Journal of Experimental Psychology:
Learning, Memory, and Cognition
2006, Vol. 32, No. 1, 131–149

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0278-7393/06/$12.00 DOI: 10.1037/0278-7393.32.1.131

Game Relativity: How Context Influences Strategic Decision Making

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Existing models of strategic decision making typically assume that only the attributes of the currently played game need be considered when reaching a decision. The results presented in this article demonstrate that the so-called "cooperativeness" of the previously played prisoner's dilemma games influence choices and predictions in the current prisoner's dilemma game, which suggests that games are not considered independently. These effects involved reinforcement-based assimilation to the previous choices and also a perceptual contrast of the present game with preceding games, depending on the range and the rank of their cooperativeness. A. Parducci's (1965) range frequency theory and H. Helson's (1964) adaptation level theory are plausible theories of relative judgment of magnitude information, which could provide an account of these context effects.

Keywords: judgment, cooperation, prisoner's dilemma, context effects

Much human behavior results from intentional decision, and decisions typically involve some judgment of the potential rewards and risk associated with each action. Choosing a career, for example, involves trading off different balances between the risks and returns of different jobs. In many complex decisions, the risk is associated with the unpredictability of the decisions of other people. For example, choosing a life partner, or whether to have children, is contingent on the cooperative decisions of another person. Understanding how people predict each others' behavior and make choices on the basis of these predictions and the available opportunities and rewards, therefore, is a central question for psychology. Moreover, how people trade off risk and return when interacting with other people is a central issue for economics, because the foundations of economic theory are rooted in models of individual and interactive (strategic) decision making. For example, to explain the behavior of markets we need a model of the decision-making behavior of buyers and sellers in those markets; to understand strategic behavior between firms, or between firms and government, requires understanding how people trade risk and reward when their decisions interact.

Most of economics and other social sciences have taken the normative theories of expected utility theory and game theory, first developed by von Neumann and Morgenstern (1947), as a starting point (see Shafir & LeBoeuf, 2002, for a review). Instead of being based on empirical data on human behavior, normative approaches aim to specify how people *should* be making risky or strategic decisions. The *rational choice* approach to social science assumes, furthermore, that these normative theories also describe how people do indeed make their decisions. Thus, expected utility theory and game theory can be viewed as both normative theories of decision making and as a descriptive psychological account.

In particular, expected utility theory is based on the assumption that people make choices that maximize their utility, and their utility for a risky decision strategy is measured by the expected utility that this option or strategy will provide. This expected utility is a function of the utilities of the possible outcomes, weighted by their probabilities. When decisions are interactive, the utility of different actions cannot be calculated straightforwardly, as decision making is recursive. That is, each player makes decisions in the context of assumptions about the decisions of the other player, but the other player may equally choose on the basis of assumptions about the decisions of the first player. Game theory aims to deal with this recursiveness by introducing the concept of a *Nash equilibrium* (Nash, 1950, 1951): A pair of decisions are in Nash equilibrium if neither player would obtain a higher expected utility by making a different decision, given that the other player's decision is viewed as fixed. Game theory and its extensions have sought to refine this notion in a number of ways (e.g., Fudenberg & Tirole, 1991; Harsanyi & Selten, 1988).

Since the development of these normative theories, psychologists and economists have been testing their descriptive accuracy and finding anomalies between the normative predictions and people's actual behavior (e.g., Kagel & Roth, 1995; Kahneman, Slovic, & Tversky, 1982; Kahneman & Tversky, 2000). In parallel, other economists have considered how robust economic theory is to such anomalies (e.g., Akerlof & Yellen, 1985; de Canio, 1979; Friedman, 1953; Nelson & Winter, 1982; Simon, 1959, 1992).

Most applications of these normative models make one fundamental assumption, which we are addressing in this article: that the choice of a prospect or a game strategy should be based only on the attributes of the current prospect or game and hence is considered independently from previous prospects or games. We call this the

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This work was conducted while Ivo Vlaev was a doctoral student at the Department of Experimental Psychology, University of Oxford. The authors were also partially supported by Economic and Social Research Council Grant R000239351. Nick Chater was partially supported by European Commission Grant RTN-HPRN-CT-1999-00065, the Leverhulme Trust, and the Human Frontiers Science Program. We thank Neil Stewart for his comments and Daniel Zizzo for providing the experimental economics laboratory at the Department of Economics, University of Oxford.

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"*sequential independence assumption.*" The present research demonstrates a new psychological phenomenon, which suggests that people do not possess a well-defined notion of the utility of a strategy in an interactive game or the "cooperativeness" of a game in particular (a notion we introduce below). Instead, we find that people make choices depending on the other games being played, which violates the sequential independence assumption. We call this phenomenon *game relativit*y. Considerable further work is required, of course, to establish the generality and scope of these results across the array of decision-making domains of psychological and economic interest. But we believe that this anomaly is sufficiently theoretically significant to motivate further exploration.

The research background on which the current project is based consists of psychological work on decision-making behavior, as well as existing research concerned with fundamental cognitive processes related to the perception and representation of sensory magnitudes. This is also part of a more general program for grounding decision-making research more directly in principles from the study of underlying cognitive mechanisms (e.g., Stewart, Chater, Stott, & Reimers, 2003).

Psychophysical Effects

When making strategic decisions in games, people must assess the magnitudes of risk and return that are associated with each strategy. For example, when deciding whether it is worth cooperating in a given situation, a person must assess the likelihood that the other player will decide to cooperate or to exploit him or her and also weigh the potential gains and losses, respectively, in these two possible outcomes.

The starting point for this research is the idea that the perception of these magnitudes, and in particular, the perception of the likelihood of cooperation expected in a particular game, will be influenced by some of the same factors that influence the perception of psychophysical magnitudes, such as brightness, loudness, or weight. Thus, cognitive principles, well studied in perception, may also substantially influence decision making (e.g., Stewart et al., 2003).

In the psychophysical literature, there is substantial evidence that people are poor at providing stable absolute judgments of sensory magnitudes and are heavily influenced by the other options presented to them in the recent past or available at the time of choice (e.g., Laming, 1997). A classic illustration is Garner's (1954) experiment in which participants judged which of a range of tones was closest to half as loud as a 90-dB reference sound. The striking result was that participants' judgments were entirely determined by the range of tones played to them and not by the absolute intensity of these tones. Participants who were played tones in the 55–65-dB range had a half-loudness point of about 60 dB. A second group received tones in the 65–75-dB range and had a half-loudness point of about 70 dB. A third group, who heard tones in the 75–85-dB range, had a half-loudness point of about 80 dB. Laming (1997) extensively discussed these and other similar findings over decades of psychophysical research, the results of which are consistent with the idea that participants are unable to make reliable decontextualized judgments of absolute sensory magnitudes. He claimed that only relative judgments can be made. That is, whenever isolated stimuli are presented, and a judgment about the magnitude of the resulting sensation must be made, there is always an implicit comparison baseline. Such a comparison might be the stimulus presented on a previous trial, or it may be some amalgam of remembered experience. Thus, Laming challenged the assumption that people have access to a stable internal psychophysical scale. Lockhead (1992, 1995; see also Holland & Lockhead, 1968; Ward & Lockhead, 1970, 1971) also summarized the growing evidence from absolute identification and magnitude estimation paradigms indicating that people typically have poor access to absolute magnitude information and instead rely on comparisons with recent or concurrent stimuli, as evident from the strong effect of preceding material demonstrated in these paradigms.

Recently, Stewart, Brown, and Chater (2002) argued that similar effects can also occur in the categorization of psychophysically simple stimuli. If no absolute magnitude information is available, categorization responses must be based on perceived difference between the current trial and previous trials. They developed a new model of unidimensional categorization, in which classification is based on the relative magnitude information from comparisons with immediately preceding stimuli. As a consequence, judgments are possible only to the extent that comparison with the preceding stimulus is accurate, whereas inaccuracies in comparisons judgment or certain biases can explain trial-to-trial context effects. Specifically, the responses on the current trial were shown to assimilate to the preceding trial and to contrast with other earlier trials. Such assimilation and contrast effects are found in many other psychological domains (e.g., Jesteadt, Luce, & Green, 1977; Petzold, 1981) and also arise in the experiments we report below. Stewart, Brown, and Chater (in press) presented a model of a wide range of absolute magnitude experiments, including trial-to-trial context effects, that not only assumes that people have no absolute access to psychophysical magnitudes, but that they can only make ordinal judgments concerning the size of "jumps" between pairs of magnitudes.

In summary, context effects, such as those found by Garner (1954), are consistent with people making perceptual judgments on the basis of relative magnitude information rather than absolute magnitude information (Laming, 1984, 1997; Stewart et al., 2002). Applying these ideas to decision making, Stewart et al. (2003) tested whether the attributes of risky prospects behave like those of perceptual stimuli, and they found similar context effects. Their experiments demonstrated a large effect of the available options set, suggesting instead that prospects of the form "p chance of x" are valued relative to one another. In particular, the judged certainty equivalent (the amount of money for certain that a person judges to be worth the same as a single chance to play the risky prospect) was strongly influenced by the other options available, that is, the set of other available certainty equivalents from which to select the preferred amount. Similarly, the choice of a preferred option from a set of prospects was strongly influenced by the prospects available. In a small number of other experiments, researchers also have investigated the effect of the set of available options in a decision under risk. For example, Birnbaum (1992) demonstrated that the selection of a certainty equivalent was influenced when skewing the distribution of options offered as certainty equivalents for simple prospects, while holding the maximum and minimum constant. When the options were positively skewed, which means that most values in the sequence were small, prospects were undervalued compared with when the options were negatively skewed and hence, most values were relatively large.

Range Frequency Theory

The findings by Stewart et al. (2003) and Birnbaum (1992) can be interpreted in terms of range frequency theory (Parducci, 1965, 1974). Parducci found that the neutral point of the judgment scale corresponded to a compromise between the midpoint, defined by the range of the distribution, and the median depending on the skew of the distribution. Thus, the *range principle* reflects a tendency to judge an event relative to the proportion of the range of stimuli lying below that event on the specified dimension of judgment (the range is defined by the minimum and maximum values of the stimuli in the set), whereas the *frequency principle* reflects a tendency to judge an event relative to the proportion of contextual stimuli lying below that event on the specified dimension of judgment (which is a measure of the rank of an event among the other events). In summary, the subjective value given to a stimulus (or its attribute) that varies along a single dimension is a function of its position within the overall range of stimuli (attributes) and its rank.

Note that this model implies that attributes are judged purely in relation to one another and that their subjective value is independent of their absolute value. Thus, if all stimuli are modified by some factor, the range of each stimulus will be unchanged because range is determined by comparison with the end items, which will also be rescaled; and the rank of each stimulus will, of course, be unchanged. But the shape of the distribution of stimuli does matter. For example, range frequency theory also predicts Birnbaum's (1992) results described above, because the subjective value of a given certainty equivalent will be larger when the other options offered as certainty equivalents are positively skewed, as this option will have a higher rank due to the presence of many smaller options. Therefore, the risky prospects will be given lower certainty equivalents on average and as a result will appear undervalued.

Adaptation Level Theory

An older view of magnitude judgment, against which range frequency theory is a reaction, is *adaptation level theory* (Helson, 1964), which provides the second main model of context effects in perceptual identification conforming to a relativistic view of magnitude perception. In adaptation level theory, the judgment of a particular event is proportional to its deviation from the mean value of other events, which is considered to be the adaptation level and is assigned a neutral value. For example, if one puts one's hands in cool water, as one adapts, the experience of the temperature of the water gradually drifts toward "neither hot nor cold," and the experience of other temperatures changes accordingly. A temperature that would be called warm in one context may feel cool in another. In other words, this theory implies that people tend to contrast judged stimuli with the mean value of the distribution, which is also a relativistic model of perceptual judgment. Brickman and Campbell (1971) based their conception of the hedonic treadmill on Helson's (1964) notion of adaptation level and proposed that a similar process of adaptation applies to the hedonic value of life circumstances. Adaptation level theory has also been used in many applied studies on the effects of reference points in consumer behavior (see Monroe, 1990, for a review). Note that prospect theory (Kahneman & Tversky, 1979) also claims that stimuli are judged according to their deviation from a reference point, which is interpreted to be the adaptation level or the current status quo. Thus, Kahneman and Varey (1991) discussed adaptation level in relation to the utility concept and the possibility of using this neutral value to match sensory or hedonic experiences across individuals.

It is crucial to note that the adaptation level is context sensitive, as it is conceived as the mean of the stimuli presented within a contextual set (Helson, 1964; Wedell, 1995). Reliance on the mean of judgments of prior experiences implies that the shape of the distribution of prior events is irrelevant. We evaluate this assumption below.

Prior research has not investigated whether the relativity of perceptual judgments, as described by range frequency and adaptation level theory, also arises in game-theoretic contexts; although, as we have noted, relativistic judgment theories have been applied in decisions under certainty (e.g., Monroe, 1990) and decisions under risk (e.g., Birnbaum, 1992; Hsee, Loewenstein, Blount, & Bazerman, 1999; Mellers & Cooke, 1994; Stewart et al., 2003). In the present paper we seek to fill this gap.

Cooperation Index Scale

Most of the studies of perceptual context effects discussed above deal either with simple one-dimensional perceptual stimuli or with choices between sets of options that vary along two dimensions, such as risk and return. We conjectured that the same effects may also hold for more complex stimuli, such as games. In a typical (noncooperative) game, two players each have a set of possible actions, which they must choose simultaneously and without knowledge of the other player's action. Each pair of actions is associated with a (possibly different) payoff for each player.

We used the prisoner's dilemma (PD) game. PD has been the subject of a large body of literature, partly because of the wide variety of real-world settings that can be viewed as having this structure, ranging from social interactions to international issues such as trade negotiations, arms races, and pollution control (see Rapoport & Chammah, 1965, & Colman, 1995, for a review); PD also plays a central role in theories of animal behavior (e.g., Maynard-Smith & Price, 1973).

In a PD (see Figure 1), each player must move either 1 (for cooperate) or 2 (for defect). In our experiments, as with most studies of PD, participants were given neutral labels for the options. The structure of PD is such that, whatever the other player does, each player will obtain a higher reward by playing 2. But if both players play 2, the outcome for both players is worse than if they both play 1. Thus, the dilemma is that, reasoning purely

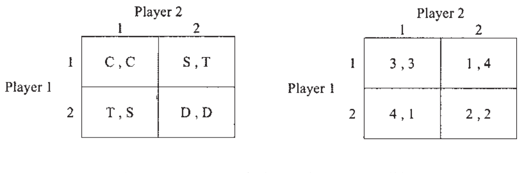

Figure 1. The structure of the prisoner's dilemma game.

individualistically, each player has a self-interest in playing 2, irrespective of what the other does. Yet if both players do this, the outcome for both of them is bad. If they could instead “cooperate” and both play 1, the outcome would be better for both of them—but then if one player cooperates, the other player will do even better by “defecting” and playing 2. Hence, from the point of view of rational self-interest, cooperation is not a viable solution to the game. The only rationally stable outcome (the only Nash equilibrium) for the game is that both players choose 2, with the poor 2,2 outcome.

Nonetheless, people frequently cooperate in anonymous “one-shot” PD (see, e.g., Dawes & Thaler, 1988) where, according to rational choice theory, they should not. There are various accounts of this behavior and the conditions under which Nash equilibrium does become established, some including misunderstanding, role of repetition of the play and the resulting reputation and retaliation affects, irrationality, motivation (incentives, altruism), communication, and so on (see Sally, 1995, for a recent review). Moreover, there is, of course, a large body of literature on cooperation in more general settings, in which rational choice may allow the possibility that self-interested players may, nonetheless, cooperate. For example, when PD interactions are repeated between pairs of players, then rational choice theories provide few constraints on possible outcomes (this follows from the so-called “folk theorem” concerning repeated games; see Fudenberg & Tirole, 1991, pp. 150–160). Equally, the rational choice theorist may consider cases in which factors related to reputation influence choice (e.g., in which people’s behavior is one-shot, but their behavior may be reported to others, with whom they may interact; Andreoni & Miller, 1993; Falk, Fehr, & Fischbacher, 2003). It is possible that in any experiment in which people play one-shot anonymous games, there is some behavioral “spillover” from these other contexts. This point applies to almost all research on one-shot games, but it is unlikely to be problematic for our research, in which the specific context effects that we observed seemed to be orthogonal to the presence of such further factors. In summary, regardless of the normative prescriptions, in practice people playing PD do, nonetheless, quite frequently choose the cooperative outcome 1, and quite often both players do this, each therefore obtaining the higher payoff resulting from 1,1.

In abstract terms, the PD game is defined by the inequality $T < C < D < S$ (see Figure 1), where C is the payoff if both “cooperate” and play 1, D is the payoff if both “defect” and play 2, T is the payoff if one defects and plays 2 and the other cooperates and plays 1 (and it’s called the “temptation” payoff), and S is the payoff if one cooperates by playing 1 and the other plays 2 and therefore defects (and this payoff is also called the “sucker” payoff).

Now, depending on the specific values of T, C, D, and S, the degree to which people cooperate in a PD task varies. Intuitively, one knows that the temptation to defect depends on how much is gained from doing so, yet the attraction of cooperating depends on the benefits of mutual cooperation in relation to mutual defection. Specifically, previous empirical studies have suggested that behavior in the game may depend, at least in part, on a one-dimensional quantity, the “cooperativeness index,” which provides an ideal test case of our conjecture that quite abstract magnitudes involved in decision making may show some of the same psychological properties as psychophysical magnitudes (Rapoport & Chammah, 1965). By using such a cooperativeness scale and treating cooperativeness as a cognitive dimension, researchers can borrow concepts from psychophysics to investigate whether the previous context influences decisions in the PD game. The relevant context is the cooperativeness of previous PD games, in a sequence. In the various experimental conditions reported in this article, the manipulated contextual variable was the statistical distribution of the cooperativeness of the games in the sequence (e.g., the mean, range, and skew of this distribution). The dependent variables were the average cooperation in each group and the expected (predicted) cooperation of the other players. The null hypothesis was that people consider each game in isolation from the other games in the sequence; that is, the attributes of the previous games do not affect the behavior in the current game.

As we have indicated, the scale or the one-dimensional continuum along which the games can be categorized analogously to perceptual stimuli is the degree of cooperativeness of each PD game. We used a concrete measure of cooperativeness, the *cooperation index* (hereafter referred to as CI) developed by Rapoport and Chammah (1965), which is defined by the ratio

$$CI = \frac{C - D}{T - S}.$$

Rapoport and Chammah (1965) also considered a variety of closely related cooperativeness measures, which are highly correlated with this measure. The measure in Equation 1 is chosen given its simplicity and good empirical support. In the games described below, the index varies from .1 (see Figure 2) to .9 (see Figure 3).

The game with index .1 is very uncooperative; that is, there is a high temptation to defect because there is a potential increase of the payoff from the cooperative outcome (CC; see Figure 1), which gives 10 units, to the DC or CD outcomes giving 20 units, and a low potential loss if both defect (DD) because the payoff decrease from the outcome with mutual cooperation (CC) to mutual defection (DD) is from 10 to 8 units. By contrast, the very cooperative game with index .9 has a low relative gain from defection because there are only two units’ increase from CC giving 19 units to DC or CD giving 20 units, and a high potential loss of 18 units when the comparison is between mutual cooperation (CC) giving 19 units and mutual defection (DD) offering only 1 unit.

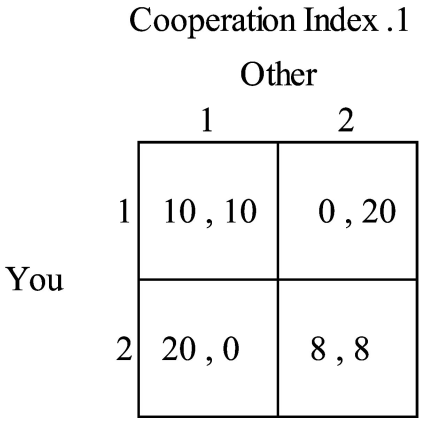

Figure 2. Prisoner’s dilemma game with index .1.

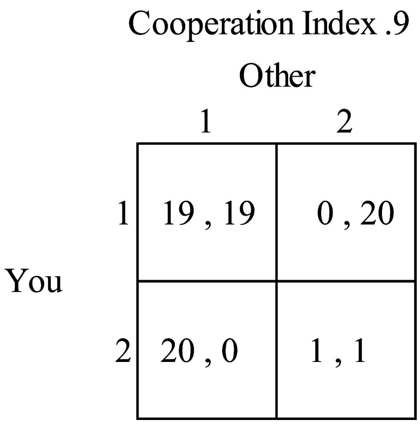

Figure 3. Prisoner's dilemma game with index .9.

Rapoport and Chammah (1965) experimentally demonstrated a linear relationship between CI and the average cooperation rate; that is, people tend to cooperate increasingly when playing games with a higher index. In their studies, however, participants either played games with only one value of the CI per session, or they played games with all nine values of the CI, and therefore, it was not clear to what extent their propensity to cooperate was affected independently only by the cooperativeness of a particular game they were playing (as assumed by the standard models) or also by the other games in the set (having a different CI). Rapoport and Chammah tried to control for this "contamination," as they called it, by varying the order of presentation of the games in the sequence. In our studies, such contamination by context is not controlled for but is the object of study. Analogously, in psychophysics there is a debate as to how to view the effects of the various contextual factors (e.g., influences of a previous stimulus on a judgment concerning the current stimulus). According to one interpretation, such effects should be eliminated or controlled for; whereas other researchers in this area have argued that these effects are a central source of insight about the functioning of the perceptual system (e.g., Laming, 1997; Lockhead, 1992, 1995).

Aside from the indexes that Rapoport and Chammah (1965) described, there are also other measures of the cooperativeness of a game, including the "*game harmony index*" (Zizzo, 2003), a measure of how harmonious (nonconflictual) or disharmonious (conflictual) the interests of players are, or the "*index of correspondence*" (Kelley & Thibaut, 1978), a measure of the scope for cooperation in a game, which is related to the amount that players stand to gain from cooperating. Both measures are more complex than the measure used in our study. There have been a few other studies showing that cooperation is a function of the incentives attached to the outcomes from the game and the relationship between these payoffs—namely, an increasing function of the payoffs associated with mutual cooperation and a decreasing function of the payoffs associated with exploitation and mutual noncooperation (Bonacich, 1972; Goehring & Kahan, 1976; Steele & Tedeschi, 1967), and the results show effects similar to the results of Rapoport and Chammah (1965). We decided to use the index developed by Rapoport and Chammah because of its relative simplicity and extensive empirical support. We believe, however, that the results we report below could be replicated with other CIs, given the high degree to which these indexes are correlated.

Summary of Hypotheses and Experiments

We have suggested that the perceived level of the "cooperativeness" of a payoff matrix may not be free from context effects and indeed that CI might behave analogously to a psychophysical magnitude. In particular, we considered whether decisions in each game with certain CI are independent from decisions in games with different CI (the sequential dependence assumption, from normative theory). If this were true in the context of game playing, then a payoff matrix could be presented within a certain context that would increase its perceived "cooperativeness," and hence players would be more likely to cooperate (play 1).

First we demonstrate that the participants were able to perfectly discriminate between games with different CI. Then we describe Experiments 1–3, in which we tested three aspects of the distribution of CIs: mean, range, and rank, respectively, which have been identified to affect judgments in psychophysical experiments and might cause similar contextual effects in games.

Game Discriminability Test

First we checked whether the participants were able to discriminate between games with different CI. For this purpose, a forced-choice test was designed. On each round participants saw two games on a computer screen, and they had to judge in which game people would be more likely to play 1, that is, to cooperate. The "correct" answer was defined by the game with the higher CI, and the participants received a point if choosing the right game. At the end of the session these points were transferred into cash according to an exchange rate.

Method

Participants. Fifty-six participants took part in this test. They (like the participants in all the experiments reported in this article) were recruited from the student population via Oxford University's Experimental Economics Research Group mailing list. All participants in this study were paid performance-related winnings of up to £3 ($5.17).

Design. A set of pairs of games was created by matching all nine indexes (from .1 to .9) with each other. This matching was done for four payoff magnitudes of each index value: the initial nine games and three variants of each, with payoffs multiplied by 4, 7, and 10 to minimize the impact of absolute payoff values on people's judgments. Table 1 presents all nine CI game types in terms of their four payoff magnitudes. The three experiments presented in this article use subsets of these games.

From all possible pairs between these games, we randomly selected only 80 game pairs for the actual test so that there were 10 trials for each of the eight possible distances between the games on the CI scale, and also the four payoff magnitudes were shown an equal number of times across the trials (i.e., each was shown 20 times). Participants were presented with the pairs and asked to select the game in which people are most likely to play strategy 1 (the cooperative response).

Procedure. Participants were presented with pairs of games on the screen and asked to click on (with the pointer of the mouse) the button next to the game in which they expected other people would be more likely to play 1. They were told that they would be presented with 90 pairs of games and that there is always a correct answer. They were also informed that they would receive one point if choosing the correct game, which would be indicated on the screen at the end of each round, and that at the end of the session these points would be exchanged with cash. It was explained that the purpose of the experiment was to investigate their ability to discriminate between games.

Table 1
Prisoner's Dilemma Games Used in Experiments 1–3

Game's CI	Outcomes as indicated in the game matrix shown in Figure 1			
	CC	ST	TS	DD
.1	10, 10	0, 20	20, 0	8, 8
.1 (× 4)	40, 40	0, 80	80, 0	32, 32
.1 (× 7)	70, 70	0, 140	140, 0	56, 56
.1 (× 10)	100, 100	0, 200	200, 0	80, 80
.2	11, 11	0, 20	20, 0	7, 7
.2 (× 4)	44, 44	0, 80	80, 0	28, 28
.2 (× 7)	77, 77	0, 140	140, 0	49, 49
.2 (× 10)	110, 110	0, 200	200, 0	70, 70
.3	12, 12	0, 20	20, 0	6, 6
.3 (× 4)	48, 48	0, 80	80, 0	24, 24
.3 (× 7)	84, 84	0, 140	140, 0	42, 42
.3 (× 10)	120, 120	0, 200	200, 0	60, 60
.4	13, 13	0, 20	20, 0	5, 5
.4 (× 4)	52, 52	0, 80	80, 0	20, 20
.4 (× 7)	91, 91	0, 140	140, 0	35, 35
.4 (× 10)	130, 130	0, 200	200, 0	50, 50
.5	14, 14	0, 20	20, 0	4, 4
.5 (× 4)	56, 56	0, 80	80, 0	16, 16
.5 (× 7)	98, 98	0, 140	140, 0	28, 28
.5 (× 10)	140, 140	0, 200	200, 0	40, 40
.6	15, 15	0, 20	20, 0	3, 3
.6 (× 4)	60, 60	0, 80	80, 0	12, 12
.6 (× 7)	105, 105	0, 140	140, 0	21, 21
.6 (× 10)	150, 150	0, 200	200, 0	30, 30
.7	16, 16	0, 20	20, 0	2, 2
.7 (× 4)	64, 64	0, 80	80, 0	8, 8
.7 (× 7)	112, 112	0, 140	140, 0	14, 14
.7 (× 10)	160, 160	0, 200	200, 0	20, 20
.8	17, 17	0, 20	20, 0	1, 1
.8 (× 4)	68, 68	0, 80	80, 0	4, 4
.8 (× 7)	119, 119	0, 140	140, 0	7, 7
.8 (× 10)	170, 170	0, 200	200, 0	10, 10
.9	19, 19	0, 20	20, 0	1, 1
.9 (× 4)	76, 76	0, 80	80, 0	4, 4
.9 (× 7)	133, 133	0, 140	140, 0	7, 7
.9 (× 10)	190, 190	0, 200	200, 0	10, 10

Note. CI = cooperation index; CC = cooperative outcome; ST = row player cooperates while column player defects; TS = row player defects while column player cooperates; DD = both players defect.

Results

Participants took approximately 20 min to complete the task. The two measured variables were the percentage of correct guesses and the response time for each possible distance between the games along the index (the distance varied from .1 to .8, and each distance was played 10 times per session). Table 2 summarizes these results averaged over participants and game pairs.

It is clear that the participants were almost perfectly able to distinguish between the games in each pair and to select the more cooperative one. Even for the smallest difference of one unit on the CI scale, people were able to recognize the more cooperative game in 85% of the cases. The response time also decreased almost linearly with the distance between the games, thus serving as an indirect demonstration that the participants were sensitive to the relative distance between the games on the scale.

Discussion

The participants were almost perfectly able to distinguish between the games, and also their response time appeared to be sensitive to the relative distance between the games on the scale. Therefore it is implausible to explain the effects in the following experiments as participants' confusion or inability to discriminate the relative cooperativeness of the games. This raises the possibility that CI is plausibly related to an underlying cognitive discrimination of cooperativeness, which may be subject to the same effects discussed above for psychophysical stimuli.

Experiment 1A

In this experiment, we isolated the effects of the mean by keeping the range of the CI identical in all conditions and varying only the mean of the distribution of CI values in the sequence. Thus we aimed to test the applicability of Helson's (1964) adaptation level theory. All participants played games along the whole range of the index (from .1 to .9). However, the participants were split into two conditions with different mean values of the distribution of the index along the session. Thus, in one condition, the distribution had a low mean CI (and was positively skewed, thus having more games with low CI) and for the other condition, the reverse. Applying adaptation level theory to CI, the CI of any individual game will be perceived not absolutely, but in terms of the mean (the adaptation level). Hence, games will be perceived as less cooperative where the mean CI is high and more cooperative where the mean CI is low. Therefore, the cooperation rate should be higher in the low mean distribution.

Note also that in both conditions of this experiment, each game was in the same position in relation to the other games in terms of range (distance between the ends of the CI scale) and rank. Thus we controlled for the effects of these two factors, which are the building blocks of the range frequency theory (Parducci, 1965, 1974).

Also worth stressing here is the potential impact of reinforcement, which predicts that people will cooperate more when the mean CI is high because the other players will be more cooperative, and therefore, cooperative actions will be less punished by defection. Thus reinforcement should always predict assimilation of the responses toward the higher or lower mean of the CI (i.e., toward the mean reinforcement and/or punishment of cooperating). However, effects of reinforcement are expected only if participants play a repeated game, but as they play successive one-shot games, one shouldn't (at least normatively) expect reinforcement effects, but later we show that reinforcement does play a role (although this is not normatively justified).

Method

Participants. Thirty-two participants in this experiment were divided into two equal groups (conditions). The participants were paid in cash at the end of the session a £2 ($3.45) fixed fee and up to £7 ($12.07) in total, with an approximate average of £6 ($10.35) depending on performance.

Design. There were two between-participants conditions. In both conditions, games were chosen across the full range of CI values (from .1 to .9).

The different frequencies of games in each condition are shown in Table 3. The numbers in the second and third row of the table represent the

Table 2
Results From the Game Discriminability Test

	Distance between games on the CI scale							
Result	1	2	3	4	5	6	7	8
Score (%)	85	100	93	100	100	100	93	100
Reaction time (s)	9.55	7.63	6.97	6.09	4.90	5.38	5.73	4.68

Note. CI = cooperation index.

frequency of appearance of each CI indicated in the top row. Thus, for example, the game with index .1 appeared 16 times in the low mean condition and only twice in the high mean condition, whereas the game with index .9 appeared twice in the low mean condition and in 16 of the trials in the high mean condition. Thus the mean of the distribution (of the CI) in the low mean condition was .33, and the mean in the high mean condition was .67. Four games with each CI were used (by multiplying all payoffs by fixed values of 1, 4, 7, and 10). Thus, the repetition of CI values became less obvious. Table 1 presents all games used in this study.

Procedure. On each trial, participants played a newly selected anonymous player from the group of eight people (which was also the group size of the other two experiments). Participants were also told that such random matching makes it impossible to infer the strategy of the other player from the history of the game. This random matching is often used in experimental economics to turn a game session within a small group into a sequence of repeated one-shot PDs against an unknown opponent, thus avoiding effects of reciprocation that would be found in a repeated PD against a single opponent. Thus, we also aimed to prevent people from learning a model of their opponent, which is another significant contextual factor that has been shown to affect behavior in games (Pruitt & Kimmel, 1977). Anonymity was ensured because the laboratory that we used is equipped with 20 computer terminals interconnected in a network (so it was possible to run interactive sessions), and each terminal is isolated in a separate box that made it impossible for the participants to see the monitors of the other players. The participants were also not allowed to communicate with each other.

Each condition consisted of a sequence of 50 rounds of the PD game (preceded by 4 practice rounds), which appeared in a random order. On each round, participants saw a matrix of the game on the computer screen, and they had to make two judgments and a decision.

The first judgment involved using the mouse to move a slider on the computer screen to a position between 0% and 100%, indicating the subjective probability that the other player will choose to play 1 in the current round. Participants were awarded additional points for the accuracy of these predictions. Note that even if people themselves always defect, measuring such predictions about other people could still indicate some biases in their perceptions, depending on the previous games (although, of course, according to game theoretic reasoning, people should expect others also to defect all the time). After making the prediction judgment, participants were asked to move a second slider between 0 and 100% to indicate their subjective confidence in the prediction. We found no interesting patterns in the confidence ratings across the conditions in this experiment and also in the other two experiments, and hence we do not discuss these results in this article. Finally, participants chose their decision strategy (1 or 2). After both players in each pair had made their decisions, participants were informed on the screen about the decision made by the other player and about the received payoff from the game and from the accuracy of the prediction.

Table 3
Distribution of the Cooperation Index Along the Whole Session in the Low Mean and High Mean Conditions

	Cooperation index								
Condition	.1	.2	.3	.4	.5	.6	.7	.8	.9
Low mean	16	8	8	4	4	4	2	2	2
High mean	2	2	2	4	4	4	8	8	16

To focus participants' attention on the differences between the games, we stated explicitly in the instructions that in every round the payoff values in the matrix would change and that we were interested in how these changes influence people's decision strategy. There was also a detailed explanation of the strategic payoff structure of the game. This aimed to combat an effect that we observed during pilot tests: that participants sometimes stop attending to each particular game and start to play according to some (usually social or value-based) rule (e.g., start always to defect or always to cooperate).

At the end of the experiment, the accumulated score (in points) was transferred into cash according to an exchange rate; that is, the experiment was conducted in an incentive-compatible way, and thus the participants were paid for their participation in cash according to their performance.

Results

Each session took no more than 60 min. Figure 4 presents the mean cooperation rates for every game (CI) in each of the two conditions. The results were averaged over all participants in each condition. The error bars represent the standard error of the mean, which is also presented in all other figures in this article.

The general trend is that the average cooperation increases as the CI increases in value, which indicates that the participants are sensitive to the index and show differential behavior depending on the values of the index. There was a higher cooperation rate on average (across all participants) in the high mean condition, where the mean rate was .59, compared with the low mean condition, where the cooperation rate was .41, and this difference was statistically significant, $t(30) = 3.01$, $p = .005$. This overpowers any effects predicted by adaptation level theory (or range frequency theory).

Figure 5 presents the mean prediction rate for every game in each of the two conditions. The results were averaged over all participants per condition. The general pattern in the results is that participants expected more cooperation as the value of the CI increased in each condition, and also, there was higher predicted cooperation for almost every game in the high mean condition compared with the low mean condition. The average prediction rate across all games was significantly higher in the high mean condition compared with the low mean condition. The mean prediction in the high mean condition was .59 versus .40 in the low

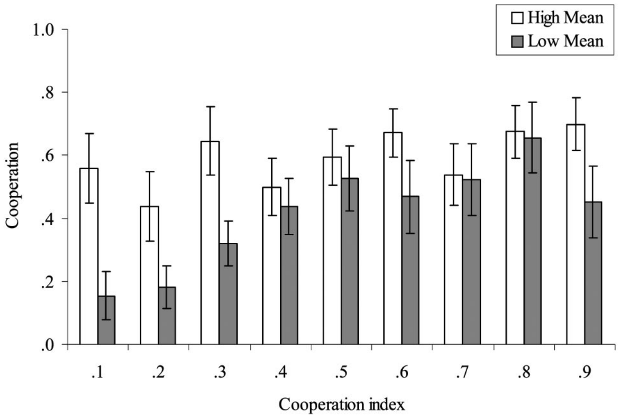

Figure 4. Mean cooperation for every game in the low mean and high mean conditions during interactive play. Error bars represent the standard error of the mean.

mean condition, $t(30) = 2.01$, $p = .053$, which is also against the prediction of the adaptation level theory.

Discussion

Participants cooperated more when the mean CI was higher. This result replicates Rapoport and Chammah's (1965) basic finding that the mean cooperation increases as the CI increases. There was a significantly higher cooperation rate and predicted cooperation in the high mean condition, which had a higher mean compared with the low mean condition. These results clearly demonstrate that when the effects of the range and the rank are held constant, and only the mean is manipulated, then a contrast of judged stimuli with the mean value of the distribution does not appear, which is contrary to the predictions of adaptation level theory (Helson, 1964). Instead, people tend to cooperate more on average in the condition with more games having higher CI, which is well documented by Rapoport and Chammah. This means that when the range and the rank of the distribution are fixed, the mean does not have an effect on judgment and choice, and then people just linearly increase their responses according to the CI (i.e., this result is just the opposite of what the adaptation level theory predicts in these conditions).

Our interpretation of this result is that the mean does not have a perceptual effect on participants' judgments and choices. However, in the high mean distribution there are more games with higher CI, and hence, in this condition the C choice is more often played and rewarded, as other players also tend to play C more often. Therefore, the C choice will be more reinforced, on average, than the D choice, and people will start to play C more often on average and also predict higher probability that the other player would play C. In the low mean distribution, the opposite tendency is clear, as most of the games have low CI and people play and predict D more often, and C gets punished most of the time, which leads to lower play of C on average. Such reinforcement effects are well documented and predict decision making in various games, including PD (Erev & Roth, 1998, 2002). In particular, there has been considerable interest in reinforcement learning (e.g., Erev & Roth, 1998): that agents tend to repeat behaviors according to the average degree of "reinforcement" with which each behavior is associated; that is, the utility for the agent of the outcome of the game reinforces the chosen strategy. From a psychological point of view, this corresponds to following Thorndike's (1911) classic "law of effect"—repeating behaviors to the degree that they are followed by positive outcomes and stamping out behaviors to the degree that they are followed by negative outcomes. In other words, reinforcement learning is driving behavior by using the mean received reward (payoff) for each action as a guide to change the probability for each action accordingly. Thus, reinforcement learning methods are based on the average amount of reinforcement that each behavior (i.e., the two responses, C and D) actually receives. In addition, agents that learn by reinforcement are theoretically attractive because such agents learn according to very simple principles that involve no "reasoning" (best-reply or otherwise) about the other agent's behavior.

In Experiment 1A, the average payoff received for C will be higher in the condition with more cooperative games because each time a player chooses C in this condition, the other player is more likely also to play C, whereas in the less cooperative condition, a C response is more likely to meet a D response. Thus, from the structure of the game, it follows that C will get higher payoff on average in the negatively skewed condition. This also partially answers the question whether action reinforcement versus predicted reinforcement is driving choice behavior. If predicted reinforcement (punishment) was behind players' actions, then there will be more defection in the more cooperative condition, where D play is more likely to meet a C response, which is more profitable than D meeting D play. The increased cooperation in the cooperative condition therefore suggests that actual reinforcement (instead of anticipated reinforcement) was guiding behavior.

In other words, when there is nothing in the distribution of games that can affect (and bias) people's perception of the cooperativeness of each game, judgments and choices are simply based on the absolute value (cooperativeness) of the stimulus (game) and the average reinforcement of each action (strategies C and D, respectively). It is possible, of course, that there are contrast effects, but these are overwhelmed by the effects of action reinforcement. However, our experimental design does not allow us to

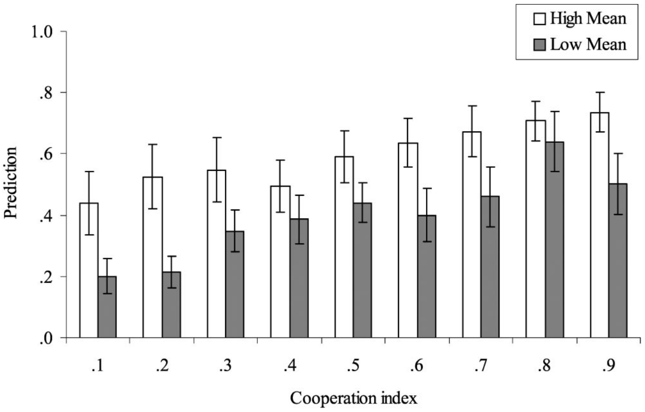

Figure 5. Mean prediction for every game in the low mean and high mean conditions during interactive play. Error bars represent the standard error of the mean.

differentiate these two effects. It is also possible to assume perceptual effect of assimilation, which may get swamped by reinforcement. To check whether the assimilation was caused only by reinforcement, we conducted Experiment 1B, in which participants had to play hypothetical versions of the games used in this experiment.

In summary, the conclusion from Experiment 1A is that the mean of the distribution (when there is no variability of the range and the rank) causes context effects in the opposite direction to those postulated by adaptation level theory (Helson, 1964). These results also do not support range frequency theory (Parducci, 1965) if we assume that games with the same CI might differ in terms of their perceived ranks between the conditions. For example, games in the middle of the range in the high mean condition were preceded by many cooperative games (on average in the sequence) and therefore should have a higher rank than the same games in the low mean condition, which were preceded by many uncooperative games. In Experiments 2 and 3, we investigated whether making the perceptual contrast (in line with Helson's, 1964, and Parducci's, 1965, theories) more distinguishable by having a fewer number of different games per session could overpower the assimilation effect based on action reinforcement.

Experiment 1B

We decided to create a hypothetical version (design) of the game for each condition of the experiment, which aimed to test to what extent the results depend on the perceptual features of the context, as we intended to demonstrate, or on some factors related to the social or dynamic interaction between the players (and this applies in future experiments too). This game design involved hypothetical play, in which the participants had to make decisions and judgments without real interaction. In this setting, on each round they made judgments about what percentage of the population will decide to cooperate in this particular game (this was the prediction task), then stated how confident they are in this prediction, and finally, they made their decisions after being asked to imagine what would they choose in this game if playing against a real opponent.

Also, if the reinforcement learning was the driving force behind the assimilation results in the interactive condition, then in the context of the no-feedback play in the hypothetical design, one would not expect to observe the assimilation effect toward the higher mean reported in Experiment 1A. In this case there would be either perceptual contrast effects if the adaptation level theory is true or no difference between the conditions if the theory is not valid.

Procedure

The hypothetical design contained 25 rounds. There were also four rounds for training at the beginning of the experimental session. The games were presented in a different random sequence for each participant. The participants in the hypothetical scenario did not receive any points on the basis of their predictions and choices. To focus participants' attention on the differences between the games, we stated explicitly in the instructions that in every round the payoff values in the matrix would change, and we were interested in how these relative changes in the game matrix influence people's decision strategy.

Results

The cooperation and prediction rates in each game were averaged over all participants in each condition. Figure 6 presents the mean cooperation for every game played in each condition. The general trend is that the average cooperation increases as the CI increases in value, which indicates that the participants are sensitive to the index and show differential behavior depending on the values of the index. In the hypothetical design there was no significant difference between the two conditions—the mean cooperation rate in the positive skew was .54 versus .60 in the negative skew, $t(142) = 0.79$, $p = .431$.

Figure 7 presents the mean prediction rate for every game in each condition. The results were averaged over all participants per condition. The general pattern in the results is that participants expected more cooperation as the value of the CI increased in each condition, and also there was higher predicted cooperation for almost every game in the negatively skewed condition compared with the positively skewed one. The average prediction rate across all games was not significantly higher in the negative skew condition compared with the positive skew condition, $t(142) = 1.44$, $p = .152$ (mean of .54 in the negative skew vs. .48 in the positive skew).

Discussion

If anticipated reinforcement (or punishment) guided behavior as participants used their estimates of others' cooperativeness to figure out what their partners would do, then in the hypothetical condition (without feedback) one would expect still to find assimilation effect, because anticipated reinforcement will be the same irrespective of the feedback. Our results demonstrated no assimilation effects, which strongly supports the actual reinforcement interpretation. In this case, a contrast effect caused by the mean should be observed if adaptation level theory is true. We did not observe such contrast, which strongly indicates that the mean of the distribution does not play the role predicted by the theory. Alternatively, one could argue that some sort of perceptual assim-

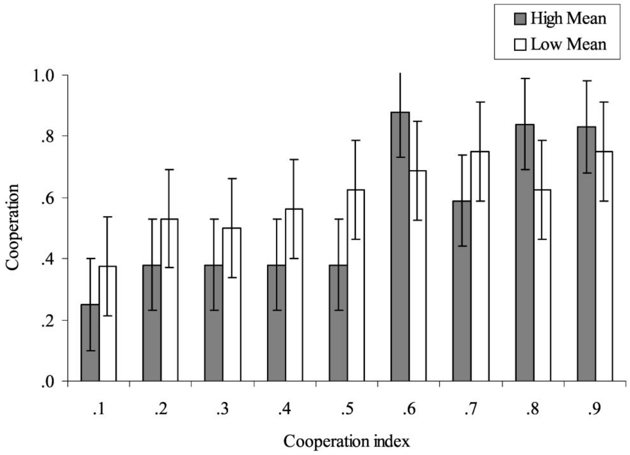

Figure 6. Mean cooperation for every game in the low mean and high mean conditions during hypothetical play. Error bars represent the standard error of the mean.

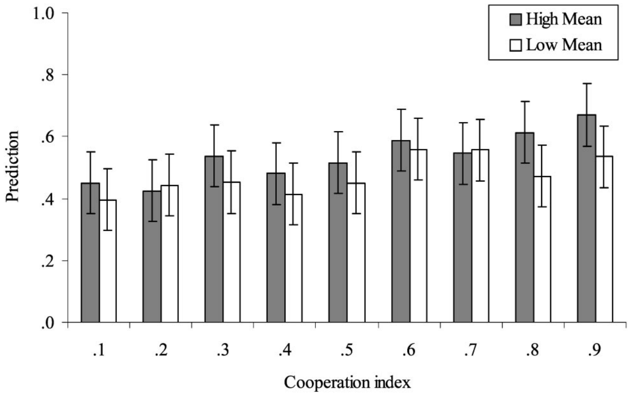

Figure 7. Mean prediction for every game in the low mean and high mean conditions during hypothetical play. Error bars represent the standard error of the mean.

ilation drove Experiment 1A's effects. However, if this were the case, then in the hypothetical scenario this would also lead to assimilation, because the stimulus set is the same. The lack of assimilation rejects the possibility for perceptual assimilation. In Experiments 2A and 3A we tried (among other goals) stronger manipulations, which may be able to overcome assimilation caused by reinforcement during actual interaction.

Experiment 2A

Experiments 1A and 1B demonstrated that there is no significant contrast effect of the mean when the range and the rank order of the games in the distribution are kept constant and only the mean is manipulated. Previous work has shown that the range of the stimuli in a distribution can affect perceptual judgments (e.g., Parducci, 1965, 1974) and also decision making under risk (Stewart et al., 2003). This experiment aimed to test whether the range would cause contrast effects if the perceived distance of a game from the minimum and maximum CI value of the set is manipulated.

There were two conditions in the experiment, and there were three CI values per condition. One value, .5, occurred in both conditions. We manipulated between conditions how much higher (lower) this game is from the lowest (highest) game in the sequence. In one condition, games had indexes of .1, .5, and .6, and in the other the indexes were .4, .5, and .9. Thus, the range distance of game .5 from the minimum value of the set is higher in the condition with games .1, .5, and .6, that is, where this distance is three units (and hence we denote it here as the high range condition) compared with this range distance in the condition with games .4, .5, and .9, where the distance of game .5 from the minimum value of the set equals only one unit (and denoted here as the low range condition). In such a design, we expected the game with index .5 in the high range condition to be perceived as more cooperative. Note also that the game with index .5 is second in rank in both conditions, and thus the rank was not expected to produce any effects.

Note, however, that the means in each condition also differ, and game .5 is higher than the mean in the ".1, .5, .6" condition and lower than the mean in the ".4, .5, .9" condition. According to adaptation level theory, this would produce the same contrast effects as predicted by range frequency theory. Therefore, the high range condition could also be called a high relative position condition, and the low range condition could be called a low relative position condition. We prefer the notation with respect to the range because in Experiment 3 the range is kept constant and we manipulate only the rank of the games, although the mean there is also manipulated, and the comparison game is again lower and higher, respectively, relative to that mean. It is important to stress here that we did not keep the means constant between the conditions on purpose, because we did not know which of the three parameters (mean vs. range or rank) would produce the most powerful contrast effect (there have not been previous studies testing the predictions of range frequency and adaptation level theory in the context of strategic games).

This experiment was also designed to contrast the opposite effects of the range and the action reinforcement caused by the higher mean, which we observed in Experiment 1A. In the condition with games with indexes .1, .5, and .6, the mean value of the CI is .4, which is lower compared with the mean of the condition containing games with indexes .4, .5, and .9, where the mean is .6. Thus, the reinforcement to cooperate more on average in the condition with games .4, .5, and .9 was expected to drive the results for game .5 to be higher than in the condition with games .1, .5, and .6. In other words, reinforcement tends to lead to assimilation for game .5, whereas range frequency theory and adaptation level theory predict contrast.

Method

Participants. There were 32 participants in this experiment, who were divided into two conditions of 16 participants each, with payment as in Experiment 1.

Design. The high range condition presented games with CIs of .1, .5, and .6, and the low range condition presented games with CIs of .4, .5, and .9. The labels "high range" and "low range" were chosen to indicate whether the key comparison games, with CIs of .5, were high or low in relation to the range of CIs. The games were presented in a random order, and the same game was never presented on two consecutive rounds; as before, four versions of each game were presented by multiplying the payoffs in a canonical game by 1, 4, 7, and 10.

Procedure. The procedure was the same as in Experiment 1A. The only difference was that there were 48 rounds (instead of 50 as in Experiment 1A) to give an equal number of repetitions of each game index in every condition (i.e., each of the three CI values was repeated 16 times; because each CI had four payoff magnitude values, the participants saw numerically the same game only four times during the 48 rounds of the session). As in Experiment 1, there were four training rounds at the beginning of each session.

Results

Figure 8 presents the mean cooperation rates for the two context conditions. The results were averaged over all participants per condition.

The average cooperation rate for the game with a CI of .5 was significantly higher in the high range condition than in the low range condition (mean of .40 in the high range vs. .16 in the low range), $t(30) = 2.46$, $p = .020$. Figure 8 shows that the cooperation rate was almost the same for games with indexes .5 and .6 in the high range condition, which suggests that the contrast effect

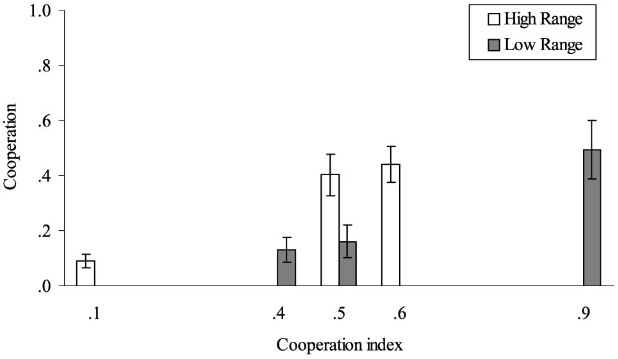

Figure 8. Mean cooperation for every game in the low range and high range conditions during interactive play. Error bars represent the standard error of the mean.

caused by the high range (in comparison with the game with index .1) had a powerful effect on the perception of the game with index .5, making it appear to be a very cooperative one. At the same time, the contrast effect of the game with index .5 in comparison with game .9 in the low range condition might also have contributed to the very low values for this game in comparison with the high range condition. Thus, the results demonstrate very significant effects of the range on the cooperation in games with index .5.

Note that in both conditions, there was a clear tendency for the cooperation to be higher in games with index .6 than in games with index .5, which indicates that the participants were sensitive to the difference between these games. The mean predicted cooperation for the high and low range conditions is shown in Figure 9. The results here were again averaged over all participants per condition.

There was a very strong effect of the range on the predicted cooperation in games with index .5, and as a result, the mean prediction for these games in the high range condition was significantly higher than the average prediction in the low range condition, $t(30) = 2.90$, $p = .007$. Here again, the results for the games with index .5 were almost the same as for the games with index .6, which suggests that the contrast of the .5 games with the much lower .1 games makes the subjective perception of the cooperativeness of the .5 games as high as the games with index .6. In the low range condition, on the other hand, there is clear evidence that the contrast of the .5 games with the .9 games reduced the prediction for .5 games to .21 (as opposed to .48 for these games in the high range condition). This result shows a strong contrast effect that is due to the manipulation of the range.

Discussion

The cooperation rate and the predicted cooperation were strongly influenced by the range of the CI of the preceding games in each condition, and participants' behavior in games with index .5 differed significantly between the two conditions. In particular, the results demonstrated that games with index .5 in the high range condition were perceived as more cooperative than games with index .5 in the low range condition, as indicated by the higher cooperation and prediction rates in the high range condition. Note that this powerful contrast effect was in the opposite direction to that predicted by considering the average level of reinforcement, which we found to be a relevant factor driving the results in Experiment 1A. The perceptual contrast effect that we observed here seemed to overpower any general effect of action reinforcement. In the low range condition, the average level of reinforcement was higher. From level of reinforcement alone we might have expected that cooperation in the .5 game would therefore be higher in this condition. But, in line with the assumption that the cooperativeness of an individual game is determined in relation to the range of cooperativeness levels observed, the opposite pattern was observed.

This result is a striking confirmation of the general idea that an abstract and complex magnitude, such as the cooperativeness of a game, can behave in the same way as a psychophysical magnitude, such as loudness or brightness; it is also a confirmation of the specific relevance of range in evaluating cooperativeness as predicted by range frequency theory. Note also that adaptation level theory also predicts the contrast effect from the lower and higher mean, respectively.

Experiment 2B

One could argue that the higher cooperation for game .5 in the high range condition compared with the low range condition in Experiment 1B could mean that cooperation was more often objectively reinforced in the high range condition than in the low range condition. That is, someone in the high range condition was more likely to be paired with a cooperating opponent, because that person was also in the high range condition. Thus, reinforcement could actually be playing a large role in Experiment 2A simply because reinforcements and punishments were not being delivered according to some objective scale but rather were being delivered by people exposed to the same skewed set of stimuli that the decision makers saw. However, in a hypothetical play, one is not paired with another player who is in the same condition, and as a consequence, the players cannot reinforce each other to cooperate more or less. In this case, perceptual effects would be the only driving force of judgment and choice. We decided to test this possibility by creating a hypothetical version of Experiment 2A.

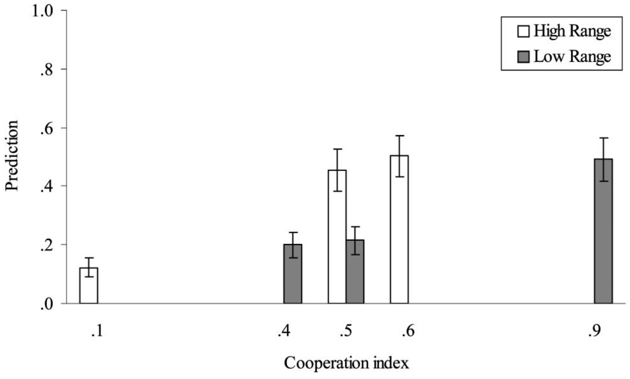

Figure 9. Mean prediction for every game in the low range and high range conditions during interactive play. Error bars represent the standard error of the mean.

Procedure

The procedure was the same as in Experiment 1B. The only difference was that the hypothetical design contained 24 rounds.

Results

Figure 10 presents the mean cooperation rates for the two context conditions of the hypothetical design. The results were averaged over all participants per condition. The graph shows very significant effects of the range on the cooperation in games with index .5 in two of the designs and no evidence for assimilation toward the mean. The average cooperation rate for game .5 was significantly higher in the high range condition than in the low range condition in the hypothetical design (mean of .80 in the high range vs. .28 in the low range), $t(14) = 3.70$, $p = .002$.

It is evident from Figure 10 that the cooperation rate was almost the same for games with indexes .5 and .6 in the high range condition, which suggests that the contrast effect due to the high range in comparison with the game with index .1 had a powerful effect on the perception of game .5, making it appear a very cooperative one. At the same time, the contrast effect of game .5 in comparison with game .9 in the low range condition might have also contributed to the very low values for this game in comparison with the high range condition in both the hypothetical and interactive designs. In the low range condition, there was also a clear tendency for the cooperation to be higher in games with index .6 than in games with index .5, which indicates that the participants were sensitive to the difference between these games.

The mean prediction for the high and low range conditions is shown in Figure 11. The results here were again averaged over all participants per condition. The prediction increased linearly along the CI, and the prediction rate for game .5 in the high range condition was higher than in the low range condition, although this difference is not statistically significant, $t(14) = 0.55$, $p = .589$.

Discussion

The cooperation rate was strongly influenced by the range of the CI of the preceding games in each condition, and participants' behavior in games with index .5 differed significantly between the two conditions. In particular, the results demonstrated that games with index .5 in the high range condition were perceived as more cooperative than games with index .5 in the low range condition, as indicated by the higher cooperation and prediction rates in the high range condition.

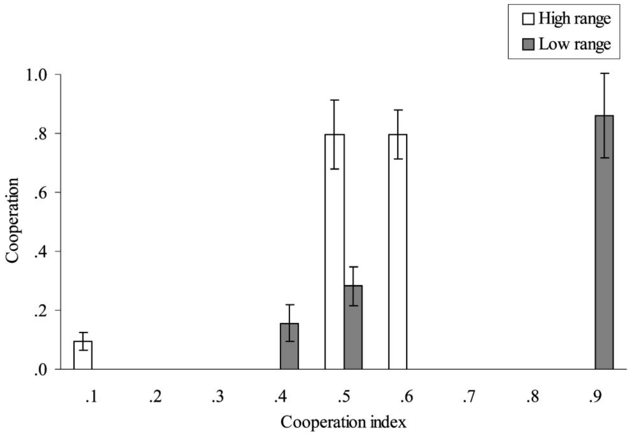

Figure 10. Mean cooperation for every game in the low range and high range conditions during hypothetical play. Error bars represent the standard error of the mean.

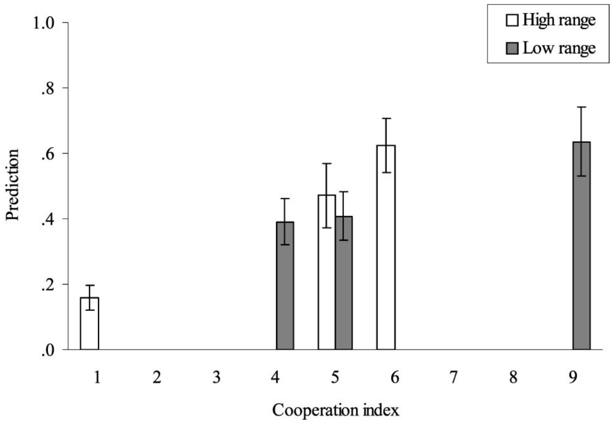

Figure 11. Mean prediction for every game in the low range and high range conditions during hypothetical play. Error bars represent the standard error of the mean.

The fact that the contrast effects were observed in the hypothetical design suggests that the context effects observed in Experiment 1A cannot be attributed to some reinforcing interaction in the high range condition. That is, one could argue that given that we found that cooperation was objectively higher in the high range than in the low range condition, it stands to reason that cooperation was more often objectively reinforced in the high range condition than the low; that is, someone in the high range condition was more likely to be paired with a cooperating opponent, because that person was also in the high range condition. However, in the hypothetical play, one is not paired with another player who is in the same condition, and as a consequence, the players cannot reinforce each other to cooperate more or less. Therefore, the only mechanisms explaining the contrast effects in the hypothetical scenario must be perceptual rather than action reinforcement ones.

Experiment 3A

Experiments 2A and 2B demonstrated that when the range of the CI is varied, with a small number of different games in the session, there are highly significant contrast effects. In Experiment 3A, we tested whether the rank is as powerful as the range in affecting cooperation, as range frequency theory would predict (Parducci, 1965, 1974). In previous work, the rank of the stimuli in a distribution has been shown to affect wage satisfaction (Brown, Gardner, Oswald, & Qian, 2003), judgments of "fair" allocation of salaries and taxes (Mellers, 1986), judgments of happiness (Smith, Diener, & Wedell, 1989), price perception (Niedrich, Sharma, & Wedell, 2001), and valuation of risky prospects (Birnbaum, 1992). This factor would predict that identical stimuli (games in this context) will be over- or undervalued compared with one another if they have respectively higher or lower rank in the distribution.

In this experiment, we kept the range of the presented games the same in all conditions and varied the rank order of the games (in terms of the CI). There were two groups playing games in conditions with different rank order between the games. The first condition included games with indexes .1, .5, .6, .7, .8, and .9, whereas in the second condition people played games with indexes .1, .2, .3, .4, .5, and .9. The expectation was that games with index .5 would be overvalued (perceived as more cooperative) in the second group because these games are fifth in rank compared with games with index .5 in the first group, in which they are second in rank. As with the predictions in Experiment 2, these predictions contrast directly with the view that the mean level of cooperativeness (or level of reinforcement) will be crucial. This is because in the first condition, the games generally have a higher CI, which would predict more reinforcement (in general) in this condition and therefore a greater tendency to cooperation in the crucial case where CI is .5. However, in this experiment there were fewer games, and hence, the perceptual contrast due to the rank order was expected to be stronger than in Experiment 1. Here again we did not keep the mean constant between the conditions, because we did not know whether the rank or the mean would produce the most powerful contrast effect.

Method

Participants. There were 32 participants in this experiment, recruited as before, who were divided into two groups of 16 participants per condition, with payment as in the previous experiments.

Design. As explained above, there were two conditions in this experiment, with different participants. The low rank condition for games with index .5 involved games with indexes .1, .5, .6, .7, .8, and .9, whereas the high rank condition for games with index .5 involved games with indexes .1, .2, .3, .4, .5, and .9. As before, the games were presented in a different random order in each session, with the constraint that identical games were never presented on consecutive rounds. Four games with each CI value were constructed, which had different absolute payoff magnitudes (as in Experiments 1 and 2).

Procedure. The procedure was the same as in Experiment 1A. There were 48 rounds in each session plus 4 rounds for training at the beginning of the experimental session. Each of the six CI values was repeated eight times, but because each CI had four payoff magnitude values, the participants saw each specific game only twice during the 48 rounds of the session.

Results

The mean cooperation rates for all games and averaged over all participants in each of the two context conditions are shown in Figure 12.

The general qualitative pattern of the cooperation for games with indexes .1 to .5 in the high rank condition is very similar to the cooperation for the games with indexes .5 to .9 in the low rank condition. This result suggests that the participants' judgments and decisions were relative to the other values in the context rather than being represented on an absolute scale.

The statistical analysis of the cooperation rate in the games with index .5 showed that in the high rank condition, these games had a significantly higher cooperation rate than in the low rank condition. Specifically, the mean in the high rank condition was .38 versus .12 in the low rank condition, $t(30) = 3.83$, $p = .001$.

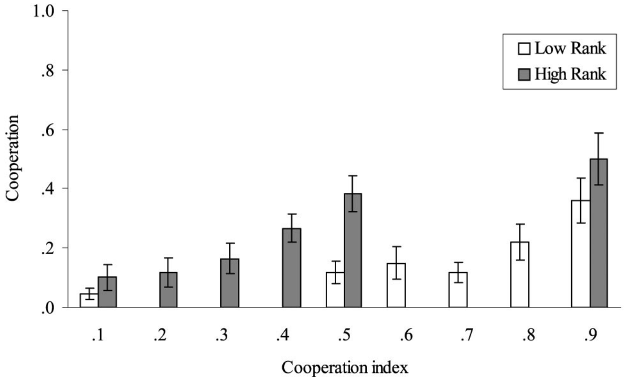

Figure 12. Mean cooperation for every game played in the low rank and high rank conditions during interactive play. Error bars represent the standard error of the mean.

The mean predictions for all games and averaged over all participants in the two context conditions is shown in Figure 13. As with cooperation rate, the prediction responses increase proportionally to the CI and show a qualitatively almost identical pattern between the prediction for games with indexes .1 to .5 in the high rank condition and the results for the games with indexes .5 to .9 in the low rank condition. These results confirm that participants' judgments were relative to the other games in the context rather than being independently judged on an absolute scale. Again, in line with cooperation results, participants' predictions of cooperation for games with index .5 were significantly higher in the high rank condition, in which they had a higher rank in the distribution: The mean in the high rank condition was .46 compared with the mean of .22 in the low rank condition, $t(30) = 3.29$, $p = .003$.

Discussion

The cooperation rate and the predicted cooperation were strongly influenced by the preceding games in the sequence, and participants' behavior in games with the same level of cooperativeness (defined by the CI) differed significantly between the two conditions. In particular, the results demonstrated that games with index .5 that had a higher rank in the distribution were perceived as more cooperative, as indicated by the higher cooperation rate and predicted cooperation of the other players.

These results show that the rank of a game in the distribution can significantly affect participants' perception of the cooperativeness of that game, and as a consequence, they will be more likely to cooperate and will predict higher cooperation when the game has a higher rank. Thus, these results on one side support Parducci's (1965, 1974) range frequency theory, which explicitly models the effects of the ranking of any given stimulus in relation to contextually relevant stimuli, but on the other side still do not exclude the possibility that there was a contrast caused by the higher and lower mean, respectively, as predicted by Helson's (1964) adaptation level theory.

Experiment 3B

The results in Experiment 3A could mean that cooperation was more often objectively reinforced in the high rank condition than

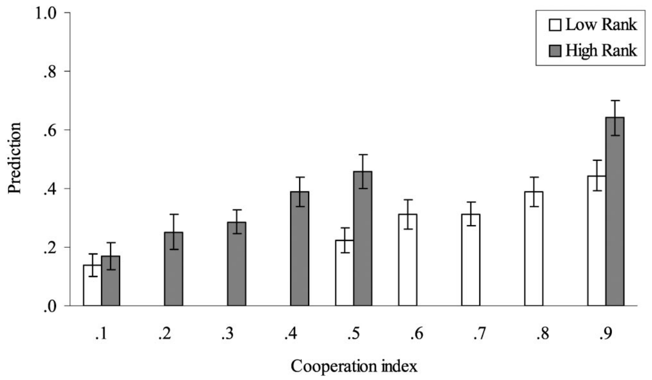

Figure 13. Mean prediction for every game played in the low rank and high rank conditions during interactive play. Error bars represent the standard error of the mean.

in the low rank condition because someone in the high rank condition was more likely to be paired with a cooperating opponent, as that person was also in that condition. This argument is similar to the one raised for Experiment 2A. Thus, reinforcement could be playing a large role in Experiment 3A because reinforcements were not being delivered according to some objective scale but were being delivered by the exposure to the same skewed set of stimuli that the decision makers saw. Here we decided to test whether the context effects observed in Experiment 3A cannot be attributed to such group dynamics. For this reason, we created a hypothetical design of Experiment 3A (analogous to Experiments 2B and 3B). In a hypothetical play, one is not paired with another player who is in the same condition, and as a consequence, the players cannot reinforce each other. In this case, perceptual effects would be the main driving force of judgment and choice.

Procedure

The procedure was the same as in Experiment 1A. The only difference was that the hypothetical design contained 28 rounds.

Results

The precise figures for the mean cooperation rates and mean predictions for all games were averaged over all participants in every condition. The mean cooperation in each game in the two context conditions is shown in Figure 14. The general pattern of the results is very similar between the cooperation rates for games with indexes .1 to .5 in the positive skew and the results for the games with indexes .5 to .9 in the negative skew. This result suggests that human judgments and decisions are relative to the other values in the context rather than being represented on an absolute scale.

The statistical analysis of the cooperation rate in games with index .5 showed that in the high rank condition, these games had a significantly higher cooperation rate compared with .5 games in the low rank condition (mean of .75 in the high rank condition vs. .35 in the low rank condition), $t(14) = 2.40$, $p = .031$. The presence of this relativity effect in the hypothetical condition indicates that the interaction was not essential to produce the context effects observed in Experiment 3A.

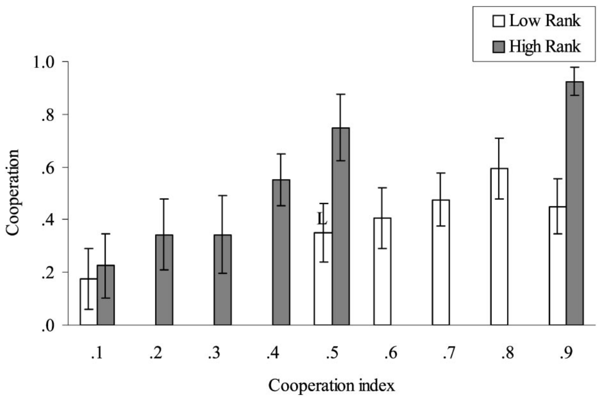

Figure 14. Mean cooperation for every game played in the low rank and high rank conditions during hypothetical play. Error bars represent the standard error of the mean.

Figure 15 presents the mean prediction responses for every game in the two context conditions (averaged over all participants). Here the prediction responses increased linearly and proportionally to the CI. The mean prediction for games with index .5 in the high rank condition was .65, which was again higher than the mean prediction for these games in the low rank condition (in which it was .51), although this difference was not statistically significant, $t(14) = 1.03$, $p = .319$.

Discussion

These results from the hypothetical play demonstrated that games with index .5 were perceived as more cooperative, as indicated by the higher cooperation rate and predicted cooperation of the other players, when these games had a higher rank in the distribution. The fact that the context effects were observed when people made only hypothetical decisions without real interaction suggests that this result cannot be attributed to some form of group dynamics.

These results clearly demonstrate that when the rank of a game in the distribution is sufficiently large, it can overcome the assim-

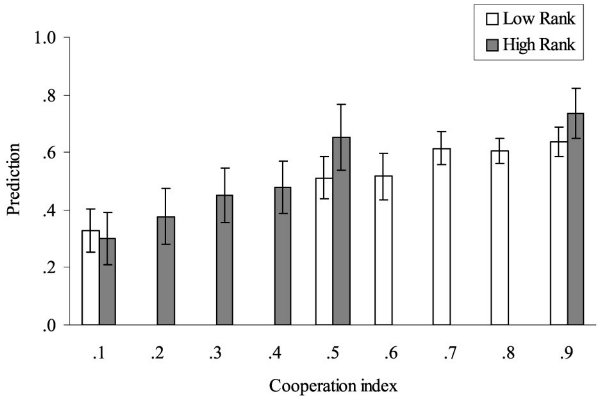

Figure 15. Mean prediction for every game played in the low rank and high rank conditions during hypothetical play. Error bars represent the standard error of the mean.

ilation effects of the mean (which in this case would drive the results in the opposite direction) and can significantly affect participants' perception of the cooperativeness of that game. As a consequence, players are more likely to cooperate and to predict higher cooperation when the game has a higher rank.

General Discussion

These experiments suggest that games are assessed, and strategic choices made, relative to previously encountered games. We call this phenomenon *game relativity*. This effect has been observed with the PD game, but we believe that the results can be generalized to other games in which behavior depends on a single, complex dimension (i.e., some analog for CI). More specifically, we conjecture that in complex strategic judgment tasks more generally, the pattern of predictions of range frequency theory (Parducci, 1965) may apply.

Experiments 1A and 1B tested Helson's (1964) adaptation level theory. In contrast to the predictions of adaptation level theory, we did not find contrast effects, depending on whether a particular game is above or below that mean (adaptation level). Instead, the condition with a higher mean leads to more, rather than less, cooperation. This effect of the mean CI can be explained simply by the assumption that cooperativeness is influenced by the amount of cooperation observed that participants received, independent of which game they are playing. This fits with reinforcement accounts of game playing (e.g., Erev & Roth, 1998). In Experiments 2A and 2B, the range difference between the games was manipulated while keeping ranks constant. The range had strong impact and produced a contrast effect so that games that were further from the minimum CI value in the sequence were perceived as more "cooperative." In Experiments 3A and 3B, we varied rank while keeping range constant and found that the rank had a significant impact on prediction and choice behavior. The same game presented in a "high rank" condition produced significantly higher cooperation and prediction than when in a "low rank" condition. In Experiments 2 and 3, the effects of range and rank are pitted against, and outweigh, the frequency effect of the mean observed in Experiment 1, which might be expected on reinforcement-based accounts (predicting that when there are more cooperative games on average, this would lead to higher cooperative feedback, reinforcing each player to cooperate more across all games). Thus, the results from Experiments 2 and 3 support the predictions about perceptual contrast in line with the range frequency theory (Parducci, 1965, 1974) and also Helson's (1964) adaptation level theory. In Experiments 1A and 1B, the perceptual contrast was perhaps not triggered strongly enough because of the even spacing of the games on the CI, which was evident in Experiment 1B. Thus, the assimilation caused by mean reinforcement may have caused the effects observed in Experiment 1A. Finally, the participants were almost perfectly able to distinguish between the games, because their cooperation and prediction rates increased almost linearly along the CI scale in each experimental condition. This raises the possibility that CI is plausibly related to an underlying cognitive scale, and hence, it may be subject to the same effects discussed above for psychophysical stimuli.

The contextual effects caused by the mean, range, and rank of the distribution were found for both the interactive and hypothetical designs, which confirmed our expectations that the relativity effects are due to some general underlying cognitive mechanisms related to the representation of perceptual magnitudes. In the interactive and hypothetical designs of Experiments 2A, 2B, 3A, and 3B, the cooperativeness of the games was purely dependent on their range and rank order, respectively.

In summary, our results show that participants do not have an absolute grip on the level of cooperativeness implicit in each game in the sequence set and instead choose a decision with reference to its cooperativeness relative to the other games in the set (while being perfectly able to discriminate between games differing in cooperativeness, as demonstrated by the game discriminability test). These results are also consistent with the results from experiments on nonstrategic risky choice, in which certainty equivalent judgments and choices of prospects (gambles) were used (Stewart et al., 2003).

Our results are consistent with a purely relative representation of cooperativeness (i.e., in relation to the range and rank of other games in the distribution). One key question here is whether this subjective CI is affected by participants' tendency to cooperate directly or by modifying the perceptions of the other player's cooperativeness, which in turn affected one's own behavior. That is, for example, in the high range conditions of Experiment 3, in which a CI of .5 looked relatively "cooperative," did higher cooperation result because defecting seemed less tempting, or did people cooperate because they judged their opponents to be less likely to defect and hence felt less concerned about being punished for cooperating?

If anticipated reinforcement guided behavior, then in Experiment 1B (using hypothetical conditions without feedback) one would expect still to find an assimilation effect in Experiment 1B, because anticipated reinforcement will be the same whether the game is actually played. Yet we found no assimilation effects, which strongly supports the actual reinforcement interpretation.

Another possible concern relates to methodology. Although opponents were anonymous, more consideration could be given to whether some sort of reputation could nonetheless have arisen. Essentially, if each participant played up to 50 total trials with one small "opponent pool," a participant certainly could develop expectations of any given opponent's future behavior on the basis of past behavior of the opponent pool. Thus, groups may have acquired reputations in the minds of individual players. In this respect, examining how quickly the context effects emerged might give insight into whether the effects emerged slowly, as a group's reputation was established, or whether they emerged very quickly, as they might if the mechanism were a lower level perceptual contrast. We examined the first 10 trials and found there were tendencies reported in the predicted direction, although the key statistical differences described for each experiment were not significant except for the prediction judgments in the hypothetical play. By the 20th trial, most of these differences were already significant. This result indicates that people need a few repetitions of the stimuli to build the internal scale according to their relative magnitudes. This is not surprising in light of the complexity of the CI. We still do not exclude the possibility of reciprocal reinforcement of these context effects in the interactive condition (although reputation cannot explain the direction of the effect), but the fact that the effects were present also in the hypothetical play supports our hypothesis that the mechanism is a lower level perceptual contrast.

Can Existing Theories of Decision Making Account for Game Relativity?

The perceptual context effects caused by the range and the rank (and possibly the mean) of the distribution confirmed our expectations that the relativity effects are determined by common principles of magnitude representation that apply across perceptual and cognitive domains. In particular, this study showed for the first time that sequential context effects found in other domains in psychology (predominantly in psychophysics and perception, but also in social cognition and individual decision making) also occur in interactive (strategic) decision making (under uncertainty).

We are not aware of an existing theory of strategic decision making in games that can account for the results presented here. Stewart et al. (2003) discussed various existing theories of a decision under risk and the account they might offer of the prospect relativity phenomena that they describe, in the context of nonstrategic, risky decision making. Their conclusion was that there are plausible theoretical accounts in which prospect attributes are compared with those of other competing prospects, such as, for example, the stochastic difference model (González-Vallejo, 2002), multialternative decision field theory (Roe, Busemeyer, & Townsend, 2001), and range frequency theory (Parducci, 1965, 1974). Most of these models cannot, however, be directly applied to the context of strategic decision making in games, in which it is crucial that the outcome of each player's decision depends, in a recursive way, on how that player expects the other player to behave.

One possible way to account for the contrast effects is to assume that the internal scale used to represent the items in question (e.g., cooperativeness, risk, and payoff) is not fixed but stretchable (e.g., the scale representing the cooperativeness of the game), perhaps to accommodate task demands most efficiently.

As discussed above, an attractive existing model of context effects in judgment conforming to these principles, which is also a plausible account of the contrast effects caused by the range and the rank of the games in the sequence, is Parducci's (1965) range frequency theory (i.e., the contrast effects observed in Experiments 2A and 2B can be explained by range affects, whereas the rank effects observed in Experiments 3A and 3B can be accounted for by the frequency principle). At the same time, in both experiments, a contrast with the mean as postulated by Helson's (1964) adaptation level theory might also explain the data. Nonetheless, there is not presently a psychological account of strategic decision making into which theories of relative judgment such as the range frequency theory or the adaptation level theory fit as a component.

Relevance for Research on Strategic Decision Making

The context effects that we have demonstrated have a substantial effect on behavior in games and therefore are of interest to those studying strategic interactions and decision making in psychology, economics, and other social sciences. There are interesting connections with earlier work on context effects in strategic games, as explored by social psychologists in the 1970s and by behavioral economists more recently. These connections help clarify how our results complement other work undermining the canonical economic model of strategic behavior as a descriptive psychological account. For example, Pruitt and Kimmel (1977) reviewed the early problems posed by research on behavior in games and raised the concern that research restricted to laboratory studies might lack external validity. Real-world interactive decision making is inevitably more complex, and to discover the settings to which an experimental result can be generalized, researchers need to find what additional (moderator) variables interact with the independent variables that are manipulated. Pruitt and Kimmel called these variables "background conditions," including situational variables (Oskamp, 1971) and the effects of the social and interpersonal nature of the interaction (e.g., Enzle, Hansen, & Lowe, 1975; Gardin, Kaplan, Firestone, & Cowan, 1973; McClintock, 1972).

We believe that the list of contextual factors should be updated to include the basic perceptual effects and factors described in the research reported here, which can strongly influence behavior in interactive settings. In addition, we used a random matching design so that players were randomly assigned to different opponents on every round, which would prevent them from learning a model of one's opponent. Such learning is another significant contextual factor that has been shown to affect strategic choice behavior (e.g., Rotter, 1971; Swingle & Gillis, 1968; see also Pruitt & Kimmel, 1977, for a review) and also the expectations about another person's cooperative intentions in the PD game (Kelley & Stahelski, 1970a, 1970b).

Psychologists have extensively explored social factors in promoting cooperative or uncooperative behavior (see Dawes, 1980, for a review), including aspects of communication, fear, and greed (e.g., Insko, Schopler, Drigotas, & Graetz, 1993); gender (Orbell, Dawes, & Schwartz, 1994); institutional context (Hargreaves & Shaun, 1994); and moral and social norms (De Jong, Peters, De-Cremer, & Vranken, 2002). Yet few studies have focused on the underlying cognitive processes and mechanisms that play an active role in interactive decision making and determine the choice behavior. One exception is the extensive research on the role of simple decision heuristics such as tit-for-tat (e.g., Axelrod & Hamilton, 1981; Messick & Liebrand, 1995; much of this work focuses on repeated PD, but there may be some overspill from human decision-making strategies developed for repeated interactions to one-off interactions) and also on the role of reinforcement learning in determining the emergence of cooperation as an adaptive response to the environmental and social context (Baker & Rachlin, 2002; Rachlin, Brown, & Baker, 2001). Note that such forms of reinforcement might have determined the behavior in Experiment 1, as discussed earlier.

We aim to contribute to this tradition by adding to the picture more pieces of the cognitive architecture that produces choice behavior during strategic interaction. Note that there are still some theorists who argue that nonstrategic forces such as attitudes, feelings, and norms have little influence on behavior in strategic environments such as the PD games reported here. For example, the goal expectation theory (Pruitt & Kimmel, 1977) deals only with the psychological forces that shape people's behavior when they are trying to be strategic and not in other contexts. In Experiments 2 and 3, we have shown, however, that even low-level perceptual effects related to the representation of magnitude information, which are general characteristics of human cognition and not specific only to strategic interactions, can have substantial impact on decision behavior. Therefore, our results imply that the models of strategic behavior need to be supplemented by a more general cognitive decision theory of strategic interaction, which grounds decision making on the underlying cognitive representations and mechanisms that produce decision behavior.

Task Demands

Another methodological concern regarding the design of the three experiments presented here is that our results might have been influenced by possible demand characteristics of the experimental setup: specifically, that the participants were told explicitly that the experimenter was interested in how the game payoff values influence their decision strategy. This raises the possibility that participants could have become overly sensitive to experimental manipulations, which otherwise might have small or null effects in a more natural setting. Note, however, that previous experimentation by Rapoport and Chammah (1965) has extensively demonstrated that CI does covary with cooperative behavior. In addition, similar results were obtained for other measures of game cooperativeness (Bonacich, 1972; Goehring & Kahan, 1976; Kelley & Thibaut, 1978; Steele & Tedeschi, 1967; Zizzo, 2003). All these measures are highly correlated in terms of their effect on cooperative behavior. Hence, people must at least be influenced by CI, whether told to pay attention to payoffs or not. But in any case, this would not explain the pattern of results that we have found, such as the specific influence of range and rank.

Limitations and Future Directions

We concluded that the assimilation effect in Experiment 1 goes against the adaptation level theory. However, there might be a possible confounding of the distribution shape, which could make this conclusion unwarranted. In the low mean CI, most games are uncooperative relative to the mean (positively skewed), and the opposite is true for the high mean CI (negatively skewed). It seems that different types of distributions, separating the effects of frequency and central tendency, need to be used to make a firmer conclusion. Manipulating the shape of the distributions would also shed light on the reinforcement hypothesis. An interesting direction would be to investigate the level of cooperation in two sets of games that are equivalent in their mean but differ in skewness. For example, one set could have a symmetric distribution around the mean, whereas the other set could have many more high CI games with one outlying low CI game. Here, a reinforcement-based approach would predict more cooperation in the second set even though the two sets have equal means. In this scenario, adaptation level theory, which postulates that people are adapting to the mean, would predict no difference in behavior. Alternatively, one could directly manipulate reinforcement level by “cheating,” that is, by artificially making reinforcement high or low, independent from CI, by using computer-generated responses.

In Experiment 2, a contrast effect occurred from the majority of the games to the minority of them. In the first case, midlevel games (with a CI of .5 or .6) appear very cooperative relative to the minimum of .1. In the second set, games with a CI of .4 or .5 appear uncooperative relative to the high game of CI equal to .9. Thus, we show that the level of cooperation depends locally on CI levels but depends globally on the distribution of games. Figure 6 shows that the level of cooperation of the two sets is virtually identical. What happens just at CI of .5 is of interest, but even more interesting is that cooperation levels can be roughly the same for games with a CI of .1 or a CI of .4 and .5, depending on the distribution. This might not be due just to the range, however, because a different pattern of cooperation may result for a distribution with identical range as the first one (high) with games at CIs of .1, .2, and .5, in which most of the games are very uncooperative. A clearer set of distributional manipulations might be needed to separate effects of distance due to a minimum value (i.e., the range) and effects due to the maximum values in a distribution and the frequency with which these values occur.

As with previous studies, the rank manipulation in Experiment 3 might be confounding the frequency of values and the target factor (in this case rank). The two sets of CI values position the target value of .5 in two different ranks but with uniform distributions. Because of this, the low rank condition contains a majority of games that are more cooperative than the target one, whereas the opposite is true of the high rank group. What if we were to have a set of games with a low rank of .5 in which 80% of the games had a CI of .1 (and the other CI values each appeared 4% of the time)? In that situation, the majority of the games would be uncooperative, and relative to this, a game with CI equal to .5 may be overvalued instead. Similarly, what if the high rank group had 80% of games with a CI of .9? Again, a crucial factor might be the frequency of occurrence of games (percentile rank) rather than simply the rank, but without a manipulation of distributions we do not know exactly what decision makers are being sensitive to.

Further research is also required concerning the precise mechanism(s) that are responsible for affecting levels of cooperation and predicted probability that the other player would choose cooperation. We do not discuss how perceived “cooperativeness” affects the probability that the other player will cooperate, although the results hinge on this relationship, and thus, it needs to be more fully specified in future research.

All that being said, clearly the set of games a person is confronted with affects the level of cooperation for individual games. We believe this is a very interesting and new result. And the possible future tests described here (involving fine-grained manipulations of the distributional parameters) will help to decide whether adaptation level theory, range frequency theory, or reinforcement theories provide the best model of the results we present. Note, however, that such fine-grained work will be much more challenging, in comparison with similar work with perceptual stimuli, given the myriad other possible factors playing a role in the interactive decisions.

Concluding Remarks

Most applications of normative models of interactive (game-theoretic) decision making typically assume that only the attributes of the game need be considered when reaching a decision. The results presented here show that the attributes of the previously seen games influenced judgments and decisions in the current game, which suggests that games are not considered independently of the previously played games, whether the indicators are predicted behavior of the opponent or choice of a strategy.

There were two types of contextual influences going in opposite directions. One was the assimilation to the mean cooperativeness in the session caused by action reinforcement, as demonstrated in Experiment 1, and some economic models use this (Erev & Roth, 1998). The other effect was the perceptual contrast effects shown in Experiments 2 and 3, and theories of perceptual judgment accounting for this behavior include Parducci's (1965) range frequency theory and Helson's (1964) adaptation level theory (in both of which stimuli are valued relative to other stimuli in the sequence or the choice set). Experiments 2 and 3 showed that when perceptual contrast is made more salient by reducing the number of different games in the session, then perceptual contrast overpowers assimilation based on action reinforcement. Experiments 2 and 3 also indicated that the contrast of the current game with the other games in the sequence could possibly depend on the position of the current game in the range, and the rank of the previous games in terms of their cooperativeness (as predicted by range frequency theory).

In summary, all three experiments demonstrate the strength of manipulation of the distribution of one-shot games played in a sequence. And future research should find out how and to what extent the precise parameters of each distribution trigger the opposite forces of action reinforcement-based assimilation and perceptual contrast. Such research should also distinguish whether range frequency or adaptation level theory is the better account of these context effects. In any case, our findings present another challenge to the standard rational choice theory and game theory as descriptive theories of decision making under uncertainty. Our results also present a challenge to descriptive theories of decision making, in which each one-shot game is considered independently.

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Received November 30, 2004
Revision received September 20, 2005
Accepted September 29, 2005 ■