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Learning to Make Decisions: When Incentives Help and Hinder

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Under what conditions do explicit incentives affect learning and performance in decision making? Noting that feedback often confounds information concerning the underlying structure of decision-making tasks (i.e., how variables are related) and levels of performance, we distinguish tasks on dimensions of complexity and exactingness. By ~~the~~ 'a' former, we mean the ease or difficulty of inferring the underlying structure of the task, by the latter the extent to which erroneous decisions are penalized. Errors are heavily penalized in exacting environments but lenient environments are forgiving. It is assumed that incentives increase effort and attention but do not have a direct effect on performance. Instead, by increasing effort and attention, incentives increase expectations of performance. When higher aspirations are not satisfied, however, frustration ensues leading to lower performance. It is argued that exactingness is unlikely to be important

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in tasks that are inferentially simple or complex because of ceiling and floor effects: in the former, there is little room for improvement; in the latter, little possibility for decrements in performance. In tasks of intermediate complexity, exactingness is predicted to have an inverted-U shaped relation with performance. This occurs because feedback in exacting environments induces contrary forces. On the one hand, it is more refined thereby providing greater opportunities for learning; on the other, it is liable to be more frustrating with outcomes falling below expectations. Because increases in expectations induced by incentives are likely to be satisfied in lenient environments but frustrated in exacting ones, incentives are predicted to help performance in lenient environments but hinder in exacting. These predictions are tested and validated in two experiments. A further experiment tests the effects of having subjects concentrate on learning the decision making task as opposed to being concerned with performance. The experimental results are discussed from both theoretical and practical perspectives and suggestions made for further research.

Learning to make decisions: When incentives help and hinder*

The relation between motivation and performance has intrigued psychologists for many decades (Yerkes & Dodson, 1908). In this paper, we consider one important aspect of this issue. What are the effects of external incentives on decision making? More specifically, under what conditions do incentives aid or impair performance when people are *learning* to perform decision-making tasks?

There are several reasons for studying this question. First, at a practical level it is important to know whether and when providing external incentives improves learning. In business or the military, for example, does the existence of real consequences in terms, say, of money or lives help people learn to make decisions more effectively? If incentives are detrimental, how can learning be structured to overcome this impediment? Second, from a theoretical viewpoint controversy exists as to whether incentives will necessarily improve performance. From naive behaviorist or economic viewpoints, for example, one could argue that incentives will always improve performance and much evidence is consistent with this contention. However, there is also evidence suggesting that under some conditions incentives may be detrimental to learning (see, e.g., Lepper & Greene, 1978). One way of reconciling these conflicting views is to specify the conditions under which the existence of external incentives is likely to help or hinder the learning of specific kinds of decision-making tasks. This is the goal of the present paper.

To achieve our goal, we argue that it is important to specify three aspects of the system linking external incentives to learning as measured by performance. These are, first, the kind of decision-making task; second, the critical features of the environment in which learning occurs; and third, the mechanisms by which external incentives affect people learning the task. Whereas the latter will be mediated by characteristics of the human decision maker, the two former aspects

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reflect task variables. Behavior, in this case learning and performance, results from the interaction of both human and task characteristics (cf., Brunswik, 1952; Simon & Newell, 1971).

The paper is organized as follows. We first comment on several studies in the literature that have considered the link between incentives and performance in decision making. Next, after specifying the decision-making task considered in this paper, we outline the critical features of task environments that we believe are important to the relations between incentives and performance. These are what we call, respectively, the *complexity* and *exactingness* of the environment. By the former, we mean the extent to which it is easy or difficult for the decision maker to infer the structure of the underlying task in which decisions are made; by the latter, we mean the extent to which the decision maker is penalized for failing to make the appropriate decisions. In an *exacting* environment, even slight deviations from "correct" decisions are heavily penalized; *lenient* environments, on the other hand, are forgiving of the decision maker's "mistakes." In discussing the effects of external incentives, we note that whereas incentives increase effort and/or attention, they do not necessarily improve performance. On the other hand, we argue that increases in effort lead to *expectations* of higher levels of performance which may or may not be realized. These three considerations concerning (a) the decision-making task, (b) the nature of the decision-making environment, and (c) effects of incentives on decision makers' expectations, lead to a series of predictions concerning the relation between incentives and learning that are tested in two experiments. In a further experiment we consider the effects of having subjects concentrate attention on learning the decision-making task as opposed to being concerned with performance. Finally, we discuss the results of our experimental work from both theoretical and practical perspectives and make suggestions for further research.

Decision making under incentives

In this work we distinguish between internal and external incentives. By internal incentives we mean any intrinsic motivation people may have to perform well in a task, the source of which can have various origins including, for example, a need to exhibit mastery (White, 1959), pride, or a wish to impress others. By external incentives we mean explicit rewards such as money that depend on performance. Thus, when in this paper we talk about manipulating incentives, we refer only to *external* incentives.

The role of incentives has been examined in several different types of decision-making tasks. For our purposes, it is useful to distinguish between studies in which subjects did or did not receive feedback following their decisions since the latter provide no opportunity for learning.

In the no-feedback studies subjects typically make judgments or choices and are informed that these will have consequences for them. For example, after having made several hypothetical choices in gambling experiments, subjects may be required to play a randomly selected choice for real stakes. Results indicate that under incentives people pay more attention and time to the task and appear to be more "motivated;" however, it is not clear whether this improves "performance" and, in particular, whether incentives lead people to concentrate on the appropriate dimensions of tasks. Indeed, in 1959 Easterbrook summarized a vast psychological literature that shows that under high drive states people restrict attention to limited ranges of available cues and that this can inhibit both learning and performance in cognitive tasks. Unless one believes that people should always be able to intuit "optimal" responses, it should come as little surprise that, in the absence of feedback, incentives have produced a variety of effects. In some tasks, subjects exhibit greater risk aversion in respect of possible losses (Slovic, 1969; Schoemaker, 1988). In others, real payoffs did not reduce the biasing effects of payoff size on inferred subjective probability estimates (Slovic, 1966; Hogarth & Einhorn, 1989), and the presence of incentives has not been found to diminish the level at which people exhibit "preference reversals" (Lichtenstein & Slovic, 1973; Grether & Plott, 1979). On the other hand, in a task involving the estimation of frequencies, Wright and Aboul-Ezz (1988) did find that incentives led to more accurate assessments.

The presence of feedback might lead one to expect a simpler relation between incentives and performance. However, this has not proven to be the case. One well-studied task is the binary-outcome prediction task in which subjects are required to predict which of two signals will appear on each of a series of trials (for an overview, see Luce & Suppes, 1965). The relative frequency of the two signals is governed by a Bernoulli process such that on each trial the probability of one signal appearing is π and that of the other $1 - \pi$. When $\pi \neq .5$, the optimal response is to predict the more frequent signal on every trial. However, this is rarely done. Instead group data tend to show that the proportion of times subjects choose the more frequent signal approaches π as the number of trials increases. This is known as probability *matching* as opposed to probability *maximizing* behavior. Effects of incentives have produced mixed results in this paradigm. Siegel (1961) used two levels of monetary incentives and found that, with the greater level of incentives, the proportion of time subjects chose the more frequent signal became quite extreme (.95 instead of the normatively appropriate 1.0). Edwards (1956) also found more extreme probabilities under incentives, and Tversky and Edwards (1966) found that although incentives changed behavior, it was still far from optimal. In general, the results of these and similar experiments is that payoffs affect subjects' behavior in the appropriate direction, but that subjects still do not behave as the normative models prescribe.

More recently, Arkes, Dawes and Christensen (1986) used a probabilistic task in which subjects were given a good rule that would have enabled them to choose correctly 70% of the time. They found that, with incentives, subjects were more willing to abandon the rule and try to outpredict the system with the result that they performed worse than those who had no incentives. As has been amply documented (see e.g., Hogarth, 1987), the strong desire to master one's environment can lead people to ignore the implications of statistical regularities and this can be exacerbated when incentives are high (see also Einhorn, 1986). In commenting on the role of incentives in the binary-outcome prediction task, Edwards (1956) suggested a hypothesis in the spirit of the Yerkes-Dodson law. With no real incentives, subjects attempt to match rather than maximize (it's more fun to try to predict each trial, see Siegel, 1961); with small incentives subjects will move toward maximizing; however, with larger payoffs subjects don't like seeing the necessary losses associated with a maximizing strategy, and, in trying to predict each trial, regress to matching behavior.

In an important paper, Schwartz (1982) has studied how reinforcement (rewards for appropriate responses) shapes the learning and performance of particular behavioral sequences such that people develop stereotypic responses. This can have both functional and dysfunctional consequences. Stereotypic responses can be highly effective if one has to deal with the same task on many future occasions. However, the development of these responses can interfere with discovering other rules or responses that could also be used to accomplish the task and can handicap the transfer of knowledge.

Other researchers have shown that rewards are not always beneficial; when, for example, external incentives are removed for performing a task which people find intrinsically interesting, subsequent interest and performance in the task can decrease (Lepper, Greene, & Nisbett, 1973; Levine & Fasnacht, 1974). The presence of incentives has also been found to reduce the amount of incidental learning people acquire in cognitive tasks, presumably because attention is focussed on the central task that is rewarded (Bahrick, 1954; Bahrick, Fitts, & Rankin, 1952).

In summarizing many studies on the effects of incentives, McCullers (1978) makes the point that incentives enhance performance when the latter depends on making "simple, routine, unchanging responses and when circumstances favor the making of such responses quickly, frequently, and vigorously" (p.14). He goes on to note that the role of incentives is far less clear in tasks that require flexible, open-ended and creative responses and, in fact, there is evidence to suggest that incentives can be detrimental in tasks requiring creativity and problem solving abilities (McGraw & McCullers, 1979). A similar distinction is made by McGraw (1978) who distinguishes, on the one hand, between tasks requiring algorithmic or heuristic, problem-solving, mental strategies, and on the other between tasks that subjects find attractive or aversive. McGraw reviews several literatures to conclude that incentives are detrimental to performance in tasks that subjects find attractive and which require heuristic, problem-solving, mental strategies.

Theoretical framework

The specific task. The task used in our studies can be characterized on a number of dimensions. First, it involved a series of discrete decisions made across time in a system with a stable, underlying data generating process in which successive observations were statistically independent. Second, on each trial the decision maker observed a value of a predictor variable and then selected a value of a decision variable. This was followed by immediate feedback expressed in "evaluation points" concerning the outcome of that decision. Third, in addition to the outcome feedback, the decision maker was also provided with some additional information concerning the implications of the decision just taken. Fourth, subjects were instructed that the object of the game was to maximize the number of evaluation points. To be concrete, in the first experiment subjects were told that they were managing a small business which sold a perishable product on a daily basis. Each day (experimental trial) the subject was shown a value of a variable "temperature" (a weather forecast) prior to making a decision which was the quantity of the product to be ordered for the day (in number of units). Subjects were told that all units unsold at the end of the day would be lost. In addition to feedback in terms of evaluation points (an unspecified function of accounting profits), feedback also consisted of information concerning the number of units sold, unsold, and the sales cost.

The structure of this task is similar to the much studied single- and multiple-cue probability learning paradigm (see, e.g., Brehmer & Joyce, 1988) and, as such, suggests that one should be able to relate results to that literature. However, there is an important difference in that, in addition to inferring the structure of the underlying system in terms of relations between variables, the subjects were required to make decisions which were evaluated.

Important dimensions of task environments. The task we employed is similar to many real-world situations in that subjects are forced to learn by attending to outcome feedback. Whereas it has long been recognized that outcome feedback can be ambiguous, and even misleading (see, e.g., Hammond, Summers, & Deane, 1973; Einhorn & Hogarth, 1978; Brehmer, 1980), we wish to emphasize a specific aspect of the ambiguity inherent in outcome feedback in these kinds of decision-making tasks. This is that outcome feedback simultaneously conveys and confounds information concerning both the structure of the underlying decision-making task and how well the subject is performing. Thus, on receiving feedback a person may attempt to infer both something about the structure of the task (e.g., how two variables are related), and the level of his or her performance (e.g., better than expected, better than a rival, etc.).

We conceive of feedback as being a function of three variables: (1) the specific action taken by the decision maker; (2) the nature of the underlying system governing outcomes; and (3) the manner in which these outcomes are evaluated in the payoffs (i.e., feedback) received by the subject. To illuminate the distinction between (2) and (3), note that if two otherwise identical tasks differed only in how outcomes were evaluated, a person making the same decisions in both tasks could receive different feedback. However, if the person was ignorant a priori of both the nature of the underlying task and how outcomes were evaluated, it would be difficult to attribute differences in feedback (i.e., payoffs) to the different evaluation functions as opposed to possible differences in the structures of the underlying tasks.

This discussion suggests the need to distinguish two aspects of decision-making tasks. These are, first, *complexity*, and second, the extent to which the payoff function evaluating outcomes is relatively *lenient* or *exacting*. We define complexity relative to the knowledge of the decision maker. A task is said to be complex to the extent that the decision maker lacks knowledge or past experience with it, there are many as opposed to few variables, the cues that suggest the nature of the underlying system are misleading and/or difficult to interpret, there is random noise in the system, and so on. Tasks are exacting to the extent that small deviations from optimal decisions are heavily punished, and lenient to the extent that they are not. In this work, we characterize decision tasks by their locations in a two-dimensional space of *complexity* and *exactingness* as represented in Figure 1.

Relative to a decision-maker's state of knowledge, tasks can be located in Figure 1 along the horizontal complexity dimension. Similarly, depending on the extent to which deviations from optimal decisions are penalized, tasks can also be located on the vertical exactingness dimension. To illustrate, Figure 1 suggests locations of four tasks at the extremes of the two dimensions. Threading a needle is exacting but simple; in contrast, brain surgery is exacting and complex. Hitting a target from a short distance falls in the lenient-simple corner; learning to drive in an open area is in the lenient-complex domain.

As noted above, decision makers frequently do not know where a task is located in this two-dimensional space. However, because the actual payoff or feedback received by the decision maker is a function of both the validity of his or her knowledge concerning the underlying system (and thus of complexity), and the manner in which payoffs are evaluated (i.e., exactingness), the interpretation of feedback is confounded. Of particular interest is how this confounding interacts with the presence or absence of external incentives.

Insert Figure 1 about here

Effects of external incentives. The experimental evidence reviewed above clearly shows that the provision of incentives increases attention and effort paid to the task. What is not clear, however, is the manner in which increased attention and effort affect performance.

The view taken here is that the effect of incentives on performance is mediated by the decision maker's expectations. Specifically, we draw on the notion and evidence that most people believe that working harder generally leads to greater rewards (Yates & Kulick, 1977). Thus, if the *direct* effect of external incentives is to increase effort, it follows that incentives *indirectly* affect *expectations* of higher levels of performance and their associated rewards.

In summary, we hypothesize that incentives increase expectations of performance and thus the aspired level of rewards, i.e., level of aspiration. However, what happens when aspirations are or are not satisfied by actual performance?

When aspirations are attained, the person has a positive experience in which some level of mastery has been exhibited. This, in turn, can lead to a sense of confidence and the knowledge that a certain set of rules will lead to satisfactory outcomes.

When aspirations are not attained, however, we postulate that people will suffer a sense of frustration which can have different effects, both positive and negative. The positive effect of frustration can be that people exert even greater efforts and, if appropriate, manage to learn from their negative experience. On the other hand, if such efforts fail, the consequence can be an even greater sense of frustration accompanied by less effective learning.

Implications. Consider Figure 1 and ask, first, how performance is liable to vary as a function of the dimensions of complexity and exactingness. Holding other variables constant, we would expect simple tasks to be learned more easily and effectively than complex tasks. However, holding complexity constant, it is not clear that the relation would be as simple for exactingness.

Lenient and exacting environments differ in the coarseness of the feedback they provide to the decision maker. Feedback provided by exacting environments is more sensitive to errors and, in this sense, provides greater opportunities for learning. On the other hand, exacting environments have greater potential for frustrating decision makers if they fail to reach their levels of aspiration. These two contrary forces therefore suggest an inverted-U shaped relation between learning and exactingness whereby performance is greater for tasks that are at intermediate rather than more extreme locations of the exactingness scale.

To consider the effects of incentives, recall that incentives are assumed to increase the decision maker's level of aspiration. In lenient environments, therefore, greater aspirations are likely to lead to better performance because the decision maker will not be frustrated by failure to reach those higher aspirations. In contrast, the effect of greater aspirations in exacting environments is to increase the probability of being frustrated by not reaching those higher aspirations and thus in lower levels of performance.

The differential effect of incentives, however, will also be mediated by the level of complexity of the task. This prediction is made on the grounds that the effects of exactingness are assumed to interact with complexity in the following fashion. In very simple tasks, where optimal responses are learned quickly, aspirations for people both with and without incentives will be virtually identical. In addition, there will be almost no effect of exactingness in the feedback observed precisely because people will receive the same feedback (i.e., when making no errors) irrespective of the exactingness of the environment. In very complex environments, however, even though people with incentives may set higher aspirations than those without, the difficulty of achieving both sets of aspirations is likely to be equally frustrating such that differences in performance will be minimal. In making these arguments about simple and complex tasks, it is important to state that we have defined limiting conditions. The key point is that our predictions concerning the interaction of incentives and the exactingness of the environment is most likely to occur in tasks that are intermediate on the dimension of complexity.

Experimental evidence

The main experimental predictions implied by the above analysis apply to tasks of intermediate complexity. These are that, first, performance is an inverted-U shaped function of exactingness. Second, incentives improve performance in lenient environments but hinder performance in exacting environments. Moreover, it follows that incentives should have little or no effect on performance in environments that are intermediate in exactingness. Finally, we predict that incentives will have little or no effect in simple or complex environments. To examine these predictions, Experiments 1 and 2 consider the effects of incentives in tasks that can be described as simple and of intermediate complexity, respectively.

An important underlying rationale of our work is that feedback confounds information concerning the underlying structure of the decision-making task and level of performance. It therefore follows that learning would be enhanced if subjects could ignore the performance dimension of feedback. This prediction is tested in Experiment 3 where subjects performed the same task as in Experiment 2 but under instructions that emphasized learning as opposed to performance.

Experiment 1

Rationale. The object was to examine the effects of incentives in an environment that could be described as *simple* thereby testing the prediction of no effects of incentives. Whereas this implies the unsatisfactory procedure of conducting an experiment in the expectation of a null result, it serves to establish a base-line against which results from the more complex environment examined in Experiment 2 can be compared.

Subjects. The subjects in this and our other experiments were all recruited in the same manner through advertisements placed around the University of Chicago. They were offered between \$ 5 and \$15 for participating in an experiment on decision making. Their mean age was 21 years and their mean educational level was 2.5 years beyond high school level. In this experiment there were 66 subjects.

Task. In the task, which was individually administered by microcomputer, subjects were told to imagine that they were managing a small business which sold a perishable product on a daily basis. The subject's task was to determine the number of units of the product to be purchased each day (i.e., experimental trial). This decision variable was labeled "quantity" and could take values from 1 to 1000. Subjects were informed that before each decision "you will be shown a weather forecast variable called 'Temperature.'" In fact, demand for the product - - and thus sales - - was a deterministic function of Temperature which was normally distributed with mean of 70 and standard deviation of 7. Although Temperature was a random variable, all subjects saw exactly the same sequence of values across trials. Subjects were told that all units unsold at the end of the day would be lost. They were informed that the object of the game was to maximize a score labeled "evaluation points" which was calculated by their boss to evaluate performance.

Unknown to the subjects, evaluation points were equal to the accounting profits made on each trial (i.e., value of sales less cost of goods ordered) *minus* a penalty that was proportional to the squared difference between the quantity of goods ordered and actual demand. In the lenient condition, the constant of proportionality was small and therefore had little potential effect; in the exacting condition, however, it was large.[1] Additional feedback was provided in the form of values for each decision on variables described as "# sold," " # unsold," and "sales cost." Subjects were permitted to take notes and were also given the ability to scroll back the computer screen and examine data from past decisions. The price per unit at which the product was sold was kept constant throughout the game ("due to government regulation") but subjects were not informed of this figure. Subjects were kept aware of their performance by having their "Average evaluation points to date" continually updated and present on the screen of the microcomputer.

Design and procedure. Each subject was assigned at random to one of four groups created by crossing two levels of two between-subject factors. These were type of decision environment (lenient vs. exacting) and level of incentives (incentives vs. no incentives). Subjects were not given any information about how evaluation points were calculated and thus did not know whether they were in the lenient or exacting condition. Subjects in the no-incentives condition were told "Your goal is to maximize evaluation points, but your pay will not be based on how well you do." Subjects in the incentives condition were told that their goal was to maximize evaluation points and that their pay would depend on how well they did. Subjects were then allowed two practice trials in order to familiarize themselves with the experimental procedures.

Subjects were first led to believe that they would only play 30 trials "in this part" of the

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[1] The actual formula used for each trial was $\pi - \alpha(Q - D)^2$ where π is accounting profit (i.e., sales less cost of goods ordered), Q is the amount (# units) ordered by the subject, and D is demand (# units). The constant α took different values according to whether the situation was lenient ($\alpha = 0.50$) or exacting ($\alpha = 2.00$). This formulation was used so that subjects would be able to observe a relation between accounting profits and evaluation points in cases in which the quantity ordered exactly matched demand.

experimental session. After playing these trials, subjects completed a small questionnaire that quizzed them about their understanding of the game. They were then informed that they were to play a further 30 trials. This second round differed from the first in that subjects who were in the no-incentives group in the first round were now told that their pay would depend on how well they performed. Thus in the second round all subjects were in an incentives condition. Moreover, and contrary to the first round, the amount of the maximum bonus they could achieve ($3) was made explicit.

To summarize, there were two rounds each with 30 trials; half of the subjects faced a lenient environment, the other half an exacting environment. In the first round, half of the subjects were given explicit external incentives to perform well (i.e., the possible bonus), and half were not. In the second round, all subjects could earn the bonus.

Results. In support of our contention that the task was simple (although not trivially so), we note that 2 of the 66 subjects had perfect scores in the first round. (As it happened, an effective hypothesis that could have been formulated after seeing the results of the two practice trials would have led subjects to acquiring the optimal rule for the game). In addition, 7 of the 66 subjects had perfect scores in the second round. Curiously, one of the subjects who had a perfect score in the first round, failed to repeat this performance in the second. We believe that this was due to calculation errors on the part of the subject.

Whereas feedback was in the form of evaluation points which differed depending on whether subjects were in the lenient or exacting environments, from our viewpoint we need to compare performance by a common metric. To do so, we established an accuracy score for each subject which is defined by subtracting the mean absolute deviations of their decisions relative to the optimal response from a constant. At the level of each trial, the absolute deviation was defined as $|Q - D|$ where Q represents the value of the decision variable chosen by the subject, and D is the actual amount of "demand" or the "correct response." We deducted mean absolute deviations from a constant so that greater accuracy scores would indicate better performance. The specific constant of 14,060 was chosen because this represents the average evaluation points associated with no errors.

Table 1 presents the means (and standard deviations) by experimental conditions and rounds for both the accuracy score defined above and evaluation points. As befits a simple task, the mean accuracy scores are large. Indeed, a repeated-measures analysis-of-variance indicates only one statistically significant effect. This is for the difference between Rounds 1 and 2 with means of 13,997 and 14,031, $p = .018$, thereby indicating the effect of learning across rounds.

Turning to evaluation points, the most striking aspect is probably the size of the standard deviations in the exacting as opposed to lenient environment. However, this is consistent with the fact that deviations from appropriate responses were more heavily penalized in the exacting environment. Because this also means that an analysis-of-variance on the raw data is inappropriate, the data were transformed to logarithms before further analysis. For Round 1, this revealed no main effect or interaction involving incentives but an effect for the difference between the lenient and exacting environments, $p = .039$. In Round 2 (where all subjects were in an incentives condition), the only result approaching statistical significance was the exacting-lenient distinction, $p = .062$. Finally, in a repeated-measures analysis-of-variance using rounds as the repeated measure, the only significant effect was also in respect of exactingness, $p = .025$.

Insert Table 1 about here

The bonus pay that subjects either did or would have received (had they been in the incentives condition), provides another measure of performance. This was a truncated variable in that mean evaluation point scores below 11,061 received no bonus and were scored 0. Attributing to no-incentives subjects in Round 1 the bonus they would have earned had they been in the incentives condition, the only significant effect revealed by a repeated-measures analysis-of-variance was in respect of the difference (increase) between Rounds 1 and 2 from $ 0.52 to $ 1.61, $p = .017$.

Data collected on a number of other measures revealed no differences between the four experimental conditions. These included the importance attached to different variables (as measured by responses to the questionnaire administered after Round 1), self-reported feelings of how much control subjects felt they had over the task, and time taken to complete the task, overall means of 13.7 and 10.8 mins in Rounds 1 and 2, respectively. The difference between mean times for the two rounds was significant, p = .04.

Discussion of Experiment 1. In a simple environment we found no effect on accuracy due to either incentives or the exactingness. In fact the only significant performance differences were in respect of evaluation points (which is hardly surprising given the different metrics used in the lenient and exacting conditions), and the improvements from Round 1 to 2 that indicated learning. Whereas, taken by themselves, these findings are not of great importance, together with the specification of the experimental task, they provide a baseline for comparing the results of Experiment 2.

Experiment 2

Rationale. The purpose of Experiment 2 was to test the predictions concerning the effects of exactingness in a task of intermediate complexity. These are that, first, performance has an inverted-U shaped relation with exactingness. In other words, when performance in environments of differing exactingness is converted into a comparable accuracy score, greater accuracy will be observed in situations that are intermediate in exactingness compared to environments that are lenient or exacting. Second, there is an interaction between incentives and exactingness. Incentives will improve performance in lenient environments but impair performance in exacting ones. From this it also follows that for tasks that are intermediate in exactingness, there will be little or no effect of incentives.

Subjects. One hundred and twenty-one subjects recruited in the same manner and from the same population as Experiment 1 participated in this experiment.

Task. To construct a task of intermediate complexity, we maintained the same underlying structure of the simulation used in Experiment 1 but modified several features. First, because inference is more difficult in the absence of cover stories (see, e.g., Sniezek, 1986), we used abstract labels for the variables. Subjects were told that they were to set a value of a "DECISION VARIABLE that can vary between 1 and 1000." Moreover, "At the time you make this decision you will see the value of another variable called W. Your performance in each period of the game will be measured by a variable called EVALUATION POINTS." As part of the feedback, subjects were also told that they would "see the values of 2 other variables that could be useful to you in your decision making. These are called A and B." (These corresponded to the # sold and # unsold variables in Experiment 1. We eliminated feedback corresponding to sales cost). Second, we included a small random disturbance in the model so that subjects would not necessarily observe the same outcomes if they repeated a response to the same W value. This was achieved by modifying the "demand" function so that it was no longer a deterministic function of W although the correlation between W and demand was high, $r = .99$. Third, as detailed below, we used different functions for calculating evaluation points. Finally, we omitted the two practice trials at the beginning of the experiment because, as noted above, the values used could suggest a good hypothesis concerning the nature of the underlying model. (Recall that 2 subjects in Experiment 1 had perfect scores in Round 1).

Design and procedure. Each subject was allocated at random to one of six groups created by crossing two levels of incentives (incentives vs. no incentives) by three types of environment (lenient, intermediate, and exacting) such that there were 20 subjects in each group. (One group had 21 subjects). Subjects in the no-incentives condition were informed, "Your pay for this part of the experiment will not depend on how well you do in the game." In contrast, subjects in the incentives condition were told that their pay would depend on how well they performed. Specifically, it was possible to score a maximum of 500 evaluation points on each trial and pay would depend on the mean evaluation points achieved over 30 trials with one cent for each point above 0. Thus remuneration could vary between \$ 0.00 and \$ 5.00. As in Experiment 1, feedback concerning mean evaluation points earned to date was continually updated and displayed on the screen of the microcomputer used for administering the task for all subjects. We specifically maintained this information on the screen so that subjects would be aware of how well they were doing and whether they were likely to be paid for participating in this part of the experiment (i.e., whether their mean score was above or below 0 which we took to be a "natural" reference point). For example, if subjects in the exacting condition made bad errors, they could easily infer that there was little chance of having a positive mean score by the end of 30 trials.

Evaluation points were calculated by subtracting from 500 (the maximum possible per trial) a penalty that was proportional to the squared difference between the amount of the decision variable selected by the subject and the actual "demand" for the product on that trial. The constants of proportionality differed in the lenient, intermediate, and exacting conditions so that whereas deviations from actual demand were heavily penalized in the exacting condition, this was not the case in the lenient. The intermediate condition was between the lenient and exacting in this respect.[2] As in Experiment 1, subjects were not informed as to how evaluation points were calculated. In addition to the feedback provided by evaluation points and the variables A and B, subjects were allowed to take notes and to examine past data by scrolling back the screen of the microcomputer.

At the outset of the experiment, subjects were told they would make 30 decisions. This was Round 1. After completing this, they were first asked to rank themselves in percentile terms in respect of how well they thought they had performed in the task relative to other University of

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[2] Evaluation points were calculated according to the formula $500 - \alpha(Q-D)^2$ where Q was the amount of the decision variable selected by the subject, D was the actual demand or "correct" amount, and α was the exactingness parameter. The settings were $\alpha = .01$ for the lenient environment, $\alpha = .05$ for the intermediate, and $\alpha = .50$ for the exacting. Note that because each function can be written as a linear function of the others, evaluation points scored by different exactingness parameters are perfectly correlated.

Chicago students. They were then told that they were to play a second series of 30 trials under exactly the same conditions. This was Round 2. Next, subjects were asked to complete a questionnaire that quizzed them about their understanding of the model underlying the task (i.e., relations between variables, and so on). They were then asked to complete a further series of 30 trials for Round 3. For this round, however, subjects who had previously been in the no-incentives condition were required to make their decisions under the same incentives conditions as the other subjects. The question on self-ranking of performance was also repeated after Rounds 2 and 3.

To summarize, the design of the experiment involved two between-subject factors, one with two levels (incentives vs. no incentives), and the other with three (lenient, intermediate, and exacting environments). There were three rounds each involving 30 trials and subjects completed a questionnaire about their understanding of the task after the second round. In the third round, all subjects made their decisions under incentive conditions.

Results. Table 2 and Figures 2, 3, and 4 provide overviews of the results. For all six experimental conditions, Table 2 reports means and standard deviations by rounds in respect of accuracy scores and evaluation points (i.e., the loss functions actually experienced by the subjects). As in Experiment 1, accuracy scores were calculated by subtracting mean absolute deviations from a constant. In this case the constant was 500, the average evaluation points associated with perfect performance. Figure 2 shows the mean accuracy scores achieved by subjects in the three different environments (i.e., lenient, intermediate, and exacting) across the three rounds. Figure 3 displays the overall means of the three rounds for each of the six experimental conditions. The three panels of Figure 4 illustrate graphically the mean accuracy scores achieved by subjects in all six experimental conditions for each of the three rounds.

Insert Table 2 and Figures 2, 3, and 4 about here

The upward sloping lines in Figure 2 indicate that performance improved across rounds, i.e., learning occurred. In addition, and as predicted, subjects in the intermediate environment outperformed those in the lenient and exacting. Figure 3 shows the overall pattern of the effects of incentives in different environments. As predicted, subjects in the lenient/incentives condition outperformed those in the lenient/no-incentives condition, overall mean of 372 vs. 308; moreover, subjects in the exacting/incentives condition were outperformed by those in the exacting/no-incentives condition, overall mean of 325 vs. 337. Disaggregating the same data by rounds, Figure 4 indicates a somewhat different pattern in Round 3 vis-à-vis Rounds 1 and 2. In the latter, incentives have the predicted detrimental effects on performance in the exacting environment, but not in Round 3. In the intermediate condition, there are no effects of incentives in the first two rounds (as predicted) but it appears that no-incentive subjects outperform the others in Round 3.

Conclusions reached by visual inspection were tested by formal statistical analysis using analysis-of-variance for each round as well as a repeated-measures analysis-of-variance treating rounds as the repeated measure. Concentrating on accuracy, we note first that performance improved significantly across rounds with means of 297, 361 and 387, $p < .001$ (from Round 1 to 2) and $p = .014$ (from Round 2 to 3). Second, whereas the overall effect of environment is only significant by the repeated-measures analysis at $p = .095$, using separate analyses by round, the significance levels are .065, .030, and .561 for Rounds 1, 2 and 3, respectively. More pertinent to our hypothesis are direct contrasts between the means of the intermediate condition and the means of the data of both the exacting and lenient environments. These show that the intermediate condition induces better performance in all three rounds, $p = .053$, $p = .002$, and $p = .007$, for Rounds 1, 2, and 3, respectively, one-tailed tests. Third, there is a significant main effect for incentives by the repeated-measures analysis, $p = .008$, as well as effects for each round, $p =$ .006, .008, and .059. Fourth, the predicted environment x incentives interaction shows a similar pattern, a significant effect by the repeated-measures analysis, $p = .028$, and when analyzed separately, effects that are significant at $p = .034$, $p = .034$, and $p = .057$ for Rounds 1, 2, and 3, respectively. Of further interest is the fact that when the intermediate condition is omitted from the analysis, the predicted interaction between incentives and environment for the lenient and exacting conditions is significant by a repeated-measures analysis, $p = .024$. The significance levels associated with the corresponding analyses by round are $p = .018$, $p = .014$, and $p = .147$. Moreover, contrasts for the effects of incentives in the intermediate condition alone reveal no statistically significant effects, $p = .764$, $p = .973$, and $p = .148$ for Rounds 1, 2, and 3, respectively. (The difference between incentive conditions for the intermediate environment is also not significant by a repeated-measures analysis, $p = .490$).

As in Experiment 1, there are large differences, in both means and standard deviations, between the evaluation points actually experienced by subjects in the different environmental conditions. Because the evaluation points in the three environmental conditions are perfectly correlated (see footnote 2), it is also instructive to analyze the data after scoring performance in all experimental conditions by one of the three evaluation functions. We therefore reanalyzed the data using the lenient evaluation function. This yielded results almost identical to our preceding analysis of the accuracy score. Using a repeated-measures analysis-of-variance, there were significant learning effects across rounds, $p < .001$ and $p = .042$; there were overall effects for incentives, $p = .005$, for environment, $p = .068$, and for the incentive x environment interaction, $p = .011$. Doing analyses of variance separately by rounds, the effects for incentives were significant at .002, .011, and .060 for Rounds 1, 2, and 3 respectively. The corresponding figures for environment were .043, .047, and .372, and those for the incentives x environment interaction were .012, .028, and .028.

Parenthetically, we note that, on average, subjects in all experimental conditions were unbiased in that the average error of their decisions was not significantly different from 0 in any of the rounds. This suggests that subjects responded appropriately in their responses to the nature of the symmetric penalty functions implicit in their feedback. Where subjects differed by experimental conditions, was in the size of their errors.

Whereas the above data are important for the outcomes of the experiment, they do not address issues concerning the processes that might have occurred in the different experimental conditions. Other sources of data, however, shed some light on these issues.

One datum collected by the microcomputer was time taken by subjects to complete each round. These averaged 22.4, 14.6, and 13.1 minutes for Rounds 1, 2, and 3, respectively. A repeated-measures analysis-of-variance showed significant differences between rounds, $p < .001$ and $p = .017$, but no significant effects for either incentives or environment and no incentive x environment interaction. On the other hand, accuracy scores were correlated at the individual level with time spent on the task, $r = .21$, .29 and .33, for Rounds 1, 2, and 3, respectively. We therefore reanalyzed our data with time as a covariate. Using accuracy score (and lenient evaluation points) as the dependent variable(s), an appropriate repeated-measures analysis revealed significant main effects for incentives, $p = .003$ ($p = .002$), environment, $p = .013$ ($p = .008$), and the incentives x environment interaction, $p = .026$ ($p = .010$). Analyses by Round 1, 2, and 3, respectively, also revealed significant main effects for incentives, $p = .008$, .007, .017 ($p = .003$, .010, .021), environment, $p = .035$, .026, .243 ($p = .023$, . 035, .143), and the incentives x environment interaction, $p = .041$, .045, .055 ($p = .016$, .040, .027). If anything, using time as a covariate strengthens our substantive conclusions.

Recall that at the end of each round subjects were asked to rank their performance in percentile terms vis-à-vis other University of Chicago students. Overall, the means rankings were at the 45.1, 54.9, and 57.6 percentiles for Rounds 1, 2, and 3, respectively. A repeated-measures analysis-of-variance showed the difference between Rounds 1 and 2 to be significant, $p = .009$, but no other significant effects. At the individual level, it was of interest to note that whereas there was essentially no relation between self-assessed rank and performance (i.e., evaluation points) for Round 1, $r = .10$, this was not the case for Rounds 2 and 3 where the analogous correlations were .42 and .48, respectively. Experience with the task did help subjects assess their own performance more accurately in relative terms.

The questionnaire completed after Round 2 contained two kinds of questions. The first were direct questions concerning which variables subjects deemed most important as well as whether they thought that "the outcomes of the game (i.e., evaluation points) are determined according to some systematic set of rules." In respect of the latter, there was an interesting effect for environment. Subjects in the intermediate condition (who performed best) rated outcomes as being determined by a more systematic set of rules than subjects in the other conditions, mean of 5.42 on a 7-point scale vs. 4.40 for lenient and 3.88 for exacting, $p = .014$.

To assess the relative importance of the variables, subjects were asked to indicate the order in which they would delete the variables W, A, B, and EVALUATION POINTS if they were forced to make decisions without them. A "1" would mean "delete this variable first," a "2" meaning "delete this variable next," and so on to "4" meaning "delete this variable last." Thus the most important variable was ranked 4, the least important 1. Assessing overall perceived importance by averaging these rankings, W and EVALUATION POINTS were seen as equally and most important with means of 3.30 and 3.32, respectively. Interestingly, there were some marginal differences by experimental conditions in how W was viewed in a manner that partially mimicked performance. W was perceived to be more important in the incentives as opposed to the no-incentives groups, 3.38 vs. 3.22, $p = .063$. In addition, the variable B ("unsold goods"), which was far less critical for understanding the task, was seen to be more important in the exacting environment, 1.70 vs. 1.43 (for lenient) and 1.44 (for intermediate), $p = .059$. These data are significant in that they suggest that in conditions in which subjects were relatively more successful, greater attention was paid to the more important variables.

This suggestion is borne out by considering answers to the open-ended questions. Subjects were asked to write "How does the game work?" by specifying the roles played by the different variables and their interrelations, and while imagining having "to explain to an agent how to play the game in your behalf," to give "a simple description of the system to convey a general sense of how it works" as well as "any specific tips you might have to achieve high evaluation points." The answers to these questions were graded like an examination using a preestablished checklist of criteria. Of particular interest was whether subjects articulated both the direction and strength of the critical relation between W and the decision variable. To simplify matters, we scored each subject's questionnaire by a 0/1 variable if they explicitly mentioned the appropriate direction, and similarly for strength. We also gave each subject a total understanding score which, in addition to the scores for direction and strength, took into account their understanding that there were two types of error (i.e., setting the decision variable too high as well as too low), recognizing an identity between the decision variable and the sum of A and B, and whether they gave any valid tips to an "agent." All these variables were also scored 0/1, and the total score was calculated by summing the scores of the components (Einhorn & Hogarth, 1975). The ratings of the questionnaires were made independently by two of the authors and their judgments averaged. As an indication of reliability, we note that the judges agreed 92% of times for both the direction and strength indices. The correlation between the scores of the two judges on the total index was .85.

The two panels of Figure 5 plot mean scores on the direction and strength indices by the six experimental conditions. Analyses of variance on these data show main effects for incentives for both direction, $p = .027$, and strength, $p = .016$. In addition, there is a significant main effect for environment for the strength variable, $p = .009$, as well as an environment x incentive interaction, $p = .025$. To interpret these data, both the direction and strength of the critical relation were more clearly articulated by subjects in the incentives condition and there was also a difference for the strength variable by environment where subjects in the intermediate condition expressed greatest understanding. Moreover, understanding the strength of the relation exactly mirrored performance as evidenced by the significant interaction for this variable. This can be seen by noting the similarity between the patterns of data exhibited in Figures 3 and 5b.

Insert Figure 5 about here

At the individual subject level, the indices are also related to performance. Across all subjects, the correlations between performance (accuracy score) and the direction index in Rounds

1, 2, and 3 are .38, .48, and .45, respectively, with the corresponding figures for the strength index being .31, .50, and .46. In addition, the correlations between performance across rounds and an index of total knowledge (including both direction and strength as components) are .43, .59, and .62. In order to obtain greater insight into the process, we gave 41 subjects (10 in the lenient, 21 in the intermediate, and 10 in the exacting environments) more extensive debriefing interviews in which they were encouraged to describe their thoughts and feelings about the experiment. In one question, subjects were asked to "describe your experience of playing the game by a few adjectives or short phrases." Because of the importance of frustration to our theorizing, we made a simple count of the number of times subjects included words involving "frustration" in their adjectives or phrases. Without any claim to statistical significance, it was interesting to note that the relative frequency of reference to frustration varied by environment. Whereas this was mentioned by 4 out of 10 subjects in the lenient environment, and 3 out of 21 in the intermediate, there were 7 out of 10 mentions in the exacting environment.

Finally, we found no significant differences when we analyzed results by demographic variables (e.g., age, gender, mathematics and science vs. non-mathematics and science background, etc.).

Discussion of Experiment 2. To summarize, Experiment 2 validates the major predictions of our theoretical framework concerning tasks of intermediate complexity. First, performance was seen to have an inverted-U shaped relation with exactingness, i.e., performance was better in the intermediate as opposed to lenient or exacting environments. This lends credence to the notion that there are both positive and negative aspects of learning under conditions where errors are heavily penalized. The positive is that exacting feedback is more informative than lenient; the negative is that exacting feedback can be more disruptive and lead to greater frustration.

Second, there was an interaction between incentives and exactingness. In lenient environments, incentives improved performance; in exacting environments, incentives impaired performance; and in an environment characterized as intermediate in exactingness, incentives had no effect.

In addition to performance, recall that we also collected data on how well subjects understood the experimental task. These were shown to mirror performance. Not only did mean scores on the direction and strength indices (indicating how well subjects articulated their understanding of the key predictive relation) match relative performance in the different experimental groups, but relatively high correlations existed between these variables and measures of performance (i.e., evaluation points) at the individual level. These are important findings. First, they indicate that performance was accompanied by an accurate awareness of the key predictive relation. This contrasts with results of Broadbent and Aston (1978) and Broadbent (1977), who found no relation between the ability to verbalize understanding of relations between variables learned through taking decisions and performance. However, the tasks explored by these investigators (an economic simulation game and a simpler laboratory task) differed from ours on many dimensions such that it is difficult to state with confidence any hypothesis for the contrasting results. Second, the fact that performance and the ability to articulate the underlying rationale were significantly correlated mitigates the possibility that results of our experiment were due to chance.

One of the main theoretical motivations for our research is the notion that feedback is confounded by the twin tasks of inferring the underlying structure of the task and assessing one's level of performance. It therefore follows that if one of these sources of confusion were removed, performance should improve. This notion was subjected to the following experimental test.

Experiment 3

Rationale. The objective was to test how well subjects would score over 30 trials in Round 3 under incentive conditions if they were allowed to learn the task *without cost* over two preceding rounds of 30 trials each.

Subjects. There were 41 subjects from the same population as Experiments 1 and 2 who were recruited in the same manner.

Task. This was the same as Experiment 2 with two exceptions. First, instructions differed in that subjects were told "The object of this game is to maximize EVALUATION POINTS. However, in playing the game you should not be concerned with how well you do. Instead, your objective is to learn how the game works." In addition, half of the subjects were specifically told to expect to be asked how the game worked and to make their understanding explicit. Second, after Rounds 2 and 3 subjects did not rank their own performance. Instead they ranked how well they thought they had understood the task relative to other University of Chicago students.

Design and procedure. Subjects were allocated at random to four groups created by crossing two levels of two between-subjects conditions. These were level of instructions (specifically told to expect to have to explain their understanding of the game vs. not explicitly told) and exactingness of the task environment (lenient vs. exacting using the same parameters as Experiment 2). Apart from the differences in the task noted above, procedures were exactly the same as in Experiment 2. In summary, subjects had two rounds of 30 trials in which their task was to discover how the system worked; they then completed the same questionnaire used in Experiment 2 prior to being switched to the same incentives condition experienced by subjects in that experiment.

Results. We first note that there were no significant main effects or interactions involving the difference in the levels of the instructions given to the subjects concerning whether they would be asked later on to explain their understanding of the game. We therefore ignore this experimental manipulation.

We contrast results with subjects in the comparable lenient and exacting environments in Experiment 2. One important finding was that subjects in Experiment 3 took, on average, 55% longer than their counterparts to complete the experimental tasks. Mean times were 33.2 vs. 22.0 mins in Round 1, 23.6 vs. 13.4 in Round 2, and 18.0 vs. 12.8 in Round 3. All differences are statistically significant, $p = .017$, $p < .001$, and $p = .016$, respectively. This result is particularly interesting because subjects in both experiments were given the same expectations concerning remuneration for participating in the experiment and had identical incentives in Round 3. In addition, neither group was told how long to spend on the experimental tasks. Apparently giving subjects a set to learn induced a more careful approach (as evidenced by time spent) that also carried over to the incentives condition in Round 3.

As in Experiment 2, there was also a relation between how well subjects thought they had performed in the task after Round 3 and actual performance in evaluation points, $r = .51$.

Table 3 summarizes data on mean accuracy scores for Round 3 (where all subjects were in an incentives condition) as well as indices of understanding in respect of direction and strength of the important predictive relation determining outcomes based on the questionnaire completed at the end of Round 2. For mean accuracy score, an appropriate analysis-of-variance reveals no significant main effects nor interactions.

Contrasts between conditions reveal, however, that although performance of subjects in Experiment 3, the "inference" group, was better than the no-incentives group, $t = 1.96$, $p = .053$, there was no difference between the incentives and inference groups, $t = .619$, $p = .538$. In other words, averaging across both lenient and exacting environments, subjects who learned under incentives in Rounds 1 and 2 performed as well in Round 3 as the inference subjects who had been given a set to learn despite the fact that the latter took much longer over the task. The inference subjects did, however, perform better than subjects who played the first two rounds under a no-incentives condition.

Insert Table 3 about here

The results on performance are mirrored by the understanding of the task expressed by the subjects at the end of Round 2. As shown in Table 3, subjects in the inference condition had a better understanding of both the direction and strength indices than the others with means of 0.61 vs. 0.50 and 0.33, for the former, and 0.56 vs. 0.30 and 0.23, for the latter. Analyses-of-variance showed that main effects of condition were statistically significant for both the direction and strength indices, p = .030 and p = .004, respectively. There were also main effects for lenient vs. exacting environments, p = .055 for direction and p = .006 for strength, and a significant incentives x environment interaction for strength, p = .028.

Further comparisons can also be made between subjects in the intermediate condition in Experiment 2 and the subjects in Experiment 3. The mean accuracy score in Round 3 of intermediate subjects did not differ significantly from the inference subjects, 415 versus 397, t = .979, p = .331. In addition, there were no significant differences between the scores both groups achieved on the direction and strength indices. In short, there were no significant differences in either performance or understanding between the inference subjects, averaging over lenient and exacting environments, and subjects in an environment of intermediate exactingness, averaging over conditions of incentives and no-incentives. In other words, there are different paths to the same levels of performance and understanding.

Finally, correlations between individual scores on the understanding indices and performance were also high for the inference group. These were for Rounds 1, 2, and 3, respectively, .24, .65, and .60 for direction; .36, .60, and .55 for strength; and .30, .75, and .67 for the index of total knowledge.

Discussion of Experiment 3

Contrasting the results of Experiment 3 (inference) with those of Experiment 2 (incentives and no-incentives), performance in Round 3 (in which all subjects were in an incentives condition) is seen to reflect subjects' prior exposure to the decision-making task. The inference subjects outperform those in the no-incentives condition but do no better on average than the incentives condition. On the other hand, the data suggest the possibility that this latter conclusion might be mediated by the exactingness of the environment because whereas inference is better in the exacting environment (mean accuracy score of 405 versus 372), it is essentially the same in the lenient (389 versus 399). Of additional interest is the fact that subjects in the inference condition were more capable of articulating an accurate understanding of the task than their counterparts in Experiment 2. A difficulty in interpreting these data, however, is that subjects in Experiment 3 took on average 55% longer to complete the tasks. Thus any gains in performance should be measured against the additional cost in time.

Whereas subjects in Rounds 1 and 2 of Experiment 3 were instructed to learn the game and thus ignore the evaluative dimension of feedback, it is unclear whether people could ignore the evaluative implications of any feedback. That this may have happened is supported by two pieces of evidence. First, if exacting feedback has greater potential for learning, one would expect subjects in the exacting condition to have learned more effectively in the absence of evaluation. However, performance in Round 3 between inference subjects in the lenient and exacting environments did not differ significantly (389 vs. 405). Second, whereas from our viewpoint scoring performance of the inference subjects lacks meaning for Rounds 1 and 2, these subjects *still observed the evaluation points they* would have achieved. Moreover, their mean accuracy "scores" were comparable to those of subjects in Experiment 2 (283 vs. 288 and 359 vs. 344) thereby suggesting that they were sensitive to the level of evaluation points.

General discussion

The present studies show that, in tasks of intermediate complexity, feedback scored by lenient or exacting evaluation functions is less effective in promoting learning than feedback scored by an intermediate evaluation function. Moreover, the type of evaluation function interacts with incentives. Incentives foster learning in lenient environments but hinder learning in exacting environments. In intermediate environments, incentives have no differential effect on learning. These conclusions refer to performance on the decision-making task but are also mirrored by the ability to articulate the key relation in the task studied. We further demonstrated the relative effectiveness of giving people a "set" to learn rather than perform but noted that this led to spending 55% more time on the task. Finally, we showed that in a simple task (Experiment 1), there were no differential effects of incentives. At the very least, the studies demonstrate the complex nature of the relation between incentives and performance. We now discuss these results from both theoretical and practical perspectives. We also suggest topics for further study.

Theory. The theoretical contributions of our work involved assumptions concerning the nature of feedback in decision-making tasks and the manner in which incentives affect individuals engaged in such tasks.

The observation that feedback may not only be ambiguous with respect to inferring the structure of decision-making tasks but is confounded with information concerning performance, led to characterizing decision-making tasks on the twin dimensions of complexity and exactingness. Whereas many other studies have considered effects of feedback on learning, we believe that our studies are unique in investigating the effects of exactingness or the manner in which decisions are evaluated within the *same* decision-making task, i.e, holding inferential complexity constant. For example, unrelated to issues of evaluating performance, the fact that outcome feedback may not be effective in learning has been noted by many investigators. Balzer, Doherty, and O'Connor (in press) review several studies demonstrating that feedback that emphasizes the nature of relations in the environment (so-called cognitive feedback) is more effective in teaching people to learn than outcomes alone. Einhorn and Hogarth (1978) showed that when feedback is incomplete, it may mislead people into believing that they understand relations when in fact they don't. This is particularly likely to be the case when actions are taken that preclude the observation of outcomes associated with the action not taken. For example, in many hiring decisions in industry, firms learn about the effectiveness of the employees they hire, but nothing about the subsequent job performance of those they don't hire. To improve learning, firms would have to experiment by hiring employees whom they judged to be unqualified and then observe their performance. Such experimentation, however, implies short-term costs and it is not clear that people are willing to make these investments (see also Brehmer, 1980; Einhorn, 1980; Schwartz, 1982). In our studies, we were struck by the fact that subjects in both the no-incentives (of Experiment 2) and inference (Experiment 3) conditions did not seem willing to trade-off learning and performance at a level different from those operating under incentives.

In an unusually thorough investigation of the process of learning, Klayman (1988) demonstrated the importance of experimentation by forcing one group of subjects to learn only through observation while allowing a second group to experiment. In subsequent performance on a prediction task, the second group was more accurate. One intriguing issue raised by this and other studies is to specify what cues encourage people to adopt a more experimental approach to learning.

Concerning the role of incentives on performance, our analysis differs from traditional accounts (for a review, see McCullers, 1978) by assuming that the relation between these two variables is *indirect* rather than direct. In our model, increases in incentives are only assumed to increase effort and attention paid to the task. Increases in effort and attention, however, are assumed to increase expectations of performance or levels of aspiration, i.e., working harder leads to expecting to do better. Actual performance, however, may or may not satisfy these new aspirations. Because higher aspirations are more likely to be satisfied in lenient environments, this leads to better performance; however, failure to reach aspirations in exacting environments leads to a heightened sense of frustration and lower levels of performance. Note that this does not mean that people will fail to learn under incentives in exacting environments. In fact, our subjects showed marked signs of learning across the three rounds of experimental trials. But it does mean that learning in exacting environments in the presence of incentives will tend to be slower and less effective.

Whereas our postulated account of the effects of incentives can account for our data, other forces may produce similar or complementary effects. In a comprehensive review, Easterbrook (1959) summarized much literature showing that in high drive states (of motivation) attention is narrowly focussed such that people consider only a limited range of cues. In lenient environments, therefore, where failure to make optimal responses involves smaller ranges of penalties than in exacting environments, incentives might induce a good match between the range of cues people are motivated to observe and the empirical ranges that are actually observable. In exacting environments, however, incentives would accentuate mismatching of ranges. Further experimental work is needed to examine this hypothesis and to contrast it with the account based on the effects of changes in levels of aspiration.

In reviewing many studies in which incentives were seen to have both positive and negative effects on performance, McGraw (1978) noted that whereas incentives would seem to be detrimental in tasks that people find "attractive" (in the sense of interesting to the subjects) and that require heuristic, problem-solving mental strategies, they help performance in tasks that people either find "aversive" (in the sense of uninteresting) or requiring practiced, algorithmic procedures such as lever pressing or remaining vigilant. This classification does not fit our findings. Whereas our task does require higher-order mental processes, it is unclear how to classify tasks varying on exactingness as to whether people find them "attractive" or "aversive" within McGraw's definition. More work is needed to find a larger conceptual framework in which to fit the findings reviewed by McGraw with our own.

Practice. Our results raise many practical issues concerning the conditions under which one would or would not wish to provide incentives to foster learning in decision-making tasks. First, however, it is appropriate to consider the limitations of our experimental paradigm and thus the extent to which the findings might be expected to generalize to a wider range of situations. In many ways, our experiments provided almost ideal opportunities for learning compared to more realistic settings. Feedback following decisions was immediate. Subjects could take notes and consult their histories of past decisions. The task did not involve a large number of variables and there was a limited number of relations between variables in the system that were important. Moreover, the system generating observations did not change across time. There are many real-world tasks that exhibit similar characteristics, for example, production and inventory scheduling decisions, predictions of economic and financial indicators, and weather forecasts. Where these tasks may differ from ours, however, is that people would typically not make so many decisions in such a short period of time (experiments tend to collapse experience in terms of time). In one sense, real-world tasks may also be more inferentially complex than ours; on the other hand, this complexity may be offset by having more time to think through issues prior to making decisions. On the other hand, there are other real-world tasks that are similar to ours and where people do experience much feedback within fairly short periods of time. These include learning to handle mechanical or electronic devices, for example, word-processing systems, where people make frequent decisions and see almost immediate feedback.

Two important dimensions of real-world tasks are whether people are aware of the exactingness of the environment and whether they or others have the ability to control or manipulate it. In many situations, where outcomes and rewards are the same (as in financial transactions), people are typically ignorant of the effects of exactingness. Thus, incentives may or may not promote effective learning. In this case, it would be advisable to learn to make decisions within an "inference" set (as in Experiment 3) before having to deal with real payoffs. On the other hand, in situations where it is possible to control how decisions are evaluated (as in our experiments), this may be used deliberately in training decision makers. The implications from our results are clear. Intermediate environments induce more effective learning than lenient or exacting ones and incentives make little difference. If one is forced into using a lenient evaluation function, however, use incentives; with an exacting function, don't use incentives. Finally, in our task subjects were not told *how* they were evaluated, i.e., how decisions and outcomes were translated into evaluation points. An argument could be made that learning would be fostered if people were aware of the exact nature of the evaluation function because this would reduce one source of ambiguity in feedback. On the other hand, because different evaluation functions induce different rates of learning, it is not clear that it would always be advantageous to reveal these functions to learners.

Issues for further study. Because, to the best of our belief, the effects of exactingness and incentives have not previously been studied together, the present research suggests many issues for further investigation. We mention a few.

First, the evaluation functions used in the tasks of intermediate complexity in Experiments 2 and 3 were symmetric. Subjects received the same penalty if they over- or undershot the appropriate setting of the decision variable. It would also be interesting to investigate different types of *asymmetric* evaluation functions. In particular, with highly skewed functions subjects would experience large variations in penalties which might be similar in effect to exacting environments with symmetric functions. However, they would probably also learn to adjust responses to avoid the larger penalties. Whether such learning would take place soon enough to avoid the effects of frustration observed in the exacting conditions in our experiments remains an open question. In our work, we adopted a simple mechanism to model exactingness in the environment. It is possible this could be achieved in other ways.

Second, the present work employed an incentives and no-incentives condition without recognizing the fact that there could be different levels of incentives. Whereas the level of incentives used was sufficient to induce effects, we have no information concerning the relation between size of incentives and effects. We suspect that in a laboratory task relatively small differences in real money paid to subjects do have motivational effects (see also Edwards, 1956; Arkes et al., 1986; Hogarth & Einhorn, 1989) but are uncertain how this might generalize outside the psychological laboratory.

Third, a related question centers on whether the effects we observed are unique to incentives per se or whether other variables which demand that greater attention and effort be paid to tasks induce similar outcomes. Two interesting variables are threat or stress and time pressure. Both can require that greater attention and effort be paid to the task. However, they could also lead to reducing levels of aspiration (cf. Mano, in press) such that one could obtain quite different effects of learning in lenient and exacting environments than observed in our studies.

Fourth, our characterization of task variables in terms of only two dimensions, complexity and exactingness, is necessarily incomplete. Recently, Hammond et al. (1987) have elaborated a theory of how characteristics of tasks map into different modes of cognition that vary on a continuum from analysis to intuition. Hammond et al. would classify our task as "analysis-inducing" such that it would best be handled by an analytical mode of cognition. It is an open and interesting issue as to whether our theoretical framework and results would also apply in tasks that could be defined as "intuition-inducing."

Fifth, we noted above that by informing people of the nature of evaluation functions one should, in principle, reduce the ambiguity of outcome feedback. However, because feedback still implies an evaluation, it is not clear that people are able to separate the informational content of feedback concerning the inferential structure of the task from its evaluative component. This suggests conducting studies similar to those reported above where the nature of the evaluation function is made explicit to the subjects. The question asked is whether it is necessarily better to inform people how they are being evaluated.

Sixth, a central premise of this work is that feedback is ambiguous. Given this ambiguity, it is legitimate to ask whether people might learn more effectively if they received less rather than more information about the effectiveness of past decisions. For example, instead of providing feedback for each decision, would subjects perform better by the end of the experimental session if they only received feedback in the form of average statistics over small blocks of trials? Advantages are that subjects might be forced to experiment with particular strategies over specific blocks of trials and the effects of random error would be mitigated by the averaging process.

To conclude, we have demonstrated that small changes in the parameter of the function that evaluates outcomes of decisions can induce significant changes in performance as well as reverse the sign of the effects of incentives. Such sensitivity to a *single* task feature merits more detailed attention.

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Table 1

Experiment 1: Accuracy scores and evaluation points

Incentive condition:	Incentives		No incentives*	
Environment:	Lenient	Exacting	Lenient	Exacting
Accuracy scores				
Round 1				
Mean	14,010	13,996	13,985	13,996
Standard deviation	39	69	78	56
Round 2*				
Mean	14,034	14,026	14,032	14,030
Standard deviation	27	50	30	48
Evaluation points				
Round 1				
Mean	8,066	-17,219	2,257	-23,791
Standard deviation	5,917	51,503	18,963	50,046
Round 2*				
Mean	11,751	1,164	11,481	3,243
Standard deviation	2,760	26,587	3,938	24,071

* All subjects in Round 2 were in the same incentives condition.

Table 2

Experiment 2: Accuracy scores and evaluation points

Incentive condition:	Incentives			No incentives*		
Environment:	Lenient	Intermediate	Exacting	Lenient	Intermediate	Exacting
Accuracy scores						
Round 1						
Mean	331	309	274	263	317	287
Standard deviation	73	89	77	77	67	71
Round 2						
Mean	386	393	328	314	394	351
Standard deviation	74	98	93	84	66	87
Round 3*						
Mean	399	395	372	347	436	372
Standard deviation	59	115	98	95	48	86
Evaluations points						
Round 1						
Mean	-10	-2,812	-40,569	-390	-2,455	-36,809
Standard deviation	362	1,977	18,853	451	1,747	18,907
Round 2						
Mean	230	-1,098	-27,065	-80	-787	-21,537
Standard deviation	267	2,137	21,719	427	1,340	20,393
Round 3*						
Mean	295	-1,125	-17,674	72	5	-17,105
Standard deviation	177	2,669	20,891	413	727	18,651

* All subjects in Round 3 were in the same incentives condition.

Table 3

Selected results from Experiment 3 contrasted with those from Experiment 2

	Experiment 3	Experiment 2		
Conditions:	Inference	Incentives	No-incentives	(Mean)
Mean accuracy scores for Round 3				
Lenient environment	389	399	347	(378)
Exacting environment	405	372	372	(383)
(Mean)	(397)	(385)	(359)	
Indices of understanding				
Direction				
Lenient environment	0.67	0.65	0.30	(0.54)
Exacting environment	0.55	0.35	0.35	(0.42)
(Mean)	(0.61)	(0.50)	(0.33)	
Strength				
Lenient environment	0.62	0.50	0.15	(0.42)
Exacting environment	0.50	0.10	0.30	(0.30)
(Mean)	(0.56)	(0.30)	(0.23)	

Figure captions

Figure 1: Space of critical task characteristics: Complexity and exactingness.

Figure 2: Experiment 2. Mean accuracy scores by types of environment (lenient, intermediate, and exacting) across rounds.

Figure 3: Overall mean accuracy scores by experimental conditions.

Figure 4: Experiment 2. Mean accuracy scores by experimental conditions for each of the three rounds.

Figure 5: Experiment 2. Analyses of mean direction and strength indices by experimental conditions.

Figure 1

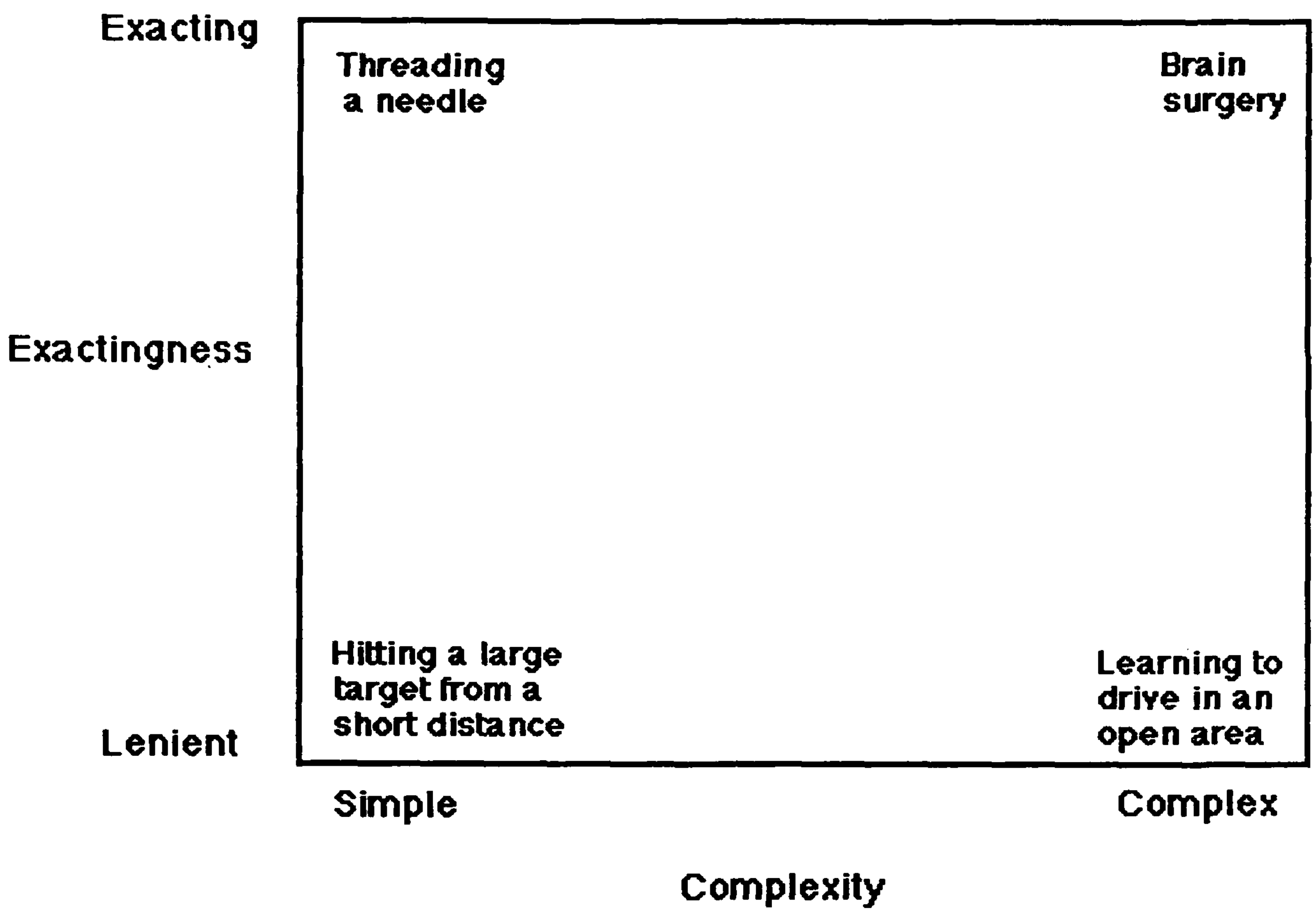

Exacting
Exactingness
Lenient
Threading a needle
Brain surgery
Hitting a large target from a short distance
Learning to drive in an open area
Simple
Complex
Complexity

Figure 2

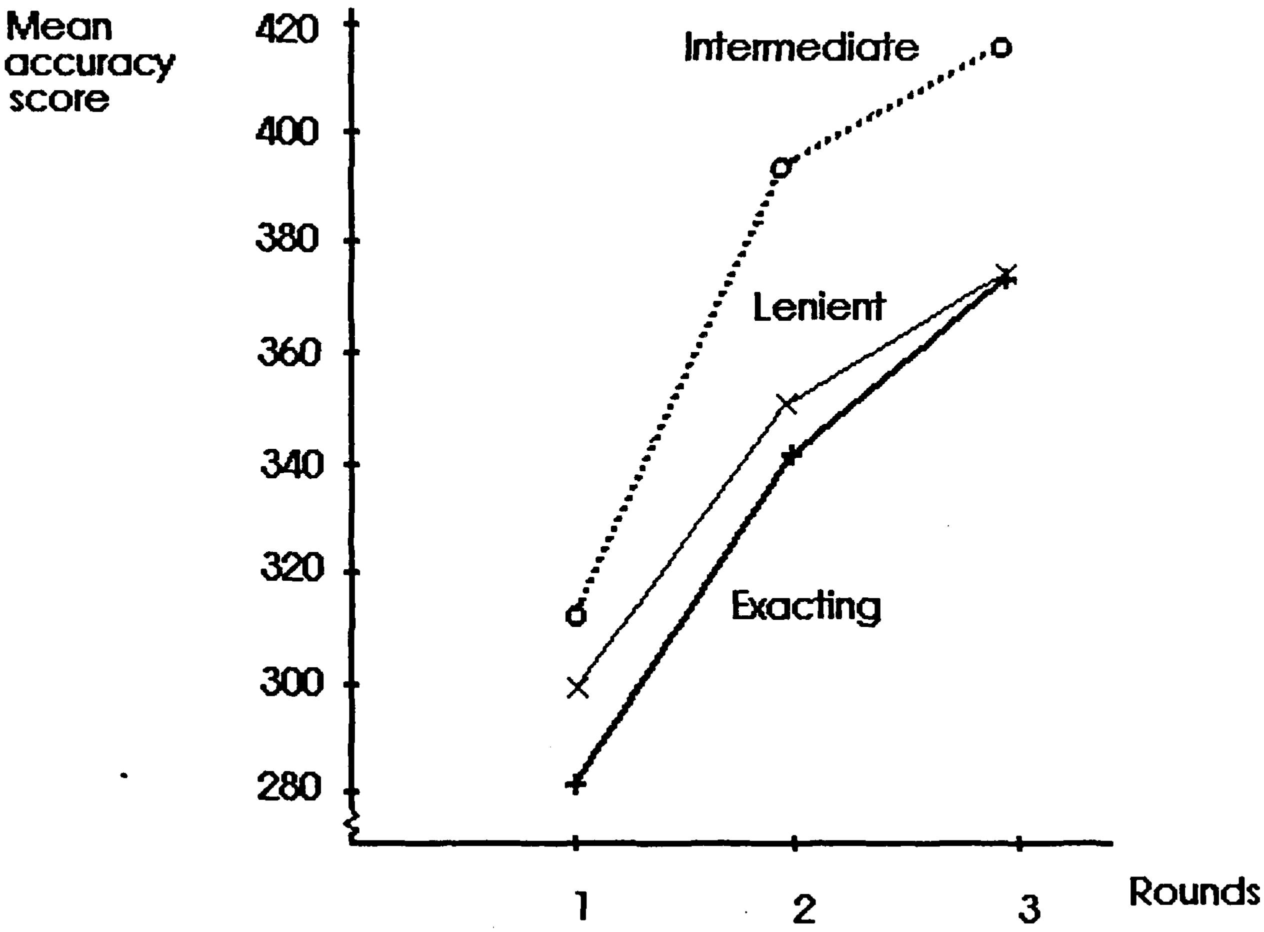
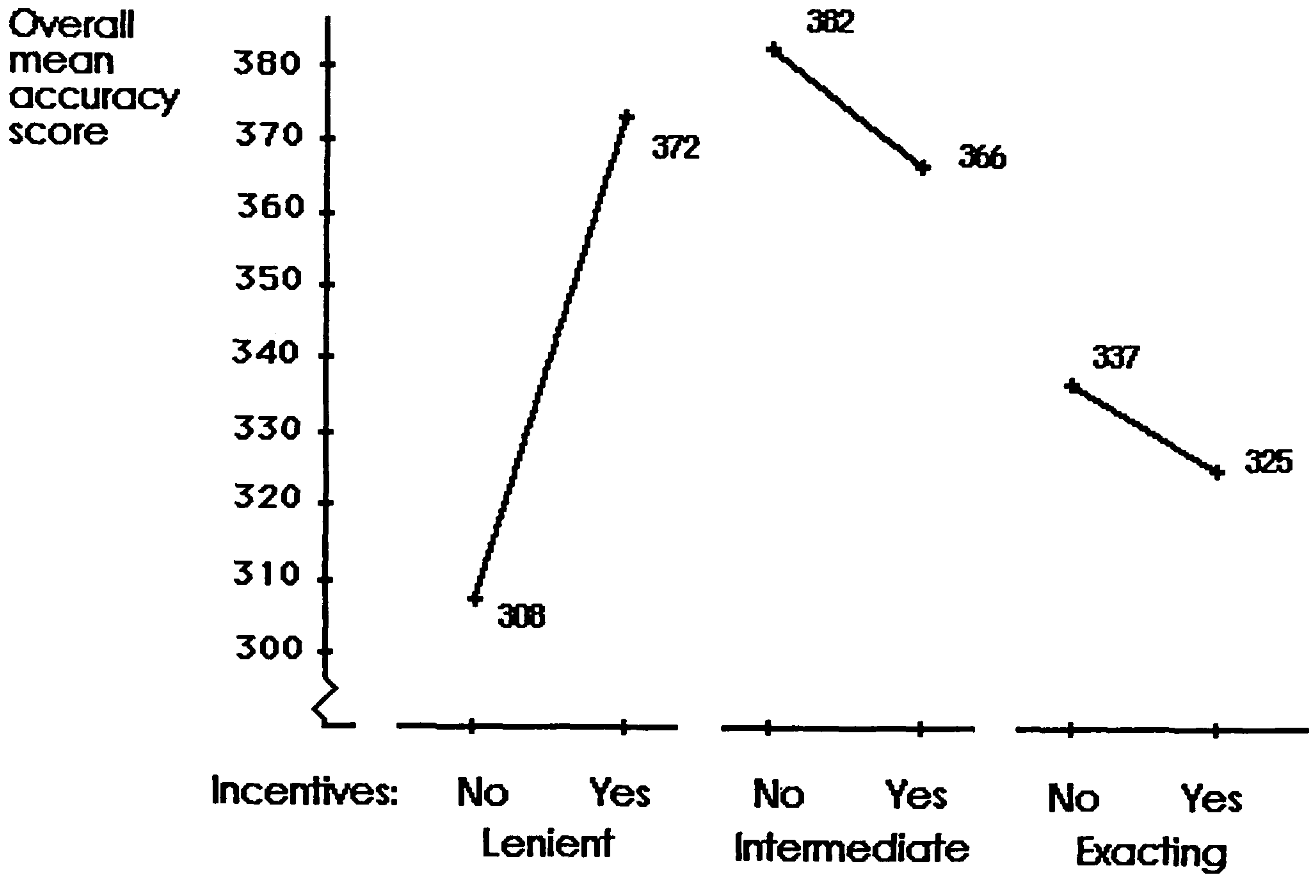
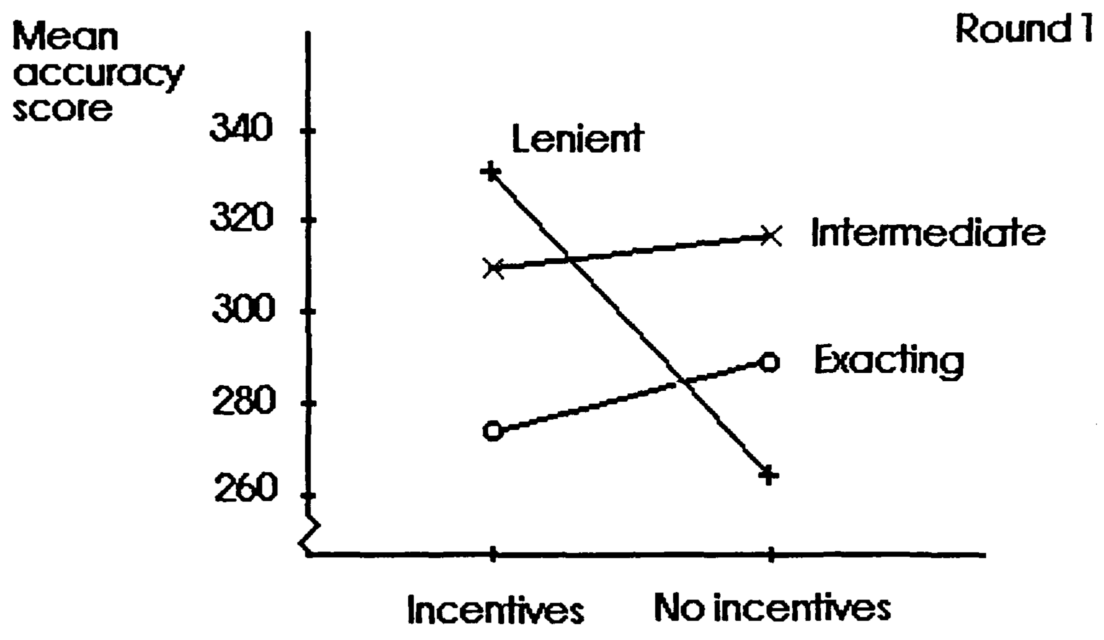

Mean accuracy score
420
400
380
360
340
320
300
280
Intermediate
Lenient
Exacting
1
2
3
Rounds

Figure 3



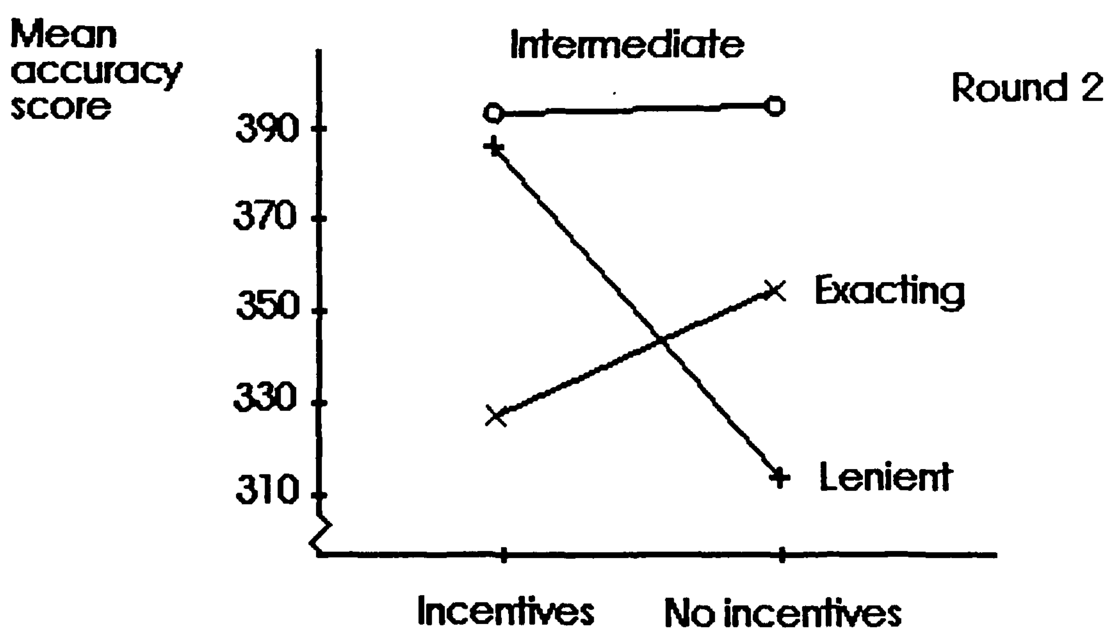
Overall mean accuracy score
380
370
360
350
340
330
320
310
300
382
372
366
337
325
308
Incentives:
No
Yes
No
Yes
No
Yes
Lenient
Intermediate
Exacting

Figure 4



Mean accuracy score
Round 1
340
320
300
280
260
Lenient
Intermediate
Exacting
Incentives
No incentives

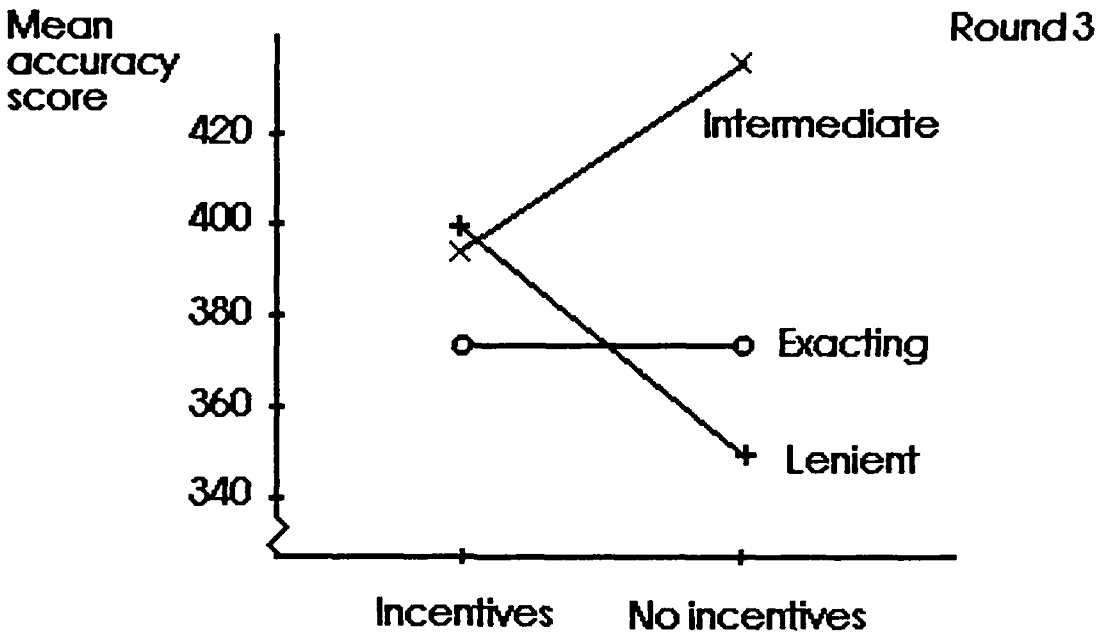
(a)



Mean accuracy score
Intermediate
Round 2
390
370
350
330
310
Exacting
Lenient
Incentives
No incentives

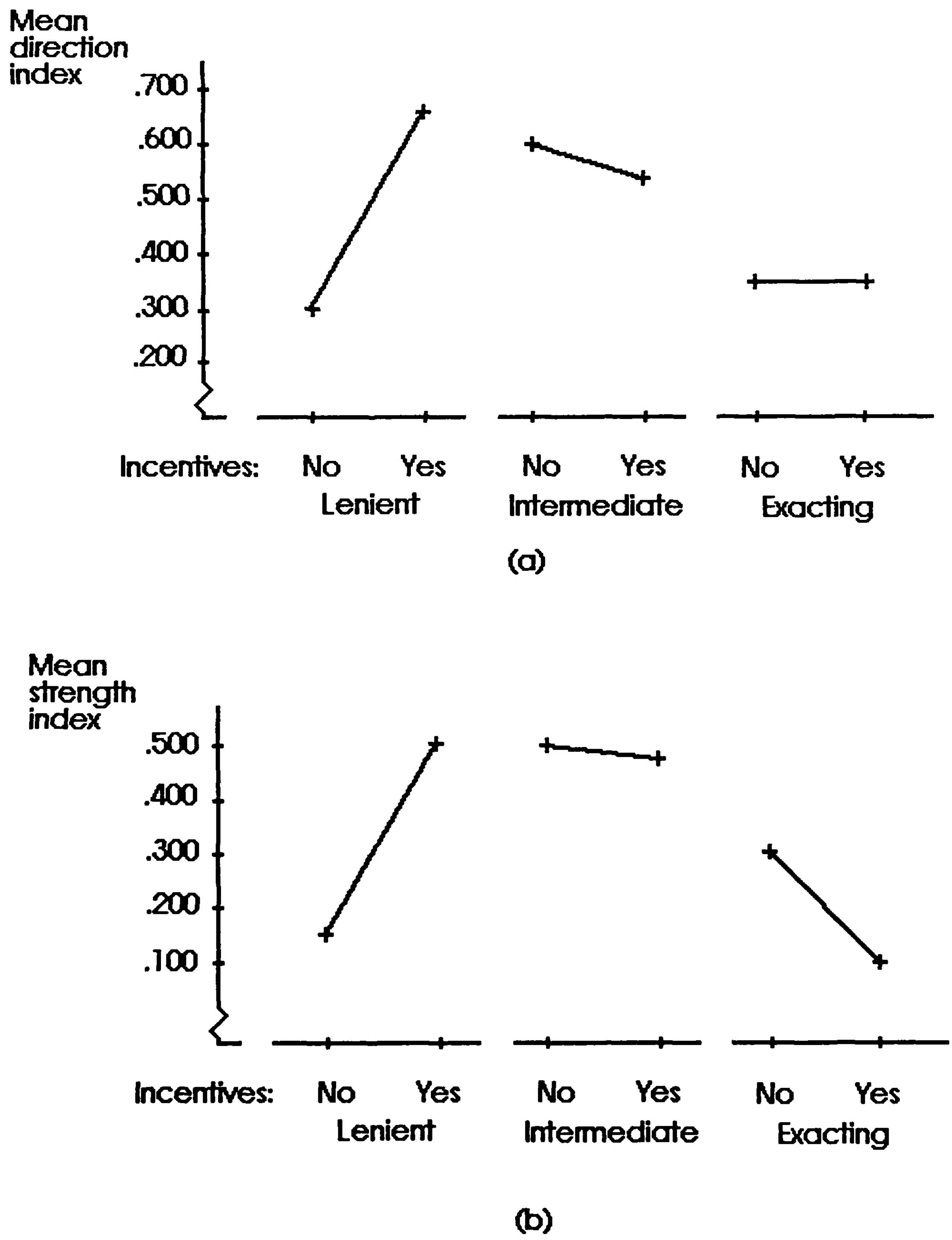
(b)

Figure 4 (cont'd)



(c)

Figure 5


Mean direction index
.700
.600
.500
.400
.300
.200
Incentives: No Yes No Yes No Yes
Lenient
Intermediate
Exacting
(a)
Mean strength index
.500
.400
.300
.200
.100
Incentives: No Yes No Yes No Yes
Lenient
Intermediate
Exacting
(b)

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