An Experiment on the Relative Effects of Ability, Temperament and Luck on Search with Uncertainty

W. Bentley MacLeod and Mark Pingle

USC Olin Research Paper No. 00-12

USC
THE LAW
SCHOOL

RESEARCH PAPER SERIES

Sponsored by the John M. Olin Foundation

University of Southern California Law School
Los Angeles, CA 90089-0071

An Experiment on the Relative Effects of Ability, Temperament and Luck on Search with Uncertainty*

W. Bentley MacLeod
Department of Economics and the Law School
University of Southern California
Los Angeles, CA 90089-0253

Mark Pingle
Department of Economics
University of Nevada
Reno NV

July 2000

Abstract

The problem of decision making in the face of uncertainty is a ubiquitous problem in day to day economic decision making. Psychologists have found that a number of factors can influence the quality of such decision making, including ability, temperament and of course sheer luck. We report the results obtained from an experimental framework that begins an evaluation of the relative importance of these factors in a simple search problem where the complexity of search is the major treatment variable. We find that variations in complexity and "luck" explain most of the variation in performance. However, individual heterogeneity also explains a significant portion. Individual differences matter most when the problem is of moderate complexity. A small portion of the heterogeneity is attributable to ability, but a more significant portion is attributable to variations in what we might label temperament. Finally, we find that framing the incentive as a bonus, rather than a penalty, encourages search but does not significantly affect performance.

*The authors would like to thank John Conlisk and Daniel Parent for helpful comments and suggestions. The authors would also like to thank the University of Nevada and NSF Grant SBR-9709333 for support of this research.

1 Introduction

Knight (1921) introduced the concept of decision making in the face of uncertainty to describe situations where an individual chooses a course of action without fully understanding the consequences; a ubiquitous problem in day to day decision making. The purpose of this paper is to present the results from an experiment on search under uncertainty that explores the interplay between costs, rewards and individual specific characteristics. The goal of the paper is quantify the relative importance of luck, ability and individual temperament in explaining performance.

Specifically, we report the results of an experiment that varies two factors: (1) The complexity of the problem, and (2) whether the incentive is framed as a bonus or a penalty. Individuals are asked to allocate a fixed amount of a resource among several tasks. The number of tasks is a treatment that is varied from 2 (most simple) to 5 (most complex). For each resource allocation problem, individuals may engage in costly, experimental search in an attempt to discover the best allocation. There are two groups, one in which the incentive to find the best choice is framed as a bonus and one in which the incentive is framed as a penalty. Subjects know the return associated with the best allocation is 50 cents, and they are given an endowment of 50 cents that may be used incrementally to pay for search. Individuals do not, however, know the deterministic relationship that exists between their allocation choice and the return received. In the case of the complexity treatment, we have constructed a panel data set, which allows us to estimate the importance of individual characteristics. We find that in addition to sheer luck, the evidence suggests that temperament plays a role in performance, while the importance of ability is greatest on tasks of intermediate difficulty (explaining about 17% of the variation).

1.1 The Experimental Literature on Search

The paradigm search problem is based upon a simplified model of the job market. Individuals apply sequentially for jobs, and when a prospective employer makes a wage offer, the individual then decides to either accept the offered wage, or, at a cost, continue searching. It is well known (e.g. Lippman and McCall (1981)) that under the hypothesis that wages come from a known and fixed offer distribution, then the optimal strategy entails forming a reservation wage, and accepting the first job whose offer is above this reservation wage.

Early work by Braunstein and Schotter (1982) and Schotter and Braunstein (1981) finds that the data is broadly consistent with the predictions of optimal search theory. They compare the case of risk to uncertainty and find that uncertainty tends to increase the amount of search. This is attributed to the difficulty that individuals have in adjusting their beliefs in the face of the information that they have received. Hey(1982, 1987) also compares the performance of individuals in the face of both risk and uncertainty, and finds that uncertainty reduced performance, and that in general individuals search too little.

Cox and Oaxaca (1989) find that one is better able to fit the data if it is assumed that individual preferences are concave. This work is refined in Cox and Oaxaca (1992) where they study directly whether or not individuals use a reservation wage strategy. They reject the hypotheses that a reservation wage reservation strategy is used when individuals are assumed to be risk neutral, but conclude that under the assumption of risk aversion observed behavior is consistent with the data. Harrison and Morgan (1990) study the way individuals may decide to sample the population of choices, and find that individuals use the most general strategy available, suggesting that the predictions of the simple search model are unlikely to hold once one allows for more complex wage offer processes.

Overall the evidence supports the hypothesis that individuals do act in a way to maximize returns.[1] These results are consistent with the work of Harless and Camerer (1994) and Hey and Orme (1994) on decision making, who find that that the hypothesis of utility maximization is a good first order model. However, there remains a great deal of unexplained variation from optimal behavior, and hence the evidence also strongly supports the hypothesis that individuals are boundedly rational. For example in one of the experiments run by Cox and Oaxaca (1992), optimal behavior would result in a payoff of $24, while observed payoffs varied from $17 to $24. In order to understand the scope of the model of optimal choice it is useful to be able to know the magnitude of the error, and how much of this error is individual specific.

1.2 The Role for Individual Specific Characteristics

Colin Camerer (1995) concludes his survey on experimental studies of decision making that economists have had three reactions to the data on individual decision making. The first is to test whether anomalies in decision making are replicable across settings, a response that he calls "destructive" and is in his judgement all too common. A second response is to construct theories that explain the anomalies. This is the approach taken in psychology, where one derives models that can represent behavior in specific contexts. This approach is particularly important and useful for engineering psychology (e.g. Wickens (1992)) where one ask questions such as the best way to design airplane controls to reduce pilot error.

Search theory in economics was originally developed to study market level phenomena, such as wages and employment. Hence, the model is at best a crude approximation to the environment that individuals face in the process of job search. Therefore, it is not clear that better understanding the detailed search algorithms individuals use in a particular setting can explain aggregate level data. To address this issue, Camerer observes that the third response to the data on decision making is to ask why do models of rational choice work so well at the aggregate level, even though individual level responses are error prone (see Plott (1986) and Smith (1991)).

[1] There is also a large literature in labor economics on search. In that literature one is not exploring the behavior of individuals, rather optimal search is assumed, and then one estimates the reservation wage of individual workers. See Devine and Kiefer (1991) for an excellent review.

In this paper we suggest an approach, common in labor economics, that is consistent with the micro-based approach of psychology, and yet applicable to the study of decision making in unstructured economic environments. We know from psychological studies that individuals use a number of heuristics and shortcuts in day to day decision making (e.g. Kahneman and Tversky (1984)). Johnson and Payne (1985) present a theory in which that the choice of heuristic depends upon the cost of making a decision, the main treatment in our experiment.[2]

An implication of this approach is that we might expect there to be *individual specific effects* in performance. In other words, for a given problem it is know that there may be a variety of responses and performance levels. One reason may be because individuals use different heuristics, with correspondingly different levels of performance. If so, then we would not expect that the relative performance of two individuals to remain the same when they face a sequence of similar tasks.

Another reason that performance may differ is bounded rationality (Simon (1955)). Some individuals may be more clever than others at solving problems, and hence as one moves to different problems, the relative performance of individuals should remain unchanged. However, if an individual's superior performance depends upon a *specific* heuristic that does not generalize to other situations then we would expect to see different sets of individuals doing well for different sets of tasks.[3] This phenomena can be explored by examining the error structure of regressions that explain performance.

Suppose we can measure performance of individual in task i in trial t, given by P_{it}, as a function of task characteristics in trial t, X_t. Then we can estimate an equation of the form:

$$P_{it} = F(X_{it}) + \mu_i + \varepsilon_{it}.$$

where μ_i is a person specific error term, and ε_{it} is an idiosyncratic error that has an *i.i.d.* distribution. This equation is analogous to a standard wage equation for panel data in labor economics.[4] If one has repeated observations of the same worker, then the variance of μ_i and ε_{it} can be estimated separately. If the variance of μ_i is significant, then we would have evidence to suggest that part of performance is due to "ability", since all that is meant by ability is that an individual is able to perform on average better than another person.

Alternatively, if performance is the result of task specific heuristics then one may (though not necessarily) observe task/person specific effects, in which case μ_i is replaced by μ_{ik}, where k denotes task. In this experiment tasks are parameterized by the complexity of the task allocation problem: individuals have to allocate resources between 2,3,4, or 5 dimensions. A key feature of our experiment is that the decisions are made in the face of uncertainty rather than risk. We feel that this better allows us to explore the role of temperament in decision making, and is more representative of the types of decisions that individuals face

[2] See Payne, Bettman, and Johnson (1993) for a book length treatment. This book also reviews the relevant psychology literature on decision making.

[3] Though this is not a necessary consequence. High ability individuals might also be persons who have learned the best heuristic for many different problems. However, if we find that different people are good at different problems then we have evidence for the specificity of heuristics, while we can draw no conclusions from the converse.

[4] See Angrist and Krueger (1999) for a good discussion of the empirical issues in empirical labor economics.

daily. Let us explain why.

1.3 Risk versus Uncertainty

The majority of the experiments reviewed above focus upon the case of search in the face of risk, namely individuals are explicitly given the distribution of rewards, and then asked to search for the best alternative. The benefit of such an approach is that there is a well defined optimal strategy at which an individuals continues searching until one receives a reward that is equal to, or greater than the reservation value. The computation of the reservation value in a search problem is a difficult task, and one that can be quite difficult to teach (as we know from experience). Moreover, even a trained person may often be unable to compute the optimal value in an experimental setting. In response to this type of argument, Friedman (1953) has argued the theory does not require individuals to consciously compute the optimal strategy, rather it is sufficient for individuals to make decisions that are consistent with optimal behavior.

A problematic aspect of decision making in the face of risk as a test of optimizing behavior in Friedman's sense is that individuals rarely know the true distribution of returns. Rather, much day to day choices involve decisions in the face of uncertainty, and hence the algorithms that individuals have developed over time are more likely to be adapted to these situations, than to the problem of decision making in the face of risk. In particular, the decision maker when given a reward distribution in an experimental setting faces the following decision. She might simply use only the most basic information about the distribution, such as the maximum and minimum values, and then view the problem as one in the face of uncertainty similar to the daily problems of find the best route to work, the nicest outfit to wear etc.

Alternatively, she might decide to *analyze* the problem, and try to work out an optimal strategy given the distribution. She might decide to do this either because she has studied optimal search before, or because she feels that it may be best to think before one acts. However, given that the theory of optimal search is one that occupied many years of research, it is very unlikely that an individual would be able to derive the solution to the search problem within the context of an experiment. Therefore, one might even find that the more able individuals perform more poorly due to the time allocated to analyzing the problem.[5]

To address this issue, we have elected to explore search in the face of uncertainty with only minimal information regarding the distribution of returns (individuals are told what is the maximum that they may earn). However the normative theory of optimal choice in economics makes few predictions regarding optimal behavior in the face of uncertainty.

This theory is based upon Savage (1972)'s model of decision making under uncertainty. In Savage's model individuals transform a problem of decision making in the face of uncertainty to one in the face of risk by first constructing a model of the possible states of the world, and then imputing probabilities to these states. The difficulty is that the reservation value strategy is not optimal unless the individual subjectively believes

[5] One might argue that choosing whether to analyze a problem or not is itself an optimal decision. Day and Pingle (1991) call this economizing economizing, and show that it leads to a problem of infinite regress. See Conlisk (1996) for a further discussion of this issue.

that she knows the distribution of returns, and that this distribution is stationary over time. If the individual is uncertain regarding the true distribution, then beliefs must be updated as she acquires information. In this case it can be shown that any observed strategy can be made optimal![6]

We could speculate regarding what is a "reasonable" set of beliefs, however ultimately we must depend upon the data to learn how individuals make decisions in practice. Our approach is to explore the relationship between the parameters of the search problem, performance and behavior. From the previous experiments it is known that individuals follow strategies that roughly correspond to a reservation wage strategy. What we would like to determine is the extent to which performance is person or person/task specific. Let us briefly discuss some of the factors that may affect behavior and performance.

1.3.1 Factors Affecting Performance

There is growing evidence that in addition to ability and cognitive characteristics, an individual's *temperament* is likely to affect the quality of decision making in the face of uncertainty. In his celebrated book, *Decarte's Error,* Damasio (1995) documents the effects of damage to the frontal temporal lobe of the brain for individual decision making. While these individuals did not score significantly different than normal individuals on tests of intellectual capacity, they were unable to perform satisfactorily when it came to making plans in the face of uncertainty.

Though the cases described by Damasio are extreme, we learn from them that performance in the face of uncertainty depends not only upon the characteristics of the problem, but also upon how an individual trades off gains today against future, uncertain gains. Even if we restrict ourselves to the range of normal behavior, Kagan (1994) has made the point that variations in individual temperament can affect individual behavior, ultimately influencing their search intensity and how well they concentrate over the length of the experiment. These effects are explored by measuring the variation of individual performance, and the effect of individual characteristics upon performance and search behavior.

In terms of our treatment with different levels of complexity, temperament can affect the person's propensity to continue search. There is some evidence from earlier experiments that on average individuals search too little, and however these experiments did not explicitly explore the importance of individual variation. If search intensity is an individual effect, then in those experiment individuals who have a tendency to search longer should perform better. What is interesting is that higher performance would not necessarily be due to higher "ability" but may be a consequence of an individual's temperament.

In addition to ability and temperament, sheer luck also plays a role in final performance. In our experiment individuals have no guidance on how to make their initial trial choice. Thus, we may view the payoff from the initial trial as exogenous. Because the subjects are informed of the maximum, we would expect the quality of the initial trial to be positively related to the ultimate performance and negatively related to the

[6] For any observed behavior, it is a straightforward excercise to construct priors such that the optimal Bayesian rule is consistent with observed behavior (see DeGroot (1972) for a review of the theory).

number of searches.[7]

2 The Experiment

2.1 Subjects

The subjects were all undergraduate students at the University of Nevada, Reno. They were recruited as volunteers from economics courses. A total of 54 subjects participated. However, for reasons noted below, two of these subjects were considered outliers and their data was discarded. Thus, the data set included data on 52 subjects.

After participating in the experiment, each subject completed a demographic questionnaire and provided the following information: age, University of Nevada GPA, completed cumulative credits, average hours worked for pay per week, gender (1=male), major (1= business major), marital status (1=yes), and an answer to the question, "Are you receiving financial aid?" (1=yes).

The questionnaire also asked subjects about any methods or rules they may have used in their decision making. In particular, subjects were asked:

1. To the best of your ability, describe the method or methods you used when playing the effort allocation game.

2. To the best of your ability, describe how you decided when to stop searching.

Using these responses an attempt was made to categorize subjects according to search method and stopping rule. Because the responses were so varied, only very simple categorizations were possible. The search method response was categorized as "systematic" (SS-rule) if the subject indicated the use of some systematic method for comparing alternatives; otherwise the response was classified as "unsystematic." A stopping rule response was classified as "cost focus" (C-rule) if only the costs of search were mentioned, "benefit focus" (B-rule) if only the benefits obtained from search were mentioned, and "cost-benefit" (CB-rule)if some notion of comparing the costs of search with the benefits of search was mentioned.

The summary demographic and decision rule statistics are presented in table 1. In the results below, we use only those variables there were found to be consistently significant in the regressions.

(Insert Table 1 About Here)

[7]individuals should not be told the maximum they can earn. However, because subjects were paid more when they performed well, they would form a subjective believe regarding the maximum they could earn if the information were not provided. By giving them a maximum, we are able to better control for beliefs. Because there is no reason for them to have similarly strong views regarding the *distribution* of rewards, the subjects were only given the maximum.

2.2 Design

In this experiment individuals are asked to solve a decision problem presented in the form of a computer game called the "effort allocation game." We now describe the game by amplifying the instructions read by subjects. (The instructions for the bonus group are included as an appendix.)

For each problem, there were a number of "tasks" the subject had to perform. To perform a task, the subject had to allocate 200 units of "effort" among the tasks. The subject was informed that allocating more effort toward one task would allow the task to be performed more effectively. However, the subject was also reminded that allocating more effort to one task would mean less effort would have to be allocated to some other task.

Figure 1 displays the game screen and some example entries. The screen indicates the game is the fourth game played by the subject; there are 5 tasks; and the second alternative (or search opportunity) is being considered. The subject allocated 45 units of effort to tasks 1, 2, 3, and 4 on this trial. The computer then entered the balance of 20 units of effort to task 5. (For all games, the computer entered the allocation for the final task in this manner so that variation would not be introduced due to adding errors.)

(Insert figure 1 about here)

The "search points" line, explained in more detail below, provides subjects with feedback about the quality of the particular choice (these are transformed into monetary payoffs). In the Figure 1 example, because $7.4 > -46.9$, the search points line shows that the second alternative (45,45,45,45,20) was better than the first alternative (34,34,34,34,64). The columns "Last" and "2 Back" show the choices and search points for the last two alternatives previously considered. (Column "2 Back" has no entries in Figure 1 because there is only one previous alternative in the example shown.) The "Best" column displays the best choice made up through the current search opportunity, along with its associated search points. Together, the columns "Best," "Last," and "2 Back" provide the subject with a memory, offering the experiment some control over variation due to varying memory capabilities.

At this point in the game, the subject whose output is displayed in Figure 1 had to decide whether to continue searching or stop searching and accept the "total earnings" of 55.8. The subject would continue by simply hitting the enter key. Upon continuing, the subject would have to enter another trial allocation. A subject could try up to 25 alternatives in any one game; i.e., for any one decision. The subject would stop by selecting the stop search option and then hitting enter. Upon stopping, the next game would immediately begin.

The subject was told that his or her goal was to allocate the effort endowment so as to maximize "total earnings." The subject was told that he or she had an incentive to do as well as possible in each game because the cumulative earnings earned over the 20 games would be paid in the form of cash. Subjects are divided into two groups, a bonus and a penalty group. For subjects in the bonus group, it was explained that total earnings was the sum of the "wage," the "decision bonus," and the "efficiency bonus." For subjects in the penalty group, it was explained that total earnings was the sum of the "wage," the "decision penalty," and the "efficiency bonus." These different types of earnings will now be described in more detail.

The "wage" was an amount of cash received for the solving the problem, regardless of the quality of the performance. For the bonus group, the wage was 1 cent for each problem, while for the penalty group, the wage was 51 cents for each problem.

The "efficiency bonus" was an amount of cash paid for conserving decision-making resources. Formally, the efficiency bonus was determined by the formula

$$EFF = \gamma_1[375 - ALT1 * 15]^{\gamma_2}.$$

EFF is the efficiency bonus. ALT1 measures the search opportunities taken by the subject, an integer that began at 1 and increased to 25 as the subject considered trial alternatives. If a subject accepted his or her first trial alternative as the choice and did so within 15 seconds, then the maximum efficiency bonus was received, which was 49.1 cents. The computer displayed the "cost of the next trial" in terms of the efficiency bonus that would be forgone. The subject was informed that, if all 25 search opportunities were tried, then the efficiency bonus would equal zero.

The rate of decline in the efficiency bonus is equal to the marginal cost of search. The parameters γ_1 and γ_2 were set at 0.33 and 0.85 so that the efficiency bonus would decrease at a slightly increasing rate as additional alternatives were considered. Using these parameters, the marginal cost of trial 2 was 1.75 cents, while the marginal cost of trial 25 was 3.30 cents. Given that the decision problem is of a finite horizon, increasing the marginal costs of search with time helped ensure that most individual would find it optimal to stop search before reaching trial 25, reducing the amount of censoring that might occur at $ALT1 = 25$ (though even with this design, a small amount of censoring did occur).

To prevent subjects from being able to think without cost, a subject was not allowed to use more than 15 seconds to try an alternative without having to pay for the time used. If the subject waited longer than 15 seconds, then the subject was charged for using a trial just as if a trial allocation had actually been entered. This meant that a subject could use at most 375 seconds to solve a problem. However, as long as a subject used less than 15 seconds to try an alternative, the cost of search (as measured by the efficiency bonus) depended upon the number of searches, not time used.

The return from search was framed differently depending upon whether individuals were in the bonus group or the penalty group. Letting S denote the return associated with the effort allocation (y_1,y_2,y_3,y_4,y_5), calculated as follows:

$$S = \alpha_1 y_1 + \alpha_2 y_2 + \alpha_3 y_3 + \alpha_4 y_4 + \alpha_5 y_5 - [\beta_1 y_1^2 + \beta_2 y_2^2 + \beta_3 y_3^2 + \beta_4 y_4^2 + \beta_5 y_5^2] - f,$$

where f is a fixed cost parameter and α_i and β_i are "benefit" and "cost" parameters for the tasks $i = 1, 2, 3, 4, 5$. The parameters were chosen so that the maximum value for S was equal to 50, given the constraint $y_1 + y_2 + y_3 + y_4 + y_5 = 200$. For games with k tasks when $k = 2, 3, 4$, α_i and β_i were set equal to zero for $i > k$. When S was larger, bonus group subjects received a larger "decision bonus," while penalty group subjects received a smaller "decision penalty."

In particular, a bonus group subject using n search opportunities received a decision bonus equal to the maximum value of S obtained over the n trials. Subjects were informed in advance that the maximum they could receive is 50 for each problem. For each trial, the computer calculated and displayed the associated return, which allowed subjects to easily assess the potential for improving the quality of the choice through further search.

A penalty group subject using n search opportunities received a decision penalty equal to 50 minus the maximum search points number the subject obtained over the n trials. Because 50 search points were obtained when the optimal choice was made, the minimum penalty was zero. Subjects in the penalty group were not told that the maximum number of search points was 50, but they were told the minimum decision penalty was zero. Thus, like the bonus group, the penalty group could easily assess the potential for improving the quality of the choice through further search.

It is important to recognize that a bonus group subject and penalty group subject playing the same game number, say game number 4, would obtain precisely the same total earnings if they made the same choice and stopped after trying the same number of alternatives. To show the relationship between the two groups in a careful way, let S_{opt} denote the search points earned when the optimal choice was made, and let S_{max} denote the maximum search points earned by the subject over the n trials used to make the choice. The following series of equations, shows the relationship between bonus group total earnings and penalty group total earnings:

$$\begin{aligned}\text{Bonus Group Total Earnings} &= \text{Bonus Group Wage} + \text{Decision Bonus} + \text{Efficiency Bonus}\\ &= 1 + S_{max} + \text{Efficiency Bonus}\\ &= [1 + S_{opt}] - [S_{opt} - S_{max}] + \text{Efficiency Bonus}\\ &= \text{Penalty Group Wage} - \text{Decision Penalty} + \text{Efficiency Bonus}\\ &= \text{Penalty Group Total Earnings.}\end{aligned}$$

Notice that both the bonus and the penalty group subjects are paid total earnings equal to $1 + S_{max} +$ Efficiency Bonus. The bonus incentive is converted into a penalty incentive without changing total earnings by adding and subtracting S_{opt} as shown. In the example presented in Figure 1, S_{max} is 7.4, S_{opt} is 50, and the efficiency bonus is 47.4. The total earnings of the penalty group subject shown is calculated as $55.8 = [1 + 50] - [50 - 7.4] + 47.4$. If a bonus group subject had made the same choice after the same number of alternatives, the total earnings would have been the same, but it would have been presented as $55.8 = 1.0 + 7.4 + 47.4$.

The final comment about the game is a technical, but significant, detail. Because subjects played 5 different games for each task type $k = 2, 3, 4, 5$, different optimal choices had to be constructed that would yield 50 search points. For each problem, the challenge for the subject was to determine which tasks deserved more effort and which tasks deserved less. Thus, the difference in the optimal level for one task level compared to the optimal levels for others could have an impact on the complexity or difficulty of the problem. To control for this potential variation across the 4 game types, a Euclidean distance measure was used in the process of setting the parameters. For each game type, the problems were constructed so that the Euclidean distance between the optimal choices was relatively large for 2 problems, small for 1 problem, and intermediate for 2 problems.

3 Procedure

The experiment included 11 sessions over a one month period, with the number of subjects participating in a session ranging from 2 to 10. At the beginning of a session, subjects were asked to read a set of game instructions. From a series of pilot experiments, it was determined that subjects would not consistently understand how to play the game merely by reading a detailed set of instructions. However, allowing subjects to gain some limited experience with the game prior to "playing for real" proved effective at eliminating outlier type behavior due to misunderstanding. The two procedures used to provide subjects with this limited experience are now described.

First, after reading the game instructions, all participants played a single, highly structured, tutorial game called an "administrative game." The purpose of the administrative game was not training, though some learning surely occurred. Rather, the purpose was to make sure all subjects understood the meaning of what was presented to them on the computer screen and knew how to use the computer keys to move from one part of the game to another. Using a "standard administrative procedure," all subjects in the session were simultaneously exposed to the salient features of the game. Subjects were not allowed to experiment with the game on their own. The administrative game ended when subjects were shown how to stop trying alternatives and make a choice.

Second, subjects played a set of four "practice games." The purpose of the practice games was to expose subjects to each of the four game types and allow subjects to get somewhat familiar with the choice problem. The practice games began with the most simple game (a 2 task game), proceeded to the games of intermediate complexity (3 tasks and then 4 tasks), and ended with the most complex game (a 5 task game). (The administrative game was a 5 task game.) Subjects played these games at their own pace and could raise their hands to ask questions during their play. Questions were answered on an individual basis so that the information provided to subjects in each session was consistent and not dependent upon the questions asked.

Upon completing the practice games, each subject played a set of 20 games. The 20 games were arranged in 5 sets of 4 games, so that a subject played a 2 task game, a 3 task game, a 4 task game, a 5 task game, and then repeated this pattern 4 times. This design allowed the impact of experience to be examined. Subjects were told that each game was independent in the sense that the performance in one game, good or bad, did not affect the earnings opportunity in any future game. Also, subjects were not shown their cumulative earnings until all 20 games had been completed.

Upon completing the 20 games, subjects were required to fill out a questionnaire containing the demographic and method questions mentioned above. After completing the questionnaire, the subject was paid the cumulative earnings generated by the 20 choices. The average earnings level was $13.70, ranging from $10.13 to $16.78.

4 The Data

Data was collected from the participation of 54 subjects. However, two subjects were considered outliers and their data was discarded. One of these subjects mistakenly thought he was required to use all 25 search opportunities. The second outlier subject got frustrated in an early round and admittedly quit trying to make good choices. Thus, the data set described here contained observations on 52 subjects, 26 from the bonus group and 26 from the penalty group.

Data was collected on 32 variables. Variables related to the decision-making process were associated with the variable ALT1. ALT1 recorded the number of search opportunities made available to a subject, while the variable ALT recorded the number of alternatives actually tried by the subject. As noted above, if a subject waited longer than 15 seconds to consider an alternative, the computer charged the subject for using a search opportunity even though no alternative was considered. Thus, ALT1 is always at least as great as ALT, and ALT1 and ALT were equal as long as the subject considered all previous alternatives in less than 15 seconds. The difference between ALT1 and ALT represents the number of times a subject took longer than 15 seconds to consider an alternative, perhaps indicating that the subject was more contemplative. A total of 8,155 observations were made on ALT1. In addition to ALT, variables that could potentially change when ALT1 changed included,

- Y1: The subject's allocation choice for task 1.
- Y2: The subject's allocation choice for task 2.
- Y3: The subject's allocation choice for task 3.
- Y4: The subject's allocation choice for task 4.
- Y5: The subject's allocation choice for task 5.
- S: Search points associated with the choice (Y1,Y2,Y3,Y4,Y5)
- SMAX: Maximum search points level found for the problem through search opportunity ALT1.
- Y1BEST: Allocation choice for task 1 associated with SMAX.
- Y2BEST: Allocation choice for task 2 associated with SMAX.
- Y3BEST: Allocation choice for task 3 associated with SMAX.

- Y4BEST: Allocation choice for task 4 associated with SMAX.
- Y5BEST: Allocation choice for task 5 associated with SMAX.
- EFF: Efficiency bonus.

Variables related to the choice ultimately made as the result of the decision-making process are associated with the dummy variable FINAL. FINAL equals 1 for the search opportunity when the choice was made and zero otherwise. The following variables are related to FINAL:

- ROUND: Problem number—runs from 1 to 20. (Changes once Final=1.)
- TASK: Number of tasks present in the problem—runs from 2 to 5. Rounds 1, 5, 9, 13, and 17 were 2 task problems. Rounds, 2, 6, 10, 14, and 18 were 3 task problems. Rounds 3, 7, 11, 14, and 19 were 4 task problems. Rounds 4, 8, 12, 16, and 20 were 5 task problems. (Changes with ROUND once Final=1.)
- CUM: Cumulative earnings for the subject after the completion of the given round. (Nonzero only when FINAL=1.)
- TIME: Time used during the round to make the choice. (Nonzero only when FINAL=1.)
- EXPi: Experience dummy. Subjects faced 5 sets of problems sequentially involving 2, 3, 4, and 5 tasks. EXPi=1 when set i is being decided upon.

Observations associated with the two outlier choices were removed. In both cases, a subject uncharacteristically accepted a very poor choice (more than 4 standard deviations below the mean) even though many more search opportunities remained available. Consequently, the data set included data on 1038 choices (52 subjects, making 20 choices each, less two outliers).

SUBJECT is a categorical variable associated with the 52 participants. The 26 bonus group subjects are associated with SUBJECT values 101 to 126, while the 26 penalty group subjects are associated with SUBJECT values 201 to 226. The following variables are associated with SUBJECT that are used in the regression:

- GROUP: Group dummy (0=bonus group, 1=penalty group)
- GPA: GPA on University of Nevada credits.
- SEX: Sex. (1=Male. 0=Female)
- MAJOR: Major. (1=Business, 0=Nonbusiness)
- FINAID: Financial need indicator. (1=Receiving financial aid. 0=Not receiving financial aid.)

- SSRULE: Search method dummy (0=Unsystematic method. 1=Systematic Method)
- CBRULE, BRULE and CRULE: Decision rule dummies (CBRULE=1 if costs compared to benefits, BRULE=1 if subject reported focusing on benefits only, while CRULE=1 if cost focus is reported by subject).

In addition to these 32 variables, other variables were calculated to examine results of interest. These calculated variables are identified and described when the results are presented.

5 Results

5.1 Performance

In Table 2, we present a series of regression results that examine the performance of subjects as a function of the observable characteristics of the decision problem. After experimentation with a number of different functional forms, we model performance using a multiplicative relationship:

$$1/\left(101 - P_{it}\right) = e^{\mu_i} e^{f(s_{it})} e^{\gamma_d} e^{\beta_e},$$

where P_{it} is performance of the individual i in trial t, μ_i is an individual fixed effects, s_{it} is the return on the first choice of trial t in the search process, and $f\left(s_{it}\right)$ is a general function of this return. The parameter γ_d is the difficult of task with d choices, where d is $2, 3, 4$, or 5. Finally, β_e is a parameter that varies with experience, which has five levels: $1, 2, 3, 4$ or 5. Individuals with experiment e have faced more than $4\left(e-1\right)$ and less than $4e$ trials. In our specification, f is represented by a general step function, and since γ_1 and β_1 cannot be separately identified they are set equal to 1. Taking logs transforms this equation into the linear equation that we estimated:

$$-\ln\left(101 - P_{it}\right) = \mu_i + f\left(s_{it}\right) + \gamma_d + \beta_e + \varepsilon_{it},$$

where ε_{it} is an unexplained *i.i.d.* error term.

We tried a number of different specifications and found that allowing for decreased sensitivity to changes in the factors for payoffs close to the maximum gave us the best fit. This finding is consistent with the expectation that a subject would find it increasingly difficult to improve the payoff as the maximum is approached.

The Effect of Task Complexity We begin with an analysis of task complexity on performance. In column (i) of Table 2, we include only the task dummies t3, t4, and t5 as explanatory variables. By excluding the task dummy t2, the constant estimate is a measure of the average performance on the easiest 2 task problem. The coefficients on t3, t4, and t5 are estimates of the difference in performance between the 2 task problem and the more difficult 3 task, 4 task, and 5 task problems. As would be expected, we find that the average performance gets significantly and progressively worse as the problem gets increasingly complex.

The results of a fixed effects regression (ii) allows us to see how much of the variation is due to individual variation in ability. Notice the fixed effects increase the R^2 and account for about 7% of the variation unexplained by the complexity of the problem. The coefficients on the task complexity dummies do not vary much when we add the fixed effects, suggesting that there are no non-linear relationships between "ability" and task complexity. In all the regressions we run, the F-test consistently rejects the null hypothesis that individual heterogeneity is not important.

The Effect of Luck These results are likely to under-estimate the importance of the fixed effect because we have not controlled for *luck.* There is nothing to guide subjects in their initial trial choice, and those who happen to guess close to the optimum would have better performance regardless of their ability. Hence, ability should be judged conditional upon the quality of the initial trial choice.

We introduce a non-parametric specification for the effect of the initial trial choice on final performance. The variables sd$\#_1$-$\#_2$ are dummies that are one when the difference *sd* between the quality of the initial trial and the optimum is between $\#_1$ and $\#_2$ points and zero otherwise. The category boundaries are chosen so approximately 10% of the sample is in each category. The estimated coefficients are relative to sd44-59, the dummy omitted to ensure identification. The effect of initial trial choice on performance is presented in column (iii). We find that subjects whose initial trial was closer to the optimal allocation performed significantly better on average. The extreme dummy categories—either very good initial choices or very poor initial choices—have the largest impact. The coefficients have the expected sign, and the inclusion of these variables improves the fit by 20%. The importance of the fixed effects increases from 7.1% to 9.4% of the unexplained variance, which is evidence that the omission of luck leads to misspecified model. Given that the initial trial is essentially a random draw, the experiment demonstrates the importance of "luck" in explaining observed performance.

In addition to luck, experience with the task may also affect performance. The variable $exp\#$ is 1 when the subject is doing a task for the #th time, and zero otherwise. The evidence presented in column (iv) is mixed. Subjects performed significantly better the third time they solved the problems than first or second times, an indication that experience was beneficial. However, the fourth and fifth times were no better than the first and second and were significantly worse than the third. It is not clear why this decline in performance occurred. Possible explanations include boredom, increasingly creative but unfruitful search, or the exceptionally strong performance the third time may have been an aberration (statistically unlikely).

We would expect the effect of individual heterogeneity to be greater when a subject makes a poorer initial trial choice. This is because there would be a greater motivation to search, and the fruitfulness of search could vary more substantially across individuals. To examine this issue, regression (v) considers only those observations for which $sd0014 \neq 1$. That is, the initial trial choice is at least 14 points away from the optimum. This results in a poorer fit, as would be expected, though all the parameters continue to have the expected sign. The important finding is that individual heterogeneity does account for more of the variation not explained by the other factors— 17% as opposed to 9%. Below, we present results on behavior that suggest this increase in the explanatory power of the fixed effect is not simply associated with the decrease in sample size, but rather is associated with the fact that some search more effectively than others.

The Effect of Observed Individual Characteristics The individual fixed effects are estimated from the data, and hence correspond to *measured* ability. One may also ask if ability is correlated with observable individual characteristics. Regressions (vi) and (vii) examine this question. Regression (vi) reports the effect of the individual's GPA and whether or not the individual received financial aid. The GPA variable has the expected sign. Surprisingly, receiving financial aid has a strong negative effect in all our regressions. While we are not sure why this occurs, it could be that not receiving financial aid, like a higher GPA, is a proxy for individual ability. Thinking of GAP and $finaid$ as proxies for individual ability, the results indicate more able subjects performed better.

We found that whether or not the subject's reported the use of a systematic search strategy had an effect. Examining regression (vii), those who indicated they used some type of systematic search method performed better. This suggests that part of being more able is an aptitude for using specific search algorithms. Whether or not a subject indicated they focused on costs, benefits, or costs and benefits in determining when to stop their search had no impact on performance.

We found that whether or not one is paid a penalty or bonus has no effect on performance. Because these two incentive schemes differ only in terms of how they were framed, this result may not be surprising. However, as we discuss below, we found that the two incentive schemes did significantly affect search behavior differently, making it surprising that performance was not also effected.

Individual Heterogeneity and Complexity It is perhaps surprising to have fixed effects explain such a small percentage of the variance, and to have more than two-thirds of the variation explained by the problem's complexity and the quality of the initial trial in the search process. One way individuals vary is in terms of their patience and ability to learn. Hence we might expect the effect of experience to vary across individuals. To study this possibility, we interacted our subject and experience variables to create a new fixed effect variable. While the fraction of variation explained by the new fixed effect variable was 21%, the F-test statistic indicated that this additional explanatory power comes from the addition more than of 200 new explanatory variables. According to the F-test, we fail to reject the hypothesis that the effect of experience varies across individuals.

To confirm this result, the model was estimated two other ways. In one case, we used maximum likelihood estimation first including only the subject indicators, and then including subject indicators and interaction terms. The subsequent likelihood ratio test indicated that adding the interaction terms made no significant difference. We also followed the same approach using a linear regression with indicator variables, testing the restriction that the interaction terms are zero. Again, the interaction terms made no significant difference. In summary, while we found that performance differed significantly across individual subjects, we failed to find evidence that some subjects were more effective at learning from experience than others.

Not only would we expect individual heterogeneity to be more important when the subject is unlucky, but also when the problem is more complex. More able individuals should have a comparative advantage in more complex problems. Table 3 reports the impact of individual characteristics on performance by complexity level.

The results here are similar to the ones we obtained using the pooled data. What is new are differences across the complexity levels in the fraction of the residual explained by the fixed effects. With the smaller cell sizes, the overall fit is worst, and hence we would expect the fixed effect to explain more of the variance. What is interesting is that the fixed effects are most important for the 3 task problem. For the more complex 4 and 5 task problems, not only is the fraction of the variance explained by the fixed effect less, but the significance of the fixed effects as measured by the F-test fall. Moreover, the significance of the initial trial increases in importance. This suggests that, for quite simple problems and for quite complex problems, luck plays a more important role relative to ability. The effects of individual characteristics tend to be much less important. It is when the problem is of intermediate complexity that individual differences in ability tend to matter.

5.2 Behavior

The next set of results explores the decision making process, and the propensity for individuals to continue searching. Table 4 reports the results from estimating a Cox proportional hazard model. In this model, it is assumed that the baseline hazard is non-parametric (which models the time varying nature of the reservation value), and that the explanatory variables enter exponentially. The probability that subject i stops search in round t, $\theta_i(t)$, is given by:

$$\theta_i(t) = e^{x'_{it}\beta}\theta_i^0(t),$$

where x_{it} is the person's state in period t, β is a set of common parameters to be estimated and $\theta_i^0(t)$ is the baseline hazard. When x_{it} includes person specific, time invariant characteristics, then it is assumed $\theta_i^0(t)$ does not depend upon the subject i (analogous to our performance regressions with no fixed effects). When all the data is time varying, we estimate a person specific baseline hazard (analogous to our performance regressions will fixed effects). In Table 4, we report the hazard ratio for each explanatory variable, with its associated standard error.

The Effect of Task Complexity The effect of task complexity on search propensity is in theory ambiguous. In this experiment, the subjects knew the maximum they could receive, and hence we might expect that they would spend more time searching in the more complex tasks because it would take more effort to get close to the maximum. In column (i) we see that this is indeed the case. Individuals were less likely to stop searching in the more difficult tasks. We stratified the sample to allow for an individual specific hazard, and hence removed any bias that might be caused by individual heterogeneity.

If the individuals are expected utility (or value) maximizers then they would use a reservation value strategy and would stop searching once the current payoff passes a threshold. We allow for a non-parametric specification of the reservation value strategy by creating variables $sm\#_1 - \#_2$ that are 1 when the best previous trial in the search process is between $\#_1$ and $\#_2$ units from the optimum, and zero otherwise. As we see in regression (ii), the inclusion of these variables greatly improves the fit. What is interesting now is that the effect of complexity is completely reversed. Conditional upon current rewards, individuals facing more complex problems are more likely to exit.

These results are consistent with a reservation value strategy. If a subject views the returns from a trial choice as randomly generated, then the optimal search rule is some form of reservation value strategy. In our experiment, search costs increase with each trial, so a subject's reservation value would theoretically decrease with each trial. A subject's reservation value would also theoretically decrease with each trial if there were a decrease subject's subjective estimate of the probability of obtaining further improvement. Our results indicate that complex problems reduce the average subject's subjective estimate of the probability of obtaining further improvement, increasing the likelihood that the subject will stop searching.

We might also expect experience to affect search intensity. This is explored in regression (iii), and we find that experience has no effect on the probability of stopping. This suggests individuals do not vary their strategies as they gain experience, evidence that is also consistent with a fixed reservation value policy.

Temperament An individual's search intensity corresponds to individual *temperament* - the level of impatience the individual has with the search problem. Previous research by Hey (1987) and Schotter and Braunstein (1981) finds that individuals tend to search too little. In this experiment, it is not possible to deduce an "optimal" search intensity since subjects do not know the true model. However, we can measure the search intensity of those who on average did better than the others. The fixed effects from the regressions in Table 2 provide a measure of how an individual performs relative to others. We construct two measures of relative ability. Using the estimated fixed effects from the regression reported in Table 2, column (iv), we obtain a measure based upon the whole sample. Using the estimated fixed effects from the regression reported in Table 2, column (v), we obtain a measure for those instances where the individual had an unlucky initial trial choice. (Because each person plays 20 times, each has a chance to be "unlucky.")

Regression (iv) in Table 4 reports the effect of the fixed effect from the full sample, which has a significant hazard ratio of 1.75. This indicates that individuals who did better on average tended to search less, or were less patient, in contrast with earlier results. However, our finding may be reconcilable because we find it to be dependent upon the individual's situation.

In particular, when the initial trial happens to be close to optimal, impatience is a virtue that saves search costs, while it is not so valuable when the initial trial is poor. In column (v) of Table 4, we look at those observations where the individual's initial trial is not close to optimal. In this sub-sample, the individuals who were successful overall still left early, though now *not* significantly so.

To explore this further, we drop the fixed effect that measures relative performance overall and add the fixed effect that measures relative performance for instances where the initial trial was not close to optimal. In regression (*vi*) in Table 4, we see that the those who performed better were actually more willing to search, though *not* significantly so.

In summary, regressions (v) and (vi) indicate individuals who did well when given a relatively poor initial trial choice did not have a search intensity that was significantly different from the mean search intensity. Alternatively, regression (iv) indicates those who did well regardless of the quality of the initial trial stopped sooner. Thus, temperament does not appear to play a significant role when the initial trial leaves much room for improvement, but plays a significant role when the initial trial is near optimal. In particular, when the initial trial is near optimal, those who are *less patient*, or more conscious of the inability to obtain further improvement, perform relatively well.

In regression (vii) we explore the effect of the treatment of bonus or penalty on behavior. We find that individuals facing a penalty treatment are more likely to exit. However, given that the group variable had no effect upon performance, the penalty treatment does not appear to make individual more decisive in a way that leads to better performance. In fact if we restrict the sample to those observations with a near optimal initial trial we find that group has no effect, but that the fixed effect from regression (iv) in Table 2 has a *larger* effect. Hence, it appears that the individual must actually be experiencing the penalty in order for the penalty treatment to have a distinguishable impact on behavior relative to the bonus treatment.

Search Technique Next we look at the effect of reported search techniques on behavior. In regression (viii) we see that individuals who report using a systematic search technique are more likely to stop search, even after controlling for current payoff. We find that the systematic search variable ssrule is positively and significantly correlated with the fixed effects from the Table 2 regressions (iv) and (v)—correlation coefficients of .363 and .366, respectively. Thus, we have evidence that more systematic search led to better performance because it enabled the individual to find a quality choice more quickly and conserve on search costs.

From regression (*viii*), we also found individuals reporting the use of stopping rule with a benefit cost or benefit-cost based rule were less likely to stop searching than those who reported the use of a stopping rule that focused solely on the cost of further search. Because these variables had no significant impact on overall performance, it is difficult to ascertain the significance of these results.

6 Discussion

One of the challenges in modeling decision making in the face of uncertainty is the lack of a benchmark measure for optimal choice. The fact that the subjective beliefs cannot be specified *a priori* greatly reduces the predictive content of the theory. Beginning with Ellsberg (1961), this problem has been addressed by studying the behavior of individuals in situations where the theory does make sharp predictions. In these cases, there is a great deal of evidence that many individuals violate Savage's axioms of choice under uncertainty.

These results establish that individuals make mistakes, but provide less guidance regarding how individuals might behave in more "typical" situations. While the experimental situations examined here are not necessarily typical, they contain features common to a variety of uncertain decision environments. This allows us to obtain insight as to what factors may be more significant in predicting performance and search intensity.

Before carrying out these experiments we had expected that individual heterogeneity in ability and temperament would be very important. Though we have strong evidence they are important, luck plays an even bigger role. When the decision problem was very simple–our two task case—all individuals did well, and there was little variation in performance. The striking observation is, when the complexity of the problem rose, the initial trial in the search process, essentially a random draw, is the most important factor in determining overall performance.

However, as our 2 task case illustrates, making luck less relevant does not necessarily imply that ability is more important. When a task is simple and search costs are moderate, all individuals can perform well. We find that variations in individual ability and/or temperament are more important with problems of moderate complexity. For incentive theory, this has a number of implications:

1. It should take more observations of performance to distinguish ability from luck when the problem is more complex. This implies pay for performance in an uncertain environment exposes employers to a great deal of risk. Hence, it is not surprising that compensation often takes the form of a fixed salary with rewards provided by promotion over a period of years.

2. For some decision problems temperament appears to be more important than "ability". In our experimental environment, being decisive in the face of a high first draw results in a higher payoff since one does not engage in costly search when close to an optimum. This may explain why in earlier work it was found that individuals searched too little. Many psychologists have suggested that individuals use simple heuristics to make decisions in complex situations. If the heuristic is used in a variety of situations, then it is not surprising that in some cases it is closer to the optimum than in others. In particular if being decisive is on average a good strategy, this might explain why there is too little search in some situations.

3. Because individual performance can be context specific, incentives which are not context specific may be ineffective. Performance in the experiment is weakly related to GPA, and individuals who report searching systematically did better. However, as we saw when comparing performance over the full sample to the sub-sample with poor initial choices, no single individual characteristic seemed to dominate in terms of performance.

Previous work has established a number of situations where human decision makers consistently make errors. Here we have shown that, even in a very simple decision situation, luck and task complexity explain a great deal of the observed variation. We have also found some preliminary evidence that *temperament* can have an impact on performance, a result that is consistent with the recent research by Damasio (1995) and Kagan (1994). This interplay between chance, temperament and incentives is a potentially complex one, whose further study may have interesting implications for the design of compensation and incentive systems.

References

Angrist, J. D. and A. B. Krueger (1999). Empirical strategies in labor economics. In O. Ashenfelter and C. D. (Eds.), *Handbook of Labor Economics*, pp. 1278–1357. Elsevier Science B.V.

Braunstein, Y. and A. Schotter (1982). Labor market search: An experimental study. *Economic Inquiry 20*, 133–44.

Camerer, C. (1995). Individual decision making. In J. H. Kagel and A. H. Roth (Eds.), *The Handbook of Experimental Economics*, Chapter 8, pp. 587–683. Princeton University Press: Princeton University Press.

Conlisk, J. (1996, June). Why bounded rationality? *Journal of Economic Literature 34*(2), 669–700.

Cox, J. C. and R. L. Oaxaca (1989). Laboratory experiments with a finite horizon job-search model. *Journal of Risk and Uncertainty 2*, 301–30.

Cox, J. C. and R. L. Oaxaca (1992, November). Direct tests of the reservation wage property. *Economic Journal 102*, 1423–1432.

Damasio, A. R. (1995). *Descartes' Error*. New York, NY: Avon Books.

Day, R. H. and M. A. Pingle (1991). Economizing economizing. In R. Frantz, H. Singh, and J. Gerber (Eds.), *Behavioral Decision Making: Handbook of Behavioral Economics*, pp. 509–22. Greenwich CT: JAI Press.

DeGroot, M. H. (1972). *Optimal Statistical Decisions*. New York, NY: McGraw-Hill Book C.

Devine, T. J. and N. M. Kiefer (1991). *Empirical Labor Economics*. Oxford, U.K.: Oxford University Press.

Ellsberg, D. (1961, November). Risk. ambiguity and the savage axioms. *Quarterly Journal of Economics 75*, 643–69.

Friedman, M. (1953). *The Methodology of Positive Economics*. Chicago, IL: University of Chicago Press.

Harless, D. W. and C. F. Camerer (1994, November). The predictive utility of generalized expected utility theories. *Econometrica 62*(6), 1251–1289.

Harrison, G. W. and P. Morgan (1990, June). Search intensity in experiments. *The Economic Journal 100*, 478–486.

Hey, J. (1982). Search for rules of search. *Journal of Economic Organization and Behavior 3*, 65–81.

Hey, J. (1987, March). Still searching. *Journal of Economic Organization and Behavior 8*, 65–81.

Hey, J. D. and C. Orme (1994, November). Investigating generalizations of expected utility theory using experimental data. *Econometrica 62*(6), 1291–1326.

Johnson, E. J. and J. W. Payne (1985). Effort and accuracy in choice. *Management Science 31*(4), 395–414.

Kagan, J. (1994). *Galen's Prophecy*. New York, NY: BasicBooks.

Kahneman, D. and A. Tversky (1984). Choices, values and frames. *American Psychologist 39*(4), 341–350.

Knight, F. H. (1921). *Risk, Uncertainty, and Profit*. New York, NY: Hart, Schaffner, and Marx.

Lippman, S. A. and J. J. McCall (1981). The economics of uncertainty: Selected topics and probabilistic methods. In K. J. Arrow and M. D. Intriligator (Eds.), *Handbook of Mathematical Economics*, Volume 1 of *Handbooks in Economics*, pp. 211–284. Amsterdam: North-Holland Publishing Company.

Payne, J. W., J. R. Bettman, and E. J. Johnson (1993). *The Adaptive Decision Maker*. Cambridge, U.K.: Cambridge University Press.

Plott, C. R. (1986). Rational choice in experimental markets. *Journal of Business 59*(4), S301–S327.

Savage, L. J. (1972). *The Foundations of Statistics*. New York, N.Y.: Dover Publications.

Schotter, A. and Y. M. Braunstein (1981, January). Economics search: An experimental study. *Economic Inquiry 19*, 1–25.

Simon, H. A. (1955). A behavioral model of rational choice. *Quarterly Journal of Economics 69*, 99–118.

Smith, V. (1991, August). Rational choice: The contrast between economics and psychology. *Journal of Political Economy 90*(4), 877–97.

Wickens, C. (1992). *Engineering Psychology and Human Performance* (2nd ed.). New York, N.Y.: Harper-Collins Publishers Inc.

A Effort Allocation Game (Bonus Group)

Thank you for volunteering to play the Effort Allocation Game.

As a participant in this experiment, you will face 20 “effort allocation problems.” For each problem, there will be “tasks” you will have to “perform.” To perform a task, you must allocate effort toward it. You will be given an endowment of 200 units of “effort.” The decision you must make is how to allocate your 200 units of effort among the tasks. Allocating more effort toward one task allows you to perform that task more effectively. However, allocating more effort to one task means you must allocate less effort to some other task.

Your goal in allocating your effort is to make your “total earnings” as high as possible. Your earnings will be paid to you in the form of “cash.” Because you get the keep the cash that you earn, you have an incentive to do as well as you can on each problem. For a given problem, your total earnings is the sum of (1) your “wage,” (2) your “decision bonus,” and (3) your efficiency bonus. These three types of earnings will now be described in more detail.

Your “wage” is the amount of cash you receive for the problem regardless of how you perform. Your wage will be 1 cent for each problem.

Your “efficiency bonus” is the amount of cash you are paid for conserving decision-making resources. If you accept your first trial as your choice and do so within 15 seconds, then you will receive the maximum efficiency bonus, which is 41.9 cents. A fraction of your efficiency bonus will be taken from you for each alternative allocation you try. (The computer will show you the cost of trying another alternative in terms of the efficiency bonus you will lose). The more alternatives you try prior to accepting a choice, the lower your efficiency bonus. You may try at most 25 alternatives. If you use all 25 alternatives, your efficiency bonus will equal zero; the computer will automatically accept your best allocation alternative as your choice for the problem; and your total earnings will equal your wage plus your decision bonus. One further point, you have 15 seconds to try an alternative. If you wait longer than 15 seconds, you will lose a fraction of your efficiency bonus just as you would had you entered a trial allocation.

Your “decision bonus” is the amount of cash you earn based upon your performance. Better effort allocation choices generate larger decision bonuses. (Note: Your decision bonus can be negative.) Using trial and error, you can attempt to increase your decision bonus by searching for better ways to allocate your effort. For each allocation you try, the computer will display your "search points", which is the decision bonus associated with the trial. To facilitate your memory, the computer will keep track of your prior attempts as you try different allocations, showing you your last two trials and your best trial. After each try, you must decide whether to continue to search or stop your search. When you chose to stop, the computer records your best trial as your choice, and the decision bonus you actually earn will be the search points associated with this best trial.

To become familiar with the game, you will first be taken through a standard “administrative” game, where you will have the opportunity to experience first hand what you have read here. After completing the administrative game, you may ask questions so as to make sure you understand how the game is played. Then you will play four “practice” games, where the first practice game involves a two task problem, the second practice game involves a three task problem, the third practice game involves a four task problem, and the fourth practice game involves a five task problem. Finally, you will play 20 “real” games, where the number of tasks involved will vary.

Remember, your goal is to maximize your total earnings over the 20 “real” games. Because each game is independent from the others, you will maximize your total earnings by doing as well as you can on each individual game. In each game, you can earn up to about 1 dollar (1 cent wage + 50 cent maximum decision bonus + 49 cent maximum efficiency bonus), meaning you can earn up to about $20 in total.

Good luck!

Figure 1: The Game Screen

Game: 4
Game Type: 5 Tasks
Search Opportunity: 2

	Best	Last	2 Back
Effort Level Y1: 45	45	34	0
Effort Level Y2: 45	45	34	0
Effort Level Y3: 45	45	34	0
Effort Level Y4: 45	45	34	0
Effort Level Y5: 20	20	64	0
Search Points: 7.4	7.4	-46.9	0

Continue Search
Stop Search

Wage:	51.0	
Decision Penalty:	-42.6	
Efficiency Bonus:	47.4	Cost of Next Trial: 1.76
Total Earnings:	55.8	

Table 1: Subject Demographics, Decision Rules, and Performance

Subject Characteristic	Mean	Standard Deviation	Min	Max
Demographic				
age	23	4.3	18	39
gpa	3.00	0.77	0.61	4.00
cred	82.1	47.7	3	179
work	13.7	13.4	0	55
sex	0.62	0.49	0	1
major	0.71	0.46	0	1
marry	0.08	0.27	0	1
finaid	0.40	0.50	0	1
Decision Rules				
SS-rule	0.37	0.49	0	1
C-rule	0.10	0.30	0	1
B-rule	0.73	0.45	0	1
CB-rule	0.17	0.38	0	1

Note: There were 52 observations (subjects) on the demgraphic and decision rule variables and 1038 observations (choices) on the performance variables.

Table 2: Determinants of Total Earnings

Dependent Variable: -log(101-Total Earnings)							
Independent Variable	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
constant	-1.37**	-1.37**	-2.45**	-2.48**	-2.33**	-2.58**	-2.63**
	(.049)	(.048)	(.104)	(.111)	(.113)	(.142)	(.163)
t3	-1.70**	-1.70**	-.76**	-.77**	-.58**	-.80**	-.80**
	(.069)	(.069)	(.079)	(.078)	(.103)	(.079)	(.079)
t4	-2.06**	-2.06**	-1.08**	-1.09**	-.90**	-1.12**	-1.12**
	(.070)	(.070)	(.080)	(.080)	(0.103)	(.080)	(.080)
t5	-2.28**	-2.28**	-1.19**	-1.21**	-1.04**	-1.24**	-1.25**
	(.070)	(.070)	(.085)	(.085)	(.104)	(.086)	(.086)
sd00-14			1.23**	1.23**		1.17**	1.18**
			(.102)	(.101)		(.100)	(.100)
sd14-32			.302**	.311**		.285**	.281**
			(.099)	(.098)		(.098)	(.098)
sd32-44			.172	.165	-0.121	.145	.150
			(.101)	(.100)	(.076)	(.099)	(.099)
sd44-59					-0.317**		
					(.081)		
sd59-80			-.068	-.051	-0.369**	-.086	-.088
			(.104)	(.103)	(.079)	(.103)	(.102)
sd80-108			.006	.021	-0.291**	-.018	.003
			(.111)	(.109)	(.085)	(.108)	(.108)
sd108-155			-.144	-.096	-0.438**	-.102	-.105
			(.109)	(.108)	(.083)	(.106)	(.106)
sd155-262			-.213	-.205	-0.549**	-.195	-.196
			(.114)	(.112)	(.089)	(.111)	(.111)
sd262-489			-.332**	-.352**	-0.668**	-.346**	-.342**
			(.124)	(.122)	(.096)	(.121)	(.121)
sd489+			-.441**	-.397**	-0.752**	-.386**	-.383**
			(.129)	(.129)	(.105)	(.128)	(.128)
exp2				-.078	0.003	-.080	-.079
				(.067)	(.068)	(.068)	(.068)
exp3				.271**	.160*	.270*	.271**
				(.066)	(.065)	(.068)	(.067)
exp4				.010	.039	.011	.012
				(.066)	(0.066)	(.068)	(.068)
exp5				-.066	-0.063	-.071	-.072
				(.068)	(.065)	(.069)	(.069)
gpa						0.067*	0.058*
						(.029)	(.029)
finaid						-0.129**	-0.104*
						(.043)	(.046)
ssrule							0.148**
							(.047)
cbrule							-.054
							(.089)
brule							.035
							(.075)
Fixed Effects?	No	Yes	Yes	Yes	Yes	No	No
Number of Observations:	1009	1009	1009	1009	717	1009	1009
R^2	0.5683	0.5683	0.6752	0.6863	0.3227	0.6912	0.6944
R^2 Including the Fixed Effects		0.5858	0.6969	0.7084	0.3635		
Variance explained by fixed effect.		0.0707	0.0944	0.0971	0.1705		
[p value] for fixed effect F-test		0.0168	0.0000	0.0000	0.0000		

**=significant at 1%
*=significant at 5%

Table 3: Determinants of Total Earnings by Complexity Level
Fixed Effect Model versus No Fixed Effect Model With Exogenous Characteristics

Independent	2 Tasks		3 Tasks		4 Tasks		5 Tasks	
Variable	FE	No FE	FE	No FE	FE	No FE	FE	No FE
constant	-1.83**	-2.20**	-3.22**	-3.40**	-3.67**	-3.84**	-3.81**	-3.83**
	(.565)	(.611)	(.149)	(.268)	(.128)	(.216)	(.124)	(.213)
sd00-14	.57	.82	.96**	.86**	.90**	.87**	1.16**	1.18**
	(.553)	(.532)	(.168)	(.167)	(.176)	(.157)	(.202)	(.184)
sd14-32	-.62	-.25	.11	-.011	.23	.30*	.52**	.50**
	(.574)	(.543)	(.156)	(.156)	(.148)	(.136)	(.175)	(.155)
sd32-44	-.48	-.18	.13	.005	.02	.04	.31*	.24
	(.620)	(.590)	(.158)	(.157)	(.171)	(.155)	(.137)	(.127)
sd44-59								
sd59-80	-.84	-.59	-.01	-.17	-.005	.009	.01	-.03
	(.651)	(.621)	(.168)	(.168)	(.151)	(.138)	(.155)	(.144)
sd80-108	.49	.330	.15	.04	.03	.06	.17	.08
	(1.08)	(1.03)	(.185)	(.183)	(.159)	(.147)	(.154)	(.139)
sd108-155			-.12	-.20	-.12	-.09	.16	.03
			(.164)	(.162)	(.157)	(.144)	(.176)	(.151)
sd155-262			-.16	-.23	-.12	-.07	-.10	-.02
			(.188)	(.183)	(.173)	(.150)	(.180)	(.161)
sd262-489			-.07	-.12	-.23	-.20	-.03	-.18
			(.217)	(.202)	(.184)	(.161)	(.204)	(.181)
sd489+			.25	.47	.06	.15	-.22	-.21
			(.348)	(.341)	(.215)	(.192)	(.184)	(.166)
exp2	-.27	-.27	.24	.26	.23*	.21*	-.30*	-.32**
	(.161)	(.169)	(.127)	(.136)	(.109)	(.107)	(.126)	(.121)
exp3	.58**	.58**	-.002	.023	-.07	-.09	.60**	.58**
	(.161)	(.169)	(.125)	(.134)	(.121)	(.116)	(.105)	(.104)
exp4	-.06	-.06	.006	.031	.37**	.35**	.02	.02
	(.195)	(.200)	(.115)	(.126)	(.131)	(.122)	(.099)	(.100)
exp5	-.03	-.03	-.15	-.14	.18	.17	-.36*	-.36*
	(.165)	(.173)	(.114)	(.125)	(.105)	(.105)	(.151)	(.143)
gpa		.055		.014		.083		.036
		(.074)		(.054)		(.043)		(.047)
finaid		.018		-.14**		-.23**		-.11
		(.117)		(.088)		(.068)		(.071)
ssrule		.034		.30**		.13		.16*
		(1.20)		(.088)		(.070)		(.072)
cbrule		-.27		.19		-.20		-.02
		(.227)		(.166)		(.130)		(.137)
brule		-.03		.17		-.03		-.06
		(.193)		(.141)		(.110)		(.115)
Number of Observations:	260	260	258	258	252	252	239	239
R^2	0.2600	0.2731	0.2767	0.3486	0.3875	0.4518	0.5435	0.5729
R^2 With Fixed Effects	0.3262		0.3531		0.4620		0.6123	
Variance explained by fixed effect.	0.2340		0.3409		0.2638		0.2291	
[p value] for fixed effect F-test	0.0257		0.0000		0.0200		0.1121	

**=significant at 1%
*=significant at 5%

Table 4: Effects on Decision to Stop Searching

Independent Variable	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
t3	0.22**	0.86	0.85	0.87	0.77	0.76	0.90	0.90
	(0.02)	(0.11)	(0.11)	(0.09)	(0.15)	(0.15)	(0.10)	(0.10)
t4	0.18**	1.26	1.25	1.04	0.93	0.94	1.12	1.14
	(0.02)	(0.18)	(0.18)	(0.13)	(0.19)	(0.19)	(0.13)	(0.14)
t5	0.14**	1.58**	1.56**	1.17	1.04	1.07	1.27	1.34*
	(0.02)	(0.23)	(0.23)	(0.15)	(0.21)	(0.22)	(0.16)	(0.17)
sm00-06		14.94**	14.83**	8.02**	7.87**	8.26**	8.46**	9.37**
		(2.82)	(2.81)	(1.28)	(1.34)	(1.41)	(1.36)	(1.51)
sm06-12		5.58**	5.53**	3.48**	3.11**	3.23**	3.56**	4.06**
		(1.03)	(1.02)	(0.56)	(0.53)	(0.55)	(0.58)	(0.66)
sm12-19		2.80**	2.79**	2.52**	2.52**	2.57**	2.51**	2.67**
		(0.53)	(0.52)	(0.42)	(0.43)	(0.43)	(0.42)	(0.45)
sm19-25		2.09**	2.07**	1.82**	1.78**	1.80**	1.84**	1.90**
		(0.41)	(0.41)	(0.32)	(0.31)	(0.31)	(0.32)	(0.33)
sm24-33		1.39	1.38	1.58**	1.54*	1.54*	1.58**	1.56*
		(0.27)	(0.27)	(0.28)	(0.27)	(0.27)	(0.28)	(0.28)
sm33-41								
sm41-53		0.75	0.75	0.94	0.96	0.96	0.94	0.96
		(0.17)	(0.17)	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)
sm53-74		0.37**	0.37**	0.53**	0.55*	0.55*	0.53**	0.52**
		(0.10)	(0.10)	(0.12)	(0.13)	(0.13)	(0.12)	(0.12)
sm74-118		0.21**	0.21**	0.28**	0.30**	0.30**	0.29**	0.29**
		(0.07)	(0.07)	(0.08)	(0.09)	(0.09)	(0.09)	(0.09)
sm118+		0.09**	0.09**	0.11**	0.14**	0.14**	0.11**	0.11**
		(0.04)	(0.04)	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)
exp2			0.98	1.06	1.08	1.08	1.04	1.04
			(0.11)	(0.11)	(0.13)	(0.13)	(0.11)	(0.11)
exp3			1.11	1.21	1.21	1.21	1.21	1.24*
			(0.13)	(0.12)	(0.15)	(0.15)	(0.12)	(0.13)
exp4			1.08	1.09	1.07	1.05	1.09	1.10
			(0.12)	(0.11)	(0.13)	(0.13)	(0.11)	(0.11)
exp5			1.05	1.16	1.15	1.16	1.19	1.19
			(0.12)	(0.12)	(0.14)	(0.14)	(0.12)	(0.12)
group							1.25**	1.35**
							(0.08)	(0.09)
Fixed Effect 2 (iv)				1.75**	1.36			
				(0.26)	(0.24)			
Fixed Effect 2 (v)						0.87		
						(0.14)		
ssrule								1.26**
								(0.09)
brule								0.71**
								(0.08)
cbrule								0.35**
								(0.05)
Stratified Sample?	Yes	Yes	Yes	No	No	No	No	No
Observations	8155	8155	8155	8155	7339	7339	8155	8155
Number of Problems	1038	1038	1038	1038	717	717	1038	1038
Log Likelihood	-2098	-1735	-1734	-5703	-3911	-3912	-5704	-5663
LR Chi^2	335.83	1061.18	1062.87	939.19	505.77	503.60	936.52	1019.93
LR Test p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

**=significant at 1%

USC LAW SCHOOL WORKING PAPER SERIES

2000

00-1 W. Bentley MacLeod, *Complexity and Contract* (forthcoming, REVUE D'ECONOMIE INDUSTRIELLE, April 2000).

00-2 Jennifer Arlen, Matthew Spitzer and Eric Talley, Endowment Effects, Other-Regarding Preferences, and Corporate Law.

00-3 Christopher Slobogin, *An End to Insanity: Recasting the Role of Mental Disability in Criminal Cases* (forthcoming in the VIRGINIA LAW REVIEW).

00-4 Christopher Slobogin, *Mental Illness and the Death Penalty*, 1 CALIFORNIA CRIMINAL LAW REVIEW 3 (2000).

00-5 R. Michael Alvarez and Edward J. McCaffery, "Is There a Gender Gap in Fiscal Political Preferences?"

00-6 Ariela Gross, "Between 'Race' and 'Nation': Indian/Black Identity in the Southern Courtroom, 1780-1840."

00-7 Peter H. Huang, *Reasons within Passions: Emotions and Intentions in Property Rights Bargaining*, (forthcoming in 79 OREGON LAW REVIEW, November 2000).

00-8 Marcel Kahan and Ehud Kamar, "Price Discrimination in the Market for Corporate Law."

00-9 Dan Klerman and Nuno Garoupa, "Optimal Law Enforcement with a Rent-Seeking Government."

00-10 Dan Klerman, "The Selection of Thirteenth-Century Criminal Disputes for Litigation."

00-11 W. Bentley MacLeod, "Cognition and the Theory of Learning by Doing."

00-12 W. Bentley MacLeod and Mark Pingle, "An Experiment on the Relative Effects of Ability, Temperament and Luck on Search with Uncertainty."

00-13 Daniel Klerman, "Female Prosecutors in Thirteenth-Century England."

1999

99-1 Howard F. Chang and Hilary Sigman, *Incentives to Settle Under Joint and Several Liability: An Empirical Analysis of Superfund Litigation* (forthcoming in 29 JOURNAL OF LEGAL STUDIES (January 2000)).

99-2 Mary L. Dudziak, The March on Washington, At Home and Abroad.

99-3 Pamela M. Kato, Thomas D. Lyon, John Flavell, Raquel S. Klibanoff, Robin Higashi, and Lynne C. Huffman, Preschoolers' Moral Judgments About Illness and Treatment: Who's Bad?

99-4 Thomas D. Lyon, *Child Witnesses and the Oath: Empirical Evidence* (forthcoming in SO. CAL. LAW REV. (2000)).

99-5 Paul Mahoney and Mark Weinstein, The Appraisal Remedy and Merger Premiums.

99-6 Eric Talley, *Precedential Cascades: An Appraisal*, 73 SO. CAL. L. REV. 87 (1999).

99-7 Eric Talley, *Taking the "I" Out of "Team": Intra-Firm Monitoring and the Content of Fiduciary Duties*, 24 J. CORP. LAW 1001 (1999).

99-8 Antonio Bernardo, Eric Talley, and Ivo Welch, *A Theory of Legal Presumptions*, 16 J. LAW, ECON. & ORG. 1 (2000).

99-9 Antonio Bernardo and Eric Talley, A Note on Presumptions with Sequential Litigation.

99-10 W. Bentley MacLeod and Daniel Parent, Job Characteristics and the Form of Compensation.

99-11 R. Michael Alvarez and Edward J. McCaffery, *Gender and Tax* (forthcoming in GENDER AND POLITICS, Jyl Josephson and Susan Tolleson-Rinehart, eds.).

99-12 Daniel Klerman, *Settlement and the Decline of Private Prosecution in Thirteenth-Century England* (forthcoming in 19 LAW & HISTORY REVIEW (2001)).

99-13 George Lefcoe, Mortgage Prepayment by Defeasance.

99-14 Edward J. McCaffery, *The Burdens of Benefits* (forthcoming in the VILLANOVA LAW REVIEW).

99-15 Edward J. McCaffery, A Life Estate Conception of Property.

99-16 Edward J. McCaffery, *The Missing Links in Tax Reform* (forthcoming in the CHAPMAN LAW REVIEW).

99-17 Alexander M. Capron, *Genetics and Insurance: Accessing and Using Private Information*, 17 SOCIAL PHILOSOPHY AND POLICY FOUNDATION 235 (Summer 2000).

99-18 Alexander Morgan Capron, *Social Science, Bioethics, and the Law: What Contributions to Policy Development?* 128 DAEDALUS (JOURNAL OF THE AMERICAN ACADEMY OF ARTS AND SCIENCES) 295 (Fall 1999).

99-19 Lorne Carmichael and W. Bentley MacLeod, Caring About Sunk Costs: A Behavioral Solution to Hold-up Problems with Small Stakes.

99-20 David B. Cruz, *"The Sexual Freedom Cases"? Contraception, Abortion, Abstinence, and the Constitution*. (A revised version of this paper is forthcoming in 35 HARVARD CIVIL RIGHTS-CIVIL LIBERTIES LAW REVIEW, No. 2, Summer 2000).

99-21 Gregory C. Keating, Fairness and Two Fundamental Questions in the Tort Law of Accidents.

99-22 Kevin Davis, Self-Interest and Altruism in the Deterrence of Transnational Bribery.

99-23 Thomas D. Lyon, *Expert Testimony on the Suggestibility of Children: Does it Fit?* (forthcoming in B.L. Bottoms, M.B. Kovera & B.D. McAuliff (eds.), CHILDREN AND THE LAW: SOCIAL SCIENCE AND POLICY (New York: Cambridge)).

99-24 Thomas D. Lyon, *Questioning Children: The Effects of Suggestive and Repeated Questioning* (forthcoming in J. Conte (ed.)., SUGGESTIBILITY OF CHILDREN AND ADULTS (Thousand Oaks, CA: Sage)).

99-25 Thomas D. Lyon, Karen J. Saywitz, Debra L. Kaplan, and Joyce S. Dorado, *Reducing Maltreated Children's Reluctance to Answer Hypothetical Oath-Taking Competence Questions*. (Revised version of Olin Working Paper No. 98-9). (Forthcoming in LAW & HUMAN BEHAVIOR).

1998

98-1 Cindy Alexander, Jennifer Arlen and Mark Cohen, *Regulating Corporate Criminal Sanctions: Evidence on the Effect of the U.S. Sentencing Guidelines,* 42 JOURNAL OF LAW & ECONOMICS 271 (1999).

98-2 Jennifer Arlen, *Economic Analysis of Tort Damages: A Survey* (forthcoming in the ENCYCLOPEDIA OF LAW AND ECONOMICS).

98-3 Howard F. Chang, *Migration as International Trade: The Economic Gains from the Liberalized Movement of Labor*, 3 UCLA JOURNAL OF INTERNATIONAL LAW AND FOREIGN AFFAIRS 371 (Fall/Winter 1998-99).

98-4 Mary L. Dudziak, Birmingham, *Addis Ababa and the Image of America: Managing the Impact of Foreign Affairs on Civil Rights in the Kennedy Administration* (forthcoming in Brenda Gayle Plummer, ed., AMERICA'S DILEMMA, University of North Carolina Press).

98-5 Ariela J. Gross, *Litigating Whiteness: Trials of Racial Determination in the Nineteenth Century South* (forthcoming in 108 YALE LAW JOURNAL (1998)).

98-6 James R. Hackney, Jr., Law and Neoclassical Economics Theory: Structure, Development, Dissolution and Resolution(?).

98-7 Daniel Klerman, Private Prosecution of Crime in Thirteenth-Century England: The Importance of Settlement and Female Prosecutors.

98-8 Michael S. Knoll, Tax Planning, Effective Tax Rates and the Structure of the Income Tax.

98-9 Thomas D. Lyon and Karen J. Saywitz, *Young Maltreated Children's Competence to Take the Oath*, 3 APPLIED DEVELOPMENTAL SCIENCE 16 (1999).

98-10 Thomas D. Lyon, *The New Wave in Children's Suggestibility Research: A Critique*, 84 CORNELL LAW REVIEW 1004 (1999).

98-11 Eric L. Talley, A Model of Hierarchical Judicial Auditing.

98-12 Charles D. Weisselberg, *Saving Miranda* (forthcoming in 84 CORNELL L. REV. (1998)).

98-13 Scott Altman, A Theory of Child Support.

98-14 Jennifer Arlen, *Comment: The Future of Behavioral Economic Analysis of Law* (forthcoming in VANDERBILT LAW REVIEW, Symposium issue (1998)).

98-15 Susan Athey, Kyle Bagwell, and Chris William Sanchirico, Collusion and Price Rigidity.

98-16 Michael S. Knoll, Hedging in an Economy with Asymmetric Taxes: A Comment on Moshe Ayre Milevsky & Eliezer Z. Prisman, *Hedging and Pricing with Tax Uncertainty: Managing Under an Arkansas Best Doctrine* (forthcoming in the ANNUAL PROCEEDINGS OF THE CHICAGO BOARD OF TRADE).

98-17 Nancy S. Marder, *The Myth of the Nullifying Jury* (forthcoming in 93 NORTHWESTERN LAW REVIEW).

98-18 Alexander S. P. Pfaff and Chris William Sanchirico, Environmental Self-Auditing: Setting the Proper Incentives for Discovering and Correcting Environmental Harm.

98-19 Chris William Sanchirico, Enforcement by Hearing: An Integrated Model of Evidence Production.

98-20 Chris William Sanchirico, Games, Information and Evidence Production: With Application to Legal History and "Decoupling."

98-21 Chris William Sanchirico, Taxes Versus Legal Rules as Instruments for Equity: A More Equitable View.

98-22 Matt Spitzer and Eric Talley, *Judicial Auditing*, 29 J. LEGAL STUD. 649 (2000).

1997

97-1 Scott Altman, *Should Child Custody Rules be Fair?*, 35 U. OF LOUISVILLE J. OF FAMILY LAW 325 (1997).

97-2 Jennifer Arlen, *Corporate Crime and Its Control*, THE NEW PALGRAVE DICTIONARY OF ECONOMICS AND THE LAW, Peter Newman (ed.), Vol. 1, p. 492 (1998)

97-3 Michael S. Knoll, *Products Liability and Legal Leverage: The Perverse Effects of Stiff Penalties*, 45 UCLA LAW REVIEW 99 (1997).

97-4 Paul G. Mahoney, *Contract Remedies: A Survey and Critique* (forthcoming in the ENCYCLOPEDIA OF LAW AND ECONOMICS).

97-5 Paul G. Mahoney, The Stock Pools and the Securities Exchange Act.

97-6 Jennifer Arlen and Reinier Kraakman, *Controlling Corporate Misconduct: An Analysis of Corporate Liability Regimes*, 72 NYU LAW REVIEW 687 (1997).

97-7 Lucian Arye Bebchuk and Howard F. Chang, *The Effect of Offer-of-Settlement Rules on the Terms of Settlement* (forthcoming in 28 JOURNAL OF LEGAL STUDIES 489 (June 1999)).

97-8 Linda S. Beres and Thomas D. Griffith, *Do "Three Strikes" Laws Make Sense? Habitual Offender Statutes and Criminal Incapacitation* (forthcoming in the GEORGETOWN LAW JOURNAL).

97-9 Thomas D. Lyon, *Scientific Support for Expert Testimony on Child Sexual Abuse Accommodation Syndrome* (forthcoming in Jon Conte, ed., THE KNOWNS AND UNKNOWNS OF CHILD SEXUAL ABUSE).

97-10 Judith Resnik, *Changing Practices, Changing Rules: Judicial and Congressional Rulemaking and Civil Juries, Civil Justice, and Civil Judging,* ALABAMA LAW REVIEW (forthcoming 1998).

97-11 Matthew L. Spitzer, *The Economics of Freedom of Expression* (forthcoming in the NEW PALGRAVE DICTIONARY OF ECONOMICS AND THE LAW).

97-12 Christopher D. Stone, *Too Many Fishing Boats, Too Few Fish: Can Trade Laws Trim Subsidies and Restore the Balance in Global Fisheries?* 24 ECOLOGY LAW QUARTERLY 505 (1997).

97-13 Eric Talley, *Interdisciplinary Gap-Filling: A Review of Game Theory and the Law*, J. OF LAW AND SOCIAL INQUIRY (forthcoming 1997).

97-14 H. Lorne Carmichael and W. Bentley MacLeod, Fair Territory: Preferences, Bargaining, and the Endowment Effect.

97-15 Ronald A. Cass, Richard D. Boltuck, Seth T. Kaplan and Michael S. Knoll, Antidumping.

97-16 Mary L. Dudziak, *The Little Rock Crisis and Foreign Affairs: Race, Resistance and the Image of American Democracy*, 70 SOUTHERN CALIFORNIA LAW REVIEW 1641 (1997).

97-17 Michael S. Knoll, Financial Innovation, Tax Arbitrage, and Retrospective Taxation: The Problem with Passive Government Lending.

97-18 Michael S. Knoll, The Principle of Marginal Deterrence in Torts: The Potentially Perverse Effects of Stiffer Tort Penalties and Higher Taxes.

97-19 George Lefcoe, How Buyers and Sellers of Development Land Deal with Regulatory Risk.

97-20 Thomas D. Lyon, *Are Battered Women Bad Mothers? Rethinking the Termination of Abused Women's Parental Rights for Failure to Protect,*in H. Dubowitz, ed., NEGLECTED CHILDREN: RESEARCH, PRACTICE AND POLICY (Thousand Oaks: Sage Publications, 1999, Chapter 12, pp. 237-260).

97-21 Thomas D. Lyon and Jonathan J. Koehler, *Where Researchers Fear to Tread: Interpretive Differences Among Testifying Experts in Child Sexual Abuse Cases*, in S. Ceci and H. Hembrooke, eds., WHAT CAN (AND SHOULD) BE SAID IN COURT: EXPERT WITNESSES IN CHILD ABUSE CASES (American Psychological Association: Washington, DC, 1998, Chapter 13, pp. 249-263).

97-22 W. Bentley MacLeod, Complexity, Contract and the Employment Relationship.

97-23 W. Bentley MacLeod and James M. Malcomson, Motivation and Markets.

97-24 Eric L. Talley, *Turning Servile Opportunities to Gold: A Strategic Analysis of the Corporate Opportunities Doctrine* (forthcoming in 108 YALE LAW JOURNAL (1998)).

1996

96-1 Antonio E. Bernardo and Eric L. Talley, *Investment Policy and Exit-Exchange Offers Within Financially Distressed Firms*, 51 J. FINANCE 871 (1996).

96-2 Howard F. Chang, *Carrots, Sticks, and International Externalities*, 17 INT'L REV. OF LAW & ECON. 309 (1997).

96-3 Howard F. Chang, *Liberalized Immigration as Free Trade: Economic Welfare and the Optimal Immigration Policy*, 145 U. PA. L. REV. 1147 (1997).

96-4 Erwin Chemerinsky & Catherine Fisk, *The Filibuster*, 49 STANFORD L. REV. 181 (1997).

96-5 Susanne Lohmann and Hugo Hopenhayn, *Delegation and the Regulation of Risk* (forthcoming in GAMES AND ECONOMIC BEHAVIOR).

96-6 Michael S. Knoll, *Products Liability and Legal Leverage: The Perverse Effects of Stiff Penalties*, 45 UCLA LAW REVIEW 99 (1997).

96-7 Susanne Lohmann, *Demosclerosis, or Special Interests 'R' Us: An Informational Rationale for Political Gridlock*, in Michelle R. Garfinkel and Stergios Skaperdas, eds., THE POLITICAL ECONOMY OF CONFLICT AND APPROPRIATION (Cambridge: Cambridge University Press, 1996, Chapter 7, pp. 119-130).

96-8 Susanne Lohmann, Electoral Incentives, Informational Asymmetries, and the Policy Bias Toward Special Interests.

96-9 Susanne Lohmann, *Federalism and Central Bank Autonomy: The Politics of German Monetary Policy, 1957-1992* (forthcoming in 51 WORLD POLITICS, April 1998).

96-10 Thomas D. Lyon & Jonathan J. Koehler, *The Relevance Ratio: Evaluating the Probative Value of Expert Testimony in Child Sexual Abuse Cases*, 82 CORNELL L. REV. 43 (1996).

1995

95-1 Ron Garet, *Deposing Finnis*, 4 SO. CAL. INTERDISCIPLINARY L. J. 605 (1995).

95-2 Michael S. Knoll, *An Accretion Corporate Income Tax*, 49 STANFORD L. REV. 1 (1996).

95-3 Michael S. Knoll, *A Primer on Prejudgment Interest*, 75 TEXAS L. REV. 293 (1996).

95-4 Mark Weinstein, *Profit Sharing Contracts in Hollywood: Evolution and Analysis*, 27 J. OF LEGAL STUDIES 23 (1998).

1994

94-1 Scott Altman, *Lurking in the Shadow*, 68 SO. CAL. L. REV. 493 (1995).

94-2 Jennifer Arlen and Deborah Weiss, *A Political Theory of Corporate Taxation*, 105 YALE L.J. 325 (1995).

94-3 Lucian Arye Bebchuk and Howard F. Chang, *An Analysis of Fee-Shifting Based on the Margin of Victory: On Frivolous Suits, Meritorious Suits, and the Role of Rule 11*, 25 J. OF LEGAL STUDIES 371 (1996).

94-4 Howard F. Chang, *An Economic Analysis of Trade Measures to Protect the Global Environment*, 83 GEORGETOWN LAW J. 2131 (1995).

94-5 Howard F. Chang, *Patent Scope, Antitrust Policy, and Cumulative Innovation*, 26 RAND J. OF ECON. 34 (1995).

94-6 Erwin Chemerinsky, *The Fifty-Fifth Cleveland-Marshall Fund Lecture: When the Government Must Make Content-Based Choices*, 42 CLEVE. ST. L. REV. 199 (1994).

94-7 Erwin Chemerinsky, *The Values of Federalism*, FLORIDA L. REV. (forthcoming).

94-8 Jeffrey A. Dubin and Matthew L. Spitzer, *Testing Minority Preferences in Broadcasting*, 68 SO. CAL. L. REV. 841 (1995).

94-9 Ronald R. Garet, *Gnostic Due Process*, 7 YALE J. LAW & HUMANITIES 97 (1995).

94-10 Gregory Keating, *Reasonableness and Rationality in Tort Theory*, 48 STANFORD L.REV. 501 (1996).

94-11 Michael S. Knoll, *A Primer on Prejudgment Interest*, 75 TEXAS L. REV. 293 (1996).

94-12 Michael S. Knoll, Put-Call Parity and the Development of the Modern Mortgage.

94-13 Michael S. Knoll, Socially Responsible Investment and Modern Financial Markets.

94-14 Elyn R. Saks, Interpreting Interpretation: The Limits of Hermeneutic Psychoanalysis (forthcoming in her book on Hermeneutics, Yale University Press, 1997).

94-15 Pablo Spiller and Matthew L. Spitzer, *Where is the Sin in Sincere? Sophisticated Manipulation of Sincere Judicial Voters (with applications to other voting environments)*, 11 J. LAW, ECON. & ORGAN. 32 (1995).

94-16 Christopher D. Stone, *What to Do About Biodiversity: Property Rights, Public Goods and the Earth's Biological Riches,* 68 SO. CAL. L. REV. 577 (1995).

94-17 Charles D. Weisselberg, *The Exclusion and Detention of Aliens: Lessons From the Lives of Ellen Knauff and Ignatz Mezei*, 143 UNIV. PENN. L. REV. 933 (1995).