ABSTRACT AND LIFELIKE
EXPERIMENTAL GAMES

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In some strange way we devalue things as soon as we give utterance to them. We believe we have dived to the uttermost depths of the abyss, and yet when we return to the surface the drop of water on our pallid finger-tips no longer resembles the sea from which it came. We think we have discovered a hoard of wonderful treasure-trove, yet when we emerge again into the light of day we see that all we have brought with us is false stones and chips of glass. But for all this, the treasure goes on glimmering in the darkness, unchanged (Maeterlinck, 1898, pp. 65-66).

die wurm roer diep in die kokon
en wag op 'n wedergeboorte met vlerke

(Brink, 1965, p. 91).

PREFACE

Like all branches of scientific enquiry, social psychology is a problem-oriented activity: it is concerned with solving problems which arise in attempting to understand the thoughts, feelings and behaviour of individuals in so far as they are influenced by the individuals' social environments. The most active areas of research in social psychology have therefore centred on such phenomena as audience and coaction effects, attitude formation and change, aggression, obedience, leadership and social influence, altruism and so forth.

Over the years, I have become increasingly aware of two peculiar features common to most of the traditional areas of research in social psychology. The first is the essentially non-social character of the phenomena under investigation. In most cases, it is the effects on individual behaviour of social factors which are studied, e.g. the effects on an individual's task performance of the presence of non-competitive coactors, or the effects on an individual's attitude towards some issue of some persuasive message. Only occasionally (as, for example, in research on the group polarization effect) are phenomena investigated which are inherently social in nature (Colman, 1980b).

Secondly, I have been struck by the extent to which social psychological models have followed the conceptual pattern of the physical sciences. This has led social psychologists to view human beings as essentially passive respondents to stimuli of various kinds which impinge upon them from the outside social environment. Models of this type have proved extraordinarily successful in the physical sciences ever since Newton developed his remarkable theory of gravitation, and it is not unnatural that a similar approach should have been tried in the explanation of human social behaviour. I believe, however, that there is a built-in limitation in the application of this approach in social psychology because of its inability adequately to take account of phenomena resulting from deliberate choice.

The theory of games seems to me to provide the most promising alternative to the traditional theories of social behaviour. Gaming models are inherently social in character (an individual's strategy choice in a game cannot even be properly defined without reference to at least one other individual) and they represent a radical departure from the "social stimulus - individual response" approach. They seem, furthermore, to be the only models which can adequately conceptualize an important (and large) class of social behaviours which arise from deliberate free choice.

I confess to being less impressed by the achievements of empirical research in the gaming tradition than by its theoretical substructure. Empirical gaming research has no doubt achieved a great deal, but the results to date do not seem to me to live up to the promise of the late 1950s and early 1960s when the first experiments were performed. There appears, however, to be at least one clearly identifiable reason for the limited success of experimental research in this field, and that is the highly abstract and formal nature of the game models which have almost invariably been used. The tasks presented to the subjects have usually been so far removed from everyday situations of interdependent choice that the applicability of the findings to non-artificial situations is open to doubt, and the behaviour of the subjects has often reflected the artificiality of the experimental tasks. This dissertation represents an attempt to re-orient experimental gaming research in the direction of greater realism and naturalness, and at the same time to investigate the manner in which the abstractness or realism of gaming tasks is relevant to an understanding of subjects' behaviour.

I should like to express my thanks to numerous British and American colleagues who have subjected my ideas to critical scrutiny in seminars, conferences and informal conversations. The following deserve special mention: Julia Gibbs for her expert advice on the design and analysis of gaming experiments, Ian Pountney for giving me the benefit of his profound understanding of mathematical and statistical ideas not touched on in standard textbooks, and Bill Williamson for creating some specialized computer software. In addition, I should like to give thanks to Bill Page for his encouragement over a number of years, to various students for their assistance in the running of some of the experiments, and to Dorothy Brydges for typing the manuscript.

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ABSTRACT

The principles of game theory are outlined with special attention to two-person zero-sum games, two-person and N-person Prisoner's Dilemma, and the game of Chicken. A critique of the minimax rule is developed and a general formula for generating two- and N-person Prisoner's Dilemma is derived. A critical literature review is given, covering all published experiments on two-person zero-sum games, N-person Prisoner's Dilemma, and comparisons between behaviour in abstract and lifelike gaming situations of all types. In Experiment I, 84 Ss played 30 trials of a 2 x 2 zero-sum saddle-point game, presented either as an abstract matrix or as a structurally equivalent lifelike simulation of a naturally occurring strategic interaction, against a minimax, random, or non-minimax opponent's strategy. Significantly more minimax choices were made in response to the minimax than the random, and in response to the random than the non-minimax opponent's strategy, and these differences were most pronounced in the abstract game. The abstract game elicited significantly more minimax choices than the lifelike simulation. Significantly greater departures from the payoff structure (through the introduction of extraneous utilities) were observed in the lifelike than the abstract game, and in response to the minimax rather than the random or non-minimax opponent's strategy. In Experiment II, 80 Ss played in pairs in a Prisoner's Dilemma presented in four structurally equivalent forms: abstract matrix, abstract positive incentive (Ss played for money), abstract negative incentive (Ss played to conserve rather than win money), and lifelike simulation. The lifelike version elicited significantly fewer cooperative choices than the abstract versions. A secular increase in cooperation over trials occurred in the negative incentive condition only. Greatest departures from the payoff structure occurred in the matrix, and least in the simulation condition, but this difference was not significant. In Experiment III, 80 Ss played in pairs in a Chicken game presented in the same four versions as in Experiment II. Least caution occurred in the lifelike version, and significantly more occurred in the positive incentive version. No significant differences emerged in departures from the game structure, but they were greatest in the matrix version. Only the negative incentive condition elicited cyclical choice behaviour. In Experiment IV, 120 Ss played in a three-person Prisoner's Dilemma, presented in the same four versions as before. Least cooperation occurred in the lifelike version, and significantly more cooperation was found in the negative incentive and matrix versions. Cooperation declined significantly across all trial blocks. Departures from the payoff structure did not differ significantly. Cyclical choice behaviour emerged in the negative incentive condition only. It is concluded from the four experiments that choice behaviour differs systematically in structurally equivalent abstract and lifelike strategic interactions, and that the ecological validity of experiments using only matrix games is therefore necessarily limited.

CHAPTER ONE

1. THEORY OF GAMES

1.1. Terminology and Orientation

The theory of games is concerned with the formalization of a particular subset of decision making processes in the area known technically as decision making under uncertainty. Before discussing the properties which distinguish games (or, more precisely, games of strategy) from other situations involving decision making under uncertainty, it is desirable to locate the general class of decision making processes to which such games belong in its widest context.

A problem of decision or choice arises whenever a person is confronted with two or more possible courses of action. These may include the negative alternative of doing nothing, and it may be assumed that the person in question has preferences among the possible outcomes or consequences of his actions; this latter stipulation does not exclude the degenerate case in which he is indifferent regarding the outcomes.

In some decision making situations the decision maker is fully cognizant of the outcomes associated with each of his available courses of action; such problems involve decision making under certainty. A rational decision maker will, by definition, seek to choose in such a way as to obtain the outcome he most prefers. A significant portion of formal theory in economics is concerned with decisions which fall into this class. The famous example of the travelling salesman will suffice to show that problems involved in decision making under certainty are not necessarily trivial. A salesman needs to visit ten specified cities; he wishes to choose the shortest possible route for his round trip. There are 10! = 3,628,800 possible routes between the ten cities, and the salesman can easily determine the distance which each involves. But his problem of choosing his most preferred route is far from trivial: an extremely complex solution via linear programming for a round trip involving seven cities has been given by Norman (1955), but no general method of solution has yet been found. A more familiar example of decision making under certainty occurs in the solution of crossword puzzles: a person may wish to find the three-letter combination which fits the clue "I am classical and arithmetic (3)". Since there are $26^3 = 17,576$ possible three-letter combinations in English, the preferred combination may be hard to find although the puzzler may be able to see at a glance whether or not a specified trigram is the one he prefers. Decisions under certainty are sometimes referred to as games of skill.

In some decision making situations, the outcome of each possible course of action is not known with certainty, but the decision maker knows the exact probability associated with each possible outcome. These problems involve decision making under risk. The most familiar examples of decisions in this category occur in the field of gambling. The formal theory of decision making under risk is virtually coincident with the theory of probability; the classical theory of probability arose in fact out of attempts to provide an understanding of the logic of gambling (an entertaining and informative history of probability is given by David, 1962). One of the most important and successful applications of probability theory, and therefore of the theory of decision making under risk, is found in the theory of statistics. One fairly elaborate example should suffice. In one of Mendel's experiments with peas, he observed 315 round and yellow, 108 round and green, 101 wrinkled and yellow, and 32 wrinkled and green. According to his genetic theory the proportions should be 9:3:3:1. On the basis of this evidence, would it be rational to reject the theory? Using statistical techniques which were not available to Mendel, and which have transformed the problem from one of decision making under uncertainty to one of decision making under risk, it can be shown that the probability of obtaining data which fit the theory as well as this on the basis of chance alone is less than one in a hundred; it would therefore not be prudent to decide that the theory was refuted (*vide* Spiegel, 1961, pp. 206-207). Decision making under risk clearly degenerates into decision making under certainty when the probabilities associated with the various outcomes are each either zero or unity. Situations in which the outcomes are not certain, i.e. true problems of decision making under risk, are often referred to as *games of chance*.

A third and final class of decision making problems arise when the decision maker does not know the outcomes of his available courses of action with certainty and does not even know the probabilities associated with them. These situations are known as *decision making under uncertainty*. A particularly interesting source of uncertainty arises in situations in which the outcome of an individual's decision depends not only on his own choice but also on the choice(s) made by one or more other decision makers; these circumstances give rise to what are known formally as *games of strategy*. The individual decision makers in a game of strategy are known as players, and the courses of action open to them are referred to as strategies. The theory of games, properly understood, is concerned exclusively with games of strategy. The concept of rationality was undefined for all decision making under uncertainty until the advent of the theory of games. The avowed goal of the architects of formal game theory was, however, to find, with respect to games of strategy, "the mathematically complete principles which define 'rational behaviour'" (von Neumann & Morgenstern, 1944, p. 31). As with highly developed theories in other fields, it is useful to distinguish three aspects of the theory: its intuitive and historical background, its formal assumptions, and its applications. These aspects of the theory are discussed in Section 1.2.

1.2. Games of Strategy

The intuitive background of games of strategy can be appreciated by considering the following examples:

(a) Two people are walking towards each other along a narrow passage. They are set on a collision course, but each would prefer not to collide with the other. Each may be assumed to have three strategies available: swerve to the left, swerve to the right, or keep going straight ahead. It may further be assumed that the straight ahead-straight ahead, left-right and right-left strategy combinations result in collisions and that all other strategy combinations avoid collisions. The outcome evidently depends upon the strategies of both players.

(b) Three retail companies are each trying to corner a slice of some specified market. They are each faced with a choice between two available strategies: cut prices or hold prices. If all three players cut prices, increased sales will exactly offset reduced profit per unit sale and none of them will gain or lose anything. The status quo will similarly be preserved if all the players adopt the strategy of holding prices. The outcomes associated with all other strategy combinations are gains for one or two of the players and corresponding losses for the other(s).

(c) A man obtains information about an extra-marital affair being conducted surreptitiously by his neighbour, and he communicates to the latter a plan to blackmail him. The strategies available to him are to go ahead with his blackmail plan or to withdraw the threat. His neighbour's available counter-strategies are to expose the would-be blackmailer to the police, to give in to the blackmailer's demands, or to call the blackmailer's bluff. Various outcomes result from the combinations of these strategies, some of which may be disliked by both players and some by one or other of the players.

It is evident from the above examples that games of strategy may involve two or more players each having two or more available strategies. The players may be individuals, groups, governments or any other decision-making agents. An examination of the examples also illustrates three different types of relationship which may exist between the preferences of the players among the possible outcomes. In the first example, the preferences of the players coincide exactly: outcomes which are preferred by one player are always preferred by the other and vice versa. Games of this type are known as <u>pure coordination games</u>. In the second example the preferences of the players are strictly opposed: an outcome which is favourable for one player is unfavourable for the others and vice versa. Such games are known as *purely competitive games*, or more formally as *constant sum* or *zero-sum games*. The third example illustrates a situation in which the preferences of the players are partly coincident and partly opposed; these games are sometimes referred to as *non-zero-sum games*, but a more usual (and less ambiguous) designation is *mixed-motive games*.

The taxonomy of games of strategy given above follows Schelling's (1963) suggested "reorientation of game theory". It should be pointed out, however, that many standard textbooks implicitly exclude pure coordination games from their definitions of games of strategy. Luce and Raiffa (1957), for example, consider situations in which the preferences of two or more decision makers coincide to be "trivial" (pp. 59, 88), and they prefer to treat such a group of players as a single decision maker (p. 13). Rapoport (1960a, p. 108) and Shubik (1964, p. 8) explicitly refer to "conflict of interests" in their definitions of games of strategy. Empirical research in various areas of social psychology, for example the well known work on communication nets in problem-solving groups, attests to the non-triviality of at least some pure coordination problems; and in any event it seems desirable to include such games in the definition for the sake of symmetry. Pure coordination games may be considered a limiting case of mixed-motive games, and zero-sum games are a limiting case at the opposite extreme.

The first person to attempt a formalization of the theory of games was the French mathematician Emil Borel in the early 1920s. Borel's work was, however, limited by his failure to derive the crucial theorem, known as the minimax theorem, which lies at the heart of formal game theory. The minimax theorem was first proved by John von Neumann in 1928. Independently of both Borel and von Neumann, R.A. Fisher (1934), whose important contribution to experimental design is well known, proved the theorem for the limited class of 2 x 2 games and introduced the term saddle-point, but he was apparently unaware of the theorem's generality.

Game theory did not attract widespread attention in France, Germany or England until the publication in the United States of von Neumann and Morgenstern's classic Theory of Games and Economic Behavior in 1944. This book stimulated a great deal of interest among mathematicians, but it was a later book by Luce and Raiffa (1957), Games and Decisions, which made game theory accessible to psychologists and social scientists.

Virtually no empirical research on the behaviour of people in gaming situations had been published before 1957, but a wave of experimental studies followed the publication of Luce and Raiffa's book. A special section in the Journal of Conflict Resolution was set aside for the publication of such work from 1965 onwards. Experimental gaming had by then become established as an active field of research. By the late 1970s more than a thousand experimental studies had appeared, and experimental gaming began to make its presence felt in non-specialist introductory text books of social psychology (e.g. Tajfel & Fraser, 1978).

The formal assumptions of game theory are as follows. Two or more autonomous decision makers or players (A, B, ..., N) each have two or more strategies available (A_1, A_2, ..., A_i, ...; B_1,

B_2, ..., B_j, ...; N_1, N_2, ..., N_k, ... respectively). It is assumed that the relevant courses of action are exhaustively specified by this scheme. The players make their strategy choices simultaneously, or, what amounts to the same thing, each player makes his choice in ignorance of the choice(s) of the other player(s). The outcomes which result from the joint strategy choices are designated a_{ij} ... b_{ij} ... n_{ij} ..., where a_{ij} ... is the payoff to A given A's strategy i, B's strategy j etc. The payoffs may be events like avoiding a collision, receiving a specified financial reward, being exposed as an adulterer, or any other contingent or noncontingent event or sequence of events which a player may potentially prefer to some other event(s). It is further assumed that each player is fully cognizant of the rules of the game, i.e. of the strategies available to each player (including himself) and the payoff function. A final implicit assumption is that each player knows that the others are playing according to the same set of rules.

One assumption built into the above model may appear excessively restrictive; this is the stipulation that each player must choose only one strategy in the game. In game-like situations in the real world, including board games like chess, the outcome may not become apparent until a series of choices has been made. This problem is dissolved by allowing the definition of a player's strategy to include a complete specification of a series of choices to cover all contingencies, i.e. "a plan which specifies what choice he will make in every possible situation for every possible actual information which he may possess at that moment" (von Neumann & Morgenstern, 1944, p. 79). The specification of a single strategy to cover all contingencies in, for example, a game of chess is of course an immense task, but it is not impossible: this is precisely what computer programs for playing chess are necessarily based upon. The important point about such programs is that they illustrate the way in which complex contingency plans can be specified so as to constitute a single strategy in the game theory sense: the chess programmer has to choose but one program for the computer to follow. The game theory assumption that each player may make only a single choice, that he may have only one bite at the cherry so to speak, is (in theory at least) therefore only apparently restrictive.

The most severe restrictions of formal game theory lie elsewhere. They are (a) that the players are fully cognizant of the rules of the game, and (b) that they have to make their decisions simultaneously or in ignorance of each others' decisions. These restrictions have, however, been relaxed in many experimental gaming studies in which either knowledge of the rules is not complete, negotiation is allowed between players, or the game is repeated a number of times. In some experiments more than one of the above restrictions have been relaxed.

Before turning to applications of the theory of games, it is useful to distinguish between formal and informal developments of the theory. The most satisfactory formal development has undoubtedly been von Neumann's (1928) solution of the class of two-person zero-sum games. An unexpected consequence of this solution was the discovery of a method for solving linear programming problems (see, e.g., Williams, 1966, pp. 210-213). A great deal of formal theory has evolved to deal with the n-person zero-sum case, but the numerous suggested solutions in this area lack the intuitive persuasiveness of the minimax solution to the two-person variety. A considerable amount of formal theorizing has also grown up around certain two-person mixed-motive games, notably the game Prisoner's Dilemma, and also more recently N-person Prisoner's Dilemma. These developments are discussed below.

Numerous writers have applied game theory to the analysis of various problems in economics (e.g. Shubik, 1959; Siegel & Fouraker, 1960; von Neumann & Morgenstern, 1944) and to military deterrence and retaliation, arms control and thermonuclear war (e.g. Rapoport, 1964; Schelling, 1963). The theory of committees and elections has been illuminated by game theory (e.g. Colman, 1980a; Colman & Pountney, 1975a, 1975b, 1978; Farquharson, 1969). Several contributions to social anthropology have been made (Buchler & Nutini, 1969), and three important applications to moral philosophy have appeared (Braithwaite, 1955; Rapoport, 1968; Schelling, 1968). A somewhat surprising area of application has recently come to light in the resolution of certain paradoxes in the theory of evolution (Maynard Smith, 1978). An attempt has also been made by means of game theory to account for certain phenomena associated with attitude change and persuasion (Colman, 1975). The most popular areas of application in psychology, however, have been to the understanding of cooperation and competition, trust, trustworthiness and suspicion, risk taking and threats. Research in these areas is reviewed in Chapter 2.

1.3. Theory of Two-person Zero-sum Games

The defining property of zero-sum or constant-sum games is strict opposition of interests among the players. In a two-person zero-sum game, one player's gain in any specified outcome is equal to his opponent's loss and vice versa. More formally, $a_{ij} = -b_{ij}$ for any outcome $a_{ij}b_{ij}$; this is equivalent to setting $a_{ij} + b_{ij} = 0$ which accounts for the term "zero-sum". It can easily be shown that any linear transformation of the payoffs in a game of strategy leaves the strategic properties of the game unaltered: a game is therefore not strategically altered and the play is not affected if we set $a_i' = a_i + c$ and $b_j' = b_j + c$. It follows that any game in which $a_i + b_j = x$, a constant-sum game, can be reduced without strategic implications to a zero-sum game by substracting $x/2$ from each of the payoffs. Only the zero-sum case need therefore be considered.

The two-person zero-sum case is considered by virtually all game theorists to be solved by the minimax theorem in the sense that an "optimal" or "rational" strategy can be prescribed for any game of this type. It has often been pointed out, however, that most two-person conflicts in everyday life are not strictly zero-sum. In wars, for example, the opposing players would both normally prefer a deadlock to mutual annihilation: in the latter outcome, one player's loss is not matched by a corresponding gain for the other, so the game is not zero-sum. Isolated battles may, on the other hand, be considered zero-sum, as may certain types of economic competition between firms, constituency elections and other forms of political conflict, and sports and board games. Most other everyday conflicts are more realistically modelled by mixed-motive games.

The most common (and for most purposes also the most useful) method of representing a two-person game is by means of a <u>payoff matrix</u>. In this "normalized" representation of the game, the strategies and payoff function, which constitute the rules of the game, are clearly shown: each row corresponds to one of Player A's available strategies, each column corresponds to one of B's strategies, and the matrix elements are payoffs associated with pairs of opposing strategies. In the case of zero-sum games it is customary to omit B's payoffs from the normalized representation, since they are necessarily merely the negatives of A's. A simple example taken from Haywood (1954) is shown in Figure 1.1.

	B_1	B_2
A_1	2	2
A_2	1	3

Figure 1.1. The Battle of Bismarck Sea

This example relates to an incident during the Second World War. In the critical phase of the struggle for New Guinea, intelligence reports indicated that the Japanese were planning a convoy from Rabaul to Lae. The convey could travel either north of the island of New Britain, where poor weather was almost certain, or south of the island, where the weather would be clear. General Kennedy had the choice between concentrating his reconnaissance aircraft on one route or the other. Once sighted, the convoy would be bombed until its arrival in Lae. Kennedy's alternative strategies are indicated by A_1 (North) and A_2 (South) in Figure 1.1.; the Japanese alternatives are shown as B_1 (North) and B_2 (South). The matrix elements correspond to estimates made by Kennedy's staff of the amount of bombing, measured in days, which could be expected in each outcome. A zero-sum model seems reasonable in this case since the Japanese valuation of the payoffs may legitimately be taken to be the negatives of those of the Americans.

The optimal or rational strategies are not difficult to find in this simple example: General Kennedy should choose A_1 and the Japanese should choose B_1. These choices were in fact made, and the Japanese suffered severe losses. According to Haywood (1954), however, "although the battle of the Bismarck sea ended in a disastrous defeat for the Japanese, we cannot say the Japanese commander erred in his decision" (p. 369), although he _might_ have come off more lightly by travelling south of the island.

In their classic exposition of formal game theory, von Neumann and Morgenstern (1944) approach the solution of two-person zero-sum games via two models which depart from the normalized game. In the first of these modified games, called the _minorant_ game, A makes his choice first, and B chooses in full knowledge of A's choice. It is clear that in this game B is confronted with a decision under certainty: assuming only that he is rational under certainty, his choice is prescribed. Given strategy A_i by A, each of B's available strategies yields a single certain outcome. Since B's payoffs are the negatives of the matrix elements, his rational choice where A_i is given is the strategy associated with the outcome

$$\min_j a_{ij}.$$

In other words, he should examine the row selected by A and choose the column corresponding to the minimum element in that row. In this minorant game, if it is further assumed that A is rational under certainty and knows that B is also rational under certainty, then A, moving first, knows that B will choose the minimum element in any row selected by A. It is therefore unnecessary for A to consider any matrix elements which are not now minima. He should therefore choose the "maximin" strategy which contains the largest of the row minima, i.e. he should choose

$$\max_i \min_j a_{ij}.$$

In von Neumann and Morgenstern's second modified version of the normalized game, known as the _majorant_ game, B chooses before A, who then chooses with certainty regarding the outcome. Given the same assumptions as in the minorant game, B should ignore matrix elements which are not column maxima, and should choose the column containing the lowest of the column maxima. His only rational choice is his "minimax" strategy

$$\min_j \max_i a_{ij}.$$

Returning now to the normalized game, neither player knows with certainty his opponent's choice before he selects his own strategy. The minimax principle which von Neumann and Morgenstern (1944) advocate is that each player should nevertheless choose _as though_ he were moving first in a minorant or majorant game, and _as though_ he were certain that his opponent was rational under certainty and assumed him to be so also. Thus A should choose his maximin strategy and B should choose his minimax strategy. (It is customary to refer informally to the prescribed strategy of either player as minimax.)

The adoption of his minimax strategy has the property of maximizing a player's security level. E.g. in the game discussed above it guarantees Kennedy a minimum of two day's bombing (which his other strategy does not) and it guarantees the Japanese a maximum of two days' bombing (which their other strategy does not). The crucial property of the minimax strategy is that it yields a payoff as good or better than any other against a minimax choice from the adversary. This payoff is known as the value of the game to each player; it can be interpreted as the amount a player should be prepared to pay for the privilege of playing the game.

In the Battle of Bismarck Sea example discussed above, both players chose their minimax strategies, which in that case were A_1 and B_1. An important property of that game was that the largest of the row minima was the same as the smallest of the column maxima. Such games are (somewhat inelegantly) described by von Neumann and Morgenstern (1944) as specially strictly determined. Not all games of this type are as simple (in the sense of possessing an immediately obvious solution) as the example cited above. A slightly more complicated example of a game of this type is shown in Figure 1.2.

	B_1	B_2	B_3	B_4	Row Min.
A_1	7	2	5	1	1
A_2	2	2	3	4	2
A_3	5	3	4	4	3*
A_4	3	2	1	6	1
Col. Max.	7	3*	5	6	

Figure 1.2. A 2 x 2 Zero-sum Game with a Saddle-point

An examination of Figure 1.2 reveals that the maximum of the row minima and the minimum of the column maxima (shown starred) are the same; the prescribed minimax solution is A_3B_2 with a payoff of 3 to A and -3 to B. Like the game shown in Figure 1 and all other specially strictly determined games, there exists in this case what is known as a saddle-point. (This term derives from the fact that a saddle is normally placed on a horse's back at the lowest point on the back-to-front axis and the highest point on the side-to-side axis. The saddle-point outcome is sometimes (e.g. Luce & Raiffa, 1957) referred to as an equilibrium pair; this term draws attention to the fact that "it does not behove either player to change his choice if the other does not change" (p. 62). When a saddle-point or equilibrium pair exists, formal game theory, prescribes that it should be chosen by both players, i.e. it corresponds to the intersection of their "optimal" or "rational" minimax strategies and it maximizes both their security levels at a point equal to the value of the game. Each player is guaranteed a payoff equal to the value of the game, and he may do better if his adversary neglects to choose his own minimax strategy.

An apparent complication arises from the fact that some games have more than one saddle-point. Consider the game shown in Figure 1.3.

	B_1	B_2	B_3	Row Min.
A_1	3	2	0	0
A_2	5	6	5	5*
A_3	4	1	2	1
Col. Max.	5*	6	5*	

Figure 1.3. A Game with Two Saddle-points

By searching for the maximum of the row minima and the minimum of the column maxima (shown starred) it can easily be determined that there are two saddle-points at A_2B_1 and A_2B_3. When more than one saddle-point exists, the solution seems on the face of it problematical since it seems possible that A may prefer one saddle-point and B another. Furthermore it seems that A may choose a row containing a saddle-point and B may do likewise without their choices intersecting in any of the saddle-point cells.

These apparent problems can be dissolved quite simply. Suppose a game has a saddle-point at A_iB_j and that A_xB_y represents any other strategy pair in the game. Since a saddle-point represents the intersection of minimax strategies, and since a minimax strategy guarantees a payoff as good or better than any other strategy against a minimax choice on the part of the adversary, we can state the following inequality:

$$a_{xj} \leq a_{ij} \leq a_{iy}.$$

Suppose now that the game has another saddle-point at A_mB_n and that A_xB_y represents any other strategy pair in the game. Then:

$$a_{xn} \leq a_{mn} \leq a_{my}.$$

Combining these inequalities by substituting for x and y we get

$$a_{ij} \leq a_{in} \leq a_{mn} \leq a_{mj} \leq a_{ij}.$$

Since the same inequality appears at the left and right, therefore

$$a_{ij} = a_{in} = a_{mn} = a_{mj}.$$

It follows firstly that the two saddle-points have equal payoffs ($a_{ij} = a_{mn}$), and secondly that the minimax strategies necessarily intersect in a saddle-point (since all combinations of minimax strategies yield the same payoffs). The proof given above can easily be generalized for three or more saddle-points.

It is widely believed, both by critics of game theory and by game theoreticians themselves, that interval-scale numerical assignment of payoffs (corresponding perhaps to utilities) is always necessary for the solution of two-person zero-sum games. Shubik (1964), for example, comments: "A basic problem . . . is the construction of a preference system and the investigation of the possibilities for the measurement of preference. The numbers in the payoff matrix have to be obtained in some manner or other. It is difficult enough to be able to state with certainty that an individual prefers to see Jones as a senator than Smith; it is more difficult (and some may say impossible) to state by how much he prefers Jones to Smith" (p. 19). The following example demonstrates that numerical utilities are not necessary for the solution of all games (see Figure 1.4).

	B_1	B_2	B_3	B_4	Row Min.
A_1	-	?	-	+	-
A_2	?	?	+	+	?*
A_3	-	+	-	+	-
A_4	?	+	?	-	-
Col. Max.	?*	+	+	+	

Figure 1.4. A 4 x 4 Saddle-point Game with Ordinal Payoffs

Figure 1.4 shows the payoff matrix of a two-person zero-sum game in which the weakly ordered payoffs are labelled Poor (-), Indifferent (?) and Good (+). It is evident that the saddle-point (A_2B_1) can be located without any assumptions being made about the degree of preference implied by the symbols.

Each of the games so far considered has been "specially strictly determined", and the minimax solution has in each case been found by locating the saddle-point of the game. Consider now the game shown in Figure 1.5.

	B_1	B_2	Row Min.
A_1	0	7	0
A_2	10	4	4*
Col. Max.	10	7*	

Figure 1.5. A 2 x 2 Zero-sum Game with No Saddle-point

In this example the maximum of the row minima (4) is not equal to the minimum of the column maxima (7); the game is not "specially strictly determined" and the strategies corresponding to the minimax choices do not intersect in a saddle-point. It is intuitively obvious from a contemplation of the minorant and majorant versions of this game that its value should lie somewhere between 4 and 7, but it seems that no strategy which A adopts can be guaranteed to improve on 4, and no strategy which B adopts can guarantee to improve on 7.

The surprising and elegant solution to non-saddle-point games, which was first proved by von Neumann (1928) rests upon the concept of a mixed strategy. The proof of the so-called minimax theorem amounts essentially to this: if mixed strategies, in the sense of probability distributions across the set of pure strategies, are included in the payoff function, then every two-person zero-sum game can be shown to have a saddle-point which corresponds to the value of the game. In the words of R.A. Fisher (1934), who independently proved the minimax theorem for the restricted class of 2 x 2 zero-sum games, the introduction of randomization ensures that "the chances of the game are stabilized in the saddle" (p. 296).

The minimax solution of the game shown in Figure 1.5 is as follows: Player A should randomize his strategies A_1 and A_2 in the ratio 6:7, and Player B should randomize B_1 and B_2 in the ratio 3:10. Since the game is played only once, this amounts to using a table of random numbers or some other randomizing device to select a pure strategy with the prescribed probability. In a 2 x 2 game (such as this), the adoption of the minimax mixed strategy by either player yields the same expectation (equal to the value of the game) against any pure or mixed strategy which his adversary may adopt.

In the game under consideration, for example, Player A's 6:7 mixed strategy yields, against a pure B_1 choice on the part of his adversary, an expectation of (6 x 0 + 7 x 10)/6 + 7 = 70/13 = 5.39. Against a pure B_2 choice from his adversary, it yields similarly (6 x 7 + 7 x 4)/6 + 7 = 5.39. Likewise, Player B's 3:10 mixed strategy yields an expectation of 5.39 against either of Player A's pure strategies. It can easily be shown that the adoption of the minimax mixed strategy by either player guarantees the same expectation against any mixed strategy on the part of his adversary as well. The value of the game under consideration is evidently 5.39. In two-person zero-sum games with more than two pure strategies per player, the adoption of the minimax mixed strategy by either player not only maximizes his security level but also gives the possibility of higher expectations should his adversary neglect to mix his own strategies in the prescribed manner.

The discovery that, by allowing his strategy choice to be dictated by choosing from a table of random numbers or rolling dice, a player can raise his security level, is surprising to say the least. The logic is nevertheless uninpeachable: in the game shown in Figure 1.5, Player A's security level (the worst he can expect) is at best equal to 4 if he uses a pure strategy, but it is raised to 5.39 if he uses the prescribed minimax mixed strategy; Player B improves his security level from 7 to 5.39 by doing likewise. A more familiar example should help to clarify the issue.

Consider the ancient game of Morra. Two players simultaneously show either one or two fingers each, and each player may simultaneously shout out a number which represents his guess regarding the total number of exposed fingers. If both players guess correctly, or if neither guesses correctly, then neither player wins. If only one of the players guesses correctly, however, he wins an amount corresponding to the number of fingers shown. Formally, this is an 8 x 8 zero-sum game without a saddle-point. It turns out (Rapoport, 1970b, p. 5) that the minimax mixed strategy for either player is: always guess "3" and randomize showing one or two fingers in the ratio 41:29. This guarantees the player an expectation of breaking even, i.e. the value of the game is zero (it is a fair game) as one might expect from its symmetry. Against an opponent who adopts a non-minimax pure or mixed strategy, it guarantees an expectation of winning; in other words, if the game is played repeatedly, a positive gain becomes increasingly certain. As explained above, this arises from the existence of more than two pure strategies per player in the payoff matrix.

1.4. Critique of the Minimax Rule

The contribution of formal game theory, as first expounded by von Neumann (1928) and subsequently elaborated by von Neumann and Morgenstern (1944), is threefold in character: (a) the precise formalization of games of strategy; (b) the derivation of the minimax theorem for two-person zero-sum games; and (c) the characterization of a minimax strategy as the "optimal", "rational" or "best" strategy available to a player. The third aspect, which may be called the minimax rule, is prescriptive or normative in character, and appears to be open to a certain amount of criticism, although it has been accepted without question by almost all students of game theory. (See, however, Ellsberg, 1956.)

In his original statement of the problem (seldom read today), von Neumann (1928) gives primary emphasis to the normative aspect of the theory: "N Spieler, S_1, S_2, ..., S_n, spielen ein gegebenis Gesellschaftsspiel G. Wie muss einer dieser Spieler, S_m, spielen, um dabei ein moeglichst guenstiges Resultat zu erzielen?" (p. 1). ("N players, S_1, S_2, ..., S_n, play a given game of strategy G. How should one of these players, S_m, play in order thereby to obtain *a* best possible result?" - my translation and my emphasis.) Several pages later, after rigorously defining Player S_1's maximin strategy in a two-person zero-sum game, von Neumann uses even more emphatic language to characterize his normative presceiption: "Dass diese Schweirigkeit auftritt, kann man sich auch so klarmachen: $\mathrm{Max}_x \, \mathrm{Min}_y \, g(x,y)$ ist das beste Resultat, das S_1 erzielen kann . . ." (p. 9). ("That this difficulty occurs, one can also illustrate as follows: $\mathrm{Max}_x \, \mathrm{Min}_y \, g(x,y)$ is *the* best result that S_1 can obtain . . . " - my translation and my emphasis again.)

Without wishing to seem pedantic, it is worth pointing out that there is a slight difference between "a best possible result" and "the best result". It is of some importance to know precisely what status the architects of game theory attach to their normative prescription. It will be shown below that the minimax rule does not necessarily yield *the* best possible result.

In the most authoritative and widely-quoted account of formal game theory, von Neumann and Morgenstern (1944) centre attention on the discovery of a rule for prescribing behaviour under uncertainty. In the special case of two-person zero-sum games they claim to give "a precise theory . . . which gives complete answers to all questions"

(p. 101). They later describe the minimax rule as follows: "It is reasonable to define a good way for 1 to play the game . . . " (p. 108, emphasis added), but they continue later on the same page: "So we have the good way (strategy) for 1 to play the game . . . " (loc.cit., emphasis added). Once again we see the fudging of meaning regarding the precise status of the minimax rule.

Two questions arise. Is the minimax strategy always necessarily the best strategy for a player to adopt? To be even more specific, should a player always adopt his minimax strategy? Interpretations of the sacred scriptures vary from conservative to radical. An example of a highly conservative interpretation is found in Luce & Raiffa's classic exposition: "What should [a player] do? Game Theory does not attempt to prescribe what he should do! It does point out that Player 1 can guarantee himself [his maximin] but what 1 should do the theory is careful to avoid saying" (pp. 62-63, emphasis in original). A typically radical interpretation is given in the equally authoritative manual by J.D. Williams (1966): "We shall always be seeking solutions to games. This means that we shall try to discover which strategy or strategies the players should use . . . . Every game of the type we shall consider does have a solution . . . ." (p. 29, emphasis in original).

What indeed ought a player to do? Let us examine the logic of the minimax rule a little more closely. The rule amounts to a prescription for playing a game which maximizes a player's security level, i.e. it guarantees a player the best of the wordt possible outcomes. The "optimal" strategy prescribed by the rule does not necessarily coincide with the maximax (Player A's highest possible payoff) or minimin (Player B's best possible outcome); the adoption of the minimax strategy may in fact ensure that these most preferred outcomes are impossible. Are there then no situations in which a player should (or would be rational to) choose a non-minimax strategy which might yield a better result than his minimax strategy?

A contemplation of the above question brings to light a hidden assumption in the minimax rule. The assumption is that a player has no reason to believe that his opponent will fail to choose rationally. If he did know with certainty that his opponent would choose irrationally, the minimax rule would fail to carry any prescriptive force. Under these conditions it would be decidedly irrational for a player to forego certain gains by choosing his minimax strategy; the minimax rule obliges a player to choose as if he knew his opponent to be rational. In many situations, however, a player may have good reasons for believing that his opponent is irrational, that he may not or will not adopt his "optimal" strategy. When confronted with an evidently irrational opponent, an "irrational" non-minimax choice may be the most sensible strategy open to a player.

Von Neumann and Morgenstern (1944) appear not to have considered the above possibilities fully. Their claims for the minimax rule appear to go beyond their justification: "the superiority of 'rational behaviour' over any other kind is to be established . . . for all conceivable situations - - including those where 'the others' behave irrationally" (p. 32). This superiority can only be demonstrated, however, on the assumption that the players have no reason to believe that their opponents will act irrationally; and such an assumption is unrealistic in many situations.

1.5. Theory of Two-person and N-person Mixed-motive Games

All the games considered in Section 1.4 possessed the following two important properties: (a) they each involved only two players, A and B; (b) each was strictly competitive or zero-sum, i.e. in every case $a_{ij} = -b_{ij}$. These properties are not inherent in the definition of games of strategy, however, and cases will now be considered in which one or both of the above restrictions is relaxed. There is relatively little formalization in these areas, and no general theory exists which can be applied to games with differing strategic properties. The discussion will therefore be restricted to the three types of mixed-motive game which are the objects of empirical research later in this dissertation, namely Prisoner's Dilemma, Chicken and N-person Prisoner's Dilemma. These three games are quite distinct although they share certain features in common.

On account, no doubt, of its exceedingly paradoxical strategic properties, the Prisoner's Dilemma Game has attracted far more attention from game theoreticians and empirical researchers than any other type of mixed motive game. The simplest possible example of Prisoner's Dilemma is given in Figure 1.6.

	B_1	B_2
A_1	3,3	1,4
A_2	4,1	2,2

Figure 1.6. Prisoner's Dilemma

Since this is a mixed-motive game, and B's payoffs are not merely the negatives of A's, it is customary to represent the payoffs as shown: for each strategy pair the outcome is represented with the left-hand matrix element corresponding to A's payoff and the right-hand matrix element to B's payoff. How should a player choose in a situation of this type? The dilemma arises from the obvious fact that if each player chooses his safe (minimax) strategy, then both players obtain a lower payoff than if they each choose "Irrationally". Thus if A chooses A_2 and B chooses B_2, then each player wins 2 units, but if A chooses A_1 and B chooses B_1, they each win 3 units.

The paradox is sharpened by the following considerations. Whether B chooses B_1 or B_2, A is better off choosing A_2 than A_1: against either strategy choice on the part of B, A achieves a higher payoff by choosing A_2 than by choosing A_1. B is similarly better off choosing B_2 against either strategy choice on the part of A than he is by choosing B_1. Nevertheless if both players adopt this individualistic philosophy, both end up by achieving lower payoffs than they would by choosing differently. Individualistic rationality leads to an evidently deficient outcome in Prisoner's Dilemma; what seems to be required is some sort of collective rationality such as is embodied in Immanuel Kant's categorical imperative: "Act in such a manner that if others acted similarly everyone would benefit thereby".

A formal definition of Prisoner's Dilemma follows. Let the following identities and inequalities hold among the matrix elements (payoffs) in a 2 x 2 game:

(i) $a_{11} = b_{11} = R$, $a_{22} = b_{22} = P$, $a_{12} = b_{12} = S$, $a_{21} = b_{12} = T$;

(ii) $T > R > P > S$;

(iii) $2R = S + T$.

The identities and inequalities in (i) and (ii) above ensure that the game has the following properties: (a) each player has a dominating strategy, i.e. a strategy which yields a higher payoff than the other against either counter-strategy on the part of the opponent; and (b) the dominating strategies intersect in a Pareto non-optimal equilibrium, i.e. if both players choose their dominating strategies, neither player has cause to regret not having chosen his other strategy (the strategies are in equilirbium), but this outcome is non-optimal for both players singly and collectively. The identity in (iii) above, which is not regarded as a necessary property by all game theoreticians, simply ensures that R is a better outcome for either player than a lottery between S and T, i.e. in repeated plays of the game, a player cannot improve on R by forming a tacit agreement with the other player whereby each player alternates between S and T.

The strategies A_1 and B_1 are frequently referred to as cooperative choices, and the strategies A_2 and B_2 as competitive or defecting strategies. The intuitive basis for these terms is fairly obvious: it is only by tacitly agreeing to cooperate that the players can achieve the payoff R, and if one player attempts to cooperate in this manner, the other may defect by choosing A_2 or B_2 and thus achieve a higher individual payoff (T). The labels, T, R, S and P refer respectively to the <u>temptation</u> (to defect), the <u>reward</u> (for joint cooperation), the <u>sucker's</u> payoff (for unilateral cooperation) and the <u>punishment</u> (for joint defection).

The paradox was first reported by Flood in 1951, but it was A.W. Tucker who formulated it explicitly and gave it its name (Rapoport, 1967). The name derives from the following frequently-cited illustration of Prisoner's Dilemma: Two prisoners, held incommunicado, are charged with involvement in the same serious crime. They cannot be convicted unless at least one of them confesses. If neither confesses, they will receive minor sentences for illegal possession of firearms. If they both confess, they will both receive fairly heavy sentences. If, however, one confesses and the other refuses to confess, the former will be set free without any sentence as a reward for turning Queen's evidence, while the latter will receive the heaviest possible sentence. The dilemma in this case can be described as follows: "It is in the interest of each to confess <u>whatever</u> the other does. But it is in their <u>collective</u> interest to hold out" (Rapoport & Chammah, 1965, p. 25).

It is far from obvious what is the rational way to choose in a game of Prisoner's Dilemma. A number of attempts have been made to resolve the paradox by defining rationality in an intuitively satisfactory way for this game, but none has succeeded in gaining much support. The most ambitious such attempt to date has been the metagame approach by N. Howard (1966a, 1966b, 1971). Howard's idea is essentially as follows. A chooses as if he is playing a minorant version of the game; he has four strategies available: A_1 (cooperate whatever B chooses), A_2 (copy B's choice), A_3 (make the opposite choice to B), A_4 (defect whatever B chooses). Now B chooses as if he were aware that A had defined his available strategies as above. B therefore has 16 conditional strategies, e.g. CCCC (cooperate whether A chooses A_1, A_2, A_3 or A_4), CDCC (defect if and only if he expects A to match his own choice by selecting A_2) and so on. This analysis generates a 4 x 16 metagame. An investigation of the resulting matrix reveals three equilibria; apart from the deficient minimax equilibrium (represented in the metagame by A_4DDDD, there are two new equilibria (A_2CCDD and A_2DCDD). Both of these equilibria yield better payoffs for each player (R) than the original minimax choices (P). The rational solution, according to Howard, is for A to copy what he expects B's choice to be, and for B to cooperate if and only if he expects that A will copy his choice, i.e. A should choose A_2 and B should choose DCDD.

Whether the metagame approach really succeeds in dissolving the paradox of the Prisoner's Dilemma is open to some doubt. Its most ardent adherent has been Rapoport (1967, 1969, 1970a) in a debate in Psychological Reports with Harris (1969a, 1969b, 1970). Howard (1970) appended his own comments on the Harris-Rapoport controversy in the same journal, but in general Harris seems to have got the better of the debate. The greatest weakness of the metagame solution appears to be the "as-if" quality of the thinking which is assumed to lie behind the players' strategy choices. This is not the place, however, for a full and penetrating analysis of this difficult question.

Let us now turn to the game of Chicken. The simplest possible example of this well-known game is shown in Figure 1.7.

	B_1	B_2
A_1	3,3	2,4
A_2	4,2	1,1

Figure 1.7. Chicken

This game bears certain resemblances to Prisoner's Dilemma, but its strategic properties are really quite different. First of all, neither player has a dominating strategy: Against B_1 A does better with A_2 than with A_1, but against B_2 he does better with A_1 than with A_2 (similar considerations apply to B's choice). Secondly, it is evident that two equilibria are present rather than one, one at A_1B_2 and one at A_2B_1: in either outcome, neither player has cause to regret not having chosen differently.

The formal definition of Chicken rests upon the following identities and inequalities among the matrix elements of a 2 x 2 game:

(i) $a_{11} = b_{11} = R$, $a_{22} = b_{22} = P$, $a_{12} = b_{21} = S$, $a_{21} = b_{12} = T$;

(ii) $T > R > S > P$.

This game is clearly distinguished from Prisoner's Dilemma by the fact that the worst possible outcome for both players occurs at the intersection of their defecting strategies. Chicken is therefore regarded as the prototype of a dangerous game (Swingle, 1970a). Its name derives from various versions of a dangerous game popular in certain American subcultures. Two players may, for example, each get into a motor car in an open area and then drive towards each other at speed. The first person to swerve is labelled "chicken". The "swerve" strategy corresponds to A_1 or B_1 in Figure 1.7, and the "don't swerve" strategy is A_2 or B_2. In all dangerous games, and in particular in Chicken, a player has to expose himself and his opponent to the risk of substantial loss in order to obtain the maximum payoff. Such games therefore contain the strategic potentialities for the use of threats.

A less frequently noticed feature of the strategic structure of Chicken is the possibility to which it gives rise for "the political uses of madness" (Ellsberg, cited in Schelling, 1963, p. 13): if a player is seen by his opponent to be "irrational" or "mad" or "not in control of himself", he is at a decided advantage in a game of Chicken. The following example of the effectiveness of deliberate irrationality in automobile chicken is taken from Kahn (1965): "The 'skillful' player may get into the car quite drunk, throwing whiskey bottles out of the window . . . . He wears very dark glasses so that it is obvious that he cannot see much, if anything. As soon as the car reaches high speed, he takes the steering wheel and throws it out of the window. If his opponent is watching he has won. If his opponent is not watching, he has a problem; likewise if both players try this strategy" (p. 11).

The final feature of Chicken to which it is worth drawing attention is the manner in which, if the game is repeated several times, "nothing succeeds like success". Put more concretely, there is a tendency for a player who wins a game of Chicken to be in a very strong position to win again if the game is reiterated. Conversely, it is very difficult for someone who has obtained the sucker's payoff in the past to get back on an even footing in the game. This characteristic of Chicken flows directly from the existence of two asymmetrical equilibria in the payoff matrix.

Turning now to the theory of N-person Prisoner's Dilemma, we enter for the first time the realm of multi-person games. The theory of N-person Prisoner's Dilemma is of much more recent origin than that of the other games discussed in this section; its origins are to be found in an important paper by Hamburger which appeared in 1973. Hamburger was not the first to use the term Prisoner's Dilemma in connection with multi-person games: he was anticipated by Rapoport (1960b); Pilisuk et al (1965); Bixenstine, Leavitt & Wilson (1966); Bixenstine & Douglas (1967); Olson (1968); Gallo, Funk & Levine (1969); Emshoff & Ackoff (1970); Hardin (1971); Kelley & Grzelak (1972); and Marwell & Schmitt (1972). Hamburger was, however, the first to provide a formal analysis of the structural similarities and dissimilarities between conventional two-person Prisoner's Dilemma and N-person Prisoner's Dilemma, and it is only since 1973 that the term N-person Prisoner's Dilemma has been widely used in the literature.

Hamburger (1973) started his analysis from the position that the following properties are sufficient and necessary to define a two-person, two-choice (2 x 2) game as Prisoner's Dilemma: (a) each player has a dominating strategy, and (b) these dominating strategies intersect in a Pareto-deficient equilibrium. He went on to draw attention to a class of multi-person games which possess analogous defining properties, and proposed that such games be designated N-person Prisoner's Dilemma.

The best known example derives from a point made by Lloyd (1833) in an essay on population and described by Hardin (1968) as "the tragedy of the commons". The examble is as follows. Ten people each own a 1,000-pound cow which grazes on a common pasture. The pasture can sustain an additional cow only at a loss: the weight of each cow would decrease to 900 pounds. Each individual faces a choice between adding a cow or not doing so. If one of the commoners decides to add a cow, he increases his personal wealth in livestock by 800 pounds, since he will then have two 900-pound cows instead of one 1,000-pound cow. But the collective wealth of the commoners would thereby be reduced from 10 x 1,000 = 10,000 pounds to 11 x 900 = 9,900 pounds. The dilemma arises from the fact that it is in each commoner's individual self-interest to add a cow to the pasture, but they would all be better off if none of them made this choice than if they all did so.

N-person Prisoner's Dilemmas are ubiquitous in political, social, economic, ecological and other social choice contexts. It has been used to model escape panics (Dawes, 1975), standing on tiptoe to watch a parade and stealing souvenirs from public places (Caldwell, 1976), pollution (Dawes, Delay & Chaplin, 1974), over-population (Kahan, 1974), the decision to join a union (Messick, 1973; Olson, 1968), and compliance with motoring speed restrictions to conserve fuel (Fox & Guyer, 1977).

A formalization of N-person Prisoner's Dilemma may be attempted as follows. Each of N players faces a choice between two strategies: C (Cooperate) and D (Defect). The outcome associated with the intersection of the N strategy choices is n, where n is the number of players who choose C (and it follows that N - n is the number of players who choose D). The payoffs may be described by the two functions C(n) and D(n), which are the payoffs to each of the players who choose C and those who choose D respectively when there are n who choose C. The two strategies open to each player in terms of the rules of the game are labelled C and D in such a way that $C(N) > D(0)$, i.e. each player receives a higher payoff when all players choose C than when all players choose D. Note that C(0) and D(N) are undefined.

The simplest possible example of a formally defined N-person Prisoner's Dilemma is one in which there are three players and the payoffs are represented by the integers 1, 2, 3 and 4. The payoff functions for this game are shown in Figure 1.8.

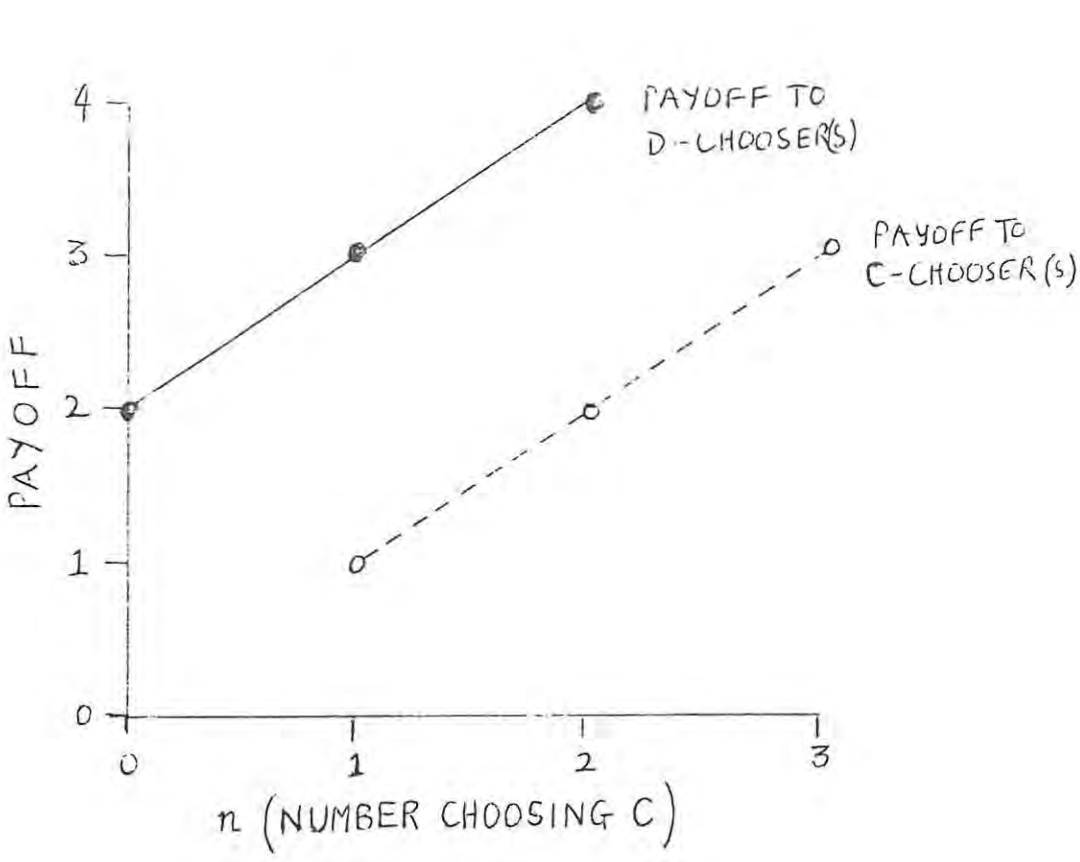


Figure 1.8. Payoff Functions for a Simple Three-Person Prisoner's Dilemma in which Payoffs are 1, 2, 3 and 4.

The formal relationship between this simple three-person Prisoner's Dilemma and the well-known two-person variety will become evident from an examination of Figure 1.9.

	B_1(C)	B_2(D)
A_1(C)	1.5,1.5	.5,2
A_2(D)	2, .5	1, 1

Figure 1.9. Two-person Prisoner's Dilemma of which the Game in Figure 1.8 is a Compound Version.

If each of three players simultaneously plays the two-person Prisoner's Dilemma Game shown in Figure 1.9 with each of the other two players, the resultant three-person game is the one shown in Figure 1.8. In each case, if all three players choose C, then each receive a payoff of 3; if two choose C, the C-choosers get 2, and the D-chooser gets 4; if one chooses C, he gets 1 while the D-choosers each get 3; finally if none of the players chooses C, they each get 2. The three-person game shown in Figure 1.8 may thus be considered a compound game based upon the two-person game shown in Figure 1.9.

An equivalent, though superficially quite different type of mathematical model can be used for conceptualizing N-person Prisoner's Dilemma. Each of several players (two players is a limiting case in this formalization) receives an amount c for choosing C or an amount d for choosing D. In addition, each player is fined an amount e for every player in the group who chooses D. In the example shown in Figure 1.8, $c = 3$, $d = 5$ and $e = 1$. In the two-person game shown in Figure 1.9, $c = 1.5$, $d = 3$ and $e = 1$. In the general case, $d - e = T$ (the temptation to be the sole defector in the group), $c = R$ (the reward for collective cooperation, $d - Ne = P$ (the punishment for collective defection) and $c - (N - 1)e = S$ (the sucker's payoff for being the sole cooperator. Many, though not all, two-person Prisoner's Dilemmas can be represented by this model; they are known technically as "separable" or "decomposable". All multi-person cases can, however, be so modelled.

In terms of this model, the general defining properties of Prisoner's Dilemma (two-person decomposable or multi-person) are given by the following inequalities:

$$(d - e) > c > (d - Ne) > c - (N - 1) e,$$

which are familiar in the limiting two-person case as $T > R > P > S$ (see above). Simplifying these inequalities, we arrive at

(i) $N > 1$ (the number of players must be two or more);

(ii) $c < d - e$; and

(iii) $d < c + Ne$.

Combining inequalities (ii) and (iii) according to conventional mathematical rules, we arrive at

(iv) $e < d - c < Ne$,

which may be regarded as a general fomrula for generating two-person (decomposable) or multi-person Prisoner's Dilemma Games. The possibility of finding such a general algebraic formula for Prisoner's Dilemma Games has apparently not been noticed before.

In the field of two-person mixed-motive and multi-person games, game theory lacks the formal prescriptive character of two-person zero-sum game theory. To many social psychologists this circumstance imbues these games with greater interest from the point of view of the empirical investigator than is present in two-person zero-sum games. The fact that formal solutions exist for the latter types of game should not, however, be taken as an indication that empirical research is without point in the two-person zero-sum case, but there are obviously rich possibilities in research on Prisoner's Dilemma (two-person and N-person) and Chicken. A review of relevant research in these areas is given in Chapter 2.

CHAPTER TWO

2. REVIEW OF RELEVANT LITERATURE AND OUTLINE OF EXPERIMENTS I - IV

2.1. Introductory Remarks

The publication in 1944 of von Neumann & Morgenstern's classic Theory of Games and Economic Behavior did not succeed in galvanizing experimental social psychologists and others who possessed the skills and resources for conducting empirical research on the behaviour of people in gaming situations into action. An explanation for the retardation in the development of experimental gaming could no doubt be attempted through an examination of the history of science and intellectual culture of the early post-war years, but this is not the place for such an excursion. One possible reason, however, is worth mentioning: von Neumann & Morgenstern's book must have appeared exceedingly dense and impenetrable to social psychologists of a generation ago, since it was evidently aimed primarily at mathematicians and mathematical economists. This, at least, is clear: virtually no reports of experimental games appeared in print until after the publication of Luce & Raiffa's (1957) Games and Decisions, a rather more approachable account of the leading ideas of the theory of games.

A few scattered experiments concerned with the behaviour of people in what amounts to gaming situations appeared between 1944 and 1957, but these experiments were not based on explicitly formulated gaming models and, with the exception of Flood (1952) and Simon (1956), there is no evidence that the researchers in question were familiar with the fundamental principles of game theory. The three most significant of these early experiments were those of Mintz (1951), Kelley (1953), and Sidowski, Wykoff & Tabory (1956). In Mintz's experiment, a number of subjects held strings to which cones were attached. The subjects were instructed to attempt to extract their cones from a narrow-necked bottle which was slowly filled with water from the bottom. Small monetary rewards were given to subjects who succeeded in removing their cones before they became wet. In general, subjects were successful only when the instructions stressed group solidarity rather than intra-group competition. The relevance of this experiment to the understanding of various forms of mob behaviour and panics has often been pointed out (e.g. Brown, 1965). Kelley reported an experiment which was conceptually similar to that of Mintz and found that increased threats of punishment for failure to escape from the situation led to a decrease in the number of subjects who succeeded in escaping. Sidowski, Wykoff and Tabory initiated a field of research on what they called the "minimal social situation". Two subjects, unaware of each other's presence, were placed in cubicles, and each was told to press either one of two buttons until instructed to desist. The pressing of the buttons resulted in either rewards (points) or punishments (electric shocks) for the other subject. The results indicated that subjects learn to cooperate in this situation despite their ignorance of each other's existence: there is a tendency towards stability of responses in which each subject rewards the other.

The publication of Luce and Raiffa's (1957) handbook was followed by a wave of experimental studies, most of which centred on the Prisoner's Dilemma Game, although zero-sum games also attracted some attention from empirical researchers in these early years. The first comprehensive review to appear was that of Rapoport & Orwant (1962) who outlined and commented upon the 30 gaming experiments which had by then found their way into print. Five of these games were two-person zero-sum games, eight were two-person mixed-motive games, seven were N-person games of various kinds, eight were games in which the subjects were given imperfect information about the payoff matrix, and two were simulation games. By 1965 the number of gaming experiments had begun to burgeon, and the editors of the *Journal of Conflict Resolution* decided to devote a special section in each issue to reports of such work. In the same year Gallo & McClintock (1965) published a fairly comprehensive review of published experiments, the bulk of which had by this time been devoted to the Prisoner's Dilemma Game. At least 13 reviews of this area have appeared since then. They are (in chronological order) by Becker & McClintock (1967); Plon (1967); Vinacke (1969); Swingle (1970b); Nemeth (1972); Wrightsman, O'Connor & Baker (1972); Deutsch (1973); Tedeschi, Schlenker & Bonoma (1973); Apfelbaum (1974); Davis, Loughlin & Komorita (1976); Pruitt & Kimmel (1977); Eiser (1978); Schlenker & Bonoma (1978); and Hamburger (1979). By the late 1970s well over 1,000 experimental gaming studies had been published, and there is no sign of any diminution in research activity in this area. It is no longer possible to write a comprehensive review of this body of work. The discussion which follows will therefore centre on work directly related to the experiments to be reported later in this dissertation: a more or less comprehensive review will be given of experiments on two-person zero-sum games and N-person Prisoner's Dilemma Games, and the most relevant findings on two-person mixed-motive games will be outlined without any attempt being made to cover all published work on these games.

2.2. Two-person Zero-sum Games

Comparatively few empirical investigators have devoted attention to two-person zero-sum games over the past three decades. There appear to be two main reasons for this state of affairs: (a) it is felt by many investigators that the existence of formal solutions to all two-person zero-sum games robs them of psychological interest; since a player's optimal strategy is always prescribed by the minimax rule, the only interest in empirical studies of behaviour in such games would derive from discovering the (apparently unenlightening) extent to which players conform to this prescription; and (b) most authorities (e.g. Rapoport, 1960a; Shubik, 1964) believe that nearly all real life conflicts are mixed-motive, hence the behaviour of players in artificial zero-sum conflict situations does not realistically model any aspect of everyday social life. Both of these arguments can, however, be challenged. It has been pointed out in Section 1.4 that the "optimal", "correct" or "rational" strategy, even in the simplest type of two-person zero-sum game (i.e. where a saddle-point exists) is undefined in cases where an opponent may be expected to choose "irrationally": a player can frequently do better by playing a non-minimax strategy against an opponent who is himself using a non-minimax strategy. In real life conflicts, peoples' behaviour is seldom completely rational and it is often unpredictable or systematically and predictably irrational. There is another comment worth making in this context: even the behaviour of people in the face of consistently rational opponents is of some empirical interest since the factors which influence their own rational or irrational choices in such circumstances may help to define the limits of human rationality. What little empirical evidence exists in this area certainly does not indicate that people invariably conform to the prescriptions of the minimax rule.

Turning to the second argument, about the unrealistic nature of zero-sum models, it must be admitted that most everyday conflicts are of the mixed-motive variety (see e.g. Gallo & McClintock, 1965); but there are many military situations (e.g. individual battles) and many economic, political and social conflicts which are probably best characterized by zero-sum games (Kahan & Rapoport, 1974). For all the reasons mentioned above, it may be strongly argued that the relative neglect of zero-sum games by empirical researchers is unwarranted.

Two-person zero-sum games fall into two classes: those that are "non-specially strictly determined" in von Neumann & Morgenstern's awkward terminology (i.e. those without saddle-points) and those that are "specially strictly determined" (saddle-point games). In the review which follows, experiments involving non-saddle-point games will be dealt with first before discussing the still less numerous experiments on saddle-point games. There will inevitably be some overlap, since investigators have occasionally used saddle-point and non-saddle-point games in the same experiment.

Atkinson & Suppes (1958) and Suppes & Atkinson (1960, Ch. 3) reported an experiment which ostensibly dealt with "an analysis of a zero-sum, two-person game situation in terms of statistical learning theory and game theory" (p. 377). In this experiment, 120 undergraduate subjects were randomly assigned to three treatment conditions. In each condition the subjects competed in pairs in a 2 x 2 zero-sum game which was reiterated over 200 trials. In each case, however, the subjects were ignorant of the payoff matrix, and they were not directly informed about their opponents' choices. The three conditions were labelled "Sure", "Pure" and "Mixed". In the "Sure" condition, a subject was confronted with a game in which there was a "sure thing" strategy available, i.e. a pure strategy which was at least as good as the other irrespective of the opponent's choice. In technical terminology, he had a strictly dominating strategy available. In the "Pure" condition, a saddle-point game was presented. Finally, in the "Mixed" condition, a game was used in which the prescribed minimax solution involved the use of a mixed strategy. The results showed that the minimax strategies were "not even crudely approximated by the observed means" (p. 374) in the "Sure" and "Pure" groups, and that the proportions observed in the "Mixed" group were also widely divergent from the minimax mixed strategies available.

Lacey & Pate (1960) reported two experiments involving 2 x 2 zero-sum games. In the first, six subjects played 240 reiterations of a non-saddle-point game against an experimenter who used either a minimax mixed strategy (Group I), or one of two non-minimax mixed strategies (Groups II and III) against them. In Group I, the subjects' choices closely approximated the minimax mixed strategy prescribed by game theory, but in Groups II and III there was some indication that they gradually learned to adopt non-minimax strategies which were more effective in exploiting the "non-optimal" behaviour of their opponents. In the second experiment, another six subjects played 360 reiterations of the same game, 240 trials against a non-human randomizing device followed by 120 trials against a live experimenter. In some cases the subject's opponent adopted a minimax mixed strategy and in others a non-minimax mixed strategy was presented. The results of this second experiment suggested that subjects were more successful at learning to exploit non-optimal play on the part of a non-human randomizing device than that of a human opponent. Both of Lacey & Pate's experiments were, however, flawed by the use of so few subjects (two subjects in each experimental condition) and the failure to provide any statistical analysis of the results. The second experiment was further flawed by the failure to counterbalance for order effects (the human opponent always followed the randomizing device).

Lieberman (1960a, see Lieberman, 1962) reported an experiment using a 2 x 2 zero-sum game without a saddle-point. Twenty undergraduate subjects played 300 reiterations of this game against an experimenter who either used a minimax mixed strategy over all 300 trials or used a minimax strategy for the first 100 trials only, thereafter switching to a non-minimax mixed strategy. Against a consistent minimax mixed strategy, none of the subjects played strategies even approximating to minimax. Against a non-minimaxing opponent, the subjects chose with increasing frequency the pure strategy which yielded the best expected value under the circumstances, but failed to exploit the weakness in the opponent's play fully. The behaviour of Lieberman's subjects when playing against a minimaxing opponent has often been interpreted not only by Lieberman but also by commentators and reviewers, as "irrational" or "non-optimal". Rapoport & Orwant (1962), for example, have this to say: "Certainly the behavior of (subjects confronted with a non-minimaxing opponent) was much more 'rational' than that of (subjects playing against a consistent minimaxer), both from the standpoint of guarding against large losses and (tentatively) attempting to increase their responses to an exploiting level" (p. 10). As has been pointed out in

Section 1.3 above, however, 2 x 2 zero-sum games without saddle-points have the property of yielding the same value to a player confronted with a minimaxing opponent whatever his own strategy choices. It is not, therefore, fair to consider Lieberman's subjects as having chosen irrationally.

Sakaguchi (1960) described two small experiments on two-person zero-sum non-saddle-point games. In the first, one pair of subjects played 50 reiterations of the game, which was not presented to the subjects in the usual normalized form by means of a payoff matrix, although each of the player's three strategies and the payoffs were well defined. There was evidence in the results that one of the players learned to adopt his minimax mixed strategy in the course of the experiment while the other did not. The second experiment made use of a conventional payoff matrix for presenting a 3 x 3 zero-sum game. One pair of players played 60 reiterations of this game. The mixed strategy of one of the players was extremely close to the prescribed minimax strategy, while the other subject deviated considerably from minimax. Not much importance can, of course, be attached to either of Sakaguchi's experiments on account of the small sample sizes used.

An experiment by Suppes & Atkinson (1960, Ch. 9) made use of a 2 x 2 zero-sum game with no saddle-point. Experimental group subjects were presented with a conventional matrix, while control subjects were simply shown a diagram which did not contain the payoff values used in the game. A total of 80 undergraduate subjects competed in pairs over 210 reiterations of the game. Subjects in the control group were found to conform quite closely to the predictions of stimulus sampling theory, while experimental subjects deviated from the stimulus sampling predictions, although not necessarily in the direction of the prescribed minimax mixed strategy. Neither of the groups, however, can be described as having conformed at all closely to the minimax rule.

Five 2 x 2 zero-sum games, one of which had a saddle-point, were used in an experiment by Kaufman & Becker (1961). The most unusual and interesting feature of Kaufman & Becker's methodology was the way in which the subjects were required to indicate their strategy choices: instead of simply selecting one pure strategy on each trial, the subjects were requested to indicate how they would divide 100 choices between the two available pure strategies in each case. Each of 20 female undergraduate subjects played 50 trials in this manner against the experimenter in a 5 x 5 latin square design replicated four times, with each subject participating in several games. The results indicated that the subjects differed markedly in their ability or willingness to adopt a minimax strategy. A majority of the subjects did, however, succeed in finding the minimax solution in at least one of the games. The greatest deviation from minimax on the part of the subjects was found in the saddle-point game. In the non-saddle-point games, it was found that the further the minimax solution deviated from a 50-50 pure strategy mixture, the less closely the subjects generally approached the prescribed minimax solution. Subjects appeared to rely more on experience than on analysis of the payoff matrices in improving their strategies over trials.

An experiment by Malcolm and Lieberman (1965) involved the use of a 2 x 2 zero-sum game without a saddle-point. The 18 male undergraduate subjects used in this experiment played against each other over 200 reiterations of the game. It is unfortunately not clear from the authors' brief description of the procedure how the game was presented to the subjects, but it may be guessed that it was presented in the conventional way by means of a payoff matrix. A slight tendency was found for the subjects as a group to approach the prescribed minimax mixed strategy over the 200 trials. On the last 25 trials, 10 of the 18 subjects were conforming more or less closely to minimax.

Four 5 x 5 zero-sum non-saddle-point games were used in an experiment by Payne (1965). The subjects were eight undergraduates, seven males and one female. Subjects competed in pairs over 200 reiterations of each of the four games in a design counterbalanced for order effects. A small though significant tendency was found for subjects to converge towards the prescribed minimax mixed strategies in all five games, although on some games this convergence was more pronounced than on others. No significant transfer of training from one game to another was found.

Kaufman & Lamb (1967) reported an experiment in which two 2 x 2 constant-sum non-saddle-point games were used. The subjects were eight male volunteers from a university summer session; they competed against each other using conventional payoff matrices. Each subject played 100 reiterations of each of the four games in a properly counterbalanced design. The main finding was that "under the conditions of the present experiment, players do not learn to play a game theory optimal strategy . . . . " (p. 958). An original and interesting feature of this experiment was that each subject was provided in some treatment conditions with a randomizing device which they could use to generate mixed strategies in any chosen proportions if they so wished. This device comprised a roulette-like wheel which could be partitioned by a slide into two parts in any proportions graduated from 0-100 before being spun. The evidence of Kaufman & Lamb's experiment indicated, however, that subjects "use the wheel as a probability-generating device less as they acquire practice with it" (p. 958), i.e. "They do not play any mixed strategy in the sense of choosing a given alternative randomly with a fixed probability" (p. 959).

In an experiment by Messick (1967), a 3 x 3 zero-sum game without a saddle-point was used. The game was presented to the subjects by means of a conventional payoff matrix. Forty-two subjects played 150 reiterations of this game against a computer which was programmed to adopt either a minimax mixed strategy or one of two non-minimax mixed strategies. Messick's interpretation of the results of his experiment were as follows: "In complete accord with previous research on the issues the study reported here unambiguously indicates that human Ss do not behave in a manner consistent with the minimax theory" (p. 46). In fact, Messick's elaborate and sophisticated analysis of his data revealed no significant tendency among the subjects to converge towards the prescribed minimax strategies in any of the treatment conditions, neither did they succeed in evolving maximally exploiting strategies against the computer when the latter played non-minimax strategies. These findings provide an important addendum to Lacey & Pate's flawed experiment (described above) which tended to suggest that subjects were successful in exploiting non-optimal play on the part of a non-human opponent. Messick appears, however, to have been ignorant of Lacey & Pate's research which anticipated his own.

Pate (1967) reported an experiment in which 18 male undergraduate subjects played a 2 x 2 non-saddle-point zero-sum game against either a human opponent or a randomizing device. The subjects were in all cases faced with a minimax mixed strategy on the part of their opponents. A conventional payoff matrix was used for presenting the game to the subjects, and 120 reiterations of the game were played. The subjects were, in addition, offered the opportunity of playing a further 120 reiterations. Since the value of Pate's game was negative (subjects could expect a payoff of -1/8 per trial), it may be designated a "losing game". Some of the subjects, but not all, recognized this feature of the game, and the subjects' acceptance or non-acceptance of the offer of continuing with further reiterations was closely related to their non-recognition or recognition respectively. The recognizers, however, did not produce strategy mixtures which were any closer to the prescribed minimax mixture than the non-recognizers. Furthermore, there was no evidence of convergence towards minimax on the part of the subjects over trials. As has been pointed out, however (see Section 1.3 above), in 2 x 2 zero-sum non-saddle-point games a player achieves the same payoff against a minimaxing opponent irrespective of what (pure or mixed) strategy he adopts: it follows that there was, in a sense, nothing for subjects in Pate's experiment to learn and therefore no rational reason for them to use a minimax strategy. Pate is evidently aware of this problem with his experiment.

A small experiment by Pate & Broughton (1970) investigated the behaviour of 15 subjects in a 2 x 2 zero-sum non-saddle-point game which had a positive value for the subjects. Independent groups of subjects (five in each treatment condition) played for valueless tokens (paper clips), imaginary money or real money (pennies). In each case, 240 reiterations of the game were played against an experimenter who used a non-minimax mixed strategy. The subjects could have exploited the experimenter's non-minimax strategy and maximized their own winnings by playing a pure strategy on every trial. The results showed a tendency on the part of the subjects to converge towards an exploitative pure strategy, but their choices in the final block of trials were still some way from this ideal. There was no significant difference between the behaviour of the subjects in the three incentive conditions.

Fox (1972) reported an experiment using a 2 x 2 zero-sum game without a saddle-point. The subjects (32 university undergraduates) played 200 reiterations of the game against a computer which was programmed either to play a minimax mixed strategy or a non-minimax mixed strategy. Against the non-minimaxer, the subjects had an exploiting strategy available which was in the opposite direction (i.e. away from a 50-50 alternation) from the minimax strategy; this enabled a clear separation to be made between tendencies towards minimax and tendencies towards pure strategy exploitation in the subjects' behaviour, in contrast to several of the experiments discussed above (e.g. Lieberman, 1960a, 1962). The results showed that the subjects tended to converge over trials towards an optimal exploiting strategy against a non-minimax opponent and towards a minimax strategy against a minimaxing opponent. This latter finding should be interpreted in the light of the fact that the players could not improve the expected value of their choices against the minimaxing opponent by themselves using a minimax strategy; it furthermore contradicts previous findings discussed above (Lieberman, 1960a, 1962; Messick, 1967; Pate, 1967).

In an experiment by Kahan & Goehring (1973), 36 subjects participated in one of two 2 x 2 zero-sum non-saddle-point games played against an experimenter. The experimenter either adopted a minimax mixed strategy throughout the 300 reiterations of each game, or played minimax for the first 100 trials before shifting to a 50-50 per cent non-minimax mixed strategy for the remaining trials, or played minimax for 100 trials followed by a non-minimax 20-80 per cent mixed strategy for the remaining trials. The experimenter's second strategy was designed to be to the advantage of subjects who failed to adopt the prescribed minimax mixed strategy in one of the games, and to be maximally exploitative against such behaviour in the other game. The results showed that when the opponent played a non-minimax strategy, subjects tended to detect this "nonoptimality" and exploit it to their own benefit. "When the opponent played according to the minimax prescription, subjects' performance was not optimal but was sufficiently and consistently close to it that arguments in terms of differences between perceived and objective probabilities provide an attractive explanation for the differences" (p. 27). Against a minimaxing opponent, there was a tendency (consistent with a previous finding of Lieberman's (1960a, 1962)) for subjects to underplay the prescribed minimax majority alternative.

Pate, Broughton, Hallman & Letterman (1974) reported three experiments involving a total of four 2 x 2 zero-sum non-saddle-point games. In all, 160 subjects took part in these experiments, each of which involved 240 reiterations of the games used. Subjects low in dogmatism were found to approach the minimax or exploiting strategies against minimax and non-minimax opponents' strategies respectively more closely than were subjects high in dogmatism. Two other personality variables (internal-external locus of control and a measure of willingness to have decisions made by a computer) were unrelated to the subjects' gaming strategies. The value of the game was found to be unrelated to the subjects' behaviour, "however, as in many previous studies, Ss did not adopt initially a minimax strategy nor did they approach closely a minimax strategy even after experience in the game-playing situation" (p. 510). Pate _et al._ were apparently not familiar with findings of some earlier investigators (discussed above) which do not agree with their own.

Before leaving two-person zero-sum saddle-point games, some brief mention is necessary of games of timing in pure conflict, particularly so-called silent duels. Games of timing are a class of two-person zero-sum games in which each player's set of strategies is infinitely large and comprises the set of real numbers between zero and unity. Unlike the more conventional matrix games, the problem facing the player is not _what_ action to take, but rather _when_ he should take action. The initial resources of the players are limited by the rules of the game and they are not necessarily equal: each player can make only a fixed number of decisions to take action within the time interval $0 \leqslant t \leqslant 1$. At $t = 0$ every attempt fails, at $t = 1$ every attempt succeeds, and at any other time there is a positive probability of success. The typical Western duel between two gunfighters walking closer and closer towards each other is a typical example of a game of timing. Two classes of games of timing have been distinguished (Karlin, 1959). In the first class are so-called noisy duels, which are games of perfect information: when either player acts, his action and its effects are immediately known to his opponent. The second class comprises silent duels in which perfect information is not present. In most discussions of games of timing to date, knowledge regarding a player's own and his opponent's initial resources is assumed given. Since two-person zero-sum games of perfect information always have saddle-points, some games of timing have pure minimax strategies and others have mixed minimax strategies available to the players.

A detailed review of experiments concerning the behaviour of subjects in silent and noisy duels would be out of place in the present context, but a brief comment is desirable for the sake of completeness. Four independent studies reporting the results of 34 separate duelling situations have been published (Kahan & Rapoport, 1974; Kahan & Rapoport, 1975; Rapoport, Kahan & Stein, 1973; and Rapoport, Kahan & Stein, 1976). The correlation between the observed mean firing times in these 34 duels and the 34 prescribed minimax strategies is .959 (Rapoport, Kahan & Stein, 1976). The authors interpret these findings to mean that game theory models are extremely good predictors of the behaviour of subjects in games of timing, both in their saddle-point and non-saddle-point forms, although the latter appear to be somewhat more difficult for the subjects.

Some two dozen experiments concerned with the behaviour of subjects in two-person zero-sum non-saddle-point games have been reviewed. Taken as a whole, the results can only be described as confusing and contradictory. In some of the experiments a tendency on the part of the subjects to converge towards the prescribed minimax mixed strategy has been reported, though it seems that in most of the better-controlled studies their strategies have deviated markedly from minimax especially when confronted with non-minimaxing opponents' strategies. More research is clearly needed in this area. Of particular interest would be studies (of which none have been reported to date) in which lifelike conflicts are presented to the subjects, rather than the highly abstract and artificial tasks used in the experiments under review. The artificiality of the experimental situations used in these experiments may indeed account, in part at least, for the apparent unpredictability of the subjects' behaviour.

Far fewer empirical studies have been reported concerning the behaviour of subjects in two-person zero-sum games with saddle-points than on the non-saddle-point variety. One reason for this state of affairs may be that the "correct" or "optimal" or "rational" strategy available to a player in these cases seems even more obvious than in non-saddle-point games, and, as has been pointed out earlier, this property of two-person zero-sum games is thought by some game theoreticians to rob them of psychological significance.

The experiment by Atkinson & Suppes (1958, Suppes & Atkinson, 1960, Ch. 3), which incorporated both saddle-point and non-saddle-point games has been discussed above, as has the mixed study of Kaufman & Becker (1961). Mention has also been made of several investigations of noisy duels, which may be regarded as saddle-point games (Kahan & Rapoport, 1974; Kahan & Rapoport, 1975). Only five other experiments on saddle-point games appear to have been published.

Lieberman (1959, 1962) used a very simple 2 x 2 zero-sum game with a saddle-point which was reiterated 200 times. The game was presented to the subjects in conventional matrix form. Of the 14 undergraduate subjects who participated in the experiment, 10 came to adopt the minimax pure strategy consistently and 4 exhibited behaviour similar but not identical to the prescribed pure minimax behaviour.

In a subsequent experiment with a slightly more complicated 3 x 3 zero-sum saddle-point game (Lieberman, 1960b) which was methodologically similar to his previous study, the following results emerged. About half of the 30 undergraduate subjects conformed to the minimax prescription 100 per cent of the time after between 10 and 125 trials, and on the final 20 trials 94 per cent of the subjects were making consistent minimax choices. Many of the non-minimax choices made by the subjects in the later trials of this experiment were, on their own accounts, motivated by a desire to alleviate the boredom of the task.

Morin (1960) presented 28 undergraduate subjects with 28 different 2 x 3 zero-sum games, all of which had saddle-points. In addition, the "correct" (Morin's term) strategy could be found in each case by eliminating strictly dominated strategies from both players' repertoires. Each subject made one choice on each of the 28 matrices. Out of 784 choices, 140 were "errors" (non-minimax choices). Morin noted that "errors" were made more often in games in which the average expected value (assuming random behaviour on the part of the opponent) of the non-minimax strategy was greater than that of the minimax strategy.

Suppes & Atkinson (1960, Ch. 6) reported an experiment involving two 2 x 4 zero-sum saddle-point games. The subjects (96 undergraduate students) were randomly assigned to one of two treatment conditions in which they competed in pairs: the two conditions involved slightly different though similar payoffs. In each case 200 reiterations of the game were played. The experiment was somewhat unusual (although similar to an experiment by the same investigators on non-saddle-point games described above) in that the subjects were not shown the payoff matrix and had to infer it as the experiment proceeded. They were, however, aware of each other's existence and of the nature of the task. The results were summarized as follows: "The observed asymptotic response probabilities . . . clearly show that the pure game-theory strategies are not even roughly approximated. There is not even any appreciable tendency for the observed probabilities to move away from the learning-theory predictions and toward the optimal game-theory strategies" (p. 151). In interpreting these results it should, however, be borne in mind that, on account of the subjects' ignorance of the payoff matrices, and other peculiarities in the design, this experiment was not concerned with gaming strategies in the strict sense.

Brayer (1964) presented 100 undergraduate subjects with 90 3 x 3 zero-sum games, all of which had saddle-points. Subjects played three trials on each game against an opponent who either chose randomly or else adhered consistently to the minimax strategy. Against the randomizing opponent, a subject's highest expected value was attainable in each case by consistently choosing one of his non-minimax pure strategies. The results showed that subjects adopted their minimax strategies .59 times on average (out of a possible maximum of 3) when playing against a randomizing opponent, compared with an average of 2.75 against a minimaxing opponent. A subsidiary finding of interest was that subjects used the minimax strategy more often when the absolute value of the game (from their point of view) was high than when it was low, although according to formal game theory the game's absolute value is irrelevant to the logic of the situation.

The findings on two-person zero-sum saddle-point games are evidently far less confusing and confused than those on non-saddle-point games reviewed earlier. The most striking feature of the findings is the failure of subjects in general to adhere to the minimax rule. That the subject's choices should not always be interpreted as "errors" in such cases is brought out most clearly in Brayer's (1964) experiment in which it was plainly in the rational self-interest of the subjects in one of the treatment conditions to use a non-minimax strategy. The experiment described below in Chapter 3 was motivated by a desire to explore this interesting and paradoxical problem further.

2.3. Two-person Mixed-motive Games

The overwhelming majority of empirical studies of the behaviour of people in gaming situations which have been published during the last three decades have been devoted to Prisoner's Dilemma and other two-person mixed-motive games. It is obviously impossible to provide an exhaustive review of these experiments: a bibliography of such studies compiled at the beginning of the 1970s (Wrightsman, O'Connor & Baker, 1972, pp. 285-341) already contained more than 1,000 items, and the body of published work has grown apace since then. The discussion which follows will therefore centre particularly on what appear to be the most significant and/or well-confirmed findings on Prisoner's Dilemma and Chicken games. A comprehensive review will, however, be given towards the end of this section of all studies which have a direct bearing on the experiments to be described in Chapters 4 and 5 below, namely studies involving lifelike Prisoner's Dilemma and Chicken gaming situations. Most of the studies which have been omitted from the discussion below are mentioned in one or more of the following reviews: Rapoport & Orwant (1962); Gallo & McClintock (1965); Becker & McClintock (1967); Plon (1967);

Vinacke (1969); Swingle (1970b); Nemeth (1972); Wrightsman, O'Connor & Baker (1972); Deutsch (1973); Tedeschi, Schlenker & Bonoma (1973); Apfelbaum (1974); Davis, Loughlin & Komorita (1976); Pruitt & Kimmel (1977); Eiser (1978); Schlenker & Bonoma (1978); and Hamburger (1979). The discussion which follows will deal with the following: general findings; effects of opponents' strategies; effects variations in the payoff structure; incentives and motivational orientation; personality and group differences; and lifelike versus abstract decision contexts.

The most striking general finding regarding Prisoner's Dilemma behaviour is undoubtedly the so-called "lock-in" effect: when pairs of players play a large number of reiterations of the game, there is a tendency for long series of DD (mutually defecting or competitive) choices to predominate. Luce & Raiffa (1957) predicted that reiterations of Prisoner's Dilemma would lead to joint cooperation on the part of the players: "We feel that in most cases an unarticulated collusion between the players will develop . . . . This arises from the knowledge that the situation will be repeated and that reprisals are possible" (p. 101). A year after the publication of Luce & Raiffa's book, however, Flood (1958) reported that the DD lock-in is the usual occurrence. This finding was replicated by Scodel, Minas, Ratoosh & Lipez (1959) and has since then been confirmed many times. The consequence of a DD lock-in is, of course, that the players are unable to achieve their collective best payoffs.

Some illumination of the DD lock-in effect has been provided by studies in which the game has been reiterated literally hundreds of times. The most detailed and thorough study of this kind is undoubtedly the work of Rapoport & Chammah (1965); these researchers were the first to map the long term time courses of cooperative and competitive choices in Prisoner's Dilemma Games. Their results, which have been confirmed by several subsequent investigators, showed that three phases typically occur in a long series of reiterations of Prisoner's Dilemma. On the first trial, the prepartion of cooperative (C) responses is typically slightly greater than .5, but this is followed by a rapid decline in the frequency of C choices (a "sobering period"). After 50 to 150 reiterations approximately, cooperative responses begin to increase slowly in frequency (a "recovery period"), usually reaching a proportion in excess of .5 by trial 300.

The initially moderately high proportion of C responses has been interpreted variously as indicating an initial reservoir of goodwill or simply a lack of comprehension on the part of the subjects of the strategic structure of the game. The sobering period may consequently reflect a decline in trust and trustworthiness, an increase in competitiveness or merely a dawning of understanding of the payoff matrix. The recovery period can be interpreted relatively unambiguously: it probably reflects the growth of an "unarticulated collusion" between the players such as was predicted by Luce & Raiffa (1957).

Another set of general findings concerns the conditions under which the games are played. A number of studies have, in particular, investigated the effects of possibilities for communication between the players on levels of cooperation in Prisoner's Dilemma and related games. In everyday mixed-motive situations, explicit communication is often possible: threats, promises, commitments etc. are frequently given voice, although of course they are often unenforceable. Common sense strongly suggests that explicit communication between players should enhance their ability to arrive at agreements of joint cooperation.

One of the best known experiments in this area is that of Evans (1964). This study investigated Prisoner's Dilemma behaviour under three conditions: enforceable promises (stiff penalties were introduced for breaking promises); unenforceable promises (explicit communication allowed but no penalties for reneging) and no promises. As expected, the highest level of cooperation was found in the enforceable promises condition and the lowest in the no promises condition.

Rapoport, Chammah, Dwyer & Gyr (1962) reported an experiment in which subjects played Prisoner's Dilemma for two or three hours (300 to 500 trials). A rest session followed, during which they ate and rested for between one and two hours. After the break, they resumed playing for a total of 1,200 trials. The proportion of cooperative choices was significantly higher in the second session compared with the first; the authors suggest that subjects took advantage of the rest period to decide on mutually cooperative strategies.

There is evidence, however, that the opportunity for communication in Prisoner's Dilemma tends to increase cooperation only when the subjects have an individualistic motivational set. Deutsch (1958) reported that such communication opportunities make little difference when the subjects are under competitive or cooperative instructions. In the latter two conditions, the indicated strategies are obviously D and C respectively; but as Nemeth (1972) has explained, "it is in the condition where subjects are instructed to think of themselves that the mix of motives and possible conflict come into play" (p. 217). Verbal communication does not always and inevitably lead to increased cooperation. Terhune (1968) has observed that "communication provides greater opportunity for cooperation, but that opportunity may either not be used, ineptly used, or used for deceit and vituperation" (p. 22).

In an unusually broad study of communication, Wichman (1970) allowed subjects either to see or hear their partners, both to see and hear them, or neither to see nor hear them. Results indicated that subjects were most cooperative in the see-and-hear condition, less cooperative in the hear-only condition, less cooperative still in the see-only condition, and least cooperative in the isolated condition. It was only in the see-and-hear condition that subjects succeeded in attaining and maintaining high and stable proportions of joint cooperative choices over the series of 70 trials. The mechanisms by which opportunities for communication lead to increased possibilities of cooperation are therefore evidently rather subtle.

The effects of opponents' strategies on cooperative behaviour in Prisoner's Dilemma and other mixed-motive games have been discussed in detail in a specialized review article by Oskamp (1971). Experiments in this area involve pitting subjects against a pre-programmed sequence of choices on the part of a human opponent or a computer in order to see how they respond to such controlled conditions. Two extreme opponents' strategies are 100 per cent C choices (unconditional cooperation) and 100 per cent D choices (unconditional defection or competition). In Prisoner's Dilemma, unconditional cooperation has been found to elicit higher levels of cooperation from subjects than has unconditional competition; the latter forces the subjects to select their D responses in self-defense. The difference is reversed in Chicken, since in this game the subject is obliged to cooperate in self-defense against an unconditional defector (Sermat, 1967).

Other studies of the effects of opponent's strategy on cooperation in Chicken have confirmed the above finding (see Oskamp, 1971). No Chicken studies using randomized opponents' strategies less extreme than 100 per cent cooperation or 0 per cent cooperation have however yielded significant differences. One finding apparently peculiar to Chicken was that of Sermat &

Gregovich (1966) who showed that either a CC or a DD first trial outcome is conducive to cooperation on the part of subjects in later trials.

In Prisoner's Dilemma studies, opponents' strategies less extreme than 100 per cent or 0 per cent cooperation have frequently yielded significant differences. These results have generally been in the same direction as the effects of the pure strategy studies when fairly extreme levels of randomized cooperation have been used, but studies using less extreme levels of cooperation have often yielded non-significant results. Significant differences have been shown for 80 per cent C vs. 20 per cent C (Heller, 1967; Knapp & Podell, 1968); 100 per cent C vs. 20 per cent C (Lave, 1965); 50 per cent C vs. 10 per cent C (Gahagan, Long & Horai, 1969).

In some experiments on Prisoner's Dilemma, the opponent has used a tit-for-tat (TFT) strategy: this amounts to choosing a C on each trial following a C choice from the subject, and choosing a D response immediately after each D choice on the part of the subject. Several studies have shown that TFT strategies elicit about the same or less cooperation from subjects than 100 per cent C (e.g. Crumbaugh & Evans, 1967; Wilson, 1969) but more than 0 per cent C (e.g. Oskamp & Perlman, 1965; Wilson, 1969). Several studies have, however, shown that a TFT strategy elicits greater cooperation from subjects than is found in free-play situations (Pilisuk, Skolnick & Overstreet, 1968; Pilisuk & Skolnick, 1968; Oskamp, 1970). Finally, mention should be made of studies on the effects of changes in strategy on subjects' choices. The most significant effects arise from so-called "reformed sinner" strategies in which the opponent is initially competitive, then switches to unconditional cooperation, and finally to TFT. "Lapsed saint" strategies are unconditionally cooperative and later TFT. A number of investigators (e.g. Harford & Hill, 1967; Harford & Solomon, 1967) have shown that "lapsed saint" opponents fail to elicit cooperation from subjects but that "reformed sinners" do elicit cooperation lasting many trials.

Turning now to studies investigating the effects of variations of the payoff structure on cooperative behaviour in mixed-motive games, three classes of studies have been distinguished by Wrightsman, O'Connor & Baker (1972, Section 2, Ch. 4): those involving manipulations of the discrepancy between T (temptation to defect) and S (sucker's payoff for sole cooperation) or between R (reward for joint cooperation) and P (punishment for joint defection); those involving the addition or subtraction of a constant from the matrix elements; and those involving the multiplication of each matrix element by a constant.

Studies in the first category have generated results which are generally in line with expectations based upon theoretical considerations and common sense assumptions. Subjects have usually been shown to be quite sensitive to variations in T, R, P and S. When temptation is increased relative to the other possible payoffs, in particular, defection on the part of the subjects tends to increase and cooperation decreases. Some representative experiments are discussed below, beginning with four experiments on Prisoner's Dilemma.

In an experiment by Komorita & Mechling (1967) subjects were first induced to adopt a cooperative orientation and were then led to believe they had been betrayed. The dependent variable was the number of trials it took the subject to return to a cooperative choice. The sucker's payoff (the subject's loss as a consequence of betrayal) and the temptation (the opponent's gain through unilateral defection) were systematically manipulated. Both independent variables were found to have a significant effect: when temptation was high, and when the sucker's payoff were low, trials to reconciliation were greater than when these relations were reversed.

Aranoff & Tedeschi (1968) manipulated the "intensity of conflict" in the payoff structure of a Prisoner's Dilemma Game. In the low intensity condition, the temptation was relatively low and the sucker's payoff relatively high compared with the high intensity condition. High intensity of conflict led to more DD outcomes, more defections, fewer CC outcomes and fewer CD outcomes than did the low intensity of conflict matrix.

A simple manipulation of the T-S difference was used in a study by Terhune (1968). Terhune used Rapoport & Chammah's (1965) index of cooperation, given by R-P/T-S. As predicted, the cooperation index was predictive of CC (mutually cooperative) choices on the part of the subjects: the higher the cooperation index, the greater the number of CC outcomes. The effect of the cooperation index on DD choices was less clear-cut.

Fisher & Smith (1969) reported a study similar to that of Aranoff & Tedeschi (1968) discussed above. In this experiment the values of R and P was held constant while T-S was either relatively small (low intensity of conflict) or great (high intensity of conflict). Consistent with Aranoff and Tedeschi's findings, these workers reported greater cooperation in the former case compared with the latter.

Minas, Scodel, Marlowe & Rawson (1960) compared the CC and DD responses of subjects in a Prisoner's Dilemma Game with that in two games (among others) in which the payoffs resulted in Chicken Games. The first main finding was that fewer CC choices and more DD outcomes resulted in the Prisoner's Dilemma than in either of the Chicken games. Secondly, the more dangerous of the two Chicken games (P alone was made more negative in this game) resulted in fewer DD outcomes and more mixed (CD or DC) outcomes than the less dangerous Chicken game.

Rapoport & Chammah (1965) used five Chicken matrices, varying the value of P from -3 to -40 to make the game more or less dangerous. The less dangerous games generated more cooperative choices in the subjects than the more dangerous games.

The most thorough investigation of variations in relative payoff values in two-person mixed-motive games was reported by Steele & Tedeschi (1967). Fifteen games, 12 of which were Prisoner's Dilemmas, were used. In all cases, R > P and T > S. Using a version of Rapoport & Chammah's (1965) cooperation index mentioned above, a correlation between expected and observed proportions of D choices across matrices was found to be .641.

Ells & Sermat (1968) used four different payoff matrices, one of which was Chicken and another Prisoner's Dilemma. As expected, the Prisoner's Dilemma yielded the lowest level of cooperation; the proportion of cooperative choices in the Chicken Game was considerably higher.

The results reviewed above are fairly consistent. The crucial property of Chicken games, namely necessity to expose onesself to great danger to achieve the maximax payoff, leads to higher levels of cooperation in Chicken than in Prisoner's Dilemma. The greater the danger in a Chicken game (the worse the P payoff), the less often it results, i.e. the more the players cooperate to avoid it. Manipulations of the relative values of T, R, P and S in both Prisoner's Dilemma and Chicken games has repeatedly been shown to produce exactly the sorts of results which one might expect on purely theoretical grounds.

Studies involving linear transformations of the matrix elements in two-person mixed-motive games, i.e. studies in which a constant is added to or subtracted from the matrix elements or where the latter are multiplied by a constant has led to confusing results. Assuming only that utilities are a linear function of payoffs, there are no logical reasons why such transformations should produce any differences in the strategy choices of the players: such transformations leave the strategic structure of the games unaltered.

Some researchers have indeed reported no significant effect of linear transformations of payoff values, as predicted by game theory. Most (of those that have found their way into print) have, however, reported significant linear transformation effects. An example of the former is a study by Dolbear & Lave (1966), who investigated the behaviour of subjects in three Prisoner's Dilemma Games which were derivable from each other by linear transformations (adding or subtracting a constant to the matrix elements). The subjects were confronted with an opponent's strategy of 12 D choices followed by 13 C choices. Results for the last 12 trials were not reported by the authors, but on trials 1-13 marginally significant differences were observed between the three treatment conditions, with greatest cooperation in the game with the lowest payoffs and least cooperation in the game with the highest payoffs.

Oskamp & Perlman (1965) altered two Prisoner's Dilemma matrices by subtracting a constant from each element. They predicted that the matrix with lower payoffs would produce higher levels of cooperation than the other, but their findings showed a significant tendency in the opposite direction. The authors concluded that the possibility of greater gain in the first matrix had a salutory effect on cooperation.

The experiments summarized above (and others reviewed by Wrightsman, O'Connor & Baker, 1972, pp. 50-65) have produced contradictory results. The only reasonable conclusion regarding the effects of linear transformations of the matrix elements in two-person mixed-motive games seems to be that further research is needed.

Although they are not strictly concerned with variations in the payoff structure of games, the effects of so-called "decomposed" modes of displaying the payoff matrix on subjects' strategy choices should be discussed briefly for the sake of completeness. A normalized Prisoner's Dilemma or Chicken game may be decomposed in the manner shown in Figure 2.1.

	B_1	B_2
A_1	12,12	0,18
A_2	18, 0	6, 6

	Your Gains	Other's Gains
C	6	6
D	12	- 6

	Your Gains	Other's Gains
C	0	12
D	6	0

Figure 2.1. A Normalized Prisoner's Dilemma Game and Two Derivative Decompositions.

Figure 2.1 shows a normalized Prisoner's Dilemma Game and two of its derivative decompositions. In a decomposition, each player receives the same matrix (such as one of the decomposed matrices shown in the figure) and knows that the other player has the identical matrix before him. As in the normalized form, each player is required to specify a choice (C or D in the examples given). In calculating his payoff, a player must take into account how much he has given himself and how much the other player has given him. Although the reward structures of the decompositions shown in Figure 2.1 are mathematically identical to the normalized version of the game, they may be psychologically quite different.

Several studies (e.g. Evans & Crumbaugh, 1966; Pruitt, 1967) have shown that subjects are unusually cooperative in decompositions of Prisoner's Dilemma in which each player's best outcome results from the other's cooperation. Pruitt & Kimmel (1977) ~~has~~ suggested that this form of display may emphasize a player's dependence on the other's willingness to cooperate "and thus facilitates development of the mutual cooperation goal" (p. 378).

More recent evidence (Pruitt, 1970; Tognoli, 1975) has shown that, in decompositions of Prisoner's Dilemma which are known to induce cooperative behaviour in players, they are especially quick to reciprocate cooperation from each other and especially slow to respond defensively to each other's non-cooperation.

Closely related to variations in the payoff structure as an independent variable in two-person mixed-motive gaming research are the effects of incentives and the motivational orientation of the subjects. In their early reviews of mixed-motive gaming research, Gallo & McClintock (1965) lamented the lack of studies using real meaningful rewards associated with the payoffs in the games used. In general, subjects in the early experiments had either played for points or for extremely small monetary gains. Gallo & McClintock speculated towards the end of their review that the low levels of cooperation generally found in research on Prisoner's Dilemma games may arise from the fact that the incentives were so limited. They implied that the subjects may have introduced extraneous utilities into the experimental situations in order to relieve the monotony of the tasks, thereby transforming the strategic structure of the games: "In effect, the S changes the game from a non-zero-sum game to a zero-sum game" (p. 76).

Gallo's own doctoral dissertation (Gallo, 1963) which later appeared in the form of a journal article (Gallo, 1966) appears to have been the first empirical study to attack the problem of incentives directly. In this influential study, powerful incentive effects were found, but since Gallo used a trucking game whose strategic structure is quite different from both Prisoner's Dilemma and Chicken, his findings are not of direct relevance to this review. Subsequent research has generated conflicting evidence on this question: the four most frequently cited experiments which found incentive effects and the five best known experiments which did not will be outlined below.

Evans (1964) manipulated the incentives associated with the payoffs in a Prisoner's Dilemma Game by instructing half the undergraduate subjects that they were playing for imaginary money and by instructing the other half that they were playing for points which would contribute towards their grades on the course. This manipulation was apparently highly effective in inducing motivation to maximize winnings in the latter treatment condition, but no significant effects were found on the strategy choices of the subjects.

Wrightsman (1966) reported two experiments which confirmed Evans's (1964) finding that reward has little effect on the subjects' gaming strategies. In each experiment, half the subjects were told that they were playing "for fun"; the other half played for points which they were told would be converted into substantial monetary rewards. The game used in both experiments was a Prisoner's Dilemma. In neither experiment was any significant incentive effect found.

Sermat (1967) reported one of the rare experiments in this field in which a Chicken Game was used. Subjects were instructed either that they were playing for points or that they were playing for payoffs in real money. No differences in their levels of cooperation were observed.

Turning now to experiments in which incentive effects were found, Radlow, Weidner & Hurst (1968) used a Prisoner's Dilemma Game and assigned subjects randomly to two conditions: in the imaginary money condition they were instructed to play as if they were playing for real money, and in the real money condition they were told that they would be paid whatever they won on one randomly selected trial out of the 98 reiterations of the game. Subjects in the real money condition were found to make significantly more cooperative choices than subjects in the imaginary money condition. This finding only held, however, when the subjects had been given a competitive orientation in the instructions at the start of the experiment; those given a cooperative orientation to start with did not manifest any significant incentive effects, although the tendency was in the same direction in this case.

Gumpert, Deutsch & Epstein (1969) reported an experiment on incentive effects in the Prisoner's Dilemma Game in which significant results were obtained in the opposite direction from those normally anticipated by other researchers. Subjects in this experiment were randomly assigned to conditions in which they played either for imaginary money or for real money, in which the incentives varied from very small to very large. The results indicated that the level of reward in the real money conditions did not significantly affect the cooperative choices of the subjects, but that subjects in the imaginary money condition were significantly more cooperative than those in the real money conditions.

Stahelski & Kelley (1969) found incentive effects in a Prisoner's Dilemma Game in spite of the fact that very small rewards were used. Subjects were either instructed that they were playing for points or that they were playing for small monetary gains. Monetary payoffs were found to lead to greater cooperation on the part of the subjects than were points.

Gallo & Sheposh (1971) reported two replications of Gumpert, Deutsch & Epstein's (1969) experiment, eliminating certain design flaws, and obtained results in the opposite direction. In the pooled results of both experiments, real money incentives were found to elicit significantly higher levels of cooperation from the subjects than were imaginary incentives.

As Schlenker & Bonoma (1978) have pointed out, "one cannot simply count the number of studies for and against each possibility and draw a conclusion, since there are a substantial number supporting each position" (p. 17). There is a strong possibility that the presence or absence of real incentives (and possibly their absolute or relative levels as well) interact with other unknown variables in determining whether or not they affect subjects' strategy choices in two-person mixed-motive games. Until such variables are discovered, the only reasonable interpretation of the findings in this area is that incentives may influence subjects' strategy choices.

Regarding personality and group differences in the behaviour of subjects in mixed-motive gaming situations, the most interesting and well researched independent variable has been the sex variable. Before the publication of Rapoport & Chammah's (1965) major investigation of behaviour in Prisoner's Dilemma Games, five studies had reported a failure to find any relationship between the sex of the players and their strategic choices (Marlow, 1959; Lutzker, 1960; Minas, Scodel, Marlowe & Rawson, 1960; Wilson & Bixenstine, 1962; and Bixenstine, Potash & Wilson, 1963), and two had reported a tendency in Prisoner's Dilemma for women to respond somewhat less cooperatively than men (Bixenstine & Wilson, 1963; Bixenstine, Chambers & Wilson, 1964). Rapoport & Chammah subsequently established that sex differences are often extremely pronounced (women typically displaying <u>less</u> cooperative behaviour than men) in Prisoner's Dilemma, Chicken and related two-person mixed-motive games, but (a) these differences are not apparent in short runs; they only emerge when the games are reiterated many times; and (b) they are found only in same-sex dyads; in male-female dyads the sex effect appears to vanish. Short runs do not elicit sex differences because the initial propensity to cooperate is similar in men and women, and the failure to find sex differences in long runs with mixed-sex dyads appears, in the case of Prisoner's Dilemma, to arise from the fact that the strategic structure of the game encourages players to become very similar to each other in their choice behaviour in order to minimize losses. In long runs (300 reiterations) with same-sex dyads, Rapoport & Chammah reported that men consistently chose cooperatively about twice as often as women. Although the results are not entirely consistent, most recent experiments have confirmed Rapoport & Chammah's findings (e.g. Bedell & Sistrunk, 1973; Black & Higbee, 1973; Conrath, 1972; Hottes & Kahn, 1974; McNeel, McClintock & Nuttin, 1972; Miller & Pyke, 1973; Wiley, 1973; Wyer & Malinowski, 1972).

These sex differences present the reviewer with a problem of interpretation. In the first place, nearly all the research on this question has been restricted to American college students: other cultures and sub-cultures need to be investigated before any conclusions can be reached regarding the cultural universality or specificity of the effect. Secondly, what precisely is implied by the sex difference? It is customary to refer to A_2 or B_2 choices in Prisoner's Dilemma and Chicken games as "competitive" or "non-cooperative" choices. As a result, many commentators have expressed their surprise regarding the greater competitiveness in women implied by research findings on these games. Terhune (1970) has however pointed out that "people who conflict are not necessarily bellicose. On the contrary, those with the best of intentions may find themselves, through ineptitude or defensiveness, locked in conflict with others" (pp. 214-215). The implication is that women, when placed in a vulnerable position (e.g. by being exploited by a defecting choice from the other player in a Prisoner's Dilemma or Chicken game) react with greater retaliation and apparent vindictiveness than do men. This is essentially the interpretation of Bixenstine, Chambers & Wilson (1964). The greater apparent competitiveness of women as compared with men may therefore be interpreted as a reaction to conquest: as a consequence of male dominance in American culture, they feel themselves to be more vulnerable to exploitation than do men, and they respond defensively especially when betrayed in mixed-motive games.

With respect to personality differences and their effects on mixed-motive gaming behaviour, two excellent specialized reviews of the literature up until a few years ago are available by Baxter (1972) and Terhune (1970). Several dozen potential personality characteristics have been investigated in this regard, but on the whole the results have shown either inconclusive tendencies or no effects whatever.

One of the most influential early experiments in this area was that of Deutsch (1960h). Deutsch's subjects played two trials on a Prisoner's Dilemma Game, the first under normal conditions and the second after being promised a cooperative response from the other player. Subjects who were "trusting" on the first trial turned out usually to be "trustworthy" on the second, while those who were "suspicious" on the first trial were usually "exploitative" or "untrustworthy" on the second. All the subjects had previously been given an F scale to fill in. Authoritarian personalities turned out to be more "exploitative" and less "trustworthy" than non-authoritarians. Non-authoritarians tended to be "trusting" and "trustworthy". Several commentators have taken issue with Deutsch's interpretation of A_1 choices on the first trial as "trusting", of A_1 choices on the second trial as "trustworthy", and of A_2 choices as "suspicious" and "exploitative" or "untrustworthy" respectively on the second trial. His results were, nevetheless, impressive. Several studies (reviewed by Baxter,1972 and Terhune, 1970) have confirmed the relationship between authoritarianism and Prisoner's Dilemma choice behaviour, but unfortunately others, e.g. Gahagan, Horai, Berger & Tedeschi, (1967) have not. The same inconclusive picture emerges from most other personality variables which have been investigated in Prisoner's Dilemma and Chicken games (see reviews by Baxter and Terhune cited above).

Baxter (1972) has outlined several possible reactions to the lack of consistent findings regarding personality and mixed-motive gaming behaviour. Firstly, it has been argued (e.g. Vinacke, 1969) that "when the game presents very limited and formal conditions, individual differences among subjects have little scope to manifest themselves" (p. 40). Secondly, it may be the case that there are important personality factors to be found, but they are not the ones typically measured by well known personality scales used in most studies to date. A third reaction is to argue that personality characteristics interact with the structure of the game at first but they are "washed out" by the spiral of conflict in reiterations (Horai & Tedeschi, 1969). Fourthly it has been argued (e.g. Rapoport & Chammah, 1965) that crude measures of the number of cooperative choices made by subjects in mixed-motive games conceal the enormous complexity of their responses; only by means of more sophisticated analyses of subjects response protocols are subtle relationships (e.g. with personality variables) likely to emerge. The fact of the matter is that it is simply not known whether and to what extent personality factors are implicated in two-person mixed-motive gaming behaviour, although common sense strongly suggests that they are likely to be implicated in some way or other. This conclusion is consistent with the stance taken by Harris in his article on experimental games as a tool for personality research (Harris, 1971).

During the 1970s, several commentators have drawn attention to the vexed question of ecological validity in relation to experimental gaming research in general, and to research on two-person mixed-motive games in particular. Before reviewing exhaustively and in some detail the handful of experiments which have been specifically designed to throw light on this question, it is desirable at this point to outline some of the major theoretical arguments on both sides of this debate.

After their comprehensive survey of the experimental literature, Wrightsman, O'Connor & Baker (1972) had this to say: "What surprises us most, in our review of the research, is that apparently no studies have compared degree of cooperative behaviour in a laboratory mixed-motive game with cooperation in different real-world tasks. While artificiality can also be assessed through laboratory manipulations, comparisons of cooperative behaviour across settings should be undertaken" (p. 277). One of the chief reasons for this lack of studies comparing the behaviour of subjects in abstract, usually matrix-type gaming situations with their behaviour in lifelike situations with comparable strategic properties was undoubtedly the position on this issue taken by Anatol Rapoport. Rapoport has been uniquely influential on account of the numerous empirical studies he has published, his several books and review articles concerned with experimental gaming and, above all, his position since its inception until recently as the editor of the gaming section of the <u>Journal of Conflict Resolution</u>. He has consistently argued against attempts to generalize the findings of experimental gaming research beyond the abstract and idealized situations normally found in laboratory games, believing that the same laws do not govern "both the events in the laboratory and those of the cosmos" (Rapoport, 1970b, p. 40). He believes that the behaviour of subjects in abstract laboratory games is sufficiently interesting and important in itself to warrant research without seeking to illuminate the behaviour of subjects in analogous "real-life" situations in the "cosmos".

An increasing number of reviewers have in recent years expressed their dissatisfaction with the position adopted on this issue by Rapoport. One of the most articulate and strident critics of the viewpoint implied by Rapoport's remarks has been Nemeth (1972). Nemeth has mounted a strident critique of experimental gaming research in general and experiments on Prisoner's Dilemma in particular, on the grounds that the artificiality of the task situations typically used are too far removed from real-life mixed-motive situations. She evidently believes that the potential interest in such research can only derive from the illumination it might cast on non-laboratory mixed-motive interactions between people. She has argued, furthermore, that the findings on Prisoner's Dilemma are therefore suspect: "My belief is that the seemingly irrational behaviour of subjects in a Prisoner's Dilemma game is due primarily to the essential incomprehensibility of the situation in which the subject is placed" (p. 213). This ambiguity, she believes, stems mainly from the inability of subjects in experimental games to communicate with each other.

Pruitt & Kimmel (1977) evidently tend to side with Nemeth rather than with Rapoport on this issue, believing as they do "that there is continuity between the laboratory and the real world" (p. 367). They are in favour of attempts to generalize from the laboratory to everyday mixed-motive situations, although they acknowledge that there are severe technical and theoretical problems in this regard: "We believe it is preferable for researchers to try to generalize their findings, because an analysis of limitations to plausible generalization can stimulate hypothesis building and the development of new research tasks" (p. 368). One cannot help being struck, however, as were Wrightsman, O'Connor & Baker (1972) by the neglect of researchers to investigate the possibilities of developing such "new research tasks".

Tedeschi, Schlenker & Bonoma (1973), in their book, *Conflict, Power and Games*, state flatly that "criteria for the assessment of ecological validity are nonexistent for experimental games" (p. 202). They go on to argue as follows: "Games represent highly artificial part-replicas of conflict situations and sometimes make what would be patently absurd assumptions if the real-world analogue were used as the criterion of judgment" (loc.cit.). They apparently regard it as desirable that generalizations to "real-world" situations could be made, since they go on to say: "No generalizations about social phenomena based exclusively on laboratory experiments could be safely assumed to be applicable in natural social environments without further inquiry" (p. 203). The key phrase in the above quotation is "without further inquiry". What the authors had in mind is perhaps revealed by a comment made by Schlenker & Bonoma (1978) in a review article published five years later. After repeating that "games do not provide a direct analogue to the real world situations to which generalizations are made . . . . By definition, an analogy is different from the real thing - - otherwise it would not be an analogy but would be the thing itself" (p. 21), they had this to say: "theoretical considerations determine judgments about generalizability, and boundary experiments can be performed to assess the limits of the hypotheses" (p. 33). Schlenker & Bonoma's suggestion concerning "boundary experiments" are very much to the point, but, as will be seen, very few researchers have turned their hands to such investigations. The experiments to be described in Chapters 3, 4, 5 and 6 were, however, motivated by precisely these considerations.

Apart from a study by Alcock & Mansell (1977), which was concerned with N-person Prisoner's Dilemma and will be discussed in detail below, only four studies appear to have been published in which an attempt has been made to compare the behaviour of subjects in abstract experimental games and in more lifelike decision context having similar strategic characteristics.

Orwant & Orwant (1970) reported an experiment with which Wrightsman, O'Connor & Baker (1972) were evidently not familiar (see their comments quoted above and their bibliography (pp. 285-341)).

Orwant & Orwant (1970) compared the strategy choices of 165 students of journalism on either 10 conventional payoff matrices, each of which possessed the strategic structure of the Prisoner's Dilemma or 10 "interpreted" versions of these matrices. One choice was made by each subject on each of the matrix or "interpreted" games. The major finding was that "the interpreted version[s] elicited significantly more cooperation than the abstract version[s]" (p. 95). There are, however, a number of difficulties with this experiment which make it hazardous to draw any conclusions from the results regarding the effects of decision context (abstract or interpreted) on comparative behaviour. Firstly, the authors acknowledge that "payoffs in the interpreted version[s] may not be isomorphic with the numerical payoffs in the abstract version[s]" (p. 96). The payoffs in the interpreted versions were in fact inherently non-numerical in all but one case. The first interpreted PD, for example, was presented as follows:

> You have been employed as a reported for the Evening Times for five months . . . . There is another paper, the Evening News, owned by a different publisher in the city. Your opposite number is a reporter for the News . . . .
>
> 1. Warehouse fire; 10 min. to deadline; 1 phone.
>
> Choice: Let the other call first / insist on calling first.
>
> Payoffs: Both call in stories/no story/scoop/ neither call in story.
>
> (p. 93).

The four payoffs were labelled R, S, T and P respectively, and were in this case assigned numerical values of 2, -5, 5 and -1 in the corresponding matrix version of the game. The assignment of payoffs to the various matrix games was, however, somewhat arbitrary, and unfortunately no attempt was made to determine whether they corresponded to the utilities present in the "interpreted" games. In one "interpreted" game~~s~~ numerical payoffs were given, but the values in the matched matrix version bear only an ordinal correspondence to them: $R = -1$, $S = -2$, $T = 2$, $P = 1$ in the "interpreted" version, compared with $R = 2$, $S = -3$, $T = 3$, $P = -2$ in the matrix version of the example above. Numerous studies have shown that the amount of cooperative behaviour displayed by subjects in Prisoner's Dilemma is extremely sensitive to differences in the matrix values even when their ordinal relationship is preserved (as it must be if the game is to remain a Prisoner's Dilemma); these findings have been reviwed above.

Orwant & Orwant (1970) make the startling admission that in some cases it is not even certain that the prescribed ordinal relationships $T > R > P > S$ were preserved in the "interpreted" versions of the game (p. 96). It is therefore not clear what conclusions can be drawn from their findings. They claim to have demonstrated that greater cooperation is elicited by "interpreted" than by matrix versions of the Prisoner's Dilemma Game, but for the reasons given above, any such conclusion seems unsafe.

Young (1977) investigated the behaviour of 60 subjects over 12 trials in an abstract, numerical version and a "structurally equivalent" lifelike version of a Prisoner's Dilemma Game. The author "created two forms of the game (one 'rich' in context, one 'poor'), empirically demonstrated the equivalence of their outcome preference structures, and explored the question of how differences in contextual realism influence . . . subjects' choice" (p. 302) and a number of other variables. He hypothesized that the realism of the lifelike version would engage norms of reciprocity and social responsibility and generate a greater frequency of cooperative choices than the abstract version. His results, however, revealed exactly the reverse.

Unfortunately, this experiment was marred by a number of rather serious design flaws and errors in the analysis of the results. Firstly, a sharp distinction was not maintained between the abstract and lifelike versions of the game. In both cases the subjects were told that the decisions involved a situation in which a football team captain may comment to the team coach in one of two alternative ways. In the abstract decision context, the subject's choice was labelled "Left" (corresponding to A_1 or

C) or "Right"(A_2 or D) and his hypothetical opposite number's (the coach's) choice was similarly labelled "Left" (B_1 or C) or "Right" (B_2 or D). The "joint outcomes" were given numerically as 6.6 (a_{11}), 5.7 (b_{11}), 1.4 (a_{12}), 2.7 (b_{12}), 3.5 (a_{21}), 3.4 (b_{21}), 3.4 (a_{22}) and 4.0 (b_{22}). It is apparent from these figures that the payoff structure is not symmetrical and the game is not Prisoner's Dilemma as normally defined (see Section 1.5 above). Even ignoring the asymmetry, $T > R$ does not hold, so there exists no temptation for either player to choose the D strategy.

In the "rich context" or lifelike version of the game, the decisions and outcomes were given verbally:

> "One relatively important second string player griped to you that he didn't get to play enough the last game and was considering leaving the team". The choice is between telling the coach about the player's upset <u>and</u> his potential plans, or merely telling him that the player seems upset. In the former case, the coach either "expresses approval . . . and agrees to talk with him about it" or "calls a meeting of the team . . . and tells the team that if all a person can do is gripe, then he doesn't belong to his team". In response to the latter decision, the coach either "nods his head in acknowledgement" or "thanks the captain for bringing it to his attention . . . . "
>
> (p. 305).

It is not clear in what sense this situation could be considered to have the strategic characteristics of Prisoner's Dilemma. The author reports "mean preferences" of subjects for the four outcomes (R - 6.04, T - 4.04, P = 3.12, S = 2.11) but no information is given about how these preferences were determined, and no attempt was made to evaluate the other player's (the coach's) perceived payoffs. The reported utilities of the subjects were then analyzed by the illegitimate use of repeated t-tests (one-way analysis of variance was called for). They do not, in any event, satisfy the prescribed inequalities $T > R > P > S$, so it can be concluded that this version of the game (like the abstract version, though for different reasons) is not Prisoner's Dilemma.

Even more seriously, the games used by Young (1977) do not even appear to have been games of strategy in the accepted sense. It is apparent from the description of the procedure (p. 304) that the hypothetical coach made his choices in each case _after_ the subject had already announced his own decision. The game was therefore not a game of strategy but what von Neumann & Morgenstern (1944) called a minorant game in which Player B was making a decision under certainty (von Neumann & Morgenstern, 1944, p. 100, _passim_). The problems facing a player under these conditions are quite different from those in a game of stratgey, since if Player A assumes that B is rational under certainty, then he too acts under certainty.

Without going into detail about sundry statistical indiscretions committed in the paper under review (which include an egregious example of probability pyramiding and the interpretation of non-significant differences as proving equality of means) it can be concluded that no useful information can be derived from this experiment regarding the effects of abstract and lifelike decision contexts on cooperative choices in mixed-motive games.

Eiser & Bhavnani (1974) investigated the behaviour of 80 subjects over 10 trials of a Prisoner's Dilemma Game against a programmed "tit-for-tat" strategy from the other player: on the first trial, the subjects received a C (B_1) choice from their supposed partners, and thereafter their partners always copied the subjects' previous choices. The decision context was similar in all treatment conditions: subjects all made their choices on the basis of an identical payoff matrix. The instructional set was however varied in a way that introduced contextual variations in the subjects' perceptions of the decision contexts. One group were given no contextual information beyond the abstract rules of the game, another were told that it was a simulation of economic bargaining, another thought it had to do with international negotiations, while a fourth group were led to believe that it concerned friendly or unfriendly interactions between pairs of individuals.

The investigators hypothesized that the interpretation of the game situation in terms of economic bargaining would tend to engage competitive motives and lead to more competitive (D or A_2) choices than the "international" and "interpersonal" treatment conditions, since the first "provides an excuse for exploitative self-interest" while in the other two types of situation "cooperation is more highly valued" (p. 94). The hypothesis was confirmed: the level of cooperation in the abstract and "economic" treatment conditions was fairly typical of those reported in the literature on Prisoner's Dilemma, but the level of cooperation in the "international" and "interpersonal" conditions was significantly higher. The authors concluded that "extrapolations from the results of PDG experiments to particular kinds of real-life situations must depend for their validity at least partly on whether the subjects themselves interpret the game as symbolic of the situations in question " (p. 97).

Although Eiser & Bhavnani's (1974) experiment was nicely conceived and competently executed, and although their results were highly suggestive, no attempt was made to use radically different decision contexts or to make any of the decision contexts genuinely lifelike. The experiment described below in Chapter 4 was motivated by a desire to provide a more radical test of Eiser & Bhavnani's hypothesis.

The final group of experiments on lifelike two-person gaming behaviour attempted to create a lifelike simulation of the game of Chicken. These experiments were reported by Sermat (1970), in a paper entitled: "Is game behaviour related to behaviour in other interpersonal situations?"

Sermat (1970) reported four experiments in which the behaviour of subjects who had previously displayed either highly cooperative or highly competitive strategy choices in Prisoner's Dilemma or Chicken games was examined in a "Paddle Game" or in a picture-interpretation task. These experiments "were designed to investigate whether cooperative and competitive behaviour remains consistent over time and in different situations " (p. 94). The justification for such an investigation was that "the evidence for or against generalizing from game to real-life situations is, at present, so incomplete that widely differing conclusions are possible" (p. 92). The results showed that, in contrast to their behaviour in the matrix games, nearly all the subjects produced highly cooperative strategies in the Paddle Game, although there was some slight indication that those who had evinced highly competitive behaviour in the Prisoner's Dilemma or Chicken games were somewhat more competitive in the Paddle Game than were the subjects who had displayed highly cooperative behaviour in these conventional games. Few differences between the two types of subjects were found in the picture-interpretation task; observers were quite unable to differentiate them on the basis of their face-to-face interactions during this task.

Sermat (1970) acknowledged that "the problem of choosing social situations in which the generality of game strategies could be tested is . . . difficult" (p. 94), but he does not seem really to have come to grips with the problem. The picture-interpretation task used in one of the experiments was, by his own account, of a completely different character from either the Prisoner's Dilemma or the Chicken games, and it might be added that it was not a game of strategy at all. It is not clear, therefore, what conclusions can be drawn from the lack of correspondence found between the behaviour of subjects in the game situations and the picture-interpretation task.

The Paddle Game, on the other hand, "was assumed to be relatively similar to conventional mixed-motive games like the Game of Chicken" (p. 94). The similarity was, however, of a rather attenuated kind. The Paddle Game was, like the normalized forms of the Prisoner's Dilemma and Chicken games, entirely abstract and no more lifelike than the more conventional games. The decisions facing the subjects at each trial involved moving a paddle backwards or forwards through a slot or leaving it in place, and the payoffs were either one unit of monetary gain or nothing. There were thus three rather than two pure strategies facing the subjects at each choice point; both Prisoner's Dilemma and Chicken have, by definition, two pure strategies available to each player. Furthermore, the payoff structure was quite unlike that which defines the game of Chicken: at some choice points the payoff was zero for both players whatever pair of strategies were chosen, and at all others the payoffs were (1,0) in one cell, (0,1) in another, and (0,0) in the remaining seven cells. The situation amounts therefore to a combination between a non-game, in which payoffs were all zero, and a 3 x 3 zero-sum game, neither of which bears the remotest resemblance to the game of Chicken, in which the payoffs occur at four levels and the structure is mixed-motive.

Sermat (1970) evidently considers his negative findings regarding the correspondence between the behaviour of subjects in the matrix games (particularly Chicken) on the one hand, and in the Paddle Game and the picture-interpretation task on the other, as being of some significance. He believes that his findings bear on the question of the ecological validity of experiments on mixed-motive games. He concludes his paper by saying that, if empirical evidence in favour of a relationship between laboratory game behaviour and behaviour in other situations is not forthcoming, "the theoretical contribution of game research may have to be stated in other terms than its relevance to interpersonal behavior in real-life situations" (p. 108). While wholeheartedly endorsing this conclusion, one is inclined to regard Sermat's empirical work irrelevant to the problem to which it was addressed. The experiment described below in Chapter 5 represents the first study of the choices of subjects in both lifelike and abstract decision contexts possessing identical strategic structures corresponding to the strategic structure of the Game of Chicken.

2.~~5~~4. N-person Prisoner's Dilemma

One of the most striking features which emerges from an examination of literature on experimental gaming in the late 1970s is the relative decline in attention being paid to two-person Prisoner's Dilemma games in favour of N-person Prisoner's Dilemmas. (The decline in two-person Prisoner's Dilemma studies is, it should be noted, only relative; the literature still abounds in experiments on the two-person game.) The most frequently cited reason for this shift of emphasis is the belief of many game theoreticians and researchers that, in comparison with the two-person variety, N-person Prisoner's Dilemma serves as a model for a wider variety of mixed-motive conflicts in everyday life. Hamburger (1979) has strongly implied this in his recent book, Games as Models by Social Phenomena, and Davis, Loughlin & Komorita (1976) have stated flatly: "The N-person case (NPD) has greater generality and applicability to real-life situations. In addition to the problems of energy conservation, ecology, and overpopulation, many other real-life problems can be represented by the NPD paradigm . . . ." (p. 520). These authors go on to say that "it seems reasonably safe to predict that we will see an increasing number of studies based on the NPD" (p. 521).

In view of the probable reasons for the sudden growth of interest in the N-person Prisoner's Dilemma Game in recent years, it is ironical that only one study has been published which addresses itself directly to the question of the ecological validity of the experimental paradigm. What follows is a fairly comprehensive review of all published experiments concerned with the behaviour of subjects in N-person Prisoner's Dilemmas, ending with a detailed exmination of the one study which has investigated the behaviour of subjects in a lifelike version of the game.

First of all, a word is necessary regarding four studies which will be excluded from detailed consideration. Bixenstine, Levitt & Wilson (1966), and Bixenstine & Douglas (1967) reported experiments on what they described as "six-person Prisoner's Dilemma" games. These experiments were, however, published before the appearance of Hamburger's (1973) important theoretical analysis of N-person Prisoner's Dilemma referred to in Section 1.5 above, and it can now be seen that the games used were not N-person Prisoner's Dilemma as properly understood; in particular, the crucial strategic property of strict dominance of the D strategy over the C strategy was violated. Essentially the same comments apply to a study by Rapoport, Chammah, Dwyer & Gyr (1962). These studies will therefore be omitted from consideration in what follows. Also omitted from further discussion will be a study by Meux (1973) who found that a questionnaire measure of how subjects would behave in life situations with regard to such matters as pollution control to be predictive of their behaviour in what she described as a 12-person Prisoner's Dilemma Game. An analysis of the payoff structure of Meux's game reveals that it is in fact an N-person Chicken Game. All other published experiments on N-person Prisoner's Dilemma are included in the discussion which follows.

Kelley & Grzelak (1972) reported an experiment in which 11 groups, varying in size from 10 to 15 players, played approximately 50 trials in an N-person Prisoner's Dilemma. Four different matrices were used in an attempt to manipulate two independent variables: "degree of individual interest" served by the competitive strategy and "degree of common interest" served by the cooperative strategy (p. 190). The subjects were not, however, informed regarding the payoff structure of the games, but had to infer them from the outcomes as they went along. The subjects were 147 undergraduate students. The findings revealed an overall frequency of cooperative choices of 32 per cent, which is similar to the figure typically recorded in experiments on two-person Prisoner's Dilemma games. The effect of an increase in competitive interest (analogous to a relative increase in the value of T in a two-person Prisoner's Dilemma) was a significant decrease in cooperative behaviour, but an increase in the common interest (analogous to an increase in the value of R in a two-person Prisoner's Dilemma) was only a marginal increase in cooperation. The authors comment: "We were struck by the lack of comprehension shown by many subjects in their informal postexperimental comments" (p. 195), and in fact there are reasons to believe that the more they understood, the more they tended to cooperate. One cannot help feeling, therefore, that Kelley & Grzelak's results may have been partly an artifact of the abstract and thus largely incomprehensible nature of the task.

Marwell & Schmitt (1972) compared the behaviour of 60 male undergraduates over 15 trials in either a two-person or a three-person Prisoner's Dilemma Game. "In order to make this comparison, a three-person Prisoner's Dilemma game was developed in which the rewards for specific behaviours are exactly the same as those in the two-person game" (p. 376). In this experiment, the main interest centred on a comparison of the behaviour of the 12 dyads with that of the 12 triads: "The basic data clearly support the hypothesis that rates of cooperation are inversely related to the number of people involved in the interaction" (p. 379). The results strongly suggested that the comparatively low frequency of cooperative choices in the three-person game (.35 per trial versus

.80 per trial in the two-person game) was "primarily the result of the difficulty of establishing mutually reinforcing relationships rather than the destruction of these relationships through defection" (p. 382).

An essentially similar experiment to that of Marwell & Schmitt (1972à was reported by Hamburger, Guyer & Fox (1975). The two experiments obtained basically the same result, and it is curious to say the least that Hamburger, Guyer & Fox make no reference to the earlier experiment in their introduction or their discussion of their results. In this later experiment, 160 undergraduate subjects were assigned either to a three-person Prisoner's Dilemma Game, or to a seven-person version of the same game. Payoffs for cooperation and competition were approximately equalized in the two versions. The games were each played over 150 trials. The results showed a greater frequency of cooperative choices in the smaller than the larger groups. The investigators feel justified in attributing this difference purely to group size, "since the method we used held utility considerations constant across three- and seven-person groups and did not vary other factors in the situations" (p. 519). They suggested that the difference may be accounted for by the phenomenon of de-individuation: in larger groups the identity and accountability of individuals may be submerged, thus freeing subjects to manifest "anti-social" behaviour. This interpretation is not in conflict with Marwell & Schmitt's explanation for their similar finding.

In an experiment by Kahan (1973), 36 male undergraduates were assigned to 12 trials. The structure of the game used was N-person Prisoner's Dilemma. An unusual feature of this experiment, which bears on Hamburger, Guyer & Fox's (1975) comments regarding de-individuation, was that the subjects made their choices anonymously: a subject could tell from the outcome on each trial how many group members had defected, but not who had defected. It is worth noting that such secrecy is never possible in two-person Prisoner's Dilemma, whether in the laboratory or in everyday life, since knowing that someone has defected in a two-person game necessarily implies knowledge of who it was. The subjects in this experiment played 100 reiterations of the game. The overall level of cooperation was extremely low (the mean frequency was .193 per trial). The investigators comment that "the choices made by the individual players were shown to have been made with apparently no regard for the choices of the other two players in the game" (p. 124). One wonders whether the same could possibly be true in a lifelike three-person conflict situation of some significance to the players.

Bonacich, Shure, Kahan & Meeker (1976) reported evidence to show that cooperation in the N-person Prisoner's Dilemma Game is not necessarily a decreasing function of group size. Their findings showed that the frequency of cooperative choices depend upon the payoff values used in the particular version of the game. The crucial factor seemed to be the relative values of R (reward for joint cooperation) and T (temptation to be the sole defector in the group). They used 10 groups of 9 subjects each; each subject made 15 choices in each of eight games, which were three-person, six-person or nine-person Prisoner's Dilemma games. In some of the games, a negative relationship between group size and cooperation was found; various explanations for this effect, including de-individuation, were discussed. With one particular type of payoff structure, however, a counter-intuitive positive relationship between group size and cooperation was found: this occurred in games in which R increased with group size but T was held constant.

The effects of sex and opportunity to communicate on cooperation in N-person Prisoner's Dilemma were investigated by Caldwell (1976). The subjects were 130 undergraduates (65 males and 65 females) assigned to 13 all-male and 13 all-female pentads. For the first 40 trials, the subjects played without full knowledge of the payoff structure of the game, but after that another 40 trials were played with a conventional payoff matrix. The results showed no significant effects due to either of the main independent variables (sex and opportunity to communicate). When subjects were permitted not only to communicate, but also to exact sanctions against subjects who failed to communicate, however, a highly significant increase in the frequency of cooperative choice was found. The author describes this as a "communication effect", but in fact the subjects in the sanctions condition were exposed to a different payoff structure with increased punishment for defection, so it is better interpreted as an effect of matrix variation. Caldwell admits that "sanctions may have, in effect, changed the nature of the payoff matrix" (p. 278), but this point should be put rather more forcefully by saying that sanctions <u>did</u> alter the payoff structure. This unavoidable interpretation unfortunately robs Caldwell's experimental findings of most of their apparent significance.

Goehring & Kahan (1976) reported an experiment in which 60 male and female subjects each played one of five three-person games, three of which were N-person Prisoner's Dilemmas, over 150 trials. In confirmation of Caldwell's (1976) finding discussed above, no sex effect was found. A competitive index, based on the relative values of T, R, P and S and analogous to the competitive indices which have been proposed for two-person Prisoner's Dilemma, was, however, found to be highly predictive of the frequency of cooperative choices in essentially the same manner as has been found with the two-person game.

A large scale experiment on N-person Prisoner's Dilemma was reported by Dawes, McTavish & Shaklee (1977). In this experiment, 284 subjects, recruited through a newspaper advertisement asking for groups of four friends, were assigned to 5-, 6-, 7-, or 8-person groups. Four different communication conditions were built into the experimental design, and in addition an ingenious but complicated procedure was used for ensuring that in some conditions subjects could, and in others they could not lose money through participation in the experiment. The four levels of communication were (a) no communication, (b) irrelevant communication, (c) relevant communication, and (d) relevant communication plus (non-binding) vote before decision. Once again, the results showed no sex effect. The loss manipulation also failed to produce any significant effect on strategy choices. A significant main effect due to communication was, however, found: relevant communication, with or without non-binding votes, had the effect of increasing the frequency of cooperative choices relative to the no communication and irrelevant communication conditions. Of great interest was Dawes, McTavish & Shaklee's incidental finding that subjects' cooperative behaviour correlated .60 with their predictions of how much cooperation they could expect from the other players: defectors were found to predict four times as much defection from the other players as was predicted by cooperators. A replication of the main experiment, using 160 subjects, confirmed the findings of the first experiment regarding the effects of communication, and also showed that having to make choices (rather than simply observing the choices of others) influenced the subjects' predictions regarding the amount of cooperation to be expected from the other players in both directions (the variance was greater). It is impossible in a summary review to do justice to all the subtleties of these two experiments.

Fox & Guyer (1977, 1978) recently reported two experiments on N-person Prisoner's Dilemma. The first was designed to investigate the effects of group size and degree of cooperation from others on a player's cooperative choices. For this purpose, computer-generated stooge responses were presented to the individual subjects. The subjects were 48 male undergraduates, randomly assigned to either 3-person or 12-person Prisoner's Dilemma games, each of which was reiterated over 200 trials. The stooges generated an average level of cooperation of either 64 per cent or 36 per cent cooperative choices. The results showed, firstly, a higher level of cooperation in the smaller compared with the larger groups. Secondly, an interaction was found: the highly cooperative others' strategy elicited 20 per cent more cooperation from the subjects in the three-person groups but not in any of the other treatment conditions. In particular, subjects in small groups were highly responsive to cooperation from others while subjects in large groups were unaffected by the level of cooperation of the other players. The authors assume that de-individuation is at the root of this interaction effect. A most interesting aspect of this experiment is that it has decisively refuted a once popular theory regarding the by now well established finding regarding the generally higher levels of cooperation found in small as compared with larger groups, namely the "bad apple" theory. According to this theory, it takes only one defector in a group to force the others into a position where they have to make defecting choices to protect their own interests; since the probability of the presence of a bad apple increases with group size, the group size effect seems to be explained by this theory. Fox & Guyer's (1977) use of computer-generated stooge responses has, however, ruled out this theory, and thereby indirectly strengthened the alternative de-individuation theory.

Fox & Guyer (1978) tested the de-individuation theory directly in a 4-person Prisoner's Dilemma experiment in which varying degrees of anonymity were allowed to the players. In this later experiment, 80 subjects (details were not given) were assigned to 20 4-person groups. In one treatment condition, the subjects exchanged names and background information; in the outhers they did not. All subjects played 30 trials in standard matrix format, either with public or anonymous choices. The results revealed that subjects in the public choice conditions were significantly (12 per cent) more cooperative on average, in terms of number of cooperative choices, than were subjects in the anonymous choice conditions. The de-individuation theory is clearly supported by these findings.

The only published study investigating the choices of subjects in a lifelike N-person Prisoner's Dilemma Game comprises a series of three experiments by Alcock & Mansell (1977). In all three of these experiments a normal payoff matrix was used, but the subjects were told that the experiment was "a simulation of animal population growth under conditions of scarce resources" (p. 447). They were given a verbal description of "the tragedy of the commons" and were assigned the roles of cattle farmers. Their choices were labelled "add" (an animal to the pasture), and "not add". In the first experiment, the subjects were 29 male and 41 female undergraduate students assigned randomly to 10 7-person groups. The game was reiterated 30 times. The proportion of cooperative choices made by the subjects under these conditions averaged .32 with no significant differences between the 10 groups. This figure is perhaps not strikingly different from what might have been anticipated in a game using an abstract decision context. A second experiment in this series, using 60 subjects drawn from the same subject pool as the first experiment, found an average level of cooperation (.39) which was hardly any higher irrespective of the fàåse feedback given to the subjects: whether the subjects were given cooperative feedback (6-8 "adds" per trial) or non-cooperative feedback (2-4 "adds" per trial) had no significant effect on the subjects' cooperative choices. A third experiment, using 64 undergraduate subjects, found no significant differences in the proportion of cooperative choices in free play, cooperative feedback, and non-cooperative feedback treatment conditions.

Alcock & Mansell's (1977) experiments described above are interesting, not merely on account of the failure of cooperative and non-cooperative feedback to produce significant differences in the bahaviour of the subjects (this is not inconsistent with Fox & Guyer's (1977) findings reported above), but more importantly is the indirect suggestion they contain that the lifelike decision context used does not elicit levels of cooperation from subjects strikingly different from those found in experiments using abstract payoff matrices only. This last inference is, however, merely conjectural, since neither these experiments nor any others in the literature on N-person Prisoner's Dilemma have systematically compared the behaviour of subjects in abstract and lifelike decision contexts under controlled conditions in the same experiment. An experiment such as this is described below in Chapter 6.

2.5. Outline of Experiments I-IV

In Sections 2.2, 2.3 and 2.4 above, the literature on two-person zero-sum, two-person mixed-motive and N-person Prisoner's Dilemma gaming experiments has been thoroughly reviewed. In particular, all published experiments on two-person zero-sum games and on N-person Prisoner's Dilemma have been discussed, and, in addition, specially close attention has been devoted to all available experiments comparing the behaviour of subjects in abstract matrix games, whether two-person zero-sum saddle-point, Prisoner's Dilemma, Chicken or N-person Prisoner's Dilemma, with the behaviour of subjects in lifelike versions of these games. It has been argued that such experiments are of crucial importance in the debate which dominates much of the gaming literature regarding the ecological validity of experimental games. Every single one of the previous experiments on this question have, however, been found either to be vitiated by design flaws, serious errors in the statistical analysis of the results, unwarranted inferences from the data or failure properly to control confounding independent variables, or to be of only incidental relevance to the question of ecological validity on account of a failure to compare the behaviour of subjects in abstract and lifelike decision contexts in the same experiment. The experiments described in Chapters 3, 4, 5 and 6 were designed in the hope of overcoming these problems and providing some relevant empirical evidence on the question of ecological validity.

The four experiments which constitute the empirical component of this dissertation resemble each other in many ways, although there are also fundamental differences between them. Before outlining the unique features of each experiment, their common characteristics will be briefly summarized. These common characteristics fall into three categories: design features, methodological principles, and techniques of data analysis.

The most important design feature common to all four experiments is the use, in each case, of at least two basic versions of the same game within the same experiment and with other potential sources of systematic variation in the subjects' behaviour controlled by the proper use of randomization. In each experiment, one treatment condition involved the use of a normalized payoff matrix, and the subjects were required to make their choices in a fashion similar to that used in the overwhelming majority of gaming experiments which have previously been reported in the literature. Another treatment condition in each case involved the use of a lifelike simulation of a conflict situation such as might be encountered in everyday experience. In these treatment conditions, instead of being confronted with an abstract payoff matrix, the subjects were presented with verbal descriptions of hypothetical lifelike conflict situations and were required to indicate the choices they might make were they to find themselves in such situations. These verbal descriptions were sufficiently rich to allow the payoffs to each player attendant upon each possible outcome to be fully and unambiguously specified, and the subjects were exhorted to make their choices solely on the basis of the specified payoffs. Great care was taken to ensure that the rules of the game were identical in each pair of abstract and lifelike decision contexts: the number of pure strategies available to each player was the same and, most importantly, the payoff structure of each version bore a ratio scale correspondence to that of the other. Apart from the manipulation of the decision context (abstract or lifelike) the instructions given to the subjects and their manner of responding were identical. Since the assignment of subjects to treatment conditions was strictly random, any significant differences in their strategic choices in different conditions could therefore be attributed to differences in the abstractness of lifelikeness of the decision contexts. This design feature was intended to throw light on the problem which has been expressed by Hamburger (1979) in this way: "A price must be paid in moving from complex situations in the world to simple games in the laboratory. When people are put in a simple artificial situation, one might argue, they will behave in ways appropriate to a simple artificial situation, thereby revealing nothing about how they will behave in a complex real situation" (p. 231). In the experiments described below, a comparison of the behaviour of subjects in the "simple artificial" treatment conditions with those in the lifelike conditions, which may be regarded as intermediate between the former and "real world" situations, enables the extent of this problem to be investigated empirically.

Certain methodological principles common to the four experiments are worth mentioning briefly. Firstly, great care was taken (with the help of tape recorded or written instructions) to avoid inadvertently introducing any systematic variations in subjects' motivational sets. The use of the word "game" was avoided in all treatment conditions, and any subjects who revealed a prior acquaintance with game theory in the post-experimental interviews were eliminated from the subsequent data analysis. Research reviewed in Section 2.4 above has indicated that subjects are very sensitive to the motivational set induced by the experimenter's instructions, yet few investigators have taken pains to adequately control this potential source of variation in their strategy choices.

Secondly, the experimenter took great care to ensure that the subjects fully understood the tasks which were presented to them. Previous investigators (e.g. Kelley & Grzelak, 1972 - - see above Section 2.4) have sometimes commented that their subjects did not seem to understand the rules of the game properly; gaming experiments are not easy from the subjects' point of view and it seems likely that they often fail to grasp the problem when first presented with it. In the experiments described below, the experimenter not only explained the rules of the games in great detail, but also quizzed the subjects in a standard manner before the experiment began and provided further explanation when necessary. The subjects were not permitted to commence until the experimenter was satisfied that they understood the rules completely, and subjects who never attained a full understanding were excluded from the data analysis.

A word should be said about the number of reiterations used in these experiments. In each case the game was reiterated 30 times. In some previous experiments, even fewer trials have been played (see above, Section 2.4), but in many a far larger number of reiterations have been used. The truth of the matter is, however, that experiments with a very large number of trials soon become tedious and frustrating for the participants. Lieberman (1960b) in fact commented, in the paper reviewed in Section 2.4 above, that boredom and tedium may have accounted for some of the "irrational" choices made by his subjects after several scores of trials. While acknowledging that there is evidence that certain phenomena (of no direct relevance to the experiments below) may only become manifest after a large number of trials (e.g. Rapoport & Chammah, 1965), it was felt that an understanding of peoples' sober and deliberate strategy choices could best be attained on the basis of a maximum of 30 trials.

A fairly searching post-experimental interview was conducted with each subject following his or her participation in each experiment. Although investigators have rarely troubled to ask their subjects about their strategy choices and their reactions in general, it was felt that such a procedure would be invaluable in interpreting their behaviour.

A final methodological principle common to the four experiments, and introduced for the first time in research in this area, is of particular importance. This concerns the attempt which was made to provide a check on the rules according to which the subjects played the games. The reason for this was that there is never any guarantee in gaming experiments that the subjects are playing the games presented to them by the experimenter. There is every reason to believe that subjects at least sometimes introduce extraneous utilities into the situation thereby effectively transforming the payoff structure of the interaction situation of the experiment. In such cases, an interpretation of the subjects' behaviour based on an assumption regarding the strategic structure of the situation may be wide of the mark. Apfelbaum (1974) has, for example, commented thus: "When used in social psychological studies, however, the matrix is a payoff device; it does not refer to utilities or, speaking more loosely, to the subjective values of the different outcomes. These values are initially unknown . . . . There is no reason to assume that they are fixed a priori" (p. 108), although she believes that "this is precisely one of the interesting features of games for social psychology" (loc.cit.). Hamburger (1979) has touched on the same problem: "Experimentation under controlled laboratory conditions holds out the hope that we can overcome our ignorance of players' utility scales" (p. 231), and only under very special circumstances may we be "justified in assuming that each player's preference among outcomes is exactly reflected by the various monetary rewards that the player gets from the outcomes" (loc. cit.). Previous researchers have seldom acknowledged the existence of this potentially terminal malady in experimental games. The indicated treatment is clearly direct investigation, but none has been attempted by previous researchers. In the four experiments described below, however, the subjects' satisfaction with the outcome of each trial was measured directly in order to provide a precise indication of any deviations from the given payoff structures in their subjective utilities. This enabled an investigation to be made not only of the extraneous utilities introduced by subjects in conventional matrix games, but also of any differences between such alteration in the rules of the game made by subjects in different treatment conditions.

Some comments are in order, finally, with regard to certain techniques of data analysis common to the four experiments described below. Mention has been made in Section 2.4 above of the rather crude dependent measures used in most previous gaming experiments: in many previous studies the dependent measure has been simply the proportions of cooperative choices (or the proportion of minimax choices) made by subjects in different treatment conditions. In the experiments described in Chapters 3, 4, 5 and 6 some rather more sophisticated analyses of the data are attempted. In the first place, Trial Blocks has been included as a factor in all the Analyses of Variance used in these experiments in an effort to provide information about the change in the subjects' behaviour over trials.

Secondly, a word is necessary about the method of analyzing the subjects' satisfaction ratings referred to earlier. In order to detect departures on the part of the subjects from the payoff structures of the games presented to them, Pearson product-moment correlation coefficients were calculated separately for each subject between the payoffs achieved by the subject on each trial and his or her satisfaction rating with the outcome of that trial. It can be assumed that subjects whose utility functions closely adhered to those given by the formal rules of the game would manifest correlations close to unity, and mutatis mutandis, low correlations may be expected to reflect the introduction of extraneous utilities into the situation. The correlation coefficients thus obtained were then used as raw scores in an Analysis of Variance in each experiment. After careful consideration, it was decided to use raw correlation coefficients in these analyses rather than z transformations for the following reasons: (a) the structural model and underlying assumptions of the F test (cf. Winer, 1962, pp. 56-62) contain nothing which implies that z transformations would be more appropriate; (b) if the sampling distributions of the raw scores in an Analysis of Variance are badly skewed, then the results are difficult to interpret, but z transformations do not (in spite of common beliefs to the contrary) have any effect of normalizing distributions: if a set of scores is skewed, then their corresponding z transformations will be skewed to exactly the same extent. In any event, contemporary authorities are unanimous in believing that when $N > 25$ the assumptions regarding normality of distribution can be safely violated; and (c) the use of transformed correlations would mean that the interpretation of any significant results would have to be made in terms of the transformed scores, which seem to have less intuitive meaning than correlation coefficients (which are nothing other than the standardized ratios of covariances to variances). Furthermore, transformations can lead to the creation of interaction effects not present in the original data and vice versa.

Finally, mention must be made of the Time Series Analysis used in the experiments described below. As Gottman (1979) has recently pointed out, "time-series techniques are not widely known to psychologists" (p. 339), yet they are obviously eminently suited to the analysis of gaming data. The Time Series Analyses used in these experiments were of an essentially straightforward and elementary kind: the techniques available are extremely complex and mathematically deep, but no attempt was made to squeeze all possible information from the data. Apart from the paper by Gottman referred to above, no applications of Time Series Analysis to social psychological data appear to have been published. The expositions of Time Series Analysis which were found most useful were the following: Anderson (1976); Bliss (1970, Vol. 2, Ch. 17); Brown (1963); Cooper, Osselton & Shaw (1974); Glaser & Ruchkin (1976); Gottman (1979); Orr & Naitoh (1976); Spiegel (1961, Ch. 16); Tukey (1967). A time series is a set of observations taken in serial order, and Time Series Analysis consists of a set of techniques designed specifically to discover characteristic movements in such observations, to detect cyclical tendencies, and to analyse relationships between one time series and another. The series of choices made by a pair or a group of subjects in an experimental game lend themselves ideally to such analysis, which in turn allows inferences to be drawn from their behaviour which could not be made by other means. In the analysis of the data of the experiments described below, a simple application of these techniques was therefore attempted.

An outline will now be given of each of the four experiments described below in Chapters 3, 4, 5 and 6.

Experiment I: Choices in Abstract and Lifelike Two-person Zero-sum Saddle-point Games. In this experiment, 84 undergraduate and post-graduate students were randomly assigned to treatment conditions in a 3 x 2 x 3 factorial design. The first factor was the independent variable Opponent's Strategy: the subjects were confronted with a programmed strategy from the other players which was either consistently minimax, random, or consistently non-minimax. The second factor was the independent variable Decision Context: the subjects played a simple 2 x 2 zero-sum saddle-point game either in conventional payoff matrix form or in terms of a lifelike simulation of decisions facing two candidates in a constituency election. The third factor was Trial Blocks: the subjects' minimax strategy choices were analyzed across three trial blocks of 10 trials each.

Experiment II: Choices in Structurally Equivalent Abstract and Lifelike Prisoner's Dilemma Games. The participants in this experiment were 80 undergraduate and post-graduate students. The design was 4 x 3 factorial. The first factor, Decision Context was varied as follows: the subjects played either for positive monetary incentives in an abatract non-matrix version of a Prisoner's Dilemma Game, for negative incentives (possible losses from an initial financial stake) in an abstract non-matrix version of the same game, in a conventional payoff matrix, or in a lifelike simulation of a situation which might confront two restauranteers in deciding whether to provide floor showed in their respective establishments. The second factor was Trial Blocks as in Experiment I.

Experiment III: Effects of Abstract and Lifelike Decision Contexts on Choices in a Game of Chicken. The design and methodology of this experiment was essentially identical to that of Experiment II. In this experiment 80 undergraduate and post-gzaduate students participated as subjects. The game in this case was Chicken and the lifelike simulation involved two hypothetical wholesalers who have to decide from week to week whether to send their goods to island customers by sea or air.

Experiment IV: Effects of Abstract and Lifelike Decision Contexts on Choices in an N-person Prisoner's Dilemma Game. The design and methodology were similar to those of Experiments II and III. The game, however, was a 3-person Prisoner's Dilemma, and the lifelike simulation involved the decisions of the finance ministers of three hypothetical oil producing nations with regard to full production versus restricted production. The subjects were 120 undergraduate students.

CHAPTER THREE

3. EXPERIMENT I: CHOICES IN ABSTRACT AND LIFELIKE TWO-PERSON ZERO-SUM SADDLE-POINT GAMES

3.1. Introduction

Although the experimental gaming tradition continues to generate a great deal of research and, according to recent commentators, an accelerating frequency of review articles and books (vide Davis, Loughlin & Komorita, 1976; Pruitt & Kimmel, 1977), there is evidence that dissatisfaction with research in this area is mounting. In particular, doubts have recently been expressed about the relevance of experimental games to (other?) real life situations (e.g. Nemeth, 1972; Schlenker & Bonoma, 1978). In real life, people are often confronted with choices between clearly defined alternatives under conditions in which the (known) possible outcomes depend also upon the choices of at least one other person. Experimental games are simply models of decision-making situations which have this property, and no one could validly question the goal of studying this important class of social interactions. If doubts about the relevance of experimental games to everyday experience are to be taken seriously, attention must therefore centre on the highly abstract nature of the models used in such research.

Most gaming experiments, and all of those which have investigated behaviour in two-person purely competitive situations (two-person zero-sum games) have used abstract models, usually payoff matrices, in which subjects play for money, tokens or simply points (see Section 2.2 above). No attempts have hitherto been made to examine the behaviour of subjects in two-person zero-sum games which resemble everyday competitive situations. It is true that many of the payoff matrices which have been used possess strategic structures identical to those found in certain commonplace predicaments, but the decision contexts in which the subjects have had to operate have been of such an abstract and unnatural character that questions naturally arise about the generalizability of the results to non-laboratory situations or, more generally, about the ecological validity of the findings. In the experiment described below, an attempt was therefore made to compare the behaviour of subjects in an abstract matrix game to their behaviour in a simulation of a lifelike predicament whose formal strategic structure, in terms of alternatives and payoffs was identical to the matrix. The matrix decision context resembled those used in previous investigations in this area, but the structurally identical simulation decision context was unlike any task presented to subjects in past research. For the reasons implied by the comments above, the major hypothesis was that the behaviour of subjects in these two decision contexts would differ systematically in spite of the structural identity between the games used, though the nature of this difference was not predicted.

For several reasons it was decided to use a 2 x 2 game with a saddle-point in this experiment. (a) This is the simplest and most easily understood zero-sum game; (b) the minimax rule is in this case completely unambiguous and easily found; (c) far too little research has been devoted to such games in past research; and (d) what little evidence there is does certainly not indicate that subjects always select their "optimal" minimax strategies in such situations (see Section 2.2 above). Following Morin's (1960) finding (discussed in Section 2.2 above) that subjects tend to choose minimax less frequently when the average expected value (assuming random choices from the opponent) of the non-minimax pure strategy is greater than that of the minimax pure strategy, the game structure used in this experiment was constructed accordingly. In addition, following Brayer's (1964) finding (see Section 2.2) that subjects choose their minimax strategy least often when the value of the game is low, the game used in this experiment was given a negative value. In short, steps were taken to ensure that, although the minimax strategy was extremely obvious, the subjects would have certain tendencies to deviate from minimax.

Following several previous studies involving zero-sum games (reviewed above in Section 2.2), notably Brayer's (1964) provocative study, the subjects in this experiment were exposed to programmed strategies from their opponents. The motivation for this design feature was threefold: firstly it enables the frequency of minimax choices on the part of the subjects to be investigated without confounding; secondly there are powerful theoretical arguments (see Section 1.3 above) against the minimax rule in situations in which the opponent chooses "irrationally" and it is of interest to compare the behaviour of subjects in such cases with that in the face of a "rational" opponent; and thirdly there is evidence (Brayer, 1964) that subjects tend to deviate from their minimax strategies more frequently when confronted with an opponent who chooses randomly than when faced with a "rational" opponent.

In order to clarify the effects of opponent's strategy on minimax choices, the experiment described below incorporated three kinds of opponent's strategy: (a) consistently minimax, (b) random, and (c) consistently non-minimax. It was hypothesized that the frequency of minimax choices would be greatest against the consistently minimaxing opponent and least against a consistently non-minimaxing opponent (since these strategies maximize the subjects' expected payoffs in each case) and that these effects would become increasingly evident over trials. These hypotheses were based on the presumption of simple common sense on the part of the subjects, although they are in direct conflict with the predictions which are implied by formal game theory: this latter theory would clearly predict no differences between any of the treatment conditions in this experiment, given the same presumption.

3.2. Method

Design. A three-factor mixed design, with repeated measures on one factor (Winer, 1962, Ch. 7) was used in this experiment. The first factor, Opponent's Strategy, occurred at three levels: minimax, random, and non-minimax. In the first level of this factor, the subjects were confronted with an opponent who invariably chose his minimax strategy. In the second level, the opponent's choices oscillated randomly between the minimax and the non-minimax strategies. In the third level, the opponent always chose his non-minimax strategy.

The second factor, Decision Context, was varied at two levels: matrix and simulation. In the former case, each subject was presented with the game matrix shown in Figure 3.1.

	L	R
L	-2,2	2,-2
R	-1,1	0, 0

Figure 3.1. Payoff Matrix Used in Matrix Decision Context

The payoff matrix shown in Figure 3.1 was drawn on a large (A4) sheet of paper, using blue ink for Row's strategies and payoffs and green ink for Column's. Diagonal lines from the top left to the bottom right of each cell were, in addition, used to separate the payoffs and make the diagram easier for the subjects to understand. Each subject was told that his choices would be between rows (and that his colour was blue) while his partner's choices would be between columns (pepresented in green). The following typewritten instructions were given to the subjects in the matrix conditions:

> Your task will consist of making a series of 30 decisions. You may each earn up to 120 points if your decisions turn out well, but you may get less. How many points you eventually end up with

> will depend not only on the decisions which you make but also on the decisions which the other person makes. You are advised, therefore, to consider each decision carefully.
>
> After each joint decision you will receive -2, -1, 0, 1 or 2 points. Your decision, as shown on the cards in front of you, will in each case be either R for Right or L for Left. The scheme for awarding points is summarised on the payoff diagram. The payoffs to one of you are represented by the blue figures and the payoffs to the other are shown in green figures. You can tell which colour applies to you by the colour of the cards in front of you. This will also tell you whether you are choosing between the rows marked L and R (that's if you are Blue) or the columns marked L and R (if you are Green).
>
> By examining the payoff diagram you can easily see what the outcome of each joint decision will be for each of you. If, for example, Blue and Green both choose L, then Blue gets -2 points and Green gets 2. If Blue and Green both choose R, then they each get 0 points. If Blue chooses L and Green chooses R, then Blue gets 2 points and Green gets -2. Finally, if Blue chooses R and Green chooses L, then Blue gets -1 point and Green gets 1 point.

Subjects in the simulation conditions were given no payoff matrix, but were presented with the following version of the game in typewritten form:

> Your task will consist of making a series of 30 decisions. You may do very well for yourself if your decisions turn out well, but you may be less successful. The outcome in each case will depend not only on the decisions which you make, but also on the decisions which the other person makes. You are advised, therefore, to consider each decision carefully.
>
> Your decisions will be based on the following hypothetical situation. You and an opponent from a rival party are the only candidates running for election in a particular constituency. At frequent intervals the local radio station invites both of you to participate in a live debate against each other in order to help the listeners to make up their minds. The problem facing you on each occasion is whether to agree to take part in such a debate or

> whether to refuse the invitation, and your sole consideration is to get as many votes as possible. In each case your decision, as shown on the cards in front of you, will be AGREE or REFUSE.
>
> The following facts were told to you by a public opinion research organization which programmed a computer with all the relevant information about the election (including the candidates' public images) and these facts you know to be perfectly accurate. If neither candidate agrees to the debate, the radio station will not bother to publicize the fact, so naturally neither candidate will lose any votes to his opponent. If you refuse to debate and your opponent agrees, however, this fact will be made known to the public, and a significant number of your supporters will transfer their allegiance to your opponent. If, on the other hand, you both agree to the debate, you will inevitably be outshone in the minds of some listeners and you will lose twice as many votes to your opponent as you would have lost in the event of your refusing to take part while the opponent agreed to. In the event of your agreeing to the debate and your opponent refusing, however, you will gain as many votes from your opponent as you would have lost in the event of the debate actually taking place and your image thereby damaged.
>
> Summary:-
>
> You refuse, he refuses: you lose/gain 0 votes, he loses/gains 0 votes;
> You refuse, he agrees: you lose s votes, he gains s votes;
> You agree, he refuses: you gain 2s votes, he loses 2s votes;
> You agree, he agrees: you lose 2s votes, he gains 2s votes.

The third factor in the experimental design was Trial Blocks: subjects in the above treatment conditions each made 30 successive decisions, which, for the purposes of analysis, were divided into three trial blocks. The chief dependent variable was the mean number of minimax choices made by the subjects in each Treatment Condition x Trial Block. According to formal game theory in general and the minimax rule in particular, subjects in the matrix coddi tions ought always to choose R while their opponents ought always to choose L, since this strategy pair represents the saddle-point of the game. The value of the game is correspondingly equal to -1. For the same reasons, mutatis mutandis, subjects in the simulation conditions should invariably choose REFUSE while their opponents should always AGREE.

Subjects. The subjects were 84 undergraduate and postgraduate students at the University of Leicester, randomly assigned to each of the six treatment conditions. There were 42 males and 42 females in the sample. Their ages ranged from 18 to 28 with a median of 19. Students reading Psychology were not included in the sample, and a small number of subjects who turned out in the post-experimental interview to have some knowledge of game theory were replaced with naive subjects.

Procedure. Subjects were tested in pairs in order to create the impression that they were interacting with each other. In fact all subjects received a pre-programmed sequence of choices from the experimenter. Each pair of subjects was randomly assigned to one of the six treatment conditions until eight pairs had completed one of these conditions, after which random assignment was made between the remaining five, and so on until all 84 subjects had been run.

The pair of subjects in each testing session were seated in a laboratory with a partition between them. Subjects in the matrix conditions were each presented with a payoff matrix similar to the one shown in Figure 3.1, a copy of the typewritten instructions described above, a scoring sheet (Appendix A), a decision card (Appendix B) and a rating scale (Appendix D). The treatment of subjects in the simulation conditions was identical, except that they received no matrix, were given the appropriate typewritten instructions described above, and their decision card was the one shown in Appendix C.

In all treatment conditions, subjects were given approximately five minutes to familiarize themselves with the materials. The experimenter then tested each subject's understanding of the game separately and assisted in cases where incomplete comprehension was evident. The experimenter continued to explain the game (the word "game" was, of course, never used) until each subject was able to demonstrate full understanding, i.e. until he could correctly answer questions of the form: "If you choose this and he chooses that, what do you get and what does he get?". Subjects who failed to reach this level of comprehension were not included in the subsequent analysis of the data.

When both subjects were ready, the following typewritten instructions were presented to them:

> You are now going to make a series of 30 joint decisions. Each decision will be made without knowledge of what the other person has chosen by pointing to one of the two cards in front of you. Your decisions will be irreversible, and any attempt to communicate with or indicate your feelings to the other person, for example by sighing or laughing, will force the experimenter to terminate the experiment. When you have both reached a decision, the experimenter will announce the choices: you will each know what the other has decided and you will be able to work out how each of you has fared in terms of payoffs.

> You will record both decisions and both payoffs on the scoring sheet, together with a rating for how pleased or displeased you are with the outcome of that particular decision, before going on to make the next decision. The experimenter will show you how to fill in the scoring sheet.
>
> You will be able to take stock of your position after each trial. Remember that your sole objective is to accumulate as many points as possible ["as much profit as possible" was substituted in the simulation conditions]. After 30 trials, you will be able to add up your points [to see how you have done over the whole period].

After reading these instructions, the subjects were invited to make their first choices by pointing to the decision cards in front of them (Appendix B or C). When they had both reached a decision, the experimenter presented each of them with an index card on which was written either (a) "YOUR PARTNER CHOSE L", (b) "YOUR PARTNER CHOSE R", (c) "YOUR PARTNER CHOSE AGREE", or (d) "YOUR PARTNER CHOSE REFUSE". This feedback was not in fact based upon the choices of the subjects' partners but was programmed in the following manner: in the matrix conditions the experimenter held a deck of 60 cards containing an equal number like (a) and (b) above. In the minimax matrix condition he simply chose cards of type (a) on each occasion to present to the subjects. In the random matrix condition, he dealt the cards two by two from a well-shuffled deck. In the non-minimax condition he always chose type (b) cards. For the simulation conditions a similar set of procedures was used, except that a deck of type (d) and type (e) cards was used.

After each trial, and after being appraised of the partner's alleged choice in the manner described above, both subjects recorded both choices on the score sheets, together with the payoffs to themselves and their supposed partners. (This provided an additional check on their understanding of the rules of the game: subjects whose score sheets were later found to contain errors were replaced with new subjects.) Before moving on to the next trial, the subjects also recorded on the score sheets their satisfaction with the outcome of that trial according to the code given on the 5-point rating scale (Appendix D) from VD (very displeased) to VP (very pleased).

After 30 choices had been made by each subject, the experimenter interviewed them separately and summarized their responses on the score sheets. Four specific questions were asked in the post-experimental interviews: (a) "How do you feel about the overall results?", (b) "How do you feel about your partner?", (c) "What was your general strategy?", and (e) "Do you have any further comments?". After the post-experimental interviews, the subjects were de-briefed and thanked for their participation.

3.3. Results

Main findings. The primary interest in the results of this experiment centres on the frequency of minimax choices made by the subjects in the various treatment conditions. Since each subject made 30 successive strategic choices, the maximum number of minimac choices is 30 in each case. In order to enable the development and change in the subjects' behaviour to be investigated, the results were tabulated in trial blocks containing 10 choices each. These results are given in tabular form in Appendix E.

An Analysis of Variance, (Opponent's Strategy x Decision Context x Trial Blocks) was performed in order to determine the significance of these factors on the mean number of minimax choices made by the subjects. The results of this analysis are given in Table 3.1.

Table 3.1: ANOVA Summary Table: Mean Minimax Choices
(Opponent's Strategy x Decision Context x Trial Blocks) N=84

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
A(Opponent's Strategy)	629.29	2, 78	314.65	10.22	30.78	$p < .001$
B(Decision Context)	242.10	1, 78	242.10	10.22	23.68	$p < .001$
C(Trial Blocks)	42.06	2,156	21.03	2.62	8.04	$p < .001$
AB	100.78	2, 78	50.39	10.22	4.93	$p < .001$
AC	182.80	4,156	45.70	2.62	17.47	$p < .001$
BB	.34	2,156	.17	2.62	.07	n.s.
ABC	58.04	4,156	14.51	2.62	5.55	$p < .001$
Total	2460.90	251				

The effect of Opponent's Strategy was highly significant. A posteriori analysis by means of the Tukey test (Bruning & Kintz, 1977, pp. 122-124) revealed that all three means differ significantly from one another beyond $p < .05$; subjects made significantly more minimax choices against a minimax Opponent's Strategy ($\overline{X} = 5.81$)

than in either of the other conditions, and they made significantly more minimax choices when their opponents adopted a random strategy ($\overline{X} = 4.19$) than when he consistently made non-minimax choices ($\overline{X} = 1.93$). These findings are in line with expectations, since it is unambiguously in a subject's best interest to adhere to the minimax rule only when his adversary may be expected to do likewise, and it is in his interest to adopt the non-minimax strategy when his adversary consistently makes non-minimax choices.

Decision Context also had a highly significant effect on the subjects' strategy choices: they made significantly more minimax choices when the game was presented in matrix form than when a simulation of a lifelike situation was used. This finding is best interpreted in the light of the Opponent's Strategy x Decision Context interaction (see below), which indicates that this difference was manifested in some treatment conditions and not others.

The subjects displayed a highly significant decrease in minimax choices over Trial Blocks; Tukey tests revealed that only the difference between Trial Block 1 and Trial Block 3 is significant beyond $p < .05$. The decrease is of course to be expected since there were twice as many subjects confronted with an "irrational" (randomizing or non-minimaxing) opponent as there were subjects who had to contend with a "rational" opponent.

An examination of Figure 3.2 is of assistance in interpreting the highly significant interaction Opponent's Strategy x Decision Context.

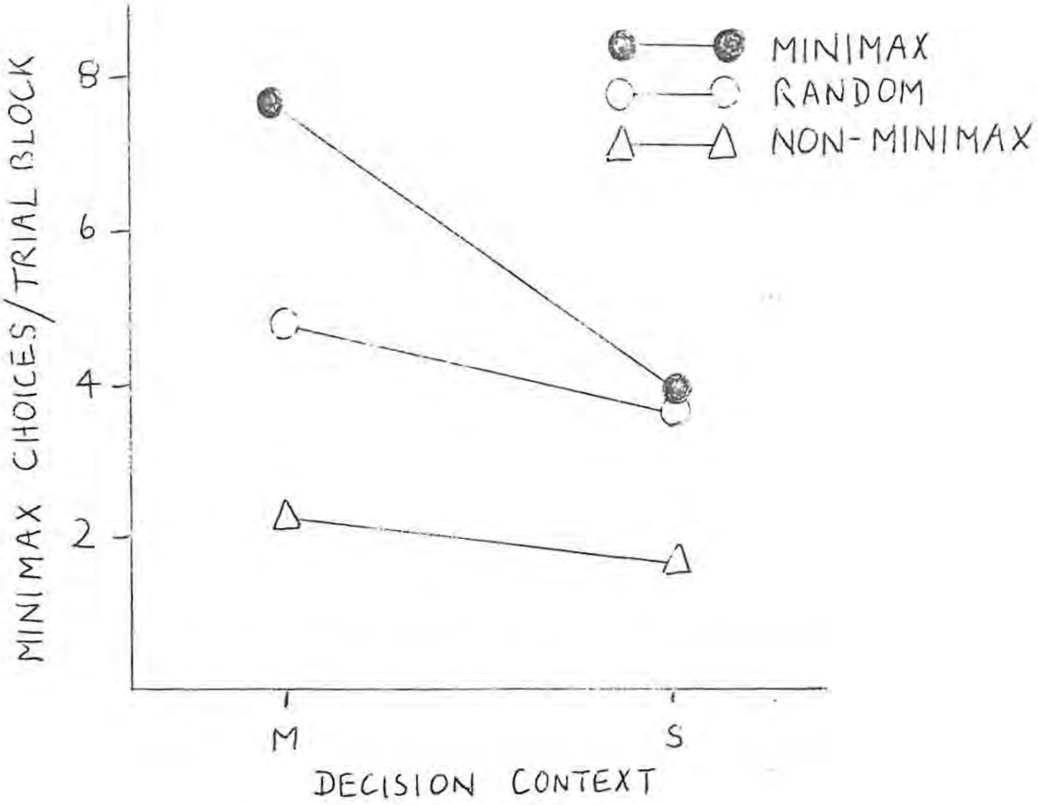


Fig. 3.2. Minimax Choices in Matrix and Simulation Decision Contexts in Response to Minimax, Random, and Non-minimax Opponents' Strategies.

The ordinal relationship between the effects of the three Opponents' Strategies on the subjects' choices is the same in both matrix and simulation Decision Contexts, but the differences are much more pronounced in the matrix than in the simulation conditions. The smaller frequency of minimax choices in the simulation than in the matrix Decision Contexts is largely due, as can be seen from Figure 3.2., to the subjects' responses to random and particularly minimax Opponents' Strategies.

The highly significant interaction Opponent's Strategy x Trial Blocks is illustrated in Figure 3.3.

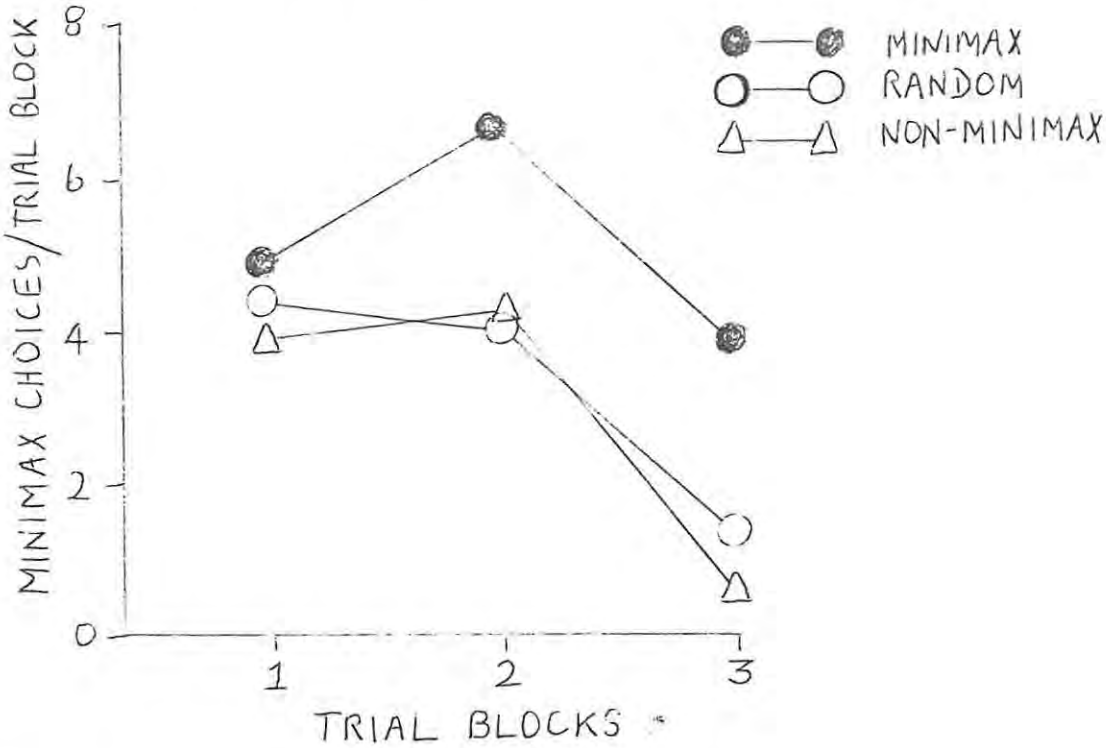


Figure 3.3. Minimax Choices in Three Trial Blocks in Response to Minimax, Random, and Non-minimax Opponents' Strategies.

It is evident from an inspection of Figure 3.3 that the tendency for the minimax Opponent's Strategy to elicit greater frequencies of minimax choices from the subjects was relatively slight on early trials and became more pronounced on later trials. This is in line with expectations, since the only way a subject has of determining his opponent's general strategy is by observing the latter's behaviour over the course of time and making inductive inferences, which are likely to become stronger as time progresses.

Figure 3.4 is of help in interpreting the highly significant three-way interaction Opponent's Strategy x Decision Context x Trial Blocks.

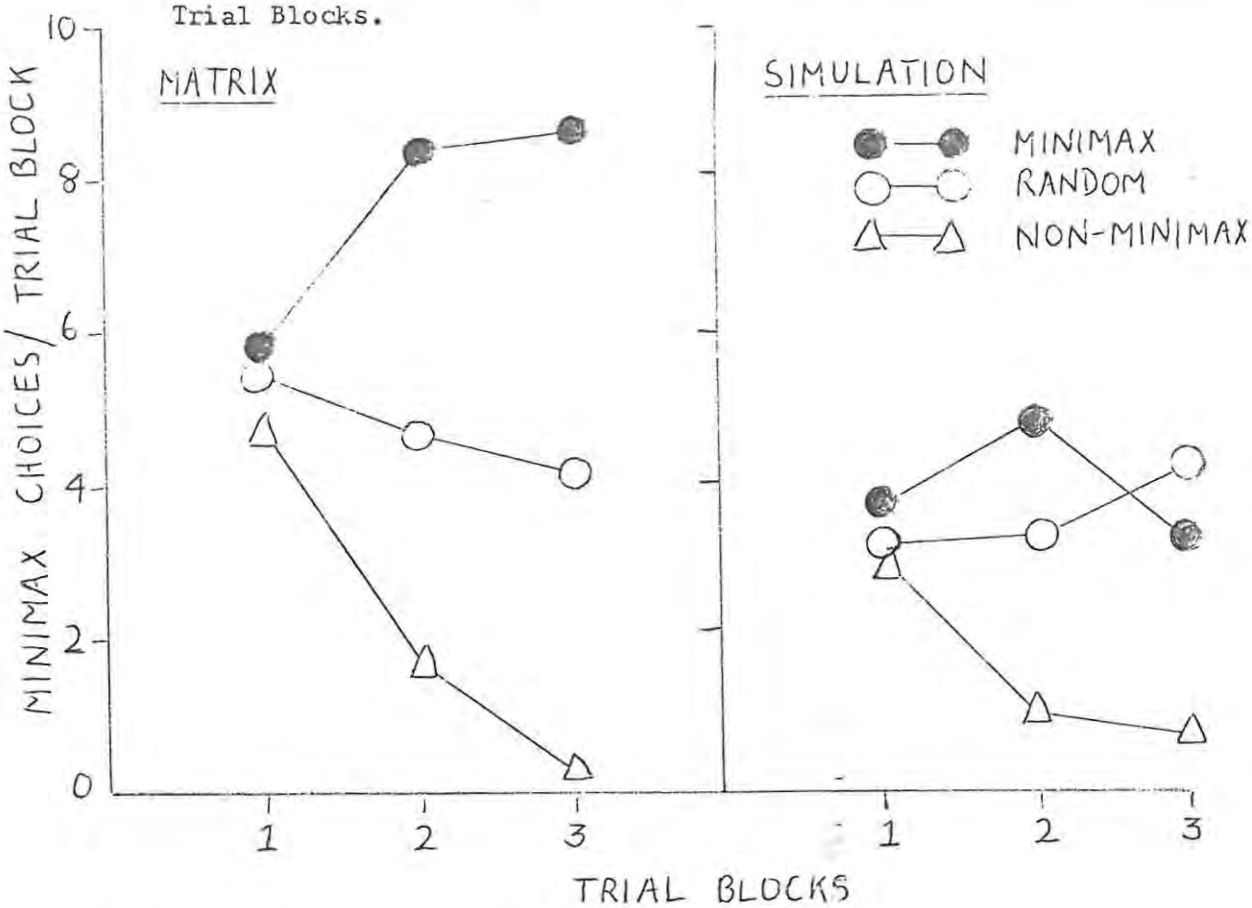


Figure 3.4. Minimax Choices in Matrix and Simulation Decision Contexts in Response to Minimax, Random, and Non-minimax Opponents' Strategies in Three Trial Blocks.

The interaction arises from the uniqueness of the third Trial Block: in the matrix Decision Context, only subjects confronted with a minimaxing adversary increased their own minimax choices in the final Trial Block, while in the simulation Decision Context, only subjects confronted with a randomizing adversary did so. This finding is somewhat surprising and suggests that subjects in the simulation conditions, at least when faced with minimaxing and randomizing opponents, may not have adhered to the payoffs presented to them in terms of their subjective utilities. This conjecture is tested directly by a series of analyses described below. In order first of all to clarify the change in the subjects' behaviour over trials apparent in Figure 3.4., however, a simple Time Series Analysis was performed on the data.

Time Series Analysis. The Time Series Analysis used in this experiment did not go beyond an elementary component analysis (Brown, 1963; Spiegel, 1961, Ch. 16): it consisted simply of calculating the 15-trial and 5-trial unweighted moving averages of the subjects' minimax choices in the various treatment conditions. The mathematical model used assumes that $Y = T + C + I$, where Y is the random variable under investigation, and T, C and I are the secular trend, cyclic and irregular components of its variation. Multiplicative models are sometimes used in the analysis of economic data (e.g. in the calculation of seasonally adjusted unemployment statistics), but an additive model is more appropriate in this case. The 15-trial moving averages reveal the characteristic secular movements in the subjects' behaviour over trials while suppressing irregular and cyclical tendencies in the data: they reveal $Y - C - I = T$. The 5-trial moving averages suppress the irregular component but reveal cyclical and secular movements $Y - I = T + C$. The 15-trial moving averages are presented in Table 3.2 and the 5-trial moving averages are shown graphically in Figure 3.5.

Table 3.2: Fifteen-trial Unweighted Moving Averages of Frequencies of Minimax Choices in Six Treatment Conditions

Matrix

Minimax 3.38, 3.52, 3.67, 3.88, 4.04, 4.12, 4.19, 4.24,
4.21, 4.23, 4.21, 4.26, 4.33, 4.28, 4.33, 4.26.

Random 2.26, 2.47, 2.43, 2.48, 2.50, 2.45, 2.36, 2.31,
2.29, 2.24, 2.19, 2.19, 2.17, 2.09, 2.19, 2.19.

Non-minimax 1.86, 1.69, 1.55, 1.40, 1.17, 0.98, 0.93, 0.86,
0.74, 0.62, 0.55, 0.52, 0.50, 0.43, 0.38, 0.38.

Simulation

Minimax 2.17, 2.33, 2.26, 2.21, 2.37, 2.26, 2.31, 2.24,
2.14, 2.17, 2.14, 2.10, 2.05, 1.98, 1.86, 1.74.

Random 1.52, 1.55, 1.64, 1.62, 1.67, 1.67, 1.64, 1.67,
1.64, 1.69, 1.71, 1.81, 1.81, 1.76, 1.67, 1.64.

Non-minimax 1.26, 1.19, 1.07, 0.95, 0.93, 0.74, 0.67, 0.62,
0.57, 0.53, 0.48, 0.43, 0.36, 0.29, 0.31, 0.36.

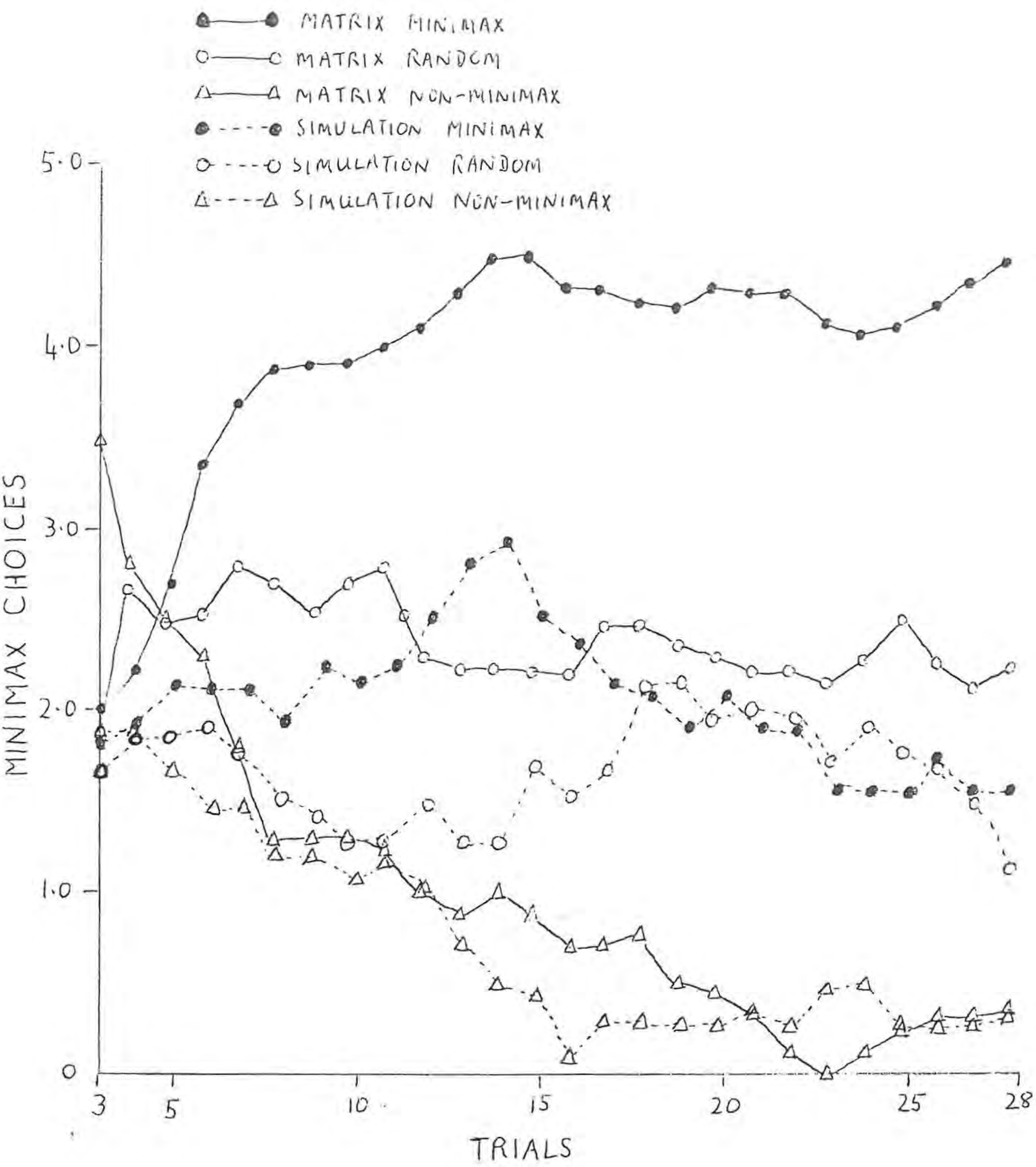


Figure 3.5. Five-trial Unweighted Moving Averages of Frequencies of Minimax Choices in Six Treatment Conditions.

An examination of Table 3.2 and Figure 3.5 reveals firstly that the movement of subjects' minimax choices in response to a minimax Opponent's Strategy is upward in the matrix conditions but takes the form of an inverted-U in the simulation conditions. This agrees essentially with the impression conveyed by Figure 3.4. It shows secondly that subjects' minimax choices decline steadily in response to a non-minimax Opponent's Strategy in both the matrix and simulation conditions, which also confirms the impression created by the interaction in Figure 3.4. It shows thirdly, however, that the movement in subjects' minimax choices in response to a random Opponent's Strategy is cyclical (approximately sinusoidal) in both matrix and simulation conditions and does not manifest any clear upward or downward secular movement as implied by the interaction graphs in Figure 3.4.

In the light of the component Time Series Analysis, the three-way interaction Opponent's Strategy x Decision Context x Trial Blocks can now be interpreted more accurately. It is evident that this interaction is due primarily to the differing responses of subjects to the minimax Opponent's Strategy: in the matrix conditions, the subjects' minimax choices increased steadily over time as would be expected on the basis of common sense, but in the simulation conditions their minimax choices rose sharply for the first half of the trials and then fell sharply for the rest. Since minimax choices are clearly in the subjects' interests when faced with a minimax Opponent's Strategy, their behaviour during the second half of the trials in the simulation condition confirms the suspicion that they had changed the rules of the game by introducing extraneous utilities into the situation. This suspicion motivated the analyses which follow.

Payoff-satisfaction correlations. If a subject behaves strictly according to the payoffs presented to him by the rules of the game, the correlations between payoffs and satisfaction ratings should approach unity. Low correlations suggest that subjects have introduced extraneous utilities into the game, thereby effectively altering the payoff structure: a subject may, for example, decide in the simulation condition that it would be immoral or cowardly to refuse to debate even though he knows he would gain votes by doing so. It was impressed on all subjects that the payoffs presented to them should be the "sole consideration" which should determine their choices, but it was felt to be worth while to check whether they in fact adhered to this admonition.

Product-moment correlation coefficients were therefore calculated separately for each subject between payoffs and satisfaction ratings for the 30 trials. These correlations are tabulated in Appendix F. The correlations turned out nearly all to be positive (there were two negative correlations, $r = -.33$ and $r = -.03$), but for only 11 of the 84 subjects were the correlations perfect ($r = 1.00$). The grand mean of the corrêlations was $r = .61$, indicating that there was in general a reasonably strong relationship between objective payoffs and subjective utilities. In the light of these results it was decided to examine whether the correlations were significantly different in different treatment conditions.

An Analysis of Variance of these correlations was therefore performed in order to determine whether the independent variables Opponent's Strategy and Decision Context had any significant effects on the tendency of subjects to introduce extraneous utilities into the game. The results of this analysis are given in Table 3.3.

Table 3.3: ANOVA Summary Table: Mean Payoff-Satisfaction Correlations (Opponent's Strategy x Decision Context) N=84

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
A(Opponent's Strategy)	10236.45	2,78	5118.23	785.07	6.52	$p < .01$
B(Decision Context)	12337.19	1,78	12337.19	785.07	15.71	$p < .001$
AB	10305.17	2,78	5152.58	785.07	6.56	$p < .01$
Total	94113.95	83				

Opponent's Strategy had a significant effect on the correlations: the highest mean correlation occurred when the subjects were confronted with a random Opponent's Strategy (.84) followed by conditions involving a non-minimax Opponent's Strategy (.65), with the minimax Opponent's Strategy eliciting the lowest mean correlation (.33). *A posteriori* Tukey tests revealed that all three of the differences between these means were significant beyond $p < .05$. These results are not surprising since it is precisely when an opponent is behaving in a highly predictable manner that subjects are most likely to reflect on extraneous factors in the situation, and when subjects are consistently losing points or votes (against a minimaxing opponent) they are most likely to seek outside the given payoffs for alternative sources of satisfaction.

Decision Context also had a highly significant though small effect on the correlations: this arose from the fact that the correlations were higher in the matrix conditions (with a mean of .62) than in the simulation conditions (with a mean of .59). This is exactly what would be expected from a consideration of the rich context of information surrounding the simulation game compared with the matrix game; there is evidently much more scope in the former for subjects to introduce moral and other extraneous considerations into the situation.

Figure 3.6 illustrates the significant interaction Opponent's Strategy x Decision Context for the payoff-satisfaction correlations.

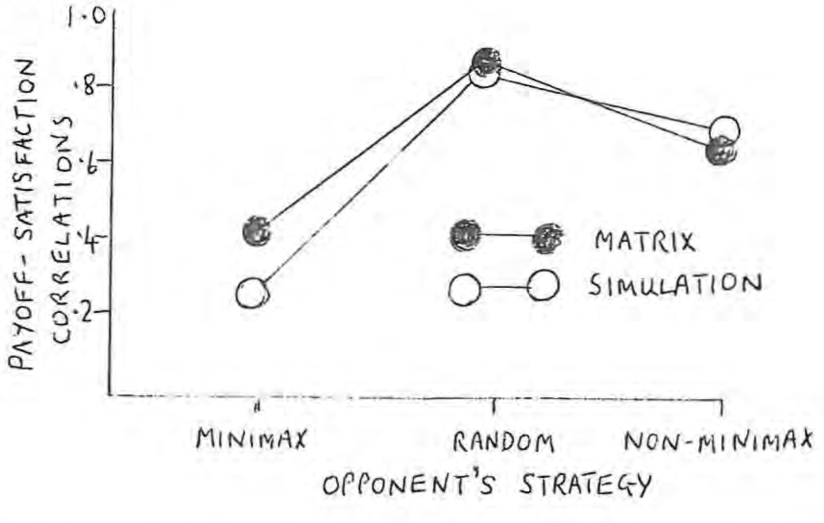


Figure 3.6. Payoff-satisfaction Correlations in Six Treatment Conditions.

The interaction may be interpreted as showing that the matrix conditions produced higher payoff-satisfaction correlations than the simulation conditions, except when the opponent adopted a non-minimax strategy where the reverse was true. A possible reason for this is that the subjects in the simulation conditions may have felt less embarrassed or guilty about winning payoffs through the opponent's self-destructive non-minimax behaviour than they may have done in the matrix conditions, because in the former case the opponent's "irrational" behaviour could be interpreted as immoral or cowardly -- he refused to debate even though he could gain votes by doing so.

Supplementary Analysis of Variance. In view of the many imperfect correlations between payoffs and satisfaction ratings, it is clear that the subjects were not all playing the game which was presented to them. Since many of them had modified the utilities, interpretation of their choice behaviour presents problems: subjects choices which are labelled "minimax" or "non-minimax" may not correspond to minimax and non-minimax choices in the unknown latent game structures which were governing their behaviour. It was therefore decided to re-analyze the minimax choices of subjects in different treatment conditions, using only those subjects whose payoff-satisfaction correlations were high. This was intended to give an indication of whether and to what extent the findings of the main analysis were artifacts of the subjects' deviations from the rules of the game.

The three subjects with the highest correlations in each of the six treatment conditions were used in this analysis. Most of the subjects used had produced payoff-satisfaction correlations in excess of r = .90, though in one treatment condition (minimax Opponent's Strategy, simulation Decision Context) two subjects with somewhat lower correlations had to be used (r = 1.00, r = .55, r = .41). The results of this analysis are given in Table 3.4.

Table 3.4: ANOVA Summary Table: Mean Minimax Choices of Subjects With High Payoff-satisfaction Correlations (Opponent's Strategy x Decision Context x Trial Blocks) N=18

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	FF Ratio	Significance Level
A(Opponent's Strategy)	109.78	2, 12	54.89	6.22	8.82	$p < .005$
B(Decision Context)	174.24	1, 12	174.24	6.22	28.00	$p < .001$
C Trial Blocks	27.11	2, 24	13.56	2.42	5.61	$p < .025$
AC	19.44	4, 24	4.86	2.42	2.01	n.s.
AB	77.48	2, 12	38.74	6.22	6.23	$p < .01$
BC	2.82	2, 24	1.41	2.42	.58	n.s.
ABC	77.30	4, 24	19.32	2.42	8.00	$p < .001$
Total	620.83	53				

These results are reassuringly similar to those of the main analysis given above (Table 3.1). As in the analysis using all the subjects, there is a highly significant main effect due to Opponent's Strategy. The mean number of minimax choices per trial block is highest ($\overline{X} = 5.28$) against a minimax Opponent's Strategy, intermediate ($\overline{X} = 4.06$) against a randomizing opponent, and lowest

($\overline{X} = 1.83$) against a non-minimax Opponent's Strategy. A posteriori analysis with the Tukey test revealed that the lowest mean was significantly different from each of the others beyond $p < .05$, but that the highest was not significantly different from the intermediate mean. Apart from the failure of this last difference to attain significance, these results essentially confirm those of the main analysis, and the means are very similar.

The highly significant main effect due to Decision Context similarly confirms the main analysis: there is nothing worth commenting upon here.

The significant decline in minimax choices over Trial Blocks found in the main analysis also emerges as significant in this subsidiary analysis. Subjects made more minimax choices in Trial Block 1 ($\overline{X} = 4.28$) than in Trial Block 2 ($\overline{X} = 4.16$) or Trial Block 3 ($\overline{X} = 2.72$). These means are again similar to those of the main analysis. A posteriori analysis with the Tukey test revealed that in this case one of the differences (between Trial Block 1 and Trial Block 2) was not significant beyond $p < .05$, but apart from this the overall pattern resembles closely the results of the main analysis.

The significant interaction Opponent's Strategy x Decision Context found in the main analysis was replicated in this subsidiary analysis. The significant interaction Opponent's Strategy x Trial Blocks found in the main analysis however failed to attain significance in this analysis. The Decision Context x Trial Blocks interaction was not significant in either analysis.

A significant three-way interaction was found in this analysis as in the larger one. Once again the most striking feature of this interaction is the increase in minimax choices against a minimax Opponent's Strategy over Trial Blocks in the matrix conditions but not in the simulation conditions. In this subsidiary analysis, this difference is even more pronounced than in the main analysis and tends to become manifest between the first and second as well as between the second and third Trial Blocks.

The overall pattern of the results is therefore remarkably similar whether all 84 subjects are used in the analysis or only those who manifested high payoff-satisfaction correlations. The suspicion that the apparently self-damaging behaviour of the subjects when confronted with a minimax Opponent's Strategy in the later trials of the simulation conditions may have been due to the extraneous utilities which they evidently introduced into the game in this treatment condition is not supported by the subsidiary analysis; a firm conclusion cannot be reached, however, since only one subject in this treatment condition had a payoff-satisfaction correlation in excess of $r = .90$. The suspicion therefore remains.

Post-experimental interview. A perusal of the comments made by the subjects in the post-experimental interviews brought to light four types of remark which tended to occur relatively frequently in the subjects' replies to the questions posed by the experimenter. Some subjects declined to make any meaningful comments, and others gave idiosyncratic replies, but the most common remarks fell into the following categories:

(a) The test was "rigged", "fixed", etc.; there was "no real opponent"; "incorrect feedback" was given. This type of remark was made by 12 of the 84 subjects.

(b) The task was "unfair", "biased", "one-sided" etc. Eight subjects made remarks like these.

(c) The partner was "clever", "sensible", "did as I would have done" etc. Such remarks were made by 20 subjects.

(d) The partner was "silly", "stupid", "lacking in understanding" etc. Remarks of this kind were made by 25 subjects.

A classification was performed without knowledge of which treatment condition each reply belonged to. The frequency with which each type of remark arose in each of the six treatment conditions is shown in Table 3.5.

Table 3.5: Classification of Post-experimental Interview Responses by Treatment Condition

	Opponent's Strategy		
	Minimax	Random	Non-minimax
(a) The test was "rigged" etc.			
Matrix Decision Context	0	2	3
Simulation Decision Context	0	2	5
(b) The test was "unfair" etc.			
Matrix Decision Context	3	5	0
Simulation Decision Context	0	0	0
(c) The partner was "clever" etc.			
Matrix Decision Context	8	1	0
Simulation Decision Context	9	2	0
(d) The partner was "silly" etc.			
Matrix Decision Context	0	6	6
Simulation Decision Context	2	2	9

These frequencies are too small to allow significant differences to be tested by means of the Chi Square test, and they are not appropriate for the use of Fisher's Exact Probability test since the classifications are not 2 x 2. Pairwise comparisons between the frequencies can, however, be evaluated for significance with the use of the binomial probability density function. From first principles, the probability of obtaining x cases in one category and N - x cases in the other can be shown by combinatorial analysis (e.g. Feller, 1968, Vol. I, Ch. 2) to be given by

$$p(x) = \frac{N!}{x!(N - x)!} p^x q^{n-x}$$

where p is the proportion of cases expected to be in one category on the basis of chance and $q = 1 - p$ is the proportion expected in the other category. By summation, it is therefore possible to determine the probability of obtaining a split between two categories as extreme or more extreme than the observed split.

Regarding category (a) above (the test was "rigged" etc.), there was no difference in the frequency between the matrix and simulation conditions. Comparing the frequency of such remarks in response to a "rational" (minimaxing) versus "irrational" (randomizing or non-minimaxing) opponent revealed a significantly greater frequency in the latter category ($p < .02$, two-tailed). This is, of course, the only natural way to dichotomize the three Opponents' Strategies having regard to formal game theory. The results are in line with expectations since an "irrational" opponent is most likely to arouse suspicions regarding the genuineness of the task situation.

The frequency of remarks in category (b) above (the test was "unfair" etc.) was significantly greater in the matrix than in the simulation conditions ($p < .008$, two-tailed), but was not significantly different in response to a "rational" versus an "irrational" Opponent's Strategy. The significant difference no doubt arises from subjects' expectations that a formal "game" should be fair in the sense of giving both players an equal chance of success. In lifelike conflicts the payoffs are seldom balanced, however, thus the subjects in the simulation conditions were less disturbed by the "unfairness" of the situation.

Remarks in category (c) above (the partner was "clever" etc.) revealed no significant difference between matrix and simulation conditions. They were, however, significantly more frequent in response to a "rational" than an "irrational" Opponent's Strategy ($p < .00002$, two-tailed). This is of course exactly what one would expect on the assumption of common sense on the part of the subjects and a knowledge of formal game theory.

Finally, remarks in category (d) above (the partner was "silly" etc.) revealed no significant difference between the matrix and simulation conditions, but they were significantly more frequent in response to an "irrational" than a "rational" Opponent's Strategy ($p < .008$, two-tailed). This finding makes sense for the reasons discussed above.

3.4. Discussion

The most important feature of this experiment, and the one which sets it apart from all previous research on two-person zero-sum games was the comparison between subjects' responses to an abstract matrix version of the game and their responses to a structurally equivalent lifelike simulation. The formal properties of the decisions and payoffs were identical, and subjects were instructed to operate strictly according to the given payoffs, but the hypothesized difference between behaviour in these two situations was richly confirmed. The results of this experiment therefore throw into sharp relief the problem raised in the Introduction concerning the generalizability and ecological validity of matrix games with respect to real life situations.

The simulation used in this experiment was of course artificial and unrealistic to a degree. The subjects were required to imagine that they were in a predicament which they were not in fact in, they were barred from communicating with their adversaries in a manner which is somewhat unusual in everyday conflict situations, and they made their choices under laboratory conditions and rather more rapidly than they would normally have had to do in the natural course of events. The simulation nevertheless represented a realization of the strategic structure of the game which was clearly more lifelike than that represented by the traditional payoff matrix. It is not unreasonable to assume that the simulation elicited behaviour from the subjects which more closely resembled their behaviour in a real life situation with the structural properties of the given game than did the matrix. The results give no cause to assume that the behaviour of subjects in matrix games is predictive of their behaviour in structurally equivalent life situations.

The nature of the difference between the subjects' behaviour in the matrix and simulation conditions was somewhat complex. It may be roughly summarized by saying that the subjects behaved more sensibly in the matrix conditions. Although the nature of the difference was not predicted in advance, it is not difficult to make sense of it in hindsight. In the matrix game, all irrelevant and potentially distracting information is removed and a subject is in a position to make his choices strictly according to the payoffs, while in the simulation game the rich informational context tends to obscure its essential strategic properties. When confronted with a minimaxing opponent, subjects in the matrix conditions gradually converged over trials towards the optimal minimax strategy, while subjects in the simulation conditions started off by doing likewise but gradually drifted away from minimax after the half-way mark. These different time course effects were vividly revealed by the Time Series Analysis.

When confronted with a minimax Opponent's Strategy, the nature of the game used meant that subjects were faced with certain loss whatever they chose to do. In the matrix conditions they sensibly chose in such a manner that their losses were minimized as time went on. In the simulation conditions, however, they responded by abandoning the given payoffs of the game and altering its subjective utility structure. It was noteworthy that only subjects who were confronted with a minimax Opponent's Strategy in the simulation conditions made frequent and large alterations in subjective utilities. This was abundantly clear from the analysis of the payoff-satisfaction correlations, although it is true that the correlations tended to be slightly lower in all simulation conditions than in the corresponding matrix conditions.

Of particular interest is the behaviour of subjects when faced with an "irrational" opponent according to the minimax rule of formal game theory. When exposed to random or non-minimax Opponents' Strategies, the results showed that the subjects still chose more sensibly in the matrix than in the simulation conditions, although the general pattern of their behaviour was similar in both cases. In both matrix and simulation conditions against an "irrational" opponent who consistently used his non-minimax strategy, subjects tended to make an increasing number of non-minimax choices as time went on; this was in line with their own self-interests. When faced with an opponent whose choices were random, in which case a prescription of the sensible way to respond is more problematical, the behaviour of subjects in both matrix and simulation versions of the game was revealed by the Time Series Analysis to be cyclical.

The results found in the matrix conditions were broadly in line with previous findings in this area. The subjects in general failed to adopt their minimax strategy consistently, but they tended to converge towards it over trials unless their opponents deviated from their own minimax strategy. The findings with respect to the effects of deviations from minimax on the part of the opponent are in line with Brayer's (1964) earlier results. The results of the present experiment however extend those of Brayer by showing how subjects in the matrix game made more minimax choices against a minimaxing opponent than against a randomizing opponent, and also more minimax choices against a randomizing opponent than against a consistent non-minimaxer. In the simulation game, furthermore, the same ordinal relationships held good, but the effects were very small and very complex in their interaction with Trial Blocks.

In the present experiment, the subjects were playing either for points or for imaginary monetary profits. In many previous experiments in this area, on the other hand, real monetary rewards have been used. It is possible that different results might have emerged had tangible incentives been incorporated into this experiment, but the impression conveyed by the post-experimental interviews, and indeed by the pattern of the results themselves, is that the subjects took the task very seriously and behaved in a meaningful way despite the absence of tangible incentives. The subjects made choices in the course of the experiment, and comments after it was over, which are entirely consistent with the belief that they understood what was required of them, and in general they found the task engaging and believable.

The question naturally arises whether the subjects' behaviour would have been the same had an entirely different lifelike simulation been devised with a formal structure identical to the one used in this experiment. The answer to this question is not known, but there are good reasons for believing that the answer would be negative. Different simulations would no doubt provide different kinds of distractions to the subjects, thereby impairing their ability to abstract the essential strategic structure of the game in different ways and perhaps to different degrees.

Finally, it is worth pondering whether differences might be found between subjects' behaviour in abstract and lifelike versions of other types of games, particularly mixed-motive games. Mixed-motive games have attracted a great deal of research, largely on account of their apparent relevance to a wide range of everyday social conflicts, but the results of the present experiment have cast a long shadow of doubt over the ecological validity of these studies and raised the question of whether they would have been the same had lifelike situations been used instead of abstract matrices. An attempt will be made to dispel some of the darkness surrounding this question in Experiments II, III and IV.

CHAPTER FOUR

4. EXPERIMENT II: CHOICES IN STRUCTURALLY EQUIVALENT ABSTRACT AND LIFELIKE PRISONER'S DILEMMA GAMES

4.1. Introduction

The overwhelming majority of experimental studies of gaming behaviour have been devoted to one particular type of two-person, two-choice mixed-motive game, namely Prisoner's Dilemma (Pruitt & Kimmel, 1977), although there is some evidence that researchers have recently begun to pay more attention to other types of game (Davis, Loughlin & Komorita, 1976). One important reason for the relative popularity of Prisoner's Dilemma has been the belief that this game possesses a strategic structure which is reflected in a wide range of conflict situations encountered in everday life. The implication is that research on Prisoner's Dilemma might be useful in illuminating the behaviour of people in such everyday conflicts. The premise seems unassailable since it is not difficult to provide numerous examples of commonplace life situations whose strategic structure is evidently Prisoner's Dilemma (the litany will not be repeated here), but the implication is open to question.

The belief that research on the behaviour of subjects in experiments using the Prisoner's Dilemma throws light on cooperative and competitive behaviour in other social situations has recently come under critical scrutiny from several commentators (e.g. Nemeth, 1972; Schlenker & Bonoma, 1978), and the results of the experiment using a zero-sum game described in Chapter 3 above has cast further doubt on the validity of this belief. One important source of doubt arises from the highly abstract and unnatural tasks which have been presented to the subjects in virtually all published experiments on Prisoner's Dilemma. In most experiments, the subjects have been provided with an abstract payoff matrix and have been required to make a number (often hundreds) of choices between alternatives labelled Left and Right, Blue and Green etc. Experiments of this kind naturally invite questions about the ecological validity of their findings.

A handful of experiments (reviewed in Section 2.3 above) have attacked the problem of ecological validity directly, but, with one exception, they have been vitiated by design flaws, errors in the statistical analyses of their results, and unwarranted inferences from the data. The solitary exception, an experiment by Eiser & Bhavnani (1974), provided extremely provocative results which suggested that subjects' interpretation of the game situation has a significant effect on their cooperative choice behaviour. In this experiment, however, no attempt was made to compare the behaviour of subjects in conventional matrix games and in lifelike situations with similar strategic properties. A conventional payoff matrix was used in all treatment conditions, but the subjects were given instructions which in some cases encouraged them to interpret the matrix in terms of various everyday political or economic conflict situations.

A stringent test of the ecological validity of Prisoner's Dilemma experimental findings necessitates the comparison of the choices made by subjects in a conventional matrix version of the game with their choices in a lifelike conflict situation whose payoff structure corresponds exactly to that used in the matrix game. The experiment reported below used four radically different but structurally equivalent versions of Prisoner's Dilemma, one of which involved a conventional payoff matrix, and another of which used a simulation of a lifelike decision-making situation.

The other two treatment conditions used in this experiment were included in order to investigate the effects of monetary incentives and linear transformations of the payoff structure. Previous research (reviewed above in Section 2.3) has provided equivocal findings regarding the effects of monetary incentives on subjects' choice behaviour. A treatment condition was therefore included in this experiment which was, like the matrix condition, entirely abstract in character, but which provided the subjects with an aopportunity of earning meaningful amounts of money through their strategic choices. The payoff structure in this positive incentive condition was identical to the payoff structure in the matrix condition, thus enabling the effects of monetary incentives to be investigated in a pure form. A second incentive condition was built into the experimental design in order to investigate, apparently for the first time, the effects of a particular kind of linear transformation of the payoff structure on subjects' choices. In this negative incentive treatment condition, instead of playing to win points or monetary units, the subjects were confronted with a situation in which they had to play to conserve what they already had. They were provided with an initial monetary stake equivalent in value to the maximum possible winnings in the positive incentive condition, but the payoffs were all either zero or negative; that is to say, after each trial the experimenter removed an appropriate sum of money from them according to this transformed payoff matrix. From a purely logical point of view, the choices facing the subjects in the positive incentive and negative incentive conditions were identical: they stood to win (or save) the same amount of money respectively in each outcome. From a psychological point of view, however, there are strong reasons to believe that the subjects may not perceive the situation similarly, and significant differences in their strategic behaviour may emerge.

The payoff structures of the games used in the four treatment conditions bore at least an interval-scale equivalence to one another; in three of the four conditions (positive incentive, matrix, and simulation) the equivalence was maintained at a ratio-scale level, and in the negative incentive condition the payoffs were obtained by subtracting a constant from the matrix elements used in the other conditions. Apart from the changes which were necessitated by the manipulation of the dependent variable (Decision Context), the treatment of the subjects in the four conditions was identical. They were randomly assigned to treatment conditions and they made their choices in eactly the same way irrespective of which conditions they were assigned to. Any significant differences in the strategic behaviour of subjects between treatment conditions may therefore be attributed to the effects of the independent variable, i.e. differences in the decision contexts in which the game was played.

Following Eiser & Bhavnani's (1974) finding that subjects given an interpretation of Prisoner's Dilemma involving economic competition behaved less cooperatively than subjects given no interpretation, it was hypothesized that subjects in the simulation condition in this experiment would show less cooperation that subjects in the other (abstract) conditions. The basis for this hypothesis was the fact that the lifelike simulation involved a hypothetical situation of economic conflict: two restauranteers competing for the same clientele have to decide whether or not to provide expensive floor shows in their respective establishments. It may be assumed that such a situation engages cultural values encouraging competitiveness in our culture, and that the subjects are likely to feel liberated in this situation to manifest a degree of ruthlessness which they might be inhibited to display in the abstract decision contexts.

It was further hypothesized that subjects in the two monetary incentive decision contexts would manifest a higher frequency of cooperative choices than subjects in the matrix decision context.

This hypothesis arose partly from an examination of the literature on this question (see Section 2.3): although the evidence is contradictory and confusing, when significant effects have been reported they have almost always indicated greater cooperation in subjects who play for money than subjects who play for points. A second reason for this hypothesis was the simple common sense idea that subjects in Prisoner's Dilemma situations have more to gain by cooperating when the stakes are high than when there are no tangible rewards involved.

A difference between the choice behaviour of subjects in the positive incentive and negative incentive decision contexts was hypothesized on essentially intuitive grounds. Although they are strategically (indeed logically) identical, the problems facing the subjects in these two task situations are evidently quite unlike each other from a psychological point of view. It was felt that this was likely to manifest itself in differences in the subjects' behaviour in these two situations, but the nature of the difference, in the absence of previous evidence, was not predicted.

Finally, it was hypothesized that the sundry differences mentioned in the paragraphs above would become increasingly distinct over trials. For reasons similar to those mentioned in Section 2.5 above, the game was reiterated only 30 times in each treatment condition. It was felt, however, that this would provide sufficient opportunity for the subjects to become immersed in the game, and as they become more familiar with the consequences of various strategy combinations, there is reason to believe that the peculiar character of each decision context may be reflected with increasing clarity in their strategic choice behaviour. In the light of numerous previous findings in this area, however, a general decline in cooperative choices over trials is to be expected in all treatment conditions, although the unusual nature of some of the decision contexts used in this experiment may refute this generalization.

4.2. Method

Design. A two-factor mixed design, with repeated measures on one factor (Winer, 1962, Ch. 7) was used in this experiment. The first factor, Decision Context, occurred at four levels (PI, NI, M and S) as follows:

PI: In this positive incentive condition, the game was presented in abstract form, but a payoff matrix was not used. The following tape recorded instructions were played to subjects in this treatment condition:

> Your task will consist of making a series of 30 decisions. You may each earn up to 120 half pence (that is 60p) in this experiment if your decisions turn out well, but you may get less. How much money you eventually go home with will depend not only on the decisions which you make, but also on the decisions which the other person makes. You are advised, therefore, to consider each decision carefully.
>
> After each decision you will receive payment in the form of 1, 2, 3 or 4 half pence. In each case your decision, as shown on the cards in front of you, will be either R for Right or L for Left.

> The rules governing payment are as follows: if you both choose R, you will each receive 2 coins. If you both choose L, you will each receive 3 coins. But if one of you chooses R and the other chooses L, then the one who chooses R will receive 4 coins and the one who chooses L will receive 1 coin. I shall repeat these details; please feel free to make notes if you have difficulty memorizing them. [This paragraph was repeated omitting the final sentence.]

NI: In this negative incentive condition, the Decision Context was identical except that the absolute value of the payoffs was zero or negative rather than positive. The payoffs were derived from those of the PI condition by subtracting four units from each payoff. An interval-scale correspondence between the payoff structures is thus maintained: in each payoff structure, not only is the defining set of Prisoner's Dilemma inequalities $S < P < R < T$ (see Section 1.5 above) maintained, but in addition $S + 1 = P$, $P + 1 = R$, $R + 1 = T$. The subjects in this treatment condition received the following tape recorded instructions.

> Your task will consist of making a series of 30 decisions. You may each earn up to 120 half pence (that is 60p) in this experiment if your decisions turn out well, but you may get less. How much money you eventually go home with will depend not only on the decisions which you make, but also on the decisions which the other person makes. You are advised, therefore, to consider each decision carefully.
>
> To start with, you will each receive 120 half pence coins. After each decision you will have to forfeit 3, 2, 1 or no coins. In each case your decision, as shown on the cards in front of you, will be either R for Right or L for Left. The rules for forfeiting coins are as follows: if you both choose R, you will each forfeit 2 coins. If you both choose L, you will each forfeit 1 coin. But if one of you chooses R and the other chooses L, then the one who chooses R will forfeit nothing, but the one who chooses L will

> forfeit 3 coins. I shall repeat these details; please feel free to make notes if you have difficulty memorizing them. [This paragraph was repeated omitting the final sentence.]

M: In the matrix condition, a conventional payoff matrix, similar to those used in most previous experiments on Prisoner's Dilemma, was used. The structure of the matrix is shown in Figure 4.1.

$$\begin{array}{c|c|c|}
 & L & R \\
\hline
L & 3,3 & 1,4 \\
\hline
R & 4,1 & 2,2 \\
\hline
\end{array}$$

Figure 4.1. Payoff Matrix Used in Matrix Decision Context.

As presented to the subjects, the payoff matrix was drawn on a sheet of A4 paper, using blue ink for Row's strategies and green ink for Column's. Diagonal lines from top left to bottom right of each cell were used to separate the payoffs to each player. The following tape recorded instructions accompanied the payoff matrix:

> Your task will consist of making a series of 30 decisions. You may each earn up to 120 points if your decisions turn out well, but you may get less. The number of points you finally end up with will depend not only on the decisions which you make, but also on the decisions which the other person makes. You are advised therefore to consider each decision carefully.
>
> After each joint decision you will receive 1, 2, 3 or 4 points. Your decision as shown on the card in front of you will be either R for right or L for left. The scheme for awarding points is summarised on the payoff diagram. The payoffs to one of

> you are represented by the blue figures and to the other of you in green figures. You can tell which colour applies to you by the colour of the cards in front of you. This will also tell you if you are choosing between the rows marked L and R, this is if you are blue, or the columns marked L and R if you are green. By examining the payoff diagram you can easily see what the outcome of each decision will be.
>
> If, for example, blue and green both choose L then blue gets 3 points and green also gets 3 points. If blue and green both choose R then they each gain 2 points. If blue chooses L and green chooses R then blue gets 1 point and green gets 4 points. Finally, if blue chooses R and green chooses L then blue gains 4 points and green gains 1 point. [This last paragraph was repeated.]

S: In the simulation condition, the game was presented as a lifelike simulation of a decision-making predicament facing two competing restauranteers. The payoffs built into this simulation were designed so as to bear a ratio-scale equivalence to those used in the PI and M conditions. In all three cases T, R, P and S occur in the ratios 4:3:2:1. The payoffs in this condition, like those used in the PI and M conditions, bear an interval-scale equivalence to the payoffs in the NI condition. The subjects in this condition received the following tape recorded instructions:

> Your task will consist of making a series of 30 decisions. You may do very well for yourself if your decisions turn out well, but you may be less successful. The outcome in each case will depend not only on the decisions which you make, but also on the decisions which the other person makes. You are advised, therefore, to consider each decision carefully.
>
> Your decisions will be based upon the following hypothetical situation. You have just taken over the ownership of a small restaurant which specializes in Greek food. There is another Greek restaurant just opened in the same town which is in

> every way similar to yours, and which caters for the same clientele. The problem facing you at the beginning of each week is whether to provide a (rather costly) floor show or not, and your sole consideration is to maximize profits. In each case your decision, as shown on the cards in front of you, will be SHOW or NO SHOW.
>
> The following facts are known to both of you from the experience of the previous owners: if you both provide floor shows, you will both make exactly the same standard profit during that week. If neither of you provides a floor show, your overheads will be less and so your profits will both be 50 per cent higher than the standard. If, however, one of the restaurants provides a floor show and the other does not, then the one which provides the floor show will attract business from the other and will make double the standard profit for the week, while the restaurant without a floor show will make half the standard profit. I shall repeat these details; please feel free to make notes if you have difficulty memorizing them. [This paragraph was repeated omitting the final sentence.]

The subjects in each of the above treatment conditions made 30 successive decisions which, for purposes of analysis, were divided into three Trial Blocks of 10 decisions each. The main dependent variables were the number of cooperative choices made on the first trial in each treatment conditions and the mean number of cooperative choices made in each Trial Blocks. According to the theory of Prisoner's Dilemma (see Section 1.5 above), a cooperative choice was defined as an L choice in the PI, NI and M conditions, and as a NO SHOW choice in the S condition.

Subjects. The subjects were 80 undergraduate and postgraduate students at the University of Leicester, randomly assigned to the four treatment conditions and then randomly assigned to play Row or Column, with the restriction that males were always paired with males and females with females. There were 36 males and 44 females. Ages ranged from 17 to 26 with a median of 20. Students reading Psychology were not included in the sample, and a handful of subjects who turned out in the post-experimental interviews to have some prior acquaintance with the theory of games were discarded together with their partners.

Procedure. Each pair of subjects was tested in a laboratory with a partition separating them. In addition to the appropriate instructions described above, each subject was presented with a score sheet (Appendix A), a decision card, (Appendix B or, with appropriate modification, one similar to Appendix C) and a rating scale (Appendix D). They were given about five minutes to familiarize themselves with the game, after which the experimenter quizzed the subjects and provided supplementary explanations where necessary in the manner described in Section 3.2 above. In the NI treatment condition, each subject was presented with 120 half-pennies. The following tape recorded instructions were then played to the subjects in all treatment conditions:

> You are now going to make a series of 30 joint decisions. Each decision will be made without knowledge of what the other person has chosen, by pointing to one of the two cards in front of you. Your decisions will be irreversible and any attempt to communicate with or indicate your feelings to the other person, that is by sighing or laughing will force the experimenter to terminate the experiment.

> When you have both reached a decision the experimenter will announce the choices. You will each know what the other has chosen and you will be able to work out how each of you has fared in terms of payoffs. You will then record both decisions and payoffs together with a rating of how pleased, displeased you are with that particular decision. The experimenter will show you how to fill in the scoring sheet.

The instructions in the PI treatment condition continued:

> You will be paid the appropriate amount after each decision. Your sole objective is to accumulate as many coins as possible. After 30 trials your money will be converted into more convenient money for you to take home.

In the NI treatment condition, the instructions continued:

> You will receive 120 coins before you begin, and the appropriate amount will be removed after each decision. Your sole objective is to retain as many coins as possible. After 30 trials your money will be converted into more convenient money for you to take home.

In the M treatment condition, there followed:

> You will be able to take stock of your position after each trial. Remember that your sole objective is to accumulate as many points as possible. After 30 trials you will be able to add up your points.

Finally, in the S treatment condition, there followed:

> You will be able to take stock of your position after each trial. Remember your sole objective is to accumulate as much profit as possible. After 30 trials you will be able to see how you have done over the whole 30 week period.

The subjects were then invited to make their first choices. When they had both reached a decision, the experimenter announced the choices and the subjects recorded both choices on the score sheets together with their own payoffs and those of their partners. (This provided an additional check on their understanding of the rules of the game: subjects whose score sheets contained errors were eliminated from the subsequent data analysis.) In the PI and NI conditions, the experimenter then made the necessary monetary exchanges. Before continuing with their second choices, the subjects also recorded on their score sheets their satisfaction with the outcome of that trial according to the code given on the five-point rating scale (Appendix D) from VD (very displeased) to VP (very pleased).

After 30 joint decisions had been made, the experimenter interviewed each subject separately and summarized their responses on the score sheets. Four specific questions were asked in these post-experimental interviews: (a) "How do you feel about the overall results?", (b) "How do you feel about your partner?", (c) "What was your general strategy?", and (d) "Do you have any further comments?". After the post-experimental interview, the subjects were de-briefed and thanked, and the accumulated winnings of those in the PI and NI treatment conditions were converted into more convenient coinage for them to take home.

4.3. Results

Cooperative choices on Trial 1. The first dependent variable to be investigated was the frequency of cooperative choices on the first trial. Since a subject's choice in the first trial cannot influence his partner's choice on the first trial, there are N = 80 independent observations available for statistical analysis. The frequency of cooperative and competitive choices in the four treatment conditions is shown in Table 4.1.

Table 4.1: Cooperative and Competitive Choices on Trial 1 in Four Treatment Conditions (N = 80)

	Treatment Condition			
	PI	NI	M	S
Cooperative Choice	8	9	7	3
Competitive choice	12	11	13	17

A Chi Square test (Siegel, 1956, pp. 175-179) revealed that the frequencies are not significantly different in the four treatment conditions ($\chi^2 = 4.75$, d.f. = 3, $.10 < p < .20$) although the differences are evidently in the expected direction.

It was hypothesized that the S condition would elicit fewer cooperative choices than the other three treatment conditions, since the decision context associated with the S condition may engage competitive motives which are culturally sanctioned in business situations. A comparison, planned in advance, was therefore made between the proportion of cooperative choices on Trial 1 in conditions PI, NI and M (24/60) and the corresponding proportion in the S condition (3/20). A z test (Bruning & Kintz, 1977, pp. 222-224) revealed that this difference was significant ($z = 2.05$, $p < .05$, two-tailed) indicating that subjects in the S condition made fewer cooperative choices on Trial 1 than did subjects in the other three treatment conditions.

Overall cooperative choices. The second dependent variable was the mean number of cooperative choices in each of the four treatment conditions over each of the three Trial Blocks. In this case the choices made by subjects in each pair are not stochastically independent of each other: the choices of one player may have some influence on the choices of his partner. It is a common error in the statistical analysis of gaming data (e.g. Sermat, 1970, Experiment IV; Wrightsman, Bruininks, Lucker & O'Connor, 1972) to use individual subjects' cooperative choices as units of analysis; this violation of the independence assumption generates a spuriously inflated N and artificially reduces the error variance. The correct procedure is to use cooperative choices *per pair* as the units of analysis with N = the number of pairs, as was done in the following analyses.

The raw cooperative scores for the 40 pairs are tabulated in Appendix G, and the means are shown in Table 4.2.

Table 4.2: Mean Frequency of Cooperative Choices Per Pair:

Treatments x Trial Blocks (N = 40)

Treatment Condition	Trial Block 1	Trial Block 2	Trial Block 3
PI	6.50	6.20	6.70
NI	8.80	6.10	7.00
M	6.40	5.70	6.20
S	4.10	3.40	3.10

The overall grand mean is 5.85, which indicates that slightly fewer than 30 percent of the choices made were cooperative. This figure agrees roughly with previous findings on Prisoner's Dilemma (see Section 2.3 above).

An Analysis of Variance (Winer, 1962, Ch. 7) was performed on these data; the results of this analysis are summarized in Table 4.3.

Table 4.3: ANOVA Summary Table: Cooperative Choices Per Pair

(Treatments x Trial Blocks) N=40

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
A(Treatments)	237.37	3, 36	79.12	29.81	2.65	$.05 < p < .10$
B(Trial Blocks)	24.80	2, 72	12.40	8.83	1.58	n.s.
AB	22.13	6, 72	3.69	7.83	.47	n.s.
Total	1921.30	119				

The analysis revealed no significant effects beyond $p < .05$, but the main effect due to treatment conditions was marginally significant. Since it was hypothesized that the S condition would elicit less cooperation than the other three conditions, an orthogonal comparison planned in advance was undertaken by means of the Tukey test (Bruning & Kintz, 1977, pp. 122-124) between frequency of cooperative choices in the S condition with those in the PI, NI and M conditions taken together. This difference was significant beyond $p < .05$, confirming that fewer cooperative choices were made in the S condition than in the other three conditions.

Payoff-satisfaction correlations. The fundamental ideas behind the payoff-satisfaction correlations and the manner of their calculation have been extensively discussed in Sections 2.5 and 3.3 above; there is no reason to repeat them here.

The product-moment correlations between each subject's payoffs and satisfaction ratings on the corresponding trials are tabulated in Appendix H. These scores may be regarded as being stochastically independent of one another, thus $N = 80$. The overall grand mean of the payoff-satisfaction ratings was $r = .685$, which indicates that slightly less than half the variance in subjects' satisfaction ratings is accounted for by the payoffs they receive. The mean correlations for the PI, NI, M and S treatment conditions were $r = .627$, $r = .735$, $r = .602$ and $r = .773$ respectively.

It is of interest to note that the lowest mean payoff-satisfaction correlation occurred in the matrix treatment condition, which is the type of game normally used in research on Prisoner's Dilemma. Equally interesting is the fact that the highest correlation is found in the treatment condition which employed a lifelike simulation. A one-way Analysis of Variance (Winer, 1962, Ch. 3) was performed on the correlations in order to determine whether the differences between these means were significant. The results are summarized in Table 4.4.

Table 4.4: ANOVA Summary Table: Payoff-satisfaction Correlations in Four Treatment Conditions (N = 80)

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
Treatments	410.62	3, 76	1368.74	75.89	1.80	n.s.
Total	6177.83	79				

There is no indication of any significant effect of treatment conditions on payoff-satisfaction correlations or, speaking more loosely, on the extent to which subjects deviated from the rules of the game presented to them by introducing extraneous utilities into the situation (there is no justification for orthogonal comparisons in this case).

The correlations were, however, by no means all close to unity (see Appendix H), so it may be inferred that some subjects did modify the subjective payoff structure in the manner described above. In a mixed-motive game such as this, one source of extraneous utilities may arise from a desire to beat the "opponent" rather than simply maximize one's own payoffs. (In a zero-sum game these motives coincide.) The extent to which this was a source of extraneous utilities in this experiment is not easy to determine. The correlations between each subject's satisfaction ratings and his partner's payoffs for the corresponding trials were however calculated, yielding an overall grand mean of $r = -.542$. This seems to suggest that just under 30 per cent of the variance in subjects' satisfaction ratings can be explained by the positive utilities attached to the low payoffs of their partners and vice versa. This inference is, however, unsafe on account of the fact that, in a Prisoner's Dilemma, a player's own payoffs and those of his partner are not independent of each other: there is a strong negative correlation between them. The mean correlation mentioned above may therefore be an artifact, and it would have been found even if no subjects had departed from the explicit rules of the game.

Subsidiary analysis. For reasons analogous to those outlined in Section 3.3 above, a subsidiary Analysis of Variance was performed on the cooperative choices made by subjects who manifested high payoff-satisfaction correlations. In each treatment condition, the three pairs of subjects with the highest median correlations were selected for this analysis. One subject pair, both of whose members manifested notional correlations of $r = 1.00$ were not however used, since both subjects had made competitive choices and given neutral ratings on every single trial. The notional correlation of $r = 1.00$ assigned to each of these subjects was motivated by common sense, although strictly speaking there exists no correlation since the variance in each of the random variables is zero. The 24 individual subjects (12 pairs) used in this subsidiary analysis manifested payoff-satisfaction correlations ranging from r = .668 to r = .976 with a median of r = .868. The pairs used in the analysis are shown starred in Appendix G. Their mean cooperative choices in each treatment condition x Trial Block are shown in Table 4.5.

Table 4.5: Mean Cooperative Choices of Subject Pairs With High Payoff-satisfaction Correlations: Treatment Conditions x Trial Blocks (N = 12)

Treatment Condition	Trial Block 1	Trial Block 2	Trial Block 3
PI	7.00	7.33	9.33
NI	8.00	5.33	4.66
M	5.33	5.33	5.33
S	5.00	5.33	4.33

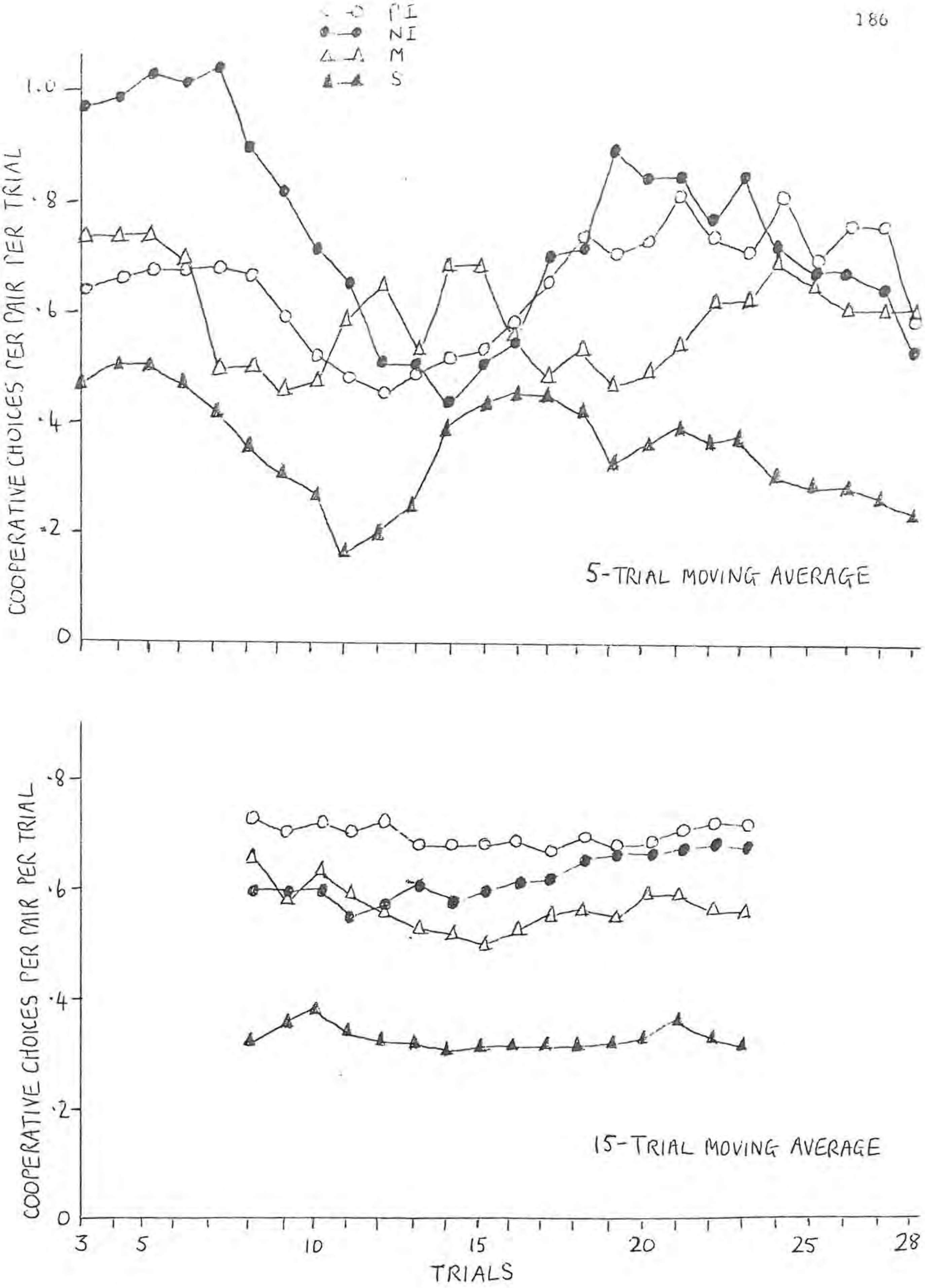
The overall grand mean (6.06) is slightly higher than that found in the main analysis (5.85) but this difference is non-significant (z = .04, n.s.). It is particularly in the PI, M and S condition that the tendency towards greater cooperation is manifested, and in the NI condition that means are in fact somewhat lower (cf. Table 4.2). An Analysis of Variance was performed on these data, and the results are summarized in Table 4.6.

Table 4.6: ANOVA Summary Table: Cooperative Choices of Subject Pairs With High Payoff-satisfaction Correlations (Treatments x Trial Blocks, N = 12)

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
A(Treatments)	51.00	3, 8	17.00	6.19	2.74	n.s.
B(Trial Blocks)	2.39	2, 16	1.19	5.82	.21	n.s.
AB	25.83	6, 16	4.31	5.82	.74	n.s.
Total	221.89	35				

As in the main analysis, none of the F ratios is significant. An a priori comparison, planned in advance, was once again made by means of the Tukey test between the S condition and the other three conditions taken together, but this difference failed to reach significance. It is worth noting, however, that the pattern of results is similar to that found in the main analysis. In particular, the lowest mean cooperative choice for each Trial Block is found in the S condition. The fact that statistical significance was not attained is unsurprising in view of the small N used in this analysis.

Time Series Analysis. No effect due to Trial Blocks was found in the main analysis or in the subsidiary analysis. Analysis of Variance is, however, an exceedingly blunt instrument for investigating such effects. A simple component Time Series Analysis (outlined above in Section 3.3) was therefore performed in an attempt to provide a more subtle interpretation of the characteristic movements of the subjects' cooperative choices over trials. To begin with, the 5-trial and 15-trial unweighted moving averages of the cooperative choices of subject pairs in each of the four treatment conditions were calculated. The results are shown graphically in Figure 4.2.



5-TRIAL MOVING AVERAGE

15-TRIAL MOVING AVERAGE

Figure 4.2. Five-trial and 15-trial Unweighted Moving Averages of Cooperative Choices Per Pair Per Trial.

The results of the component analysis are very illuminating. The 5-trial moving averages demonstrate that in all treatment conditions there was a short-lived rise followed after about six trials by a fall in cooperation, then a large and sustained second rise, and finally a fall during approximately the last ten trials. The matrix condition was atypical, manifesting three maxima and two minima, the latter occurring round Trial 10 and Trial 20. The level of cooperation was generally relatively high on early trials, and only in the PI condition was it exceeded in later trials. The decline in initial cooperation was most rapid in the matrix condition, and slowest in the incentive conditions (PI and NI), and the subsequent recovery was most rapid in the matrix and simulation conditions and slowest in the NI condition. The 15-trial moving averages, for their part, demonstrate that the characteristic secular movement was slightly downwards in all conditions except NI, which manifested a gradual rise in cooperation over trials. These trend graphs also provide a vivid illustration of the generally lower cooperation displayed by subjects in the simulation condition compared with the three abstract decision context treatment conditions.

A slightly more ambitious Time Series Analysis was performed in order to investigate the possible existence of short-term periodicities or consistencies in the subjects' strategy choices. Essentially what was done was to calculate the autocorrelation functions of the cooperative choices of subjects in each treatment condition. An autocorrelation (see e.g. Anderson, 1976; Tukey, 1967) is simply a correlation between a time series (a number of observations taken in serial order) with a replica of itself displaced in time. An autocorrelation function is a set of autocorrelations for a given time series from lag = 0 to lag = k. The autocorrelation at lag = 0 is, of course always equal to r(k) = 1.00. The formula used for the calculation of the autocorrelation functions was as follows:

$$r(K) = \frac{\sum_{N=1}^{N-K} \left[X_i \left(\frac{\sum_{N=1}^{N-K} X_i}{N-K} \right) \right] \times \left[Y_{i+K} - \left(\frac{\sum_{K+1}^{N} Y_i}{N-K} \right) \right]}{\left\{ \sum_{N-1}^{N-K} \left[X_i - \left(\frac{\sum_{N=1}^{N-K} X_i}{N-K} \right) \right]^2 \times \sum_{K+1}^{N} \left[Y_{i+K} - \left(\frac{\sum_{K+1}^{N} Y_i}{N-K} \right) \right]^2 \right\}^{1/2}}$$

where $X_1, X_2, \ldots, X_N$ are the X-scores X_i;

$Y_1, Y_2, \ldots, Y_N$ are the Y-scores Y_i;

K = 0, 1, 2, ... is the lag; and

N = 2, 3, 4, ... is the number of X-scores.

Fierce though it may look, this formula is merely a version of the well-known product-moment formula elaborated in such a way as to allow a time series (X_i) to be correlated with a replica of itself (Y_i) at a specified time lag (K).

The autocorrelation functions up to lag = 5 for cooperative choices in the four treatment conditions are shown graphically in Figure 4.3.

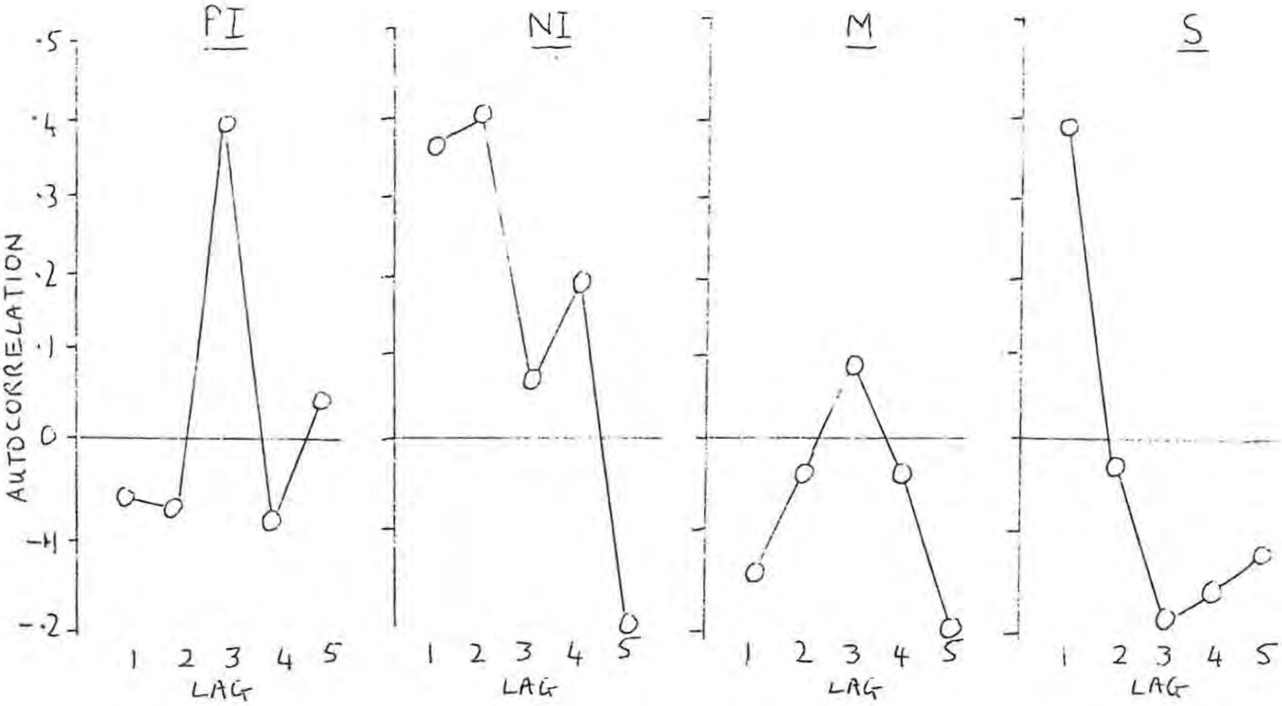


Figure 4.3. Autocorrelation Functions of Cooperative Choices in Four Treatment Conditions.

The autocorrelation functions reveal inconsistent and non-periodic choice behaviour in the PI and M conditions. In the NI condition, however, a different picture emerges: there was evidently a tendency for subjects to stick to the same strategy choice for five trials before switching strategies (i.e. up to lag = 4). The subjects in the S condition tended to stick to the same choice for two trials before switching, although the tendency to switch was still very slight after two trials.

Post-experimental interviews. A variety of responses emerged from the post-experimental interviews. Feelings tended occasionally to run high, particularly in the PI condition: one subject described his partner as "despicable" and another used the apithet "bastard". One subject in the S condition also described his partner as a "bastard", while two others in this condition considered their partners "selfish" and "stubborn". All these examples are taken from subjects whose partners adopted a high frequency of competitive strategies.

A preliminary perusal of the protocols revealed that only three types of response occurred sufficiently frequently to make statistical analysis meaningful. Firstly, in answer to the question "How do you feel about the overall results?", answers like "Pleased", "Quite pleased", "O.K.", etc. occurred with frequencies 7, 10, 9 and 7 in the PI, NI, M and S conditions respectively. These frequencies are not significantly different from chance (X^2 = .911, n.s.).

In answer to the question "How do you feel about your partner?", answers of the type "Sensible", "Intelligent", "Did as I would have done" etc. occurred with frequencies 5, 1, 6 and 3 in the PI, NI, M and S conditions respectively. The frequencies are too low to allow the use of Chi Square, but since the main interest centres on the difference between the S condition and the other three, the frequencies for the PI, NI and M conditions were combined, and a z test for the differences between the proportion in these three groups giving an answer in this category with the corresponding proportion in the S group, revealed that the subjects in the S group considered their partners "Sensible" etc. significantly less frequently than did other subjects ($z = 1.99$, $p < .05$, two-tailed).

In response to the question "What was your overall strategy?", responses indicating a general policy of competing occurred with frequencies 6, 3, 5 and 7 in the PI, NI, M and S conditions respectively, but these frequencies were not significantly different from one another ($\chi^2 = 1.38$, n.s.).

The same question elicited a general policy of cooperating with frequencies 4, 3, 2 and 1 in the PI, NI, M and S groups respectively. Combining PI, NI and M and comparing the proportion of such replies in this group with the corresponding proportion in the S group, a significantly lower frequency of generally cooperative orientation was found in the S group than in the others ($z = 1.98$, $p < .05$, two-tailed). This orientation is of course reflected in the subjects' actual choice behaviour described above.

4.4. Discussion

In this experiment, the cooperative and competitive choices of subjects in four structurally equivalent versions of a Prisoner's Dilemma game were examined. From a logical or strategic point of view, the four task situations presented identical problems to the subjects, and any observed differences in their behaviour could be attributed to formally irrelevant aspects of the decision contexts in which the game was played. Three of the decision contexts were of an abstract nature, and the other (the simulation condition) was a lifelike simulation of a decision-making situation whose payoff structure corresponded to the matrix used in one of the abstract decision contexts, and to the payoff structure in one of the other abstract decision contexts as well, in an exact ratio-scale sense. The fourth decision context corresponded to the others at the level of interval-scale equivalence.

The lifelike simulation used in this experiment revolved around the decisions of two restauranteers, competing for the same clientele, whether or not to stage a rather expensive floor show in each of their respective restaurants. The decision contexts surrounding the three abstract versions of the game were somewhat different from one another. In the matrix condition, the subjects made their choices in the traditional manner according to a payoff matrix and were rewarded with points. In the positive incentive condition they played without a payoff matrix but with full knowledge of the payoff structure and were rewarded with real money. The negative incentive version was very similar to the positive incentive condition except that the monetary incentives were negative, i.e. the subjects played to conserve the money that had already been given rather than to win money: they stood to lose money rather than to gain it.

The major hypothesis was that, in spite of the fact that the same game with corresponding payoffs in each cell was used in all treatment conditions, the frequency of cooperative choices would be affected by the various decision contexts in which the game was played. In particular, it was hypothesized that the simulation condition would engage socially sanctioned competitive motives associated with commercial condict in Western industrial culture. Any differences between the behaviour of the subjects in the simulation condition and that in the abstract decision context conditions was, however, felt to have a bearing on the ecological validity of findings of traditional Prisoner's Dilemma research which has almost invariably used abstract decision contexts, notably the ubiquitous payoff matrix.

The findings confirmed the major hypothesis in a variety of different ways. Subjects in the simulation condition were found to make fewer cooperative choices on the first trial, and fewer cooperative choices throughout the 30 trials of the experiment, than subjects in the abstract decision context conditions. In the post-experimental interviews, subjects in the simulation condition described their general strategy throughout the experiment as having been one of cooperation significantly less often than did subjects in the abstract decision context conditions. It is evident that the behaviour of subjects in the lifelike simulation used in the S condition differed in a systematic and consistent fashion from the behaviour of subjects in the other decision contexts. On the other hand, no significant differences emerged from the main analyses between any of the abstract conditions in spite of the radically different decision contexts involved. No significant differences were found, for example, between the frequency of cooperative choices in the incentive versus the no incentive treatment conditions; this is in line with numerous non-significant findings which have been reported by previous investigators in this area.

One might have anticipated that playing to conserve what one already has, as in the negative incentive condition, might have elicited very different behaviour from the subjects than playing for gain, as in the other treatment conditions. No such difference emerged from the Analyses of Variance. Some noticeable changes in the subjects' behaviour over trials was also anticipated, yet once again no such changes emerged from the Analyses of Variance. It was only when the rather more sensitive techniques of Time Series Analysis were applied that any clear picture emerged regarding the differences between subjects' behaviour in different abstract decision contexts over trials, and the peculiar effects of the negative incentive decision context became evident. More will be said about the results of the Time Series Analysis below. At this point, however, a few words should be said about the correlational analysis.

A unique feature of this experiment was the attempt made to examine the correspondence or lack of correspondence between the payoff structure of the given game situation and the latent utilities attached to the various outcomes by the subjects in the various treatment conditions. An implicit assumption underlying all experimental gaming research is that there is a one-to-one correspondence between manifest and latent utilities, but (apart from Experiment I above), this assumption has not been tested before. It seems, on the face of it, a rather implau~~x~~sible assumption especially in experiments in which the game is played over a number of trials. Boredom, a desire to "beat" the "opponent" or minimize his payoffs, and satisfactions or disappointments arising from patterns developing over repeated plays are among the potential sources of extraneous utilities which one might expect some subjects to introduce into the task situation; the list is, of course, strictly innumerable.

The correlations between subjects' payoffs on each trial and their ratings of satisfaction with the outcome of the corresponding joint decisions indicated a correspondence between payoffs and latent utilities (thus operationalized) which was surprisingly high in view of the considerations outlined above. The overall grand mean of the correlations ($r = .685$) indicated that slightly less than half the variation in satisfaction ratings was accounted for by the payoffs which the subjects received. The correlations were lowest in the matrix condition (with a mean of $r = .602$) and, interestingly enough, highest in the simulation condition ($r = .773$); even the incentive conditions did not yield average correlations as high as that found in the simulation condition. Although the differences between the correlations were not significant, the implication of these findings is that the traditional payoff matrix may be the least satisfactory decision context in which to investigate gaming behaviour, at least as far as the assumption of correspondence between manifest payoffs and latent utilities is concerned. These findings argue strongly for the feasibility of lifelike decision contexts in gaming research.

For the majority of subjects, the correlations between payoffs and satisfaction ratings were less than perfect, indicating that adherence to the given game structure was not complete. It is obviously of interest to know what the pattern of results would have looked like if all the subjects had played the Prisoner's Dilemma Game presented to them, and more particularly, whether the results of the main analyses would under these conditions have been at all different. A subsidiary analysis, using 12 pairs of subjects whose payoff-satisfaction correlations were all very high, was therefore performed. The results of this subsidiary analysis were quite reassuring. The general frequency of cooperative choices in this subset of subjects was slightly higher than that of the entire sample of 80 subjects, with an overall grand mean of 6.06 cooperative choices per pair per Trial Block compared with a mean of 5.85 in the main analysis. The overall pattern of the results was however essentially similar to that found in the main analysis. In particular, the level of cooperation was once again found to be lowest in the simulation decision context condition, in line with the major hypothesis. Although this effect failed to reach statistical significance in the subsidiary analysis, possibly on account of the small N, the results give no reason to suspect that the pattern of results found in the main analysis were artifacts caused by departures on the part of the subjects from the given payoff structure, and the means were very similar. As mentioned earlier, the greatest departures from the manifest game structure were found in the traditional matrix condition, but this does not seem to have led to any systematic difference in their cooperative choice behaviour.

It was hypothesized that the frequency of cooperative choices would show some significant changes over trials, but the results of the Analyses of Variance failed to confirm this hypothesis. The Time Series Analysis, however, provided a vivid illustration of the different strategic behaviour of subjects in the four treatment conditions over trials. A component analysis revealed firstly that the characteristic secular movement in cooperative choices was downwards in all treatment conditions except the one which used a negative incentive decision context: in the latter case a slight but steady increase in cooperation over trials was evident when cyclical and iregular fluctuations were suppressed. This finding is extremely interesting in the light of previous findings on Prisoner's Dilemma. In previous research (e.g. Rapoport & Chammah, 1965) a steady decline in cooperation over the first 30 trials has invariably been reported, whether or not monetary incentives have been attached to the payoffs. This effect was replicated in this experiment in the matrix condition (which resembles previous experiments) and in the positive incentive and simulation conditions. But the psychological strangeness of playing to conserve what one has rather than to gain anything was manifested in this unique finding of a steady increase of cooperation over trials in the negative incentive condition. This type of decision context (which is often encountered in everyday social interactions) evidently warrants further investigation.

When only irregular fluctuations in choice behaviour were suppressed in the component Time Series Analysis, further interesting effects emerged. The cyclical pattern of the subjects' behaviour was similar in all treatment conditions with the exception of the traditional matrix condition; in most treatment conditions a short-lived increase in cooperation was followed after about six trials by a rapid and dramatic decline, then a large and sustained recovery starting around Trial 12 and continuing up until about Trial 20, and finally a slight decline during the last 10 trials. The matrix condition, however, produced comparatively wild and peculiar cycles of choice behaviour, with three maxima and two minima. Yet again it appears that the abstract payoff matrix, beloved by gaming researchers, elicits behaviour from subjects which is untypical of their behaviour in other strategically equivalent situations. The erratic behaviour in this condition may reflect the meaninglessness of the task from the subjects' viewpoint.

The second part of the Time Series Analysis involved the calculation of autocorrelation functions up to lag = 5 for all treatment conditions in order to investigate short-term consistencies and periodicities in the subjects' choice behaviour. The results revealed that choices in the matrix and positive incentive conditions were inconsistent and unpredictable over the short term. The subjects in the negative incentive condition, however, revealed a tendency to stick to the same strategy choice for approximately five trials, and subjects in the simulation condition tended to stick to the same choice for at least two trials. It may be inferred that the decision contexts used in these latter treatment conditions induced more resolute opinions about what sort of strategy was appropriate in the short run than did the others.

The primary motivation behind this experiment arose out of a desire to explore the assumption underlying most experimental gaming research regarding the relevance of laboratory findings to the everyday decision-making predicaments which they purport to model. Large and consistent differences were found between the behaviour of subjects in abstract versions of the Prisoner's Dilemma Game and a lifelike simulation whose structural properties were identical to the abstract versions. The simulation was of a lifelike decision-making situation which the subjects evidently found engaging and believable, and it is not unreasonable to assume that it reflected the likely behaviour of people in a corresponding real life situation more closely than did the abstract decision contexts. In some ways, the behaviour of subjects in the traditional matrix version of the game was least similar of all the abstract versions to their behaviour in the lifelike simulation.

It is not know what would be found if a different lifelike simulation of the same game had been used, but the results of this experiment nevertheless provide strong arguments against casual extrapolation of the results of traditional gaming experiments, at least as far as Prisoner's Dilemma is concerned, to other life situations. Further research, described below in Chapters 5 and 6, is needed before the same can be claimed with respect to other mixed-motive games.

CHAPTER FIVE

5. EXPERIMENT III: EFFECTS OF ABSTRACT AND LIFELIKE DECISION CONTEXTS ON CHOICES IN A GAME OF CHICKEN

5.1. Introduction

The overwhelming majority of experimental investigations of mixed-motive gaming behaviour have been devoted to the Prisoner's Dilemma Game (Apfelbaum, 1974; Davis, Loughlin & Komorita, 1976; Nemeth, 1972; Pruitt & Kimmel, 1977; Schlenker & Bonoma, 1978; Wrightsman, O'Connor & Baker, 1972). The popularity of Prisoner's Dilemma with empirical researchers has stemmed partly from the apparent relevance of the type of strategic structure embodied in this game to a wide range of everyday interpersonal and inter-group conflicts.

There is another class of mixed-motive games, however, often referred to as dangerous games, which also have relevance to many socially significant strategic interactions in everyday life (Swingle, 1970a). The characteristic feature of these games, which distinguishes them from Prisoner's Dilemma, is the necessity for a player to risk his worst possible payoff in order to achieve his best possible payoff. An aircraft hijacker may, for example, threaten to explode a bomb in the aircraft if anyone attempts to disarm him. If his threat is genuine, then by issuing it he risks death in order to achieve a desired goal. Anyone who threatens to disarm the hijacker similarly risks death in order to achieve the desired goal of subduing him.

The hijack situation described above is one which can be reasonably modelled by the best known dangerous game, namely the game of Chicken. Chicken is a two-person two-choice game whose essential structure has been explained in Section 1.5 above. Its payoff structure resembles that of Prisoner's Dilemma, and the payoffs are customarily labelled in the same way, i.e.:

$$a_{11} = b_{11} = R, \ a_{12} = b_{21} = S, \ a_{21} = b_{12} = T, \ a_{22} = b_{22} = P,$$

where a_{11} is the payoff to Player A given a choice by A of strategy 1 and a choice by B of strategy 1 etc. The defining set of inequalities in the game of Chicken is, however:

$$T > R > S > P.$$

The difference between Chicken and Prisoner's Dilemma arises from the inequality $S > P$ in Chicken, compared with $P > S$ in Prisoner's Dilemma. In Prisoner's Dilemma, a single equilibrium exists at A_2B_2, i.e. neither player can unilaterally improve on the payoff associated with this outcome by switching strategies (although both do better if both switch -- hence the dilemma). In Chicken, however, two equilibria exist at A_2B_1 and at A_1B_2, but Player A prefers the first and Player B the second of these two outcomes. In discussions of Chicken, the choices A_1 and B_1 are often labelled "cooperative", and A_2 and B_2 are often labelled "competitive". This arises from a generalization based upon Prisoner's Dilemma and seems somewhat inappropriate in the game of Chicken. From an intuitive point of view, it seems much more natural to label the A_1 and B_1 choices "cautious" and the A_2 and B_2 choices "risky".

Largely out of a desire to throw light on the behaviour of people in situations of bilateral threat, a number of empirical studies have been devoted to investigating the choices of subjects in strategic interactions which conform to the payoff structure described above (see Section 2.3). In almost every case, however, the decision contexts in which the subjects have had to make their choices have been entirely abstract -- usually payoff matrices -- and far removed from the everyday strategic interactions which the game of Chicken purports to model. One study (Sermat, 1970) has been reported in which an attempt was made to compare the behaviour of subjects in an abstract Chicken game with their behaviour in a different type of decision context; this experiment did not, however, involve the use of any lifelike situation and was, in any event, marred by serious design flaws, errors in statistical treatment of results, and faulty inferences from the data (see detailed discussion in Section 2.3 above).

The experiment reported below represents an attempt to fill this gap by comparing the strategic choice behaviour of subjects in four structurally equivalent decision contexts, including one resembling the decision context used in most previous experiments, and another involving a lifelike simulation of a real life situation. The lifelike simulation involved a hypothetical situation in which two food wholesalers have to decide whether to send their goods to customers on an island by sea or air. Apart from the conventional payoff matrix, the other treatment conditions used in this experiment involved real monetary incentives: the subjects played without a payoff matrix either to win money or to conserve as much money as possible from an initial allowance provided to them at the start of the experiment. Apart from the fact that the payoff structure used in this experiment conformed to the specifications of Chicken rather than Prisoner's Dilemma, and that the lifelike simulation was necessarily quite different in content from that used in the experiment described above in Chapter 4, the treatment conditions were identical to those used in the above experiment. Details can be found in Sections 4.1 and 4.2 above; they will not be repeated here.

The major hypothesis in this experiment was that the strategic choice behaviour of the subjects would differ between different treatment conditions, in spite of their logical equivalence, since the four decision contexts were, from a psychological point of view, quite different from one another. It was further hypothesized that these differences would become more pronounced over the 30 repetitions of the game as the subjects became more familiar with the various decision contexts and the consequences of their strategy choices.

A more specific hypothesis was that subjects in the simulation condition would produce fewer cautious (or more risky) choices than those in the other three (abstract decision context) conditions. This hypothesis derives from the literature on the group polarization phenomenon in general (vide recent reviews by Lamm & Myers, 1978; Myers & Lamm, 1976) and what used to be called the risky shift in particular. There is abundant evidence from research in this area that risk is a value in Western industrial culture in the sense that risk taking tends to be admired in most situations to a greater degree than caution. In the game of Chicken, a player's choice is always between a relatively cautious strategy (A_1 or B_1) which guarantees a certain minimum payoff (R, or at worst S) and a strategy (A_2 or B_2) which offers the possibility of a higher payoff than R(T) but involves the risk of a lower one than S(P). The resemblance between the choices in a game of Chicken and in a Choice Dilemma such as those used in most experiments on the risky shift is striking. The game of Chicken in fact presents what amounts to a Choice Dilemma in a particularly clearly specified form, since in Chicken, unlike in a conventional Choice Dilemma, the payoffs are precisely quantified.

In the payoff structure used in the experiment described below, the average expected payoff (assuming random choices from the other player) for each strategy choice was equal, though one strategy was clearly riskier than the other. It was therefore hypothesized that the greater contextual realism surrounding the lifelike simulation would engage cultural values associated with risk taking to a greater extent than would the abstract decision contexts, thereby inducing the subjects to produce a larger number of risky choices (fewer cautious choices). This hypothesis is strengthened by the knowledge that the decision context surrounding the lifelike simulation involved an area of decision making (commercial enterprise) in which risk taking is known to be particularly highly valued.

On commonsense grounds it was further hypothesized that greater caution would be displayed by subjects in the incentive conditions than in the non-incentive conditions: they stood to gain more (or lose less) in these conditions and caution would seem to be at a premium when something of real value is at stake.

Finally, it was hypothesized that subjects would display greater caution in the negative incentive than in the positive incentive condition. This was also based upon intuitive reasoning: there seems to be less motive to take risks when one is attempting to act conservatively than when one is attempting to attain rather than to conserve a tangible reward.

5.2. Method

Design. The design of this experiment was a 4 x 3 factorial with repeated measures on the second factor (Winer, 1962, Ch. 7). The four levels of the first factor were labelled PI (positive incentive), NI (negative incentive), M (matrix), and S (simulation). Apart from the use of a different payoff structure (see Figure 5.1 below) and a correspondingly different lifelike simulation (see below), the manipulation of this independent variable was identical to that used in Experiment II above. Details were given in Section 4.2.

Subjects were randomly assigned to treatment conditions in pairs, and made 30 successive decisions which were divided into three Trial Blocks of 10 decisions each. Trial Blocks is thus the second factor in the experimental design. The dependent variables were the number of cautious choices made on the first trial in each treatment condition, and the number of cautious choices per pair in each Treatment Condition x Trial Block. According to the theory of Chicken, a cautious choice was defined as an A_1 or B_1 choice, labelled "L" in the abstract conditions and "SEA" in the simulation condition (see below).

Subjects. The subjects were 80 undergraduate and postgraduate students at the University of Leicester, randomly assigned to treatment conditions and then randomly assigned to play Row or Column, with the restriction that males were always paired with males and females with females. There were 39 males and 41 females in the sample. Ages ranged from 17 to 33 with a median of 20. Students reading Psychology were not included in the sample, and two subjects who turned out in the post-experimental interview to have some prior acquaintance with the theory of games were excluded together with their partners. The data for one pair of subjects in the negative incentive condition were unfortunately mislaid; in the main Analysis of Variance the missing scores were therefore estimated by the method of unweighted means (Winer, 1962, pp. 281-283).

Procedure. The procedure was identical to that used in Experiment II, except that (a) a different payoff matrix was used in the matrix treatment condition (and correspondingly different payoff structures in the other conditions); (b) a different lifelike simulation was used in the simulation condition; and (c) instructions to the subjects were given in typewritten rather than tape recorded form. The fine details of the procedure were described in Section 4.2. All aspects of the procedure, apart from those mentioned above, were identical in the two experiments. The instructions to the subjects were word-for-word the same apart from the values attached to the payoffs.

The payoff matrix used in the M condition is shown in Figure 5.1.

$$\begin{array}{c|c|c|}
 & L & R \\
\hline
L & 3,3 & 2,4 \\
\hline
R & 4,2 & 1,1 \\
\hline
\end{array}$$

Figure 5.1. Payoff Matrix Used in Matrix Decision Context.

The positive incentive and similation conditions used the identical payoff structure, and the payoff structure used in the negative incentive condition was derived from that in Figure 5.1 by subtracting four units from each payoff. The lifelike simulation used in condition S was as follows:

> Your task will consist of making a series of 30 decisions. You may do very well for yourself if your decisions turn out well, but you may be less

successful. The outcome in each case will depend not only on the decisions which you make, but also on the decisions which the other person makes. You are advised, therefore, to consider each decision carefully.

Your decisions will be based on the following hypothetical situation. You have just taken over ownership and management of a firm whose sole business involves supplying a small island with fresh vegetables. There is another firm just started business in the same area which is in every way similar to yours, and which sells its vegetables to the same island. The problem facing you each morning is whether to send your goods to the island by air or by sea, and your sole consideration is to maximize profits. In each case your decision, as shown on the cards in front of you, will be AIR or SEA.

Theffollowing facts are known to both of you from the experience of the previous owners: If you both use the air freight, the airport authorities will not despatch such a large cargo immediately, but will wait for the departure of a large aircraft in the afternoon, on which to send the goods. The goods will consequently arrive very late at the market, and each firm will in that case have to be content with a minimal standard profit on that day. If both firms use the sea crossing, the goods will arrive only slightly late, and in these circumstances each firm will make three times the standard minimal profit on that day. If, however, one of the firms uses the air freight and the other uses the sea freight, the firm which uses the air freight will have its (relatively small) cargo despatched immediately on the light aircraft, while the firm which uses the sea freight will again have its goods delivered slightly late. In that case, the firm using the air freight will make four times the standard minimal profit for the day (since his goods will be on sale from the opening of the market) while the firm using the sea freight will make only double the standard minimal profit (since by the time its goods are on sale many customers will have bought the goods supplied by the other firm).

Summary:-

Both send by air -- each gets s;
Both send by sea -- each gets 3s;
One by air, one by sea -- 4s and 2s respectively.

5.3. Results

Cautious choices on Trial 1. The first dependent variable to be investigated was the number of cautious choices made by the subjects in the four treatment conditions on the first trial. Since a subject's choice on Trial 1 is uninfluenced by any knowledge of how his partner has chosen, there are N = 78 independent observations in this analysis (in view of the missing data from one pair of subjects). The number of cautious and risky first choices made by the subjects in each treatment condition is shown in Table 5.1.

Table 5.1: Cautious and Risky Choices on Trial 1 in Four Treatment Conditions (N = 78)

	Treatment Condition			
	PI	NI	M	S
Cautious Choice	10	12	10	7
Risky Choice	10	6	10	13

A Chi Square test (Siegel, 1956, pp. 175-179) revealed that the frequencies were not significantly different from chance ($\chi^2 = 2.90$, d.f. = 3, n.s.), although it is worth noting that only in the S condition were the majority of initial choices risky.

A major hypothesis was that the S condition would elicit fewer cautious choices than would the other three (abstract decision context) conditions: the cultural value associated with risk was expected to be enhanced by the lifelike contextual information in the S condition, particularly since the simulation involved business decisions in which risk has been shown to be culturally valued. A comparison, planned in advance, was therefore made between the proportion of cautious choices made on the first trial in conditions PI, NI and M (32/58) and the corresponding proportion in the S condition (7/20). A z test (Bruning & Kintz, 1977, pp. 222-224) revealed that this difference was only marginally significant by a two-tailed test ($z = 1.56$, $.10 < p < .11$, two-tailed). The difference is clearly in the predicted direction, with subjects in the S condition evincing a smaller proportion of cautious choices than subjects in the other (abstract) conditions, but the use of one-tailed tests in cases like this is frowned upon by the best authorities (e.g. Edwards, 1967, pp. 238-239).

Overall cautious choices. The major dependent variable was the number of cautious choices made in each of the four treatment conditions over each of the three Trial Blocks. In this case the choices made by one member of a pair are not stochastically independent of those of his partner. The units of analysis were therefore the aggregate scores per Trial Block from each pair of subjects, with N = the number of pairs. The raw scores for the 40 pairs (including the unweighted means estimate for the missing pair) are tabulated in Appendix I, and the means are shown in Table 5.2.

Table 5.2: Mean Frequency of Cautious Choices Per Pair: Treatments x Trial Blocks ($N = 40^{a}$)

Treatment Condition	Trial Block 1	Trial Block 2	Trial Block 3
PI	10.40	11.10	9.40
NI	8.67	8.56	8.56
M	7.90	7.10	6.70
S	6.90	6.10	5.80

[a] These data include unweighted means estimate for one pair in condition NI.

The overall grand mean is 8.09, which indicates that approximately 40 per cent of the choices made by subjects in this experiment were cautious. This figure agrees roughly with what has been found in previous experiments on Chicken.

An Analysis of Variance (Winer, 1962, Ch. 7) was performed on these data; the results og the analysis are given in Table 5.3.

Table 5.3: ANOVA Summary Table: Cautious Choices Per Pair (Treatments x Trial Blocks, $N = 40^{a}$)

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
A(Treatments)	275.86	3, 36	91.95	21.93	4.19	$p < .025$
B(Trial Blocks)	15.35	2, 72	7.67	6.78	1.13	n.s.
AB	13.27	6, 72	2.21	6.78	.33	n.s.
Total	1581.92	119				

[a] Including the unweighted means estimate for one pair in condition NI.

The only significant effect to emerge from this analysis is the main effect due to treatments. A posteriori analysis by means of the Tukey test (Bruning & Kintz, 1977, pp. 122-124) revealed that, although fewer cautious choices were made in the S condition than in any of the others, only the difference between S and PI is significant beyond $p < .05$.

Payoff-satisfaction correlations. In order to examine the degree of correspondence between the payoff structure of the game and the latent utility structures in the minds of the subjects, a product-moment correlation coefficient was computed separately for each subject between his payoffs on each trial and his corresponding satisfaction ratings. Details and an explanation were given above in Sections 2.5 and 3.3.

The product-moment correlations between each subject's payoffs and satisfaction ratings are tabulated in Appendix J. The scores are independent, and $N = 78$. The grand mean was $r = .712$, which indicates that more than half the variance in the satisfaction ratings is accounted for by the payoffs received by the subjects on the corresponding trials. The mean correlations for the PI, NI, M and S treatment conditions were $r = .779$, $r = .695$, $r = .654$ and $r = .721$ respectively.

It is worth noting that the lowest mean correlation occurred in the M treatment condition, which corresponds to the type of decision context used in most previous experimental gaming research. A one-way Analysis of Variance (Winer, 1962, Ch. 3) was performed on the correlations in order to determine whether the differences between these means were significant. The results of this analysis are summarized in Table 5.4.

Table 5.4: ANOVA Summary Table: Correlations Between Subjects' Payoffs and Satisfaction Ratings in Four Treatment Conditions ($N = 80^a$)

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
Treatments	.16	3, 76	.05	.04	1.45	n.s.
Total	3.03	79				

[a] Unweighted means estimates for two subjects included.

From this analysis no significant effect of treatment conditions on payoff-satisfaction correlations emerged. It is evident, however, that extraneous utilities were introduced into the game by many subjects: none of the correlations reached unity. This raises the question of whether the findings of the main analysis would hold good in the case of subjects who were playing the game presented to them, i.e. subjects whose subjective utilities correspond closely to the objective payoffs embodied in the games. A contemplation of this question motivated the subsidiary Analysis of Variance described immediately below.

Subsidiary analysis. For reasons analogous to those outlined in Section 3.3 above, a subsidiary Analysis of Variance was performed on the strategy choices of a small subset of subjects who had manifested high payoff-satisfaction correlations. In each treatment condition, the three subject pairs with the highest median correlations were selected for this analysis. The 24 individual subjects in this subset manifested correlations ranging from r = .645 to r = .989 with a median of r = .863. The pairs used in this analysis are shown starred in Appendix I. Their mean cautious choices in each treatment condition x Trial Block are shown in Table 5.5.

Table 5.5: Mean Cautious Choices of Subject Pairs With High Payoff-satisfaction Correlations: Treatment Conditions x Trial Blocks (N = 12)

Treatment Condition	Trial Block 1	Trial Block 2	Trial Block 3
PI	11.00	13.67	9.33
NI	9.00	6.00	7.33
M	10.33	8.00	8.33
S	8.33	6.33	6.67

These means are similar to those found in the main analysis (cf. Table 5.2). The overall grand mean (8.69) is slightly higher than the overall grand mean based on all 78 subjects (8.09), but not significantly so (z = .12, n.s.). The main difference is that the means for the M condition are somewhat higher, and the means for the NI condition are slightly lower than those found for all the subjects taken together. An Analysis of Variance (Winer, 1962, Ch. 7) was performed on these data, and the results are given in Table 5.6.

Table 5.6: ANOVA Summary Table: Cautious Choices of Subject Pairs With High Payoff-satisfaction Correlations (Treatments x Trial Blocks, N = 12)

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
A(Treatments)	99.64	3, 8	33.21	17.25	1.93	$.10 < p < .20$
B(Trial Blocks)	19.06	2, 16	9.53	9.96	.96	n.s.
AB	39.61	6, 16	6.60	9.96	.66	n.s.
Total	455.64	35				

Not surprisingly in view of the small N, none of the differences in this subsidiary analysis are significant. As in the main analysis, three a priori comparisons, planned in advance, were made by means of the Tukey test (Bruning & Kintz, 1977, pp. 122-124) between the mean cautious choices of subjects in the S condition and the three abstract decision context conditions, but none of these differences reached significance beyond $p < .05$. It is reassuring to note, however, that the overall pattern of results is similar to that found in the main analysis; in particular, the lowest mean cautious choice for each Trial Block is once again found in the S condition.

Time Series Analysis. No main effect due to Trial Blocks emerged from the main or subsidiary Analyses of Variance. For the investigation of such time-bound effects, however, Analysis of Variance is a rather crude device; it was not conceived with such applications in mind (Fisher, 1951). In an effort to provide a rather more subtle insight into the changes in the subjects' strategic choices over trials, an elementary component Time Series Analysis (as outlined in Section 3.3) was first of all performed. Unweighted moving averages of order 5 and 15 were computed for the mean cautious choices in each of the four treatment conditions (per subject pair) across the 30 trials. The results are illustrated graphically in Figure 5.2.

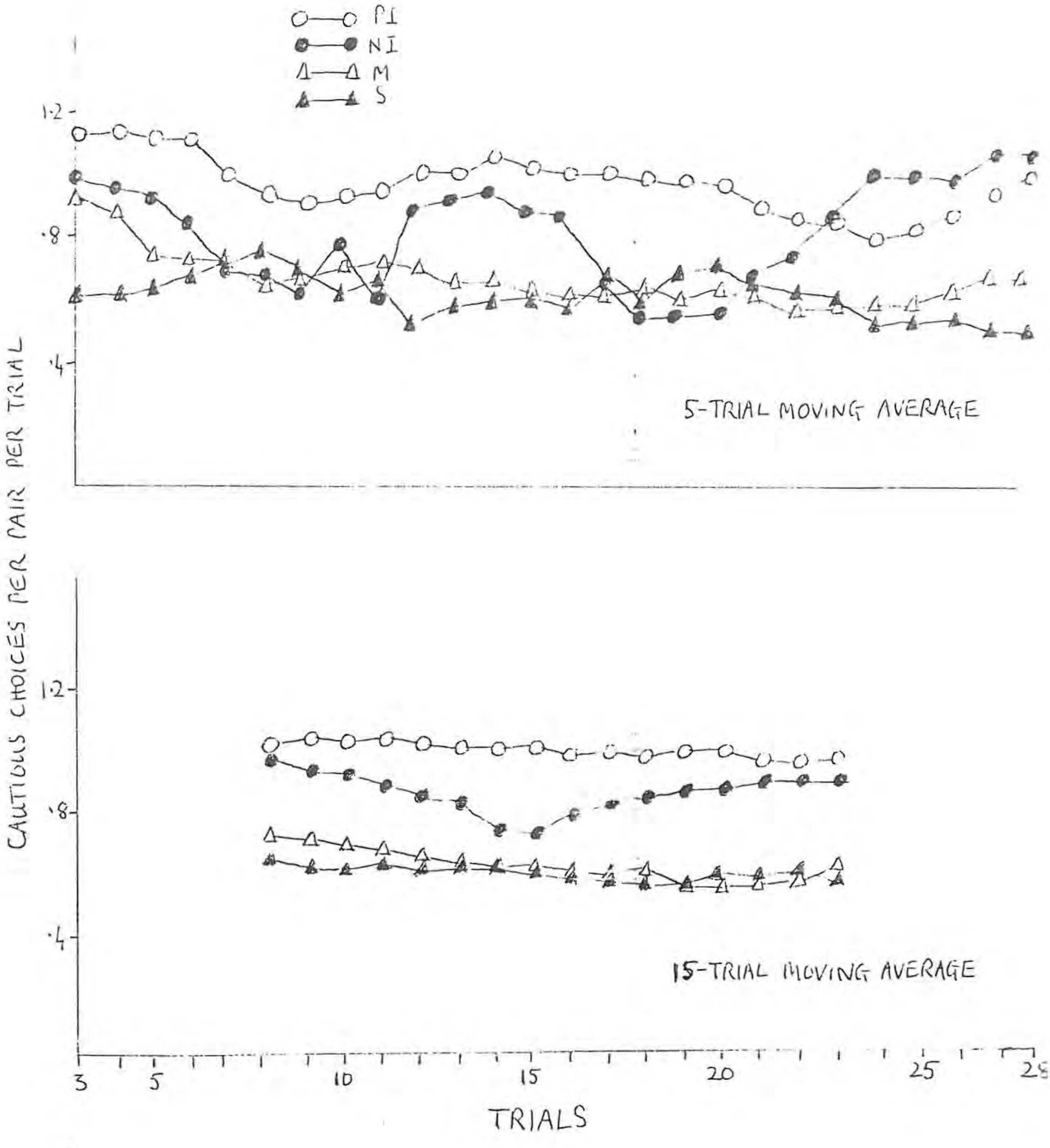


Figure 5.2. Five-trial and 15-trial Unweighted Moving Averages of Cautious Choices Per Pair Per Trial.

The component analysis reveals that there was in general a very small but steady decline in caution over trials in all treatment conditions except NI. The characteristic movement is approximately linear in the PI, M and S conditions, but there is a strong cyclical component in the NI condition as shown by the 5-trial moving averages, with minima occurring around trials 11 and 18. The characteristic movements of the M and S conditions are very similar to each other, apart from the slightly higher cooperation in the M condition. The relative caution displayed by subjects in the PI condition, and to some extent by those in the NI condition, when compared with behaviour in the two non-incentive conditions, is vividly displayed by the results of the component Time Series Analysis.

An analysis using the autocorrelation formula given in Section 4.3 above was performed in order to discover possible short-term consistencies or periodicities in the subjects' strategy choices. The autocorrelation functions up to lag = 5 for cautious choices in the four treatment conditions are shown graphically in Figure 5.3.

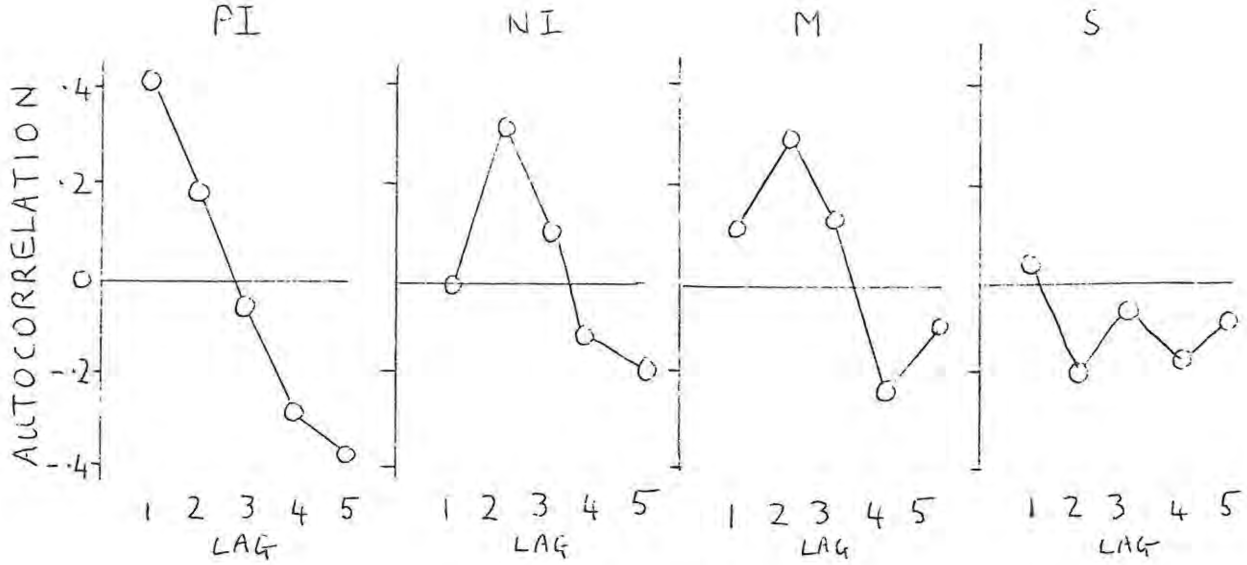


Figure 5.3. Autocorrelation Functions of Cautious Choices in Four Treatment Conditions.

The autocorrelation functions reveal a tendency in all treatment conditions for subjects to stick to the same strategy choice for at least a couple of trials. This tendency was weakest in the S condition where the correlations are very small. In the PI condition, quite a strong tendency existed for subjects to stick to one strategy choice for three or four trials and then switch to another. In the NI and M conditions as well, subjects in general tended to change their choices only after four trials. In general, therefore, these results reveal a tendency in subjects in all conditions except condition S to manifest short-term consistency in their strategy choices. No short-term periodicities of a clear-cut nature are evident.

Post-experimental interviews. In response to the first question in the post-experimental interview, "How do you feel about the overall results?", 44 subjects gave responses which could confidently be coded as "Pleased" (including "Satisfied", "Great" etc.) or "Displeased" (including "Disappointed", "Upset" etc). These responses, and the responses to two other post-experimental interview questions, were distributed across the four treatment conditions as shown in Table 5.7.

Table 5.7: Frequencies of Post-experimental Interview Responses Classified by Treatment Conditions

Question 1: How do you feel about the overall results?

	Treatment Condition			
	PI	NI	M	S
Pleased	11	6	7	7
Displeased	2	4	2	5

Question 2: How do you feel about your partner?

Negative	7	5	4	5

Question 3: What was your general strategy?

Caution	5	5	4	4
Cooperation	7	2	3	3

Although the subjects in the PI treatment condition seem to have been somewhat more pleased with the overall results than subjects in other treatment conditions, it is clear from inspection that the differences are too slight to be statistically significant.

The second question, "How do you feel about your partner?", elicited unambiguously negative comments of various kinds from 21 subjects. The most common negative comments included "Selfish", "Greedy", Inflexible", "Stubborn", "Obstinate", "Uncooperative" and "Submissive", though some subjects used idiosyncratic abusive epithets like "Twat", "A bit of a capitalist", "Pig-headed" etc. Of the remaining subjects, most gave non-committal replies ("Nothing", "Don't know her" etc.) and only a handful gave responses which could possibly be interpreted as positive in tone ("Reasonable", "Sensible" etc.). The distribution of negative comments across treatment conditions is shown in Table 5.7. By inspection, it is clear that the frequencies are not significantly different from chance.

The third post-experimental interview question, "What was your general strategy?", stumped most of the subjects, who gave evasive replies indicating either that they had no general strategy or that they had used an obscure or idiosyncratic one. Of the remainder, most gave replies which clearly indicated that their general strategy was either to stick by and large to the cautious choice ("Choose L most of the time", "Play safe with L on most trials" etc.), or to achieve a joint cooperative outcome with their partners ("Try to get LL", "Persuade him to agree to SEA-SEA" etc.). The distribution of these two overall policies across treatment conditions is shown in Table 5.7. Once again, inspection shows that the frequencies are not different from chance.

Many subjects made insightful and amusing comments on the post-experimental interviews. It is clear from their responses that the subjects generally understood and enjoyed the tasks which were presented to them, and that they entered into the spirit of the game. One subject's comment (from the S condition) is worth quoting, since it cheered the experimenter and seemed to sum up the fundamental idea behind experimental gaming research: "I have never before done an experiment of this kind, but I feel it demonstrates the difficulties of making decisions which involve other people's judgments".

5.4. Discussion

One of the decision contexts in which the game used in this experiment was presented to the subjects -- the matrix decision context -- was similar to that used in previous investigations of behaviour in Chicken games: the subjects were presented with a conventional payoff matrix which embodied a set of rules for making their choices. This type of decision context is entirely abstract in character in the sense that no contextual meaning is given to the strategies and outcomes. Two other decision contexts used in this experiment, the positive incentive and negative incentive decision contexts, were also abstract, but they were designed in such a manner as to appear different from each other and from the matrix decision context. In these incentive conditions, the subjects were presented with a set of verbal instructions, but they were not given a payoff matrix. In the first of these incentive conditions they played to win money, and in the other they played to retain as much money as possible from an allowance given to them before the experiment began. The simulation decision context was, unlike the other three used in this experiment, of an essentially lifelike character: the strategies available to the players were described verbally in terms of a hypothetical situation involving the sale of wholesale vegetables, and the outcomes were imaginary profits.

Each of the payoff structures not only conformed with the formal requirements of the game of Chicken, but bore a ratio-scale equivalence, or in the case of the negative incentive condition an interval-scale equivalence to the others. This ensured that from a logical or strategic point of view, the problem facing the subjects in the four decision contexts was identical; any differences in their behaviour in the four conditions may be attributed entirely to the incentives or the strategically irrelevant contextual information associated with the four versions of the game presented to them. It was hypothesized that this would lead to differences in their strategic choice behaviour, and that the simulation decision context in particular would elicit fewer cautious choices than the other three decision contexts in view of the cultural value attached to risk taking in commercial contexts. The incentive conditions, particularly the negative incentive condition, were expected to elicit greater caution than the matrix condition for intuitive reasons, and differences were expected to become more pronounced over trials.

The results confirmed the major hypotheses in a number of different ways. Firstly, a marginally significant tendency was found for subjects in the simulation decision context condition to produce fewer cautious choices on the first trial than subjects in the abstract decision contexts: only in the simulation condition were a majority of the initial choices risky. Secondly, the overall frequency of cautious choices across the 30 trials of the experiment was less in the simulation condition than in the abstract conditions. The highest level of caution was found in the positive incentive condition, partially confirming a hypothesis mentioned above, and it was found to be significantly higher than that produced by subjects in the simulation condition. The hypothesis that the negative incentive condition would lead to greater caution than the matrix and positive incentive conditions was not confirmed.

The overall frequency of cautious choices in all four treatment conditions was 8.09 per pair per Trial Block, i.e. approximately 40 per cent, which agrees quite well with the frequency found in previous research on Chicken. In the simulation condition, however, the average frequency of about 30 per cent is decidedly on the low side. It is clear that the simulation decision context elicited low levels of caution (or relatively risky choices) in line with the major hypothesis. The most obvious interpretation of this finding centres on the relative realism of the simulation decision context in comparison with the others; it is not surprising that contextual realism should engage cultural values associated with risk taking to a greater degree than abstract or meaningless tasks.

An attempt was made in this experiment to monitor the extent to which subjects departed from the rules of the game explicitly given to them, by introducing extraneous utilities into the situation. After each trial, subjects rated on a five-point scale their degree of satisfaction or dissatisfaction with the outcome of that trial, and the correlations between these satisfaction ratings and the payoffs on corresponding trials was computed separately for each subject. The overall grand mean of these correlations was $r = .712$, indicating that more than half the variance in subjective utilities (as measured by satisfaction ratings) was accounted for by the payoffs received by the subjects. The mean correlation was slightly higher for subjects in the simulation condition, and considerably higher for subjects in the positive incentive condition. It was lowest, however, in the traditional matrix decision context. The differences between these correlations did not attain statistical significance, but the following conclusion is certainly justified: there is no reason whatever to believe that the traditional matrix decision context guarantees a stricter adherence on the part of the subjects to the given payoffs than does a more life-like simulation decision context such as the one used in this experiment.

A subsidiary analysis of the choices made over Trial Blocks by a selected subset of subjects all of whom had adhered closely to the "official" payoffs, revealed a pattern of results which closely resembled the pattern found in the main analysis. The effect found in the main analysis cannot therefore be explained away as an artifact caused by departures from the given payoff structure by subjects in some or all decision contexts.

The component Time Series Analysis revealed that the characteristic secular movements in the subjects cautious choices was slightly downward in all conditions except the negative incentive condition; in the latter a slight upward trend over trials was revealed. The pattern of choices over trials was very similar in the matrix and simulation decision contexts, but this pattern was markedly different from the pattern found in the other two decision contexts. The subjects in the positive incentive condition maintained a much more cautious level of behaviour throughout, while the subjects in the negative incentive condition manifested a cyclical oscillation between cautious and risky choices, with minima occurring at trials 11 and 18. These findings is not easy to interpret, but the following speculations may be offered. Subjects in all treatment conditions manifested relatively high levels of caution to start with; this may be due to the initial unfamiliarity of the tasks and may reflect a period of "feeling out". The steady decline in caution in the positive incentive, matrix and simulation conditions may be accounted for in terms of familiarization: people generally become bolder and are more prepared to take chances the more they feel at home in a situation. For the first 10 trials, subjects in the negative incentive condition gradually shifted to risk in this fashion as well. They could certainly afford (literally) to act riskily because they had already been given a monetary allowance. The sobering period which followed, uniquely in the negative incentive condition, may possibly reflect a growing awareness among subjects in this condition that their resources were rapidly being depleted and an attempt to call a halt to the rapid decline. Their second risky excursion towards trial 18 is harder to explain, but it may have arisen out of a desire on the part of the subjects in this condition to exploit their partners, since by about the 15th trial a high frequency of joint cautious choices were being made (and this is not an equilibrium outcome). Within a few trials their partners may be assumed to have responded to the threat and, if these speculations are correct, a second change of course towards caution would become necessary to avoid repeated financial disasters.

The autocorrelational analysis was motivated by a desire to examine possible short-term periodicities and consistencies in the subjects' choice behaviour, This analysis revealed a tendency of subjects in all treatment conditions to repeat the same strategy choice for at least two consecutive trials. Subjects in the positive incentive condition tended to stick with the same strategy for three or four trials before switching, and a similar tendency was found in the negative incentive condition. This short-term consistency in choice behaviour was least evident in the simulation condition. It may be interpreted as indicating that subjects in all treatment conditions -- though least so in the simulation and most clearly so in the positive incentive conditions -- were fairly resolute in their beliefs about what was the appropriate strategy to adopt, and did not in general switch to the alternative strategy until they had repeated the same choice a few times.

The post-experimental interviews revealed that most subjects in all treatment conditions were either neutral or rather pleased about the overall results of their strategy choices. Nearly all of those who expressed any opinion about their partners, however, produced strikingly negative comments which were sometimes quite abusive. This finding throws into sharp relief the ugly nature of dangerous games in general and the game of Chicken in particular. The strategic structure of Chicken does not encourage people who play it, whether in everyday life or in the laboratory, to grow fond of each other.

The results of this experiment have gone a little way towards answering the question about the relevance of experimental games to everyday strategic interaction. The lifelike simulation used in this experiment was not, of course, a "real" situation in the sense that the choices made were of crucial importance to the subjects, but it was unarguably more lifelike than the abstract games used in most previous gaming experiments.

The behaviour of the subjects in the lifelike simulation was not grossly different from that in the matrix and other abstract decision contexts, but there were some clear-cut differences which in general confirmed the major hypothesis. In particular, subjects behaved less cautiously in the simulation condition. This may be due to the specific content of the lifelike simulation used; other lifelike simulations may engage different cultural values in the minds of the subjects and evoke correspondingly higher or lower levels of risk or caution. The experiment has demonstrated, however, that the use of an abstract payoff matrix from which all contextual meaning is removed may give a rather misleading impression about the behaviour of people in everyday social situations whose strategic structure corresponds to that of the game used in the experiment.

CHAPTER SIX

6. EXPERIMENT IV: ABSTRACT AND LIFELIKE DECISION CONTEXT EFFECTS IN N-PERSON PRISONER'S DILEMMA

6.1. Introduction

The theory of the N-person Prisoner's Dilemma Game has been explained above in Section 1.5, and the published experiments concerned with subjects' strategy choices in this game have been exhaustively reviewed in Section 2.4.

With one notable exception, previous empirical investigations in this area have involved the presentation to subjects of an entirely abstract version of N-person Prisoner's Dilemma. In most cases, the subjects in previous investigations have made their choices on the basis of a payoff matrix similar to the one used in the matrix treatment condition of the experiment described below. Alcock & Mansell (1977) reported an experiment on N-person Prisoner's Dilemma in which a conventional payoff matrix was used, but the subjects were told that the experiment was "a simulation of animal population growth under conditions of scarce resources" (p. 447). They were given, in addition to the payoff matrix, a verbal description of "the tragedy of the commons" and were assigned the roles of cattle farmers. Their strategy choices were labelled "Add" (an animal to the pasture) and "Not add". The proportion of cooperative choices of 70 subjects assigned to 10 7-person groups averaged .32, and false feedback regarding the cooperative or competitive choices of the other group members had no significant effect on the strategy choices of the subjects.

Alcock & Mansell's (1977) investigation is interesting largely on account of the implication of their findings (in the light of other studies in the area) that the relatively lifelike decision context in which the game was presented to the subjects did not lead to behaviour which was strikingly different from that found in experiments using abstract decision contexts. This inference is conjectural, however, since no attempt was made to compare the behaviour of subjects in abstract and lifelike decision contexts in the same experiment, i.e. under controlled conditions in which all other sources of systematic variation are held constant. The experiment reported below represents the first attempt to make such a comparison.

In this experiment, four decision contexts were used for the presentation of the game. One involved a lifelike simulation of a dilemma facing the Economics Ministers of three hypothetical oil-producing nations regarding the desirability of full versus restricted production. Another of the decision contexts resembled the matrix presentation used in most previous experiments in this area. Two further treatment conditions allowed the effects of monetary incentives to be investigated for the first time in an N-person Prisoner's Dilemma Game: in one the subjects played for half-pennies according to a verbally-presented payoff structure (without a matrix) and in another they played under similar conditions except for the fact that they lost money according to the payoff structure after each trial rather than gaining it. This last decision context models situations, well known in real life, in which the best a person can hope for is the preservation of the status quo and the worst to be feared is substantial loss.

An interval-scale equivalence was maintained between the payoff structures used in the four treatment conditions. The matrix, positive incentive and simulation conditions had payoff structures which were, in fact, identical to one another on an interval scale. The payoff structure used in the negative incentive treatment condition was derived from the others by subtracting four units from each of the payoffs in the other payoff structure. The four decision contexts may thus be regarded as versions of the same game. Any differences observed in the behaviour of the subjects in the four treatment conditions can be attributed to the psychological peculiarities of the various decision contexts -- the formal strategic properties are the same in all cases and there are no logical reasons for any differences in choice behaviour.

In view of the questions which have recently been raised about the ecological validity of the findings of gaming experiments (e.g. Nemeth, 1972; Schlenker & Bonoma, 1978), a comparison of the choices of subjects in a lifelike dilemma with those in the various abstract versions of the game is of some considerable interest and importance. In particular, the behaviour of subjects in the lifelike simulation may give a more realistic indication of how they might behave in a genuine life dilemma than can an abstract and essentially meaningless task. The effects of monetary incentives are also of some interest in view of the contradictory findings regarding incentives in two-person Prisoner's Dilemma games (see Section 2.3 above). A comparison of the behaviour of subjects in the positive incentive and negative incentive conditions may furthermore demonstrate the effects of acquisitive versus conservative psychological motivations on strategic choice behaviour.

The major hypothesis was that the frequency of cooperative choices would differ between different treatment conditions on account of their obvious (though logically irrelevant) psychological differences. A more specific hypothesis was that subjects in the lifelike simulation would manifest less cooperative choice behaviour than subjects in the other (abstract decision context) treatment conditions. This hypothesis was based upon a finding by Eiser & Bhavnani (1974) that decision-making situations involving commerce tend to engage cultural values associated with competitiveness in Western industrial societies: since the simulation involved decisions in this area it was felt that subjects would feel encouraged to display a culturally valued competitive orientation to a greater degree than in the abstract decision contexts. A final hypothesis was that the frequency of cooperative choices in all treatment conditions would decline over trials; this hypothesis derived from numerous findings showing a decline in cooperation over the first 30 trials in two-person Prisoner's Dilemma (see above Section 2.3).

6.2. Method

Design. The design of this experiment was a 4 x 3 factorial with repeated measures on the second factor (Winer, 1962, Ch. 7). The four levels of the first factor were labelled PI (positive incentive), NI (negative incentive), M (matrix) and S (simulation). The manipulation of this independent variable was identical to that used in Experiments II and III above, apart from the fact that a different payoff structure appropriate to 3-person Prisoner's Dilemma (see Figure 6.1 below) was used, together with a correspondingly different lifelike simulation (see below). Details of the design were given in Section 4.2.

Subjects were randomly assigned to treatment conditions in groups of three, and made 30 successive joint decisions which were divided up into three Trial Blocks of 10 decisions each. Trial Blocks was thus the second factor in the experimental design. The dependent variables were the number of cooperative choices made on the first trial in each treatment condition, and the number of cooperative choices per group made in each Treatment Condition x Trial Block. According to the theory of N-person Prisoner's Dilemma, a cooperative choice was defined as "L" in the abstract conditions and "RESTRICTED" in the simulation condition (see below).

Subjects. The subjects were 120 undergraduate students at the University of Leicester, randomly assigned to each of the four treatment conditions in groups of three, and then randomly assigned to play Blue, Green or Red. There were 66 males and 54 females. Ages ranged from 18 to 39 with a median of 20. Second and third year students reading Psychology were not included in the sample.

Procedure. Each group of three subjects was tested in a small room. The subjects were seated facing into the corners of the room in such a way that they could not see each other. Each was provided with a score sheet similar to that shown in Appendix A, modified to allow the choices and payoffs of two others to be recorded, a decision card in blue, green or red, similar to Appendix B in the abstract treatment conditions and similar to Appendix C in the simulation condition, but containing the words RESTRICTED and FULL, and a rating scale (Appendix D).

Subjects in the PI condition received the following typewritten instructions:

> Your task will consist of making a series of 30 decisions. You may each earn up to 120 half pence (that is, 60p) in this experiment if your decisions turn out well, but you may get less. How much money you eventually go home with will depend not only on the decisions which you make but also on the decisions which the other people make. You are advised, therefore, to consider each decision carefully.
>
> After each decision you will receive payment in the form of 1, 2, 3 or 4 half pence. In each case your decision, as shown on the cards in front of you, will be either R for Right or L for Left. The rules governing payment are as follows: if you all choose L, you each get 3 coins. If you all choose R, you each get two coins. If one person chooses L and the other two choose R, the person choosing L gets 1 coin, and the people choosing R get 3 coins each. If, finally, two people choose L, they each get 2 coins while the third group member (who has chosen R) gets 4 coins.

Subjects in the NI condition were given the following typewritten instructions:

> Your task will consist of making a series of 30 decisions. You may earn up to 120 half pence (that is 60p) in this experiment if your decisions turn out well, but you may get less. How much money you eventually go home with will depend not only on the decisions which you make, but also on the decisions which the other people make. You are advised, therefore, to consider each decision carefully.
>
> To start with, you will each receive 120 half pence coins. After each decision you will have to forfeit 3, 2, 1 or 0 coins. In each case your decision, as shown on the cards in front of you, will be either R for Right, or L for Left. The rules for forfeiting coins are as follows:
>
> If you all choose L, you each forfeit 1 coin. If you all choose R, you each forfeit 2 coins. If one person chooses L and the other two choose R, the person choosing L forfeits 3 coins, and the people choosing R forfeit 1 coin each. If, finally, two people choose L, they each forfeit 2 coins while the third group member forfeits 0 coins (i.e. he keeps all his coins).

In the M condition, each subject was given a payoff matrix similar to the one shown in Figure 6.1.

Number Choosing R	Number Choosing L	Payoff to each R chooser	Payoff to each L chooser
0	3	-	3
1	2	4	2
2	1	3	1
3	0	2	-

Figure 6.1. Payoff Matrix Used in Matrix Treatment Condition.

In addition, subjects in this condition received the following explanatory instructions in typewritten form:

> Your task will consist of making a series of 30 decisions. You may each earn up to 120 points if your decisions turn out well, but you may get less. How many points you eventually end up with will depend not only on the decisions which you make but also on the decisions which the other people make. You are advised, therefore, to consider each decision carefully.
>
> After each joint decision you will receive 1, 2, 3 or 4 points. Your decision, as shown on the cards in front of you, will in each case be either R for Right or L for Left. The scheme for awarding points is summarised on the payoff diagram. The number of points you gain on each trial will depend entirely on the number of people in the group who choose L and R, as shown.
>
> By examining the payoff diagram you can easily see what the outcome of each joint decision will be for each of you. If, for example, one person chooses R and the rest choose L, then the person choosing R gets 4 points and each of the people choosing L gets 2 points, and so on.

In the S condition, the following typewritten instructions were presented to the subjects:

> Your task will consist of making a series of 30 decisions. You may do very well for yourself if your decisions turn out well, but you may be less successful. The outcome in each case will depend not only on the decisions which you make, but also on the decisions which the other people make. You are advised, therefore, to consider each decision carefully.
>
> Your decisions will be based on the following hypothetical situation. You are the Minister of Economics of one of the leading oil exporting countries. The other people in your group are the ministers representing the other leading oil exporters. The decision facing each of you at the start of each financial year is whether to adopt a policy of restricted oil production or whether to go in for full production, and your sole objective is to maximize the revenue your own country will receive from foreign sales of oil; all other considerations are irrelevant to you.

> The following facts are known to you and to all the other members of the group. A policy of restricted oil production, provided it is adhered to by all the group members, will result in the price of oil on world markets being kept high for that financial year, and each country will receive £3m in foreign revenue. If, however, one of the ministers opts for full production while the other two restrict production, his country will sell more oil although, because of the effects of supply and demand, the price of oil on world markets will fall somewhat, and this fall will affect all the members of the group. The net effect of this will be that the country opting for full production will earn £4 in foreign revenue, while the other two will be reduced to £2m each. If two of the three countries go in for full production, the corresponding fall in world oil prices will be greater, and they will each earn £3m, while the third country's revenue will be reduced to £1m. If, finally, all three countries go in for full production, they will each earn only £2m for the financial year. The net effect of these considerations is summarised below.
>
> (a) All three restrict production; each earns £3m.
>
> (b) One country only chooses full production; that country earns £4m, the other two get £2m each.
>
> (c) Two countries choose full production; they each earn £3m, the third earns £1m.
>
> (d) All three choose full production; each earns £2m.

Subjects in all treatment conditions were given approximately five minutes to familiarize themselves with the materials and instructions, after which the experimenter quizzed them and provided additional explanations in the manner described in Section 3.2 above. In the NI treatment condition, each of the three subjects was provided with 120 half-pennies. In all treatment conditions, the following typewritten instructions were then issued:

> You are now going to make a series of 30 joint decisions. Each decision will be made without knowledge of what the other people have chosen by pointint to one of the two cards in front of you. Your decisions will be irreversible, and any attempt to communicate with or indicate your feelings to the other people, for example by sighing or laughing, will force the experimenter to terminate the experiment. When you have all teached a decision, the experimenter will announce the choices: you will all know what the other people have decided and you will be able to work out how each of you has fared in terms of payoffs.
>
> You will then record all three decisions and all three payoffs on the scoring sheet, together with a rating for how pleased or displeased you are with the outcome of that particular decision, before going on to make the next decision. The experimenter will show you how to fill in the scoring sheet.

In the PI condition, the following was appended:

> You will be paid the appropriate amount after each decision. Remember that your sole objective is to accumulate as many coins as possible. After 30 trials your money will be converted into more convenient coins for you to take home.

The NI condition had this addition:

> You will receive 120 coins each before you begin, and the appropriate amount will be removed after each decision. Remember that your sole objective is to retain as many coins as possible. After 30 trials, your money will be converted into more convenient coins for you to take home.

The following was added in the M condition:

> You will be able to take stock of your position after each trial. Remember that your sole objective is to accumulate as many points as possible. After 30 trials, you will be able to add up your points.

Finally, in the S condition, the following was added:

> You will be able to take stock of your position after each trial. Remember that your sole objective is to accumulate as much foreign revenue as possible. After 30 trials, you will be able to see how you have done over the whole period.

The subjects were then invited to make their first choices, and the procedure continued exactly as described in Section 4.2, up to and including the post-experimental interview and de-briefing.

6.3. Results

Cooperative Choices on Trial 1. As has been mentioned above, initial choices in experimental games may be regarded as being stochastically independent of one another. The number of independent observations is therefore N = 120 in this case. The choices of one group in the M condition were however excluded from this analysis on account of a recording error. The distribution of cooperative and competitive initial choices across the four treatment conditions is shown in Table 6.1.

Table 6.1: Cooperative and Competitive Choices on Trial 1 in Four Treatment Conditions (N = 117[a])

	Treatment Condition			
	PI	NI	M	S
Cooperative Choice	12	16	13	8
Competitive Choice	18	14	14	22

[a] The choices of one group in condition M excluded on account of a recording error.

A Chi Square test (Siegel, 1956, pp. 175-179) performed on this data produced a non-significant result ($\chi^2 = 4.39$, d.f. = 3, $.05 < p < .30$). A major hypothesis was that subjects in the S condition would display less cooperative behaviour than subjects in the abstract decision context conditions on account of the cultural value associated with competitiveness in business ventures. An a priori comparison was therefore made between the proportion of cooperative choices made on the first trial in conditions PI, NI and M (41/87) and the corresponding proportion in the S condition (8/30). A z test (Bruning & Kintz, 1977, pp. 222-224) produced a significant difference ($z = 1.96$, $p < .05$, two-tailed) thus confirming the hypothesis with regard to initial choices.

Overall cooperative choices. The second dependent variable was the number of cooperative choices made in each of the four treatment conditions over each of the three Trial Blocks. The scores within each group are not stochastically independent of one another, so the units of analysis to be used are aggregate scores per Trial Block for each group of subjects; N = the number of groups in each case. The raw scores for the 40 groups are tabulated in Appendix K and the means are shown in Table 6.2.

Table 6.2: Mean Frequency of Cooperative Choices Per Group:

Treatments x Trial Blocks (N = 40)

Treatment Condition	Trial Block 1	Trial Block 2	Trial Block 3
PI	10.50	6.00	4.40
NI	12.20	10.80	7.50
M	12.90	9.90	7.40
S	7.90	4.50	4.40

The overall grand mean is 8.20, which (taking into account that there were three subjects in each group) indicates that a little over 27 per cent of the choices made by subjects in this experiment were cooperative. An Analysis of Variance (Winer, 1962, Ch. 7) was performed on these data; the results of this analysis are summarized in Table 6.3.

Table 6.3: ANOVA Summary Table: Cooperative Choices Per Group:

(Treatments x Trial Blocks, N = 40)

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
A(Treatments)	486.23	3, 36	162.08	32.31	5.02	$p < .01$
B(Trial Blocks)	522.82	2, 72	261.41	4.85	53.93	$p < .001$
AB	40.85	6, 72	6.81	4.85	1.40	n.s.
Total	2561.99	119				

Two significant main effects emerged from this analysis: an effect due to Treatments and one due to Trial Blocks. *A posteriori* analysis with the Tukey test (Bruning & Kintz, 1977, pp. 122-124) revealed that the following pair-wise comparisons were significant beyond $p < .05$: the S condition elicited significantly fewer cooperative choices than either the NI or the M conditions, and the frequency of cooperative choices decreased from Trial Block 1 to Trial Block 2, and from Trial Block 2 to Trial Block 3.

Payoff-satisfaction correlations. For various reasons, subjects in experimental games may be unable or unwilling, to adhere strictly to the payoffs built into the game structures, and in such cases an interpretation of their behaviour is problematical on account of the unknown latent structure of the games being played. In an attempt to minotor such departures from the explicit payoff structure of the game used in this experiment, product-moment correlations were computed separately for each subject between his payoffs on each trial and his satisfaction ratings for the corresponding outcomes. Details and an explanation of this procedure have been explained above in Sections 2.5 and 3.3.

The product-moment correlations between payoffs and satisfaction ratings are tabulated in Appendix L. The scores are stochastically independent of one another, and $N = 120$. The grand mean is $r = .760$, which indicates that almost 60 per cent of the variance in satisfaction ratings is accounted for by the payoffs received by the subjects. The mean correlations for the treatment conditions PI, NI, M and S were $r = .78$, $r = .73$, $r = .77$ and $r = .76$ respectively. A one-way Analysis of Variance

(Winer, 1962, Ch. 3) was computed in order to determine whether the differences between the correlations in the four treatment conditions are significantly different from one another. The results of this analysis are summarized in Table 6.4.

Table 6.4: ANOVA Summary Table: Correlations Between Subjects' Payoffs and Satisfaction Ratings in Four Treatment Conditions (N = 120)

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
Treatments	.06	3,116	.02	.01	.67	n.s.
Total	1.17	119				

The differences between the correlations is evidently not significant. It is worth pointing out, however, that only three subjects manifested perfect payoff-satisfaction correlations: two in condition PI and one in condition S. This implies that extraneous utilities were introduced into the game by most subjects. A subsidiary Analysis of Variance was therefore performed in order to discover whether the significant effects found in the main Analysis of Variance were artifacts caused by departures on the part of some subjects from the explicit game structure.

Subsidiary analysis. The subsidiary Analysis of Variance, like the main analysis described above, was based upon the cooperative choices of subjects in each of the four treatment conditions over the three Trial Blocks. In this case, however, only the scores of subjects who manifested extremely high payoff-satisfaction correlations were used. In each treatment condition, the five subjects (out of 30) with the highest correlations were identified. These subjects are shown starred in Appendix L. The number of cooperative choices which these 20 subjects made in each of the three Trial Blocks were used as raw scores in the subsidiary analysis. As things turned out, none of these subjects happened to belong to the same group as one of the others (see Appendix L); their cooperative choices may therefore be regarded as being stochastically independent of one another. Their payoff-satisfaction correlations ranged from $r = .86$ to $r = 1.00$ with a median of $r = .95$, and their mean cooperative choices are shown in Table 6.5.

Table 6.5: Mean Cooperative Choices of Subjects With High Payoff-satisfaction Correlations: Treatment Conditions x Trial Blocks ($N = 20$)

Treatment Condition	Trial Block 1	Trial Block 2	Trial Block 3
PI	4.60	1.20	1.40
NI	3.80	5.00	1.80
M	4.40	3.20	1.80
S	2.00	.40	.40

These means are roughly comparable with those found on the entire sample of 120 subjects (Table 6.2) when the appropriate transformation is made to allow for the fact that the scores in this case are taken from individual subjects rather than from groups of three. The most noticeable difference is that the means for the S condition are proportionately lower than those of the entire sample. An Analysis of Variance (Winer, 1962, Ch. 7) was performed on this data, and the results of this analysis are summarized in Table 6.6.

Table 6.6: ANOVA Summary Table: Cooperative Choices of Subjects With High Payoff-satisfaction Correlations (Treatment x Trial Blocks, N = 20)

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Error Term	F Ratio	Significance Level
A(Treatments)	59.00	3, 16	19.67	5.00	3.93	$p < .05$
B(Trial Blocks)	55.30	2, 32	27.65	1.56	17.70	$p < .001$
AB	32.70	6, 32	5.45	1.56	3.49	$p < .01$
Total	277.00	59				

The significant main effects due to Treatments and Trial Blocks found in the main Analysis of Variance (Table 6.3) were replicated in this subsidiary analysis. In addition, a significant interaction Treatments x Trial Blocks was found in this subsidiary analysis. The interaction is illustrated graphically in Figure 6.2.

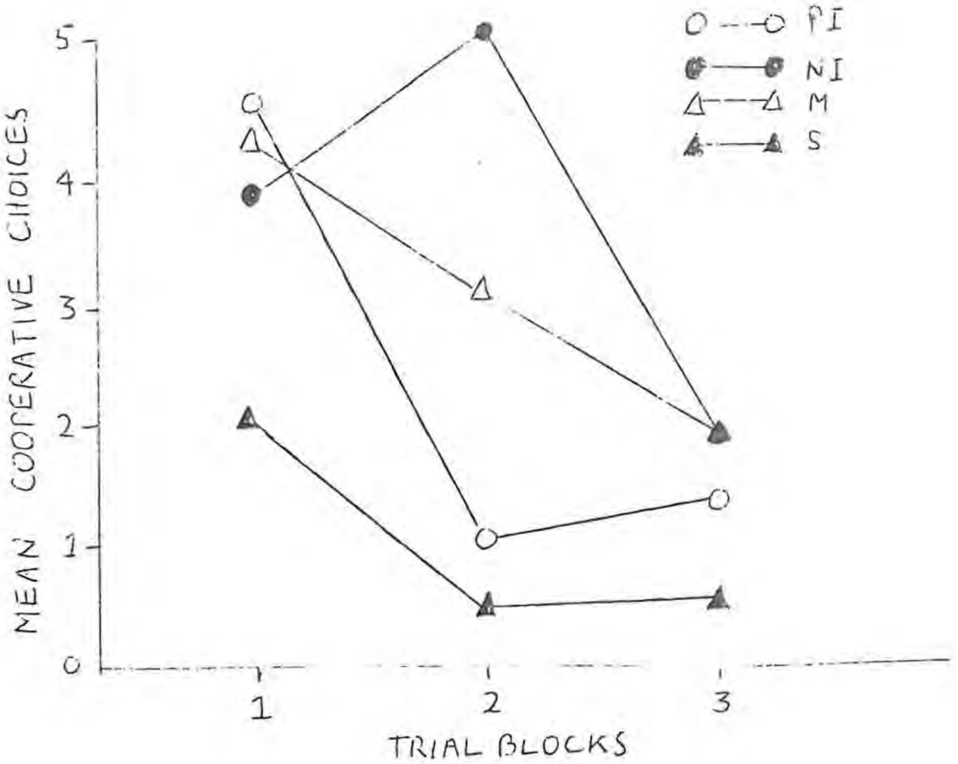


Figure 6.2. Cooperative Choices of Subjects With High Payoff-Satisfaction Correlations in Four Treatment Conditions: N = 20.

A posteriori analysis with the Tukey test (Bruning & Kintz, 1977, pp. 122-124) confirmed the result of the main analysis in which the differences between the Trial Block means were all significant beyond $p < .05$, showing a decline from Trial Block 1 to Trial Block 2 and from Trial Block 2 to Trial Block 3. In this case, however, only one difference between the means of the treatment conditions was significant beyond $p < .05$: the S condition elicited fewer cooperative choices than the NI condition.

The most striking features of the Treatments x Trial Blocks interaction shown in Figure 6.2 are the following: (a) only the subjects in the NI condition manifested an increase in cooperation from Trial Block 1 to Trial Block 2; subjects in the other treatment conditions showed a sharp decline; and (b) subjects in the

NI and M conditions manifested a decline in cooperation from Trial Block 2 to Trial Block 3, while those in conditions PI and S maintained a roughly constant mean level of cooperation from Trial Block 2 to Trial Block 3.

It is worth noting that the overall pattern of these results is roughly similar to that found in the group scores of the entire sample of 120 subjects, although in that analysis subjects in all conditions with the exception of condition S manifested a decline in cooperation from Trial Block 1 to Trial Block 2 and from Trial Block 2 to Trial Block 3, which accounts for the failure to find a significant interaction in the main analysis.

Time Series Analysis. The changes which occurred in the frequency of cooperative choices over trials in this experiment were interesting and complex. Analysis of Variance is not, however, the ideal statistical technique for investigating such time-bound effects; the appropriate techniques are those of Time Series Analysis. The first part of the Time Series Analysis performed on the data was a simple component analysis (Brown, 1963; Spiegel, 1961, Ch. 16). Unweighted moving averages of order 5 and 15 were calculated for the mean cooperative choices in each of the four treatment conditions (averaged over groups) across the 30 trials of the experiment. The results are illustrated graphically in Figure 6.3.

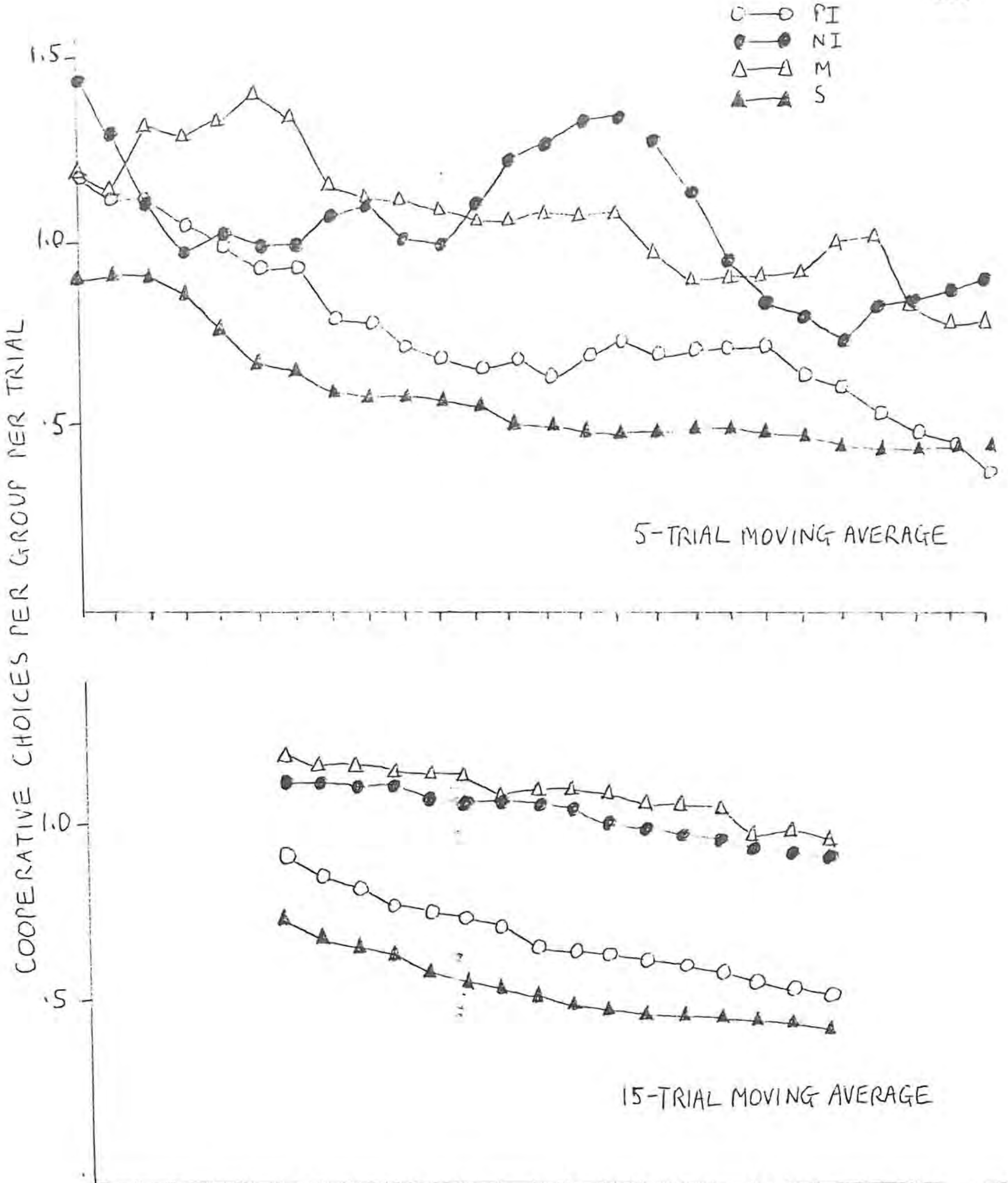


Figure 6.3. Five-trial and 15-trial Unweighted Moving Averages of Cooperative Choices Per Group Per Trial in Four Treatment Conditions.

The 15-trial moving averages reveal a steady secular decline in cooperation over trials in all treatment conditions. They also reveal that the relatively low frequency of cooperative behaviour in the S condition compared with the abstract conditions, and in the PI condition compared with the NI and M conditions, was consistent over time when cyclical and irregular fluctuations are suppressed. When only irregular fluctuations are suppressed -- in the graphs of the 5-trial moving averages -- the differences between treatment conditions mentioned in the previous sentence are still evident with almost total consistency over time.

The 5-trial moving averages reveal furthermore that very little cyclical activity was present in the characteristic movements of the time series except in the NI condition and possibly to a small degree in the M condition. The characteristic movements in the PI and S conditions were approximately linear. In the NI condition there was a strong cyclical component, with minima occurring at Trials 6 and 24 and a peak at Trial 18.

A further Time Series Analysis using the autocorrelation formula given in Section 4.3 above was performed in order to investigate possible short-term consistencies and periodicities in the subjects' choice behaviour. The autocorrelation functions up to lag = 5 for cautious choices in the four treatment conditions are shown graphically in Figure 6.4.

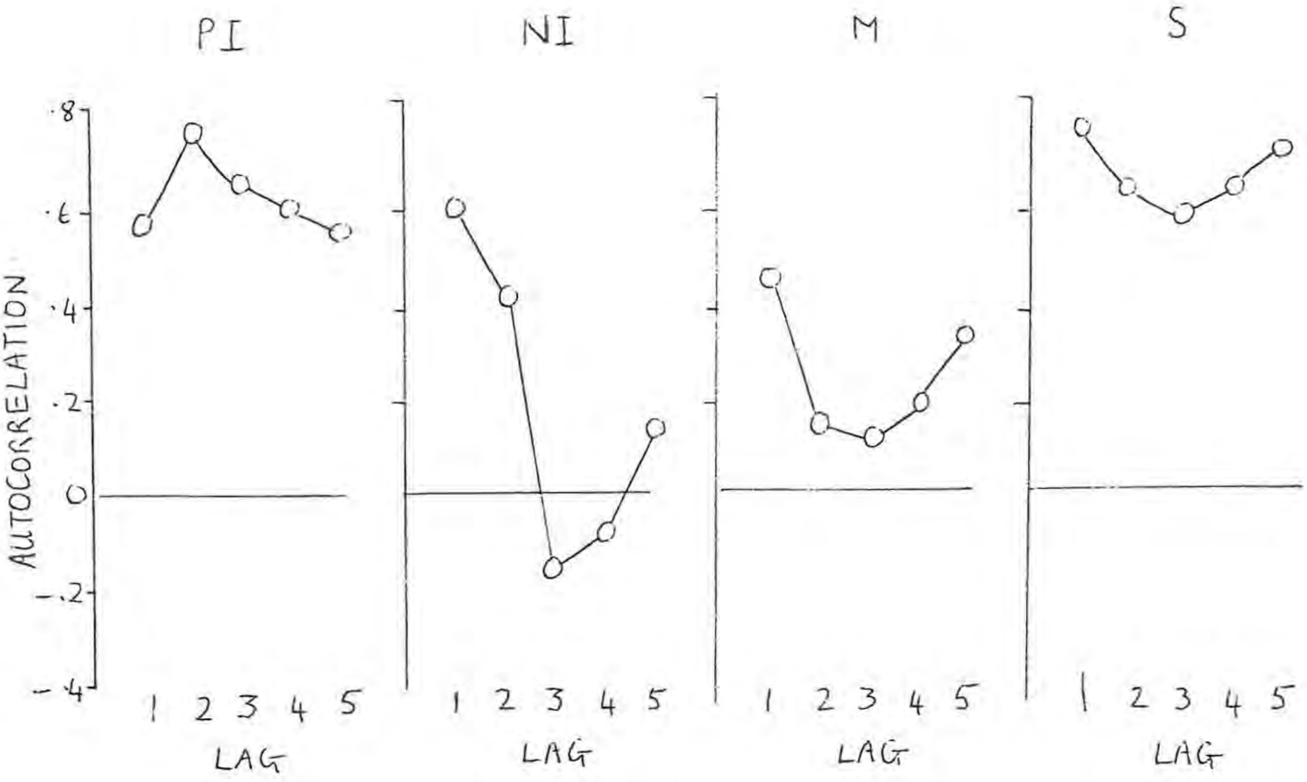


Figure 6.4. Autocorrelation Functions of Cooperative Choices in Four Treatment Conditions.

The results of the autocorrelational analysis first of all confirm, with respect to short-term tendencies, the findings of the component Time Series Analysis. An extremely strong short-term consistency is evident in the subjects' strategy choices in the PI and S treatment conditions only. In these conditions, the subjects displayed a very strong tendency to adhere to the same strategy choice for at least six trials (up to lag = 5). The same consistent tendency is evident in the M condition, but is not so strong in this case (the correlations are weaker). In the NI condition, however, subjects tended to adhere to the same strategy choice for three trials only, after which there was a weak tendency to switch to the alternative strategy choice.

Post-experimental interviews. The post-experimental interviews elicited a large number of comments indicating high levels of involvement on the part of the subjects in the game in all treatment conditions. Most of the subjects apparently took the task very seriously, and strong emotions were frequently expressed. One subject in the PI condition, for example, described her fellow group members as "a pair of - -!", and epithets like "bastards" or "capitalist bastards" were by no means uncommon. A subject in the NI condition confessed somewhat poignantly: "I found the exercise quite exciting, and after a while began to take losses perhaps too seriously". Another subject in this treatment condition said: "Green shouldn't be in a university. He should be in an E.S.N. School". Strongly coloured remarks were least common in the M condition as might be expected, but were extremely common in the S condition. One subject in the S condition admitted: "I would not make a very good Minister of Economics"; another reflected: "I suppose I'm either an idealist or a sucker"; and a third took a more self-assertive line: "Knowing that some fool would be immovable, perhaps I should have just made sure I'd get the better of them".

In answer to the question: "How do you feel about the overall results?", 69 subjects gave replies which could confidently be classified as "Pleased" (including "Satisfied", "Reasonably content" etc.) or "Displeased" (including "Annoyed", "Upset" etc.). In answer to the question: "How do you feel about the other group members?", 69 gave replies which fell clearly into the categories

"Positive" ("Sensible", "Reasonable" etc.) or "Negative" ("Uncooperative", "Silly", and various more or less abusive predicates). In response to the question: "What was your general strategy?", 61 subjects gave replies indicating either a general policy of competitiveness ("R" or "FULL") or one of attempting to achieve a joint cooperative outcome ("LLL" or "All RESTRICTED"). The frequencies of these typical responses, classified according to treatment conditions, as shown in Table 6.7.

Table 6.7: Frequencies of Post-experimental Interview Responses Classified by Treatment Conditions

Question 1: How do you feel about the overall results?

	Treatment Condition			
	PI	NI	M	S
Pleased	10	6	9	8
Displeased	7	10	9	10

Question 2: How do you feel about the other group members?

	PI	NI	M	S
Positive	4	2	11	0
Negative	15	15	5	17

Question 3: What was your general strategy?

	PI	NI	M	S
Cooperate	5	5	1	5
Compete	18	8	11	8

The distribution of replies to Question 1 are fairly evenly balanced, and this distribution does not differ significantly from chance ($\chi^2 = 1.69$, d.f. = 3, n.s.).

Question 2, on the other hand, generated a rather unbalanced distribution of replies. It is evident, for example, that negative comments were more than three times as frequent as positive comments. Unfortunately the frequencies are too small to permit the use of the Chi Square test. The main interest of this experiment, however, rests upon the differences between the lifelike and abstract decision contexts; the frequencies of the PI, NI and M conditions were therefore combined, and a comparison was made between the proportion of negative comments in these groups (35/52) and the corresponding proportion in the S group (17/17). Using a version of the z test (Bruning & Kintz, 1977, pp.222-224), the difference turned out to be highly significant ($z = 2.71$, $p < .007$, two-tailed), indicating that subjects in the S condition significantly more often expressed negative opinions about their fellow group members than did subjects in the abstract decision context conditions.

The distribution of responses to Question 3 is uneven, and once again the expected frequencies are too small to permit the use of the Chi Square test. Following the procedure described above, it was found that the proportion of competitive general policies was not significantly smaller in the S condition than in the PI, NI and M conditions combined ($z = 1.13$, $.05 < p < .26$, two-tailed). It is evident from an examination of Table 6.7, however, that the proportion of competitive general policies greatly exceeds the proportion of cooperative general policies in all treatment conditions: a test of significance is once again unnecessary in this case.

6.4. Discussion

The major hypotheses were that the frequencies of cooperative choices would be affected by the decision contexts in which the game was presented and, more specifically, that the prevailing cultural value attached to competitive behaviour in business ventures would result in less cooperation in the simulation condition than in the abstract decision context conditions.

The results strongly confirmed the major hypotheses. On the first trial, the proportion of cooperative choices was significantly smaller in the simulation condition than in the abstract decision context conditions. The overall frequency of cooperative choices over the 30 trials of the experiment was significantly less in the simulation condition than in the negative incentive and matrix conditions (it was also less than the frequency found in the positive incentive condition, but this difference was not significant). The conclusion is inescapable that the decision context in which the game was presented influenced the strategy choices of the subjects in a systematic fashion in accordance with the main predictions. The component Time Series Analysis provided a vivid illustration of the consistent way in which subjects' behaviour varied across the different treatment conditions.

On the basis of previous findings on two-person Prisoner's Dilemmas, it was further hypothesized that the frequency of cooperative choices, irrespective of treatment conditions, would decline over Trial Blocks. This hypothesis was also strongly confirmed by the results: there was a significant decline from Trial Block 1 to Trial Block 2, and a further decline from Trial Block 2 to Trial Block 3. The component Time Series Analysis revealed this decline particularly clearly, and also showed that it was consistent across treatment conditions. The characteristic movement in cooperative choices was approximately linear in all treatment conditions except the negative incentive condition; this latter condition revealed a strong cyclical component -- a sharp decline in cooperation lasting six trials followed by an increase across the following 12 trials, followed by a decline for a further six trials, and finally an increase over the final six trials. The approximately linear secular decline in cooperation in the positive incentive, matrix and simulation conditions is not surprising: it probably reflects a "sobering period" (Rapoport & Chammah, 1965)during which the subjects gradually became aware of the dilemma in the strategic structure of the game after an initial attitude of goodwill (or perhaps naivety). The cyclical activity in the negative incentive condition is harder to explain, and deserves further comment.

What was unique (from a psychological point of view) about the negative incentive decision context was that the subjects were playing to conserve rather than to gain utilities. The values of the various possible outcomes were equivalent to those of the other decision contexts in the sense that a subject would achieve the same gross payoff after a particular sequence of outcomes in this decision context as in the others. For example, after a series of 30 joint cooperative outcomes he would end up, as would subjects in any other decision contexts, with a gross accumulation of 90 payoff units, and <u>mutatis mutandis</u> the same is true for all other possible sequences of outcomes. An interpretation of the unusual cyclical characteristic movement in cooperative choices in the negative incentive condition must necessarily focus on the peculiar psychological features of this decision context. The key may perhaps be found in the psychological construction of joint competitive outcomes. In the positive incentive, matrix and simulation conditions, the result of an unbroken series of joint competitive outcomes would be small gains to each of the players; in the negative incentive condition the result would be the same in a logical sense, but these small gains are likely to have been perceived as large losses. This implies that the linear decline in cooperation in the positive incentive, matrix and simulation conditions may have reflected the willingness of subjects to settle for small gains, and the cyclical activity in the negative incentive condition may have reflected the unwillingness of subjects to tolerate what appear to be large losses for more than a few trials. The results of the autocorrelational Time Series Analysis are consistent with this interpretation: the tendency to adhere to the same strategy choice for several trials was least evident in the negative incentive condition. Long series of joint cooperative choices are, of course, not to be expected for reasons related to the strategic structure of the game: the joint cooperative outcome (unlike the joint competitive outcome) is not an equilibrium point in the game -- there is always a temptation to defect from this outcome.

An attempt was made in this experiment to examine the extent to which subjects adhered to the payoff structure of the game or, on the other hand, introduced extraneous utilities which might have altered the game structure. Correlation coefficients were calculated, separately for each of the 120 subjects, between their payoffs on each trial and their satisfaction, rated on a five-point scale, with the outcomes on the corresponding trials. The results were rather encouraging: the grand mean of the correlations ($r = .760$) indicated that more than 60 per cent of the variance in subjective utilities (as measured by satisfaction ratings) was accounted for by the payoffs. Of some interest was the negative finding that these correlations did not differ significantly between treatment conditions. There was, in other words, no indication that departures from the given game structure were any greater in one decision context than any other. It is particularly encouraging that the mean correlation in the simulation condition was very close indeed to those in the other (abstract) decision contexts. The use of a lifelike simulation for investigating subjects' choices in a N-person Prisoner's Dilemma can not, therefore, be rejected on the ground that subjects are more likely to alter the subjective payoff structure in such a decision context than in the traditional abstract versions of the game.

The payoff-satisfaction correlations were for the most part less than unity, however, indicating slight deviations from the payoff structure in all treatment conditions. It is impossible to guess the extent to which this may have influenced the results of the main analyses. A subsidiary Analysis of Variance was therefore performed, using a small subset of subjects who had all manifested extremely high payoff-satisfaction correlations (the median was $r = .95$). The pattern of results was strikingly similar to that found when all subjects were used. The chief differences were firstly that the generally low level of cooperation in the simulation condition was even more pronounced, and secondly than an interaction Treatments x Trial Blocks emerged. This interaction was due to the atypical rise and fall in cooperative choices in the negative incentive condition, which the main Analysis of Variance had failed to detect.

The post-experimental interviews revealed an extremely high level of emotional involvement on the part of subjects in all treatment conditions. A striking discovery, though it is in line with earlier findings reported by Dawes, Delay & Chaplin (1974), was the high frequency of strong negative reactions which the subjects evinced towards one another following the gaming experience. Negative comments about fellow group members were more than three times as common as favourable comments, and many quite abusive epithets were used. This was particularly noticeable in the lifelike simulation, in which a significantly greater proportion of such comments were made than in the abstract decision context conditions. The results of the post-experimental interviews tend to support the view that the behaviour of subjects in an N-person Prisoner's Dilemma may reasonably reflect their behaviour in corresponding real life situations. This inference seems most justified in cases where a lifelike simulation rather than an abstract game is used.

The most important aspect of this experiment was the establishment of a viable methodology for presenting N-person Prisoner's Dilemma in a lifelike form in a manner which makes generalizations from the laboratory to other social situations less dubious. The lifelike simulation may be regarded as intermediate between the highly abstract matrix conditions used in previous research in this area and the social dilemmas of everyday life which the N-person Prisoner's Dilemma purports to model.

A related inference is that traditional N-person Prisoner's Dilemma research findings may have limited ecological validity. Subjects evidently do not behave in the same manner in abstract and lifelike N-person Prisoner's Dilemmas. The use of a different lifelike simulation from the one used in this experiment might have produced a different set of results. The use of abstract games from which all contextual meaning is removed does not, however, appear to be the appropriate approach to research which is intended to illuminate the behaviour of people in meaningful life situations.

CHAPTER SEVEN

7. CONCLUSIONS

7.1. Conspectus

The results of each of the four experiments which constitute the empirical component of this dissertation have been discussed in some detail in Sections 3.4, 4.4, 5.4 and 6.4 above; nothing would be gained by covering the same ground or by simply reiterating in this chapter the discursive comments made above. Since there are several conceptual and methodological threads connecting the four experiments, however, there does seem to be some merit in the idea of gathering these threads and knitting them together in order to obtain a more integrated picture of the conclusions to which they lead. This chapter will therefore be brief, and will consist chiefly of a comparison and integration of the findings of Experiments I, II, III and IV. Before embarking on a discussion of these findings, however, it seems desirable -- even at the risk of some repetition -- to re-consider the conceptual background of the experiments.

The experiments reported in this dissertation were all addressed to the question of the validity of experimental games as a method for the investigation of behaviour in strategic interactions. Validity is a complex concept: there are many facets to it, and various classificatory schemes with regard to validity and reliability have been suggested by psychometricians since the 1930s. In recent years, increasing attention has been paid to the distinction between internal validity (the extent to which the results of an experiment may be attributed to the independent variable[s] under investigation), external validity (the extent to which the results of an experiment may be generalized to other experimental situations in which the same hypotheses are tested), and ecological validity (the extent to which the results of an experiment may be generalized to naturally occurring situations). It is the question of ecological validity which has increasingly interested social psychologists in the past decade, and it is this question in particular which motivated the experiments described in this dissertation.

The problem of ecological validity is especially acute in the field of experimental gaming. There are two interrelated reasons for this: firstly, the task situations with which subjects have been confronted in all but a handful of experimental games reported in the literature over the past 35 years have been of a highly abstract, unnatural and essentially meaningless nature, and secondly, no attempt had previously been made to examine whether the subjects in these experiments have been playing the games presented to them according to the rules embodied in the payoff structures devised by the investigators. Nemeth (1972), for example, has commented on the first aspect of the problem that the apparently irrational behaviour of subjects in gaming experiments "is due primarily to the essential incomprehensibility of the situation in which the subject is placed" (p. 213). On the second aspect of the problem, Apfelbaum (1974) has pointed out that "when used in social psychological studies . . . the matrix is a payoff device; it does not refer to utilities or, speaking more loosely, to the subjective values of the different outcomes . . . ." (p. 108). Some researchers, notably Anatol Rapoport (e.g. 1970b) have adjusted to this state of affairs by arguing that the question of ecological validity is irrelevant to the justification of experimental games. Others, however, have adopted the more tenable position that "it is preferable for researchers to try to generalize their findings [to naturally occurring situations]" (Pruitt & Kimmel, 1977, p. 368).

The ecological validity problem is, to a degree at least, an empirical one, and it is in principle experimentally tractable. Nevertheless, after an exhaustive review of expperimental gaming research up until the early 1970s, Wrightsman, O'Connor & Baker (1972) reached the following conclusion (quoted earlier): "What surprises us most in our review of research, is that apparently no studies have compared the degree of cooperative behavior in a laboratory mixed-motive game with cooperation in different real-world tasks. While artificiality can also be assessed through laboratory manipulations, comparisons of cooperative behavior across settings should be undertaken" (p. 277). The situation is not fundamentally different today. Only a tiny handful of experiments, including those of Orwant & Orwant (1970) and Sermat (1970), which were already in print when Wrightsman et al. were writing and are listed in their comprehensive bibliography but were evidently overlooked by these authors, have attempted to make such comparisons. These experiments have been critically reviewed in considerable detail in Sections 2.3 and 2.4 above, and for sundry reasons they have all been shown to have contributed virtually nothing to the solution of the ecological validity problem.

In Experiments I, II, III and IV, both aspects of the ecological validity problem referred to above were tackled directly. Firstly, in each of the experiments a comparison was made between the behaviour of subjects in an abstract matrix version of the game, similar to those used in the majority of previous experimental gaming studies, with their behaviour in a lifelike simulation of an everyday situation whose strategic structure was identical to the abstract game. Internal validity was ensured in each case by controlling other potential sources of systematic variation in choice behaviour other than the decision context in which the game was played. These lifelike simulations may be regarded as being intermediate between the highly abstract traditional matrix presentation on the one hand, and naturally occurring social situations similar to the ones modelled in the simulations on the other. The second aspect of the ecological validity problem -- the question of whether subjects adhere to the payoff structures of the games presented to them -- was investigated in each experiment by means of a novel procedure in which the subjects' subjective utilities were measured; an index of adherence to the payoff structure (a payoff-satisfaction correlation) was computed for each subject in each treatment condition. In each experiment, a subsidiary analysis was run, using a small subset of subjects who had adhered closely (in terms of subjective utilities) to the given payoff structure, in order to check whether the main findings were valid for subjects who were playing the game essentially as presented to them. A comparison and integration of the results of these four experiments will now be undertaken under the following headings: "Rationality", Cooperation and Caution; Adherence to the Game Rules; Time Course Effects; and Incidental Findings.

7.2. "Rationality", Cooperation and Caution

In Experiment I, in addition to comparing subjects' strategy choices in an abstract and a lifelike version of a 2 x 2 zero-sum saddle-point game, an attempt was made to investigate the effects of three programmed opponents' strategies on the subjects' frequency of minimax choices: a minimax opponent's strategy, a random opponent's strategy and a non-minimax opponent's strategy. It has been argued in Section 1.4 above that rationality is undefined against the last-mentioned two types of opponent's strategy, since a subject can improve his payoffs in these cases (especially in the latter) by deviating from the minimax rule. The results showed that the overall frequency of minimax choices on the part of the subjects was about 40 per cent, and, as predicted, the highest frequency was found in response to a minimax opponent's strategy and the lowest in response to a non-minimax opponent's strategy. Interaction effects revealed that these differences, especially the tendency for a minimax opponent's strategy to elicit relatively high levels of minimax choices from the subjects, tended to increase over trials. The abstract version of the game elicited more minimax choices from the subjects, and produced a pattern of results more clearly in line with predictions, than did the lifelike simulation. Since these predictions were premised on the assumption of common sense on the part of the subjects, the results seem to suggest firstly that the subjects were capable of more sensible behaviour than that prescribed by the minimax rule of formal game theory, and secondly that they are more capable of such common sense behaviour in an abstract than in a lifelike decision context. It may be concluded that the lifelike simulation in some way distracted the subjects from the essential strategic properties of the game, thereby interfering with their ability to make sensible (as opposed to game theoretically "rational") choices.

In the games used in Experiments II, III and IV, namely Prisoner's Dilemma, Chicken and N-person Prisoner's Dilemma respectively, a prescription of the "sensible" way to behave is more problematical. The strategy choices can, however, be designated cooperative or competitive in Experiments II and IV and cautious or risky in Experiment III. In the mixed-motive games used in these three experiments, programmed strategies were not used: the subjects interacted with one another in free-play situations. In addition to the abstract matrix versions of each game and the structurally equivalent lifelike simulations, however, two further (abstract) decision contexts were built into the experimental designs in each case: a positive incentive condition in which the subjects played for real monetary rewards, and a negative incentive condition in which they played to conserve as much money as possible (on the basis of the payoff structure of the game) from an allowance presented to them before the experiment began.

The results of these three experiments using mixed-motive games were in many important ways very similar to one another. In the Prisoner's Dilemma and N-person Prisoner's Dilemma experiments (II and IV) an almost identical overall frequency of cooperative choices (slightly less than 30 per cent) was found, and in the Chicken experiment (III) the overall proportion of cautious choices was about 40 per cent. These figures agree quite well with those of previous investigators in these areas. Of particular importance were the findings that in each case a significantly smaller frequency of cooperation or caution occurred in the lifelike simulation than in the corresponding abstract games. This tendency was even evident on the first trial of each experiment: in each case the frequency of initial cooperative or cautious choices was smaller in the lifelike simulation than in the corresponding abstract games. The conclusion seems justified that the behaviour of subjects in lifelike situations of the types used in these experiments tends to be significantly more competitive or risky than is their behaviour in structurally equivalent abstract games. This conclusion is strengthened by the fact that the effect was observed in three quite different games. It should be pointed out, however, that the lifelike simulations used in these experiments all modelled situations of economic conflict; it may be the case that such situations engage cultural values associated with competition and risk taking in a manner not to be assumed in other situations with similar or even identical strategic properties.

Of some interest were the negative findings of Experiments II, III and IV regarding the effects of incentives. In none of these three experiments were any significant differences found in the Analyses of Variance between the frequency of cooperative/cautious choices in the matrix, positive incentive and negative incentive treatment conditions. These findings echo those of numerous previous investigators who have reported non-significant incentive effects. More subtle analyses of time course effects than are possible by meaaa of Analysis of Variance did, however, subsequently uncover some extremely interesting differences between the behaviour of subjects in the three abstract incentive and non-incentive treatment conditions (see below). It seems reasonable to conclude, therefore, that the negative results of the Analyses of Variance with respect to incentive effects in Experiments II, III and IV, and perhaps also those of previous investigators, were attributable (in part at least) to the use of insufficiently sensitive analytical techniques.

7.3. Adherence to the Game Rules

The lowest correlations between the subjects' payoffs and their utilities (as measured by their ratings of satisfaction with the outcome of each joint decision) were found in Experiment I in which a zero-sum game was used: the mean correlation in this case was $r = .611$. The highest mean correlation was observed in the N-person Prisoner's Dilemma experiment (IV) -- $r = .760$. The mean correlations found in the Prisoner's Dilemma and Chicken experiments (II and III) were $r = .685$ and $r = .712$ respectively.

The exceptionally low correlations observed in the zero-sum experiment (I) were possibly due partly to the fact that the value of the game (unlike any of the others) was negative in all treatment conditions: it was a losing game from the subjects' point of view. In addition, monetary incentives were not used in this experiment but were given in some treatment conditions in all the other experiments. Both of these factors may have encouraged the subjects to search outside the given payoff structure in Experiment I for extraneous sources of utilities (positive and negative) to a greater extent than subjects in the other experiments. The exceptionally high correlation in the N-person Prisoner's Dilemma experiment (IV) is more difficult to account for, but it was probably not unconnected with the unusually high levels of emotional involvement of subjects in this game which emerged from a comparison of their post-experimental interview responses with those of subjects in Experiments I, II and III (see below).

In two of the experiments (II and III) the lowest mean correlations were found in the conventional matrix treatment condition. Only in the zero-sum experiment (I) was the mean correlation in the lifelike simulation condition significantly lower than that in the abstract matrix condition, although this difference was very small ($r = .59$ versus $r = .62$) and was reversed in the treatment conditions in which subjects were confronted with a non-minimax opponent's strategy. Two related conclusions flow from these comparative findings: firstly there seem to be few grounds for believing that subjects are in general more likely to introduce extraneous utilities into experimental games when lifelike simulations rather than traditional abstract matrix versions are used; and secondly, the viability of a methodology for the use of such lifelike task situations in gaming experiments seems to have been convincingly established.

In each of the four experiments, a subsidiary Analysis of Variance, using a small subset of subjects with extremely high payoff-satisfaction correlations, revealed a pattern of results remarkably similar to the pattern found in the main analyses which included all the subjects. In particular, the smaller frequency of "rational", cooperative and cautious choices in the lifelike simulation compared with the abstract games was convincingly replicated in each case. It can be concluded from this that the findings mentioned above are robust in the sense of not being mere artifacts caused by the failure of subjects to play the games presented to them in terms of the "official" payoff structures, although many subjects did break the rules in this way. This aspect of the ecological validity problem may thus be less serious than it seems: the duplication of the robustness finding in all four experiments suggests that the findings of previous investigators are quite likely to be similarly robust in this sense. This in no way diminishes the seriousness of the problem arising from the abstract nature of previous gaming experiments, however.

7.4. Time Course Effects

The only time course effects to emerge as main effects from the Analyses of Variance used in Experiments I, II, III and IV were (a) a decline in minimax choices in Experiment I from Trial Block 1 to Trial Block 3, and (b) a decline in cooperative choices in Experiment IV from Trial Block 1 to Trial Block 2, and from Trial Block 2 to Trial Block 3. It was argued, however, that Analysis of Variance is an exceedingly blunt instrument for the detection of such time-bound effects, and that the appropriate techniques for investigating time course effects in gaming experiments are those of Time Series Analysis, never before applied to the results of experimental games.

The Time Series Analyses used in Experiments I, II, III and IV revealed numerous subtle and interesting effects which were not evident in the raw data and failed to emerge from the conventional statistical analyses. In Experiment I it was shown that the characteristic secular movement in minimax choices was steadily upwards over trials in response to a minimax opponent's strategy, steadily downwards in response to a non-minimax opponent's strategy, and distinctly cyclical in response to a random opponent's strategy. These findings are in line with the assumption of common sense on the part of the subjects: against a consistent minimax or non-minimax opponent's strategy they gradually adapted their choice behaviour to get the most out of the situation, and faced with a random opponent's strategy they fluctuated between one pure strategy and the other, presumably attempting (alas! in vain) to detect a pattern in their opponents' strategy choices.

In the mixed-motive gaming experiments (II, III and IV), the psychological strangeness of the negative incentive treatment condition became evident through Time Series Analysis. In Experiment II, only subjects in the negative incentive condition displayed a characteristic increase in cooperation over trials; in all other treatment conditions a steady decline in cooperation was observed. It should be noted that an increase in cooperation during the first 30 trials of a Prisoner's Dilemma experiment is an exceedingly rare (and possibly unique) finding. It certainly merits further investigation; but the techniques of Time Series Analysis may be necessary as in Experiment II to detect this peculiar negative incentive effect in the Prisoner's Dilemma Game.

In Experiments III, and IV, the only treatment condition which elicited cyclical activity in the subjects' strategy choices was once again the negative incentive condition; there was a more or less linear (and certainly monotonic) decline in cooperation or caution in all other treatment conditions in both experiments. Detailed interpretations of these effects have been offered above and there is no point in repeating them here, but the following general conclusion seems justified: when the structure of a game is of such a nature as to induce a conservative rather than an acquisitive incentive motivation in the subjects, as it often is in everyday non-laboratory strategic interactions, their behaviour may be quite different from that in other strategically similar situations. Once again, the duplication of this general finding in three quite different games is striking, although the behaviour of subjects in the negative incentive conditions was not peculiar in exactly the same way in all three situations. Further research, using Time Series Analysis, ought to be devoted to this problem in order to illuminate it further: the findings reported above are merely suggestive.

The analysis using autocorrelation functions revealed a short-term consistency in the choice behaviour of subjects in most treatment conditions in Experiments II, III and IV. This consistency was strongest in Experiment IV, particularly in subjects in the positive incentive and lifelike simulation treatment conditions. This may be interpreted as reflecting the relatively high level of emotional involvement of subjects in the N-person

Prisoner's Dilemma experiment, particularly in the two treatment conditions mentioned. The post-experimental interview responses of subjects in these treatment conditions compared with the others, and compared with the responses of subjects to the post-experimental interviews in the other experiments, supports this interpretation. A high level of emotional involvement may be expected to become evident in firmly-held views about what strategy is appropriate in the short term, or to put it another way, a lack of involvement would tend to lead to inconsistent and unpredictable behaviour on the part of many subjects. In the light of this conjecture it is particularly noteworthy that relatively weak autocorrelations were observed in the abstract matrix treatment condition in all three experiments in which they were computed (II, III and IV). A tentative conclusion from this is that the traditional matrix presentation of experimental games may be less ego involving for the subjects than other presentations. It certainly seems to generate less consistent choice behaviour, in any event, as the experiments above have shown in three quite different types of game.

7.5. Incidental Findings

The interpretation of the subjects' behaviour in Experiments I, II, III and IV rests upon two assumptions: (a) that they understood what was required of them by the rules of the game, and (b) that they rook the games seriously. With regard to (a), the experimenter went to unusual lengths to ensure that all subjects understood the games: elaborate instructions were given, subjects were not permitted to begin playing until they could demonstrate a full understanding of the payoff structure, their score sheets were scrutinized for errors which betrayed a lack of understanding and so on. Regarding (b) above, the results of the post-experimental interviews were most illuminating.

The replies given by the subjects in the post-experimental interviews in all four experiments revealed that in the overwhelming majority of cases they took the tasks seriously, made the best choices they could, and in general enjoyed playing the games. These generalizations should be qualified by saying that feelings occasionally tended to run high (particularly in Experiment IV in the N-person Prisoner's Dilemma) and that strong negative reactions to gaming partners were frequently evoked in the Chicken and N-person Prisoner's Dilemma experiments (III and IV). The replies in general made good sense, and statistical analyses where applied usually confirmed the investigator's common sense hunches about how the subjects would respond.

In Experiment I, for example, each subject played against a programmed strategy from a stooge opponent; in the post-experimental interviews they expressed suspicions about the experiment being "rigged" significantly more frequently when pitted against an "irrational" (random or non-minimax) opponent's strategy than when confronted with a "rational" (minimax) opponent's strategy. They considered the game "unfair" (as has been mentioned, its value was negative) significantly more frequently in the abstract than in the lifelike treatment condition; presumably subjects expect a meaningless laboratory game to at least be "fair" although they accept that everyday life situations are not. They regarded their opponent as "silly" significantly more often when he played "irrationally" than when he played "tationally", and they considered him "clever" significantly more often when he played "rationally". All these findings are consistent with the belief that the subjects understood what the game was about and treated it seriously.

In Experiment II, subjects in the lifelike simulation described their "general strategy" as one of cooperation significantly less frequently than did subjects in the abstract versions of the game; this difference was, of course, reflected in differences in the subjects' actual strategy choices. Possibly as a result of the lower level of cooperation in the lifelike game, subjects also considered their partners "sensible" significantly less frequently in this treatment condition than in the abstract treatment conditions. The responses to the post-experimental interviews in the other experiments were also generally in line with expectations based on the assumptions that the subjects understood the tasks presented to them and took the games seriously. This conclusion increases confidence in the probable ecological validity of the findings derived from the lifelike versions of the games, particularly when it is borne in mind that ego involvement was often apparently highest in the lifelike games.

Considering the findings of Experiments I, II, III and IV together, the following major conclusions do not seem to be too radical: (a) behaviour does differ in structurally equivalent abstract and lifelike strategic interactions, and the ecological validity of experiments using only matrix games is therefore necessarily limited; (b) if the purpose of experimental games is to increase our understanding of the behaviour of men, women and children in everyday strategic interactions, there does not appear to be any obvious reason for the continued use of abstract, unnatural and essentially meaningless games; and (c) Tedeschi, Schlenker & Bonoma's (1973) bold assertion that "criteria for the assessment of ecological validity are non-existent for experimental games" (p. 202) seems to have been cast into serious doubt by the establishment of a workable method for comparing strategic choices in structurally equivalent abstract and lifelike games.

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APPENDICES

Subject code:
Sex:
Age:

APPENDIX A - Score Sheet
PI - NI - M - S

Partner codes:
Sex:
Age:

	Own choice	Others' choice(s)	Own payoff	Others' payoff(s)	Rating	

APPENDIX B

Decision Card (Matrix)

APPENDIX C

Decision Card (Simulation, Experiment I)

AGREE

REFUSE

APPENDIX D

Rating Scale

VD	D	N	P	VP
Very displeased	Displeased	Neutral	Pleased	Very Pleased

N

APPENDIX E

Experiment I: Minimax Choices

		Trial Block 1	Trial Block 2	Trial Block 3
Minimax	Matrix	5,5,3,3,7,8,10, 5,6,6,7,4,6,7, ($\overline{X}$ = 5.86)	10,5,5,9,6,10,10, 4,9,10,10,10,10,10, ($\overline{X}$ = 8.43)	10,5,8,10,10,10,3, 5,10,10,10,10,10,10, ($\overline{X}$ = 8.64)
	Simulation	10,2,2,2,3,9,1, 8,1,2,1,5,6,1, ($\overline{X}$ = 3.79)	10,0,3,1,4,10,7, 6,4,5,2,8,8,0, ($\overline{X}$ = 4.86)	10,0,1,0,1,10,6, 0,0,0,1,8,9,0, ($\overline{X}$ = 3.29)
Random	Matrix	6,4,7,4,4,7,5, 7,3,6,8,3,6,7, ($\overline{X}$ = 5.50)	5,2,3,4,7,6,5, 3,6,4,6,4,3,7, ($\overline{X}$ = 4.64)	5,2,3,5,4,8,4, 0,4,5,8,5,1,4, ($\overline{X}$ = 4.14)
	Simulation	6,4,4,4,2,2,2, 3,3,2,1,4,5,3, ($\overline{X}$ = 3.21)	6,6,4,5,5,3,1, 3,4,1,1,1,4,3, ($\overline{X}$ = 3.36)	2,3,6,4,2,5,0, 6,4,1,4,0,1,4, ($\overline{X}$ = 4.29)
Non-minimax	Matrix	3,3,8,6,5,8,3, 5,4,2,5,7,4,3, ($\overline{X}$ = 4.71)	2,0,0,4,2,9,2, 3,0,0,0,1,0,1, ($\overline{X}$ = 1.71)	0,0,1,0,1,0,0, 1,0,0,0,2,0,0, ($\overline{X}$ = 0.29)
	Simulation	0,4,1,2,4,2,0, 3,2,3,4,6,5,7, ($\overline{X}$ = 3.07)	0,0,0,0,3,0,0, 2,1,1,3,1,0,3, ($\overline{X}$ = 1.00)	0,0,0,0,2,0,0, 0,0,0,4,0,3,2. ($\overline{X}$ = 0.79)

APPENDIX F

Experiment I: Payoff-satisfaction Correlations

	Opponent's Strategy		
	Minimax	Random	Non-minimax
	.08	.33	.26
	.19	.79	1.00
	.03	.91	.45
	.27	.99	1.00
	.31	.64	.15
	.25	.73	1.00
Matrix	1.00	.89	.79
	.48	.51	.28
	.70	.85	1.00
	.14	.93	.95
	.56	.34	.05
	.77	.84	.47
	.78	.88	.60
	.09	.79	.80
	$\overline{X}$ = .40	$\overline{X}$ = .84	$\overline{X}$ = .63
	.36	.89	.30
	.41	.90	.72
	.09	.86	1.00
	-.33	.84	1.00
	.11	.94	.46
Simulation	.05	.82	.668
	.24	.73	1.00
	.55	.75	.76
	.00	.71	.42
	.37	.64	1.00
	.04	.80	1.00
	.38	.88	.49
	.34	.91	.65
	1.00	.94	-.03
	$\overline{X}$ = .26	$\overline{X}$ = .83	$\overline{X}$ = .68

APPENDIX G

Experiment II: Cooperative Choices Per Pair

Decision Context	Trial Block 1	Trial Block 2	Trial Block 3
PI	7*,9*,3,6*,3, 7,8,2,11,9. $\overline{X}$ = 6.50.	7*,10*,8,5*,1, 6,5,0,19,1. $\overline{X}$ = 6.20.	5*,10*,20,13*,1, 2,5,0,9,2. $\overline{X}$ = 6.70.
NI	18,7,11,7*,7*, 10*,9,6,10,3. $\overline{X}$ = 8.80.	14,7,7,4*,6*, 6*,6,3,7,1. $\overline{X}$ = 6.10.	14,5,8,7*,3*, 4*,9,3,7,10. $\overline{X}$ = 7.00.
M	11,10,10,0,4*, 4,8*,4*,10,3. $\overline{X}$ = 6.40.	7,4,11,0,4*, 7.4*,8*,7,5. $\overline{X}$ = 5.70.	5,6,13,0,3*, 7,3*,10*,11,4. $\overline{X}$ = 6.20.
S	3*,6,2,6,7*, 5,5*,2,1,4. $\overline{X}$ = 4.10.	5*,4,1,3,8*, 4,3*,6,0,0. $\overline{X}$ = 3.40.	4*,4,0,2,4*, 1,5*,11,0,0. $\overline{X}$ = 3.10.

* Subject pairs with high payoff-satisfaction correlations used in the subsidiary analysis.

APPENDIX H

Experiment II: Payoff-satisfaction Correlations

PI	NI	M	S
.816	.863	.672	.962
.876	.659	.567	.823
.826	.894	.775	.840
.668	.527	-.108	.491
.536	.648	.435	.415
.643	.517	.469	.831
.765	.843	-.174	.777
.932	.860	1.000	.783
.728	.925	.765	.841
.748	.777	.976	.920
.393	.893	.797	.676
.552	.855	.865	.926
.372	.852	.953	.918
.279	.785	.875	.943
.175	.742	.918	.869
.580	.890	.844	.226
.862	.836	.786	1.000
.438	.638	.099	1.000
.868	-.210	.452	.956
.474	.917	.080	.267
$\overline{X}$ = .627	$\overline{X}$ = .735	$\overline{X}$ = .602	$\overline{X}$ = .773

APPENDIX I

Experiment III: Cautious Choices Per Pair

Decision Context	Trial Block 1	Trial Block 2	Trial Block 3
PI	15,10,8*,20*,14, 10,3,8,5*,11. $\overline{X}$ = 10.40.	9,15,9*,19*,13, 10,8,8,13*,7. $\overline{X}$ = 11.10.	14,9,7*,8*,13, 5,7,13,13*,5. $\overline{X}$ = 9.40.
NI	6,6,8*,13,11*, 8*,8,6,12,8.67. $\overline{X}$ = 8.67.	7,9,7*,14,3*, 8*,9,7,13,8.56. $\overline{X}$ = 8.56.	4,3,5*,17,8*, 9*,10,4,17,8.56. $\overline{X}$ = 8.56.
M	11*,11*,8,11,6, 3,7,9*,7,6. $\overline{X}$ = 7.90.	6*,8*,5,9,9, 13,2,10*,3,6. $\overline{X}$ = 7.10.	6*,8*,7,5,6, 11,4,11*,1,8. $\overline{X}$ = 6.70.
S	6,8,4*,10,6, 10*,4,11*,4,6. $\overline{X}$ = 6.90.	4,7,6*,12,6, 6*,6,7*,2,5. $\overline{X}$ = 6.10.	5,7,4*,9,7, 6*,2,10*,2,6. $\overline{X}$ = 5.80.

* Subject pairs with high payoff-satisfaction correlations used in the subsidiary analysis.

APPENDIX J

Experiment III: Payoff-satisfaction Correlations

PI	NI	M	S
.878	.756	.980	.524
.786	.609	.690	.498
.689	.922	.645	.681
.778	.392	.916	.911
.865	.832	.875	.750
.890	.956	.525	.958
.819	.403	.980	.682
.907	.753	.351	.725
.907	.753	.351	.725
.767	.932	.453	.606
.949	.820	.747	.823
.668	.861	.672	.874
.399	.671	.752	.867
.937	.625	.628	.059
.431	.797	.439	.695
.777	.637	.787	.816
.781	.764	.651	.989
.913	.273	.593	.382
.753	.512	.243	.917
.914	-	.751	.896
.681	-	.404	.773
$\overline{X}$ = .779	$\overline{X}$ = .695	$\overline{X}$ = .654	$\overline{X}$ = .721

APPENDIX K

Experiment IV: Cooperative Choices Per Group

Decision Context	Trial Block 1	Trial Block 2	Trial Block 3
<u>PI</u>	9,14,10,8,8, 13,13,9,6,15. $\overline{X}$ = 10.50.	5,10,7,1,6, 7,8,6,3,7. $\overline{X}$ = 6.00.	1,7,2,0,5, 12,7,4,0,6. $\overline{X}$ = 4.40.
<u>NI</u>	7,19,10,5,13, 16,12,14,13,13. $\overline{X}$ = 12.20.	6,17,13,11,10, 20,3,10,8,10. $\overline{X}$ = 10.80.	1,11,5,11,5, 17,3,12,3,6. $\overline{X}$ = 7.50.
<u>M</u>	11,12,20,14,14, 13,10,9,14,12. $\overline{X}$ = 12.90.	8,10,18,9,6, 6,10,8,16,8. $\overline{X}$ = 9.90.	4,9,13,3,11, 7,7,4,11,5. $\overline{X}$ = 7.40.
<u>S</u>	7,8,7,3,5, 14,5,7,12,11. $\overline{X}$ = 7.90.	5,5,3,0,0, 9,6,2,7,8. $\overline{X}$ = 4.50.	6,6,1,0,0, 10,7,1,4,5. $\overline{X}$ = 4.40.

APPENDIX L

Experiment IV: Payoff-satisfaction Correlations

	PI			NI			M			S	
.48	.88	.46	.19	.79	.61	.91*	.71	.86	.97*	.85	.90
.84	.65	.91*	.84	.97*	.70	.71	.95*	.80	.89*	.88	.77
.40	.97*	.86	.73	.61	.85	.80	.76	.71	.79	.82	.83
.79	.74	1.00*	.88*	.15	.63	.78	.87	.87	1.00*	.59	.47
.67	.67	.88	.81	.86*	.61	.97*	.46	.76	.89	.73	.83
.76	.87	.93	.77	.74	.85	.28	.91	.89*	.20	.89	.94*
.72	.86	.53	.71	.85	.79	.64	.80	.88	.74	.86	.94
.90*	.74	.82	.69	.84	.97*	.85	.62	.81	.49	.88	.96*
.67	.88	1.00*	.82	.88*	.84	.83	.50	.91*	.75	.79	.29
.79	.89	.88	.62	.51	.78	.72	.88	.82	.40	.88	.52
$\overline{X}$ = .78			$\overline{X}$ = .73			$\overline{X}$ = .77			$\overline{X}$ = .76		

* **Subjects with high payoff-satisfaction correlations used in the subsidiary analysis.**