2.8 Theory

The regularities above can be explained by maintaining the assumptions that players maximize utility (and that Proposers believe Responders do so), but allowing the utility to reflect a social preference for what others get. Theorists worry—justifiably—that altering the utility function allows one to explain anything. One unpublished working paper concluded, "This [finding] puts the basis of our modeling on unobservable preferences, and raises the specter of extensive ad hoc modeling with a basis primarily in psycho babble."[29]

Comments such as this do not appreciate how constructive and parsimonious behavioral economics strives to be (and how different it is from psychology in that respect). The goal is *not* to explain every different finding by adjusting the utility function just so; the goal is to find parsimonious utility functions, supported by psychological intuition, that are general enough to explain many phenomena in one fell swoop, and also make new predictions.

Several theories are good candidates. In the earliest theories, players care about the commodity bundles (Becker, 1974) or utilities (Edgeworth, 1881) of others, or get a "warm glow" from acting charitably that is independent of its impact on a recipient (e.g., Andreoni, 1990). Modern theories can be loosely classified into two kinds. Many theories substitute a social utility for a vector of payoffs. "Inequality-aversion" theories assume people care about their own payoff and their relative payoff (see Bolton, 1991, and Chapter 4; Fehr and Schmidt, 1999; and Bolton and Ockenfels, 2000). Charness and Rabin (2002) present a "Rawlsitarian" me-min-us theory in which players care about their own payoffs and the minimum payoff and total payoffs.

Others assume that the ways other players behave affect whether a player cares positively or negatively about that player. Rabin (1993) is the pioneering paper in this approach (cf. Hirshleifer, 1987). Sally (2002b) uses a similar framework, in which players can choose a level of "sympathy" that affects utilities, to study collusion and sympathetic solutions to the lemons problem. Levine (1998) proposed a signaling-type explanation which is closely related to Rabin's. Dufwenberg and Kirchsteiger (1998) and Falk and Fischbacher (1998) extended Rabin's approach to extensive-form games.

Now I sketch several of the theories and compare the intuitions they embody.

[29] Although I've included this sentence because it represents the views some economists articulate about the ability of psychology to inform economics, I've withheld the citation because the sentence was edited out from a later version of the paper.

2.8.1 Pure and Impure Altruism

An obvious starting point for a theory of social utility is that one player's utility increases in the other player's consumption or utility ("pure altruism"). Alternatively, a player may get utility from the act of contributing to others ("impure altruism"; Andreoni, 1990).

Sugden (1982) notes that if players in linear public goods games (discussed at the beginning of this chapter) have altruistic preferences, and concave utility, then when others contribute a lot, the marginal return from the altruism component of utility is low, so altruistic players will *reduce* their contributions. (That is, altruistic giving is "crowded out" if others give.) Theories in which players have reciprocal or inequality-averse preferences (see below) predict the opposite pattern: Contributions and beliefs will be positively correlated. Direct measurement of contributions shows that about half of subjects are "conditional cooperators" (they give more if others give more) and a third or so free ride (Fischbacher, Gächter, and Fehr, 2001; see also Croson, 1999). This strong correlation between beliefs and behavior goes against the altruistic model and in favor of reciprocity theories. Theories of purely altruistic preferences fall short because they do not gracefully explain why players sometimes act negatively toward others (rejecting ultimatum offers, for example) and sometimes act positively (giving generously in dictator games and reciprocating trust). (Or imagine a romantic marriage and bitter divorce, in which the *same two people* treat each other first very positively, then very negatively!) Of course, it is easy to "explain" this switch by flipping the sign of the coefficient on other players' utilities. But a good theory should be able to do this *endogeneously*, based on the structure of the game and on beliefs. (Many of the theories described in the rest of this section do that.)

2.8.2 Inequality-Aversion Theories

Fehr and Schmidt (1999) propose a model of "inequality-aversion" (or envy and "guilt"[30]) in which players care about their own payoffs and the *differences* between their payoffs and those of others. Player i's utility for the social allocation $X \equiv \{x_1, x_2, \ldots, x_n\}$ is

$$U_i(X) = x_i - \frac{\alpha}{n-1}\sum_{k\neq i}\max(x_k - x_i, 0) - \frac{\beta}{n-1}\sum_{k\neq i}\max(x_i - x_k, 0). \quad (2.8.1)$$

[30] Guilt is not quite the right word since it is usually induced by actions, which are not part of these brand of inequality-aversion models. And note that when guilt is created by outcomes alone, like the "survivor's guilt" felt by those who survive accidents their loved ones don't survive, it is considered irrational.

They assume $0 \leq \beta_i < 1$ and $\beta_i \leq \alpha_i$. Intuitively, players feel envy and guilt: They dislike having lower allocations than others (with envy weight α_i) and also dislike (somewhat less) having higher allocations (with guilt weight β_i). Given these utilities, Fehr and Schmidt apply standard equilibrium concepts and derive values of the envy and guilt weights needed to explain experimental results.

In ultimatum games, the Responder should reject an offer less than $\alpha_R/1+2\alpha_R$ (where α_R denotes the Responder's envy weight). Proposer offers depend on the guilt weight β_P and the Proposer's guess about the distribution of rejection thresholds. In games with proposer competition, several Proposers make offers to a single Responder. As noted above, Proposers quickly and reliably begin offering almost everything to the Responder. Inequality-aversion can explain this result. Players who are outbid get no money and feel envious; if they outbid the highest-bidding player they can earn money and feel guilt rather than more envy. If $\beta_i \leq \alpha_i$ they prefer to bid higher and, in equilibrium, they give the Responder almost everything. Note that this result does *not* depend on the number of Proposers (as long as there are at least two).

Güth, Marchand, and Rulliere (1998) studied Responder competition. A single Proposer (such as a monopolist seller) makes an offer and two or more Responders indicate whether they will take it or not. If more than one says yes, the Proposer randomly allocates the offer. If there are two or more Responders, Fehr–Schmidt prove there is an equilibrium in which Responders will accept any offer, the Proposer will offer zero iff $\beta_P < n-1/n$, and the highest (perfect) equilibrium offer is

$$\min_{k \in \{2,...,n\}} \alpha_i/(\beta_i + 2\alpha_i + (n-1)(1-\beta_i)).$$

In contrast to Proposer competition, there is a role for the amount of competition in this game because increasing n increases the denominator of the minimum, lowering the highest possible offer. Somebody should do an experiment comparing the effects of the number of competing Proposers (which shouldn't matter according to Fehr–Schmidt) and the number of competing Responders (which should).

In public goods contribution games, both contribution and costly punishment can emerge if players are guilty and envious enough. Suppose player i contributes $g_i \in [0, y]$, $G \equiv \{g_1, g_2, \ldots, g_n\}$ and earns $x_i(G) = y - g_i + m\sum_{k=1}^{n} g_k$, where m is the marginal private return from the public good. In a two-stage version of the game with punishment, after contributions are made all players are informed about G. Player i can punish k one unit with a punishment p_{ik}, which costs the punisher $c < 1$.[31] The standard model with

[31] The payoff function in the public goods game with punishment is $x_i(G) = y - g_i + a\sum_{k=1}^{n} g_k - \sum_{k=1}^{n} p_{ki} - c\sum_{k=1}^{n} p_{ik}$.

no social preference predicts complete free riding in the public good game ($g_i = 0$) and no costly punishment in the two-stage game ($p_{ik} = 0$). As noted at the beginning of the chapter, although repeated public goods games do converge close to complete free-riding, in games with punishment there is a small amount of punishment that generates convergence to substantial rates of contribution. (Note that the effectiveness of punishment might also be linked to the cost to the punisher—punishees might feel worse if the punisher went out of her way to teach them a lesson.)

Fehr and Schmidt prove several properties of behavior in public goods games when there is envy and guilt. Players with $\beta_i < 1 - a$ will always free ride; if k players free ride and $k > a(n-1)/2$, then everyone free rides; if there are a small enough number of free riders and enough inequality-averters of the right sort ($(a + \beta_k - 1)/(\alpha_k + \beta_k) > k/(n-1)$ for players with $\beta_k < 1 - a$) then the free riders contribute nothing and others contribute some amount $g_k \in [0, y]$. In the game with punishment, suppose there are n^* players who are sufficiently guilty ($\beta_i \leq 1 - a$) and also sufficiently envious ($\alpha_i > c(n-1)(1+\alpha_i) - c(n^* - 1)(\alpha_i + \beta_i)$). These "conditionally cooperative enforcers" guarantee the possibility of a perfect equilibrium with some positive contribution. The n^* enforcers will be willing to punish defectors (which reduces the inequality in their payoffs since it hurts the punishee more than it costs the defector), which supports cooperation.

Bolton and Ockenfels (2000) proposed a very similar model of inequality-aversion that they call "ERC" (for equity, reciprocity, and competition). In ERC, players care about their own payoffs and their *relative* share, so

$$U_i(X) = U\left(x_i, \frac{x_i}{\sum_{k=1}^{n} x_k}\right). \tag{2.8.2}$$

In ERC, players strictly prefer a relative payoff that is equal to the average payoff $1/n$ (i.e., $v_2^i(x_i, 1/n) = 0$, $v_{22}^i < 0$), which means players will sacrifice to move their share closer to the average if they are either below or above it.

Like Fehr and Schmidt, Bolton and Ockenfels combine standard game-theoretic concepts with their specification of social preference and prove several propositions about what will happen in different games which match the empirical facts nicely. In dictator games, people will give between half and all of the pie. (This result does not come from Fehr–Schmidt preferences unless concave utilities are included.) In ultimatum games, Responders will reject zero offers all the time, the rejection rate will fall with increasing percentage offers and decrease in the pie size (fixing percentage offer), and Responders will never reject 50 percent offers. Ultimatum offers will be less than half, and will be greater than Dictator allocations. In the three-player combination ultimatum–dictator games of Güth and Van Damme (1998), ERC predicts that the allocation to the inactive Recipient will be ignored, which is roughly what is observed.

2.8.3 Fairness Equilibrium (Rabin)

Rabin's (1993) "fairness equilibrium" approach is motivated by the fact that people both behave nicely toward those who treat them nicely, and behave meanly toward those who harm them. A model that can explain these stylized facts must include players' judgments of whether others are nice or mean, so Rabin adopts the psychological games framework of Geanakoplos, Pearce, and Stacchetti (1989) in which utilities can depend directly on players' *beliefs*.[32] Suppose there are two players, denoted 1 and 2. Denote strategies by a_i, i's beliefs about the strategy of the other player by b_{3-i} (i.e., b_2 is 1's belief about what 2 will do, and b_1 is 2's belief about what 1 will do), and beliefs about beliefs by c_i. The central construct in Rabin's approach is a player's "kindness" (and perceived kindness).

To assess kindness, take a player's point of view, and fix their belief about what the other player will do. For example, suppose player 1 has the belief b_2 about what player 2 will do. Then from player 1's point of view, her own choice is an allocation to player 2 of a possible payoff out of the set of possible payoffs we denote $\Pi(b_2)$. Call the highest and lowest of these payoffs, for player 2, $\pi_2^{\max}(b_2)$ and $\pi_2^{\min}(b_2)$. Define an equitable (or fair) payoff by $\pi_2^{\text{fair}}(b_2)$. (Rabin assumes $\pi_2^{\text{fair}}(b_2)$ is the average of the highest and lowest payoffs, excluding Pareto-dominated payoff pairs, but this particular definition is not essential to most of what follows.) Then player 1's kindness toward 2, which depends on her actual choice a_1, is

$$f_1(a_1, b_2) = (\pi_2(b_2, a_1) - \pi_2^{\text{fair}}(b_2))/(\pi_2^{\max}(b_2) - \pi_2^{\min}(b_2)). \qquad (2.8.3)$$

Thus, kindness is a fraction of the way above or below the fair point (scaled by the range of payoffs player 1 could have dictated) that player 2's actual payoff lies. A positive $f_1(a_1, b_2)$ is kind because it means player 2 got a payoff higher than the fair one; a negative value is mean because player 2 got a lower than fair payoff.

For player 1 to decide how kind player 2 is being toward her, she forms a perceived kindness number,

$$\tilde{f}_2(b_2, c_1) = (\pi_1(c_1, b_2) - \pi_1^{\text{fair}}(c_1))/(\pi_1^{\max}(c_1) - \pi_1^{\min}(c_1)). \qquad (2.8.4)$$

This perceived kindness is a conjecture about what player 2's own kindness toward 1 is. To form this conjecture player 1 must guess what player 2 thinks player 1 will do, so player 1's iterated belief plays a role.

[32] This generalization allows phenomena such as surprise, in which players may actually get pleasure from having their belief that they won't get a gift (for example) proved wrong. See Ruffle (1999) for an application.

Table 2.12. *Prisoners' dilemma with social preferences*

		Cooperate	Defect
Cooperate	C	$4 + 0.75\alpha, 4 + 0.75\alpha$	$0 - 0.5\alpha, 6$
Defect	D	$6, 0 - 0.5\alpha$	$0, 0$

Source: Rabin (1993).

Rabin assumes that player 1's social preferences are

$$U_i(a_i, b_j, c_i) = \pi_i(a_i, b_j) + \alpha\tilde{f}_2(b_2, c_1) + \alpha\tilde{f}_2(b_2, c_1) \cdot f_1(a_1, b_2). \quad (2.8.5)$$

That is, players care about their monetary payoffs (the first term), whether they are being treated kindly or not (the second term), and the product of the kindness they expect and their own kindness. The weight α simply weighs fairness utilities against money.[33]

By multiplying a player's kindness, and the kindness she expects, this social preference function captures reciprocity motives—players prefer to be nice (positive kindness) to people who act nice to them, and to be mean (negative kindness) toward people who act mean.

Rabin then applies an equilibrium concept in which players maximize social utilities, and their beliefs are rational expectations of what actually happens—i.e., $a_i = b_j = c_i$ (beliefs about what others will choose are correct, and beliefs about what others will believe are correct also). He calls this a "fairness equilibrium." Empirical bite comes from the assumption that the fairness term becomes relatively less important as the payoffs π_i get larger.

An easy example is the prisoners' dilemma. Table 2.12 shows payoffs adjusted for fairness. The (C,C) money payoffs are (4,4), the (D,D) payoffs are (0,0), and the (C,D) payoffs are (0,6).

Consider the row player's payoffs. When α is large enough, she feels worse choosing C when she expects her opponent to choose D, because D is a mean choice by the column player. Oppositely, if she expects the column player to choose C then she earns $4 + 0.75\alpha$ from reciprocating cooperation, because doing so is nice (it gives column a better payoff than if row defected), and column is also being nice by not taking advantage and defecting.

Thus, mutual cooperation is a fairness equilibrium when α is large enough and the monetary payoffs relatively small. In this view, PD is a coordination game in which players try to coordinate their emotions or levels of niceness. This jibes nicely with the experimental observation that

[33] The weight α isn't in Rabin's original approach, but I've added it to highlight how self-interest can be a special case.

Table 2.13. *Chicken with social preferences*

	Dare	Chicken
Dare	$-2,-2$	$2, 0-0.5\alpha$
Chicken	$0-0.5\alpha, 2$	$1+0.75\alpha, 1+0.75\alpha$

Source: Rabin (1993).

players who expect others to cooperate are more likely to cooperate also (i.e., the belief in C and the tendency to choose C are correlated), which is hard to explain in many models.

Another interesting example is "chicken," with fairness-adjusted payoffs shown in Table 2.13. The Nash equilibria ($\alpha = 0$) are (C,D) and (D,C)—one player backs down and chooses C when the other is expected to choose D. However, when α is large enough (above 4), a player who expects the other to D(are) would rather choose D, earning -2, than choose C(hicken) and let the mean player take advantage, suffering a loss of 0.5α. Similarly, when $\alpha > 4/3$ then reciprocating the choice of Chicken with Chicken pays because it repays the other player's kindness. Thus, if fairness effects are large then (C,C) and (D,D) are fairness equilibria. In this lovely example, the set of outcomes allowed by fairness is completely opposite those required by standard equilibrium. The game also captures both the mutually happy and the mutually angry aspects of social preference, like a couple who sacrifice to please each other, only to end up in an ugly *War of the Roses* divorce in which their sole goal is to harm the other person who harms them. Some experimental evidence is also supportive of the (C,C) and (D,D) outcomes (Rutström, McDaniel, and Williams, 1994).

Rabin proves several propositions about the existence and characterization of a fairness equilibrium. He also describes an application to monopoly pricing: Firms cannot extract all the surplus because a buyer will reciprocate a price that seems too high by withholding demand.[34] An application to gift exchange in employment also shows how a worker will reciprocate a high wage with high effort.

2.8.4 Extensive-Form Fairness Equilibrium

Dufwenberg and Kirchsteiger (1998) extended the Rabin framework to extensive-form games. They define strategies to be behavior strategies which assign to each of a player's information sets a probability distribution of possible choices at that information set. One difference from Rabin is that

[34] There is some evidence of demand withholding in monopoly pricing experiments (e.g., Ruffle, 2000).

Pareto-inefficient strategies are defined as those that yield weakly less for all players than some other strategy *for all subgames.* This is a subtle departure from Rabin, who defines inefficient strategies as those that yield less for everyone on the equilibrium path (i.e., for the player's beliefs about the others' strategies).

Their social preferences differ from Rabin's slightly. First they define kindness functions as differences between payoffs and fair payoffs, so i's kindness to j is

$$f_i(a_i, b_j) = \pi_j(a_i, b_j) - \pi_j^{\text{fair}}(b_j), \quad \text{where } \pi_j^{\text{fair}} = (\pi_j^{\max}(a_i, b_j) + \pi_i^{\min}(a_i, b_j))/2$$

(where the max and min payoffs are defined for efficient strategies only). Their utility function is

$$U_i(a_i, b_j, c_i) = \pi(a_i, b_j) + Y_i \sum_{j \neq i} f_i(a_i, b_j)\tilde{f}_j(b_j, c_i).$$

Their specification of social preferences differs from Rabin in three ways: They do not scale the kindness terms by the range of $\pi^{\max}$ and $\pi^{\min}$ as Rabin does; the kindness of player(s) j does not enter directly; and the product of i's kindness and i's perceived kindness is summed across players j. They prove the existence of a sequential reciprocity equilibrium (SRE) in which players maximize social utility, and strategies match beliefs. They also show that using normalized kindness functions and a belief-dependent definition of efficiency destroys the guarantee of existence.

Dufwenberg and Kirchsteiger apply the model to sequential prisoners' dilemma (PD) and ultimatum games. In sequential PD, the second player always reciprocates defection and she reciprocates cooperation if her value of Y_i is high enough. The first player's behavior is not sharply restricted, unfortunately. For example, if $Y_2 > 1$, then player 1 either cooperates for any value of Y_1, defects if $Y_1 > 1$, or cooperates with probability $(Y_1 - 1)/2Y_1$.

Behavior in ultimatum games is also somewhat indeterminate. The Responder has thresholds of offers she always accepts and always rejects (which depend on the Responder's parameter Y_R). There is always an SRE in which the Proposer offers the minimum acceptable amount, but if the values of Y_i are large enough there is also an SRE in which the Proposer's offer is rejected.

Falk and Fischbacher (1998) also use psychological game theory to incorporate reciprocity in extensive-form games. Their innovative approach departs from Rabin's in two interesting ways—measuring intentions more directly and (like Dufwenberg and Kirchsteiger) measuring kindness by differences across players' payoffs. Since they are interested in applications to extensive-form games, they define emotional terms at each node. Denote i's strategy by s_i, i's belief about j's choice by s_j', and i's belief about

j's belief about i's choice by s_i''. As in Rabin, fixing beliefs and second-order beliefs, i regards j's choice as allocating a payoff to i, from the set of possible payoffs. Then their outcome term $\Delta(n)$ at node n is defined by $\pi_i(n, s_i'', s_j'') - \pi_j(n, s_i'', s_j')$. That is, players measure fair treatment by the difference between their own expected payoffs and the other player's payoff. This is a fundamentally different approach than Rabin's, which judges kindness by how much a player gets relative to some "fair" payoff for themselves (for example, the average of their own possible payoffs). In Falk and Fischbacher's approach, kindness as measured by $\Delta(n)$ is multiplied by a factor measuring how *intentional* j's kindness was. The intention function $\Omega(\pi_i, \pi_j, \pi_i^0, \pi_j^0)$ compares a set of possible payoffs π_i^0, π_j^0 with alternative payoffs π_i, π_j. Roughly speaking, intention is equal to 1 when player j gives more to i than to herself and she could have given i less, or when she gives less to i and could have given more. Intentions are equal to a value ϵ_i if j gives i more than to herself but could have given even more, or if j gives i less but could have given even less. Fixing a node and set of beliefs s_i', s_i'', the intention factor $\nu(n)$ is the maximum of the intention factors defined above across all alternative payoffs π_i, π_j available at node n.

Denote the node following n, on a path to endnode f, by $\nu(n, f)$. Then player i's reciprocation of j is measured by $\sigma(n, f) = \pi_j(\nu(n, f), s_i', s_i'') - \pi_j(n, s_i', s_i'')$. This reciprocation is the difference between what player j expected at node n, and what i actually "awards" at the subsequent node $\nu(n, f)$. Then player i's utility at an endnode f is given by

$$U_i(f) = \pi_i(f) + \rho_i \sum_{n \to f} \nu(n)\Delta(n)\sigma(n, f), \tag{2.8.6}$$

where $n \to f$ is the set of nodes n that precede f (either directly or indirectly).

This utility includes both "material payoffs" $\pi_i(f)$ and "emotional" payoffs, which sum up the products of intention, kindness, and reciprocation across nodes on the path to f. The factor ρ_i is the weight i places on emotional payoffs (if this is zero, the self-interested model results as a special case). The intention term is an important innovation. When $\epsilon_i = 1$, then $\nu(n)$ is always 1 and the model reduces to one in which players care only about differences in payoff allocations, and not about possible differences on unchosen paths (i.e., they don't care whether players could have treated them worse or badly).

Falk and Fischbacher apply standard equilibrium concepts with the additional restriction (as in Geanakoplos et al. and Rabin) that beliefs are correct in equilibrium ($s_i'' = s_j' = s_i$). They apply their model to several games mentioned in this chapter, to see how well its predictions correspond to the data. They get a very impressive array of precise predictions that loosely correspond to stylized facts.

For example, in the prisoners' dilemma they predict less cooperation in the simultaneous-move game than in sequential games, and predict cooperation following cooperation in the sequential game (if ρ_i is large enough). In dictator games, the Dictator offers $d^* = \max(0, 0.5 - (1/2\epsilon_1\rho_1))$; if the outcome concern parameter ϵ_i and reciprocation taste ρ_i are high enough, Dictators will give something (but never more than half).

In ultimatum games, offers are the maximum of the Dictator offer (with $\epsilon_i = 1$) or $c^* = (1 + 3\rho_2 - \sqrt{1 + 6\rho_2 + \rho_2^2})/(4\rho_2)$. Acceptance thresholds are $c/[\rho_2(1-2c)(1-c)]$, which start at zero and rise in a convex function toward 1. In the ultimatum game where offers are made by a third party (as in Blount, 1995), the acceptance threshold is $c/[\epsilon_2\rho_2(1-2c)(1-c)]$, which is larger if $\epsilon_2 < 1$. (Intuitively, the intention measurement term ϵ_1 is in the ultimatum game but can be lower in the random-offer version, reflecting the Responder's awareness that the beneficiary of a low offer had no mean intentions if the offer was generated by somebody else.)

The model makes a sharp prediction about ultimatum and "best-shot" public goods games. Compare the mini-ultimatum game, in which player 1 can choose a left branch with payoffs (5,5) and (0,0) or a right branch with uneven payoffs (8,2) and (0,0). Now replace the equal payoff node (5,5) with an unequal payoff node (2,8), creating a sequential battle of the sexes (BOS). Theories that evaluate only the social utility of endnode payoffs regard player 2's choice between the (8,2) and (0,0) nodes in both games as identical (what's on the unchosen branch doesn't matter). As Falk, Fehr, and Fischbacher (in press) showed, forgone payoffs *do* matter. In the Falk–Fischbacher model, the probability of player 2 choosing the (8,2) outcome from the right branch is $\max(1, 5/12\rho_2)$ in the mini-ultimatum game and $\max(1, 5/12\epsilon_2\rho_2)$ in the best-shot game. Thus, for concern parameters $\epsilon_2 < 1$ players are more likely to accept unequal payoffs in the best-shot game (BOS) because player 1's intentions are not so mean in that game (as Andreoni, Brown, and Vesterlund, 2002, confirmed).

2.8.5 Comparing Approaches

Since many of the theories just described make similar predictions, a busy and important area of current research is finding games in which the theories differ and testing them.

ERC (Bolton–Ockenfels) and guilt–envy (Fehr–Schmidt) theories of inequality-aversion differ in two ways: ERC assumes players care about relative shares whereas guilt–envy assumes players care about absolute differences; and ERC does not compare one player's payoffs with each other player's (it sums other players' payoffs) whereas guilt–envy does.

Both features of ERC appear to be the wrong modeling choices. Consider a payoff allocation to three players $(x, x-\epsilon, x+\epsilon)$. ERC predicts that

Table 2.14. *Two games that distinguish social preference theories*

	Responder action			Predicted rejection frequency			
Offer	Accept	Reject	Rejection frequency	Bolton—Ockenfels	Fehr—Schmidt	Dufwenberg—Kirchsteiger	Falk—Fischbacher
Equal	5,5	0.5,0.5	—	—	—	—	—
Unequal	8,2	0.8,0.2	0.38	None	Some	Some	Some
Equal	5,5	3,3	—	—	—	—	—
Unequal	8,2	6,0	0.19	None	None	Some	Some

Source: Falk, Fehr, and Fischbacher (in press).

the preference of the first player (who receives x) is independent of ϵ; since she gets an equal share of the total, she is neutral toward total-preserving spreads in payoffs of others. Guilt–envy predicts utility will fall as ϵ increases (if guilt is weaker than envy). Charness and Rabin (2000) report that preferences do fall with ϵ as guilt–envy theory predicts, contrary to ERC.[35] And in public goods games with punishment, guilt–envy theory predicts, correctly, that players punish the biggest free riders to reduce the largest envy gaps, whereas ERC makes no prediction about *who* is punished.

The difference between relative share and absolute difference specifications can also be explored with variants of ultimatum games, shown in Table 2.14. In the game in the top panel, a Proposer offers (5,5) or (8,2). If the Responder rejects, the payoffs are 10 percent of the original offer. Since the relative shares are the same whether Responders accept or reject, and they earn less money by rejecting, ERC predicts they should never reject. The linear guilt–envy specification predicts indifference (but it is easy to explain rejections by allowing concave utilities for money, guilt, and envy). As Table 2.14 shows, almost 40 percent of unequal offers are rejected, contrary to ERC.

Another important distinction among theories is whether utilities of terminal-node payoffs are separable from the path through the tree and from payoffs on unchosen branches. Inequality-aversion theories assume separability. Separability is analytically useful because it allows a modeler simply to substitute social utilities at terminal nodes and then use standard concepts (e.g., subgame perfection) to derive equilibria.

Some evidence suggests separability is systematically violated. As noted above, players are less likely to reject ultimatum offers generated at random compared with those of Proposers who benefit from lopsided offers

[35] In contexts such as social processes of taxation, voters' reaction to this sort of "neutral" income inequality is important because it will determine how middle-class voters in the middle of a national income distribution will react to attempts to reduce income inequality.

(Blount, 1995). Similarly, there is (weak) evidence that Trustees in trust games repay more when their stake was generated by an initial investment (compared with a control dictator game). Experimental results of Falk, Fehr, and Fischbacher (in press) (see Table 2.6), discussed in detail above (see also Brandts and Sola, 2001), also show modest violations of separability which have plausible psychological interpretations. The bottom panel of Table 2.14 shows a modified ultimatum game which tests separability further. If the Responder rejects, two units are subtracted from both players' payoffs. Since this subtraction lowers the relative share of unequal offers, and keeps the difference in payoffs constant, both ERC and guilt–envy theories predict Responders should never reject unequal (8,2) offers. But they do so about 20 percent of the time. Dufwenberg–Kirschteiger and Falk–Fischbacher's theories allow such rejections.

Other evidence suggests separability is a good approximation. For example, Bolton, Brandts, and Ockenfels (1998) found that whether one player takes a cooperative move (sacrificing to help the other) is roughly independent of whether the first player sacrifices or not. And, as noted above, the difference between Trustee repayments and Dictator allocations from equal sums is not very large (and is zero in one study). Although these findings are in urgent need of further study, they suggest that players sometimes exhibit stronger social preferences when they are punishing or rewarding actions of others. Charness and Haruvy (2002) estimate a general model that tests many of the theories above, and they find support for several forces, especially reciprocity, in gift exchange settings.

Inequality-aversion theories also require a careful specification of *which* other players' payoffs a person is comparing herself to. This is an important question that is rarely discussed. A natural assumption is that a subject in an experiment compares herself with others she is playing with, but there are other possibilities—Why not compare with all others in the same experimental session? Or in all sessions of the same experiment?—which have different implications.[36] Exploring the boundaries of social comparison will prove important in generalizing these results to applications such as job titles and firm structure.[37]

[36] For example, why not worry about other players in the same experimental session? Consider two players in an ultimatum game in an experimental session with N other pairs. If N is very large, and a player cares about the payoffs of all $N-1$ other players (who are likely to earn some money), then her relative share will fall with N, and envy will increase, which should guide her toward self-interest.

[37] There are well-known industry-wide and firm-specific wage differentials (e.g., janitors at a fancy law firm where top attorneys are well paid are paid more than janitors at a fast-food restaurant). Wages are presumably driven by social comparison with those in the same firm. One response is to outsource low-cost work (e.g., the janitors at Stanford University work for a private company, not for Stanford), which has implications for firm structures.

Summary: Several parsimonious theories have been proposed to explain a broad sweep of data with a single model of social preference (and some parametric flexibility). In theories of pure and impure altruism, players care about the utilities of others or get satisfaction from treating others kindly. In inequality-aversion approaches, players care about earning more money and earning the same as others. In Rabin's reciprocal approach, players make a judgment about whether another player's action is kind or mean (i.e., gives the target player a good or bad relative payoff) and have a taste for reciprocating both kindness and meanness with the same. Rabin's theory, developed for normal-form games, was extended by Dufwenberg and Kirschsteiger and by Falk and Fischbacher.

Most data support either inequality-aversion or the reciprocal approaches. Altruism theories do not explain both negative and positive behavior toward others without crudely changing the signs of coefficients exogeneously. Inequality-aversion theories are very promising but predict a kind of separability—utilities of terminal-node allocations are independent of how those allocations arose, and of allocations from unchosen alternatives—that is psychologically suspect and violated in several experiments.

I like the reciprocal approaches because they get the psychology right. Furthermore, the main argument in favor of inequality-aversion is analytical simplicity. But the history of economic thought shows how quickly the profession can learn to use a tool that seems too unwieldy at first. After all, applying the reciprocity approaches to interesting games cannot possibly be more difficult than the incredibly complicated mathematics now being done in areas such as asset pricing, macroeconomics, epistemological game theory, and econometrics. At the same time, when the empirical dust settles, both approaches may prove useful in different technical applications, much as theories of consumer choice and production sometimes use Cobb–Douglas functions and sometimes use CES (constant elasticity of substitution) or linear expenditure.

2.9 Conclusion

This chapter describes experimental regularities in simple bargaining games, an area of research that is growing rapidly because the games are so useful for shaping theories of social preference. In dictator games, players offer modest sums to others (about 20 percent of the amount being divided). When others have entrusted the Dictators with large sums, the Dictators tend to return more (and trust is repaid, on average, although results vary widely). Proposers offer nearly-equal splits in ultimatum games (about 40 percent) for fear of having low offers rejected. Offers around 20 percent *are* rejected half the time. We know a rejection is a punishment by the Responder to a Proposer who has behaved unfairly because Responders are more likely to accept offers when they don't know how much the Proposer is earning, when uneven offers are generated by chance, and when rejecting does not hurt the Proposer as much as it hurts the Responder.

It is sometimes said that fairness is simply a label for the behavior rather than an explanation. This is no longer true because the many experimental results *do* suggest an explanation: Fairness is a judgment people make about an action players take or its consequences, and that judgment affects their preferences for actions and allocations. Whether an action is judged fair, and what players do as a result, respond to observable variables in intuitive ways. It is fair to keep more if you became the Proposer by winning a contest, or if keeping more is the only way a Proposer can play a second time and earn more money.

The fabric of fairness perceptions and their effects on behavior have been explored in studies that control or measure five kinds of variables: methodological, demographic, cultural, descriptive, and structural.

Methodological studies have concentrated on the amount being divided, whether the game is repeated or not, and whether experimenters know exactly what each subject did. There is little evidence that stakes matter much once subjects are paid something (although Responders reject larger amounts, and reject percentage offers less often, when stakes rise). Repeating the game does not matter much. Anonymity of subjects' actions from the experimenter lowers Dictator allocations in some experiments but does not affect ultimatum behavior.

Despite much interest in whether or not people in different demographic groups play differently, only the effect of academic major has replicated reliably (economics majors offer and accept less), and often it has no effect. Gender effects on offers and rejections have not proved reliable, except that women are more price sensitive in punishing unfairness and allocating money. Age effects are important because young children are actually very self-interested, which implies that deviations from self-interested strategic predictions are learned as children grow up, and are not mistakes that more learning will undo.

Perhaps the most intriguing experiments are those that cross cultures. For example, the Machiguenga farmers in Peru are one of the least-educated groups ever studied . . . and also conform *most* closely to the game-theoretic model (based on self-interest). Comparing cultures is also informative because the degree of market integration is positively correlated with equality of offers across a dozen or so small-scale societies, as if market exchange either requires or cultivates norms of equal sharing.

Descriptive variables can matter. For example, calling an ultimatum game a buyer–seller exchange lowers offers. These effects are like other kinds of context-dependent etiquette. In Beijing it is considered acceptable to spit on the sidewalk and disgusting to blow your nose into a handkerchief that you then carry around. In Los Angeles the opposite is true. Similarly, in bargaining, monopoly sellers are allowed to gouge buyers by charging a high price which leaves little consumer surplus, but two people jointly claiming money are expected to share.

Structural variables that add moves to the game have proved most interesting. Inducing a sense of entitlement, by allowing the winner of a contest to be the Proposer, lowers offers. If Responders aren't sure how much Proposers get, they accept less. Games with multiple players suggest that Responders care about whether Proposers are unfair to *them*, but don't care much how Proposers treat others. Competition among Proposers or Responders drives offers to extremes.

The effects of multiple players and limited information suggest a general conjecture about bargaining and markets. In two-person games with perfect information about how much each side is earning, fairness concerns loom largest. As players are added, competition can create very lopsided allocations. And, as the Responder's knowledge about the Proposer's gain becomes hazier, Responders become more tolerant of low offers (since they aren't sure how unfair the Proposer is being). The concern for fairness evident in two-player perfect information games therefore disappears in large markets. That does *not* mean traders in such markets do not care about fairness per se. They may care, but they behave self-interestedly because they aren't sure whether others are being fair and can't easily punish unfairness. A competitive market is simply a place in which it is hard to express your concern for fairness because buying or selling (or refusing to do so) will not generally change your inequality much. This does not mean that "fairness doesn't matter in important situations"; it just means that people will then express social preferences about unfair market outcomes through "voice" (protest, newspaper editorials), regulation, and law. For example, many states that are disaster prone have laws prohibiting "gouging," which is defined as raising prices of basic commodities such as water and gasoline after a shortage due to a disaster. These laws codify a social norm of fairness.

At this point, we should declare a moratorium on creating ultimatum game data and shift attention toward new games and new theories. Good new theories codify precisely what fairness *is*, organize observed regularities, and make new predictions.

Rabin's reciprocity-based theory suggests that the way players feel about others depends on how they expect to be treated. Theories based on inequality-aversion (Fehr–Schmidt and Bolton–Ockenfels) suggest that players dislike getting less than a fair share *and* (less intuitively) dislike getting more than a fair share. The latter assumption serves as a kind of proxy for reciprocity, since it often motivates players to refuse to take advantage of others who have made themselves vulnerable.

The reciprocity-based view is surely more psychologically correct because players do care about the intentions of other players and unchosen paths. At the same time, inequality-aversion is easy to use analytically because social utilities can just be substituted into cells of a payoff matrix, or nodes of a tree, before doing standard equilibrium analyses.

Another idea often espoused to explain these results, but rarely formalized, is an evolutionary explanation. The argument is that, when the human brain and body physically evolved in our ancestral past, people lived in small hunter–gatherer bands. In these groups, the argument assumes, one-shot interactions were rare, protecting property (such as the spoils of a hunt) was important, but sharing norms were useful as social insurance against the risks of bad harvests or hunts. The evolutionary argument is that repeated-game behavior (like rejection in ultimatum games to teach Proposers to offer more) was ingrained in the ancestral environment. When our caveman brains play one-shot games in modern laboratories, we cannot suppress repeated-game instincts or acquired habits, much as our evolved caveman tastes for fatty foods lead to widespread obesity when unleashed in a world of 15,000 McDonald's restaurants and guaranteed half-hour delivery of pizzas with cheese *inside* the crust.

There is surely some truth to the evolutionary argument. But it is difficult to falsify and there is collateral evidence against it. Subjects are usually well aware that one-shot games are strategically different than repeated ones—they say so on Jacobsen and Sadrieh's (1996) videotapes—and they *do* reject more in repeated ultimatum games with partner-matching where they can build reputation (see Slembeck, 1998). More importantly, the evolutionary account has not produced clear fresh predictions. It is time either to make the evolutionary theory precise (Samuelson, 2001, is an important start), derive surprising implications, and test them; or to quit talking about evolution as if it were an obviously better explanation than other theories that *have* passed such tests.

There are many potential applications of all these theories. Obvious examples include bargaining which seems to exhibit costly delay that can partly be ascribed to social preferences—labor–management strikes, ugly divorces, and custody battles. Social comparison of workers with others, and the implications of those comparisons for wage-setting, may also be neatly modeled by these theories. Dictator game experiments may help us understand the determinants of charitable giving. Trust games seem extremely helpful for investigating how 'social capital' affects the development of different economies, favors exchange inside organizations (Prendergast, 1999b), and so forth.

All these applications will also require further development of our understanding of other psychological aspects of social preferences. For example, do angry workers consider "management" to be a single monolithic player, and get angry at "management" the same way they get angry at a spouse who threatens to leave them or a driver who cuts them off on the LA freeway? Perhaps they do, perhaps not. Whom do workers in a firm compare themselves to when their utilities are altered by perceptions of inequality? The cognitive boundaries of comparison groups will matter for most applications, and need to be investigated.

More experimental studies, a new wave of 'second-generation' experiments testing the theories described above, and a lot of applications to field phenomena promise to provide a coherent theory of social preference which should replace the simple caricature of self-interest in economic theory.

Finally, it is important to keep in mind that all the social preference theories permit the possibility that many people are self-interested much of the time. Institutional arrangements can be understood as responding to a world in which there are some sociopaths and some saints, but mostly regular folks who are capable of both kinds of behavior. Dawes and Thaler (1988, p. 195) tell a story which makes this point nicely:

> In the rural areas around Ithaca it is common for farmers to put some fresh produce on a table by the road. There is a cash box on the table, and customers are expected to put money in the box in return for the vegetables they take. The box has just a small slit, so money can only be put in, not taken out. Also, the box is attached to the table, so no one can (easily) make off with the money. We think that the farmers who use this system have just about the right model of human nature. They feel that enough people will volunteer to pay for the fresh corn to make it worthwhile to put it out there. The farmers also know that if it were easy enough to take the money, someone would do so.

3
Mixed-Strategy Equilibrium

In games with mixed-strategy equilibria (MSE) players are predicted to choose probabilistic "mixtures" in which no single strategy is played all the time. Common examples are zero-sum games in which your win is my loss—as in sports, perhaps war and diplomacy, and some other domains. In these games, if I always choose a particular strategy, and you anticipate that strategy, then you will win; so I shouldn't behave so predictably. The only equilibrium will involve unpredictable mixing. Randomizing is also sensible when a little genuine unpredictability will deter another player from doing something you dislike. (Think of random searches of passengers boarding U.S. airlines after the September 11, 2001, attacks, or the liquor store in the South with a large sign outside—"This establishment is guarded by a vicious dog three nights a week. . . . See if you can guess which nights.")

Games with mixed equilibria do not have some of the complications of other games described in this book. The games reported in this chapter do not have multiple equilibria, so there is no question about which equilibria are selected (as in Chapter 7). And in constant-sum games it is not possible for one player to help another without hurting herself, so social preferences and reciprocation (recall Chapter 2) play no role.

Furthermore, in constant-sum MSE games the maximin solution—in which players just choose strategies that maximize the minimum they can get—is also a Nash equilibrium.[1] Maximin leads straight to Nash equilibrium

[1] I shall use the terms maximin and minimax interchangeably.

in zero-sum games because players' interests are strictly opposed: If others will do whatever they can to get the most, their actions will give me the least, so I should try to maximize the least I can get. Since maximin is a particularly simple decision rule and coincides with Nash, in these games there is a good chance that the Nash equilibrium will describe what players do in these games.

That's the good news. The bad news is that, from a behavioral point of view, MSE games raise difficult challenges for Nash equilibrium and for learning.

One way to think about the complexity of reaching an equilibrium in a game is to ask what assumptions about players' mutual rationality and understanding of each other's likely behavior are mathematically required to derive an equilibrium. (These assumptions are called "epistemic foundations"; e.g., Brandenburger, 1992.) In dominance-solvable games, for example (Chapter 5), players need only to have several steps of mutual rationality—that is, faith that others are thinking rationally. But in games with mixed equilibria, in equilibrium players must somehow accurately guess each other's strategies (or infer these from knowing the other players' beliefs, and believing they are rational). This is a tall order.

Perhaps mixed equilibria can be easily learned. But learning dynamics that assume players move toward strategies with higher expected payoffs ("gradient processes") are known to spiral *away* from mixed equilibria (Crawford, 1985); so it is not clear how equilibrium can actually be reached by learning. And in an MSE the expected utility payoff to each strategy used in the mixture is exactly the same (by definition), so there is no positive incentive to mix "properly" or to stick to an equilibrium mixture if it is ever reached.

Furthermore, MSE presumes that players use a probability mixture, or—importantly—appear to (and use the same mixture over time). If randomization is unnatural for players, or undesirable,[2] they will not randomize independently when they play repeatedly. In fact, when experimental subjects are asked to produce random sequences, they produce sequences that reliably deviate from random ones: There are too few long runs, too many alternations, and, consequently, sample relative frequen-

[2] For example, if preferences are quasi-convex then players dislike mixtures of equally preferred items and will dislike randomizing (although equilibrium existence can still be rescued as an equilibrium in beliefs; see Crawford, 1990). This may seem unlikely, but it does point out that, although MSE games are simple in some respects, probability predictions require assumptions about preferences over mixtures (typically the expected utility hypothesis), which is a stronger assumption than is needed to compute equilibria in other games (which typically just use monotonic utilities).

cies are too close to event probabilities. These misconceptions also affect field phenomena such as patterns of betting on lottery numbers (people quit betting on a number for a few days after it has won, until it gradually becomes "due" again; see, e.g., Clotfelter and Cook, 1993; cf. Rabin, 2002).

Another question that is special to games with MSE is raised by experiments in which players are randomly matched with others in a population. In a population-matching protocol, it is possible for an MSE to occur in the *population*, even though *individual* players are not adhering to the MSE. Consider matching pennies, in which two players choose H(eads) or T(ails). The row player wins one if she matches the column player, and the column player wins one if they mismatch. In the unique MSE, both players randomize equally and both guarantee an expected 0.5 chance of winning.

Now imagine a population in which row and column players are randomly rematched and do not know their partner's identity (or, equivalently, forget their history). Then the MSE can be reached at a population level if half the players *always* choose H and half *always* choose T. This population-mixture interpretation is common and persuasive in theoretical biology. It is plausible that different animals adopt pure strategies but selection pressures guide a population toward an equilibrium mixture in which each pure strategy has the same expected payoff. In experiments we can observe both individuals and populations so we can see whether populations appear mixed, in the aggregate, even if individuals are not mixing.

An interesting puzzle is why this sort of stable polymorphic population distribution argument should apply to individual subjects in a group in the lab. One mechanism is learning but, as noted above, this is problematic because players who have learned by switching their strategies have no incentive *not* to switch once they reach equilibrium. How exactly this process works is an interesting open question.

A preview of what lies ahead can be seen in Fig. 3.1. Each point in this figure corresponds to a single strategy in a particular experiment. The graph shows the predicted MSE probability with which that strategy will be played (on the x-axis) against the actual relative frequency of play throughout the experiment (y-axis). Although there is dispersion, the MSE predictions are not bad. There is a slight tendency for low-probability strategies to be played too often (e.g., strategies that should never be played are actually played about 5 percent of the time) and high-probability strategies are not played often enough. Nevertheless, the strong relation across all the points shows that MSE has a lot of predictive power.

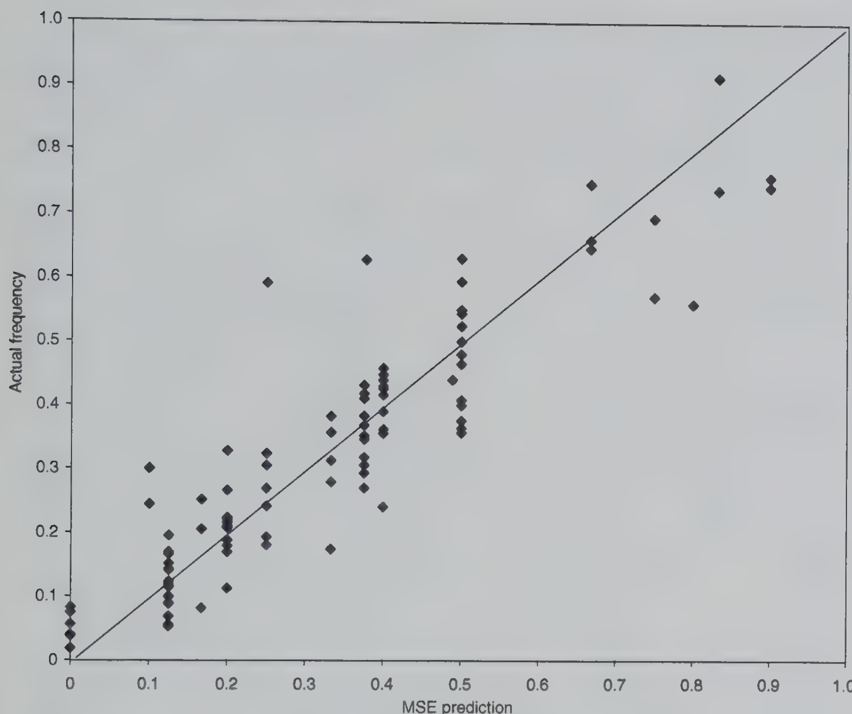


Figure 3.1. Frequencies of different strategy choices predicted by mixed-strategy equilibrium and actual frequencies.

3.1 Early Studies

The first wave of studies on games with MSE occurred in the late 1950s. An important early study was Kalisch et al. (1954), who were discouraged by a perceived failure of game theory predictions. Atkinson and Suppes (1958) and Suppes and Atkinson (1960) were interested in simple one-person models of learning in low-information environments. Their designs treated humans like "lower" animals. In many of their experiments subjects are not told they're playing a game (or, worse, are told they are *not* playing a game when they are) and do not know the matrix of payoffs. Despite being unable to compute equilibria, subjects tend to learn in the correct direction, and long-run frequencies are often close to MSE predictions.

A common early design used one person playing against a computerized strategy. This design is certainly useful for answering some questions. But nothing in game theory says players should use MSE mixtures against a computerized opponent. And the results are often hard to interpret because subjects usually were not told details of what the computerized opponents were doing. These experiments end up being about subjects' intuitions about the contents of computer programs created by experimenters, rather than about game theory per se.[3]

Nevertheless, the basic result in these experiments—relative frequencies of choices are somewhere between equiprobable and the MSE prediction—has held up in more recent studies. To illustrate the early style and results, I will describe a few studies in some detail (see also Estes, 1957).

Lieberman (1962) studied the 2×2 zero-sum game in Table 3.1. The payoffs are the row player's payoff in pennies; in a zero-sum game the column player's payoff is always the row player's times -1. So only row payoffs are shown. In condition O, row subjects play against the experimenter (who use MSE probabilities "in an optimal manner"). We are not told whether the experimenter choices are actually "iid" (independent and identically distributed) draws, although the choices are fixed in advance and are the same for all subjects. Lieberman reports that "subjects tended to look for patterns in the experimenter's play and anticipate his response on each trial in an attempt to win the small amounts of money involved" (1962, p. 213).

In condition O, the experimenter used the MSE probability ($P(E1) = 0.75$)) for 300 trials. Condition N had 100 trials using the MSE probabilities and then switched to 200 trials in which E1 and E2 were played equally often. In a second experiment (Malcolm and Lieberman, 1965), pairs of subjects played each other.

There is little learning across trials so only aggregate results are reported in Table 3.1. (I shall use this convention throughout: aggregate results are reported if there is little learning.) The row player frequencies for each row strategy are reported in the row corresponding to that strategy; similarly for column strategy frequencies.

Contrary to the MSE prediction, row subjects in condition O play S1 less than half the time (40.9 percent) against the properly mixing column experimenter. However, subjects *are* sensitive to the experimenter's behavior, because they switch to S2 substantially more often (65 percent) when the experimenter switches to playing E1 and E2 with equal frequency in condition N, when S2 is a strict best response. When pairs of subjects played each other in experiment 2, their frequencies were roughly halfway between equiprobable and MSE.

Lieberman also experimented with a zero-sum game with a saddle point (pure strategy equilibrium). Both he and Brayer (1964) found strong sup-

[3] These are the games that Aumann (1990), cited in Chapter 1, justifiably complains about.

Table 3.1. *Payoffs (in pennies) and results in a 2×2 game*

	Column player (computer)			Experiment 1		Experiment 2
Choice	E1	E2	MSE probability	Condition O frequency	Condition N frequency	Frequency
S1	3	−1	0.75	0.409	0.651	0.571
S2	−9	3	0.25	0.591	0.349	0.429
MSE probability	0.25	0.75				
Experiment 1: condition O frequency	0.250	0.750				
Experiment 1: condition N frequency	0.500	0.500				
Experiment 2: frequency	0.306	0.694				

Sources: Lieberman (1962); Malcom and Lieberman (1965).

port for equilibrum play in these zero-sum games with a pure equilibrium (see also Binmore, Swierzbinski, and Proulx, 2001). These results establish that the very simplest game-theoretic concepts (which do not involve mixing) are well supported even under old-fashioned experimental conditions.

Subjects in Kaufman and Becker (1961) played five 2×2 zero-sum games. Each game has a maximin payoff of 0.95 points, and MSE probabilities ranging from 0.5 to 1.0. The subjects actually give mixtures (allocations of 100 choices in any mixture they wanted). The twist is that subjects play an impossible-to-beat rule implemented by the experimenter: The experimenter observes the subjects' mixture, and then randomizes among all experimenter mixtures that give the subject *less* than his or her maximin outcome. Because they are forced to tip their hand to the experimenter, the best subjects can do is to play maximin.

Subjects played each of the five games fifty times. The results are underreported so it is impossible to compile them in a table, but the accuracy of the MSE prediction is not bad. Absolute probability deviations averaged from 0.10 to 0.20, and decline across the five-game sequence. A surprising number of subjects converge to the exact MSE mixture on every trial until the end, and the fraction of these true MSE players rose from 10 percent to 65 percent across the five-game sequence. This unique study shows that players do choose maximin when deviations are punished by playing sequentially.

Messick (1967) studied the 3×3 game shown in Table 3.2. His subjects played against computerized opponents programmed with one of three strategies: Maximin (MSE mixtures); and two variants of fictitious play learning rules.[4] Subjects were told:

> The computer has been programmed to play so as to make as much money as possible. Its goal in the game is to minimize the amount of money you win and to maximize its own winnings. In one way the computer will have an advantage since it can remember every choice you have made and use this information to decrease your winnings. You, on the other hand, have the advantage of being human, of possessing intelligence and complete freedom and flexibility of choice which should allow you to capitalize on any weakness you perceive in the play of the machine. (Messick, 1967, p. 35)

This instruction illustrates the difficulty of interpreting results from this sort of experiment. Referring to the computer's memory seems to suggest that the computer is tracking previous choices, although it is *not* when it plays MSE.

Results from the condition in which the computerized player plays MSE are summarized in Table 3.2. Choice frequencies in the first seventy-five

[4] In fictitious play, a player's belief is simply an arithmetic average of what her opponent has done in the past. See Chapter 6 for details.

Table 3.2. Payoffs and results in a 3 × 3 game

	Column player (computer)			MSE	Actual frequency	
Row choice	A	B	C	probability	Periods 1–75	Periods 76–150
a	0	2	−1	0.400	0.270	0.250
b	−3	3	5	0.111	0.390	0.290
c	1	−2	0	0.489	0.340	0.460
MSE probability	0.556	0.200	0.244			

Source: Messick (1967).

periods are close to equal, but there is movement toward MSE (increased play of strategy c) in the last seventy-five periods.

Perhaps surprisingly, subjects who played against computerized strategies using variants of fictitious play were able to beat the stuffing out of the computerized strategies. Subjects also tend to use the same strategy after a win (59 percent "stay") but switch after a loss (38 percent). This "win–stay, lose–shift" heuristic is a coarse version of reinforcement learning and shows up in many other games (e.g., the "email game" described in Chapter 5).

Summary: In early 1960s' studies, incentives were low and subjects often played against computerized rules. These results are frequently hard to interpret because there is no control over what subjects thought their computerized opponents were doing. In two cases, however, there is strong support for MSE. In Kaufman and Becker's experiment, subjects moved first and the experimenter's rule exploited deviations from MSE. Most subjects learned to play exactly the MSE within fifty periods. In Malcolm and Lieberman's experiment, pairs of subjects playing each other generated choice frequencies that lie between equal randomization and the MSE prediction.

3.2 Modern Studies

Because the interpretation of these early results was discouraging, research on zero-sum games waned. Modern work was revived by O'Neill (1987). He noted that predicted mixture probabilities depend on players' utilities for outcomes (i.e., risk-aversion), unless there are only two possible outcomes in a game. To avoid concerns about unmeasured utilities, and to improve power, O'Neill constructed a 4 × 4 zero-sum game with row payoffs (shown in Table 3.3). The game has four strategies, represented by playing cards 1–3 and joker (J). The payoffs are a 5 cent win or loss for each player, so that any monotonic utility function in which players prefer to win gives the

Table 3.3. Row player payoffs (in cents) and results in a 4 × 4 game

Row choice	Column player 1	2	3	J	MSE probability	Actual frequency	QRE estimate
1	−5	5	5	−5	0.20	0.221	0.213
2	5	−5	5	−5	0.20	0.215	0.213
3	5	5	−5	−5	0.20	0.203	0.213
J	−5	−5	−5	5	0.40	0.362	0.360
MSE probability	0.20	0.20	0.20	0.40			
Actual frequency	0.226	0.179	0.169	0.426			
QRE estimate	0.191	0.191	0.191	0.426			

Source: O'Neill (1987).
Note: QRE estimate is $\hat{\lambda} = 1.313$.

same mixture probabilities. The unique MSE probabilities are 0.20 each for cards 1–3 and 0.40 for J. His subjects were matched in fixed pairs. Results are summarized in Table 3.3. The overall relative frequencies are remarkably close to the MSE predictions. Row players are predicted to win 40.0 percent of the time in MSE and actually win 41.0 percent of the time.

Also reported in Table 3.3 are estimates from the quantal response equilibrium (QRE) of McKelvey and Palfrey (1995) and Chen, Friedman, and Thisse (1996) (cf. Rosenthal, 1989). In a quantal response equilibrium (QRE), players do not choose the best response with probability 1 (as in a Nash equilibrium). Instead, they "better-respond" and choose responses with higher expected payoffs with higher probability. In practice, the QRE often uses a logit payoff response function:

$$P(s_i) = e^{\lambda \sum_{s_{-i}} P(s_{-i}) u_i(s_i, s_{-i})} \Big/ \Big(\sum_{s_k} e^{\lambda \sum_{s_{-i}} P(s_{-i}) u_i(s_k, s_{-i})} \Big). \qquad (3.2.1)$$

QRE uses one parameter (λ) to measure how noisily players respond to expected payoffs, and typically generate predictions somewhere between equal randomization and MSE. In the O'Neill game the QRE estimates fit the joker frequency very accurately, but mistakenly estimate, as MSE does, that other card probabilities will be equal.

Brown and Rosenthal (1990) criticized O'Neill's interpretation of his results as overly supportive of MSE. They noted that aggregate tests lack the power to distinguish MSE from alternative theories, and finer-grained analyses reject the assumption that players mix independently using the MSE probabilities. Their analysis set the stage for several other experiments described below.

Table 3.4. *Tests for temporal dependence in zero-sum games*

Effect	Coefficient	Percentage of players with significant ($p < .05$) effects BR	RB exp. 1	RB exp. 2	BR94
Guessing	b_0	8	8	5	10
Previous opponent choices	b_1, b_2	30	42	35	12
Previous outcomes	c_1, c_2	38	28	20	6
Previous choices and outcomes	b_1, b_2, c_1, c_2	44	55	38	12
Previous own choices	a_1, a_2	48	48	42	42
All effects	a_i, b_i, c_i	62	72	50	52

Sources: Brown and Rosenthal (1990); Rapoport and Boebel (1992); Budescu and Rapoport (1994).

Notes: BR denotes Brown and Rosenthal (1990); RB denotes Rapoport and Boebel (1992); BR94 denotes Budescu and Rapoport (1994). $J_{t+1} = a_0 + a_1 J_t + a_2 J_{t-1} + b_0 J^*_{t+1} + b_1 J^*_t + b_2 J^*_{t-1} + c_1 J_t J^*_t + c_2 J_{t-1} J^*_{t-1}$.

They first noted that win rates across specific pairs range from 30 percent to 54 percent. Much of the variation in these win rates comes from correlation between the players' choices.[5] Where is the correlation coming from? To investigate, Brown and Rosenthal ran a logit regression of players' choices of J in period $t+1$, denoted J_{t+1}, against two lags of opponents' choices (denoted J^*_{t+1}), and own choices and the interaction between own and opponent choices. If players are randomizing and are not able to detect nonrandomization in others' choices,[6] all the coefficients (other than the constant a_0) should be zero.

Column BR in Table 3.4 reports results from a suite of regressions that Brown and Rosenthal ran exploring different types of temporal dependence. For more than half the players (62 percent), the null hypothesis that all lag coefficients are zero can be rejected (at 0.05). For about half the subjects, the coefficients on their own lagged choices (a_1, a_2) are significant, which means players are *not* randomizing independently (typically, they play J twice in a row too rarely). A third of the subjects had significant coefficients (b_1, b_2) on lags of their opponent's J choice, which means they are trying to guess their opponent's temporal dependence. Only 8 percent

[5] That is, the win rate is substantially different from what it would have been if players used their observed frequencies but made draws across the 105 periods.

[6] As we shall see, the typical deviation from independent randomization is to alternate too frequently. But a player who thinks that an H is due after a string of Ts should therefore be able to guess the over-alternation and beat her opponent. So there must be some cognitive limit, or interference from a player trying to control her own randomization, that prevents widespread exploitation of nonrandomness.

of the players had a significant coefficient of contemporaneous choice J^*_{t+1}; thus, although players did respond to what they had seen (and done) in the past, they were *not* able to use this information to guess what their opponent would do in the current period. Brown and Rosenthal conclude that there is little support for the maximin hypothesis.

A sensible interpretation of what's going on follows from the "purification" interpretation of mixed equilibrium (due to Harsanyi and Aumann). Suppose a player does not think she is consciously randomizing—for example, she observes a hunch variable and conditions play on that variable. If the other player does not see the variable that generates nonrandomization, then the other player believes she faces a mixture. MSE can then be an equilibrium in *beliefs* rather than in mixtures.

A key fact in Brown and Rosenthal's reanalysis is this: Even though there is substantial temporal dependence in choices, very few players are able to detect dependence in their opponent's choice and outguess what their opponent will do (as evidenced by very few significant b_0 coefficients). Thus, there *are* substantial deviations from maximin, but players cannot detect them so the equilibrium-in-beliefs interpretation is supported. That is, although players are not mixing randomly, opponents' departures are not being detected.

Rapoport and Boebel (1992) replicated O'Neill's study with four small differences.[7] Their overall results, reported in Table 3.5, replicate O'Neill's. Actual frequencies of play are generally between MSE predictions and equal probability, but the MSE predictions can be rejected by a χ^2 test for 85 percent of the subjects. Frequencies are similar across win/loss conditions in experiments 1–2, so strategic equivalence holds.[8] Note that QRE estimates do an adequate job of capturing deviations from MSE, especially for row players.

An interesting fact is that there are no persistent skill differences in these games, because the correlation between a single subject's win rates in his or her two different sessions is only 0.17. Rapoport and Boebel also replicated the Brown–Rosenthal findings on temporal dependence (see Table 3.4) and found a little support for a behavioral model in which mixture proportions depend on the proportion of "wins" in the matrix.

Mookerjhee and Sopher (1997; see also 1994) studied four constant-sum games, with an emphasis on learning (described later in Chapter 6).

[7] They used a 5 × 5 game; stakes were higher; subjects play as both row and column players; and they test for strategic equivalence by varying win and loss payoffs in a way that should not affect MSE probabilities.

[8] It would be useful to have an alternative theory of why strategic equivalence might *not* hold for these particular parameters. For example, in standard utility theory the win and loss utilities can simply be set to 0 and 1, so they should be invariant to the absolute magnitude of the win and loss payoffs. But if win and loss utilities are cardinally scaled, and losses loom larger than gains ("loss-aversion"), then MSE predictions will differ in the two cases. It would be useful to work through specific predictions along these lines, and to choose design parameters in which strategic equivalence is most likely to be rejected in the direction of a plausible alternative.

Table 3.5. *Payoffs and results in 5 × 5 games*

Row choice	Column choice C	L	F	I	O	MSE probability	Actual frequency Experiment 1	Experiment 2	QRE estimates Experiment 1	Experiment 2
C	W	L	L	L	L	0.375	0.293	0.306	0.286	0.309
L	L	L	W	W	W	0.250	0.305	0.324	0.302	0.296
F	L	W	L	L	W	0.125	0.123	0.100	0.138	0.132
I	L	W	L	W	L	0.125	0.119	0.115	0.138	0.132
O	L	W	W	L	L	0.125	0.160	0.155	0.138	0.132
Equilibrium probability	0.375	0.250	0.125	0.125	0.125					
Actual frequency (experiment 1)	0.352	0.180	0.218	0.099	0.151					
Actual frequency (experiment 2)	0.346	0.193	0.202	0.116	0.143					
QRE estimate (experiment 1)	0.412	0.169	0.140	0.140	0.140					
QRE estimate (experiment 2)	0.410	0.184	0.135	0.135	0.135					

Source: Rapoport and Boebel (1992).

Note: (W,L) payoffs are (\$10,–\$6) in experiment 1, (\$15,–\$1) in experiment 2. QRE estimate parameters are $\hat{\lambda}_1 = 0.248$, $\hat{\lambda}_2 - 0.327$.

Table 3.6. Row payoffs and results in games 1 and 3

Row choice	Column choice 1	2	3	4	MSE probability	Actual frequency
1	W	L	L	W	0.375	0.318
2	L	L	W	W	0.250	0.169
3	L	W	$\frac{2}{3}$W	$\frac{2}{3}$W	0.375	0.431
4	L	L	$\frac{1}{3}$W	W	0.000	0.083
MSE probability	0.375	0.250	0.375	0.000		
Actual frequency	0.383	0.308	0.270	0.040		

Source: Mookerjhee and Sopher (1997).
Note: L, W, *p*W denote loss, win, and a *p* chance of win.

Table 3.7. Row payoffs and results in games 2 and 4

Row choice	Column choice 1	2	3	4	5	6	MSE probability	Actual frequency
1	W	L	L	L	L	W	0.375	0.410
2	L	L	W	W	W	W	0.250	0.241
3	L	W	L	L	W	L	0.125	0.048
4	L	W	W	L	L	L	0.125	0.069
5	L	W	L	W	L	W	0.125	0.195
6	L	L	W	L	W	W	0.000	0.038
MSE probability	0.375	0.250	0.125	0.125	0.125	0.000		
actual frequency	0.368	0.269	0.099	0.166	0.080	0.019		

Source: Mookerjhee and Sopher (1997).

Their games are shown in Tables 3.6 and 3.7. To test for the effects of doubling stakes, a win (denoted W) is worth 5 rupees in games 1 and 3 and 10 rupees in games 2 and 4.[9] Game 1 is like game 2 except that strategies 3–5 from game 2 are collapsed into a single strategy 3 in game 1. Under the maximin hypothesis these games should be equivalent.

Pooled results across all forty periods are shown in Tables 3.6 and 3.7. Although the multinomial MSE prediction can be rejected, the results are close to MSE. Weakly dominated strategies are rarely played (2–8 percent of the time). Strategic equivalence from combining strategies 3–5 in game 2

[9] Monthly room and board in a student dorm is 600 rupees, so these stakes are large.

Table 3.8. *Games and results*

Row choice	Column choice 7	8	9	MSE probability	Actual frequency
Game 1					
1	20,0	8,16	8,16	0.167	0.311
2	5,12	20,4	5,10	0.333	0.313
3	0,12	0,12	20,8	0.500	0.376
MSE probability	0.167	0.333	0.500		
Actual frequency	0.163	0.313	0.524		
Game 2					
1	0,0	12,16	12,16	0.167	0.074
2	15,12	0,4	15,10	0.333	0.382
3	20,12	20,12	0,8	0.500	0.544
MSE probability	0.167	0.333	0.500		
Actual frequency	0.462	0.174	0.364		
Game 3					
1	4,0	10,12	12,16	0.167	0.235
2	15,15	0,6	15,10	0.333	0.357
3	18,10	0,12	14,8	0.500	0.408
MSE probability	0.167	0.333	0.500		
Actual frequency	0.321	0.279	0.400		

Source: Tang (2001).

into a single strategy in game 1 holds on average, although columns play the combined strategy more often and rows play it less often.

Tang (1996a–c, 2001) reports experiments on three games with mixed-strategy equilibria and estimates a dizzying variety of learning models on those data. The games are shown in Table 3.8. All three games have a mixed-strategy equilibrium in which the row players play strategies 1–3 and column players play strategies 7–9, with probabilities (1/6, 1/3, 1/2). In addition, game 2 has two other classes of equilibria in which only the column player mixes.[10] The pattern of results should be familiar by now to readers who are paying attention: Deviations from the MSE proportions are small

[10] In one class of equilibria, the row player chooses 3, and the column player chooses any mixture of strategies 7 and 8. In the other class, the row player chooses 1 and the column player mixes between 8 and 9, with a probability of choosing 8 between 0.2 and 0.6.

Table 3.9. Game 1–5 row payoffs and results

Row choice	Column choice 1	1	MSE probability	Actual frequency
Game 1				
1	−2	3	0.167	0.251
2	−1	−2	0.833	0.749
MSE probability	0.833	0.167		
Actual frequency	0.915	0.085		

Row choice	Column choice 1	2	3	MSE probability	Actual frequency
Game 2					
1	−3	−2	−3	0.000	0.044
2	−1	−1	0	1.000	0.888
3	3	−3	−3	0.000	0.068
MSE probability	0.000	1.000	0.000		
Actual frequency	0.011	0.918	0.071		

Row choice	Column choice 1	2	3	MSE probability	Actual frequency
Game 3					
1	−2	3	−3	0.167	0.205
2	−1	−3	0	0.000	0.056
3	0	−1	1	0.833	0.739
MSE probability	0.667	0.333	0.000		
Actual frequency	0.647	0.279	0.075		

but significant, and the predicted ranking of strategies by MSE probability corresponds to rankings of actual frequencies in most cases.

Binmore, Swierzbinski, and Proulx (2001) conducted an ambitious study with eight different games. Their view is that equilibrium play can *only* come about from learning, which implies that equilibration *necessarily* generates temporal dependence of the sort Brown and Rosenthal pointed out. Dependence is therefore *evidence* of equilibration.

The five main zero-sum games they study are shown in Table 3.9 (game 5 is O'Neill's game). Since they observe some convergence toward MSE over time, only frequencies from the last third of the experiment are reported

Table 3.9. (*continued*)

Row choice	Column choice 1	2	3	MSE probability	Actual frequency
Game 4					
1	0	2	−1	0.167	0.207
2	2	0	−1	0.167	0.133
3	−1	−1	0	0.667	0.660
MSE probability	0.167	0.167	0.667		
Actual frequency	0.081	0.171	0.748		

Row choice	Column choice 1	2	3	4	MSE probability	Actual frequency
Game 5						
1	1	−1	−1	−1	0.400	0.439
2	−1	−1	1	1	0.200	0.187
3	−1	1	−1	1	0.200	0.224
4	−1	1	1	−1	0.200	0.150
MSE probability	0.400	0.200	0.200	0.200		
actual frequency	0.448	0.266	0.112	0.174		

Source: Binmore, Swierzbinski, and Proulx (2001).

in these tables. Relative frequencies are again close to the MSE predictions, with a slight bias toward 0.5. Fine-grained analyses show that the most frequent observations are close to the MSE on average, although the deviations are statistically significant.[11] The learning dynamics are generally consistent with best response: About 60 percent of the changes in the overall frequencies of the choice are a best response to history. Players are also very sensitive to the moving averages of their own payoffs (they switch more when their moving average is lower).

Summary: Modern studies have all used pairs of subjects playing against each other, for modest to very large stakes (in Mookerjhee and Sopher,

[11] Since MSE predictions are multiples of 1/6, and there are six pairs in each period, the datum from a single period can be placed into forty-nine bins representing each of the combinations of frequencies (0, 1/6, 2/6, . . . , 1) for each of the row and column players. Call the bin that has the most points in it, after all the period data are placed in bins, the "best unit predictor" (BUP).

Binmore et al. have nice plots of boxes around this BUP (1/3 wide in probability terms) and show their location relative to the MSE. In three cases the BUP boxes just graze the MSE prediction, in one game (3) the BUP box is centered right on MSE, and in game 5 it includes the MSE.

1997), in games with several strategies and (typically) only win–loss payoffs so risk tastes can't possibly matter. Actual frequencies are not far from the MSE predictions. The deviations are smallest in the experiments by Binmore et al., in which players play for 150 periods and can track moving averages of the payoffs of themselves and others. However, deviations are usually highly significant, either at the individual level or in the aggregate. Most subjects also exhibit temporal dependence—their choices depend on previous choices by themselves and others.

3.3 Subjective Randomization and Mixed Strategies

Rapoport and Budescu wrote several papers linking MSE games with psychological studies of subjective perceptions of randomness. The idea is to see whether the implicit randomization that players should do (if they want to reduce predictability in their choices when playing opponents who know their history) is similar to the kind of randomization evident when subjects are asked to produce or recognize sequences.

Many psychology studies use "production tasks" in which subjects generate random sequences or arrange a sample of outcomes in a random order. Other studies use "recognition tasks" in which subjects are asked to rate how random different series appear to be. Many conditions and response modes have been explored (see Bar-Hillel and Wagenaar, 1991).

The studies show that people reliably produce sequences whose features resemble the underlying statistical process more closely than short random sequences actually do. For example, in a class exercise I ask some students actually to flip a coin twenty times, and I ask other students to generate a sequence of flips that looks as much like a coin flip sequence as possible.[12] Student-generated sequences can usually be recognized by three tell-tale clues: (1) The number of heads is more likely to be exactly 10, and less likely to be below 8 or above 12, than the actual coin sequences; (2) there are too few runs of identical flips in the student-generated sequences (there is an average of $(n+1)/2$ in a random sequence); and (3) the longest run in the student sequences is usually only three or four flips, whereas maximum length runs of five or six are common in actual sequences.

Alternating outcomes ("negative recency") are common, with two interesting exceptions: Children don't seem to learn this misconception until after fifth grade (Ross and Levy, 1958); and subjects trained to produce many sixty-trial sequences with extensive feedback are able to produce remarkably random sequences (Neuringer, 1986). The evidence from children is im-

[12] This can be done in an incentive-compatible way by telling the students who produce artificial sequences that they win money if they can fool a statistical algorithm or person who is trying to tell their sequence apart from a random one.

portant because it implies that misrandomizing is *not* a mistake that is easily erased by learning. Quite the opposite: It is a mistake that is *caused* as developing minds acquire the erroneous intuition that small samples should have all the properties of large ones (the facetiously named "law of small numbers").

Rapoport and Budescu (1992) were the first to compare sequences from a production task with strategies in a constant-sum game. Their study was motivated by four criticisms of the production and recognition paradigms: Instructional biases or vagueness may cause subjects to generate nonrandom sequences;[13] tests for randomness are problematic; few studies paid performance-based rewards; and production tasks are artificial (they don't correspond to anything people explicitly do in everyday life). All these criticisms are overcome by using games in which randomized mixing is desirable.

Rapoport and Budescu's study had three conditions. In condition D, subjects played a game of matching pennies 150 times in the usual trial-by-trial way; in condition S, subjects gave an entire sequence of 150 plays at once (which were then paired with elements of an opponent's sequence to determine 150 outcomes and payoffs); and in condition R subjects were asked to "simulate the random outcome of tossing an unbiased coin 150 times in succession."

As usual, subjects produced sequences with too many runs: *Z*-tests on individuals reject the iid hypothesis for 40 percent, 65 percent, and 80 percent of the subjects in conditions D, S, and R. (Note that twice as many subjects show temporal dependence in the production condition R as in the game condition D.) The game-playing environment seems to reduce deviations from randomness.

Budescu and Rapoport (1994) extended their earlier paper to a 3×3 version of a zero-sum matching pennies game. The players choose one of three colored cards; if the colors match, row wins 2 and, if they mismatch, row loses 1. The MSE is for both players to choose all three cards equally often. They again compared a game-theoretic condition D with a random-generation condition R. Regressions show significant temporal dependence, as in earlier studies (see Table 3.4, column BR94). More of the R subjects exhibit temporal dependence (typically over-alternation).

Table 3.10 reports frequencies of selected patterns. The pattern (x,x) denotes *any* of the three possible runs of two identical cards in a row, and (x,y) denotes any pair of non-identical cards. Patterns are predicted to be uncommon (compared with iid) if they are "unbalanced" and have streaks, and are predicted to be more common if they are balanced. For example, the pattern (x,x,x) has only one run and one color of card, and is thus highly

[13] For example, Ayton, Hunt, and Wright (1989) note that, in many experiments, subjects are told not to generate sequences with "identifiable patterns." This may discourage them from generating long runs, leading to over-alternation.

Table 3.10. Frequency of selected patterns in a three-strategy experiment

Pattern length	Pattern type	Predicted frequency relative to iid	Frequency in condition D (game)	Frequency in condition R (production)	Expected frequency if iid
2	(x,x)	Lower	0.269	0.272	0.333
	(x,y)	Higher	0.731	0.728	0.667
3	(x,x,x)	Lowest	0.073	0.063	0.111
	(x,x,y)	Lower	0.196	0.209	0.222
	(x,y,y)	Lower	0.196	0.210	0.222
	(x,y,x)	Higher	0.237	0.160	0.222
	(x,y,z)	Highest	0.297	0.359	0.222
4	(x,x,x,x)	Lowest	0.020	0.018	0.037
	(x,x,x,y)	Lower	0.053	0.045	0.074
	(y,x,x,x)	Lower	0.054	0.045	0.074
	(x,y,x,x)	Lower	0.056	0.035	0.074
	(x,x,y,x)	Lower	0.058	0.037	0.074
	(y,x,z,x)	Higher	0.096	0.078	0.074
	(x,y,x,z)	Higher	0.099	0.079	0.074
	(x,y,z,x)	Highest	0.121	0.173	0.074

Source: Budescu and Rapoport (1994).
Note: iid = independent and identically distributed.

unrepresentative (and predicted to occur too rarely in subjects' choices). The pattern (x,y,z) contains one of each of the three choices and is most representative. There is strong statistical bias in which actual patterns are most and least frequent, in the direction of balance. The bias is larger in the random-generation condition R than in the game-playing condition D.

In the second part of their paper (based on Budescu, Freiman, and Rapoport, 1993) they contrast two different explanations for why game-playing subjects exhibit less temporal dependence. One hypothesis is that the game-playing subjects have a clearer goal and more motivation than sequence-generators: maintain unpredictable play so your opponent cannot outguess you. An alternative explanation is "interference": Working memory is so overwhelmed keeping track of one's own choices *and* the opponent's choices that game players literally remember less of what they did in the past, which leads them closer to "memoryless" randomization. In this view, game players are not "more rational" per se; instead, the constraint of bounded memory inhibits their ability to *mis*randomize because playing a game increases the memory constraint. This hypothesis predicts that departures from short-run randomness will be similar for D and R subjects, but departures based on more "distant memory" (such as three- and four-outcome patterns) will be stronger for R subjects. The data in Table 3.10 are consistent with this prediction.[14]

Rapoport and Budescu (1997) propose an elegant model to explain departures from randomization in a production task (cf. Rabin, 2002). It is a useful example of how good psychological modeling works. Their model combines limited working memory with the intuition behind the "representativeness heuristic." Representativeness is a heuristic used to judge conditional probabilities $P(hypothesis|evidence)$ by how representative the evidence is of the hypothesis (see Tversky and Kahneman, 1982). The idea is that a hypothesis and a piece (or sample) of evidence both have statistical properties (mean, variance, and so forth), but also have features or proximity to a category exemplar. People judge the likelihood of a hypothesis, given evidence, by how well the features of the evidence match features of the hypothesis. The same psycho-logic can extend to judgments such as $P(category|example)$. For example, if you meet a very attractive person in a Los Angeles nightclub, is he or she more likely to be a schoolteacher or a model? Since all exemplars in the category "model" are attractive, it is common to overestimate the chance the person you met is a model, neglecting the fact that the overall frequency, or base rate, of schoolteachers is much larger. The psychology of representativeness judgments is fundamentally different than Bayesian statistical reasoning because there is no natural place in it for concepts such as sampling variation, regression toward the mean in a time series, and use of base rates (priors) (see Kahneman and Frederick, 2002).

The same heuristic can be applied to producing a random sequence of coin flips. When generating samples to match the coin-flip process, subjects who are feature matching will generate samples that are "too balanced" in percentages of H and T elements. Similarly, when people are not sure about the statistical properties of a time series, if they see surprisingly long runs they may mistakenly infer serial correlation where there is none. A surprising and well-established example is the mythical belief in the "hot hand" in basketball shooting (see Gilovich, Vallone, and Tversky, 1985).[15]

In the Rapoport and Budescu model, subjects remember only the previous m elements in their sequence and use the feature-matching heuristic. They choose the $m + 1$st element to balance the number of H and T choices in the last $m + 1$ flips. If the memory length m is not very large, subjects will

[14] To investigate interference, Budescu et al. gave an unexpected memory quiz, asking subjects to write down as many of their own and their opponent's choices as they could remember after a matching pennies game. Subjects in the game condition D remembered fewer previous choices and were less accurate at recalling basic features of their sequences, consistent with interference.

[15] Hot hand beliefs also affect betting odds for National Basketball Association games (Camerer, 1989). Odds set, in effect, by the (dollar-weighted) median bettor were biased in favor of teams with winning streaks and against teams with losing streaks.

alternate choices too frequently. The model makes very specific predictions. For example, in two experiments with binary outcomes, H and T subjects had an estimated $P(H|H) = 0.42, P(H|HH) = 0.32, P(H|HHH) = 0.21$. The model yields predictions very close to these actual numbers if memory length m is around 7. This implied constraint on short-term memory is amazingly close to the "7 plus or minus 2" discussed almost fifty years ago in a classic psychology article by Miller (1956).

Summary: In experiments comparing randomization by subjects playing games with MSE and subjects producing sequences, the sequence-generating groups show more temporal dependence. One study suggests that the difference in behavior is due to the cognitive demands of playing the game and attending to the opponents' choices. Playing a game ties up limited working memory and interferes with the players' instinct to randomize incorrectly. Over-alternation is consistent with remembering about seven previous choices and making new choices to balance the remembered portion of the sequence.

3.4 Explicit Randomization

Three modern researchers allow players to randomize deliberately over strategies. (None knew that Kaufman and Becker, 1961, had done this thirty years earlier!) "Controlled randomization" is important because it allows players who would *like* to use iid draws, but find it hard to do so, an explicit device that helps them randomize. In addition, it gives experimenters a chance to observe explicit randomization that is rarely observable in the field.

Bloomfield (1994) studied a 2×2 game, shown in Table 3.11. His subjects controlled randomization by allocating a total of fifty choices to either of their two strategies. For example, the row player could execute the MSE ($p(A) = 0.4$) by allocating twenty choices to row A and thirty choices to row B. Players were paired randomly and each player's payoff was the *expected* payoff given both players' distributions.[16]

Bloomfield told subjects their opponent's actual (mixed) strategy and the expected payoffs they would have earned from every possible mixture. In an extra "disclosure" condition, subjects were also told their opponents' ex post expected payoffs for all strategies, to see if this information would create anticipatory or sophisticated reasoning and speed up equilibration. These information treatments are influenced by Bloomfield's interest in an adaptive dynamic in which a player's strategy probability p is adjusted by a

[16] For example, if the row player submitted 20 As and 30 Bs, and the column player allocated 10 Xs and 40 Ys, the row player's payoff would be $(0.4)[(0.2)80 + (0.8)40] + (0.6)[(0.2)40 + (0.8)100] = 72$.

Table 3.11. A 2 × 2 game and results

	Column player		MSE	Actual frequencies	
	X	Y	probability	Control	Disclosure
Row player					
A	80,40	40,100	0.400	0.416	0.397
B	40,80	100,40	0.600	0.584	0.603
MSE probability	0.600	0.400			
Actual frequency					
Control	0.545	0.455			
Disclosure	0.556	0.444			

Source: Bloomfield (1994).

fraction of the payoff gradient (the derivative of the expected utility with respect to p) based on the previous period's outcome. Adjustment based on payoff gradients leads players to spiral *away* from the MSE.[17]

Results are reported in Table 3.11. Once again, the aggregate proportions are close to the predicted MTE frequencies. Regressions of the actual mixed-strategy proportions against a player's previous probability and the payoff gradient give significant coefficients of 0.45 and 0.14, respectively, for the control conditions. The corresponding coefficients are 0.24 and 0.04 in the disclosure condition. The effect-of-payoff gradient is therefore reduced by a factor of about three by disclosure, which is consistent with players anticipating adjustment by others predictably and responding less to experience.

Ochs (1995b) also allowed explicit randomization in three versions of a matching pennies game (shown in Table 3.12), in which the payoff to the row player from the (1,1) upper-left match varied. Players choose an allocation of ten plays to each of the strategies 1 or 2. This is used to produce a randomized sequence (without replacement) of ten actual plays, which are then matched with ten plays from their opponent.

Table 3.12 shows the games, results, and estimates of steady-state frequencies derived from a useful difference equation.[18] When the row player's (1,1) match payoff changes, the row player's MSE probabilities *should not*

[17] To see this, suppose players are at the point $p = P(A) = 0.5$ and $q = P(X) = 0.5$. Then the expected utilities for the row and column players are $70 - 10p$ and $70 - 10q$, so the payoff gradients are both -10. The new probabilities will be $p' = p - 10h$ and $q' = q - 10h$ (where h tunes the sensitivity to the gradient). Calculation shows that (p', q') is further from the MSE of (0.4,0.6) in squared (Euclidean) distance than the starting point (0.5,0.5) is. See Crawford (1985) for details on how general this result is.

[18] The difference equation is $X_t = a + bX_{t-1} + e_t$, where X_t is "the relative frequency of 1s played as of round t." The estimator $\hat{b}/(1 - \hat{a})$ gives an estimate of the steady-state frequency, an idea first used to my knowledge in experimental economics by Camerer (1987).

Table 3.12. Games and results

Row choice	Column choice 1	Column choice 2	MSE probability	Actual frequency	Steady-state estimate	QRE estimate
Game 1						
1	1,0	0,1	0.500	0.500	0.502	—
2	0,1	1,0	0.500	0.500	0.498	—
MSE probability	0.500	0.500				
Actual frequency	0.480	0.520				
Steady-state estimate	0.482	0.518				
Game 2						
1	9,0	0,1	0.500	0.600	0.631	0.649
2	0,1	1,0	0.500	0.400	0.369	0.351
MSE probability	0.100	0.900				
Actual frequency	0.300	0.700				
Steady-state estimate	0.350	0.650				
QRE estimate	0.254	0.746				
Game 3						
1	4,0	0,1	0.500	0.540	0.534	0.619
2	0,1	1,0	0.500	0.460	0.466	0.381
MSE probability	0.200	0.800				
Actual frequency	0.340	0.560				
Steady-state estimate	0.562	0.438				
QRE estimate	0.331	0.669				

Source: Ochs (1995b).
Note: Actual frequencies are estimated from Ochs's Figure 2. QRE parameter estimates are $\hat{\lambda}_2 = 3.24$, $\hat{\lambda}_3 = 2.66$. See McKelvey and Palfrey (1995) for details.

change, but the column player's probability of playing strategy 1 should fall. This wacky prediction is surprisingly close to correct. The relative frequencies of the row player choosing 1 vary only from 0.50 to 0.60 across games, while the frequencies of the column player choosing strategy 1 fall from 0.48 to 0.30 across games. QRE estimates account for the deviations from MSE in games 2 and 3 fairly well. Furthermore, a large number of players are strictly best responding rather than mixing—there are spikes at all-1 and all-2 allocations of the ten plays for most players.

Table 3.13. Choice frequencies in 4 × 4 O'Neill game

Row choice	Column choice 1	2	3	J	MSE probability	Actual frequency	z-statistic
1	0.051	0.049	0.039	0.073	0.200	0.212	(1.60)
2	0.045	0.045	0.044	0.088	0.200	0.222	(2.93)
3	0.045	0.044	0.039	0.082	0.200	0.210	(2.47)
J	0.075	0.069	0.065	0.147	0.400	0.356	(−4.76)
MSE probability		0.200	0.200	0.200	0.400		
Actual frequency		0.216	0.207	0.187	0.390		
z-statistic		(3.46)	(1.52)	(−2.81)	(−1.08)		

Source: Shachat (2002).

Shachat (2002) also allowed explicit randomization. His treatment 1 uses the four-strategy O'Neill game with minor changes. His randomization procedure corrects a very subtle flaw in earlier procedures.[19] In treatment 2, players specify a probability distribution by filling a computer-displayed "shoe" of 100 cards with a composition of actions. The shoe is shuffled and the computer selects the "top" card as the realized strategy. Subjects learn their opponent's realized strategy after each period. In treatment 3 they *also* learn the opponent's mixture.

Shachat's results closely replicate O'Neill's. All three information treatments give similar results, so only pooled results are shown in Table 3.13. The table shows the relative frequencies of choices, with z-statistics testing MSE predictions in parentheses. Attentive readers can sing along with the familiar refrain: Deviations from MSE are small in size but significant.

Shachat did a more sophisticated test for temporal dependence than Brown and Rosenthal did. The independent variable is the *joint* realization

[19] Consider Ochs's design, in which subjects give K pure strategies and they are played (without replacement) in each of K pairings. Consider $K = 4$ in a matching pennies game with an MSE of 0.5 on each of two strategies. The unique solution in this design is for each player to designate two H and two T strategies. When they are played without replacement, however, the distribution of the number of wins for a particular player, from 0 to 4, is 1/6, 0, 2/3, 0, 1/6. This is different from the binomial distribution (1/16, 1/4, 3/8, 1/4, 1/16) that would result in a series of one-shot games in which players randomize. The difference is owing to the fact that the H and T strategies are drawn without replacement, so that wins across the K periods are negatively correlated, which spreads probability toward the chances of never winning, winning exactly twice, and always winning. In principle, feedback from these slightly "distorted" win totals could hinder the subjects' ability to learn the mixed equilibrium. Bloomfield's design errs in the opposite direction—since the expected payoff is used rather than a sample of realizations, the distribution of payoffs is a spike which gives players an average payoff more often than they would get from mixing in a series of stage games.

of a pair of J plays, using only one time lag. Tests for independence from *own* and *opponent* previous choices reject for 15 percent and 22 percent of the players, respectively, compared with 24 percent of O'Neill's players. About 10 percent of Shachat's subjects are "purists" who used pure strategies (all 100 cards of one type) in most periods. Half the subjects sometimes mix and sometimes use pure strategies. The overall card proportions used by the purists and the mixers, as groups, closely approximate the MSE mixtures. The rest of the subjects rarely used pure strategies (nine times or less). Their strategies are biased away from MSE toward equiprobable play. (Several subjects mix equally among the four strategies most of the time.)

Summary: Three studies allowed subjects explicitly to mix by choosing allocations of tickets to different strategies. The results in these controlled randomization experiments replicate earlier ones. Most subjects do not choose mixtures that correspond to the MSE proportions, and a large fraction often use pure strategies (either best-responding to what their opponent has done, or speculating on the success of a particular strategy). However, population mixtures across subjects are close to MSE predictions.

3.5 Patent Race and Location Games with Mixed Equilibria

This section describes two games that model specific examples of economic interest. Other interesting examples are omitted (e.g., mixing in posted-offer markets with market power; see Holt and Solis-Soberon, 1992; Davis and Wilson, in press).

Rapoport and Almadoss (2000) studied an investment game.[20] In the symmetric game, two firms each have an equal endowment e (such as an R&D budget) they can invest in a patent race in integer amounts. Firms keep the amount of their budget they do not spend. The firm that invests the most wins a fixed prize r; if the firms spend the same amount, neither earns the prize (reflecting, say, dissipation of gains through patent disputes). There is a unique symmetric MSE in which firms invest their entire endowment with probability $(r - e)/r$ and invest all smaller integer amounts with equal probabilities $1/r$.

Results are reported in Table 3.14. Guess what? The results are close to MSE. What a shock! Subjects do choose 0 too often, but actual choices of 1–4 are about equally frequent and close to the MSE prediction of $1/r$. The

[20] Amaldoss and Jain (2002) analyze a related game in which players have asymmetric mixed strategies. They find good support for the mixed-strategy equilibrium.

Table 3.14. *Results in a symmetric patent race*

	Game L ($e = 5$, $r = 8$)		Game H ($e = 5$, $r = 20$)	
Investment	MSE probability	Actual frequency	MSE probability	Actual frequency
0	0.125	0.169	0.050	0.141
1	0.125	0.116	0.050	0.055
2	0.125	0.088	0.050	0.053
3	0.125	0.118	0.050	0.053
4	0.125	0.090	0.050	0.069
5	0.375	0.418	0.750	0.628

Source: Rapoport and Almadoss (2000).

modal choice is to bet the whole endowment of 5, as predicted.[21] Individual choices show some purification because there is a wide dispersion in the number of times a subject invests the entire endowment. In game L, for example, one-fifth of the subjects hardly ever invest the whole endowment *e* and a similar number invest it almost every period.

Collins and Sherstyuk (2000) looked at a simultaneous spatial location game (à la Hotelling). Three subjects simultaneously chose integers in [0,100] which correspond to "locations" of firms. Simulated customers are located at each of the 100 points and buy units from the firm nearest to them. Firm profits are proportional to units sold. Thus, subjects want to choose a location that is as far away from the other firms as possible so they can sell the most units to nearby customers (knowing other firms are trying to do the same).

Brown-Kruse, Cronshaw, and Schenk (1993) studied the classic two-firm location game, in which firms should snuggle together back-to-back at 50 (and do, experimentally). Hück, Müller, and Vriend (2002) studied a four-firm game in which firms should locate in two clusters, at 25 and 75. They found clustering at those locations, but also disequilibrium clustering in the middle.

The three-firm game is interesting because firms should avoid clustering in the middle so that they do not get squeezed by firms on both sides. Shaked (1982) showed that the unique symmetric MSE is for each firm completely to avoid locations below 25 and above 75, and to randomize uniformly over the

[21] Rapoport and Amaldoss estimate three learning models discussed in Chapter 6: experience-weighted attraction (EWA), reinforcement, and belief learning. EWA fits slightly better than reinforcement, but there is not much learning and none does a good job at fitting the data over time.

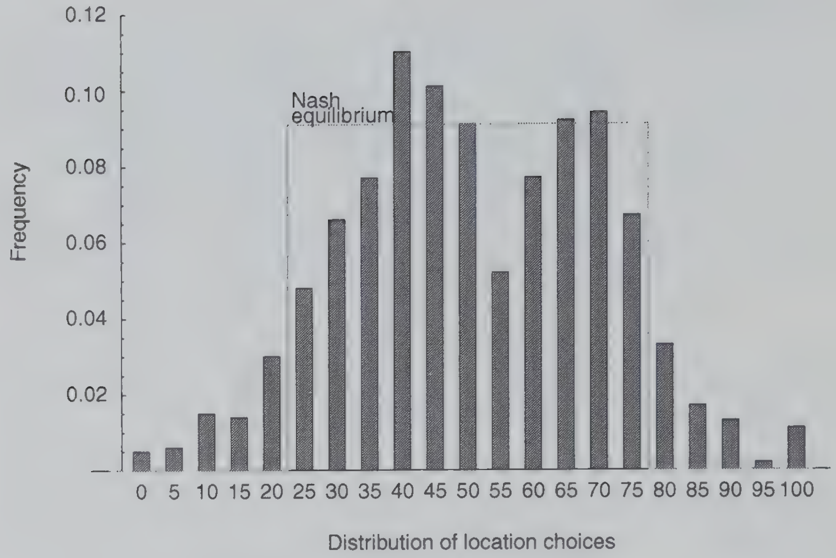


Figure 3.2. *Frequency of location choices in three-person simultaneous Hotelling game. Source: Based on Collins and Sherstyuk (2000).*

interval [25,75]. This is a bold prediction: It implies that half of the locations will never be chosen, and all others will be chosen with equal frequency.

Distributions of locations were similar across sessions and over time, so the aggregate distribution is shown in Figure 3.2 (along with the Nash equilibrium, a thick line). Choices look like a smoothed version of the equilibrium: Low (< 25) and high (> 75) choices are rare, and most choices are in the middle. There is a dip at 55 and two prominent modes at 40 and 70—subjects are avoiding the center for fear of being squeezed. The center-avoidance can be explained by adding risk-aversion to MSE or assuming behavior is consistent with approximate (ϵ) equilibrium.[22]

Summary: Experiments modeled after patent races and three-firm spatial location show strong, and surprising, consistency with counterintuitive MSE predictions.

[22] The expected payoff for each of the locations [25,75] is the same, but the standard deviation is highest at 50 (because of the "squeeze risk") and falls as you move to either side. Risk-aversion may therefore explain why subjects avoid the center and cluster around 40 and 70. Collins and Sherstyuk also compute ϵ-equilibria (in which subjects choose strategies that are within $1 - \epsilon$ of best-response utility) and show that behavior is consistent with these equilibria with $\epsilon = 1$ percent or 5 percent.

3.6 Two Field Studies

In sports, players must randomize a physical move—where a pitch is thrown, a soccer ball is kicked, a tennis ball served—or be vulnerable to exploitation by another player who can guess where the ball is heading. Tennis is a good game to study because serves are hit to the left (L) or right (R) side of the opponent's court, and it is easy to see where the serve went. In a mixed equilibrium with competitive players, the expected payoffs from serving L or R should be equal.[23] If the payoffs from L and R are different, the players are not mixing properly.

Walker and Wooders (2001) collected data from ten matches between famous tennis pros, for the period 1974–97. They chose important matches (Grand Slam and Master's tournaments) that were long enough that many points were generated, to permit powerful statistical tests.[24] Testing the equilibrium prediction is simple: Are the relative frequencies of winning on L and R points close together?

The answer is yes. Each match has two players serving from two different halves of the court (called the "ad" and "deuce" court), so the data have a total of forty comparisons from the ten matches. The L and R winning frequencies are statistically different (at $p < .10$) in only two of the forty comparisons, compared with fifteen out of fifty win-rate differences in O'Neill's (1987) experiment.

The over-alternation of strategies observed in the lab is present in the tennis data too, although weaker: In eight of forty comparisons, there are either too many (six of the eight) or too few (two of the eight) runs at the $p < .10$ level.

Those who don't think experimental results generalize to natural settings are tempted to crow, "See! When the stakes are high among experienced players game theory *does* work!" Walker and Wooders (2001, p. 1535) draw a more thoughtful and even-handed conclusion:

> We do not view these results as an indictment of the many experiments that have been conducted to test for equilibrium play. The experiments have established convincingly that in strategic situations requiring unpredictable play, inexperienced players will not generally mix in the

[23] This isn't generally true, of course. If Pete Sampras serves to me, he can always serve to the same side of the court and still win every point. The MSE holds when players are competitive enough that consistently serving to one side of the court reduces a player's expected payoff, compared with an interior mixture.

[24] Miguel Costa-Gomes (personal communication) pointed out that choosing long matches could create a selection bias. In matches where one player is deviating from the proper MSE, that player is likely to lose more quickly; so excluding such matches may create a selection bias in favor of matches that are unusually close to MSE. Since the Walker–Wooders results *are* quite supportive of MSE, this unmet criticism is a real concern. Measuring the magnitude of any such selection bias requires comparison of long and short matches, which hasn't been done yet.

> equilibrium proportions. . . . There is a spectrum of experience and expertise, with novices (such as typical experimental subjects) at one extreme and our world-class tennis players at the other. The theory applies well (but not perfectly) at the "expert" end of the spectrum, in spite of its failure at the "novice" end. There is a very large gulf between the two extremes, and little, if anything, is presently known about how to place a given strategic situation along this spectrum.

Note that there are two interesting ways in which previous experiments *do* jibe with what is found in tennis. First, Neuringer (1986) found that in experiments with very large amounts of training—perhaps comparable to the thousands of hours of practice serious tennis players engage in—subjects in the lab *could* learn to randomize. The tennis study is a kind of field replication of this experimental finding. Second, the tennis players over-alternate just as lab subjects do. Indeed, the presence of equal L–R win rates *and* statistically significant temporal dependence suggests that players receiving serves were not able to detect patterns in the serves. This is a reminder that behavior consistent with MSE could come about in two very different ways: Players could be truly randomizing; or they could exhibit temporal dependence which their opponent doesn't detect. The latter possibility means MSE is still an equilibrium in beliefs, but players' beliefs do not use information that they could (namely, temporal dependence).

Palacios-Heurta (2001) essentially replicated the Walker and Wooders paper, using data on penalty kicks in European soccer. Penalty kicks occur after certain penalties, and allow the kicker to place the ball on a penalty mark 12 yards from the goal. Other players can line up between the kicker and the goal but must be 10 yards from the ball. (Players usually bunch together in the middle, in effect forcing the player to kick either left or right.) Since it takes only 0.3 seconds for the ball to zoom into the net, the game is essentially a simultaneous-move game: The kicker usually aims for a left (L) or right (R) corner, and the goalie must commit, leaping to the kicker's L or R, before seeing where the ball is headed.

An advantage of Palacios-Huerta's study is that he is easily able to code the moves of *both* goalie and kicker, so he can tell whether they are in a mutual best-response equilibrium. (Recall that Walker and Wooders code only the direction of the serve, so it is possible given their results that servers are not minimaxing and *receiver* win rates from standing left or right are not equal.) Another advantage is that he has a sample of *all* penalty kicks, so there is no possible bias toward selection of matches that are more likely to be in equilibrium, as in Walker and Wooders.

He finds the same basic result as Walker and Wooders: Win rates for L and R kicks, and L and R goalie moves, are quite close. Aggregate results are summarized for left- and right-footed kickers in Table 3.15. The table

Table 3.15. Scoring rates for left and right moves in soccer

	Goalie move direction		
Kick direction	L	R	Overall
Left-footed kickers			
L	0.62	0.95	0.76
R	0.94	0.61	0.81
Overall	0.76	0.80	0.78
Right-footed kickers			
L	0.50	0.94	0.76
R	0.98	0.73	0.83
Overall	0.77	0.82	0.80

Source: Palacios-Huerta (2001).

shows scoring rates for each combination of L and R kicks and goalie moves, and overall rates. The overall rates are very close for L and R for both kicks (rightmost column) and goalie moves (bottom row), for both types of kickers. The differences between L and R scoring rates on a player-by-player basis are very close to what you would expect under randomness. There is also no serial correlation in players' moves. But this is not surprising because there are long time lags between kicks (there are only a couple per game, and usually not by the same kicker) so the bias toward small-sample representativeness of temporally local sequences probably does not occur. Chiappori, Levitt, and Groseclose (2001) find basically the same result from European penalty kicks.

Summary: In MSE, serving to the left and right sides of the court or kicking toward the left or right of a soccer goal should yield the same frequencies of winning points. This prediction is confirmed in data from ten major tennis tournaments and from soccer matches.

3.7 Conclusion

This chapter has reviewed studies in which subjects play games (mostly zero-sum) with unique mixed-strategy equilibria (MSE). These games are important because MSE can be derived without the sophisticated logic required in later chapters, and social preferences to help or hurt others are limited, so game theory may predict better than in other games. At the same time, players may not actually choose equilibrium mixtures because the incentives to learn MSE are weak and proper randomizing conflicts with intuitive misperceptions of what random sequences look like.

Early studies of games with MSE are difficult to interpret because performance-based incentives were low or nonexistent, and subjects usually played against a computerized strategy whose details were not known (so there was no reason for them to choose an MSE mixture). Modern studies use higher incentives, control for possible risk-aversion, and pit two people against each other. The typical result in these modern experiments, replicated very reliably, is that aggregate frequencies of play are surprisingly close to the MSE predictions in magnitude, but the deviations are large enough to be statistically significant. The results can be summarized visually in Figure 3.1 (previewed earlier), which uses most of the data reported in this chapter. Each point in Figure 3.1 graphs the MSE prediction for a specific strategy against that strategy's observed relative frequencies.[25] The actual frequencies are sprinkled around the MSE predictions. Frequencies are a little too high for low MSE predictions, and too low for high MSE predictions. The mean absolute deviation is only 0.057 and the R^2 is .84.

MSE does well in two important senses. First, the MSE predictions are precise and counterintuitive. If you were to give these payoff matrices to somebody who knew little about game theory, and asked them to predict the probabilities with which people choose different strategies, it is unlikely that they would derive anything remotely as accurate as the MSE predictions. Many people would be surprised at where the MSE predictions come from, and might be even *more* skeptical after you go through the calculations! Good theories are not just right, they are boldly and surprisingly right. Using these criteria, MSE is a good theory, although the deviations are significant.

Second, deviations are damning only if they suggest a better alternative theory. However, it is hard to think of a radical alternative to MSE to explain observed deviations. Learning theories are a natural candidate, but there is often little apparent learning in these MSE games and the data are too statistically noisy to permit accurate identification of how players are learning (see Chapter 6).

A candidate alternative theory is quantal response equilibrium (QRE). Figure 3.3 plots observed frequencies against QRE estimates, for the subsample of games where QRE estimates have been computed (reported throughout this chapter). The points are a little closer to the identity line in Figure

[25] The graph uses $n - 1$ data points from games with n strategies, since using all the data creates an obvious "double-counting" because the relative frequencies in a game add to one. In games with three or more strategies, the strategy with an MSE proportion closest to 0.50 is excluded, unless there are two or more strategies with equal predicted frequencies; then one of the strategies predicted to be equiprobable is excluded. Furthermore, strategies that are predicted to be played with zero probability are always included, to see at a glance how often weak dominance is violated.

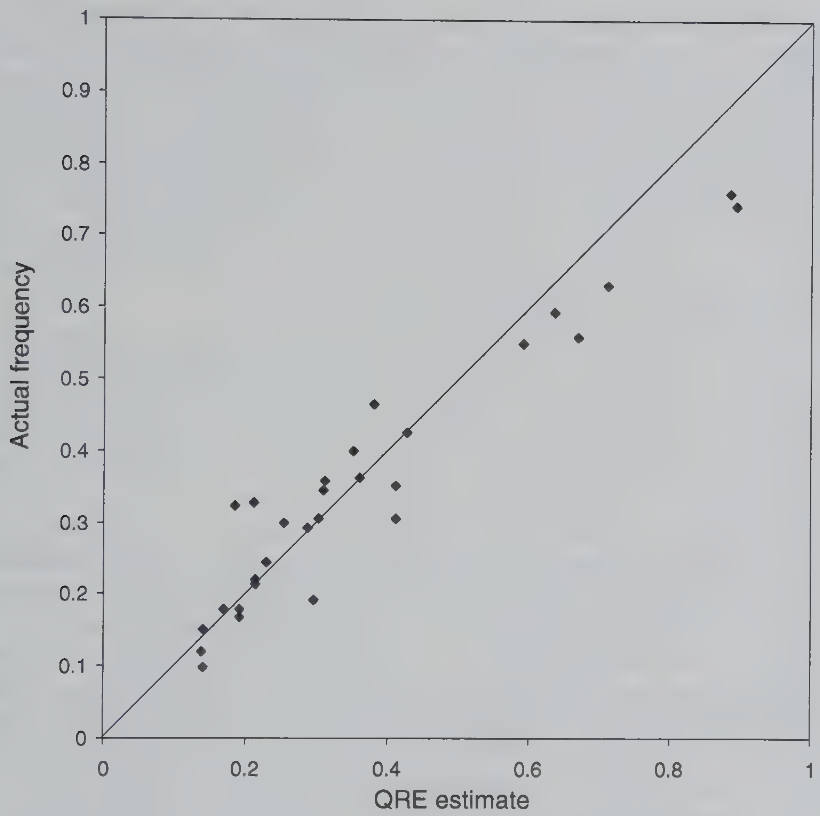


Figure 3.3. *Frequencies of different strategy choices predicted by quantal response equilibrium and actual frequencies.*

3.3 than in Figure 3.1, which is visual evidence that QRE is reducing deviations.[26] However, QRE predicts certain kinds of payoff sensitivity which are not observed in the data (see McKelvey, Palfrey, and Weber, 2000), so there is room for improvement (see also Goeree, Holt, and Palfrey, 2000).

In repeated games with the same partner, a player should choose randomly from an iid distribution so an opponent cannot see any temporal dependence in a player's sequence of choices and outguess her. In fact, there is pronounced temporal dependence in virtually every game where

[26] The R^2 for the QRE–actual Figure 3.3 plot is .92, compared with .84 for the corresponding subsample of points out of all those in Figure 3.1. (Not all Figure 3.1 points are in Figure 3.3 because there are not QRE estimates for all the games.) The mean absolute deviation is 0.047, compared to 0.067 for the same points in the MSE-actual comparison.

scientists have looked for it: Players alternate choices too frequently. It appears that players have limited working memory (recalling seven to eight previous plays, consistent with the typical measured size of short-term memory) and try to balance the number of previous choices to represent the MSE proportions. These two ingredients lead to over-alternation, and are parametrically quite accurate in explaining the sequences subjects produce. The fact that younger children do *not* over-alternate is an important part of the psychological story too—as their minds develop, they learn away from randomization toward the mistaken idea that short sequences should represent the properties of the underlying statistical process.

A modern interpretation of MSE is that players need not actually randomize, as long as other players cannot guess what they will do. An MSE can be an "equilibrium in beliefs": Players' beliefs about the likely frequency with which their current opponent will choose different strategies are correct on average, and make them indifferent about which strategy they play. Three recent studies allowed players explicitly to randomize, so that the experimenter could observe whether they are truly mixing or not. Many players *do not* explicitly mix in any given period, but aggregate frequencies are close to the MSE proportions, which is consistent with an equilibrium in beliefs.

A clever field study by Walker and Wooders using data from tennis tournaments exploits the fact that, in an MSE, a player's expected payoffs to different strategies should be equal if other players are randomizing. The percentage of points won when serving to the left- and right-hand sides of the court, in a sample of ten major tennis tournaments, is statistically very close, as MSE predicts. These top professional players also over-alternate as we see in the lab (although less strongly). Seasoned pros with much at stake and decades of experience seem to have learned how to mix; laboratory subjects have not. Since most naturally occurring game playing probably lies between these extremes, having both sorts of data available is useful. What we should be doing now is searching for general theory to explain the full range of behavior.

4
Bargaining

BARGAINING IS THE PROCESS by which economic agents agree on the terms of a deal. Defined this broadly, bargaining is possibly the most basic activity in economic life. Even in thick, competitive markets where traders closely resemble the "price-takers" of economic theory, haggling occurs over time of delivery, repairs, side payments, and quality, as well as price. Prices on commodities exchanges, for example, are actually bargains between traders who shout at each other in the pit.

Since bargaining is central to economic life, it has been the subject of constant attention. Edgeworth (1881) created the famous "Edgeworth box" to show the range of possible outcomes of a bargain, but was frustrated by his inability to reach a unique solution. Zeuthen (1930) and Hicks (1932) later defined theories of bargaining which proceeded in several steps, to describe bargaining as it often takes place and to come to a sharp prediction about what outcomes would result. Nash (1950, 1951) approached bargaining in two different ways. First, he proposed a series of axioms that any reasonable bargaining solution should obey, and showed that maximizing the product of the agents' utilities (beyond their utilities from an exogeneous "threat point") was the solution that uniquely satisfied his axioms (subsequently called the "Nash bargaining solution"). Second, in a quite different paper, he proposed a "noncooperative" solution in which predicted outcomes depend on the structure by which the bargaining took place. Nash envisioned a unification of the two approaches—the "Nash program." The unification finally came in the 1980s, when Binmore, Rubinstein, and Wolinsky (1986) showed that subgame perfect equilibria of noncooperative games with a certain alternating-offer structure were the same as the utility product maxima prescribed by the Nash bargaining solution.

Because bargaining seems so indeterminate, experiments on what happens proved useful (beginning with Fouraker and Siegel, 1963). This chapter reviews experimental studies of bargaining. As throughout the book, I primarily describe cumulation of regularities from experiments by comparing behavior with sharp theoretical predictions. Empirical regularity then suggests psychological principles that can be formalized in behavioral game theory. The interested reader should also read Roth's (1995b) chapter in the *Handbook of Experimental Economics*, which covers much the same ground with important differences.

Paralleling Nash's two-pronged approach, experimental studies of bargaining can be divided into two classes: In *unstructured* bargaining, the details of how the bargaining proceeds (the type of messages players can send, the order in which they make offers, and so forth) are left up to the players; in *structured* bargaining, the details of the bargaining procedure are specified by the experimenter. Structured experiments have the advantage of enabling an observer to predict what bargaining outcomes might occur from theories of noncooperative equilibrium behavior. Unstructured bargaining tells us what results when players are free to invent their own rules, and is arguably a better model of naturally occurring bargaining.

A quite different approach to the study of unstructured bargaining has flourished in applied psychology, under the rubric of "negotiation research" (see Bazerman et al., 2000, for a review). Negotiation research blends three elements: an early interest among social psychologists (beginning in the 1960s) in negotiation as an area of application; Raiffa's (1982) use of elements of game theory and decision theory to improve negotiating skills; and the idea from 1980s behavioral decision research that we can identify systematic ways in which negotiators depart from the prescription of decision analysis.

In one of several widely used negotiation paradigms (Bazerman, Magliozzi, and Neale, 1985), participants negotiate over numerical or categorical levels of each of several issues (e.g., prices or quality) using free-form communication with a time deadline. They each have a private point schedule that shows how many points they earn depending on how each issue is resolved (but little information about what other players' point schedules might look like). Since subjects have clear preferences, it is easy to evaluate whether an agreement is Pareto-efficient, which side benefits most, and so forth. One general finding is that deals are not Pareto-efficient, and are affected by systematic heuristics (e.g., players instinctively assume points sum to a constant, so the negotiation is purely competitive), normatively irrelevant factors (e.g., whether points are positively or negatively "framed"), and other cognitive variables that are not part of game-theoretic bargaining theories.

To date, there has been little mutual influence between negotiation research and the game theory experiments reported in this chapter (though some researchers do both, and the overlap is growing). Most negotiation researchers think game theory assumes too much rationality to be descriptively accurate, while experimental economists view game theory as an approximation that can be improved by careful observation. Many negotiation researchers are psychologists who care more about cognitive processes, rather than simply focusing on outcomes as economists typically do. The style of experimentation in negotiation research also makes it hard to know which kind of game theory to apply (since the negotiations are dynamic games of incomplete information designed to reflect the complex world of actual negotiation rather than the simplified world of theory). Many negotiation researchers also teach in business or policy schools and don't find game theory the most useful prescription for improving negotiation. It is tempting to conclude that the research interests of experimental economists and negotiation researchers are simply more different than they appear at first blush; so both sides can safely ignore each other. But many of the research questions *are* the same, and are arguably converging (especially as experimental economists become more interested in issues such as mental representation, learning, and bounded rationality, which have long been on the minds of negotiation researchers). And there are surely some unexploited gains to intellectual exchange between the two fields.

4.1 Unstructured Bargaining

4.1.1 Unstructured Bargaining over Ticket Allocations

Early studies centered around the cooperative Nash bargaining solution. Because the bargaining solution did not specify a protocol or set of rules, these experiments usually gave subjects a period of time in which to bargain and did not restrict communication at all.

Nash's solution assumes that the key structural features in any bargaining situation are the set of feasible agreement points (S) and the disagreement point (d_1, d_2). Nash showed that there is a unique Pareto-optimal solution point S^* which obeys several appealing axioms.[1] The Nash solution is the point that maximizes the *product* of the utility gains above the disagreement point, i.e., $S^* = \text{argmax}_{(x_1,x_2)}(x_1 - d_1)(x_2 - d_2)$.

A crucial assumption is that points in S are assumed to be measures of the preferences a player has for various bargaining outcomes. In practice, of course, this requires some specification of a utility function for outcomes. In early tests, when the Nash solution predicted unequal monetary payoffs,

[1] The axioms are symmetry (if $d_1 = d_2$, if the point (x_1, x_2) is in S^* then (x_2, x_1) is also); independence of irrelevant alternatives (if the solution to a game (S, d) is contained in a subset T of S, then that point is the solution to (T, d) also); and independence of the solution from affine transformations of payoff utilities.

players often deviated in the direction of equal payoffs (e.g., Nydegger and Owen, 1975; Rapoport, Frenkel, and Perner, 1977). However, these early experiments did not control for the way in which payoffs yield utility. As a result, a rejection of the theory is a rejection only of the *joint* hypothesis that the solution is being applied *and* that payoffs are mapped into utilities in a particular way (typically risk-neutrality).

This realization led Roth and Malouf (1979) to look for a procedure that might permit them to know how bargaining payoffs lead to utilities. There are three general strategies for controlling preferences in experiments: assume, measure, or induce (see Chapter 1 for more discussion of methodology). Early studies assumed preferences were risk neutral. An alternative strategy is to elicit measures of utility for various bargaining outcomes, and plug these measurements into the theory to generate predictions (e.g., Murnighan, Roth, and Schoumaker, 1988). Yet another alternative is to try to induce (or control) preferences.

Roth and Malouf (1979) used a "binary lottery" technique to induce risk-neutrality, first proposed by Cedric Smith (1961) and later generalized to induce any risk preference by Schotter and Braunstein (1981) and Berg et al. (1986). The idea is that players bargain over a distribution of one hundred lottery tickets. Each player has a fixed cash prize, and the number of lottery tickets they get by bargaining determines their chance (out of one hundred) of earning the fixed prize. A player who bargains for seventy-two tickets, for example, has a 0.72 chance of winning her fixed prize and a 0.28 chance of winning nothing.

The binary lottery technique induces risk-neutral preferences if subjects are indifferent between compound lotteries and their single-stage equivalents (e.g., if they are indifferent between having a 0.5 chance of thirty-two tickets, or having sixteen tickets for sure). I am cautiously pessimistic about success of the binary lottery technique, because reduction of compound lotteries has failed repeatedly in many direct and indirect tests. Furthermore, there are *no* published studies showing that paying in lottery tickets actually works and gives different results than paying money (though Prasnikar, 1999, is supportive). However, the technique may prove useful once the conditions under which it works are better established (see Chapter 1 for a longer discussion.)

Nash's theory takes as an axiom that bargaining solutions should not be sensitive to affine transformations of payoffs (i.e., adding a constant or multiplying by a positive constant). This is actually quite a strong property. It implies, for example, that bargaining over lottery tickets should not be sensitive to the size of the monetary prizes players can earn if they win their lotteries (or to their information about those prizes). Roth and several colleagues conducted a thorough series of tests, which generally reject this property and suggest an alternative.

Table 4.1. *Results in binary lottery bargaining games*

Information condition	Money prizes	Number of tickets for player 2							Fraction of disagreement
		20	25	30	35	40	45	50	
Full	($1,$1)	0	0	1	0	1	0	20	0.00
	($1.25,$3.75)	1	6	3	2	2	1	4	0.14
Partial	($1,$1)	0	0	0	0	0	1	14	0.06
	($1.25,$3.75)	0	0	0	0	0	3	13	0.00

Source: Roth and Malouf (1979).

In Roth and Malouf (1979), players bargained over allocations of one hundred lottery tickets. Tickets determined the chance of winning either equal prizes ($1) or unequal prizes ($1.25 for player 1 and $3.75 for player 2). They also varied whether both subjects knew both prizes ("full information"), or each subject knew only her own prize ("partial information").[2] Under all conditions, the Nash bargaining solution predicts the 100 chips would be divided evenly.

The number of agreements giving different fractions of lottery tickets is shown in Table 4.1. Although the sample is very small (only nineteen bargaining pairs), the results are very clear, and replicate earlier findings in which tickets were converted into money at different rates (e.g., Nydegger and Owen, 1975). Agreements cluster heavily on 50–50 splits and disagreement is rare except when *both* subjects know that prizes are unequal.[3] When player 2 has the larger $3.75 prize, the allocation of twenty-five tickets to her and seventy-five to player 1 equalizes the expected *dollar* payoffs of the players. When prizes are known to be unequal, players seem to be torn between dividing tickets evenly (giving fifty to player 2) and dividing tickets unevenly to equate dollar payoffs (giving twenty-five to player 2), and there are also many agreements between these two modes. Players are clearly sensitive to

[2] They also varied the maximum number of tickets that player 2 could earn. In their games 1 and 3 the maximum was one hundred; in games 2 and 4 the maximum was sixty. This variation is important because a solution proposed by Raiffa (1953) is sensitive to the maximum payoff a bargainer can get. Raiffa's idea is that the hypothetical ideal point, $(\bar{x}_1, \bar{x}_2)$, consisting of the maximal feasible payoffs each player might get, could influence the bargaining (even though this point is usually not itself a feasible outcome). Raiffa suggested that the *ratio* of the players' gains, relative to the disagreement point, should equal the ratio of the gains from the disagreement point to $(\bar{x}_1, \bar{x}_2)$. Kalai and Smorodinsky (1975) later showed that Raiffa's solution is the unique solution following from the four Nash axioms, with a property called individual monotonicity substituted for independence of irrelevant alternatives. In fact, behavior in games 1 and 2 is quite similar, and behavior in games 3 and 4 is quite similar, so that Raiffa's idea is rejected in this experiment.

[3] This strong tendency to conform to a simple focal division is also observed in the field; see Young and Burke (2001) on sharecropping contracts in Illinois.

money payoffs which result from various ticket allocations, violating the axiom of independence from affine transformations.[4] There are also more disagreements (14 percent) when prizes are known to be unequal. Roth and Murnighan (1982) replicated this result in a more complex design (see Roth, 1995b, for details).

Murnighan, Roth, and Schoumaker (1988) pointed out an interesting regularity in several of their earlier experiments: Virtually all pairs settled in the final minutes of the nine to twelve minutes allotted. The obvious explanation is that players are trying to convey private information about stubbornness or costs of delay (see Roth, 1995b, 323–27).

Roth and Schoumaker (1983) explored focal points in bargaining with a neat twist: Players bargained with a computer that was programmed (unbeknownst to them) to give the subject a disproportionate share. As a result, the players developed expectations about receiving disproportionate shares. Then players began to bargain with other human players, and all players' histories were commonly known. Players who had "strong reputations," having received generous agreements in the past, were able to get generous agreements when playing against the new subjects. Reputations that were developed exogeneously determined later equilibrium allocations.

A similar focal point effect in bargaining was documented by Mehta, Starmer, and Sugden (1992). In their games two players divide £10 in a "Nash demand game." Both players state a demand. If the demands add to £10 or less, they get their demands; otherwise they get nothing. Before bargaining, subjects are dealt four cards randomly from a deck with eight cards: four aces and four deuces. Since subjects know the composition of the deck, they can tell from their own hand how many aces the other subject has (namely, four minus their own number). Subjects were told that all four aces were worth £10 together, so that to earn money they had to pool their aces and agree on how to divide the £10 in unstructured bargaining. The game is like two players bringing resources to a partnership, which are worthless outside of the partnership but which might be perceived as affecting how much of the gains they are entitled to. The aces were worthless—having more of them did not improve a person's outside option payoff—but they might still create focal points. For example, if a person who was dealt one ace thought that the person with the three aces would demand £7.50, then the first person should demand only £2.50.

[4] Roth and Malouf propose an alternative axiom, in which allocations are independent of transformations of payoffs that preserve ordinal preferences, and that preserve information about which player makes larger gains at any given payoff. They show, rather remarkably, that substituting this alternative axiom in lieu of Nash's, along with Pareto-optimality, symmetry, and independence of irrelevant alternatives, implies a unique solution that yields the largest *minimum* gain from disagreement for the two players. This point always equalizes gains, and has a maximin or Rawlsian flavor. If players are bargaining over expected dollar payoffs rather than tickets, this solution is the (25,75) point frequently observed in the data.

Table 4.2. Bargaining demands by number of aces held

	Number of aces		
Demand	1	2	3
£2.50	11	0	0
£3.00–£4.50	5	1	1
£5.00	16	40	17
£5.50–£7.00	0	1	11
£7.50	0	0	4
Sample size	32	42	33

Source: Mehta, Starmer and Sugden (1992).

Table 4.2 shows the distribution of demands.[5] When both bargainers had two aces, there is a very sharp agreement on an equal split (demanding £5). However, when one had one ace and the other had three, about half the subjects demanded half the pie, and the other half demanded a fraction equal (or near) to the fraction of aces they held. As a result, there is 22 percent disagreement in cases with one and three aces. These results show how descriptions of the game that are completely payoff irrelevant can affect bargaining substantially (and, by introducing competing focal points, also create disagreement).

Roth (1985) proposes a simple way to explain outcomes, and disagreement rates, as the result of coordination among multiple focal points. Suppose players make simultaneous proposals to divide one hundred tickets either (50,50) or $(h, 100-h)$, which gives a fraction h to player 1 and $1-h$ to player 2. This coordination game has a mixed-strategy equilibrium in which player 1 demands the larger amount h with probability $(h-50)/(150-h)$ and player 2 demands (50,50) with probability $(h-50)/(h+50)$. The mixed-strategy equilibrium predicts a very specific disagreement rate, $(h-50)^2/[(150-h)(50+h)]$. Roth's earlier (pre-1985) experiments with different values of h that equalize dollar payoffs allow a test of the theory. Predicted disagreement rates in the mixed-strategy equilibrium in three different h conditions are 0, 7 percent, and 10 percent. The corresponding observed disagreement rates are 7 percent, 18 percent (22 percent in Mehta et al.), and 25 percent. The observed disagreement

[5] No subjects were dealt zero or four aces, although their behavior would be interesting.

rates are too high, but they do rise with h as predicted and are close enough to encourage a further test.

Murnighan, Roth, and Schoumaker (1988) designed a test with a wider range of prize pairs, creating equal-dollar focal points at h values of 60, 70, 80, and 90. The disagreement frequency was constant across h, although it should vary from 1 percent to 19 percent if the coordination failure model is right, so the coordination model did not hold up as well as hoped.

4.1.2 Self-Serving Interpretations of Evidence in Unstructured Bargaining

Serious bargaining disputes, such as divorces, wars, and strikes, are often inflamed by a difference in the bargainers' perceptions about what is fair. This difference is often "self-serving": Players believe that what is better for them is also fair.

For example, in the studies by Roth et al. described in the previous subsection, the self-serving bias predicts that players will be drawn to the focal point that gives them more. Indeed, Roth and Malouf (1979) report that, in the messages players sent to one another, arguments in favor of the (50,50) split of lottery tickets almost always came from high-prize players (who get more than from equal-ticket splits than they would from equal-payoff divisions). The model of disagreement as coordination failure could be easily extended to incorporate self-serving bias if players choose the equilibrium demand that benefits themselves with a probability that is *greater* than the mixed-strategy equilibrium probability. This extension would raise the predicted disagreement rate, bringing it closer in line to the data.

Kagel, Kim, and Moser (1996) report evidence consistent with self-serving biases from ultimatum game bargaining (see Chapter 2). Closely related examples are games in which the presence of an outside option can create multiple focal points and disagreement results if players favor the focal points that are better for themselves (see Knez and Camerer, 1995, in Chapter 2). Similar results are reported in outside option games by Binmore, Shaked, and Sutton (1989) and Binmore et al. (1998) (see below). When outside options are large enough to create a substantial gap between the equal-split and equal-surplus points, disagreement is common.

There is a large body of literature in psychology, some of which has seeped into economics, on the degree to which preferences influence beliefs self-servingly (see Babcock and Loewenstein, 1997). Self-serving biases come in many varieties.

Many studies show that people are overconfident about how they compare with others. For example, *everyone* thinks their sense of humor is above average. In a College Board study of a million high school students, virtually all students rated themselves as being at least average at "getting along well with others" and a quarter of the students said they ranked in the top

1 percent. A related phenomenon is "wishful thinking"—the belief that good outcomes are particuarly likely.[6] However, these expressed beliefs have been linked to monetary rewards in only a few studies.[7]

Loewenstein et al. (1993) and Babcock et al. (1995, 1997) conducted a thorough series of experiments on self-serving biases (see also Thompson and Loewenstein, 1992, and Gächter and Reidl, 2000, for a clever recent paper). In their studies, pairs of subjects bargained over how to settle a legal case, adapted from an actual suit and consisting of twenty-seven pages of background, depositions, and exhibits. In the case, a motorcyclist plaintiff sues an automobile driver defendant for $100,000 in damages for injury. Subjects had thirty minutes in which to settle the case by agreeing on a payment from the defendant to the plaintiff. Every five minutes, both sides incurred $5,000 in legal fees. If the case did not settle after six periods, an award was imposed by a judge (an actual retired judge who presided over such cases). Before negotiating, subjects guessed the amount the judge would award and what they thought was fair. Subjects were paid either in dollars, scaling $10,000 case dollars to one actual dollar, or in grade points derived by comparing the performance of each player in a given role to others in the same role.[8] Subjects were students at Carnegie-Mellon (in public policy), Penn (undergraduate business), Texas (law), and Chicago (MBA).[9]

Some summary statistics are shown in Table 4.3. In the two control conditions, settlement takes three to four five-minute periods on average and about 70 percent of the cases settle. The gap between the plaintiff's guess about the expected judgment and the defendant's (shown in the second-from-right column) is about $20,000, which is a substantial disagreement for a case worth up to $100,000. The difference in expected judgments is also highly predictive of whether bargaining pairs settle or not.

[6] Forsythe, Rietz, and Ross (1999) show wishful thinking effects in experimental "political stock markets." Bar-Hillel and Budescu (1995) note that wishful thinking is often hard to establish empirically. A likely explanation is that wishful thinking is partly offset by "defensive pessimism," which leads people who strongly prefer an event to occur to hedge their emotional bet by downplaying its likelihood to reduce ex post disappointment.

[7] Camerer and Lovallo (1999) found that overconfidence about their relative skill in trivia led subjects to enter a competitive market, in which more skilled players earned more, too frequently. The result was that, collectively, entrants were guaranteed to lose money. Another example is the reaction of traders in the Iowa "experimental political stock market" to common news. In those markets, traders purchase shares that pay a dividend if a particular candidate wins an election (see Forsythe, Rietz, and Ross, 1999). After a televised presidential election debate between Bush and Dukakis, traders who were pro-Bush supporters before the debate bought more Bush shares (i.e., shares that paid a dividend if Bush won the election) and pro-Dukakis supporters bought more Dukakis shares. Both sides thought their candidate won the debate, and were willing to bet money that they were right.

[8] There was no significant difference in paying dollars or grade points. Indeed, a casual calculation in Camerer and Loewenstein (1993) suggests the grade-point scheme might create very large financial incentives, to the extent that superior grades help students get better-paying jobs.

[9] There were no differences among these subject pools.

Table 4.3. Settlement and judgment bias in "sudden impact" experiments

		Settlement statistics			Difference in E (judgment)	
Experimental condition	No. of pairs	Frequency	No. of periods	Standard error	Mean	Standard error
Control (knew roles)	47	72%	3.75	.28	$18,555	3,787
Did not know roles	47	94%	2.51	.21	–$6,275	4,179
Significance		(< .01)	(< .01)			(< .01)
Control	26	65%	4.08	.46	$21,783	3,956
List weaknesses	23	96%	2.39	.34	$4,676	6,091
Significance		(.01)	(.01)			(.02)

The cause of the self-serving bias, and a way to get rid of it, are established by different treatment conditions. The top panel shows the difference between a control condition, in which bargainers are informed of their role *before* they read the case, and a treatment condition, in which they read the case first, *then* find out their role, state what they think is fair and what they expect the judge to award, and bargain (see Babcock et al., 1995). If the self-serving bias occurs during the process of encoding case information, there should be no bias in the "did not know roles" condition.[10] Indeed, these subjects have no significant bias, and settle more rapidly (2.51 periods) and more frequently (94 percent) than the control group. Of course, assigning roles *after* learning about the case is not a practical way to reduce self-serving biases in practice because prospective defendants and plaintiffs know their roles in advance.[11] Therefore, Babcock et al. (1997) investigated other "debiasing" techniques that could reduce the self-serving bias and increase settlement. The bottom panel of Table 4.3 shows one technique that works—weakness listing. In this condition, after learning their roles and reading the case (but *before* saying what was fair and guessing what the judge awarded), these subjects were told about possible bias and asked to list weaknesses in their case. Listing weaknesses works: 96 percent of pairs settle, in an average of 2.39 periods, and there is no self-serving bias in expectations about the judge's award. Psychologists feel that they understand a phenomenon when they can "turn it on and off." (Affecting policy also

[10] Similar "top–down" encoding biases occur routinely in perception, categorization, and other cognitive processes. For example, after buying a new car, people suddenly notice more of the same type of car on the road.

[11] However, many lawyers say that part of their job is precisely this sort of "debiasing," by drawing a veil of ignorance in front of their clients' eyes or helping them see their cases as others would.

relies on this kind of understanding.) Weakness listing passes this test.[12] Formal models of self-serving bias could be built by allowing preferences to influence beliefs, then applying Bayes' rule (see Rabin and Schrag, 1999).

Summary: Bargaining outcomes are affected by focal points—psychologically prominent divisions which are noticed by both players. The dependence of outcomes on chip value observed by Roth et al. violates a basic assumption in the bargaining solution (that affine transformations of utility should not matter). Experimental outcomes can be interpreted as the result of bargainers trying to coordinate when there are multiple focal points (although a precise model of this sort did not predict well in the one study designed to test it). Self-serving biases occur when players think that an outcome that favors themselves is particularly likely. Experiments by Babcock, Loewenstein, Issacharoff, and myself show such biases in bargaining over a legal case, and the size of the bias in a pair predicts the length of costly delay and chance of settlement. The bias occurs largely during the encoding phase—when reading case facts, one's role biases what information is attended to.

4.2 Structured Bargaining

There are many experiments on structured bargaining. In this section I describe results from alternating-offer games with finite and "infinite" horizons, and games with random termination and outside options.

4.2.1 Finite Alternating-Offer Games

In naturally occurring bargaining, players often alternate offers (perhaps because making two offers in a row is a sign of weakness). Delay is costly because the amount being bargained over loses value (owing to conventional time discounting, impatience, or perishability), or because there is a fixed cost to delay (owing to opportunity costs from lost wages and profits in a strike, for example).

[12] Weakness listing helps because it improves the settlement rate, reducing the joint costs to delay. An interesting, unanswered, question is whether having one side list weaknesses benefits that side, if the other side does not. The net effect could go either way: The side that lists weaknesses is more inclined to settle, but might also settle on unfavorable terms. These "sudden impact" studies also illustrate how the contextual domain of the experiment can affect behavior. If self-serving biases occur because preferences affect how ambiguous information is encoded, or retrieved from memory, then it is important to conduct experiments in which encoding and retrieval biases may occur. Explicit random devices such as draws from bingo cages may produce different results since it is hard to misperceive the color of a bingo ball (though see Forsythe, Rietz, and Ross, 1999).

There is much theory about alternating-offer bargaining with costly delay, and several experiments as well (see Stähl, 1972; Rubinstein, 1982).[13] In theory, the player with the higher discount factor[14] has a large advantage (if others players know this). More patient players can afford to wait out a protracted process of bargaining and extract concessions from impatient players. An example is wealthy tourists travelling in relatively poor countries, trying to buy goods from local merchants. The merchants know that the tourists are in a hurry, and can often exploit their impatience by drawing out haggling over many minutes or hours. Children also have a knack for sensing disparities in waiting costs, realizing that a well-timed temper tantrum or "work slowdown" ("I *am* hurrying. . . . You know I just learned how to tie my shoes!") when a parent is in a hurry may give them power to bargain for candy or a cool car.

The first alternating-offer experiments were conducted by Binmore, Shaked, and Sutton (1985). They used a two-period game. In the first period player 1 offers a division of 100 British pence to player 2. If player 2 rejects it, then the "pie" being divided shrinks to 25p (i.e., $\delta = 0.25$) and player 2 makes a counteroffer to player 1. If player 1 rejects that counteroffer, the game is over and neither player gets anything (i.e., the second round of the game is an ultimatum game). The subgame perfect equilibria are centered around an initial demand of 75p, leaving 25p for player 2.

In the first period, there is a sharp mode around the equal split point (50p), some offers near the subgame perfect equilibrium of 25p, and quite a few offers between those two points. After the first play, subjects in the role of player 2 made hypothetical opening demands as player 1 in a second game B.[15] Their opening offers shift dramatically toward the perfect prediction of 25p.

Binmore et al. concluded that the experience subjects had in the player 2 role in the first game led them to realize that a sensible player 2 would accept any offer that leaves them 25p. Then they "exploit" this imagined behavior in their new roles as player 1 in the second game, offering only 25p. Further studies with this kind of "role reversal" protocol sometimes

[13] Rubinstein is careful to note that he does not think having a truly infinite horizon is crucial. Instead, the key point is simply that there is no end that is commonly known by the players, so the periodicity exploited to derive the solution is conceivable.

[14] The discount factor δ refers to the multiplier on future gains that measure their present value. The discount rate r is related to δ by $\delta = 1/(1 + r)$.

[15] The comparison of player-2 offers in the hypothetical second game with player-1 offers in the first game creates a triple "confound" in experiment conditions: The second-game offers could be different because of experience (second game versus first game), role reversal (player 2s making opening offers versus player 1s), or incentives (they were hypothetical in the second game versus real in the first game). The confounds are unfortunate because these data are the strongest evidence of rapid learning of perfect equilibrium.

Table 4.4. Frequencies of offers and rejections in alternating-offer bargaining

	Two rounds		Three rounds		Five rounds	
Offer category ($)	Offer	Rejection	Offer	Rejection	Offer	Rejection
> 2.50	—	—	0.10	—	—	—
2.50	0.05	—	*0.70*	—	0.05	—
2.01–2.49	—	—	0.05	—	—	—
1.71–2.00	0.03	—	0.08	—	0.38	0.067
1.70	—	—	—	—	*0.35*	0.071
1.51–1.70	—	—	—	—	0.10	—
1.26–1.50	*0.45*	0.11	0.08	0.67	0.05	—
1.25	0.38	0.20	—	—	—	—
< 1.25	0.10	1.00	—	—	0.08	1.000

Source: Neelin, Sonnenschein, and Spiegel (1988).
Note: Medians are in italic type.

show learning (see Harrison and McCabe, 1992, described below), but the learning is not as fast as Binmore et al. observed.

These data inspired a skeptical partial replication by Neelin, Sonnenschein, and Spiegel (1988). In their design, players participated in a two-round game like that of Binmore et al., with payoffs $5.00 and $1.25, a three-round game with pie sizes of $5.00, $2.50, and $1.25, and a five-round game with pies of $5.00, $1.70, $0.58, $0.20, $0.07.[16] By design, the subgame perfect equilibria are the same in all three games (player 1 should offer $1.25). Subjects were eighty undergraduate students at Princeton in an intermediate microeconomic theory course.

The results are shown in Table 4.4. The two-round game fails to replicate the Binmore et al. result, because there is a large frequency of opening-round offers of $1.25, close to the perfect equilibrium prediction. Opening-round offers are around $2.50 in the three-round game and $1.70 in the five-round game. The modal offer is just the second-round pie size, as if players truncate each game to just two stages and play the perfect equilibrium.

The difference between these results and those of Binmore et al. is curious. A likely explanation is that the subjects were economics undergraduate students, who learned one step of backward induction in an example involving a two-stage game, or perhaps simply induced it, then "overapplied" the two-round heuristic in the three- and five-round games.

[16] The games were played one after the other, in that order, so there was no attempt to control for an effect of the order in which a game was played, which is a small design flaw.

There are noticeable differences in the instructions between the Binmore et al. and Neelin et al. experiments as well. The key differences are subtle—only one sentence—but are worth dwelling on as a case study in methodology.

Binmore et al. were worried that subjects might think the experimenters expected them to be fair-minded, and would respond to this "demand effect." So they included a sentence (in capitals) stating, "YOU WOULD BE DOING US A FAVOR IF YOU SIMPLY SET OUT TO MAXIMIZE YOUR WINNINGS." Of course, this instruction risks bending over too far and inducing fair-minded subjects to behave "too" self-interestedly.

The instructions of Neelin et al. mention that "You will be discussing the theory this experiment is designed to test in class." This kind of instruction breaks a conventional rule in experimental economics: Try to isolate subjects' behavior in the lab from other uncontrolled influences that might affect their motivation.[17] A reminder that results will be discussed in class might have led subjects to conform more closely to some theory they were being taught. Since the Neelin et al. results *do* show more conformity with subgame perfection than in Binmore et al. in the two-round game, the possibility that subjects were trying extra hard to please the experimenter is consistent with the data and can be ruled out only by further experimentation.

Responding to the earlier conflict in results, Ochs and Roth (1989) ran a more complex experiment. Their design combined old and new structural features: The number of rounds was two or three, the discount factors were 0.4 and 0.6, and discount rates could be different for different players.[18]

Table 4.5 shows the parameter configurations, and perfect equilibria (dollars offered to player 2 out of \$30 total). The first four cells of the design, when $T = 2$, are simplest: In theory, player 1 should just offer player 2 the number of chips equal to her discount factor (δ_2) times \$30 (since the second period is an ultimatum game in which player 2 will claim all the chips). Comparing cells 1–4 ($T = 2$) and cells 5–8 ($T = 3$) shows that adding a third period lowers the amount player 2 should be offered, because being able to counteroffer in the third period gives player 1 extra bargaining power.

[17] For example, many experimenters pay subjects privately one at a time, in a separate room, while other subjects sit inside the lab for their names to be called, to make it more difficult for subjects to compare monetary earnings or implement post-experiment sharing between well-paid and poorly paid subjects. In addition, experimenters avoid using students in the same class, particularly in situations where their behavior might be scrutinized later in a public forum such as a class discussion.

[18] To implement different discount rates, players bargained over one hundred chips (worth \$0.30 each), but the amount of money the chips were worth was reduced each period according to each player's discount rate.

Table 4.5. Predictions and offers in alternating-offer bargaining

Cell	Number of rounds (T)	Discount factors δ_1	δ_2	Perfect equilibrium	Average opening offer Round 1	Round 10	Rate of rejection
1	2	0.4	0.4	12.00	13.19	12.03	0.10
2	2	0.6	0.4	12.00	14.73	14.34	0.15
3	2	0.6	0.6	18.00	13.88	14.70	0.13
4	2	0.4	0.6	18.00	14.67	13.57	0.20
5	3	0.4	0.4	7.20	13.02	12.81	0.12
6	3	0.6	0.4	4.80	14.04	13.17	0.14
7	3	0.6	0.6	7.20	13.93	13.70	0.15
8	3	0.4	0.6	10.50	13.90	14.23	0.29

Source: Ochs and Roth (1989).

Table 4.5 reports average opening offers in rounds 1 and 10, and the rate of first-round rejections across all ten rounds. Offers average around \$14, and do not vary nearly as much across cells as they are predicted to. There is significant learning between rounds 1 and 10 in cells 1, 3, and 4, but the change is in the wrong direction in cell 4.

Many of the predictions across cells go in the wrong direction, or are statistically weak. When $T = 2$, offers should not depend on player 1's discount factor δ_1 but later offers actually fall with δ_1. Player 2 should be offered less when $T = 3$ compared with $T = 2$, but this difference is significant in only two of four comparisons (cells 2–6 and 3–7). However, Ochs and Roth's statistical tests are conservative.[19] In fact, evidence for subgame perfection is mildly favorable when all comparisons are taken together: Across pairs of cells, there are twenty-five predicted differences in opening offers; seventeen differences go in the correct direction ($p = .05$ by a binomial test).

The analysis above concerns only opening offers to player 2. How often are offers rejected? And what happens when they are? The rightmost column of Table 4.5 shows rejection rates in each cell. The overall rejection rate of 16 percent figure is quite similar to the rates in earlier studies (15 percent in Binmore et al., 14 percent in Neelin et al.) and in ultimatum games (see Chapter 2). Counteroffers that follow rejections have two interesting properties. First, second-period counteroffers are rejected more often

[19] Their cross-cell tests use only round 10 data, comparing just ten offers with ten other offers, which lacks the power to detect modest effects. A better test would use all the data while somehow controlling for dependence over trials, or use a within-subjects design in which a single subject is exposed to different cells (i.e., a subject "acts as her own control group").

(40 percent) than opening offers are, and third-period rejections are even more common (54 percent).[20] Second, most (81 percent) counteroffers are "disadvantageous"—a player who rejects an offer then makes a counteroffer that leaves herself *less* than she was offered. Disadvantageous counteroffers are also common in the studies by Binmore et al. (75 percent) and Neelin et al. (65 percent), and *all* rejections in ultimatum games are disadvantageous (since they leave the Responder with nothing).

There are two plausible reasons for disadvantageous counteroffers: Either players do not try to maximize their own monetary payoffs (the "social utility hypothesis"); or players do not think ahead sufficiently to realize that, by rejecting X, they may be forced to earn less than X (a form of "limited computation").[21] Social utility and limited computation probably both contribute to the frequency of disadvantageous counteroffers, because there is evidence for both forces in other studies. Support for the limited computation hypothesis is found in "Mouselab" studies, described below, and Chapter 2 describes evidence of social utility.

One social utility model is worth noting here. Bolton (1991) conjectured that players care about money earnings *and* their relative share of earnings, and demand more money when their relative share is low. Now suppose players earn money only from their rank in a constant-sum "tournament," in which their rank and tournament earnings depend only on their bargaining performance relative to other players in the same role as they are. Bolton notes that players should never reject equilibrium offers in the bargainining games if they care only about their tournament earnings and their share of tournament money (and don't care about the nominal amounts "earned" in the bargaining that determines their tournament rank).

Why? Because rejecting an equilibrium offer only reduces the amount they can earn from the tournament and it *also* reduces their share of the

[20] The rise in rejection rate across rounds could be due to heterogeneity and sample selection bias—some combative or demanding player 2s both reject opening offers and propose stingy counteroffers, so that the samples of subjects in round 1 and in later rounds are systematically different. Or the difference could be due to negative reciprocity (player 1s whose offers were rejected fight back by rejecting player 2's counteroffer).

[21] Limited computation could include players guessing incorrectly, at the time that they reject an offer, about how large a counteroffer they could make in the next period without risking rejection. For example, take $T = 2$ games. A player with $\delta_2 = 0.6$ might reject a low offer of forty chips, worth \$12, because she thinks she may get up to \$18 in the subgame (which is an ultimatum game). Upon reaching the subgame, she begins to worry whether player 1, angered by the first-period rejection, would accept a very lopsided division of the \$18 so she may end up offering an equal split, disadvantageously counteroffering \$9 for herself after rejecting \$12. In this story, the limit in computation is her inability to imagine player 1's likely reaction to counteroffers at the time at which she rejects the initial offer. Such an inability to project out of one emotional state to another has been well documented in a variety of contexts by Loewenstein and Schkade (1999), who call it a "hot–cold empathy gap."

tournament money (which is fixed). Bolton then compared regular and tournament incentives in experiments. The results are mixed. Tournament offers among inexperienced subjects are about the same as in the regular bargaining control groups, except that the variance is higher and there are some differences in rejection rates. When bargainers return for a second session, tournament incentives nudge offers about a quarter of the way closer to the perfect equilibrium, but subjects in a third tournament session move back toward the equal split, *away* from perfect equilibrium. Carter and McAloon (1996) also report that tournament incentives in ultimatum games do not move offers and acceptances toward self-interest, as Bolton's idea predicts.

4.2.2 Limited Computation

What about learning? Perhaps players do not respect the equilibrium impact of structural features on bargaining power right away, but they learn to over time. Binmore et al. found that a single period of role reversal produced rapid convergence to equilibrium. Ochs and Roth, and Bolton, however, observed little convergence in ten periods. Since most of the subjects in the latter studies suffered only one or two rejections, they might not have experienced subgame play often enough to learn from it.

Precisely this concern motivated a clever paper by Harrison and McCabe (1992). They begin by pointing out that in pricing finitely lived assets (Forsythe, Palfrey, and Plott, 1982) and learning to vote strategically rather than sincerely (Eckel and Holt, 1989) players learn a backward induction solution only by playing through subgames. Applying the same idea, their subjects played seven three-round games with pie sizes of 100, 50, and 25 points, alternated between seven two-round games with pies of 50 and 25 points. The pie sizes in the two-round game are precisely those in the subgame that begins in the second round of the three-round game. In theory, subjects could learn from the two-round games what to expect if their first-round offer was rejected in the three-round game and play then proceeded to the second-round subgame. Across the seven three-round games, opening offers averaged 47, 40, 41, 35, 34, 30, and 29, converging close to the equilibrium prediction of 25. (Offers in the two-round games averaged 24–25 in every sequence.)

In the Harrison-McCabe design, the perfect equilibrium in the two-round subgame, 25, is the same as an equal split. This coincidence helps ensure that the perfect equilibrium is reached in the two-round game, leading to equilibrium in the three-round game. To test the robustness of this finding, Carpenter (2000) alternated two-round games with discount factors of either 0.75 or 0.25 with a one-period ultimatum game that was exactly the same as the second round of the associated two-round game. (That is, a two-round game with pies of 100 and 25 was compared with an ultimatum dividing 25, in the $\delta = 0.25$ condition.) The results did not replicate Harrison and McCabe's strong conclusion. Offers averaged 40 percent of the pie in all games. There was a very slight, insignificant, drift toward equilibrium in the two-round game.

Since learning effects are mixed to weak, the main question about alternating-offer bargaining is what combination of stable social preference and failures of backward induction explains what happens. To study limitations on strategic calculation, Camerer et al. (1994) and Johnson et al. (2002) ran experiments using two novel design features. One design feature was having subjects play against computerized opponents that were programmed to maximize their own earnings (and think their opponents are trying to maximize earnings too). If human subjects are able to calculate perfect equilibria, they should make equilibrium offers to the robots. If they do not make equilibrium offers, it must be that they don't know how to calculate them.

The second design feature is that the "pie sizes" in each of three rounds of bargaining were hidden in boxes on a computer screen. The boxes could be "opened," revealing the pie size, only by moving a cursor into the box. When the cursor moved out, the box closed. Figure 4.1 shows the computer display subjects saw, with the first-round box open, showing a pie size of \$5.00. This "mouselab" system has been extensively used to study individual decision making. It provides a "subject-eye view" of what information players are using to test hypotheses about reasoning. The mind is treated like a "thinking factory." Measuring the flow of inputs (information) into the factory and the length of time between input arrivals enables one to draw inferences about the unobserved production process going on inside the factory.[22]

Subjects bargained over three periods in which the pie sizes were around \$5.00, \$2.50, and \$1.25. To prevent memorization, the pie sizes were perturbed in each period such that the subgame perfect prediction ("equilibrium" hereafter) was fixed at \$1.25. The bargaining results replicate earlier

[22] Taking subgame perfection to follow from a specific computational process is obviously a departure from the "as if" conception in positive economics. But, since the equilibrium prediction is rejected, we know that either the computational process subjects use is not backward induction, or subjects are in an equilibrium modified by social preferences. Studying information processing is the most efficient way to determine which hypothesis is true. The direct approach has at least two admirers. Rubinstein (1999, p. 163) called it "one of the most beautiful experiments I have ever come across." And Costa-Gomes, Crawford, and Broseta (2001) later used Mouselab to study iterated reasoning (see Chapter 5).

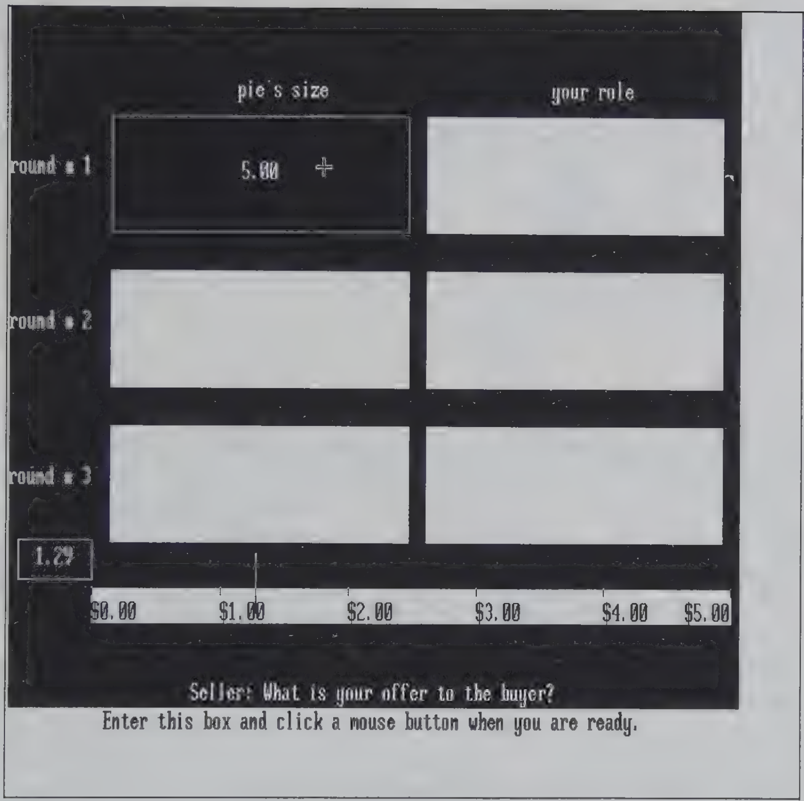


Figure 4.1. *The information display in alternating-offer mouselab experiments. Source: Johnson et al. (2002), p. 22, Figure 1; reproduced from* Journal of Economic Theory *with permission of Academic Press.*

findings. Most offers were between \$2.50 and \$2.00, averaging \$2.11. Offers less than \$1.80 were rejected about half the time. The overall rejection rate was 12 percent.

What did subjects pay attention to? Let's start with player 1s who are deciding how much to offer in the first round. Table 4.6 shows the average number of lookups (the number of times each box was opened in a period), total gaze time (the number of seconds that each round's box was open), and the number of transitions from the box representing one round to another round. (The number 2.55, for example, means that on average subjects moved the cursor from the round 1 box to the round 2 box between two and three times per trial.)

Table 4.6. Average lookups, gaze times, and transitions by player 1s

Round	Pie size ($)	Number of lookups	Fraction of periods with zero lookups	Gaze time	Number of transitions from row round to column round 1	2	3
1	5.00	4.38	0.00	12.91	—	2.55	0.65
2	2.50	3.80	0.19	6.67	2.10	—	1.24
3	1.25	2.12	0.10	1.24	0.50	0.88	—

Source: Johnson et al. (2002).

Table 4.6 shows that players looked longest at the first-round box and about half as long at the second-round box, and barely glanced at the third-round box.[23] They also made slightly more forward box-to-box transitions (upper-right entries) than backward (lower-left). A key fact is that players did not open the second- and third-round boxes at all on 19 percent and 10 percent of the trials, respectively.[24] The fact that they don't always open the future boxes is a death blow to the strong form of the backward induction hypothesis.

Grouping trials by the offers player 1s made and comparing their information-processing patterns provide a test of whether offers and information processing are correlated. Figure 4.2 shows an "icon graph" cooked up by my collaborator Eric Johnson to represent attentional statistics. The figure represents the statistics graphically, as features of icons. The three rectangular boxes represent data from each of the three-round boxes subjects saw on their screens (the top one represents round 1, and so forth). The fraction of each box that is shaded is proportional to the relative gaze time for that box. The width of each box is proportional to the number of lookups. And the thickness of the arrows pointing down and up is proportional to the average number of transitions from box to box (if there was less than one transition on average, no arrow is shown).

The icon graph shows at a glance that the round 1 box is opened more often (it's wider) and gazed at longer (it's more shaded). Most of the transitions from box to box are back and forth between the first- and second-round pie sizes. Comparing across offer categories, player 1s who made the

[23] Rubinstein (1999) found a similar result with a simpler method: Subjects were asked the order in which they wanted to see the first- and second-round pies in a two-round game. Nearly two-thirds chose to see the first-round pie size first.

[24] The results were not due to a top–down display bias, because a control experiment in which the boxes were displayed "upside down" (with the third-round on top) showed the same pattern of search across rounds.

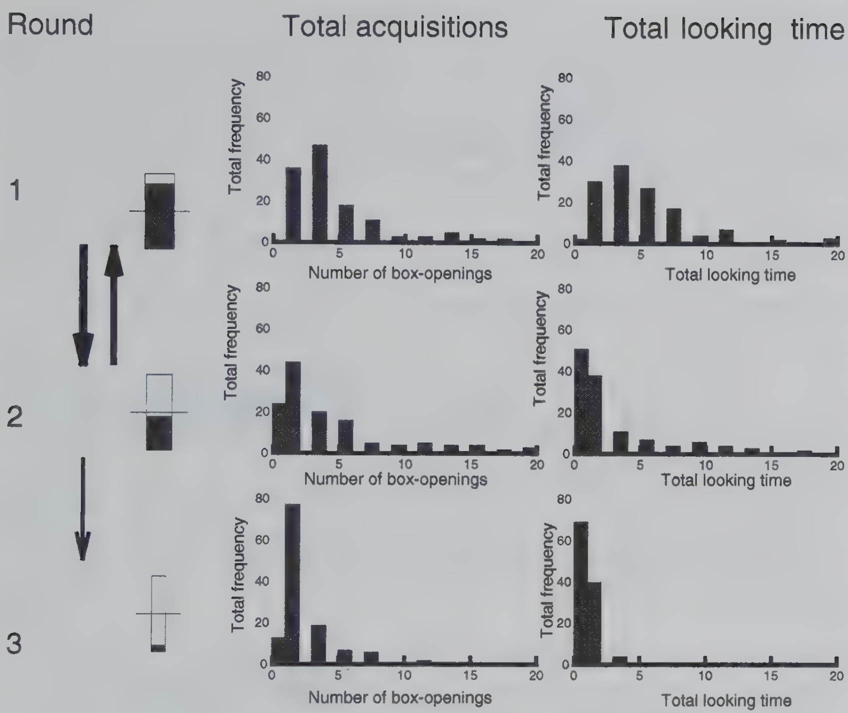


Figure 4.2. Icon graphs for player 1's attention statistics in alternating-offer experiments. Source: Johnson et al. (2002), p. 29, Figure 3; reproduced from Journal of Economic Theory *with permission of Academic Press.*

lowest offers (below $2.00) looked at the second-round pie size more often and longer than those who made higher offers. The relation between offers and looking patterns means you could use the subject's looking patterns to predict what offer they would make, before they even made it. After player 1 made an offer, player 2s could then look at the boxes and decide whether to accept the offer or not. Players who accepted the low offers spent more time looking at the round 2 pie size compared with players who rejected similar offers.

Violations of the subgame perfect prediction could come from subjects doubting the rationality or self-interest of others. We tested this explanation by having human subjects play computerized opponents who are known to maximize their own earnings (and were known to expect their opponent—the human subject—to do the same). Subjects who are self-interested and able to make equilibrium calculations should offer $1.25 to the computerized opponents. The average offer to the computerized opponents in these

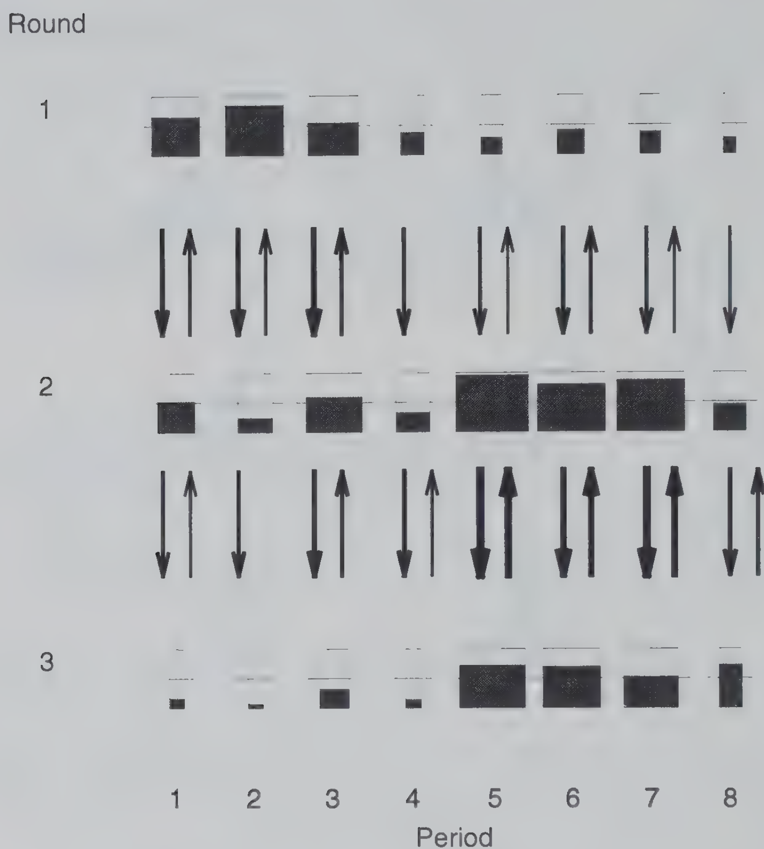


Figure 4.3. *Icon graphs for player 1's attention statistics before (periods 1–4) and after (periods 5–8) instruction. Source: Johnson et al. (2002), p. 34, Figure 6; reproduced with permission of Academic Press.*

periods was $1.84, compared with $2.11 to human subjects but well above $1.25, so erasing doubts that opponents are rational does *not* induce equilibrium play.

Figure 4.3 shows icon graphs across eight periods with robots—four before instruction, and four after brief instruction about backward induction. After brief instruction in backward induction, subjects' lookup patterns and gaze times were quite different: They looked mostly at the second- and third-round boxes, and made more transitions between those boxes. The average offer in those periods was $1.22, just pennies away from the equilibrium prediction.

A final experimental session combined untrained subjects with trained subjects who had learned to make equilibrium offers to the computerized opponents. Who "trains" whom? The result was a "tug-of-war" between the two types of subjects. Untrained subjects often rejected the trained subjects' lower offers, but they also learned to offer less. The trained subjects gradually raised offers after rejections. The result was an average offer of $1.60. Injecting some trained subjects into an untrained population therefore does have an effect, but it does not guarantee population convergence to perfect equilibrium.

Where does this leave us? One modeling approach is to assume that players' mental representations of games are truncated games in some way (compared with the true game), then they operate on the truncated game using simple decision rules or equilibrium concepts (see Camerer, 1998, and Johnson et al., 2002). (Including some decision cost or exogeneous cognitive constraint could lead to "optimally suboptimal" versions of this approach; see Samuelson, 2001.) Another approach is to posit decision rules that predict both observed decisions (e.g., offers and acceptances) and particular patterns of cognitive processing; then use both decisions and attentional statistics to infer decision rules (see Costa-Gomes, Crawford, and Broseta, 2001; Chapter 5 of this volume).

Of course, evidence that heuristic procedures are used in place of equilibrium reasoning does not imply that social preferences don't exist as well. It simply means modeling social preference is *insufficient* to explain everything we see. Note well that the attentional statistics and results from computerized opponents also reject the new generation of theories which assume social preferences along with equilibrium reasoning (e.g., Rabin, 1993; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). Note also that subjects are able to learn backward induction quickly, and they transfer their skills to games with different pie sizes. This means backward induction is not computationally difficult; it is simply unnatural to learn without guidance (like learning to ride a bike, compute a present value, or windsurf).

Summary: Opening offers in alternating-offer bargaining lie somewhere between equal splits and equilibrium predictions and are fairly insensitive to structural parameters. Some rejections occur, which are usually followed by disadvantageous counteroffers. Learning was not evident except in one study (Binmore et al.) and in a study designed to expose subjects to subgame play even if opening offers were accepted (Harrison and McCabe), although the latter study's finding did not replicate with a different game (Carpenter). Three experimental findings show that limited computation (or limited understanding of how bargaining power emerges from structure) is part of the explanation for deviations from perfect equilibrium. First, in many trials players do not even look ahead to how much will be divided one or two periods ahead if an offer is rejected; second, when human players bargain with computerized opponents (so that social preferences for fairness are turned off) they still do not make equilibrium offers; and third, after training in backward induction, they *do* make equilibrium offers to computerized opponents (so they *are* cognitively capable of backward induction, they just don't do it instinctively).

4.2.3 Random Termination

One interpretation of discounting in bargaining is that players realize an exogeneous force may suddenly terminate bargaining, with a known probability. Examples include regulatory intervention in a merger, the sudden appearance of a better bargaining partner, management turnover, physical disruption such as travel restrictions or communication snafus, or random emotional disruptions, which players can anticipate statistically but are helpless to resist once they occur.

To test the equivalence between time discounting of future payoffs and termination, Zwick, Rapoport, and Howard (1992) had players alternate offers of how to divide \$30 with a random termination probability of $1 - p$ if an offer is rejected. If players are risk neutral, then bargaining in this way is equivalent to bargaining over a pie that is discounted by a discount factor p; instead of a portion of the pie shrinking, the probability of termination induces a probability of the entire pie shrinking. In practice, bargaining could be different if players have certain kinds of social preferences.[25] And since each subgame is identical (the pie does not shrink), random termination also allows a kind of repeated experience with subgames, which may speed up learning.

Zwick et al. used three continuation probabilities: 0.90, 0.67, and 0.17. The subgame perfect predictions are 14.21, 12.00, and 4.29 in the three conditions. As in earlier studies, offers were below the equal split point (\$15) in the direction of perfect equilibrium and there was little learning. The rate of rejections was high (36 percent in the last six trials). The averages of accepted *final* offer across the three conditions were 14.97, 14.76, and 13.92. The corresponding averages in studies with pie shrinkage are 14.90, 14.64, and 13.57,[26] so termination and discounting results are very close.

[25] The Ochs–Roth "minimum acceptable offer" idea, for example, will predict a lot more later-period rejections in the shrinkage design, and hence more overall rejection, than in the random termination design. The Bolton (1991) theory makes the same prediction. The Fehr–Schmidt (1999) theory predicts equivalence.

[26] The first number comes from Binmore, Shaked, and Sutton's (1989)'s control group, and the latter two from Weg, Rapoport, and Felsenthal (1990). The amounts reported are rescaled for pie size to match the \$30 scale.

4.2.4 Games with Fixed Delay Costs and Outside Options

In many bargaining situations, the costs of delay are fixed. In litigation, each party runs up legal bills that are essentially independent of any final settlement. In strikes, the costs of delay are lost wages and profits. Games with fixed delay costs have very lopsided equilibria: The side with the lower delay cost should get almost everything. Suppose player i has delay cost c_i and player 1 moves first. Then player 1 should get the *whole* pie if her delay cost is lower ($c_1 < c_2$) and should get only c_2 if her delay cost is higher ($c_1 > c_2$). This is an equilibrium because prolonged bargaining just gives a larger and larger incentive for the higher-cost bargainer to settle, so the high-cost bargainer can never do better than accepting a pittance right away. When the bargainers have equal costs, there is a range of equilibria in which the first-mover gets something between the (common) delay cost and the entire pie.

Rapoport, Weg, and Felsenthal (1990) examined bargaining with fixed costs. Their players divided 30 Israeli shekels, and the fixed costs for players 1 and 2 were 0.10 and 2.50 or 0.20 and 3.0. They created a pseudo-infinite horizon by telling subjects the experiment would be terminated if it lasted too long.

Table 4.7 shows the average final offer (gross of delay costs), and the proportion of trials ending in the first trial. The results go strongly in the predicted direction, and get closer over trials. In the strong condition, for example, strong players are offering only 4.4–7.9 out of 30 shekels in the final block, and the weak player 2s accept these low opening offers 60–80 percent of the time. Weak first-movers are reluctant to accept the cold fact that higher delay costs undermine their bargaining power, so only 30 percent of their first offers are accepted. The horrible truth sinks in quickly, however. In the second round of weak-condition games, 35 percent settle, and another 22 percent settle in the third or fourth rounds.

In Binmore, Shaked, and Sutton (1989), players bargain over a £7 pie and player 2 has an outside option that is commonly known (£0, £2, or £4). Cooperative theories, such as the Nash bargaining solution, prescribe and predict that bargaining outcomes will divide the surplus that can be gained *beyond the players' threat points*. These concepts do not ask whether the threat to quit bargaining is credible or not. Suppose player 2's threat point is £2 (and the other's threat point is £0). Since cooperative theories do not question the credibility of the implicit threat to quit, player 2 should get £2 plus some fraction of the £5 surplus to be gained. But if player 2's likely total is greater than £2, why would she ever exercise the implicit threat? And if she won't exercise it, why should the threat point matter?

Noncooperative theories incorporate this skepticism by evaluating whether threats would be carried out in equilibrium, and ignoring those that will not. For example, Rubinstein–Stahl alternating-offer bargaining

Table 4.7. Fixed-cost bargaining results

	Trials			Equilibrium prediction
	1–6	7–12	13–18	
Experiment 1				
Strong ($c_1 = 0.10 < c_2 = 2.5$)				
Mean final offer	9.2	7.4	4.4	0.00
Percent accepted on first trial	50	67	83	1.00
Weak ($c_1 = 2.5 > c_2 = 0.10$)				
Mean final offer	20.0	23.2	25.4	29.9
Percent accepted on first trial	39	28	33	1.00
Equal ($c_1 = c_2 = 2.5$)				
Mean final offer	14.8	16.1	15.6	[0,27.5]
Percent accepted on first trial	78	83	83	1.00
Experiment 2				
Strong ($c_1 = 0.20 < c_2 = 3.0$)				
Mean final offer	12.8	8.6	7.9	0.00
Percent accepted on first trial	44	39	61	1.00
Weak ($c_1 = 3.0 > c_2 = 0.20$)				
Mean final offer	17.9	18.5	21.6	29.8
Percent accepted on first trial	28	22	28	1.00
Equal ($c_1 = c_2 = 3.0$)				
Mean final offer	14.8	14.6	14.7	[0,27.0]
Percent accepted on first trial	94	94	94	1.00

Source: Rapoport, Weg, and Felsenthal (1990).

predicts a solution in which player 2 gets a fraction $\delta/(1+\delta)$ of the surplus (where δ is the common discount factor). If δ is close to 1, this solution predicts divisions that are close to equal splits. Since player 2 is predicted to get around half of the £7 pie, or £3.50, player 2's outside option should not matter if it is £2. However, if player 2's option is more than what she can expect to get in equilibrium, then player 2 should just get her outside option.

The noncooperative equilibrium that ignores outside options less than $\delta/(1-\delta)$ is called "deal-me-out." Solutions that divide the surplus net of the outside options are called "split-the-difference" solutions. In the experiments by Binmore et al., the discount factor is $\delta = 0.9$. Deal-me-out predicts that player 2 will get 47 percent of the pie when the outside option is £0 or £2, and 57 percent (=£4/£7) when the outside option is £4. Split-the-difference predicts 47 percent, 64 percent, and 76 percent for the three option values. Subjects bargain by exchanging alternating offers.

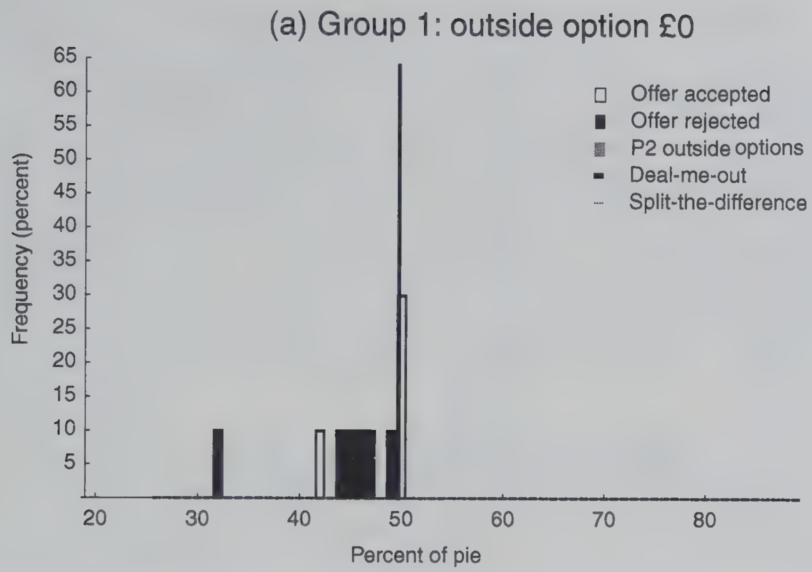
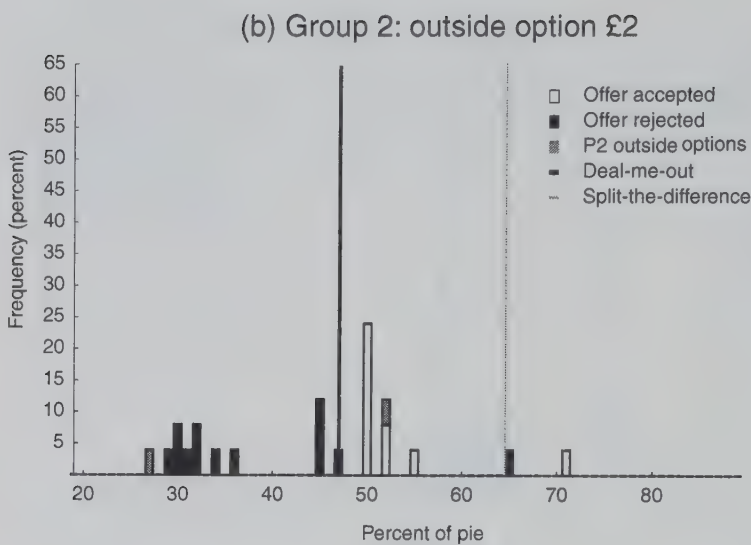


Figure 4.4. Distribution of outcomes in outside option games. Source: Based on Binmore, Shaked, and Sutton (1989).

Figure 4.4 shows the final outcomes, in terms of the percentage amount offered by player 2 as a fraction of the original £7 cake, in three option value conditions (groups 1–3 correspond to option values of £0, £2, and £4). The deal-me-out and split-the-difference predictions are indicated by thick and dotted lines, respectively. Dark bars indicate offers rejected in the first round and light bars indicate offers accepted in the first round. Gray bars indicate cases in which player 2 took the outside option. Deal-me-out predicts much better than split-the-difference: There is a large spike of agreements around 50 percent when the outside option is £0 or £2, and agreements cluster around 57 percent when the option is £4. Player 2 does not get much more when the outside option is £2 than when it is £0.

Figure 4.4 illustrates important subtlety in how these bargaining outcomes are reached. Notice that when player 2's outside option is £2 (group 2), there are several cases where initial offers are rejected (black bars) and the final agreements give player 2 about a third of the original pie. In those cases, player 2s are not taking their outside options, and usually get about half of the remaining pie, because bargaining lasted about three rounds. It takes player 2s a couple of rounds of costly pie shrinkage to learn that they are unlikely to get more than half. Thus, the value of the outside option does



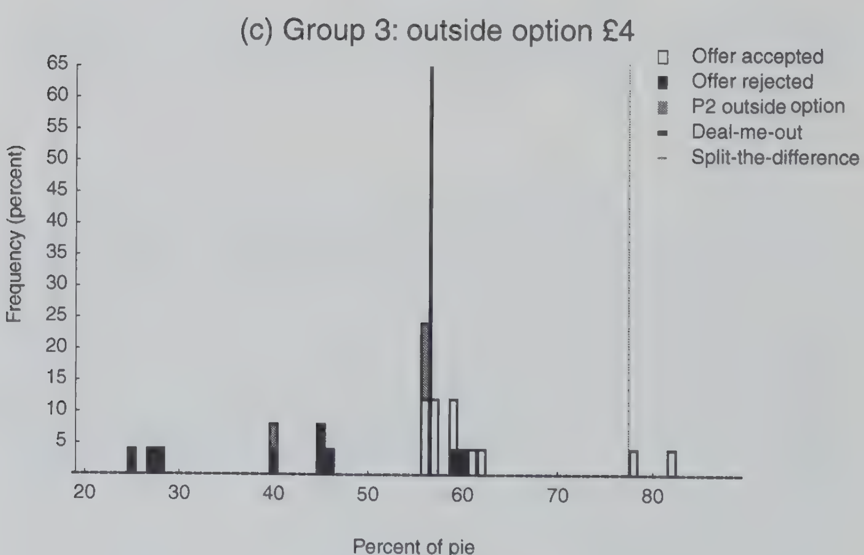


Figure 4.4 (continued)

affect *equilibration*—it produces some costly delay—but it does not affect the distribution of the discounted pie.

Binmore et al. (1998) were interested in cases where one player's option is very attractive, so the joint gains from partnership are small. Their players divide \$10. Player 2 has an option worth α (\$0.90, \$2.50, \$4.90, \$6.40, or \$8.10). Player 2 can take the option or give it up and play a Nash demand game. In the demand game, both players write down demands, and if they sum to less than \$10 each player gets her own demand (otherwise they get nothing). Since players in \$10 Nash demand games almost always demand close to \$5, the interesting cases are when the option is worth \$6.40 or \$8.10: Is the option player willing to bet that the other player will essentially concede, allowing the option-rejecting player to earn at least \$6.40 or \$8.10?[27]

The same subjects played repeatedly with different values of α. The results, reported in Table 4.8, support the deal-me-out theory. The third column shows that the option player 2s claim close to \$5 when $\alpha < \$5$ and claim exactly α when α is \$6.40 or \$8.10.[28] There is variation in what the player 1s demand. For example, when $\alpha = \$8.10$, the median player 1 demand is an accommodating \$1.65, but the (0.05,0.95) interval ranges from \$0.95 to \$4.50. Player 2s wisely opt out more than half the time when $\alpha = \$6.40$ and about 80 percent of the time when $\alpha = \$8.10$. Using the actual player 1 behavior, one can calculate the expected profits for player 2s who did not opt out. These expected payoffs are below the option value when α is \$6.40 or \$8.10.

Forsythe, Kennan, and Sopher (1991a) ran outside option experiments motivated by a "joint-cost" theory of strikes.[29] The idea is simple: Strikes in which joint costs are higher should be less likely. The top of Table 4.9 shows their design parameters. The total gain from exchange is the revenue minus the threat points; this per period difference, multiplied by the number of periods, is the "pie size." In every case, the stronger player's threat point is 7/12 or 58 percent of the available revenue R, so the strong player should always demand at least this much. The Nash split-the-difference solution and the deal-me-out solution, which gives the strong player the maximum of half and their 58 percent threat point, are also shown in Table 4.9. Bargaining took place by exchange of free-form messages over a computer network.

[27] Notice that this experiment tests forward induction (see also Chapter 7), since player 1s should know that rational player 2s who choose to play will expect to earn at least their outside option, and should accordingly demand less.

[28] Between 5 percent and 10 percent of player 2s actually opt in and demand *less* than α when it is \$6.40 or \$8.10. This is either a dumb mistake or an expression of the guilty belief that they do not deserve to earn that much from the bargaining, even when they can earn it for sure by opting out.

[29] See Kennan (1980) and experiments by Sopher (1990).

Table 4.8. *Results in outside option experiments*

Value of option α (\$)	Percent of player 2s opting out	Median claim of player 2s when opting in (\$)	Median claim of player 1s (\$)	Percent of player 1s demands leaving player 2s less than α
0.90	0.0	4.97	4.90	0.0
2.50	1.0	4.95	4.90	0.0
4.90	33.4	5.00	4.65	0.9
6.40	59.8	6.40	3.20	11.1
8.10	80.9	8.10	1.65	17.0

Source: Binmore et al. (1998).

Table 4.9. *Design and results in strike-cost experiments*

	Pie size $4		Pie size $8	
	Game I	Game II	Game III	Game IV
Per-period revenue	$2.40	$1.20	$2.40	$1.20
Number of periods (T)	4	8	8	16
Threat points (strong, weak)	($1.40,0)	($0.70,0)	($1.40,0)	($0.70,0)
Average strike length	0.74	1.17	1.47	1.44
Average total payoffs to (strong, weak)	(5.77,3.09)	(6.49,2.53)	(11.62,6.14)	(12.50,5.97)
Predictions				
Nash	(7.60,2.00)	(7.60,2.00)	(15.20,4.00)	(15.20,4.00)
Deal-me-out	(5.60,4.00)	(5.60,4.00)	(11.20,8.00)	(11.20,8.00)

Source: Forsythe, Kennan, and Sopher (1991a).

Results from the condition in which players negotiate period-by-period are shown in Table 4.9. A "strike period" is a period in which players failed to agree and earned only their threat points. The joint-cost avoidance theory predicts shorter strikes in games I and III because more net revenue is at stake; this prediction is wrong because average strike lengths were similar. Average payoffs support the deal-me-out prediction: Weak players were able to get about 70 percent of the surplus (beyond threat points); strong players got only 5–10 percent of the surplus; and the rest was lost owing to inefficient strikes.

The paper by Forsythe et al. reports the messages players sent, which document a verbal tug-of-war strongly rooted in everyday concepts of fairness and game-theoretic concepts of bargaining power. Strong bargainers persistently reminded their weak partners that, even if they failed to agree, the strong would earn plenty of money. The weak bargainers begged the strong to charitably offer equal divisions (implicitly favoring the weak; this plea usually fell on deaf ears), or at least split the surplus equally. Subjects promised to logroll across periods, grabbing a large share in one period but making it up generously in the next. One subject described how "cute" he is, and his partner groped vaguely for decisive social connections ("Are you in a frat? If so maybe I'll deal and maybe I won't. Depends on whatever."). Another negotiation erupted into a hateful war of words lasting for several periods, ending with all gains dissipated ("Go back to Burge [dormitory] and roll in the barf," followed by "I'm a junior and live in Mayflower and I'd love for you to stop by sometime and visit"). These transcripts are a reminder of the ambitious possibility of mapping a large space of socially nuanced announcements into sharp predictions of who gets what.

Finally, Binmore et al. (1991) compared exercising outside options with exogenous (forced) termination. If termination is exogenous, then even low option values (below the noncooperative equilibrium share) matter because the options may be credibly exercised "by accident." They found that options do influence bargaining differently when exercising them is voluntary or exogenous, as predicted by equilibrium theory. Furthermore, subjects were asked afterwards what divisions seemed most fair, and their answers were influenced by their experience, which is the first direct evidence that fairness conceptions may be malleable.

Summary: Random termination and discounting of future payoffs yield very similar bargaining outcomes, except that there are more rejections when termination is random. There is a big difference between the lopsided divisions in the fixed-cost games and the persistent tendency toward equal splits in fixed-discounting games seen previously in this chapter. The difference is an important puzzle. Theories of social preference that can explain the equality-biased results in fixed-discounting games should also be able to explain these highly *unequal* results in fixed-cost games. Experiments in which players have outside options vindicate the noncooperative view, in which options matter only if they yield more than the perfect equilibrium offer, compared with the Nash bargaining solution or "threat point" approach in which players with better options should always get more. However, it takes time for the high-option players to learn that they will get little of the surplus.

4.3 Bargaining under Incomplete Information

In theory, asymmetries in information fundamentally change the nature of how people bargain. Introducing asymmetries undermines efficiency because bargaining strategies then serve two different purposes: Players bargain both to get the most they can and to convey information. The two purposes usually conflict (and, in some models, inevitably do; e.g., Rubinstein, 1985). A typical outcome is that players who would like to accept a pending offer must turn it down to convey something about how patient they are to the other players, or to signal how good their outside options are.

So far, few experiments have been done to control information asymmetry and test theory. There are two sorts: a little work on "strikes" by Forsythe et al.; and a larger body of work on the sealed-bid mechanism under incomplete information, primarily by Radner and Schotter and by Rapoport et al.

4.3.1 One-Sided Buyer Information with Seller-Only Offers

In Rapoport, Erev, and Zwick (1995), the seller has a good that is worthless to her, and the buyer has private information about his reservation price (which is uniform from (0,1)). Only the seller can make offers. This kind of situation is common in retail selling.[30] Assuming a common discount factor of δ, there is a unique sequential equilibrium in which the seller offers $p_0 = \gamma(1-\delta)/(1-\gamma\cdot\delta)$ in the first period (where $\gamma = [1-(1-\delta)^{0.5}]/\delta$) and $p_t = \gamma^t \cdot p_0$ in subsequent periods t. A buyer with value v accepts if and only if the price is below $v(1-\delta)/(1-\gamma\cdot\delta)$.

Although the math is messy, the economic intuition is not. Low-valuation buyers simply cannot afford to accept high prices. This enables the seller to price discriminate—offering high prices at first, then gradually lowering them. But why don't high-value buyers wait for the price to drop? The reason is that equilibrium prices decline exponentially, so the amount of price reduction slows down rapidly. The surplus that high-value buyers earn does not grow much if they wait, so the *discounted* surplus falls if they wait too long. High-value buyers are forced to buy early, and low-value buyers must wait till the price drops to where they can afford it. The results are obviously sensitive to the discount factor δ. If δ is close to 1, the high-value buyers can wait and wait, and so the seller is forced to offer a low price right away. If $\delta = 0$ (all the buyers have to catch a plane immediately), the seller maximizes expected profit by setting a price equal to half of the highest value.

To test the effect of δ, Rapoport et al. used three values: H (0.90), M (0.67), and L (0.33). The time series of average prices in each bargaining period is shown in Figure 4.5 for the three discount factor conditions. Look first at first-period prices. The predicted opening prices are 24, 36, and 45 for H, M, and L. Initial offers are higher than predicted, and vary with the discount factor in the opposite of the predicted direction. However, offers *do* decline across periods at rates amazingly close to the rates predicted by the sequential equilibrium. The predicted exponential factors (γ) are 0.76, 0.68, and 0.55 across H, M, and L conditions. The estimated exponential declines are 0.81, 0.68, and 0.55.

Buyers' acceptance decisions are close to those predicted by theory in one sense. Buyers should accept offers that maximize their discounted surplus, $\delta^t(v-p_t)$. This means that whenever an offer is accepted, the discounted surplus that was passed up in previous rounds should not be larger than the discounted surplus they end up with. This "no regret" condition is

[30] Ausubel and Deneckere (1992) proved that, if the time between periods is short (or, equivalently, the discount factor is close to 1), then it is never optimal for buyers to make offers, which justifies restricting attention to the case where only the seller makes offers.

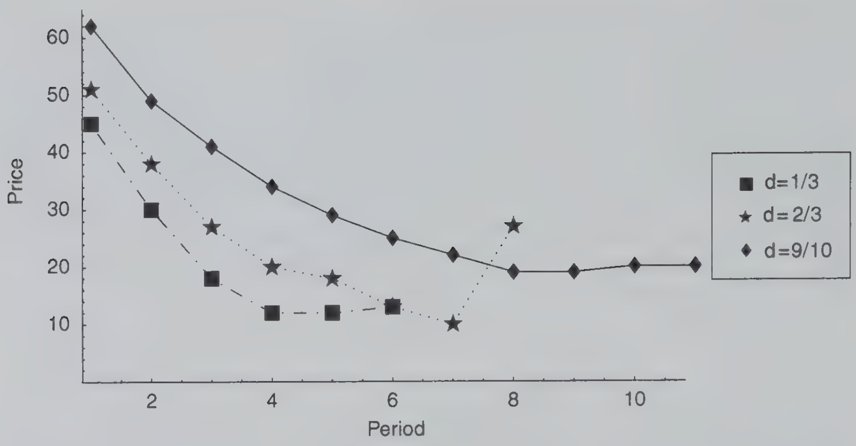


Figure 4.5. Average prices in seller-offer bargaining with unknown buyer valuations. Source: Based on Rapoport, Erev, and Zwick (1995).

rarely violated in those cases where a deal was eventually made.[31] Although buyers rarely regret waiting, they often accept offers too soon. Buyers typically accepted the first offer that was below their value v, or the second such offer, but could have done better by waiting. Because the buyers jump too quickly, sellers ask prices that are too high and end up with a larger share of profit than is predicted by the equilibrium. The fact that buyers accept offers too quickly is probably sensitive to information conditions.[32] If buyers were allowed to observe the price sequences offered by all sellers, they could see the "sale prices" in later periods and might learn to be more patient.

4.3.2 One-Sided Private Information and Strikes

Forsythe, Kennan, and Sopher (1991b) analyzed games in which the amount to be divided is either large or small (π_g or π_b, for "good" or "bad" states, whose probabilities are commonly known), and only one of two bargainers (the informed one, I) knows that amount.

Can predictions be made if the details of how bargaining occurs are not known? Amazingly enough, the answer is Yes. In one of the most beautiful

[31] Ten of the thirteen violations occurred when buyers had valuations above 50. Rapoport et al. speculate that these high-value buyers are waiting too long to punish sellers (at a cost to themselves) for asking too much and cutting their prices too slowly, which may reflect some expression of preference for fairness. As a result, these buyers end up taking prices too late, which give them less discounted surplus.

[32] This regularity is related to patterns of price convergence in posted-offer auctions for perishable goods (see Holt, 1995), in which sellers' prices tend to start above the competitive equilibrium, and gradually come down as sellers undercut each other.

pieces of modern economic theory, Myerson (1979) showed that *any* Nash equilibrium of the bargaining game, regardless of its rules, is equivalent to a "direct revelation game" in which the informed bargainer announces the state truthfully, the pie is reduced to amounts $\gamma_g \pi_g$ and $\gamma_b \pi_b$, and the uninformed bargainer, U, gets the amounts x_g and x_b. (These reductions and payments are together called a "mechanism.")

The truth-telling constraint has enormous analytical power because it severely restricts the possible values of the reduction factors γ_g and γ_b, and the shares x_g and x_b. The uninformed player U's shares must make I better off, announcing that the state is good when it really is, given the amount that she will then get, and announcing that the state is bad when it really is. A little algebra shows that these two constraints imply[33]

$$(\gamma_g - \gamma_b)\pi_b \leq x_g - x_b \leq (\gamma_g - \gamma_b)\pi_g. \tag{4.3.1}$$

Since $\pi_g > \pi_b$, the only way both inequalities can hold is if $\gamma_g - \gamma_b$ is positive—i.e., there is more shrinkage when the state is claimed to be bad. (This shrinkage keeps I from claiming the pie is large when it really is small.)

We can say more. Suppose the mechanism is "interim incentive efficient" (hereafter, just "efficient") in the sense that the payoff profile to the three pseudo-players—U, I in the good state, and I in the bad state—is Pareto optimal. Then it pays to make $\gamma_g = 1$ and make x_g and x_b as large as possible, which implies $x_g - x_b = (1 - \gamma_b)\pi_g$. A little more logic implies that strikes can be efficient only when a "strike condition" $p\pi_g > \pi_b$ holds (where p is the probability of the bad state), but there are efficient mechanisms that yield strikes when $p\pi_g < \pi_b$. Thus, the revelation principle and the presumption of efficiency yield a very strong prediction: There should be no strikes when $p\pi_g < \pi_b$, and strikes are expected when $p\pi_g > \pi_b$. (A stike occurs when time ends and players did not agree how to divide the pie.)

This theory predicts when strikes will occur but says nothing about players' shares. Further assumptions make a precise prediction about the shrinkage factors and the shares they imply.[34] Social scientists who are interested in bargaining should be flabbergasted that such sharp prediction can be derived from thin air (and that theorists would take it seriously!). If the

[33] If the state is good, the payoff from a truthful announcement to I is $\gamma_g \pi_g - x_g$ and the payoff to lying is $\gamma_b \pi_g - x_b$. If the state is good, the payoffs to truth-telling and lying are $\gamma_b \pi_b - x_b$ and the payoff to lying is $\gamma_g \pi_b - x_g$. Requiring truth to be more profitable than lying and combining the two inequalities gives the inequality in the text.

[34] Suppose players would agree to a fair (random) mixture of the mechanisms (i.e., the shrinkage factors and shares for U) each would impose if they had dictatorial power. This "random dictator" (RD) axiom yields very precise predictions: $\gamma_g = 1$, $x_g = \pi_g/2$, $\gamma_b = 1/2$, $x_b = 0$ when $p\pi_g > \pi_b$ and $\gamma_g = 1$, $x_g = \pi_b/2$, $\gamma_b = 1$, $x_b = \pi_b/2$ when $p\pi_g < \pi_b$. Thus, there should be exactly 50 percent strikes when the strike condition is on and the state is bad. The RD axiom also yields precise predictions about how much U gets, depending on whether the strike condition holds or not.

Table 4.10. Results in incomplete information bargaining

Game	Probability of bad state	State	Pie size (π)	Payoffs U	Payoffs I	Value of information	Total payoff	Percentage inefficiency
I	0.5	Bad	1.00	0.31	0.30	−0.01	0.61	39.0
		Good	6.00	1.78	3.70	1.92	5.48	8.7
		Mean	3.50	1.05	2.00	0.95	3.05	13.0
		Prediction		1.50	1.75	0.25	3.25	7.1
II	0.75	Bad	2.30	1.06	0.84	−0.21	1.90	17.2
		Good	3.90	1.53	2.07	0.54	3.59	7.9
		Mean	3.50	1.41	1.76	0.35	3.18	9.3
		Prediction		1.46	1.75	0.29	3.21	8.3
III	0.5	Bad	2.80	1.47	1.18	−0.29	2.66	5.2
		Good	4.20	1.52	2.41	0.89	3.93	6.5
		Mean	3.50	1.50	1.80	0.30	3.29	6.0
		Prediction		1.40	2.10	0.70	3.50	0.0
IV	0.25	Bad	2.40	1.08	1.04	−0.04	2.12	11.8
		Good	6.80	1.58	5.03	3.45	6.61	2.9
		Mean	3.50	1.21	2.04	0.83	3.24	7.4
		Prediction		1.20	2.30	1.10	3.50	0.0

Source: Forsythe, Kennan, and Sopher (1991b).

theory is even close to right, that would be a triumph. If it is not, the nature of deviations may tell us something about which assumptions underlying the theory need replacement (and by what). The stage is set for an experiment that cannot fail. In their experiments, Forsythe et al. conducted ten-minute bargaining sessions in which players bargained through handwritten messages prescribing how much U would get. Messages were passed between rooms, to limit the strong effect of face-to-face communication.

Averages from pooling all sessions are shown in the action-packed Table 4.10. In games I and II, the strike condition is on; in games III and IV, the strike condition is off. The RD-based theory predicts 50 percent strikes in the bad state in games I and II, and no strikes in all other conditions (good states in I and II and all states in III and IV). Although the sharp predictions of the theory are not that close to the data in absolute terms, the predicted differences across conditions are confirmed. There are frequent strikes in the bad states in games I and II (17 percent–39 percent, when theory predicts 50 percent), and fewer strikes in bad states of games III and IV (around 10 percent, when theory predicts none).[35]

Thus, although game-theoretic predictions are sometimes inaccurate in their details, the presumption that very complicated bargaining can be approximated by direct mechanisms (which is the central principle in this body of theory) passes a difficult test. Furthermore, most of the deviations can be accounted for by small deviations from self-interest (bargainers are not as ruthless as the theory assumes).

4.3.3 Sealed-Bid Mechanisms for Bilateral Bargaining

A simple way to determine a price in bilateral buyer–seller bargaining is for both sides to write down a price and to trade at the average of their prices if they overlap (i.e., if the seller names a selling price larger than the buyer's bid). When there are many buyers and sellers bargaining simultaneously, this "sealed-bid mechanism" is known as a "call market" and has been studied a fair amount experimentally (e.g., Cason and Friedman, 1999; Hsia, 1999). Call markets are used to create an opening price at the Paris Bourse and in some other naturally occurring markets.

The two-person sealed-bid mechanism is ripe for empirical testing because there is much theory about it. Suppose the valuations of the buyer and seller, V and C, are drawn from a uniform distribution [0,100], which is commonly known. Chatterjee and Samuelson (1983) showed that there existed a piecewise-linear equilibrium in which buyers bid their values up to 25, then "shaved" their bids, bidding $(25/3) + (2/3)V$ for values $V \geq 25$. Similarly, sellers bid their cost if $C \geq 75$ and otherwise overbid $25 + (2/3)C$. While there are other equilibria,[36] Myerson and Satterthwaite (1983) proved that the piecewise-linear equilibrium maximizes the ex ante gains from trade that can be achieved by *any* Bayesian–Nash equilibrium (among mechanisms that are individually rational and that give all of the buyer's payment to the seller). As in the strike games described above, in theory there is an inevitable loss of surplus caused by the information asymmetry—there will be times when they should trade, but don't (or "can't").

Radner and Schotter's (1989) experimental study of the sealed-bid mechanism investigated whether players use bidding strategies such as the linear equilibrium. They ran eight sessions with various design changes. Sessions 1–2 and 8 use the uniform value distribution and had the linear

[35] Note that the rates of striking in the good states of the bargaining games, 2.9–8.7 percent, are within a factor of two of the rates of disagreement observed in complete information games, where strikes should also never occur.

[36] Leininger, Linhart, and Radner (1989) show that there are many other equilibrium bidding functions for the sealed-bid mechanism. There is a two-parameter family of nonlinear differentiable bidding functions, as well as discontinuous step-function equilibria.

equilibrium given above. Session 3 used a different pricing rule—subjects traded at a price of $(v + c + 50)/3$ if v exceeded c by 25 or more. Subjects should simply bid their values ($v = V$, $c = C$) under this mechanism, in equilibrium, and it should produce the same pattern of prices and efficiencies as in session 1. In session 4, the price was equal to the buyer's bid v if v and c overlapped (rather than setting the price at the midpoint); buyers should bid half their value. In sessions 5–6 they changed the value distributions to increase the number of expected trades (and, hence, learning), which reduced the bid function slope to 0.438. In session 7, subjects engaged in unstructured face-to-face bargaining.

Variation of bid function slopes across the experimental treatments can be seen from pooled regressions in Table 4.11. The results show that subjects *do* bid roughly linearly in their values (linear regressions of bids against values fit well). The table breaks observations into two samples—those below the critical values at which the slope is predicted to change, and those above the critical value—to test for piecewise linearity.[37] T-statistics testing whether the slope coefficient is equal to the predicted coefficient (in parentheses) are generally small, so bids are consistent with equilibrium bidding. In sessions 1–2 and 8, the slopes should be 0.67 (above the critical value for buyers, and below the critical value for sellers), and are close, from 0.58 to 1.06.[38] In sessions 4–6, the slopes should be lower (from 0.438 to 0.500) and they are. Subjects did change the degree to which they shaded their values across sessions, in the direction predicted by theory.[39] The prediction that bid functions are piecewise linear is borne out because slopes *are* generally different below and above critical values, except for buyers in session 2.

In the face-to-face session 7, efficiency is 110 percent. Some subjects truthfully revealed their values, but the variance of profits was also much higher in this condition, which means there was a large dispersion in how truthful different subjects were. The efficiency and variance of face-to-face bargaining was surprising to theorists.[40] Radner and Schotter concluded (1989, p. 210) that "The success of the face-to-face mechanism, if replicated,

[37] To test whether piecewise linearity holds more sharply when it is predicted to (in sessions 1–2 and 8) than when it is not predicted to (in sessions 3–4), fixing the value distribution, the table breaks observations into subsamples below and above 25 (for buyers) and 75 (for sellers) even for sessions 3–4, where no break is predicted. Then we can see whether there is more of a break in sessions 1–2 and 8 than in sessions 3–4, as there is predicted to be, holding the break point constant.

[38] Notice that the binary lottery procedure used in session 8 does not produce systematically different results than paying money in baseline session 1.

[39] An important exception is the direct mechanism session 3. In this session, subjects should simply bid their values, so the slopes should be 1. However, the estimated slope is 0.726 for buyers and 1.06 for sellers, so the mechanism works only for one side.

[40] Roth (1995b) reports a small sample of data from ultimatum games showing that players coordinate even more sharply than usual on 50–50 divisions, with essentially no rejections, when bargaining face to face.

Table 4.11. Estimated bid function slope coefficients

	Below critical value			Above critical value		
Session	Predicted β	Estimated $\hat{\beta}$	*t*-test $(\hat{\beta} - \beta)$	Predicted β	Estimated $\hat{\beta}$	*t*-test $(\hat{\beta} - \beta)$
Regressions of buyer bids against values						
1	1.0	1.00	(0.01)	0.670	0.85	(4.14)
2	1.0	0.91	(−0.52)	0.670	1.06	(1.28)
8	1.0	0.91	(−0.14)	0.670	0.80	(2.32)
3	1.0	0.92	(−0.08)	1.000	0.73	(−2.64)
4	0.5	0.55	(0.66)	0.500	0.58	(2.32)
5	1.0	0.80	(−4.17)	0.438	0.50	(1.12)
6 (1–20)	1.0	.085	(−1.40)	0.438	0.40	(−0.56)
6 (21–40)	1.0	1.11	(0.70)	0.438	0.32	(−1.55)
Regressions of seller bids against costs						
1	0.670	0.58	(−1.38)	1.0	0.97	(−0.32)
2	0.670	0.74	(1.28)	1.0	1.07	(0.14)
8	0.670	0.75	(1.65)	1.0	1.07	(0.17)
3	1.000	1.06	(1.04)	1.0	0.67	(−0.58)
5	0.438	0.48	(0.87)	1.0	1.00	(0.60)
6 (1–20)	0.438	0.57	(2.16)	1.0	0.97	(−0.79)
6 (21–40)	0.438	0.52	(1.20)	1.0	0.95	(−0.69)

Source: Radner and Schotter (1989).

might lead to a halt in the search for better ways to structure bargaining in situations of incomplete information. It would create, however, a need for a theory of such structured bargaining in order to enable us to understand why the mechanism is so successful."

Schotter, Snyder, and Zheng (2000) introduced agents. Players first drew valuations (using the asymmetric distributions from Radner and Schotter's sessions 5–7). Then buyers (sellers) told an agent the maximum (minimum) they were willing to bid, and the agents bargained face to face with other agents. Agents were paid either a percentage of the surplus they earned or a fixed fee for each trade.[41]

Principals typically gave their agents a maximum reservation price below the true valuation. Regressions of the instructed reservation prices against

[41] Subjects were NYU undergraduates. Ten pairs of subjects were run in each fee condition for fifteen rounds.

values yield slopes of 0.78 in the percentage fee condition and 0.70 in the fixed-fee condition, halfway between the predicted slope of 0.438 and the truthful revelation slope of 1. The latitude subjects allowed their agents is also correlated with the apparent skill of the agents.

Rapoport and Fuller (1995) replicated the Radner–Schotter results on the sealed-bid mechanism with two important extensions. Their first experiment used uniformly-distributed values and closely replicated Radner and Schotter's results, even when subjects gave bid functions for each of twenty-five possible values (see also Selten and Buchta, 1998).

Daniel, Seale, and Rapoport (1998) first replicated Rapoport and Fuller's second experiment, which used asymmetric value distributions.[42] Buyer (seller) values were uniformly distributed over the interval [0,200] ([0,100]). The equilibrium bid function is linear for the seller ($c = 50 + (2/3)C$). The buyer's bid function is piecewise linear: Bid V for $V < 50$, bid $(50 + 2V)/3$ for values between 50 and 150, and bid a flat 116.7 for all values above 150 (since, in equilibrium, the most the seller would ask is 116.7). Median estimates of the buyer slopes for the last two intervals are 0.56 and 0.28, close to the predictions of 0.67 and 0. Daniel et al. also conducted a second experiment with more extreme asymmetry, in which seller values were distributed uniformly on the interval [0,20] and buyer values were uniform from [0,200]. With these value distributions, sellers should ask $c = 50 + (2/3)C$, marking their costs way up to exploit the fact that the buyer's value is likely to be far above their cost. Buyers should bid their value ($v = V$) for $V \leq 50$, bid $v = (50 + 2V)/3$ for $50 \leq V \leq 70$, and bid 63.3 for any value V above 70.

This equilibrium makes for an interesting empirical test. Sellers should ask a price that is much higher than any possible cost, which they may be reluctant to do. And for most of their values (above 70), buyers should bid a flat 63.3.

Figure 4.6 shows a scatter plots of bids, pooling buyers in experiment 2. It is hard to see sharp piecewise linearity in the buyers' bid functions, but buyers are clearly bidding a smaller fraction of their values when the values are high than when values are low. Sellers do mark up their costs dramatically; their bids are widely dispersed and a little below the equilibrium prediction.

Table 4.12 shows means or medians of estimated intercepts and slopes of the bid functions across subjects, for experiments 1 and 2 separately. The same predictions are made across experiments, although the type of bidder and value interval to which the prediction applies vary. To look for piecewise linearity, bid function slopes are estimated using spline regression.[43]

[42] Their replication had fifty periods (rather than thirty), used a computerized network (rather than message passing), and showed each subject her complete history (which Rapoport and Fuller did not).

[43] The spline regression allows the slopes to vary but constrains the intercepts so the segments meet.

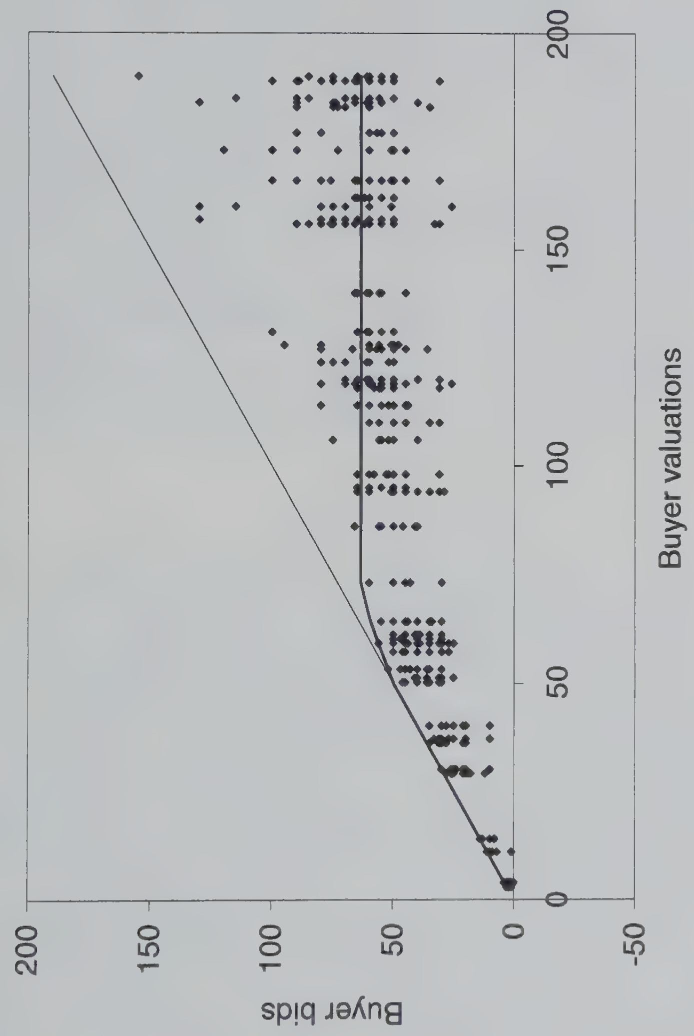


Figure 4.6. *Buyer valuations and bids in sealed-bid mechanism bargaining. Source: Based on data from Daniel, Seale, and Rapoport (1998).*

Table 4.12. Estimated bid function parameters

	Seller estimates			Buyer slope estimates: Buyer value ranges			
	Intercept	Slope	R^2	0–50	51–150	151–200	R^2
Prediction	50.0	0.67	—	1.00	0.67	0.00	—
Means (DSR exp. 1)	39.0	0.73	0.67	0.88	0.61	0.16	0.87
Medians (RDS exp. 1)	26.3	0.84	0.83	0.89	0.64	−0.08	0.88
				Buyer value ranges			
				0–50	51–70	71–200	
Medians (DSR exp. 2)	34.0	0.66	0.05	0.78	0.46	0.21	0.76
	Buyer estimates			Seller estimates			
				151–200	51–150	0–50	
Medians (RDS exp. 2)	15.0	0.71	0.80	0.95	0.62	0.05	0.91

Source: Daniel, Seale, and Rapaport (1998) and Rapaport, Daniel, and Seale (1998).

In Daniel et al. (DSR) experiment 2, the estimated slopes are fairly close to the equilibrium predictions, and R^2 values are high. There is also substantial learning: Bidders with high values often bid way too much in the first ten periods but learn to bid much lower after a while (which poses a challenge for learning models; e.g., Camerer, Hsia, and Ho, 2002).

Rapoport, Daniel, and Seale (1998) replicated DSR's experiment 1, and conducted a second experiment (with fixed pairings of subjects rather than random rematching). Seller costs C were uniformly drawn from [0,200] and the buyer values were uniformly drawn from [100,200]. The linear equilibrium then flips around the bid functions of the buyer and seller. As in Daniel et al., the median slopes are quite close to those predicted.

Following Radner and Schotter (1989), Valley et al. (2002) studied the effect of communication in the sealed-bid mechanism in more detail. In their studies, buyers and sellers drew values uniformly in the interval [0,$50] and bargained by stating bids. There were seven trading periods with no repeat rematching. Half the subjects participated in a no-feedback condition in which they received no feedback about the other subjects' bids. In a no-communication condition, players had two minutes to think about what to bid. In a written-communication condition, players exchanged written messages (through couriers) for thirteen minutes, then submitted final bids. In a face-to-face condition, players discussed anything they wanted, in person, then returned to separate rooms and submitted final bids.

Valley et al. (2002) report two studies. In their second, larger study Valley et al. find that communication enhances efficiency of trade. This can be seen in Figure 4.7, which plot pairs of buyer values v (on the y-axis) and seller costs c (on the x-axis), for the cases where mutually profitable trade is possible (i.e., $v > c$), across the three communication conditions. Pairs who made a trade are plotted as diamonds; failures to trade are plotted as open squares. The linear equilibrium predicts trade should not occur when v exceeds c by less than 12.5 (intuitively, when there is too little gain from trade, the possible gains do not overcome the incentive subjects have to shade their valuations). This "no-trade zone" is the area between the two thick lines. In theory, the no-trade zone should be filled with squares, and the larger zone to the upper left should be filled with diamonds. This prediction is accurate in the no-communication condition. But when there is written and face-to-face communication, however, there are a lot of trades (diamonds) in the no-trade zone.

The special contribution of Valley et al. is to figure out where the added efficiency comes from. They first note that regressions of bids against

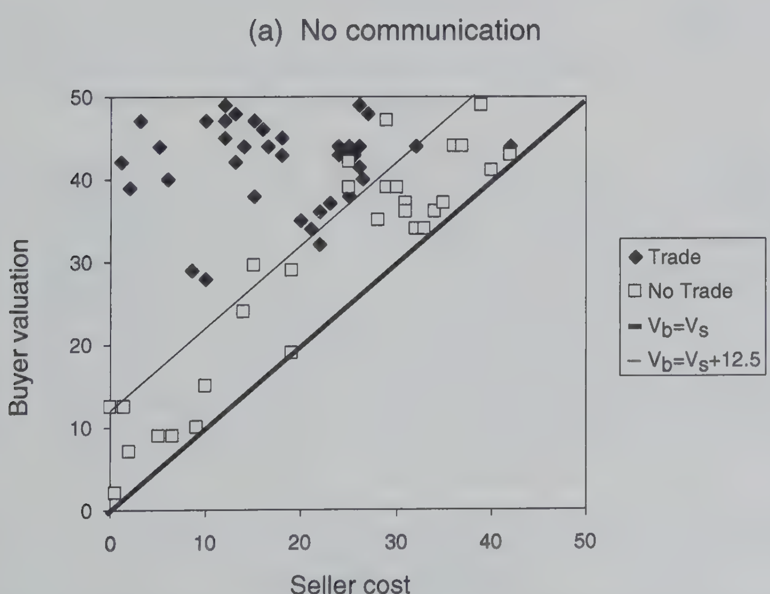
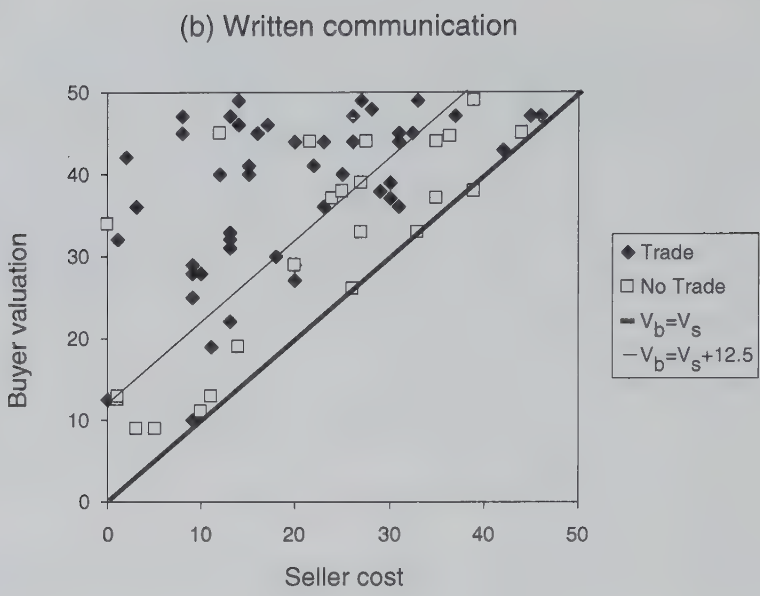


Figure 4.7. Buyer-seller value pairs and trade incidence. Source: Valley et al. (1998), p. 138, Figure 3; redrawn from original with permission of Academic Press.



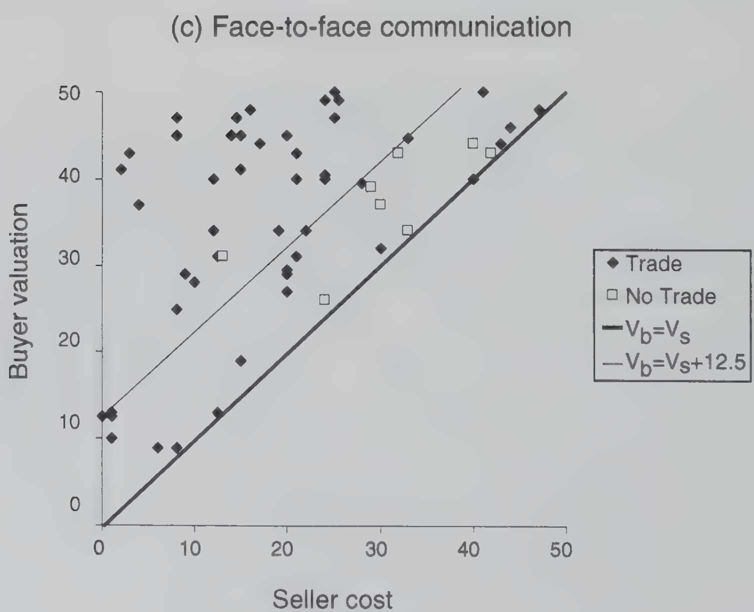


Figure 4.7 (continued)

valuations show empirical bid functions (slopes around 0.7) that are quite close to the linear equilibrium slope of 0.67. If the subjects were using these bid functions, there should be few trades in the no-trade zone in the communication conditions. But there *are* many trades in the no-trade zone. What gives?

The key clue comes from running a regression of the buyer's bid on the buyer's value *and* the seller's bid. The regression slope of buyer bids against seller bids is 0.60, which means the buyer *somehow* knows to bid higher when the seller is bidding higher. Further analysis shows three ways a pair of bargainers (a dyad) coordinate their behavior: mutual bidding of values; mutual revelation of values (they tell each other their true values); and coordinating on a price (i.e., agreeing on a single bid they both will make, so the buyer's bid will exactly match the seller's and they will trade at that bid price). Mutual value bidding and mutual revelation of values are rare,[44] but coordinating on a single price is common: It happens about 40 percent of the time with written communication and 70 percent of the time with face-to-face communication.

Thus, communication does enhance efficiency dramatically, but mostly *not* by encouraging mutual truth telling.[45] Subjects apparently feel each other out for approximate revelations of values—with a healthy dose of actual lying—then coordinate on a price that gives each side enough surplus to make her happy. The modal result is an equal split of surplus (i.e., the agreed-upon price is halfway between their values). Communication also doubles the variance of surplus shares because bidders who reveal values are often matched with others who don't.

Summary: Several experiments have been run in which some bargainers know something about their values that others don't know, but the uninformed side knows that the others are better informed. This is surely how most bargaining works. Experimental results are surprisingly supportive of theories that are bold and often counterintuitive, although sometimes wrong on key details. In one-sided bargaining in which sellers make a sequence of offers (and buyers know their own values), initial offers are higher than predicted, but they decline at close to the predicted rate. In bargaining games with large and small surplus, predictions about when "strikes"

[44] Mutual revelation and value bidding are more common in their first study, where subjects are students who know each other from a class, and norms of honesty among acquaintances apparently encourage truth telling. This is a reminder that the social context of bargaining matters.

[45] In the written and face-to-face conditions, only one-third of subjects truthfully revealed their actual values and another third actively misrepresented their values (more so in writing). Truth telling is not coordinated within dyads, because in only 4 percent and 8 percent of dyads did *both* players reveal in the written and face-to-face conditions.

(failures to agree) will occur, and how much players will earn, are roughly confirmed.

In the sealed-bid mechanism (or bilateral call market), buyers and sellers learn their own valuations and bid, and they trade at the halfway price if the seller offers less than the buyer bids. Strategies resemble the piecewise-linear equilibria predicted by theory to a remarkable degree. Changes in parameters lead to changes in bids in the predicted direction.

Preplay communication has two effects: Talking reduces strategic misrepresentation, resulting in efficiency that is *higher* than predicted (owing to a combination of norms, empathy, nonverbal cues, etc.); and players use communication to agree on a "one-price" equilibrium (an agreed-upon price at which both will bid) that is self-enforcing.

4.4 Conclusion

This chapter has surveyed dozens of experiments on bargaining. I will recap the key regularities and note directions for productive future research. In unstructured bargaining, players gravitate toward focal divisions, such as equal splits. When players bargain over chips that are converted to money at different rates, there are competing focal points: equal-chip divisions, and unequal divisions of chips that equalize money payoffs. Roth et al. showed that having two focal points leads to bimodality in agreements and increases the disagreement rate. Research on environments with richer information, which permits larger self-serving biases in perception, helps explain the rate of disagreement. Loewenstein, Babcock, Issacharoff, and myself showed that interested bargainers encode information self-servingly—they say that arguments that favor them are more important. Self-serving bias is not merely cheap talk, because it is correlated with the length of costly delay. The bias can be erased by having subjects absorb case facts before they know which side of the bargaining they are on (which clearly implicates cognitive encoding as a primary cause of self-serving bias) or by having subjects describe the weaknesses in their own case.

Theoretical work by Rubinstein (1982) and others led to many experiments on alternating-offer bargaining in which players strictly alternate offers and delay is costly because future gains from agreeing are discounted or there is a fixed cost to delay. Several experiments study alternating-offer bargaining with discounting. Generally, players deviate from (perfect) equilibrium predictions in the direction of equal splits, and offers are only weakly sensitive to the structural factors that are predicted to matter by game theory (e.g., bargaining horizon and discount factors). There is a little evidence that "suitable experience" creates convergence to perfect equilibrium, but these conditions seem to be special and fragile (playing separate games corresponding to subgames of a larger game in one study, or special instruction in backward induction).

Formal theories have been developed to explain deviations from self-interest in simple bargaining games, by incorporating a distaste for unequal divisions of money or an (indirect) taste for reciprocating behavior (see Chapter 2). Only one study applies these theories to complex bargaining games, with promising results (see Goeree and Holt, 2001).

These theories replace the self-interest assumption but retain the assumption of equilibrium. However, my work with Johnson et al. using computer displays to record attentional statistics showed deviations from equilibrium that cannot be attributed to social preferences. Players in a three-round game did not look ahead to the second and third rounds as much as backward induction requires—in 15 percent of trials, they did not even bother to open a box showing the "pie size" a round or two ahead. Their lookahead is limited even when they play computerized opponents that are self-interested and rational. Although it is not clear how to integrate limited computation formally with social utility theories, doing so would be an important breakthrough.

Binmore et al. and others have explored how outside options or threat points affect bargaining. The evidence squarely supports the noncooperative game theory view that outside threats affect bargaining divisions *only* if the divisions themselves make it credible for players to exercise the threats. (Cooperative solution concepts, such as the Nash bargaining solution, do not endogeneously determine whether threats are credible, and hence are sensitive to threat points even when the subjects are not.)

One unusual result deserves mention. In bargaining with fixed costs of delay, equilibrium divisions give nearly everything to the player with a lower fixed cost (since a weaker player who holds out suffers relatively more and more as bargaining drags on). In the only experiment on these games, results are quite close to the equilibrium predictions, which are very unequal, in sharp contrast to results from shrinking-pie discounting games. It would be nice to know why perfect (self-interested) equilibria are reached when delay costs are fixed, but not in other structures.

Given the prominence of information asymmetries in naturally occurring bargaining, and the number of theoretical models of such situations, there are relatively few experiments with controlled differences in information. In an early study, Forsythe et al. found that the revelation principle, coupled with a "random dictator" assumption, does a remarkably good job of explaining when disagreements occur. Rapoport et al. investigate a "bazaar mechanism" in which a seller does not know a buyer's value, and makes a series of offers which decline over time. Offers decline as the theory predicts, but respond to changes in discount rates in the wrong direction. Buyers also accept offers too early, so there is scope for learning which should be explored in further experiments.

A few experiments study bilateral bargaining with two-sided incomplete information. These experiments use the "sealed-bid mechanism" in which both sides state a bid, and trade at the point halfway between the bids if the buyer bids more than the seller asks. Bids tend to lie somewhere between truthful revelation (bidding one's value) and piecewise-linear equilibria, which require buyers to bid less, and sellers to ask more, than their values. When the piecewise-linear bid function bends sharply, the empirical bid function usually bends as well.

Radner and Schotter (1989) noticed that in face-to-face bargaining with two-sided incomplete information players often overreveal their values (acting "too honestly," compared with what would maximize their payoffs), which leads to *more* efficiency than is predicted by individual rationality. Exploring this effect, Valley et al. (2002) found that players use the chance to communicate to agree on a single price, which they later bid—although usually not by revealing their actual values. These "one-price" agreements create more efficiency than is possible in the piecewise-linear equilibrium because players whose values are close wouldn't trade in the linear equilibrium, but are able to coordinate a mutually acceptable price when they can talk or pass messages.

It is interesting to contrast the results of the sealed-bid mechanism experiments, in which equilibrium predictions are remarkably accurate (after learning), with the results of alternating-offer bargaining, in which subgame perfection does not generally predict well. One explanation for the difference is that the sealed-bid mechanism is cognitively more transparent—players simply grope for a reasonable markdown factor and don't have to figure out how discount rates and time horizon convey bargaining power. Another explanation is that, since players in the alternating-offer games know precisely how much others are earning, fairness concerns loom larger than in the sealed-bid mechanism (in which the other player's earnings are not known ex ante because their values are not observed).

General theory that could reconcile the descriptive accuracy of game theory in the one domain with its failures in another would be a real triumph. Such a theory will probably weave together perceptions of equity (which may be shared or self-servingly disagreed about), stable social preferences for equal payoffs or fair treatment, heuristic computation, and individual differences (which necessarily create information asymmetries, since players are not sure about their opponent's patience or bargaining skill). Opening the Pandora's box of face-to-face bargaining should also be a research priority.

5
Dominance-Solvable Games

DOMINANCE IS THE MOST BASIC PRINCIPLE Strategy A strictly dominates B if the payoff from choosing A is higher than the payoff from B, for *any* strategy choice by other players' strategy. A weakly dominates B if A's payoffs are higher for some choices by others, and never lower.[1] Dominance is extremely appealing because, if A dominates B, A will turn out as least as good as B no matter *what* you think other players will do. This also means that you should choose a dominant strategy over a dominated one even if you don't know what other players' payoffs are, or how rational they are.

Assuming that other players obey dominance gives a player a way to make a simple, conservative guess about what others will do. Assuming others obey dominance can then enable a player to infer that certain payoffs from her own undominated strategies will never be realized—because they come about only if other players violate dominance. This inference can make a strategy that is initially undominated in effect dominated. Dominance can therefore be applied iteratively: First eliminate dominated strategies for all players; then check whether that first round of elimination makes some (initially undominated) strategies dominated; eliminate those (iteratedly) dominated strategies, and repeat. Games in which this process of iteratively deleting dominated strategies leads to a unique equilibrium are called "dominance solvable."

[1] A related concept which will occasionally come in handy is "stochastic dominance": A risky choice A stochastically dominates B if the chance of earning a fixed amount X or larger is always greater if you pick A than if you pick B.

Two examples will illustrate. Suppose driving the wrong way down a one-way street is akin to a violation of dominance. Then a pedestrian who thinks drivers obey dominance will expect cars to come from only one direction—the correct one—and need look only one way for oncoming cars. Looking both ways before crossing one-way streets therefore implies that the pedestrian thinks the driver may violate dominance.

The importance of a second level of mutual reasoning is illustrated by the words painted on the back of some large eighteen-wheeler trucks. The words are not painted very large, so drivers with normal vision (which excludes Superman and Mr. Magoo) can see them only as they get close to the truck. The words are: "If you *can* see this, I *can't* see you." That means drivers who are close enough to read the words are in the truck driver's "blind spot" (an area behind the truck not visible in the truck driver's side rear-view mirrors). The words install knowledge in the car driver's head about what the truck driver knows (or actually, what the truck driver *doesn't* know—namely, that a car is right behind). Truck drivers use this reminder because they have learned that drivers don't always realize that other drivers may not see or know the same things they themselves do, and that error causes accidents. (This "auto-autism" seems to be on the rise in modern America because drivers of jumbo sport utility vehicles often literally can't see that others can't see.)

Another example is previewed here and in Chapter 1 and is discussed much more later. In the "2/3 beauty contest" game, players choose numbers between 0 and 100. The number closest to (2/3) of the average number wins a fixed prize. Since the average will never be above 100, (2/3) of the average will never be above 67, so choosing a number above 67 violates dominance—no matter what people choose, you have a higher chance of winning by choosing 67 or below than choosing above it. Now if you think people obey dominance, then the average will be no more than 67, and (2/3) of the average will be no more than 44. Therefore, choosing above 44 violates *two* steps of iterated dominance. Choosing above (2/3)44, or 29, violates three steps of iterated dominance, and so forth.

Behavior in dominance-solvable games is an implicit measure of the extent of iterated deletion of dominated strategies. These measures could connect game theory to other social sciences, which study the beliefs people have about the beliefs and behavior of others.[2] The degree of subjects' strategic sophistication is also important in many basic social science de-

[2] For example, developmental psychologists have studied when and how children acquire the concept of "belief". Three year olds know that people who see inside a container know its contents, but do not know how perceptual input affects knowledge (e.g., they think that people who touch a blue ball without looking at it would know that it's blue) (see Wellman, 1990). A prominent theory of autism holds that autistics lack an understanding of beliefs of others, or "theory of mind" (Baron-Cohen, 1995).