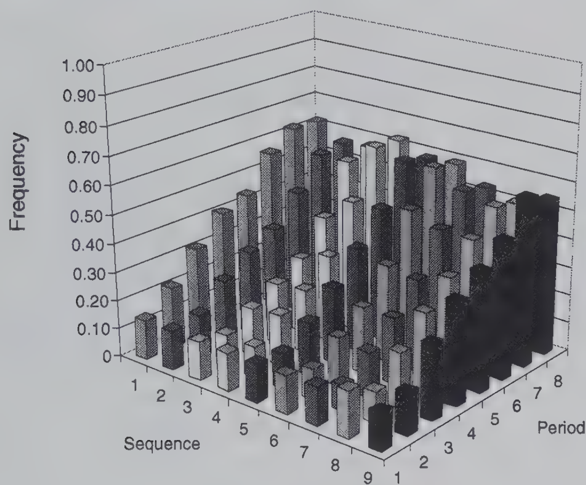


Figure 8.3. *Results and model fits in trust games. Source: Camerer, Ho, and Chong (2002a), pp. 170–71, Figure 5; reproduced with permission of Academic Press.*

(c) Predicted frequency of not lending



(d) Predicted frequency of default

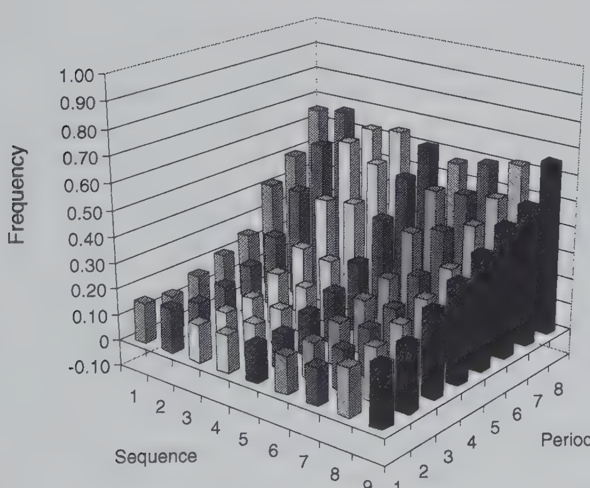


Figure 8.3 (continued)

the same way that people who thought heavier-than-air flight was impossible should be impressed that the Wright brothers' crude airplane even got off the ground.

8.4 Conclusion

In signaling games, one player observes private information and takes an action which may signal their information to a "receiver" player (who knows the distribution of the private information but not exactly what the information is). These games are common in applied social science, particularly economics. They are useful for describing the choice of price, product quality, and other features of products (e.g., warranties). They are also useful for understanding personal actions people take such as getting education and everyday gestures. Signaling games are also popular explanations for why players might appear to behave irrationally. In signaling, players use actions to generate high payoffs *and* to convey information. Sometimes the need to convey information leads to "irrational" behavior (e.g., labor strikes).

A few experiments used abstract signaling games to explore the predictive accuracy of logical "refinements." These refinements deem some equilibria to be more plausible than others, depending on whether out-of-equilibrium beliefs about which type of player deviated from an equilibrium reflect economic incentives for different types to deviate. Results from these games are supportive of very simple equilibrium refinements—particularly sequentiality (requiring receivers to have *some* belief about which types deviated). However, equilibration takes time. And games can be constructed in which players converge to unintuitive equilibria, when historical paths of which types deviated during early play conflict with logical refinement criteria.

Other experiments were modeled after specific economic phenomena—lobbying, equity financing by firms, ratchet effects in production systems with quotas, and limit pricing. In these games, equilibration is often sharp and impressive, although the ratchet and limit pricing games show limited strategic sophistication (players reveal their types unwittingly, allowing their production quotas to be ratcheted upward or entrants to enter).

In repeated games, players who have "normal" types have a strong incentive to mimic unusual types—fighting in repeated entry-deterrence games, and repaying loans in repeated trust games—when that mimicry in early periods benefits them in later periods. Behavior in these games is often remarkably consistent, with very precise, bold predictions of (intuitive) sequential equilibrium, although many subtler predictions of the reputation-formation theories are quite wrong.

The general result from these experiments is characterized thoughtfully by Cooper, Garvin, and Kagel (1997b, p. 555), who wrote:

> game theory is at its foundation a hypothesis about how people behave. In particular, it posits that individuals will attempt to anticipate the behavior of others and respond accordingly. This is the soul of game theory, and the experiments indicate that it is alive and well. What may not be so healthy are the legion of assumptions which have been added to the theory in order to get tractable results. In the rush to get theories which give sensible outcomes and are easily employed by theorists, the reality of how people actually behave may have been left behind. We do not suggest that game theory be abandoned, but rather as a descriptive model that it needs to incorporate more fully how people actually behave.

The theoretical limits that are strained by observed behavior are the limits one might expect. Players are myopic at first, rather than fully anticipating how their choices may unwittingly reveal information. Players do not make immediate (or equilibrium) inferences about what types are revealed by choices, as equilibrium theories expect. And counterintutive properties of equilibria—such as independence of one player's mixing probabilities from their own payoffs—are generally not consistent with what players do.

In Selten's original article on the chain-store paradox (1978) he noted that, in a twenty-period entry game, backward induction leads logically to unravelling. He suggested an alternative "deterrence theory." He wrote (pp. 131–32):

> It is true that the reasoning of the induction theory is very compelling for the last periods of the game. Therefore player A [the incumbent] cannot neglect these arguments completely. He should decide on the basis of his intuition for how many of the last periods he wants to accept the induction argument. . . . Deterrence theory does not yield precise rules of behavior since some details are left to the intuition of the players, but this does not impair the practical applicability of the theory.

I think it is crucial to try to be as precise as possible about the "intuition" players are likely to have, and to make bold predictions about a variety of models. The right way to generalize the equilibrium theories and to formalize the intuition Selten wrote about is to take their basic building blocks and weaken some of the rationality assumptions.

Models such as quantal-response equilibrium or ϵ-equilibrium may explain the correspondence of actual behavior with equilibrium behavior, where it is observed, and explain observed deviations as well. These generalizations of equilibrium are certainly a step in the right direction. However, learning theories will also prove useful in explaining where equilibration occurs, and where it does not. Many of the studies in this chapter (particularly those modeled after specific economic or political games) do include a learning analysis, typically based on belief learning or EWA learning. The influence of learning is magnified when some players understand that others are learning. These "sophisticated" players have an incentive to take short-run actions that, although perhaps costly, will "teach" the players who learn what to expect in a way that benefits the teachers.

Cooper, Garvin, and Kagel also noted that, because the logics of type revelation and inference are not as empirically sharp as equilibria predict, "there may be quite a bit of room for redundant signals to help clarify messages in signaling games, a notion that is routinely dismissed in most economic models" (1997b, p. 573). If some players are boundedly rational and cannot perfectly calculate or infer what choices by others mean, then a variety of communication mechanisms can either exploit these bounds, or work around them for mutual gain.

9

Conclusion: What Do We Know, and Where Do We Go?

GAME THEORY WAS CREATED to provide a mathematical language for describing social interaction. Since then, game theory has become the standard tool in economics, is increasingly used in biology and political science, and is sporadically used in sociology, psychology, and anthropology.

This book describes a large, and rapidly growing, body of experimental data designed to address two major criticisms of game theory: first, that game theory assumes more calculation, foresight, perceived rationality of others, and (in empirical applications) self-interest than most people are naturally capable of; and, second, that in most applied domains there is too much theorizing about how rational people *would* interact strategically, relative to the modest amount of empirical evidence on how they *do* interact. (No science—*especially* the "hard" sciences economists envy most, such as physics, chemistry and biology—has flourished without a very large dose of data-constraining theorizing.)

Both criticisms can be addressed by observing how people behave in experiments in which their information and incentives are carefully controlled. These experiments test how accurately game-theoretic principles predict the behavior. When principles are not accurate, the results of the experiment usually suggest alternative principles. This dialogue between theory and observation creates an approach called "behavioral game theory," which is a formal modification of rational game theory aided by experimental evidence and psychological intuition. The modifier "behavioral" is a reminder that the theory is explicitly intended to predict behavior of people (and collectivities like firms), and draws as directly as possible on evidence from psychology to do so. The eventual goal is for game theorists to accept behavioral game theory as useful and necessary. When that time comes, the central ideas in this book will be part of every standard game theory book and the term "behavioral" can be shed.

An important preface to the summary of results that comes next is that behavioral game theory is *not* a scolding catalog of how poorly game theory describes choices. In fact, the results are uniformly mixed, in a way that encourages the view that better theory is close at hand. In "simple" games with mixed equilibria (Chapter 3) and in more complex games involving signaling of private information (Chapter 8), after a couple of hours of experimental interaction behavior is often surprisingly close to the predictions which have no business working so well. (In some of these cases, such as Rubinstein's email game, see Chapter 5, subjects start out from a bizarre equilibrium and end up close to it only two hours later.) And where game theory clearly describes badly—as in simple bargaining games (Chapter 2) and dominance-solvable games (Chapter 5)—simple parametric modifications promise to be excellent replacements for standard ideas. It appears to be easy to modify theories so self-interested people are human, and infinite steps become finite, while preserving the central principle in game theory—namely, that players think about what others are likely to do, and do so with some degree of thought.

9.1 Summary of Results

Summarizing the results in each chapter, the lessons from hundreds of experiments are the following:

9.1.1 Simple Bargaining Games

In prisoners' dilemma (PD) and public goods (PG) games, players pit their own self-interest against collective interest: Cooperating risks earning less, but yields more if others cooperate. PD and PG games have been studied in thousands of experiments, which show that people often cooperate in these games. Although these games are good models of many social situations, they cannot distinguish between players who are vindictive or negatively reciprocal and those who are simply self-interested (both types of players defect in PD and give nothing in PG). So a wider range of simple games is actually more useful than simply PD and PG for figuring out what sorts of choices people will make to express their emotional attitudes. In the dictator game—not a game at all, just a decision—one player dictates a division of a sum of money between herself and another player. Ultimatum games add a rejection option to the dictator game—a second Responder player can reject the ultimatum offer (leaving both with nothing). In trust games, the amount to be allocated by a dictator (Trustee) is determined by an investment by an

Investor. These games are the minimal building blocks of social interaction (e.g., any bargaining process with a sharp deadline, as is typical in litigation and labor–management negotiations, ends in an ultimatum game). More importantly, the games are tools that can be used to measure the structure of social preferences (i.e., how utilities for money allocations depend on one's own earnings and how much others earn, perhaps depending on the sequence of moves leading up to the allocation choice).

The basic regularities from a large body of experiments are these. Players exhibit a small degree of altruism by allocating 10–20 percent of their endowment to others in the dictator game. However, they also show negative reciprocity by rejecting ultimatum-game offers that are less than 20 percent about half the time. (Generally, players proposing ultimatum offers anticipate this rejection profile and offer 30–50 percent.) In trust games, where one player's investment determines how much a second-mover Trustee can allocate back in a dictator game, players risk about half their investment and earn essentially nothing for their investment (i.e., they get back about as much as they invested). The dictator and trust games also seem to be "weaker" social situations; hence results vary with independent variables substantially whereas ultimatum offers are relatively stable. Although Trustee repayments are often interpreted as evidence of positive reciprocity, studies that compare how much Trustees allocate to first-moving Investors (reciprocating positively) with allocations of equivalent amounts in dictator games suggest that positive reciprocity is weak. But players are more likely to reject an unfair offer in an ultimatum game, compared with their propensity to reject the same offers generated by a random device. Taken together, these data suggest that positive reciprocity is weak relative to negative reciprocity. (As in life, laboratory subjects are quicker to avenge perceived attacks than they are to write thank-you notes.)

In these experiments, there are also substantial individual differences and interesting effects of the way the game is labeled and the cultures in which subjects live. Anthropologists have investigated about a dozen simple, small-scale societies. Pure self-interest predicts that players in ultimatum games offer very little and accept whatever is offered. The fact that these primitive groups behave self-interestedly, but subjects in most developed countries do not, shows that there is a large cultural component to social preference.

Results have generated the most impressive dialogue between observation and theorizing, in the form of several models that posit a social utility function intended to explain *all* the results from different games in a parsimonious way. In "inequality-aversion" models, players care about their own earnings and their share, or the difference in earnings between themselves and others. In reciprocity-based models, players form a judgment about whether another player has treated them fairly, and respond to negative treatment with negative treatment and to positive treatment with positive treatment.

I cannot emphasize enough how important these new models are, and how rapidly they should be incorporated into mainstream economic theory. They show that concern for the payoffs (and behavior) of others *can* be modeled parsimoniously, and those models are precise enough to be falsified by new data. The models also rest on the idea that self-interest is an exception, or a heuristic outcome, rather than the rule in human behavior. Most of the models permit players to behave self-interestedly when the stakes are large, or when they do not know how much others are earning, or when their decisions cannot benefit a player they would like to help without harming somebody they would like to hurt (as in a competitive market). Thus, self-interested behavior can occur in a model in which people do care about others, when the cost of expressing that concern is too high or when the market's competitive structure makes it impossible to sacrifice one's own earnings to reduce inequality. Given the ability of these models to explain *both* self-interest and social preference, there is no good reason why these models should not immediately replace self-interest assumptions in social science as the frontier for interesting new research.

9.1.2 Mixed-Strategy Equilibria

In games with only mixed-strategy equilibria, players are assumed to randomize to prevent others from detecting some pattern in their choices, which can be exploited if the game is played repeatedly. (Or, in the modern interpretation, players may not feel as if they are consciously randomizing, but other players have uncertain beliefs about what these players will do, which are correct on average.) Although behavior in these games is often said to contradict game-theoretic predictions, my reading of the data (see Chapter 3) is quite different: In most of these games players choose strategies with proportions that lie somewhere between equal probability and the mixed-strategy prediction. Furthermore, it is hard to think of a simple theory that improves on the predictions of mixed-strategy equilibrium (though quantal response equilibrium and cognitive hierarchy models are the best bets to do so). The relative success of mixed-strategy predictions is surprising because explicit randomization seems unnatural. And people do exhibit small biases when they attempt to generate random sequences (they alternate too much, although they are closer to independent random draws when they play games against others compared with generating random sequences). The overall picture is that mixed equilibria do not provide bad guesses about how people behave, on average.

9.1.3 Bargaining

In unstructured bargaining, the precise order of moves and allowable message-passing is deliberately left uncontrolled. In structured bargaining, a specific order and message protocol is imposed. Examples of structured bargaining include alternating-offer bargaining with a "shrinking pie," or fixed bargaining costs, and models in which players have asymmetric information about their valuations. The basic finding from these studies is that offers and counteroffers are usually somewhere between an equal split of the money being bargained over and the offer predicted by subgame-perfect game theory. Many of these results are approximately equilibrium outcomes when social preferences are properly specified. But there is also clear evidence from measuring players' attention that they do not instinctively use backward induction algorithms as some game-theoretic equilibria assume, even in games much simpler than those that are routinely played in life.

9.1.4 Iterated Dominance

In many interesting games, an equilibrium choice can be predicted by sufficient iterated deletion of dominated strategies. Games in this class include simple "bets on the rationality of others," "beauty contest" guessing games, imperfect price competition, a patent race game, centipede games, "dirty faces" games adapted from logic, and Rubinstein's "email game." These games lead to a unique predicted strategy choice if dominated strategies are eliminated iteratedly (assume others will not choose dominated strategies, eliminate strategies that are dominated if that assumption holds, and so forth). In all these games, the strategies players choose are correlated with the number of steps of iterated dominance they do. The results can therefore be used to measure, indirectly, the level of iterated reasoning. Most studies indicate that players use two–three levels of iterated dominance; that is, they do not violate dominance in their own choices, but act as if they are unsure that others obey dominance or that others think *they* obey dominance. In experiments with substantial learning opportunity, however, players do converge in the direction of the dominance-solvable outcome, sometimes amazingly so.

9.1.5 Learning

An exciting area of recent research is the careful testing of precise models of how individuals learn, using experimental data. (Evolutionary models have also been tested against data rarely, but are usually strongly rejected relative to individual-level models.) My conclusions about this area of research are strongly influenced by my own work on experience-weighted attraction (EWA) learning (with Teck Ho and Kuan Chong).

There is now little doubt that any of several learning theories can account for the time path of experimental data better than equilibrium concepts can (which, of course, do not predict any change over time at all and hence represent a basic hurdle, but a low one). The central idea in our theory is that players attend to forgone payoffs from strategies they did not choose, but they also pay special attention to the strategies they did choose. The two best-known models, reinforcement and belief learning, are extremes of our hybrid approach in which players pay *no* special attention to what they actually earned (belief learning) or care *only* about what they actually earned (reinforcement learning). There is ample evidence from various sources that both of these simplifying assumptions are wrong enough that they can be easily relaxed in a slightly more general model that allows both intuitions (special attention to actual payoffs and *some* attention to forgone payoffs). As a result, the hybrid EWA model predicts new data and new games more robustly than reinforcement and belief models do, in the sense that the hybrid never forecasts much more poorly, and sometimes forecasts much more accurately.

A simpler variant of EWA theory could turn out to be very useful as a general theory of learning because it fits and predicts well, and it rests on good psychological intuitions. When sophistication is incorporated (the belief that others are learning), it nests equilibrium concepts as special no-learning benchmarks, and can also be used to understand "strategic teaching" in repeated games. However, in games where players do not know forgone payoffs—as in many naturally occurring situations—players must form some guess about forgone payoffs (perhaps using a "payoff learning" module, which may include familiar heuristics such as hill-climbing). The most interesting competitor to EWA is "rule learning," most extensively investigated in games by Dale Stahl. In rule learning, players implicitly keep track of how several different learning rules do, and gradually shift weight toward those that do better.

9.1.6 Coordination

In games with multiple equilibria, a variety of "selection principles" have been proposed to predict (or prescribe) which equilibria are most likely to arise. I distinguish three kinds of coordination games: pure matching games, in which all equilibria have the same payoffs for a player, so that only "focal" or psychologically prominent strategy labels can be used to coordinate on a particular equilibrium; stag hunt or "assurance" games, in which the payoff-dominant equilibrium is often risky; and battle-of-the-sexes (BOS) games, in which different equilibria are preferred by different players. The basic regularities are that coordination failure is common, in two senses: Players do not know how to mutually best-respond rapidly, and they converge toward payoff-dominated equilibria. However, coordination failure is typical and predictable and depends on the amount of risk in choosing a payoff-dominant equilibrium strategy and on some features of adaptive dynamics. Communication also has an interesting effect in these games: It improves coordination when strategic uncertainty can be resolved by preplay announcements (in stag hunt games), but helps resolve BOS only if one player gets to make an announcement. (In BOS, one announcement means taking charge, but two announcements create an argument.)

9.1.7 Signaling

In signaling games with asymmetric information, the sender's action may signal private information to a receiver. These games are quite prominent in many theories of economic and social phenomena—strikes, education, trust-building, entry deterrence—but have not been thoroughly tested experimentally. Evidence is particularly useful because signaling games often have several Bayesian–Nash equilibria, which can be distinguished by whether they obey certain criteria ("refinements") for the plausibility of beliefs players have about which type of sender would make a very unlikely choice. The experiments indicate that players do converge toward signaling equilibria. However, fancy refinement criteria do not help much in predicting which equilibria emerge, and it is not hard to construct games in which less refined equilibria emerge reliably.

Behavioral game theory attempts to explain this broad pattern of regularities using simple models that generalize standard tools. My basic outlook agrees wholeheartedly with the conclusion of Vince Crawford (1997, pp. 235–36)—except for one word: "the results of experiments give good reason to hope that most strategic behavior can be understood via a synthesis that combines elements from each of the leading theoretical frameworks [traditional noncooperative game theory, cooperative game theory, evolutionary game theory, and adaptive learning models] with a modicum of empirical information about behavior." My semantic quibble with Crawford's quote is the word "modicum." Its dictionary definition is "a small portion; a limited quantity" (like the amount of salt used in cooking a dish—you don't want to use too much!). I think the opposite is true—*lots* of evidence is needed, not just a small portion, to suggest which of the leading theoretical frameworks is most suitable, and to refine precisely how to generalize theory.

Some people like to cook straight from a recipe. Here is my personal recipe for using behavioral game theory. Like many cooking recipes, it is personalized (using lots of "home-grown" mathematical ingredients), but it also permits substitution of ingredients and should certainly be tinkered with. Here it is:

Are you interested in wage-setting, bargaining, tax policy, or local public good production, and looking for a parsimonious specification of social preferences? Try the Fehr–Schmidt inequality-aversion approach (the easiest to use, Chapter 2). Functional form: the utility of player i from vector X with payoff components x_k is

$$U_i(X) = x_i - \frac{\alpha}{n-1}\sum_{k\neq i}\max(x_k - x_i, 0) - \frac{\beta}{n-1}\sum_{k\neq i}\max(x_i - x_k, 0).$$

Zero-calorie parametric shortcut: try $\beta = 0, \alpha = 1/3$. Suggested substitutes: Bolton–Ockenfels ERC or Charness–Rabin Rawlsitarian, or some variant of Rabin's theory, which allows true reciprocity.

Do you want a parsimonious alternative to Nash equilibrium, to give a reasonable guess about what might happen in the first period of a game, or as initial conditions for a model of learning? Try the Camerer–Ho–Chong cognitive hierarchy approach (Chapter 5). Functional form: $f(0 \mid \tau)$ percent of players randomize across h strategies. For (integer) $k > 0$ (cap it at eight levels), the fraction $f(k \mid \tau)$ of level-k thinkers choose strategy s_i with probability $P_k(s^i) = I(s^i, s_k^*)$, where

$$s_k^* = \text{argmax}_{s^j} \sum_{m=1}^{h} \pi_i(s^j, s^m)\left[\sum_{c=0}^{k-1} f(c)P_c(s^m) \Big/ \sum_{d=0}^{k-1} f(d)\right]$$

(where $\pi_i(s^j, s^h)$ denotes i's payoff when she plays s^j and others play s^h). (That is, level-k thinkers best-respond against a normalized perceived distribution of lower-level types.) The predicted percentage of play of strategy s_i is $f(0 \mid \tau)(1/h) + \sum_{k=1}^{8} f(k \mid \tau)P_k(s_i)$. Zero-calorie parametric shortcut: try $f(k \mid \tau) = e^{\tau}\tau^k/k!$ (Poisson) and $\tau = 1.5$. Suggested substitute: quantal response equilibrium.

Do you want a model of learning that is flexible across games (Chapter 6)? Try the Camerer–Ho–Chong functional EWA model. Functional form of attraction updating after period t ($\kappa_{it} = 0$, $s_i(t)$ denotes actual choice of i in t):

$$A_i^j(t) = \frac{\phi_{it} \cdot N(t-1) \cdot A_i^j(t-1) + [\delta_{it} + (1-\delta_{it}) \cdot I(s_i^j, s_i(t))] \cdot \pi_i(s_i^j, s_{-i}(t))}{N(t-1) \cdot \phi_{it} + 1}.$$

Fix $N(0) = 1$, calculate the change-detector

$$\phi_i(t) = \delta_{it} = 1 - 0.5\left(\sum_{j=1}^{m_{-i}}\left[\frac{\sum_{\tau=t-W+1}^{t} I(s_{-i}^{j}, s_{-i}(\tau))}{W} - \frac{\sum_{\tau=1}^{t} I(s_{-i}^{j}, s_{-i}(\tau))}{t}\right]^2\right),$$

where W is the (smallest) number of strategies in the support of Nash equilibrium. Compute choice probabilities using the logit rule $P_i^j(t+1) = e^{\lambda \cdot A_i^j(t)} / \sum_{k=1}^{m_i} e^{\lambda \cdot A_i^k(t)}$. (Initial conditions $A_i^j(0)$ can be equilibrium payoffs or expected payoffs from the cognitive hierarchy model.) Zero-calorie parametric shortcuts: try $\lambda = 1$ (for payoffs around \$1); or very large λ (best-response). Suggested substitutes: weighted fictitious play ($\phi = 0.8$) or, in a changing environment where most players get nonzero payoffs, reinforcement with payoff variability.

I'll end this book with a "top ten list" describing ten research questions that are likely to be asked, and perhaps answered, in the next ten years. Of course, forecasting is always perilous, and forecasting scientific direction is especially so. Rational forecasts *should* vary less than the variable being forecasted, so it would be surprising—and disappointing—if the most exciting new work did not veer off in directions dramatically different than those listed below. But lists are compact, useful, good, and irresistible; so here's one.

9.2 Top Ten Open Research Questions

The first five questions are more conservative, in the sense that answers are emerging rapidly. The last five are more speculative.

1. *How do people value the payoffs of others?* Theories of social preferences that answer this question are progressing nicely, in the form of precise inequality-aversion and reciprocity-based approaches (see Chapter 2). A new wave of experiments tests these approaches against each other, and will probably spawn some syntheses of these theories and a sense of which theories work well, and when.
2. *How do people learn?* Learning theories explain how people go from choices in previous periods to a choice in the current period, when they play a game repeatedly (or generalize from similar games) (see Chapter 6 for loving detail). As with social preference theories, there is a healthy variety of theories and they have been tested on many data sets in various ways. An important step would be focusing on simple versions of theories that fit robustly and well to see what their theoretical properties are. A quite different step is to explore learning

in very complex environments with limited information (where simply defining strategies is a challenge).

3. *How do social preferences vary across people and environments (e.g., cultures)?* Basic experimental protocols for measuring altruism (dictator games), negative reciprocity (ultimatums), and positive reciprocity (trust games) are now well established—we know what sorts of variables matter a lot, and which matter a little. The next move is carefully to investigate differences in these games. Amazing work of this sort is being done by anthropologists (e.g., Henrich et al., 2002) studying small-scale societies in far-flung places. (Their work will also sharpen experimental methods and provoke debate, since experimental luxuries taken for granted in university laboratories—such as subject comprehension, contagion among subjects, the influence of numerical training examples, and the ability to carry large sums of cash without being robbed—cannot be taken for granted in Africa and Papua New Guinea.)
4. *What happens when people confront "new" games?* Emerging theories of initial play rely on some combination of limited iterated thinking and quantal response. Simple rules such as choosing a strategy with a high expected payoff if others are randomizing, and iterating in some disciplined way, go a very long way to explaining first-period play (Haruvy and Stahl, 1998; Camerer, Ho, and Chong, 2001). Quantal response equilibrium is appealing because it uses only one parameter, and it avoids many of the most counterintuitive properties of standard equilibrium concepts.
5. *How exactly are people thinking in games?* An emerging literature uses detailed cognitive evidence—measuring beliefs, response times, and attention to payoffs in computer boxes—to figure out what people are thinking. These data create both an econometric challenge (identifying decision rules simultaneously with choices and cognitive measures; see Johnson et al., 2002; Costa-Gomes, Crawford, and Broseta, 2001) and an opportunity to improve model identification by using multiple measures.
6. *What game do people think they are playing?* After an experiment at Caltech on the coordination game stag hunt (Chapter 7), an uppity student wrote on a debriefing form, "I can't believe you guys are still studying the prisoners' dilemma!" Later questioning showed that, since individual rationality can lead to inefficiency in stag hunt (and *does*, if players are self-interested, in the prisoners' dilemma), the trigger-happy student had confused the two. He apparently clustered games in which rationality can (or does) lead to inefficiency into one class, a mistake no careful student of game theory would make. The student's mistake shows that the way in which games are represented or grouped is crucial to understanding what they do. (The student "defected" continually,

thinking his strategy was necessarily the best one, but of course it is not in stag hunt.) The "theory of mental representations" is an emerging body of observations, largely psychological, of how people form mental models or perceptions of elements of the game—who the players are, what the strategies are (including repeated-game strategies), what the monetary or material payoffs are, and so forth. The theory of mental representations maps raw descriptions of social situations into the kinds of familiar games theorists study and the kinds of rules people use to decide what to choose. No unified theory exists (although see Camerer, 1998; and Warglien, Devetag, and Legrenzi, 1999), but even a few crisp ideas would be helpful to explain how people and firms actually construe the interactions they face. Related to this is the important concept of the transfer of learning across games, which is presumably based on perceived similarity (the psychologist Thorndike said transfer depended on "identical elements"), which is in turn related to concepts of analogy, pattern recognition, case-based decision-making (Gilboa and Schmeidler, 2001), and so forth. Interesting recent papers along these lines include Knez and Camerer (2000), Van Huyck, Rankin, and Battalio (2000), Van Huyck and Battalio (2002), and Jehiel (2001).

7. *Can experiments sharpen the design of new institutions?* A large body of theory on how to design rules to achieve objectives ("mechanism design") has bloomed in recent years. But many of these mechanisms impose constraints on individual rationality, and presume rational response to rules, which are sometimes cognitively implausible or difficult for even designers to compute. These mechanisms won't work if people can't figure out whether to participate, or how to react to these rules. As in other domains of economic design, experiments are an efficient way to "test-bed" mechanisms and craft good theory (and hence, practice) of boundedly rational mechanism design.
8. *How do teams, groups, and firms play games?* Virtually all the experiments described in this book maintained a standard hypothesis in game theory, that whether players are genes, nonhuman animals, people, households, work groups, companies, or nation-states makes little difference to whether standard tools are applicable. So most experiments just made individual subjects responsible for their own decisions. But labs are certainly flexible enough to permit experiments in which subjects are, say, teams, to see how decision making differs (see, e.g., Bornstein and Yaniv, 1998, or Kocher and Sutter, 2000). The fact that theories of collective decision making in games are not always well-developed should not inhibit experimenters from using facts to lead theory, rather than follow it.
9. *How do people behave in very complex games?* The experiments described in this book were all constrained by the desire to use a game form that

subjects commonly know, so that equilibrium predictions that proceed from common knowledge assumptions can be compared with behavior. This approach lets theory drive design. Many other economic experiments proceed in the opposite direction (e.g., the vast body of research on complex non-Walrasian markets, which are well understood experimentally but are still not well understood theoretically). Given a wider range of new tools to predict how people are likely to behave in much more complex systems, it is useful to begin to explore them experimentally. A special example is networks, in which players can form links, then bargain or trade (e.g., Corbae and Duffy, 2002).

10. *How do socio-cognitive dimensions influence behavior in games?* Most experiments inherit the strange bias that has existed in game theory for decades—that the identity of subjects, their shared background, and what they have to say to each other before they interact either make little difference (talk is "cheap") or are so powerful that allowing such influences opens a Pandora's Box of causal influences too complex to untangle. Newer experiments allow communication, social cues, and overlapping generations (involving "parents" giving advice to "children").

There is no reason to be anything but optimistic that the synthesis Crawford described above will emerge rapidly, and that provocative findings about each of the ten questions above will emerge. The components of behavioral game theory have been carefully codified and based on evidence and psychological intuition (except, as noted, ideas about mental representation, which have yet to be actively researched). I suggest that, in the next ten years or so of experimentation, we should move past the common practice of comparing results with simple concepts such as Nash equilibrium, because the result of that comparison is now well known. Instead, researchers should compare results to a cognitive hierarchy model, to quantal response equilibrium (QRE), and to learning theories. (A version of EWA learning that includes recursively "sophisticated" players generalizes QRE and would be a reasonable, but tough, benchmark for comparison with data in the next few years.) Behavioral theories will also be useful in applications that are intended to give advice, since in strategic situations the quality of the advice depends crucially on what others are likely to do. If new behavioral tools continue to capture regularity as well as they have in recent studies, and also prove useful in theorizing, they should earn a prominent place in textbook discussions. Then the term "behavioral" will melt away. The sooner it does, the better.