Essays on Boundedly Rational Choice

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⋆ Dedication ⋆

This thesis is dedicated to both my parents. My father, the Late Tapas Das did not only raise and nurture me but also taxed himself dearly over the years for my education and intellectual development. He was not only my teacher, He was/ is my inspiration, motivation and strength. My mother, Smt. Soma Das has been a source of motivation and strength during moments of despair and discouragement. Her motherly care and support have been shown in incredible ways always. Without my parents none of my success would be possible.

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Contents

Chapter 1

Introduction

Decision theory or the theory of choice is the analysis of individual behavior, typically in non-interactive situations. We can conceptualize two types of decision theory - normative and descriptive. A normative theory is concerned with identifying the best decision to make, modeling a decision maker who comports to certain ideals. A descriptive theory is a theory about how decisions are made. Such a theory is concerned with explaining observed behavior or predicting behavior under the assumption that the decision-maker or decision process follows some rules. The predictions about behavior that descriptive theory produces allow further tests of the assumed underlying, and unobservable, decision-making rules.

The concept of rationality occupies a central position in decision theory. A rational decision-maker is an individual with a consistent preference structure, given some definition of consistency. Rational choice theory (RCT) therefore is concerned with the decisions and behavior of rational individuals. RCT assumes that any individual has a preference structure over the available choice alternatives that allows him/her to determine which option is preferred. A rational agent is assumed to take account of available information, probabilities of events, and potential costs and benefits in forming preferences, and to act consistently in choosing the self-determined best alternative. Typical assumptions related to preferences which enable the constitution of the idea of consistency include completeness and transitivity.

Rationality is widely used as an assumption regarding the behavior of individuals in microeconomic models and analyses, and RCT can be said to be an integral part of the currently dominant theoretical approach in microeconomics. Explicit theories of rational economic choices began to get developed in the late 19^{th} century. These theories commonly linked choice of an object to the increase in happiness or satisfaction or utility an increment of this object would bring; classical economists like Jevons for example held that agents make consumption choices so as to maximize their own happiness (see e.g. Grüne-Yanoff [45]). However there has been increasing dissociation in economics through the course of the late 20^{th} and early 21^{st} centuries of happiness or related concepts from the ambit of RCT. The theory now focuses on rationality as the maintenance of a consistent ranking of alternatives, and not so much on the explication of the rationality of choices resulting from an effort to maximize happiness.

In sum therefore the basic premises of RCT are *i)* human beings base their behavior on rational calculation, *ii)* they act with rationality when making choices, and *iii)* their choices are aimed at optimization of their pleasure or profit. The theory is powerful and elegant and widely used. At the same time, RCT has many critics who point, for example, to the fact that RCT cannot easily explain the existence of many phenomena such as altruism, reciprocity and trust, and why individuals voluntarily join associations where collective and not individual benefits are pursued. An important line of criticism focuses on the notion of preference consistency, the claim being that consistency is unlikely to hold in practice given that in reality information processing capacities are not unlimited, and knowledge is not perfect.

These criticisms have given rise to alternatives to RCT. A particularly important such alternative is what is called 'boundedly rational' choice theory (BRCT). The idea is that bounded rationality gives psychologically more plausible models of human decision-making without hurting the notion of rationality altogether.

Bounded rationality conceptualizes decision makers as working under three unavoidable constraints - *i)* only limited, often unreliable, information is available regarding possible alternatives and their consequences, *ii)* the human mind has only a limited capacity to evaluate and process information that is available, *iii)* only a limited amount of time is available to make decisions. Therefore even individuals who intend to make rational choices are not bound to do so in complex situations. According to Simon [89](Pg. 266), the point of bounded rationality is to

"...designate rational choice that takes into account the cognitive limitations of the

decision-maker - limitations of both knowledge and computational capacity."

Simon's theory was largely directed at finding an adequate formal characterization of rationality. Other authors espousing similar aims such as Rubinstein (see e.g. [82]) have proposed to model bounded rationality by explicitly specifying decision-making procedures. Alternative approaches to bounded rationality have considered how decisions may be weakened by the limitations on rationality. Gigerenzer, for example (see e.g. [38] and [39]), has proposed that focus should be on informal heuristics, which often lead to better decisions. Dixon similarly (see e.g. [29]) has proposed the notion of epsilon-optimization, which helps decision-makers to choose an action that gets them 'close' to the optimum, arguing that it may not be necessary to analyze in detail the process of reasoning underlying bounded rationality. Nowadays bounded rationality has acquired a more general meaning (see e.g. Klaes and Sent [59]), concentrating on cognitive, informational and other limitations that RCT ignores.

Experimental research has long made significant contribution to the understanding of boundedly rational decision-making (see e.g. Pruitt [79], Selten and Berg [87] and Tietz and Weber [92]). Experimental economics is the application of experimental methods to study economic questions. Data collected in experiments are used to estimate effect size, test the validity of economic theories and illuminate the workings of economic mechanisms. Economic experiments usually use monetary incentives to motivate subjects in order to copy real world incentives. A basic aspect is design of the experiment. Experiments may be conducted in field or in laboratory settings, on issues related to individual or group behavior. Economic experiments are now generally used in a wide set of areas - markets, evolutionary game theory, games, decision making, bargaining, auctions, coordination, social preferences, learning, matching etc.

Experimental analysis of individual decision making in economic contexts is usually dated from the work of Thurstone [91], who first examined ordinal utility theory. The range of experimental investigations into choice or decision theory started expanding shortly after the arrival of the work of von Neumann and Morgenstern [97]. This work presented and brought to vast attention a more powerful theory of individual choice. The predictions of expected utility theory gave a new focus to experiments concerned with individual choice. Early experimental analyses such as Preston and Baratta [78] and Mosteller and Nogee [72] for example argued that the von Neumann-Morgenstern expected utility functions are derived from assumptions about individual choice behavior and that laboratory experimentation provides an opportunity to look at the behavior unconfounded by other considerations. For the last 30 years most research on individual decision-making has taken normative theories of choice as null hypotheses about behavior and tested those hypotheses experimentally, the aim being to test whether normative rules are systematically violated and to suggest alternative theories to explain any observed violations.[1]

Comprehensive experiments were designed in the 1980s to test several theories at once. The efficiency of these designs is impressive and could serve as a model for researchers in other areas of how to test several theories which can all explain some basic phenomenon but can be distinguished by careful designs. The first such paper is Chew and Waller [23], who used an ingenious design suggested in Chew and MacCrimmon [24]. Their basic idea, extending the approach of Allais [2], was to find sets of pairwise choices such that different theories predicting certain choice patterns would or would not be supported. Many others such as Loomes and Sugden [64] and Camerer [18], [19] used this "pattern paradigm" and extended it. Because there were many data sets but few clear conclusions, Harless and Camerer [47] showed one statistical way that data from many different experiments with different choice patterns could be "added up" to draw robust conclusions. The general results show that expected utility violations were systematic and replicable, but violations of some of the new theories could be generated as well.

My research analyzes individual decision making in different environments. I conducted laboratory as well as field experiments, either hand run or computerized, usually using the Ztree software. My thesis has four chapters, of which two focus on risky environments and two focus on deterministic yet cognitively challenging environments. The connecting theme across the four chapters is an examination of the edges of RCT.

The thesis is primarily a descriptive exercise, focussed around explaining how decisions are made through analysis of observed choices. In each chapter I take an aspect of the decision

[1] We know that the main purpose of rational choice theory is to lay out in clear and transparent terms what conditions are necessary and/or sufficient for the validity of statements about consistent human behavior. Schotter [4] showed in this context that strong criteria for rationality are 'wrong' if understood as positive descriptions.

making environment that is outside the domain of standard RCT and examine the impact of its presence on choice. Since these aspects are not considered to be among the necessary conditions framing any problem by standard theory, the theory by implication predicts that choice will be invariant to any modification in any of them. This implicitly induces a consistency criterion for any of the aspects considered. A primary motivation for this thesis is to provide tests of such criteria in an attempt to assess their empirical validity. If any criterion is satisfied in the laboratory, this would provide support to RCT and its descriptive power. A failure of the criterion on the other hand would suggest it may not always be easy to extend RCT beyond its usual confines to make it more relevant and applicable, and that BRCT may provide a more fruitful avenue toward a comprehensive framework for a theory of choice with predictive power.

The second chapter, *"Are Contingent Choices Consistent ?"* discusses whether individuals behave consistently with respect to contingent planning. A contingent plan is a vector of choices, one for each contingency or potential choice problem, should that problem be actualized. A contingent plan is consistent if the specification for any particular contingency in the plan is invariant to the set of alternative contingencies or, equivalently, is independent of irrelevant information emerging from alternative contingencies or choice problems. From the standpoint of rationality, one could expect contingent plans to be consistent. This is because each contingency is viewed as a fully specified, standard choice problem and theory assumes that a predictive framework can in principle be constructed for a given choice problem if data were available on the feasible set of options, and the preference ordering, and perhaps probability distributions, over outcomes, for the problem under consideration. This implies that information arriving from other alternative contingencies is irrelevant for decision with regard to any other contingency, and hence cannot affect choice for it. The argument outlined above induces a consistency criterion which can be called consistency of contingent plans or independence of irrelevant information. The chapter reports results from two laboratory experiments designed to test the empirical validity of this criterion. Our two experiments developed two different environments. One experiment had salient choice and subject chose allocations in financial options with fully specified outcomes and probabilities, while choice was hypothetical in the other, with subjects confronting a variety of everyday goods, durables, activities, assets and services, with a few features given in each case. Our broad finding was that consistency was more likely to obtain when problems were complete (fully specified outcomes, probabilities etc.) but may fail in more complex settings.

The third chapter, *"The Impact of Past Outcomes on Choice in a Cognitively Demanding Financial Environment"* investigated through a laboratory experiment whether performance in a cognitively demanding financial tasks can depend on prior outcome from independent and unrelated financial tasks. Standard theory argues that prior outcome cannot affect future choice in such settings. Findings from two recent strands of literature have hinted however that such a link may be possible in cognitively demanding environments. One of these explores the link between cognitive capacity and quality of financial choice and shows a positive association may exist and other examines the link between economic circumstance and cognitive capacity and finds there may be causal dependence. Together, these imply that economic circumstance determined by past outcomes can affect the quality of financial choice in cognitively demanding financial environments. The issue assumes particular importance in arenas where better financial choice plays a large role in determining future economic circumstance, such as the financial industry, or the informal or unorganized sector. We test this proposition directly through an experiment in this chapter, with past outcomes generated endogenously in the laboratory. The experiment was in two parts. The first part confronted subjects with a sequence of binary lottery choice problems and generated a controlled history of financial outcome. Subjects had to allocate a budget across two deterministic options in the second part. One of these was a simple option with a linear return function, and the other had a non-linear and patternless return function, and was hence complex. This feature made the task in the second part cognitively demanding. We found support for the proposition that past outcome can affect future choice, with a superior or more positive history of past outcomes enhancing cognitive performance. We also found some evidence that the performance of some section of the population, those with weaker performance, may be immune to personal history of outcome.

The fourth chapter *"The Impact of a Deadline with Decision under Risk"* examines the effect of a deadline in a risky decision environment. The conventional theoretical description of decision making does not leave room for a deadline. This might leave the impression that theory suggests deadlines will have no impacts. If one assumes that decision-making is not instantaneous and requires cognitive resources, then a deadline can have at least two impacts. One, it can cause a reallocation of cognitive resources to the task which needs to be completed within some time, leading to faster or better completion, and two, it can directly consume cognitive resources through focus on it, leading to slower or worse completion. Under the latter scenario, a deadline can become a binding constraint on decision making. In such a situation, conventional theory suggests that a deadline would be associated with a shadow cost. This chapter aims to identify whether a deadline can be binding, and to provide a measure of the shadow cost if it is, in the context of decision under risk. A two-phase experiment was conducted. Subjects in treatment conditions faced 20 risky investment choice problems, all identical in structure, in the second phase. Each problem represented a payoff prospect, and subjects received payment only for those problems completed within a deadline. The deadline was set on an individual basis, to account for the fact that subjects may take different amounts of time to complete the task normally, i.e., without any deadline. These individual level normal times were calculated from the first phase, where subjects faced 20 risky choice problems, all identical in structure to those in the second phase. The time a subject took to complete these problems in the first phase was used as the deadline in the second. This endogenous derivation of subject-specific deadlines based on subject level behavioral data is a novel design element and our main, methodological, contribution, which separates us from all previous studies involving deadlines. We found some subjects tend to display paralysis when confronted with deadlines, leading to inefficiency. Our results also confirm findings from prior studies, without endogenous deadlines, that most subjects tend to accelerate decision making and become effectively less risk-averse in the presence of a deadline.

The fifth chapter *"Rational Imitation under Cognitive Pressure"* aims to check whether individuals tend to imitate others' choices in cognitively demanding environments, and, if so, whether such imitation can be characterized as rational. The main line of experimental economics research on these topics has followed a Bayesian framework with incomplete information being the driver of learning or imitation. Another line has followed the insight that cognitive pressure or environmental complexity may generate imitation. In this view, the complexity of the decision environment can trigger imitative behavior as a heuristic response to save cognitive effort and decision cost. Our chapter is situated within this line. We ask first if cognitive pressure can be a causal basis for imitative behavior, i.e., is the presence of cognitive pressure associated with imitative behavior and the absence associated with the lack of such tendencies? Our second question is whether any identified imitation constitutes pure mimicry, where imitation is an end in itself, or whether it can be described as purposive or rational. We pursued these questions through a field experiment using ordinary citizens as volunteer subjects. Subjects faced two decision problems sequentially, in each of which a budget had to be split across two options. The return functions were deterministic, and was linear for one of the options, and non-linear for the other (the complex option). All subjects decided independently for the first problem faced. For the second problem, half the subjects saw the response of one other subject before deciding. We varied the degree of non-linearity of the return function for the complex option in an attempt to manipulate the degree of cognitive pressure, i.e., the level of cognitive pressure imposed by the structure of problem was used as a treatment variable. We found no evidence of imitative behavior in cognitively undemanding environments, and strong evidence in favor of imitation in the presence of cognitive pressure. Our findings further suggested purposive imitation in the latter case, with imitators tending to be sensitive to the quality of the decision being potentially imitated, and imitation tending to increase payoff.

Chapter 2

Are Contingent Choices Consistent ?

2.1 Introduction

Consider a decision-maker who knows she will face one of many possible contingent outcomes or contingencies in the future, and is formulating a contingent plan. The plan will specify which choice she will make among the available options given whichever particular contingency is actually realized. Suppose we conceive of each contingency as a fully specified choice problem in and of itself and allow the decision-maker to be cognizant of all relevant aspects of these different choice problems at the time of formulation of the contingent plan. Then a question is whether the plan is consistent in the sense that the specification for any particular contingency in the plan formulated is invariant to the set of alternative contingencies.

As an example, think of a person who knows she will receive a bonus this year from her employer, and has decided to spend it to buy a car. She also knows that the bonus amount will be either \$10,000 or \$25,000. She does not know however which amount it will be. She is deciding which car to buy in either eventuality or contingency. She has completed her research and test drives and narrowed her choice down to between the Toyota Tercel and the Hyundai Elantra in case she receives \$10,000, and between the Volkswagen Passat and the Nissan Altima in case she receives \$25,000. Suppose she formulates the plan (Tercel if 10, Passat if 25).

Now consider the same scenario as above except that the amounts are \$25,000 and \$50,000. After completing her research and test drives, she has narrowed the choice down to between the

Mercedes-Benz E250 and the BMW 528i if she receives \$50,000. Her possible choices remain the Passat and the Altima in case she receives \$25,000.

The issue of consistency surrounds her specification for the \$25,000 contingency. If she specifies the Passat in the second scenario for this contingency, then we can say her plan is consistent, as her choice for the \$25,000 contingency remains the same no matter what alternative contingency (\$10,000 or \$50,000) she contemplates at the time of plan formulation. This is because the two contingencies in any scenario constitute different and unrelated choice problems, since cars available at the three expenditure levels are distinguished sharply in terms of marques, features, specifications and prices, and also because only one of the contingencies can actually be faced. So if she specifies the Altima, her decision-making displays inconsistency, and raises the possibility that her choice for any particular problem may in general be influenced by information arising from other, and in principle unrelated, problems.

From the standpoint of rationality, one would intuitively expect contingent plans to be consistent. This is because each contingency is viewed as a fully specified, standard choice problem (see Rubinstein [81], Lectures 1 through 4), and theory assumes that a predictive framework can in principle be constructed for a given choice problem if data were available on the feasible set of options, and the preference ordering, and perhaps probability distributions, over outcomes, for the problem under consideration. No data are required for predictive purposes in this view from outside the problem in question, in particular on alternative problems the decision maker may be confronting at the time of choice. Thus the choice for a particular problem, say A, should be based only on information emerging from the description of A. Information arriving from other problems, or alternative contingencies, should not affect her choice for A, as such information is essentially irrelevant as far as her decision regarding A is concerned. It seems therefore a consistency assumption is implicit.

Indeed, the idea that consistency may be fundamentally associated with rationality has already received formal attention. Green and Osband [42] for example relate consistency of action in the face of changing information and probability assessments to the characterizability of expected utility maximization. Green and Park [43] develop this point further, particularly in the context of contingent plans, and argue that consistency of contingent choices may be necessary and sufficient for such plans to be rationalizable by maximization of conditional expected utility. Zambrano [95] in turn points out that such a condition is essentially equivalent to requiring that a contingent plan not react to irrelevant information.

Empirical evidence, mainly experimental and significantly from psychology, has mounted on the other hand that the presence of irrelevant or extraneous information can affect decision-making. In an early such laboratory study, Bodenhausen and Wyer [14] found that subjects' decisions with respect to punishment in hypothetical infringement cases could depend on whether the name given to the offender was stereotypical or not. Coman, Coman and Hirst [22] similarly found that subject choices in a medical decision-making laboratory experiment reacted to the presence of irrelevant information on the hypothesized available treatment.

Since alternative contingencies represent irrelevant information, from the perspective of any specific contingency or choice problem, these indicate that consistency of contingent plans may not always be satisfied in reality. Further, such effects have also been demonstrated in studies using experts as subjects. Dror, Charlton and Péron [31] for example showed in a field experiment that fingerprint experts could change decisions regarding identification of subjects once presented with extraneous information.[1] Jørgensen and Grimstad [57] similarly showed that estimates by expert software developers of time required for software development could depend on the presence of irrelevant information.

The presence of these two conflicting strands suggests the importance of resolution. This paper reports results from two laboratory experiments designed to help advance understanding of whether and when choices can be invariant to irrelevant information in the form of an alternative contingency, or when contingent plans can be consistent. The aim is to take a step toward understanding what variables and factors determine if and when someone will be inconsistent, and identifying domains were consistency may be a reasonable presumption, and domains where it may not hold. Since happenstance data on contingent plans are hard to obtain, the laboratory forms an ideal venue for empirical investigation in this regard.

There does not appear to be a prior literature on this specific topic. We therefore form the broad conjecture that consistency would be greater if problems were complete, and had possible

[1] See also Dror and Rosenthal [32].

outcomes which were monetary only and immediate, with choice being in principle easily calculable or determinable. Our two experiments hence developed two different environments. Experiment 1 had salient choice and subjects chose allocations in financial securities with fully specified outcomes and probabilities. Choice in Experiment 2 was hypothetical and subjects confronted a variety of everyday goods, durables, activities, assets and services, with a few features given in each case.[2]

Our subjects faced several decision problems, each framed as a contingency. Each was faced twice, once in conjunction with another choice problem (an alternative contingency), in a two-contingency situation, and once unitarily, as a single-contingency situation. The question was whether on average the two choices for a decision problem, from the two different occasions it was encountered, were the same or differed from each other.

Our within subject design raises the issue of order: does whether a subject is exposed to two-contingency situations before or after the ones with a single contingency matter? We counterbalanced and used order as a treatment variable to address this question: one group of subjects faced two-contingency situations prior to single-contingency ones, while the sequence was reversed for the other group.

A null hypothesis of consistency leaves no room for order effects. Such effects may appear if there are some inconsistent subjects however, without it being necessarily clear which direction these would go. For example, one possibility is that a situtation confronted on its own acts as an anchor, and hence consistency would be higher if single-contingency situations preceded in the order. On the other hand, plan formulation may tend to get disturbed when new information emerges, even if irrelevant, and hence consistency would be lower if single-contingency situations precede.

Our results provide support for our broad conjecture, subject to the caveat in footnote 2. Choices in Experiment 1 were mostly consistent, while those in Experiment 2 displayed significant inconsistency. Further, order mattered, and inconsistency was more likely if single-contingency situations preceded. Though Psychological evidence that preferences can be ma-

[2]The two experiments differ by more than the manipulation, however, as there were other differences such as subjects pools: see Sections 2.2.3 and 2.3.3 which discuss in detail and argue that some inferences with regard to differences across the experiments are yet possible here despite this confound.

nipulated by normatively irrelevant factors, such as option framing, changes in the choice context,or the presence of prior cues or anchors. However the tasks used in such experiments are more quantitative and directional. For example, Tversky and Kahneman (see [96]) spun a wheel offortune with numbers that ranged from 0 to 100, asked subjects whether the number of African nations in the United Nations was greater than or less than that number, and then instructed subjects to estimate the actual figure. Estimates were significantly related to the number spun on the wheel (the anchor), even though subjects could clearly see that the number had been generated by a purely chance process. See Ariely et al. [8] for a discussion and experiments.

For experiment 2, there was inconsistency no matter which order was followed with inconsistency significantly higher when single-contingency situations preceded. For experiment 1, choices when two-contingency situations preceded were almost universally consistent. In comparison, choices when single-contingency situations preceded represented a definite movement away from consistency, though differences were rarely significant. While choices displayed consistency overall in the latter case, it was not unambiguous.

Our findings with respect to order effects seem to favor the latter type of argument outlined above. While further examination is precluded by the limitations of our design, a possible explanation may lie in the relation between the information available and the choice made. In particular, if all information available, relevant or not, is used to decide choice on the first occasion, and is also retained in memory at the time of the second decision, then stability of choice may be more likely to be observed if there is a reduction in the information set through exclusion of irrelevant information, than if there is an expansion through inclusion.[3]

The rest of the paper is organized as follows. Our design and procedure are detailed in Section 2.2, which also develops the hypotheses to be tested. Section 2.3 presents our analysis and discusses findings, while Section 2.4 concludes.

[3]On average, about 15 minutes elapsed between the two occasions for any subject.

2.2 Design and Procedure, and Hypotheses

There was a single session for every treatment irrespective of experiment. Moreover, each treatment had 35 subjects, who were recruited using flyers, word of mouth and email solicitations. No subject participated in more than one treatment. Most subjects took between 35 and 45 minutes to complete. We now discuss specific features of the two experiments.

2.2.1 Experiment 1: Salient choice experiment

For the salient choice experiment, subjects had to decide investments in financial securities. For every choice problem, they had an endowment of 100 which they had to allocate across two financial securities (in integer amounts). An example of such a choice problem is given below.

You have an endowment of 100.

How much will you invest in 1 if the options are

(the remaining amount will be invested in 2):

1		**2**	
return	probability	return	probability
0.23	0.15	3.32	0.74
2.13	0.85	0.99	0.24

The table gives possible returns (per unit of investment), together with associated probabilities, for the two securities. We constructed each security such that (i) one possible return lay between 1 and 4, and the other lay between 0 and 1, and (ii) the expected value exceeded 1. Further, every security lay on one of two indifference curves constructed using a mean-variance utility function:

$$u = \mu - \frac{\lambda}{2}\sigma^2$$

where μ is the mean, σ^2 is the variance, and λ is a parameter (the Arrow-Pratt risk-aversion index, see Sargent [**?**]). We took $\lambda = 3$, as is commonly done in the applied finance literature (see Fabozzi, Kolm, Pachamanova and Forcardi [34]). The two utility values chosen were 1.156 and 1.056. Half the securities lay on each indifference curve.

We constructed 40 such choice problems, with a total of 80 ($= 40 \times 2$) securities. We designate 20 of these as *reference* problems, and the remaining 20 as *alternate* problems (subjects were not exposed to these terms). For every problem, reference or alternate, both securities lay on the same indifference curve.

Subjects faced each of these 20 reference problems on two different occasions, once on its own, in a *single-contingency* situation, and once in combination with an alternate problem, in a *two-contingency* situation (subjects were not exposed to the term contingency). Hence subjects faced 60 problems in 40 situations, 20 with a single contingency (only reference problems; the set of alternative contingencies being the null set) and 20 with two contingencies (reference-alternate pairs; the set of alternative contingencies being a singleton). A single-contingency example has already been given above. A two-contingency example is given below:[4]

You have an endowment of 100.

How much will you invest in 1 if the options are

(the remaining amount will be invested in 2):

1		**2**	
return	probability	return	probability
0.23	0.15	3.32	0.74
2.13	0.85	0.99	0.24

What if the options are instead

(again, what you do not invest in 1A

will be automatically invested in 2A):

1A		**2A**	
return	probability	return	probability
0.48	0.2	0.84	0.2
2.19	0.8	3.7	0.8

Subjects thus had to make 60 choices, 20 for single-contingency situations, and 40 for two-contingency situations. Subjects were presented example problems and situations with

[4]The reference problem was placed first, as in this example, half the time. This holds for the hypothetical choice experiment as well.

earning calculations during instruction, and were aware from the beginning they would be facing problems in two different kinds of situations (see Appendix for instructions).

There were two treatments, T11 and T12. In T11, subjects faced single-contingency situations first, followed by two-contingency situations, while in T12, subjects faced two-contingency situations first, followed by single-contingency situations.

Subjects received a show-up fee. Additionally, for each subject, five of the sixty problems were picked at random, and corresponding securities implemented in accordance with actual investment decisions, and the average of the resulting outcomes was given as payment privately at the end of the session. Subjects were aware of the payment rule and received INR 300 on average[5].

Subjects were first assembled together, each in front of a computer terminal. After receiving instructions through a projector, they connected to an internet form, where they entered their choices. The first page of the form repeated the instructions already given. Experiment 1 was conducted at Ambedkar University in Delhi, India. Subjects were mainly undergraduate students from a variety of disciplinary backgrounds.

2.2.2 Experiment 2 : Hypothetical choice experiment

For the hypothetical choice experiment, subjects' choice problems concerned a variety of everyday consumer goods, durables, activities, assets and services.[6] Each problem had two (definite) options, drawn from the same product. Subjects could choose any one of them and were also allowed to be indifferent. For every definite option in every problem, 4 characteristics were displayed. An example of such a choice problem is given below:

We again constructed 40 such choice problems, 20 reference and 20 alternate. One reference and one alternate problem were developed for each product. As before, subjects faced each reference problem twice, once in a single-contingency situation, and once in a two-contingency situation. For the latter cases, reference and alternate problems in any situation were for the

[5]The purchasing power parity exchange rate between the Indian Rupee and the US Dollar for 2010 was 16.84 rupees to a dollar according to the Penn World Tables ([49]).

[6]A total of 20: cup, mobile, medical facility, restaurant, shopping, route, flat, bank, car, camera, computer, B-school, investment, internet connection, entertainment, picnic, accommodation, travel agency, mosquito coil, movie theater.

Which cup would you prefer if the options are C1, C2 and C3?

C1	C2	C3
1. Small 2. No handle. 3. White with floral pattern 4. Normal design	1. Small-Medium 2. With handle 3. Light yellow no pattern 4. Octagonal design.	Indifferent

same product. A two-contingency example is given below:

Which cup would you prefer if the options are C1, C2 and C3?

C1	C2	C3
1. Small 2. No handle. 3. White with floral pattern 4. Normal design	1. Small-Medium 2. With handle 3. Light yellow no pattern 4. Octagonal design.	Indifferent

What if the options are instead

C1A	C2A	C3A
1. Small-Medium 2. Base smaller than rim. 3. Black with geometric pattern 4. Hexagonal design	1. Small 2. Base and rim are of same size 3. White with blue band 4. Hexagonal design.	Indifferent

Subjects thus again had to make 60 choices (they had seen examples and aware from the beginning they would be facing the two different kinds of situations: see Appendix for instructions). There were two treatments as before, T21 and T22. Subjects faced single-contingency situations first in T21, followed by two-contingency situations. The sequence was reversed in T22.

The experiment was hand-run. Subjects were assembled together and, after receiving instructions, were administered a questionnaire containing the problems.

Experiment 2 was conducted at Ramakrishna Mission Vidyamandir College in Belur, near Calcutta, India. Subjects were undergraduate students from a variety of disciplinary backgrounds.

The college (run by missionaries) did not permit any monetary payments to the students. Volunteer subjects were given a lunch packet worth about INR 300 in lieu of a participation fee.

2.2.3 Differences between the experiments

The two experiments differ in terms of the manipulation, which yields different types of problems, and salient versus hypothetical choice. As detailed above, there are other differences. These arose partly due to our failure to continue collecting data from a single location. The two experiments were thus conducted at different institutions. The two locations had different ethnic and linguistic majorities. We summarize the remaining differences between Experiments 1 and 2:

- variable compensation in cash versus fixed compensation in kind

- mainly undergraduate subjects versus undergraduate subjects

- cardinality of the choice set 101 versus 3[7]

- computer run with projected and oral instruction versus hand run with written and oral instruction

The differences in population induce concern regarding the extent to which the experiments can be compared with respect to effects of the manipulation. One way this concern regarding the quality of inference from differences across the experiments would be allayed is if differences across the orders or treatments within each experiment were similar. This is because if treatment comparisons go in the same direction in both experiments, this would indicate some stability in underlying choice making across populations with respect to the order aspect of the manipulation. To the extent this implies overall stability in underlying choice procedures, this would increase the likelihood that differences across the experiments can be attributed at least in part to the manipulation, thereby mitigating the confound. As discussed in Section 2.3.3, our results indeed provide support in that treatment differences do go in the same direction for both experiments, even though significance was rare in Experiment 1.

[7]The indifference option was inserted for Experiment 2 to correct for a potential bias favoring inconsistency: see Section 2.3.2 for details. The issue arises because choice for the same problem is recorded twice. It is not really of importance in Experiment 1, with its allocation out of 100 yielding a large choice set. A binary lottery choice problem would have restored the issue however.

2.2.4 Hypotheses

As argued in the Introduction, rational choice theory implicitly assumes consistency. We therefore posit consistency as the null hypothesis. Our basic hypothesis for any treatment in either experiment is thus that there is consistency on average (after aggregating all problems faced by all subjects). As mentioned, we conjecture this is more likely to receive support in Experiment 1, and more likely to be rejected in Experiment 2. This hypothesis indicates the absence of aggregate treatment effects, which we treat as a second hypothesis.

A very strict standard would require consistency in the responses to each reference problem for every treatment. As before this would imply the absence of treatment differences for every problem in either experiment. A weaker standard for disaggregated analysis at the level of problems, given aggregate consistency, would require the proportion for which inconsistent responses have been recorded to be invariant across treatments within an experiment.

A strict standard would also require consistent choices on the part of every subject. Disaggregated analysis at the level of subjects, under conditions of consistency in the aggregate, could also proceed on the requirement that the proportion of subjects classified as inconsistent is the same across treatments within an experiment.

2.3 Results

We first present and summarize results from Experiment 1 in Section 2.3.1, and Experiment 2 in Section 2.3.2. Some discussion related to interpretation of our findings are relegated to Section 2.3.3, after the presentation of results from both experiments.

As mentioned in footnote 4, half the reference problems in the two-contingency situations were placed before corresponding alternate problems, with alternate problems placed first for the other half. Substantive impact of placement order would presumably be either because of framing effects or because the subject does not view the reference problem as an independent problem on each occasion it is encountered. The latter seems a remote possibility given the appearance of the wording *What if the options were instead...* in the instructions, in between the reference and alternate problems, indicating a different choice set, and the impossibility of outcome from more than one contingency. Framing effects have of course been noted in a variety of situations, but their presence would raise the concern that any observed inconsistency is due to framing and not to the use of irrelevant information.

We checked for effects of placement order and found these were usually insignificant, but occasionally present. When present however, they were haphazard with no clear pattern or indication as to which placement order was more effective in promoting consistency. For this reason, given that placement order effects are quite mild in general, we present results using the pooled sample only.

2.3.1 Experiment 1

First we test the hypothesis that there is aggregate consistency within each treatment. The central question is whether a subject chooses differently the two occasions she faces any reference problem. Evidence of substantial difference would militate against the hypothesis of consistency. To address this, we calculated two average allocations per subject across all 20 problems (for reference problems only), one for choices from the first occasion, and the other for the second-occasion choices.

In T11, mean and median first-occasion allocations across all subjects were respectively 46.5 and 46.1, while corresponding mean and median second-occasion allocations were respectively 49.3 and 50.1. The numbers for T12 were 53.1 and 51.3 (respectively mean and median for first-occasion allocations), and 54.3 and 54.1 (respectively mean and median for second-occasion allocations).

We then tested whether these two matched samples (within each treatment separately), each with 35 observations, one for each subject, yielded the same average. The following table gives two-tailed p-values from t-tests and Wilcoxon signed-rank tests.

Table 2.1: Overview of treatments T11 and T12

	T11	T12
t-test	0.1042	0.4015
Wilcoxon	0.0099^{**}	0.2870

** $p < 0.01$

We found there was no statistical difference between subjects' average first-occasion and second-occasion allocations for T12. The t-test gave a similar result for T11. The Wilcoxon test however indicated significant difference between average first-occasion and second-occasion allocations for T11.

Findings from T12, where two-contingency situations were faced first, thus support the hypothesis of consistency. T11, with single-contingency situations being faced first, on the other hand yielded an ambiguous finding, and therefore provides limited support for the consistency hypothesis.

At the same time, the fact that subjects seemed to be more prone to display inconsistency when single-contingency situations are faced earlier in the sequence is supportive of the possibility that decisions are more likely to be changed when irrelevant information appears than when it disappears. In any case, the results above suggest that the order in which subjects faced the two situations can make a difference. The suggestions is weak, however, as all tests did not produce aligned results. For resolution, we directly investigate the hypothesis that the degree of consistency is indistinguishable across the treatments.

To do this, we first calculated the difference in the average allocation for reference problems across the two occasions for every subject. We then performed comparison tests of these samples of differences across the treatments. The mean and median differences across subjects were 2.8 and 4 respectively for T11, while the corresponding numbers were lower at 1.2 and 2.8 respectively for T12.

Our tests showed that these differences were statistically indistinguishable across the treatments (two-tailed p-values: t-test = 0.4459, Mann-Whitney ranked sum test = 0.1253). This result therefore weakens the prior finding that order is of importance, as, had it been, we would have expected some treatment differences (in the amount of deviation in the allocations across the two occasions) to emerge.

We now disaggregate the data, to explore consistency at the levels of problems and subjects.

2.3.1.1 Problems

Within any treatment, every reference problem was faced twice by any subject. For both treatments therefore we have a series of matched pairs of allocations (35 independent observations) for all 20 problems individually. The hypothesis for each problem, within each treatment, is that choices display consistency.

We performed within treatment comparison tests for each of these problems, and found inconsistency for two problems in T11 and one problem in T12 (allowing for significance level upto 5%). All tests indicated consistency for all other problems. Table 2.2 below identifies the problems in question and reports results from mean and median comparison tests.[8]

Table 2.2: Within treatment comparisons by problems for T11 and T12

problem no.	treatment	t-test	Wilcoxon
6	T11	0.0006	0.0007
15	T11	0.0002	0.0008
11	T12	0.0210	0.0412

Entries are two-tailed p-values

Signs of inconsistency at the level of problems within treatments were thus fairly weak. Moreover, different problems produced inconsistency across treatments, yielding no particular pattern. We now examine the hypothesis that for every problem, the degree of consistency is invariant across the treatments, by studying whether difference in allocation varies between them for any problem.

We found inconsistency only for two problems, nos. 6 and 15 identified in the prior table. Table 2.3 below gives results of comparison tests for these two. All tests gave consistency for all other problems.

Table 2.3: Across treatment comparisons by problems

problem no.	t-test	Mann-Whitney
6	0.0277	0.0180
15	0.0002	0.0011

Entries are two-tailed p-values

[8]The numbers of the problems in the table refer to an order independent of the ones implemented in the treatments.

There was thus no inconsistency for at least 90% of the problems in either treatment. Further, different problems showed inconsistency in the two treatments, yielding no particular pattern. For the hypothesis that the proportion of problems for which inconsistent responses have been recorded is invariant across treatments, we categorized any problem as either consistent or inconsistent on the basis of Table 2.2, and tested whether the inconsistency rate (number of inconsistent problems) differed across the treatments. We found no difference in terms of a two-tailed as well as a one-tailed proportion test. Our overall conclusions therefore are that the signs of consistency found in the aggregate are strongly supported at the level of individual problems.

2.3.1.2 Subjects

We now perform disaggregation at the level of subjects. The hypothesis for each subject is that choices display consistency. We can use choice data from the 20 reference problems faced by any subject to help us address this matter. We pursued two approaches, one based on comparison tests, and the other on regression.

For the former, we compared the first and second occasion allocations for every subject, using Wilcoxon tests and matched sample t-tests. A subject's choices were deemed to be consistent if no significant difference was found between allocations from the two occasions.

We used a Newey-West adjusted OLS, to account for possible failure of independence at the level of the individual subject arising from some correlation in observation errors across time, for the latter. Specified lags of 0, 1 and 2 yielded similar results, and we only report outcomes for lag 1.

For any regression our specification used the difference in allocation across the two occasions as the dependent variable. No independent variable was specified. A constant was used. Thus insignificance of the constant provides support to the hypothesis of consistency, as had choices been inconsistent, we would have expected the difference to be non-zero.

The constant was found to be significant for 7 subjects in T11. Results are shown in Table 2.4 below.

For T12, the number of subjects displaying inconsistency was 4. Results are shown in Table

Table 2.4: Newey-West regression results for T11

	S3	S14	S19	S20	S22	S28	S35
constant	-11.50**	-26.65***	37.72*	-11.25*	-7*	-9.25*	-1.85*
	(3.890)	(7.970)	(5.735)	(4.310)	(3.039)	(3.568)	(0.683)

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

2.5 below.

Table 2.5: Newey-West regression results for T12

	S15	S23	S32	S34
constant	20*	15.9**	-16***	-19.05*
	(9.402)	(5.135)	(2.706)	(7.928)

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Results from t-tests were nearly identical to those from the regression analysis, reported above. For either treatment, the same set of subjects were identified as inconsistent (see Tables 2.6 and 2.7). The p-values were also very similar for all subjects identified in T12 and 4 of the subjects identified in T11. For the remainder, S3, S14 and S19, there was some reduction in significance for S3 and S14, and considerable increase in significance for S19.

Table 2.6: t-test and Wilcoxon test results for T11

	S3	S14	S19	S20	S22	S28	S35
t-test	0.0294	0.0094	0	0.0420	0.0352	0.0327	0.0313
Wilcoxon	0.0246	0.0144	0.0001	-	-	0.0177	0.0147

Entries are two-tailed p-values.

Wilcoxon tests showed results which were also similar, but not so close (again, see Tables 2.6 and 2.7). For either treatment, a strict subset of the subjects from the regression analysis above were identified to be inconsistent. The absence of S20 and S22 from T11, and S15 from T12 left the number of inconsistent subjects at 5 in T11 and 3 in T12. The levels of significance were also very close for those remaining in T12. The same was found for S28 and S35 in T11, with significant changes for S3, S14 and S19, in the same directions as for the t-tests.

Thus around 10-20% of subjects in total displayed choice inconsistency. For the hypothesis that the proportion of subjects for whom inconsistent responses have been recorded is invariant

Table 2.7: t-test and Wilcoxon test results for T12

	S15	S23	S32	S34
t-test	0.0509	0.0082	0.0002	0.0195
Wilcoxon	-	0.0018	0.0009	0.0196

Entries are two-tailed p-values.

across treatments, we categorized any subject as either consistent or inconsistent and tested whether the inconsistency rate (number of inconsistent subjects) differed across the treatments.[9] We found no difference in terms of two-tailed or one-tailed proportion tests. With at least 80% of subjects choosing consistently, we conclude overall therefore that the consistency found in the aggregate sample is quite robustly replicated at the level of individual subjects.

Results from Experiment 1 therefore lend considerable support to the null hypothesis of consistency. The support is not universal however, as the findings from the Wilcoxon tests reported in Table 2.1) suggest a violation of consistency. Additionally, all comparisons indicate a lessening of consistency in T11, though explicit treatment comparisons did not yield significance.

2.3.2 Experiment 2

We test the hypotheses in exactly the same order as in Experiment 1. The central question remains whether a subject chooses differently the two occasions she faces any reference problem. The measure of consistency in this experiment is the *switch* rate. For any subject in any treatment, data on two choices are available for any reference problem, one from each occasion it is faced. We will say there is no switch if the two choices made are the same, and there is a switch if the two are different. The switch rate for a subject is then the proportion of times she switched out of 20.

We admitted the indifference option to account for the following difficulty. A subject in the corresponding binary environment who is truly indifferent between the two definite options could choose differently on the two occasions as a result of random choice. Her choice would then be observationally inconsistent, whereas it actually is not. With uniform randomization,

[9] One categorization was done on the basis of regression/t-test results and another on the basis of Wilcoxon test results.

this event would occur with probability 0.5. Permitting such a subject to express indifference allows the chance of such cases to be reduced, as long as truly indifferent subjects are more likely to choose indifference rather than one of the definite options. The issue does not really arise in Experiment 1 because of the fine choice grid and the nature of the problems.

The definition of the switch rate ignores whether the switch was from one definite option to another, or whether it involved indifference (a switch from a definite option to indifference or the other way round). As it happens, subjects overwhelmingly chose one of the two definite options. The aggregate indifference rate (the number of times indifference was reported as a fraction of the total number of decisions made by all subjects taken together) for reference problems was 111/1400 or about 8% for T21 and 152/1400 or about 11% for T22. Additionally, most switches were from one definite option to another: about 70% of all switches in T21 (180/249) and 60% in T22 (54/90).

As indicated in the final sentence of the paragraph above, the aggregate switch rate is 249/700 or 35.5% in T21, and 90/700 or 12.9% in T22. We first test if these are respectively positive. We calculated the switch rate for every subject using the procedure above and tested whether the mean of this sample of 35 observations (using a t-test) for any treatment was different from zero. We found they were: the right-tailed p-values for both treatments were less than 0.001. The same result obtained when we used the median instead of the mean (vide a Snedecor-Cochran sign test).

We then tested if the switch rate was different across the two treatments. The figures given above suggest that the switch rate is higher for T21, where single-contingency situations were faced first relative to T22, where two-contingency situations were faced first. Statistical analysis revealed that average switch rates were indeed different across the two. We performed a t-test as well as a Mann-Whitney test, both of which indicated difference with two-tailed p-values less than 0.001.

Thus the data support the possibility that inconsistency may be greater when single-contingency situations are faced earlier in the sequence, so decisions may be more likely to be changed when irrelevant information arrives than when it departs. At the same time, our finding is also that there is significant inconsistency when single-contingency situations are faced later in the sequence. Hence the presence of some inconsistency in decision-making may be endemic, and decisions might change whenever there is alteration in associated irrelevant information.

We now disaggregate the data, to explore consistency at the levels of the subjects and the problems.

2.3.2.1 Problems

Within any treatment, every reference problem was faced twice by any subject, and we know for every problem whether a switch occurred or not. Coding a switch as 1, and a consistent choice as 0, we therefore have a series of 35 independent observations (consisting of zeros and ones) for every problem within each treatment.

We tested if the switch rates associated with the problems were positive. We used a t-test as well as a Snedecor-Cochran test for every problem within the two treatments separately. We found severe signs of inconsistency (allowing significance level upto 5%): the null of zero switch rate was rejected for every problem in at least one treatment. Consistency was found for only three problems in T21 (1,3,12) and 6 problems in T22 (6,7,9,14,15,20).[10]

Tables 2.8 & 2.9 below (for T21 and T22 respectively) report right-tailed p-values from the tests, only for the problems displaying inconsistency.

We now analyze consistency across the two treatments for each problem by examining whether there is variation in the switch rate. We found consistency, i.e., statistical indistinguishability of switch rates, for 8 problems. Results from these tests are given Table 2.10 below, which reports two-tailed p-values only for the problems with cross-treatment inconsistency.

There was thus substantive inconsistency within and across treatments for most problems. This leads us to conclude that the inconsistency found in the aggregate is strongly reproduced at the level of individual problems. However the specific pattern found in the aggregate was not replicated, as we found that the inconsistency rate (number of inconsistent problems) did not differ across the treatments, in terms of either a two-tailed or a one-tailed proportion test. The categorization of problems as either consistent or inconsistent was on the basis of Tables

[10] The numbers of the problems in the table below refer to an order independent of the ones implemented in the treatments.

Table 2.8: t-test and Snedecor-Cochran test results for T21 by problems

problem no.	t-test	Snedecor-Cochran
2	0.0016	0.0039
4	0	0
5	0	0
6	0.0002	0
7	0	0
8	0	0.0001
9	0.0060	0.0156
10	0.0004	0.001
11	0	0
13	0.0016	0.0039
14	0.0016	0.0039
15	0	0
16	0	0
17	0	0.0001
18	0	0
19	0	0
20	0.0002	0.0005

Table 2.9: t-test and Snedecor-Cochran test results for T22 by problems

problem no.	t-test	Snedecor-Cochran
1	0.0016	0.0039
2	0.0219	0.0625
3	0.0219	0.0625
4	0.0115	0.0312
5	0.0060	0.0156
8	0.0060	0.0156
10	0.0016	0.0039
11	0.0031	0.0078
12	0.0219	0.0625
13	0.0115	0.0312
16	0.0219	0.0625
17	0.0060	0.0156
18	0.0115	0.0312
19	0.0060	0.0156

Table 2.10: Across treatments comparison by problems

problem no.	t-test	Mann-Whitney
4	0	0.0001
5	0.0009	0.0013
6	0.0083	0.0092
7	0	0
8	0.0346	0.0356
11	0.0056	0.0064
15	0.0001	0.0002
16	0	0
17	0.0096	0.0106
18	0.0001	0.0002
19	0.0046	0.0055
20	0.0002	0.0003

2.8 and 2.9.

2.3.2.2 Subjects

We now perform disaggregation at the level of subjects. The question is again whether any of these 70 subjects in any treatment individually displayed inconsistency. As before, this is a within treatment analysis.

For every subject, we know whether she switched or not for each of the 20 problems. Consistency would be displayed for a problem by a subject if there is no switch and by the subject overall if the switch rate is zero. We investigate consistency for each subject once again both through comparison tests (t-tests and Snedecor-Cochran tests) as well as through regression analyses.

For the latter approach, we estimated a linear probability model with Newey-West correction for each subject. The strategy mirrors that applied to the data from Experiment 1. The dependent variable indicated whether a switch had been observed or not. There was a constant, but no independent variable. The significance or lack thereof of the constant is used to determine inconsistency or consistency respectively. Lags of 0, 1 and 2 again yielded similar results, and we report only results where lag 1 was specified.

The constant was found to be significant for 31 subjects in T21 (choices of S2, S4, S5 and S12 displayed consistency). Results are shown in Table 2.11 below in a transposed format.

Table 2.11: Newey-West regression results for T21

subject	constant	subject	constant
S1	.3**	S21	.3*
	(0.093)		(0.106)
S3	.35*	S22	.45**
	(0.125)		(0.116)
S6	.25*	S23	.65***
	(0.098)		(0.093)
S7	.25**	S24	.3**
	(0.084)		(0.108)
S8	.4**	S25	.35**
	(0.124)		(0.106)
S9	.45**	S26	.35**
	(0.122)		(0.114)
S10	.35**	S27	.6***
	(0.102)		(0.119)
S11	.4***	S28	.35**
	(0.100)		(0.114)
S13	.25*	S29	.45***
	(0.095)		(0.098)
S14	.35**	S30	.4**
	(0.102)		(0.119)
S15	.4**	S31	.55***
	(0.112)		(0.116)
S16	.45**	S32	.35**
	(0.122)		(0.114)
S17	.4**	S33	.6***
	(0.108)		(0.105)
S18	.3*	S34	.4***
	(0.118)		(0.095)
S19	.2*	S35	.2*
	(0.082)		(0.094)
S20	.35**		
	(0.102)		

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.12: t-test and Snedecor-Cochran test results for T21

subject	t-test	Snedecor-Cochran
S1	0.0051	0.0156
S3	0.0024	0.0078
S4	0.0210	-
S5	0.0105	-
S6	0.0010	0.0313
S7	0.0105	0.0313
S8	0.0010	0.0039
S9	0.0004	0.0020
S10	0.0024	0.0078
S11	0.0010	0.0078
S13	0.0105	0.0313
S14	0.0024	0.0078
S15	0.0010	0.0039
S16	0.0004	0.0020
S17	0.0010	0.0039
S18	0.0051	0.0156
S19	0.0210	-
S20	0.0024	0.0078
S21	0.0051	0.0156
S22	0.0004	0.0020
S23	0	0.0039
S24	0.0010	0.0001
S25	0.0024	0.0078
S26	0.0024	0.0078
S27	0	0.0002
S28	0.0024	0.0078
S29	0.0004	0.0020
S30	0.0010	0.0039
S31	0.0001	0.0005
S32	0.0024	0.0078
S33	0	0.0002
S34	0.0010	0.0039
S35	0.0210	-

Entries are right-tailed p-values

Results from comparison tests for T21 are shown in Table 2.12, which has right-tailed p-values as entries. The t-tests find 33 subjects to be inconsistent (all but S2 and S12). The Snedecor-Cochran procedure yields the number 29 (all but S2, S4, S5, S12, S19 and S35).

For T22, the number of subjects displaying inconsistency was 7 (S3, S8, S14, S15, S16, S24 and S35), according to the regression approach. Results are shown in Table 2.13 below.

Table 2.13: Newey-West regression results for T22

	S3	S8	S14	S15	S16	S24	S35
constant	-.45***	.25**	.2*	.35**	.2*	.25**	.45***
	(0.091)	(0.084)	(0.082)	(0.106)	(0.094)	(0.084)	(0.091)

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Comparison tests yielded somewhat similar results for T22. The results are shown in Table 2.14 (entries are right-tailed p-values). The t-tests determined 10 subjects to be inconsistent (S1, S3, S8, S9, S14, S15, S16, S25, S26 and S35) while the Snedecor-Cochran tests found 5 (S3, S8, S15, S25 and S26).

Table 2.14: t-test and Snedecor-Cochran test results for T22

subject	t-test	Snedecor-Cochran
S1	0.0414	-
S3	0.0004	0.0020
S8	0.0105	0.0313
S9	0.0414	-
S14	0.0210	-
S15	0.0024	0.0078
S16	0.0210	-
S25	0.0105	0.0313
S26	0.0414	-
S35	0.0004	0.0020

Entries are right-tailed p-values

The results thus support the possibility that inconsistency may sometimes be endemic. More than 80% of subjects displayed inconsistency in T21, where single-contingency situations arrived earlier in the sequence. Of course, less than one third of subjects displayed inconsistency in the reverse sequence, hence the issue may not be equally serious in all settings. Overall our conclusion is that the lack of consistency found in the aggregate sample is replicated at the level of individual subjects. Moreover, the pattern was also similar to that in the aggregate: proportion tests of whether the inconsistency rate (number of inconsistent subjects) differed across the treatments showed that the rate was higher in T21 (p-values < 0.001).[11] Results from Experiment 2 therefore strongly reject the null hypothesis of consistency. Further, all comparisons indicate an increase in inconsistency in T21, with all but one explicit treatment comparison yielding significance (the exception being vide the test of the weak hypothesis for problems in Section 2.3.2.1).

2.3.3 Discussion

To what extent are the conclusions and interpretations we have drawn from our findings reasonable? A central concern is that the experiments may not be readily comparable, since they differ by more than the manipulation.

Treatment differences were mostly insignificant in experiment 1, and mostly significant in experiment 2. Common to both experiments however was a visible movement toward consistency in every comparison when going from treatment 1 (single-contingency first) to treatment 2. This indicates a stable relationship between the order of provision of information in the form of alternate contingencies and choice, rather than a manifestation of differences in subject pools etc., as detailed in Section 2.2.3. This stability enables comparison across the experiments in terms of the order aspect of the manipulation.

A question is whether the stability with respect to differences across treatments extends to the main, or environmental, aspect of the manipulation. In this context arguing that there is no stability in choice making across the experimental subject pools, resulting in all differences and similarities to be independent of the experimental manipulation, seems difficult without a delineation of why then the data appear to indicate some stability with respect to treatment differences. It also seems hard to argue that there is stability with respect to the order aspect, but not with respect to the environmental aspect, as that would require some explanation for this particular pattern. It seems easiest to argue therefore that there is stability of choice making across the subject pools, indicating that the support found in the results for our conjecture can be attributed at least in part to the manipulation, rather than entirely to the unobserved

[11] Categorization of subjects as either consistent or inconsistent was done on the bases of regression, t-test, as well as Snedecor-Cochran test results.

characteristics of the subject pools.

2.4 Conclusions

This paper reports results from experiments examining whether subjects' contingent choices satisfy consistency. It found that consistency may mostly be a reasonable presumption when contingencies are complete, and outcomes are monetary and immediate, but is unlikely to hold entirely when outcomes are distant and not fully monetary, and choice less easily determinable.

We further found that decisions may be more likely to change when irrelevant or extraneous information arises rather than subsides. Since actual decisions are often one-shot and made in the presence of both irrelevant and relevant information, this suggests that experimental explorations of consistency using a within subject design should control for order effects to improve accuracy of results. Hence studies which only expose subjects first to relevant information, and then to all information, relevant as well as irrelevant, without counterbalancing, such as Dror, Charlton and Péron [31], may stand the risk of overstating the degree of inconsistency.

The source of whatever failure of consistency is observed appears to be the use of irrelevant information, emerging from alternative contingencies or choice problems, in decision-making. Given that the use of such information is normally precluded by rationality, which in turn is closely related to consistency, our findings indicate that such notions of rationality may not apply equally to all settings. This article may therefore contribute to the continuing debate on whether or when sufficiently unrestricted Bayesian models of cognition or behavior can generate testable hypotheses (see e.g. Zambrano [95] and Jones and Love [56]). Many have shown within frameworks such as subjective expected utility theory (e.g. Savage [85]) that conditions for rationalizability can be found: Green and Park [43] for example show that consistency of contingent plans is such a necessary and sufficient condition. In this context, our results therefore suggest both that the explanatory power of the framework as currently expressed may not manifest itself equally in all settings, and also that expansion of the ambit of the framework may have to rest on a reconsideration of what constitutes the boundary for any choice problem.

Reference and corresponding alternate problems in our experimental implementation were similar in some sense. In the example in the Introduction, choice was over the same object, car, in each contingency, again inducing some similarity or relevance. It may not be trivial to reach agreement or definition on similarity or relevance, but a possible future research direction could be to explore what kind of alternate problems can affect choice for a given problem of interest, which might help shed light on subject notions of similarity. Such an approach may also be useful for addressing whether choice ecologies or sets of alternate problems with predictable inconsistency exist.

Appendix

Instructions for Experiment 1:

Thank you for your participation. Please read the instructions carefully.

You will face 40 questions one after the other. After finishing a question, please press the CONTINUE button, and the next question will appear.

Here is a sample question:

You have decided to invest 100 rupees by buying units of financial options. There are two options available, and one unit costs 1 rupee for either option. The table below gives the possible returns and the corresponding chances per unit for both. How much of your 100 rupees will you invest in option 1, i.e., how many units of option 1 will you buy? Whatever remains will be used to buy units of option 2. Your answer must be an integer between and including 0 and 100.

O1		**O2**	
return	*chance*	*return*	*chance*
2.2	0.2	3.1	0.4
0.9	0.8	0.6	0.6

Thus, your answer here will be the number of units of option 1 you are buying.

Here is another sample question:

You have decided to invest 100 rupees by buying units of financial options. There are two options available, and one unit costs 1 rupee for either option. The table below gives the possible returns and the corresponding chances per unit for both. How much of your 100 rupees will you invest in option 1, i.e., how many units of option 1 will you buy? Whatever remains will be used to buy units of option 2. Your answer must be an integer between and including 0 and 100.

O1		**O2**	
return	*chance*	*return*	*chance*
2.2	0.2	3.1	0.4
0.9	0.8	0.6	0.6

What if the options are instead (again, what you do not invest in 1A will be automatically invested in 2A and your answer must be an integer):

O1A		**O2A**	
return	*chance*	*return*	*chance*
2.5	0.25	4	0.5
0.1	0.75	0.7	0.5

Thus, you will be giving two answers here, one the number of units of option 1 you are buying if faced with a choice between options 1 and 2, and two the number of units of option 1A you are buying if faced with a choice between options 1A and 2A.

For every question, you will have to allocate 100 across two options each with two possible returns. Here is an example on what you might expect to get as return, using the second sample question.

Suppose you have chosen to invest 70 in option 1 and therefore 30 in option 2 for the first case, and for the second case your choice is 40 in option 1A and therefore 60 in option 2A.

Then the outcomes for the first case can be either:

(1) 2.2 in option 1, and 3.1 in option 2. The chance of this is $0.2 * \times 0.4 = 8\%$. Then your return is $70 \times 2.2 + 30 \times 3.1 = 247$

(2) 2.2 in option 1, and 0.6 in option 2. The chance of this is $0.2 \times 0.6 = 12\%$. Then your return is $70 \times 2.2 + 30 \times 0.6 = 172$

(3) 0.9 in option 1, and 3.1 in option 2. The chance of this is $0.8 \times 0.4 = 32\%$. Then your return is $70 \times 0.9 + 30 \times 3.1 = 156$

(4) 0.9 in option 1, and 0.6 in option 2. The chance of this is $0.8 \times 0.6 = 48\%$. Then your return is $70 \times 0.9 + 30 \times 0.6 = 81$

And the outcomes for the second case can be either:

(1) 2.5 in option 1, and 4 in option 2. The chance of this is $0.25 \times 0.5 = 12.5\%$. Then your return is $40 \times 2.5 + 60 \times 4 = 340$

(2) 2.5 in option 1, and 0.7 in option 2. The chance of this is $0.25 \times 0.5 = 12.5\%$. Then your return is $40 \times 2.5 + 60 \times 0.7 = 142$

(3) 0.1 in option 1, and 4 in option 2. The chance of this is $0.75 \times 0.5 = 37.5\%$. Then your return is $40 \times 0.1 + 60 \times 4 = 244$

(4) 0.1 in option 1, and 0.7 in option 2. The chance of this is $0.75 \times 0.5 = 37.5\%$. Then your return is $40 \times 0.1 + 60 \times 0.7 = 46$

As you know, you will earn money for today's participation, and how much you will earn will depend on how you decide and some luck. Five of your investment choices will be picked at random at the end, and their corresponding options will be implemented in accordance with your choices. You will get the average of their returns, plus a fee for showing up.

Let's start! Read the questions carefully, and then choose.

Instructions for Experiment 2:

Thank you for your participation. Please read the instructions carefully.

You will face 40 questions one after the other.

Here is a sample question:

You have to choose one of the three options - C1, C2, C3. Put a tick (✓) in the relevant box.

Please choose only one out of the three options. You are thus giving a single answer here.

Which cup would you prefer if the options are C1, C2 and C3?

C1 ☐	**C2** ☐	**C3** ☐
1. Small 2. No handle. 3. White with floral pattern 4. Normal design	1. Small-Medium 2. With handle 3. Light yellow no pattern 4. Octagonal design.	Indifferent

Here is another sample question:

Here too you have to choose one of the three options. Put a tick in the relevant box.

Which cup would you prefer if the options are C1, C2 and C3?

C1 ☐	**C2** ☐	**C3** ☐
1. Small 2. No handle. 3. White with floral pattern 4. Normal design	1. Small-Medium 2. With handle 3. Light yellow no pattern 4. Octagonal design.	Indifferent

What if the options are instead

C1A ☐	**C2A** ☐	**C3A** ☐
1. Small-Medium 2. Base smaller than rim. 3. Black with geometric pattern 4. Hexagonal design	1. Small 2. Base and rim are of same size 3. White with blue band 4. Hexagonal design.	Indifferent

In either case, please choose only one out of the three options. You are thus giving two answers here, your choice if faced with options C1, C2, and C3, as well as your choice if faced with options C1A, C2A, and C3A.

For every question, you will face some good, durable, activity, asset or service, with four characteristics given for any option. There will be two main options. You will have to compare and choose one of them. Or you can be indifferent.

Let's start! Read the questions carefully, and then choose.

Chapter 3

The Impact of Past Outcomes on Choice in a Cognitively Demanding Financial Environment

3.1 Introduction

Will two individuals differing only in economic circumstance, say one with higher earnings and the other with lower, decide differently in a given financial choice problem? Cumulated evidence from two recent lines of research suggest they might if the problem is cognitively demanding, with superior circumstance yielding better choice. One of these (see Christelis, Jappelli and Padula [25], Grinblatt, Keloharju and Linnainmaa [44], Agarwal and Mazumdar [1] and Cole, Paulson and Shastry [21]) explores the link between cognitive capacity and quality of financial choice and shows a positive association may exist. The other, initiated by Mani, Mullainathan, Shafir and Zhao [67], examines the link between economic circumstance and cognitive capacity, and finds there may be causal dependence.

Whether superior economic circumstance can yield better financial choice can have particular importance in settings where better financial choice plays a large role in determining future economic circumstance, such as the informal or unorganized sector.[1] It is well known

[1]The size of the informal economy as a proportion of GDP is typically estimated to be between 10 and 25% for developed economies (see e.g. Andrews, Sánchez and Johansson [5]). Estimates of the proportion of the

that individuals engaged in informal or unorganized economic activity are mainly poor with tight budgets, high indebtedness and significant financial exposure, and that incomes from such activity can be subject to wide and unpredictable variation (see e.g. ILO [54] and Meier and Rauch [69]), so that economic circumstance is quite sensitive to the quality of financial choice. Take in this situation someone who has been made temporarily poor due to a negative income shock: will such a person be less cognitively capable, and hence make worse financial choices? Can cognitive capacity then vary with relative income status when income flows and cycles are unstable and highly variable? Particular fragilities can be generated if so, as an individual who has been made unanticipatedly poor due to some sudden negative financial incident can now become mired in poverty due to poor financial choices driven by cognitive indequacy. Such outcomes can reduce effectiveness of anti-poverty and income security policies aimed at the most vulnerable segments of the population, making it more difficult to lift relevant sections out of poverty traps, and also have distributional implications.

In this paper, we directly address the question of whether economic circumstance can impact quality of financial choice in cognitively demanding environments. Specifically, we report results from a laboratory experiment investigating whether performance of subjects in a cognitively demanding financial task can depend on prior outcome from independent and unrelated financial tasks. The experiment was in two parts. The first part confronted subjects with a sequence of binary lottery choice problems and generated a controlled history of financial outcome. Subjects had to allocate a budget across two deterministic options in the second part. One of these was a simple option with a linear return function, and the other had a non-linear and patternless return function, and was hence complex. This feature made the task in the second part cognitively demanding.

We were particularly concerned to understand the behavior of those not achieving the globally optimal allocation through our measurement: what do those who choose non-optimal allocations do? For this purpose, with a deterministic portfolio choice problem from a fixed budget, it made sense to focus on the (non-global) local maxima of the aggregate return function. We therefore structured the aggregate return function such that there was a unique global maximum

non-agricultural workforce engaged in informal activities in developing economies typically exceed 50% (see e.g. ILO [55]).

and different kinds of local maxima, close and distant with respect to the global.

Our results indicate that prior financial outcome can indeed affect cognitive performance, with those bringing a more positive history being more likely to achieve the optimum, and those with a more negative history more likely to achieve a close maximum. The scarcity framework of Mullainathan and Shafir ([73]) and Mani et al ([67]) provides an avenue to explain these findings. The theory proposes that financial preoccupations consume scarce cognitive resources, leaving less of such resources for activities and choices (see e.g. Miller [70] and Luck and Vogel [65] for evidence that cognitive capacity is not unlimited), and argues that someone in a worse financial and economic position can face greater preoccupation and hence make poorer choices. Our findings are consonant with this theory, as they are consistent with greater distress, discouragement or distraction, consuming more cognitive resources and leading to weaker performance, being generated by negative outcome histories relative to positive ones. Competing explanations to the pre-occupation/distraction hypothesis includes concepts of behavioural inertia such as the gambler's fallacy, i.e.- believing that past and current states will persist in the future (see Croson and Sundali, [26]) and the related imaginability bias, i.e.- being able to imagine states from the past better than potential states that may arise in the future (see Gilbert and Malone [40]). See also Tversky and Kahneman [96] for an explanation of the availability heuristic, which examines the idea that human brains use more cognitively available resources / knowledge when having to make a choice in a hard problem with an answer they don't know.

Somewhat surprisingly, we found that the proportion of subjects settling at a distant local maximum or outside a neighborhood of any maximum, global or local, was independent of specific history faced, or whether a financial outcome history had been faced at all or not. This finding suggests that cognitive performance of some proportion of the population may be immune to prior personal experience and, more troublingly, that they may constitute the set of weaker performers.

The rest of the paper is organized as follows. The next section details our design and procedure, while Sections 3.3 presents some analytical preliminaries. Section 3.4 provides and discusses results, while Section 3.5 contains concluding observations. Most figures (including instructions) are relegated to the Appendix.

3.2 Design and Procedure

Our experiment involved two parts. For the second part, each subject was asked to allocate an endowment of 100 between a linear option, which gave a fixed return of 10 per unit invested, and another option which had a highly non-linear return function, with many local maxima and maximands quite close to each other (see Figures 3.2 and 3.3). These properties of the latter or complex option made the net payoff function complicated, and thus rendered the task of finding the global payoff maximum cognitively demanding. Outcome in this task, as to whether the subject achieves or is close to the global maximum, or one of the local maxima, is used as the measure of cognitive performance.

The first part consisted of three binary lottery choice problems, faced one after the other (see Figures 3.4, 3.5 and 3.6). These problems were independent of each other and also of the allocation problem in part 2. In each such problem, a subject had to choose one of two risky financial options. Each of these options had two possible outcomes, with sufficient distance between them such that for any option one outcome was clearly high and the other clearly low. Additionally, the two high as well as low outcomes across the two options in any given problem were respectively chosen to be quite close to each other. This made the two options in any given problem quite similar in appearance to each other: they also had similar expected returns. These features made the task of choosing one from the two options cognitively undemanding compared to the problem from part 2. Further, there were two escalation features across the problems. Firstly, expected returns approximately doubled from the first to the second, and from the second to the third problem.[2] Secondly, the gap between the high and low outcomes for any option increased from the first to the second, and from the second to the third problem.[3]

For the teatment conditions, part 1 provided a controlled history of financial outcome to

[2]The expected outcome in terms of points obtainable by the subject from the third problem in part 1 was chosen to be approximately equal to half the points obtainable from the allocation problem in part 2 conditional on optimal choice (see Figures 3.2 and 3.6).

[3]In fact, the options were constructed such that the low outcomes for either option decreased from the first to the second, and from the second to the third problem, while the high outcomes for either option increased from the first to the second, and from the second to the third problem.

any subject prior to her choosing in part 2, which produced data on cognitive performance. We preset outcomes in part 1 to induce required variation in financial history. There were four histories induced out of the possible 8. These were **HHH** (high outcome in all three problems), **LHH** (low outcome in problem 1, high outcome in problems 2 and 3), **HLL** (high outcome in problem 1, low outcome in problems 2 and 3), and **LLL** (low outcome in all three problems). Subjects were randomly assigned to the histories.

In the control condition, subjects did not observe outcomes of choices in part 1 until the end of part 2. In the treatment conditions, they observed outcome immediately after choosing for every problem in part 1. The outcome report was accompanied by a statement as to whether the outcome was high or low, together with a respectively congratulatory or commiserative term, and a smiley, respectively happy or sad (for examples see Figures 3.7 and 3.8). Subjects in the control condition therefore faced the same sequence of financial decision problems as those in the treatment conditions, but differed in that they were unaware of the outcomes of the problems from part 1, and hence had no history of financial outcome, at the time of choosing in the problem from part 2.

We had a total of 180 subjects, 60 in the control condition, and 120 in the treatment conditions (30 facing each outcome history). Subjects were students of various colleges of Delhi University from humanities or social sciences backgrounds, with economics excepted. The pool was restricted in this manner to increase the chance that subjects would have a limited mathematics training, and thus would find the problem from part 2 quite hard. Participation was on an individual basis: each subject was given a pen and a sheet of paper, and seated in front of a computer on arrival, and then administered instructions for part 1 (see Figure 3.9). After completing part 1, the subject received instructions for part 2 (see Figures 3.10 and 3.11) along with Figures 3.2 and 3.3.

The subject left after completing decisions and getting paid in private. We collected gender and family income information at this time. Nearly all subjects took less than 25 minutes, and the average subject received INR 200.[4]

[4]The purchasing power parity exchange rate between the Indian Rupee and the US Dollar for 2010 was 16.84 rupees to a dollar according to the Penn World Tables (see Heston, Summers and Aten [49]).

3.2.1 Discussion of Design

Controlled induction of financial outcome histories, which we provide for part 1 in the treatment conditions, links us to an earlier literature on financial decision-making which studies how past outcomes affect choices in purely risky environments (see e.g. Thaler and Johnson [90] and Weber and Milliman [93]). The two-part procedure we have adopted is borrowed from this literature, as is the presetting of financial outcome history. The latter considerably eases the task of inducing precise outcome histories for part 1, which exists to create a history, and where subject decisions are not the object of analysis.[5]

Pilot studies suggested the use of a sequence of three problems in part 1. Since we were primarily interested in inducing high or low financial outcome histories, we chose binary options with outcomes identifiably either high or low for every option in every problem. 8 histories are possible with three problems, with some of them not easily classifiable as either high or low. We restricted the set of financial histories actually induced for this reason. The logical limit would have been to restrict the set only to **HHH** and **LLL**, but we allowed 4 histories (including **HHH** and **LLL**) to avoid such an extremity.

Observation of outcome should impact financial concern induced by a problem (relatively) positively or negatively depending on respectively high or low outcome. We wanted to increase the differential concern along the sequence of problems in part 1 leading up to the cognitive problem in part 2. The escalation features were introduced for this purpose. Given these features, the differential concern induced should increase along the sequence of problems, and hence histories **HHH** and **LHH** should have similar effects on decisions in part 2, as should **LLL** and **HLL**. We therefore allowed histories **LHH** and **HLL** along with **HHH** and **LLL**, but disallowed **HLH**, **LHL**, **HHL** and **LLH**, to create the set of 4 allowable histories. The statement in the outcome report with its emphasis on whether outcome had been high or low, the congratulatory/commiserative term and the smiley were chosen to accompany the report to provide reinforcement to the concern differential.

[5]The issue of presetting is evidently relevant only for the treatment conditions, but we preset outcomes in the control condition also to maintain parity.

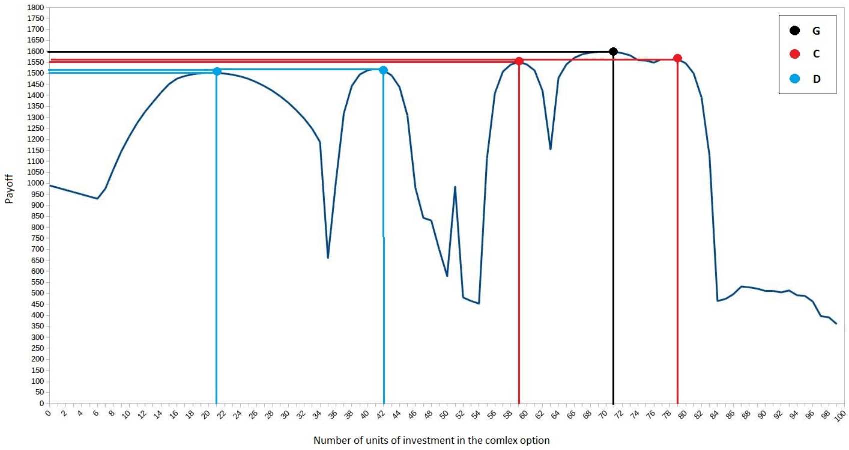


Figure 3.1: Pay-off curve

3.3 Preliminary Analysis, Treatments, Hypotheses

Subjects knew at the beginning of part 2 that they had an endowment of 100 and that the simple option yielded a return of 10 units per unit of investment, and were also provided the return function for the complex option (return from the complex option as a function of the number of units of investment in it) vide Figures 3.2 and 3.3. They were not however provided any table or figure for the aggregate payoff function (= xy(x) + 10(100-x), where x is the investment in the complex option, and y(x) is the return from the complex option given x units of investment in the complex option): see Figure 3.1.

The return function for the complex option had five local maxima, one of which was the *global* maximum. This structure was replicated for the aggregate payoff function, with the global maximum achieved at 71 units of investment in the complex option (identified as **G** in the figure). Of the four remaining (non-global) local maxima, two were relatively *close* to the global maximum (one was achieved for 60 units of investment in the complex option, and the other for 79 units of investment in the complex option, identified as **C** in the figure). The other two were relatively *distant* from the global maximum (one was achieved for 21 units of investment in the complex option, and the other for 42 units of investment in the complex option, identified as **D** in the figure).[6]

[6]The gap in terms of points obtainable between the two close maxima was about 1.5% of each other, and the same was true for that between the two distant maxima. The gap between the optimum and the close maxima was in the range of 2 - 3%, and that between the close and distant maxima was in the range of 2 - 4%. The gap between the optimum and the least payoff-yielding distant maximum was about 6%.

Rationality is sometimes associated with the absence of cognitive limits (see e.g. Hahn [46]). For the problem in part 2 this benchmark would imply that all subjects should achieve the global maximum, i.e., choose 71 units of investment in the complex option. Allowing for the possibility of errors, we would at least expect subjects to choose a level of investment close to 71 units. Keeping this possibility in mind, we will assume that a subject who chooses to allocate 71 ± 2 in the complex option is achieving the global maximum. We will say that any subject who allocates in this range is in category G.

All subjects may not achieve the global maximum however if cognitive limits matter. Indeed some, presumably those most susceptible to cognitive pressures, may not achieve any maximum, global or local, at all. For this purpose, we assume that a subject who chooses to allocate 60 ± 2 or 79 ± 2 in the complex option is achieving a close local maximum. We will say that any subject who allocates in either of these ranges is in category C. Further, we assume that a subject who chooses to allocate 21 ± 2 or 42 ± 2 in the complex option is achieving a distant local maximum. We will say that any such subject is in category D. Finally, we assume that a subject who chooses to allocate an amount in the complex option outside any of the ranges specified above is not achieving an optimum. We will say that any such subject is in category N.

Our data does not support the hypothesis that all subjects achieve the global maximum or near it. Of our 180 subjects, we found that 29 were in category G, while 66, 29, and 56 were respectively in categories in C, D, and N[7]. These findings suggest that the allocation problem was in fact cognitively demanding, and that there is variation in cognitive performance among the subjects.

Nevertheless, since the allocation problem is independent of and different in structure compared to the problems faced in part 1, the benchmark leads to the expectation that the rate of achievement of the global maximum, or the other maxima, or the rate of non-achievement of any maximum should be independent of the specific history. This idea frames the hypotheses of the paper. Before presenting the hypotheses precisely, we discuss how we structure the treat-

[7] Subjects wishing to avoid cognitive load could have guaranteed themselves a minimum payoff by allocating their endowment entirely to the simple option. Of the 56 subjects in N we found that 26 of them obtained a payoff less than this minimum. In condition **H** there were such 5 subjects, in condition **B** there were such 9 subjects and in condition **L** there were such 12 subjects.

Table 3.1: High histories

	G	*C*	*D*	*N*	Total
HHH	8	10	4	8	30
LHH	7	8	4	11	30

Table 3.2: Low histories

	G	*C*	*D*	*N*	Total
HLL	3	12	6	9	30
LLL	1	15	5	9	30

ment conditions. The control (or baseline) condition, which we denote by **B**, has already been described.

3.3.1 Treatment Conditions

Our main aim was to examine whether cognitive performance in the allocation problem from part 2 of subjects who had experienced a high or positive outcome history differed from that of those who had experienced a low or negative outcome history. Given the escalation features in part 1, two of the induced histories, **HHH** and **LHH**, can be identified as high. This suggests on a priori grounds that these two histories can be clubbed together to form a single treatment condition representing a positive history of financial outcome. Table 3.1 gives cognitive performance of subjects facing these two histories and shows that indeed performance appears to be very similar across them.

The escalation features also imply that the other two histories, **HLL** and **LLL**, can be identified as low. Once again, this suggests on a priori grounds that these two can be clubbed together to form a single treatment condition representing a negative history of financial outcome. Table 3.2 gives performance of subjects facing these two histories and shows as for the previous case that performance appears to be quite similar across them.

We formally tested if indeed **HHH** and **LHH** on the one hand, and **HLL** and **LLL** on the other, generated comparable behavior. We report results in Table 3.3, which gives two-tailed p-values. The table shows that behavior in histories **HHH** and **LHH** are indistinguishable from each other, as are behavior across histories **HLL** and **LLL**.

Visual inspection of Tables 3.1 and 3.2 indicate that behavior patterns in histories **HHH**

Table 3.3: History comparisons

	HHH vs LHH	HLL vs LLL
proportion of *G*	0.7656	0.3006
proportion of *C*	0.5731	0.4363
proportion of *D*	1	0.7386
proportion of *N*	0.4051	1

Table 3.4: Other history comparisons

	HHH vs HLL	LHH vs HLL	HHH vs LLL	LHH vs LLL
proportion of *G*	0.0953*	0.1659	0.0114**	0.0227**
proportion of *C*	0.5921	0.2733	0.1904	0.0631*
proportion of *D*	0.4884	0.4884	0.7177	0.7177
proportion of *N*	0.7745	0.5839	0.7745	0.5839

*Entries are two-tailed p-values. * and ** respectively indicate significance at the 10% and 5% levels.*

and **LHH** differ from those in histories **HLL** and **LLL**. Comparison tests reported in Table 3.4 indeed show that behavior in **LLL** differs from that in **HHH** or **LHH**. Tests also show behavior in **HLL** can be distinguished from that in **HHH**. However, tests do not reveal significant difference in behavior distinguishing **HLL** from **LHH**.

For this reason, we combine histories **HHH** and **LHH** to form one treatment condition, which we denote by **H** for high, and also combine histories **HLL** and **LLL** to form another treatment condition, which we denote by **L** for low. Table 3.5 summarizes performance of subjects across the treatment conditions and also the control or baseline condition.

3.3.2 Hypotheses

As discussed above, the null hypothesis is that cognitive performance of subjects is invariant to condition, i.e., the proportions of subjects in the different categories (G, C, D, N) are the same across the three conditions **H**, **B** and **L**. An alternative that can be posited against this is that subjects with a negative financial history will produce worse performance than those with

Table 3.5: Conditions

	G	*C*	*D*	*N*	Total
H	15	18	8	19	60
B	10	21	10	19	60
L	4	27	11	18	60

a positive financial history. This alternative can be expressed more precisely as the following: the proportion of subjects in category G will be higher in **H** than in **L**, while the proportions of subjects in categories C, D and N will be lower in **H** than in **L**.

This alternative can be justified by the argument, as in Mani et al [67], that a negative financial outcome history will generate great preoccupation and distraction, and hence weaker performance due to the non-availability of cognitive resources. A question is how the experience of a positive financial outcome history will affect the distribution of cognitive resources. The presence of condition **B** allows some responses to this question.

One possibility is that exposure to a history of financial outcome will generate preoccupation no matter whether the history is positive or negative. This would imply that performance of subjects in **H** would be worse than those in **B**. More precisely, the alternative then becomes the following: the proportion of subjects in category G will be higher in **B** than in **H** than in **L**, while the proportions of subjects in categories C, D and N will be lower in **B** than in **H** than in **L**. We dub this the preoccupation hypothesis.

The converse possibility is that exposure to a positive history of financial outcome will generate encouragement or elation and perhaps a temporary augmentation of cognitive resources.[8] This would imply that performance of subjects in **H** would be better than those in **B**. More precisely, the alternative then becomes the following: the proportion of subjects in category G will be higher in **H** than in **B** than in **L**, while the proportions of subjects in categories C, D and N will be lower in **H** than in **B** than in **L**. We dub this the encouragement hypothesis.

3.4 Results

We now test if cognitive performance differs across the conditions. The data presented in Table 3.5 suggest that there is little difference across the conditions as far as non-achievement of any optimum (the proportion of subjects in category N) is concerned. Additionally, while the number of subjects in category D is slightly higher in **B** than in **H**, and it is highest in **L**, the differences between the three conditions is quite small, indicating the number is more or less

[8] For prior evidence that encouragement and feedback can lead to superior goal pursuit, cognitive augmentation and improved performance, see e.g. Kluger and DeNisi [60], Goleman [41], Hattie and Timperley [48] and Fishbach, Eyal and Finkelstein [35].

Table 3.6: Comparing conditions (univariate)

	H vs B	B vs L	H vs L
proportion of G	0.1305	0.044**	0.003***
proportion of C	0.2794	0.1318	0.0448**
proportion of D	0.3046	0.4051	0.2266
proportion of N	0.5	0.5784	0.5784

*** and *** respectively indicate significance at the 5% and 1% levels.*

invariant across the conditions.

For C, the data indicate that there are more subjects in this category in **L** than in **B** than in **H**. The gap between **L** and **H** is fairly large, while the numbers are close across **B** and **H**. The ordering across the conditions is reversed for category G, with the number of subjects in this category highest for **H**. The gaps, particularly for that between **L** and **H**, are also not small.

For each category, we formally test if the proportion of subjects in it differs across the conditions. The data from Table 3.5 indicate it's unlikely we shall find support for the null as far as categories G and C are concerned. Indeed the relevant numbers point to the encouragement or the latter of the alternative hypotheses receiving greatest support. For categories D and N however, Table 3.5 suggests that the null may remain unrejected.

Results are presented in Table 3.6 which gives one-tailed p-values from proportions tests. While the number of subjects in category G is higher for **H** than for **B**, the difference is not significant. The number of subjects in category G is higher for **B** at the 5% level than for **L**, however. Further, it is higher for **H** than for **L** at the 1% level. For category C similarly, **H** and **B** cannot be distinguished. Moreover, the number of subjects in C cannot be distinguished across **B** and **L**, while it is lower in **H** than in **L** at the 5% level. Finally, as expected, the null hypothesis receives support for categories D and N.

These findings thus indicate that a positive or negative history of financial outcome can indeed create difference in cognitive performance. The performance difference also follows a very particular pattern. No differences are induced as far as achievement of a non-optimum is concerned. Differences as far as achievement of a distant maximum are also slight and insignificant. Differences as far as achievement of a close maximum are significant, with a more positive history making it less likely to be achieved. Differences are also significant as far as achievement of the global maximum is concerned, with a more positive history making it more likely to be achieved.

As far as the distinction between **H** and **B** is concerned, although visual inspection suggests support for the encouragement rather than the preoccupation hypothesis, tests reveal no significant difference in subject performance across them, resulting in support for the null.[9] This leaves us unable to formally determine how the experience of a positive financial outcome history affects the distribution of cognitive resources. We now introduce control variables to ascertain the extent to which the findings reported above are robust.

3.4.1 Multivariate Comparisons

For each subject, we have data on her choices for the three problems for part 1. In addition, we have data on gender for each subject, which the experimenter recorded based on visual observation. We also requested each subject to reveal which of 6 slabs best represented her annual family income.[10] We now compare the proportions of subjects in the different categories (G, C, D, and N) across conditions through a regression approach with the variables described above used as regressors.

We ran three probit regressions for each category, one each comparing **H** and **B**, **B** and **L**, and **H** and **L**. The dependent variable was a dummy coded as 1 if the response put the subject in the category in question, and 0 otherwise. A condition dummy was also used as an independent variable. This dummy took the value 0 for **B** and 1 for **H** in the first regression, 0 for **L** and 1 for **B** in the second regression, and 0 for **L** and 1 for **H** in the third regression.

Coefficients on the choices for the problems from part 1 were rarely significant in any of the regressions. When significant, no consistent pattern was indicated by the signs of the coefficients. These findings suggest that choices for the problems in part 1 are essentially unrelated to choice for the problem in part 2, and are expected, as the problems from part 1 are structurally quite different from that in part 2. We also found no impact of gender or income: the coefficients for income and gender were insignificant in respectively all and virtually all

[9] Tests reveal more subjects in category G for **B** than for **L**, which is inconsistent with the null, but consistent with both the encouragement and the preoccupation hypotheses.

[10] The slabs were (all figures in INR): 1 - less 100,000, 2 - between 100,000 and 250,000, 3 - between 250,000 and 500,000, 4 - between 500,000 and 750,000, 5 - between 750,000 and 1,000,000, 6 - more than 1,000,000.

Table 3.7: Comparing conditions (multivariate)

	H vs B	B vs L	H vs L
proportion of G	0.3707	0.5302	0.9219***
	(0.2896)	(0.3578)	(0.3325)
proportion of C	-0.1237	-0.3003	-0.4538*
	(0.2588)	(0.2513)	(0.2447)
proportion of D	-0.3413	0.0356	-0.1643
	(0.3123)	(0.2958)	(0.2943)
proportion of N	0.0695	0.0444	0.0545
	(0.2561)	(0.2656)	(0.2436)

** and *** respectively indicate significance in terms of two-tailed p-values at the 10% and 1% levels.*

the regressions. The finding for income has the potential to raise concern, as Mani et al [67] had found in their laboratory experiment that higher income results in superior cognitive performance. However our study and theirs have many differences, the most important possibly being that they did not induce different financial outcome histories: our findings can thus be taken as indicating that income has no impact on cognitive performance once past financial outcome histories are controlled for.[11]

Results with respect to treatment differences are presented in Table 3.7. Each cell gives the coefficient on the condition dummy variable, and reports the associated standard error. We do not report coefficients on any of additional regressors due to insignificance, as outlined in the paragraph above.[12]

Table 3.7 shows that there are no statistical differences in cognitive performance between **B** and **H**. Differences were also not found between **B** and **L**. Differences in cognitive performance emerged however between the treatment conditions **H** and **L**, with subjects more likely to achieve the global maximum in **H** at the 1% level, and less likely to achieve a close local maximum at at the 10% level.

Comparison of Tables 3.6 and 3.7 thus shows that the non-distinguishability between **B** and

[11]While the coefficient on the income variable was insignificant in all 12 regressions, it had a positive sign in all 3 regressions relating to category G, suggesting that higher income could make it more likely that a subject would achieve the global maximum. Also, the coefficient had a negative sign in 2 of the 3 regressions relating to category N, suggesting that higher income could make it more likely that a subject would achieve some maximum, global or local. These findings are aligned with those of Mani et al [67].

[12]With respect to the comparisons between **H** and **L**, we also examined a specification which controlled for the effect of particular history. The results hardly changed with the introduction of this additional control, and showed history had no impact once treatment was controlled for: this is of course consistent with the discussion in Section 3.3.1.

H persists when control variables are introduced. While **B** was distinguishable from **L** in univariate comparison in terms of the proportion of subjects achieving the global maximum, the introduction of controls renders the two conditions indistinguishable. Further, the comparison shows that the introduction of controls does not disturb the distinguishability of the two treatment conditions in terms of the rates of achievement of the global maximum and close local maxima.[13]

Overall, our results show that the proportions of subjects in categories *D* and *N* are invariant across the conditions. Since these subjects are the ones with weakest cognitive performance, this finding points to the possibility of the cognitively less able being immune to personal experience of financial decisions or outcome in terms of their performance. For categories *G* and *C*, the results clearly reject the null for the comparison between **L** and **H**, and indicate that negative prior financial outcome history causes greater preoccupation and distraction and thereby worse performance. The comparison between **B** and the two treatment conditions does not yield an unequivocal answer. There is certainly a suggestion in our data and results (see Tables 3.5 and 3.6) that performance in **H** (**L**) is superior (inferior) to that in **B**, supporting the encouragement rather than the preoccupation hypothesis, but differences are not statistically significant in multivariate comparisons.

3.5 Conclusions

This paper shows that financial outcomes induced in the laboratory can produce cognitive impacts: subjects who faced a negative financial outcome history in a series of binary lottery choice problems performed poorly, relative to those who faced a positive financial history, in a subsequent cognitively demanding deterministic portfolio allocation problem. The difference in performance followed a particular pattern, with a negative outcome history making it more likely that a close local maximum, and less likely that the global maximum, of the aggregate payoff function will be achieved. These findings are consistent with negative histories being

[13] We also attempted to isolate the determinants of cognitive performance: what affects the likelihood a subject would achieve a particular category (*G*, *C*, *D*, *N*)? Different specifications indicated experimental condition to be the only substantive determinant. In particular, gender was mostly insignificant. Income too did not assume significance within any condition; however, there were indications in every condition that higher income made it more likely a subject would be in category *G*, and less likely that she would be in category *N*. See also fn 11.

accompanied by more preoccupation and distraction and hence weaker performance.

Subjects facing a positive history also tended to perform better than those who faced the same sequence of financial decisions, but did not observe outcome, and hence were not equipped with a financial outcome history, at the time of taking the cognitively relevant decision. This finding, with the proviso that differences between the two conditions were not significant, is consistent with a positive history producing encouragement leading to cognitive augmentation rather than preoccupation consuming cognitive resources.[14]

The proportions of subjects achieving a distant local maximum or choosing clearly sub-optimally were found to be invariant across the conditions. This suggests that financial choices of cognitively weaker segments may not respond to personal financial experience.

Our results point to potential fragilities in financial decision making in cognitively demanding environments. In real life settings say of unorganized activity in developing economies, where mostly poor individuals face unpredictable income insecurity, this would imlpy that small negative shocks can result in long term negative consequences. Such a possibility points to the challenges facing income security or anti-poverty policies being sharper than previously conceived. Another implication is that indirect antipoverty policies which leave decision making fully up to the target, such as those based on cash transfers, may not be as effective as usually thought, and that measures may have to include a direct interventionary component targeted at cognitively weaker segments.

[14]A tendency was also detected for better performance in the control relative to that in the negative history condition, with significance emerging in a univariate comparison.

Appendix

Investment: Number of units of investment in option 2
Return: Return from option 2

Investment	**Return**	*Investment*	**Return**	*Investment*	**Return**	*Investment*	**Return**
0	0	26	735	51	88	76	1318
1	0	27	730	52	504	77	1319
2	0	28	722	53	11	78	1345
3	0	29	711	54	5	79	1361
4	0	30	696	55	3	80	1364
5	0	31	677	56	672	81	1356
6	0	32	653	57	980	82	1320
7	0	33	625	58	1088	83	1220
8	56	34	589	59	1130	84	965
9	153	35	538	60	1151	85	315
10	245	36	21	61	1151	86	334
11	323	37	378	62	1133	87	366
12	394	38	699	63	1050	88	411
13	456	39	833	64	795	89	417
14	510	40	895	65	1130	90	421
15	563	41	924	66	1201	91	420
16	611	42	942	67	1241	92	431
17	646	43	943	68	1267	93	434
18	668	44	931	69	1284	94	453
19	686	45	888	70	1298	95	441
20	701	46	768	71	1309	96	448
21	713	47	450	72	1317	97	432
22	722	48	323	73	1322	98	376
23	729	49	321	74	1322	99	381
24	734	50	199	75	1310	100	380
25	736						

Figure 3.2: Table for complex option given to subjects

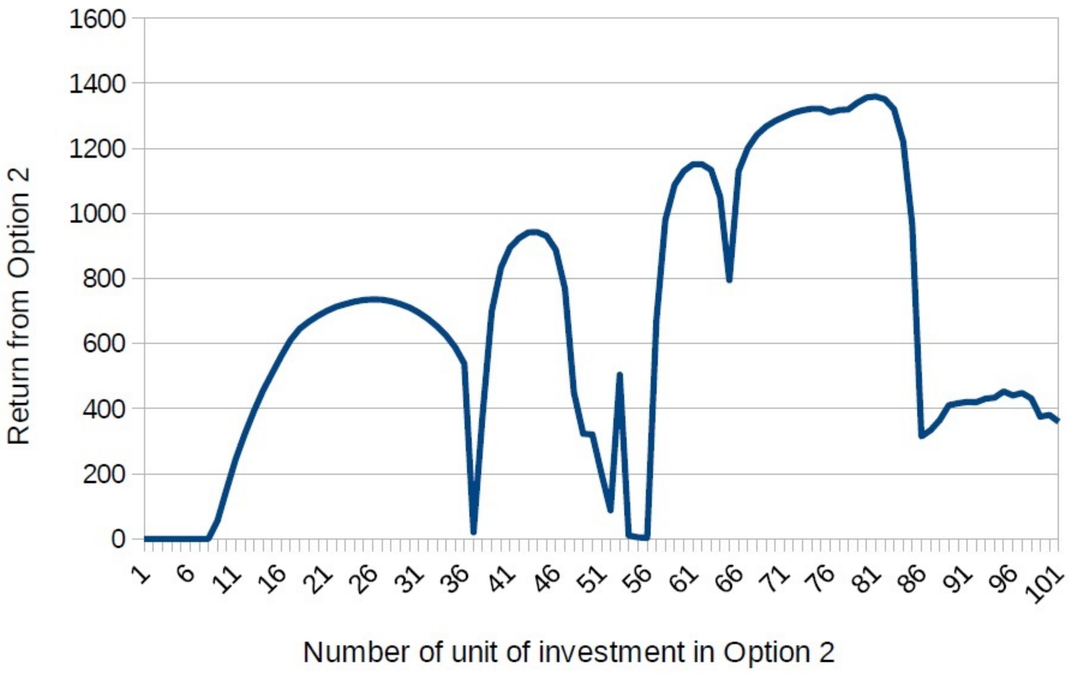


Figure 3.3: Figure for complex option given to subjects

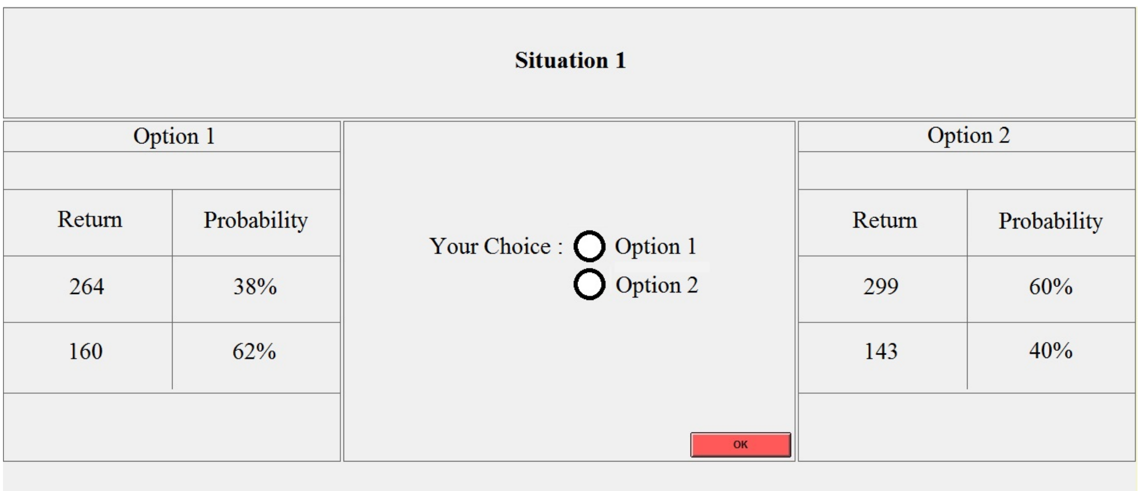


Figure 3.4: Problem 1 of Part 1

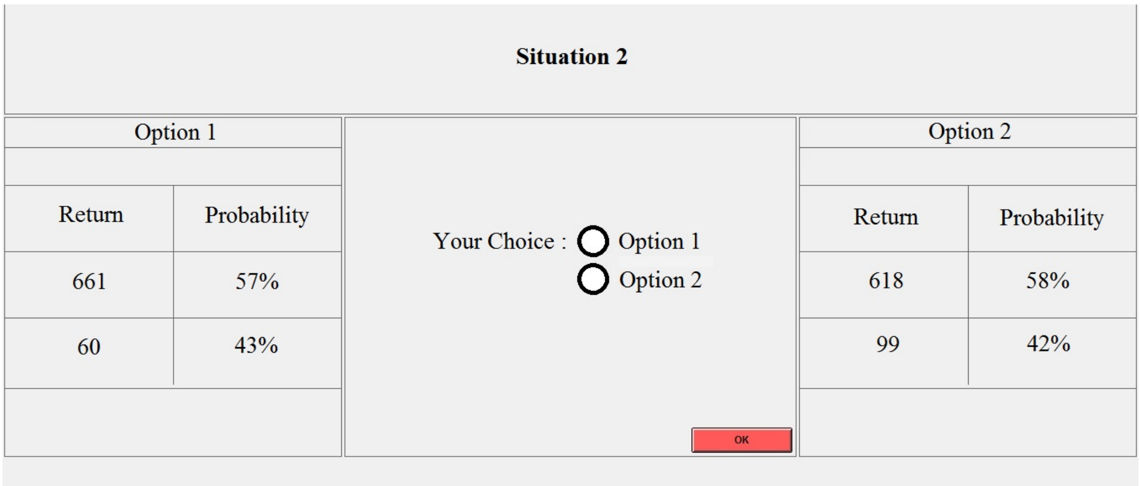


Figure 3.5: Problem 2 of Part 1

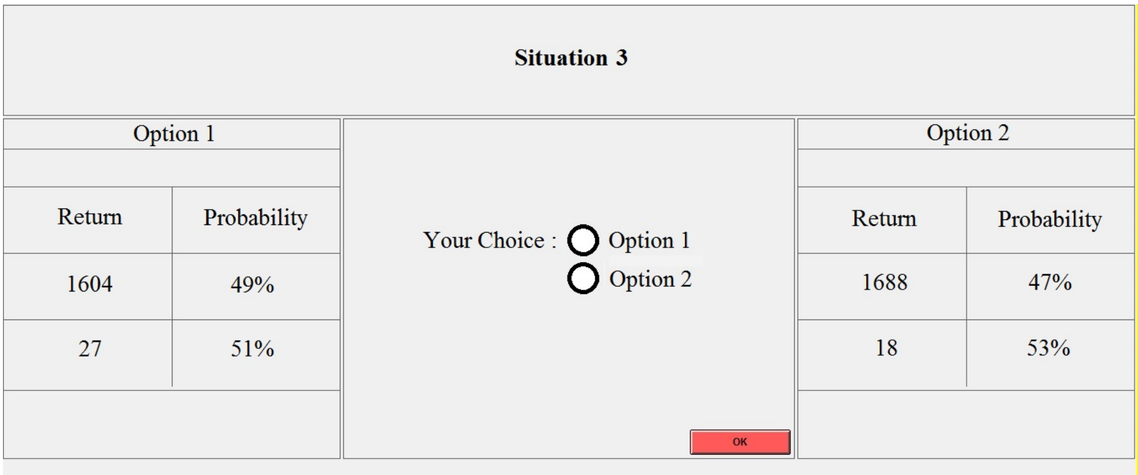


Figure 3.6: Problem 3 of Part 1

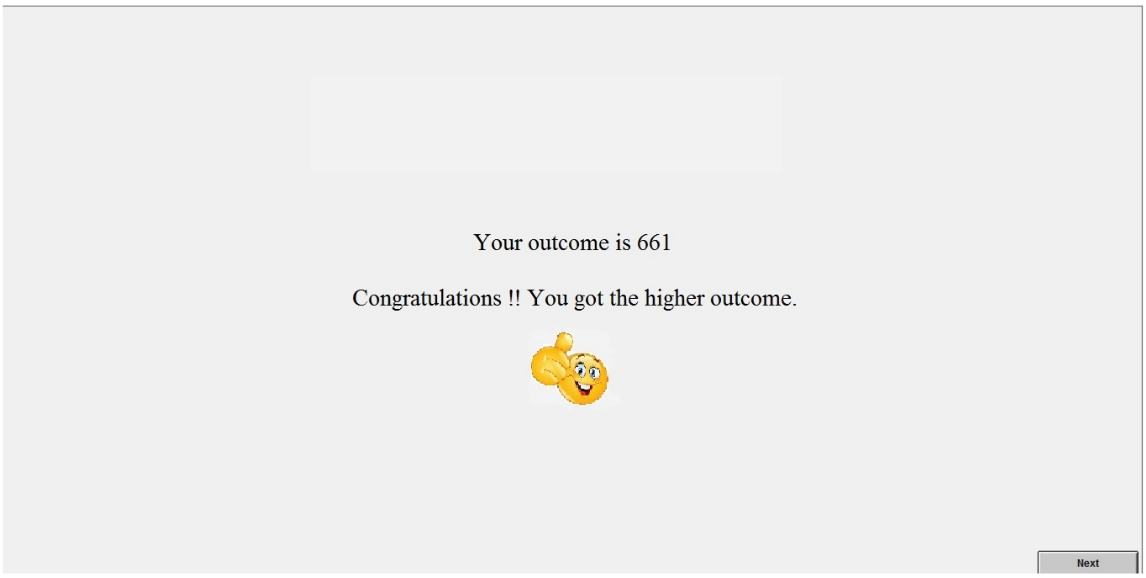


Figure 3.7: High outcome

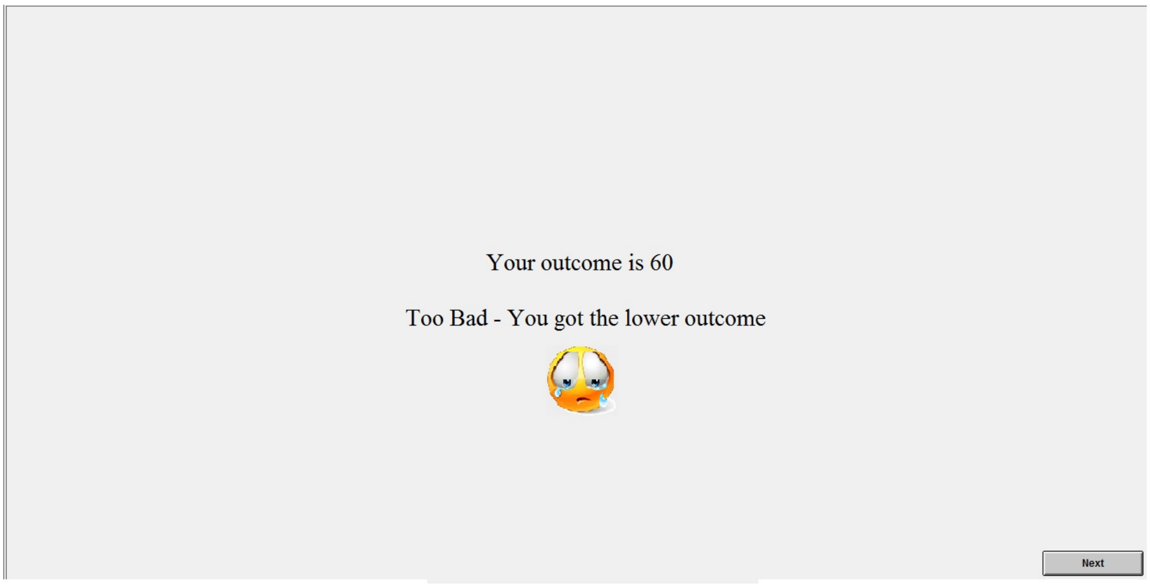


Figure 3.8: Low outcome

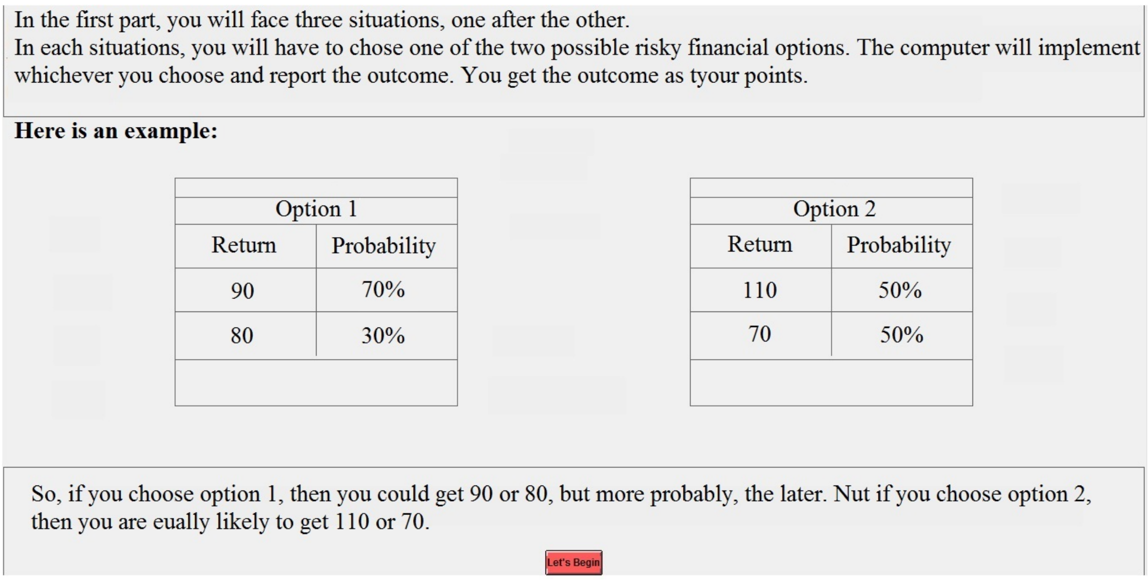


Figure 3.9: Instruction for part 1

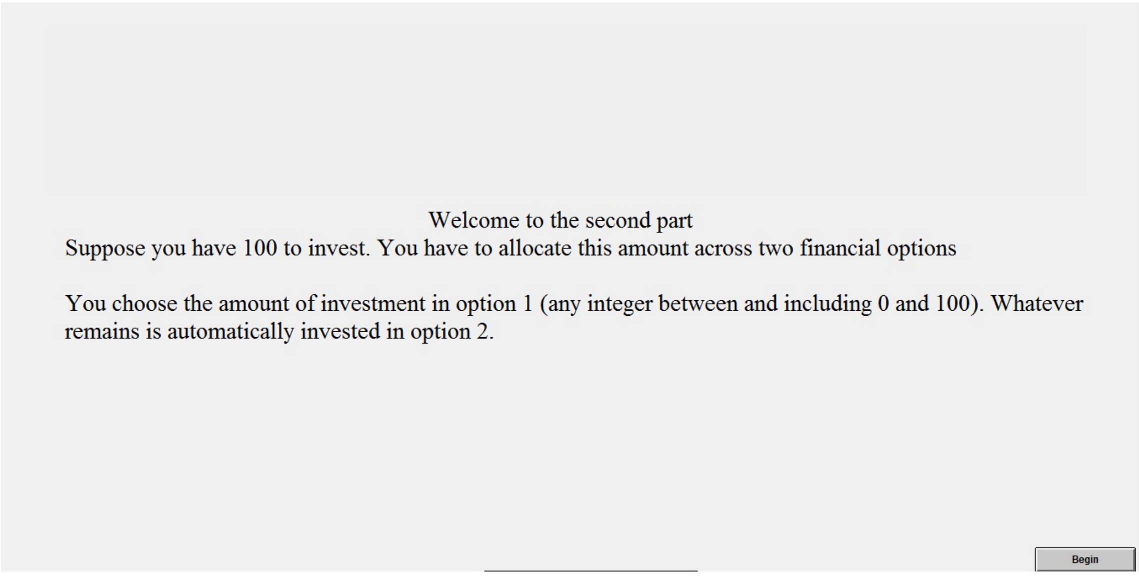


Figure 3.10: Instruction for part 2

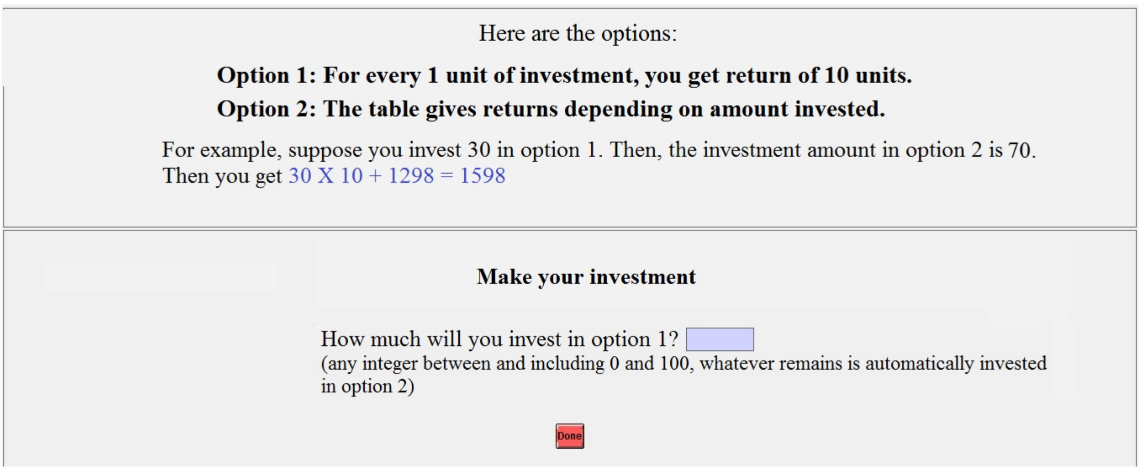


Figure 3.11: Decision screen for part 2

Chapter 4

The Impact of a Deadline with Decision under Risk

4.1 Introduction

Deadlines are ubiquitous in daily life, as well as in a variety of settings such as corporate and contract negotiations, judicial and administrative decision-making, legislative bargaining, market transactions etc. Given their omnipotence, a natural question is whether deadlines exert economic impacts.

This question is not easy to answer based on existing literature, as empirical research on the effect of deadlines in economic environments is sparse. In particular, there has been limited research on the impact of deadlines in individual decision settings. The problem is compounded by the fact that it has proved difficult to collect reliable happenstance data on deadlines and their effects, in part because it is often not known how 'hard' a stated deadline actually is in practice, as deadlines often tend to get violated, extended or renegotiated. This chapter aims to contribute to the existing literature by investigating through a laboratory experiment whether a deadline can leave any impact in the context of decision under risk.

The conventional theoretical description of decision under risk does not leave room for a deadline. This might leave the impression that theory suggests deadlines will have no impacts. However, if one assumes that decision-making is not instantaneous and requires cognitive resources, then a deadline can have at least two impacts. One, it can cause a reallocation of cognitive resources to the task which needs to be completed within some time, leading to faster or better completion, and two, it can directly consume cognitive resources through focus on it, leading to slower or worse completion.

Under the latter scenario, a deadline can become a binding constraint on decision making. In such a situation, conventional theory suggests that a deadline would be associated with a shadow cost. This chapter aims to explore this possibility to identify whether a deadline can be binding, and to provide a measure of the shadow cost if it is.

A two-phase experiment was conducted to investigate these issues. Subjects in treatment conditions faced 20 risky investment choice problems, all identical in structure, in the second phase. Each problem represented a payoff prospect (payoff conditional on choice was equal in expected terms across all problems), and subjects received payment only for those problems completed within a deadline. The deadline was set on an individual basis, to account for the fact that subjects may take different amounts of time to complete the task normally, i.e., without any deadline. These individual level normal times were calculated from the first phase, where subjects faced 20 risky choice problems, all identical in structure to those in the second phase. The time a subject took to complete these problems in the first phase was used as the deadline in the second. This endogenous derivation of subject-specific deadlines based on subject level behavioral data is a novel design element and our main, methodological, contribution, which separates us from all previous studies involving deadlines.

Comparison of investment choices and times taken across the two phases in the treatment conditions as described above to identify effects of a deadline is confounded. This is because learning or familiarity may differentiate behavior in the second phase relative to the first even without any deadline. The presence of equivalent control conditions enabled comparison. Control and treatment conditions were identical except that there was no deadline in the second phase for subjects in the former.

Deadlines were found to be binding for about 10% of treatment subjects. The utilization rate for these subjects for their 20 potential prospects was about $2/3$ on average. The likelihood of completing all 20 problems and the number of problems completed were found to be increasing in the deadline, with this tendency more pronounced in the presence of higher risk. The average number of problems completed across all treatment subjects fell short of the maximum by about 5%. This gap was significant, suggesting that deadlines can impose an economic cost.

A deadline appeared to induce some change in investment choices. Treatment subjects who completed the task within the deadline showed a slight tendency toward less risk-averse behavior compared to their control counterparts. Treatment subjects who failed to complete the task within the deadline did not however demonstrate such a defined pattern.

As far as times taken are concerned, treatment subjects completing all 20 problems in the second phase tended to be prompter in task completion compared to their control counterparts (by about 15 - 20% on average), particularly in the presence of higher risk. This points to a potential benefit of deadlines, in that these treatment subjects could have used the time saved in alternative productive tasks. Our design is not however capable of providing a measure of such benefits, as these subjects were not allowed any alternative opportunities after completion of the task within the deadline.

The rest of the chapter is organized as follows. Section 4.2 reviews related literature. The next section details our design and procedure, while Sections 4.4 presents some analytical preliminaries. Section 4.5 provides and discusses results, while Section 4.6 contains concluding observations. Figures (including instructions) are relegated to the Appendix.

4.2 Related Literature

A significant experimental economics literature exists studying the effect of deadlines on behavior in strategic settings. The most important strand is on bargaining under deadlines: see e.g. Roth, Murnighan and Schoumaker [80]. This literature looks at the impact of deadlines on inefficiency and delay in Rubinstein-Stahl and other strategic bargaining frameworks. Other strategic analyses include Kocher and Sutter [62], who focus on the impact of deadlines and time-dependent incentives on choice in a beauty contest game. The existence of strategic interactions differentiates this literature sharply from the current enquiry, which is in a decision-theoretic setting.

The exercise in this chapter continues the recently emerged investigation into the possible efficiency impacts of deadlines: see e.g. Ariely and Wertenbroch [7], Burger, Charness and Lynham [17], and Bisin and Hyndman [13]. A major difference between the current exercise and prior papers is that earlier literature has concerned itself with the impact of deadlines in the context of procrastination, whereas the possibility of procrastination plays no role in our setting. This feature renders some differences in questions asked in the current paper relative to those in the literature. There are also several differences between the approach in these papers compared to ours. Firstly, these articles use long duration tasks (lasting several days, and sometimes several months) typically in a field or online setting, whereas our tasks are in the laboratory and of short duration (usually taking less than 30 minutes). Secondly, we construct our tasks using standard risky choice problems, whereas these papers use tasks (mostly related to course material or syllabi, or studying or doing homework) which cannot be readily fitted within that framework. Thirdly, and this goes to the point raised above related to procrastination, the tasks studied by earlier papers typically have a binding exogenous final deadline, with the experimental deadlines applying at interim stages (an exception is Study 2 of Ariely and Wertenbroch [7]), while there is no distinction between interm and final deadlines in our environment. Fourthly, and most importantly, a subject's deadline in our environment is individual-specific, exogenously imposed (by the experimenter), but endogenously derived using that subject's behavioral data, whereas these essays allow either an exogenous deadline which is not subject-specific (i.e., not endogenously derived), or one that is set by the subject herself.

A debate within this literature is whether a deadline can improve task performance or completion rates, i.e., yield efficieny gains. Ariely and Wertenbroch [7] find that deadlines can have beneficial impacts. Burger et al [17] and Bisin and Hyndman [13] find to the contrary no such positive effects. Our findings are mixed, though mainly in support of the latter papers. On the one hand, imposition of a deadline caused some subjects to fail to complete the task, so much so that the averate rate of task completion fell short of the maximum feasible, generating a deadline cost. Additionally, task completion rates increased with the deadline set, suggesting that tighter deadlines can impose greater costs. On the other hand, subjects who did complete the task within the deadline tended to save time, pointing to the possibility of some potential benefit.

The enquiry in this chaper is also related to those in three other lines of literature. First, a psychology literature, starting with Ben-Zur and Bresnitz [12] (see also e.g. Dror, Busemeyer and Basola [30] and Madan, Spetch and Ludvig [66]), studying the effects of time-pressure or deadlines on decision under risk. This literature concerns itself mainly with changes in mental and affective states and decision strategies induced by deadlines, with deadlines being set without reference to subjects' native behavior. Most papers have found that time pressure tends to yield riskier choices, a finding which is weakly confirmed by our results. The literature has also largely found that decisions tend to get accelerated by deadlines. We find similar evidence of such decision acceleration, for subjects who managed to complete the task within the deadline. For the rest of the subjects, by definition, we find contrary evidence of decision paralysis.

The second is an emergent economics and finance literature, intimately related to the line above, studying how risk and/or reward preferences are affected by time pressure: our paper is closely related to this strand and complements it. Our approach differs from existing papers in two main ways. First, there a difference in focus, as prior essays do not consider the efficiency costs of deadlines. Second, the deadline is the same for all subjects in any given condition, leaving no room for endogenously derived subject-specific deadlines, which is a crucial element in our design. The two papers in this strand are Kocher, Pahlke and Trautmann [61] and Nursimulu and Bossaerts [76].[1] Kocher et al [61] analyze whether time pressure causes differential change in decision makers' risk preferences over gain and loss domains for prospects. The main finding is that time pressure does not affect risk attitudes for gain domains, but increases risk-aversion in loss domains. This is partially in constrast to our finding, and the majority finding from the psychology literature cited above, that deadlines can reduce risk-aversion. Nursimulu and Bossaerts [76] compare the performances of classical moment based theory and prospect theory in the presence of time pressure, and find that the former has greater explanatory power. They also find some evidence of decreased risk aversion under deadlines, which is in line with our finding.

[1] See also Lindner and Rose [63] who study the impact of time pressure on intertemporal allocation decisions.

The third is a nascent political science literature using happenstance data, which, as here, is interested in analyzing the costs (and benefits) of deadlines. The literature (see e.g. Carpenter, Zucker and Avorn [20], Gersen and O'Connell [37] and Yackee and Yackee [94]) looks at deadlines as exogenous impositions by elected officials on bureaucratic or judicial decision-makers in a US context as a means of elected officials exercising control over the duration and other aspects of administrative or judicial decision processes. The concerns mainly focus on the impact of deadlines on the expected time to decision and regulatory uncertainty, but also cover error rates.

4.3 Design and Procedure

There were a total of four conditions, two control conditions, and two treatment conditions. The two control conditions differed from each other only through the level of risk in the economic environments presented to subjects. The same holds for the two treatment conditions.

We first describe the high-risk control condition, which we denote by **CH**. The experiment was in two phases, separated by about a month. Subjects were informed of this fact at the time of recruitment, though they were not told anything about the nature of decisions to be made in either phase, except that they were going to participate in a study on financial decision-making.

Subjects faced 20 allocation problems in the first phase, presented in a fixed sequence. For each problem a subject had to allocate a budget of 100 units across two financial options (see Figure 4.1). One was a safe option yielding return 100 units per unit invested. The other was a risky option which had three possible returns, with probabilities given for each. The risky option yielded an expected return of 225, and had a variance of 20,000, per unit, though subjects were not made aware of these two facts. Subjects were given a worked out example, and faced two trial problems, all identical in structure to the 20 main problems, before commencing decisions for the 20 payoff-yielding problems (see Figures 4.2, 4.3 and 4.4).

The second phase of this condition was structurally identical to the first phase. Subjects completing phase 1 were not however told anything about the nature of decisions to be made in the second phase (except, as mentioned above, they were already aware they were participating in a study on financial decision-making). Subjects first saw a worked out example (which was the same as that given for the frst phase), and then faced two trial problems (which were different from those from the first phase), after which they had to choose allocations for 20 problems in a fixed sequence, all identical in structure to the 20 problems from the first phase (see Figure 4.5). They were not given any reminders about choices from the first phase, nor any information about outcomes from the first phase, at the beginning of the second, to avoid issues of reference-dependence.

We now describe the low-risk control condition, which we denote by **CL**. It was identical to the high-risk control condition described above, except that the risky option in every case had a variance of 8,000 per unit (see Figures 4.6 and 4.7).

We now describe the treatment conditions, which we denote analogously by **TH** and **TL**. As for the control conditions, the low- and high-risk variants were identical to each other, except that the variance of risky options in all problems, including trial problems and the worked out example, were 8,000 for the low-risk condition, and 20,000 for the high-risk condition, per unit. Also, as for the control conditions, the experiment was in two phases.

Phase 1 of any treatment condition was identical to the first phase of the corresponding control condition. The second phase was also identical, except that subjects faced a deadline for completion of the 20 payoff-yielding problems. They knew this fact from the beginning of phase 2, and were also informed of the exact deadline, specific to each subject. The deadline counter appeared on the screen as they took decisions for the payoff-yielding problems, which process commenced after the second trial problem.

The deadline worked as follows (subjects were informed of this at the beginning of phase 2): subjects would earn points and corresponding payments only for problems for which decisions were made within the deadline. No points or payments would be received for problems not completed within the deadline. The actual deadline for any subject in phase 2 was the time taken by that subject to complete the 20 allocation problems in phase 1, rounded to the nearest minute.[2]

We had 45 subjects per condition for a total of 180.[3] Subjects were undergraduate students

[2]Subjects were not informed of the method by which her deadline was set. Rounding was done as pilot studies suggested that a deadline presented in integer minutes was more comprehensible than one presented in seconds.

[3]45 subjects per condition completed both phases. About 85% of subjects who completed phase 1 returned to complete phase 2.

from a variety of disciplines studying in colleges and institutions in the Calcutta area. Data collection was on an individual basis. Subjects had been told to bring a calculator for each phase (most used their phones). On arriving, they were given a pencil and a sheet of paper, and seated in front of a computer, after which they were administered instructions (see Figures 4.8, 4.9 and 4.10 for initial instruction screens).

We collected gender and family income information at the end of the second phase for each subject. Subjects received INR 1 for every 1250 points earned for problems completed across the two phases.[4] They also received a show-up fee of INR 100 for the first phase, and INR 200 for the second phase. The show-up fee for the first phase was given at the end of the phase. All other earnings were given at the end of the second phase in private. The average subject received about INR 800 including show up fees across the two phases.

4.4 Preliminary Analysis

We present data from the first phases of the experimental conditions in this section, and some preliminary tests using these data. This exploration sets the stage for the main analysis presented in the next section.

Two variables were measured in the first phase for every subject: the actual allocations for the 20 problems, and the time taken in seconds to complete each. Figures 4.11 and 4.12 below respectively give the distributions of per subject average allocations in the safe option and the per subject average times taken to complete 20 problems across the conditions.

Since the first phase was identical for treatment and control given level of risk, a natural hypothesis for either per subject average amount allocated to the safe option, or per subject average time taken to complete problems is that they should be invariant across **CH** and **TH**, and also across **CL** and **TL**. Also, since the average amount allocated to the safe option can be taken as a rough measure of risk-preference at the level of the subject, it can be hypothesized that the average amount allocated to the safe option should be higher in **CH** or **TH** than in **CL** or **TL**. There is no such obvious natural hypothesis available regarding comparison of average

[4] The purchasing power parity exchange rate between the Indian Rupee and the US Dollar for 2010 was 16.84 rupees to a dollar according to the Penn World Tables (see Heston, Summers and Aten [49]).

times taken across the high and low risk conditions. We take as a null that the average times taken should be invariant across all conditions. If we suppose that higher risk presents a subject with greater difficulty in reaching decisions, then a tentative alternative hypothesis is that time taken should be higher in **CH** or **TH** than in **CL** or **TL**.

The mean amount allocated to the safe option out of 100 and the mean time taken per problem in seconds for each condition are given in Table 4.1 below. The numbers show that average allocations as well as average times taken are similar across control and treatment conditions given risk level. They further show that average allocations in the safe option as well as average times taken are somewhat higher in the high-risk conditions.

Table 4.1: Means: Phase I

	Amount allocated		Time taken	
	C	**T**	C	T
H	54.94	52.74	31.16	33.32
L	46.79	49.84	28.29	30.17

Classical t-tests and Mann-Whitney (MW) tests were run with respect to the hypotheses outlined above. p-values from the two tests are given for all pairwise comparisons in Table 4.2 below. The table shows that no significant differences were recorded for times taken on average across any of the condition pairs, resulting in failure to reject the null hypotheses in any of the cases. The hypothesis that average time should be identical across treatment and control given risk level is thus supported (the first two rows under the ‘Time taken’ columns). Further, for the comparison of average time taken across high and low risk conditions, the alternative hypothesis receives no support, suggesting that risk level does not impact decision time (the last four rows under the ‘Time taken’ columns).

The table also shows indistinguishability across control and treatment given the level of risk for average allocations, supporting our hypothesis in this regard (the first two rows under the ‘Amount allocated’ columns). While average allocations in the safe option were numerically higher in the high-risk conditions in every case (see Table 4.1), the differences were statistically significant in the hypothesized direction for four out of the eight tests we conduct (the last four rows under the ‘Amount allocated’ columns). Our hypothesis thus receives some, though not perfect, support in this case.

Table 4.2: p-values: Phase I

	Amount allocated		Time taken	
	t-test	MW	t-test	MW
CH vs **TH**	0.4410	0.1558	0.6218	0.4036
CL vs **TL**	0.4312	0.6112	0.5962	0.6895
CH vs **CL**	0.0059***	0.0375**	0.3307	0.3923
CH vs **TL**	0.1175	0.0188**	0.7919	0.3451
TH vs **CL**	0.0975*	0.1579	0.2418	0.5999
TH vs **TL**	0.4522	0.3597	0.5184	0.7591

** , ** and *** respectively indicate significance in terms of two-tailed p-values at the 10%, 5% and 1% levels.*

Multivariate analysis supports the results found above through univariate comparison. We ran two pooled linear regressions, each with a dummy indicating control versus treatment condition (called codecondition, coded as 0 for treatment and 1 for control), another dummy indicating risk level (called riskcode, coded as 0 for low risk and 1 for high risk), a gender dummy (this dummy was coded as 0 for male and 1 for female), and an income dummy as independent variables.[5] The two regressions respectively had average investment and average time taken as the dependent variables.

Results are presented in Table 4.3 below. They show that average investment does not differ across control and treatment conditions (insignificance of variable codecondition), while investment in the safe option is higher for the high-risk conditions (significance and positive sign for variable riskcode).[6] The results also show that average time taken is invariant across control and treatment condition (insignificance of variable codecondition), and does not depend on whether a subject was in a high or a low risk condition (insignificance of variable riskcode). We further found that higher income lead to weakly higher investment in the safe option, and that females tended to invest more in the safe option, as well as take more time to decide.

We now move to our main results, on the effect of deadlines. For this purpose we focus on data from the second phases of the treatment conditions, but also use data from the second phases of the control conditions, as well as data from the first phases of all conditions.

[5]The slabs were (all figures in INR): 1 - less 100,000, 2 - between 100,000 and 250,000, 3 - between 250,000 and 500,000, 4 - between 500,000 and 750,000, 5 - between 750,000 and 1,000,000, 6 - more than 1,000,000.

[6]These results remained unchanged in disaggregated regressions as well, with one exception: a comparison only between **TH** and **TL** did not yield significance for the variable riskcode, though the sign was positive.

Table 4.3: Regressions: Phase I

	average investment in safe	average time taken
codecondition	-1.096	-2.259
	(2.383)	(2.796)
riskcode	5.078**	2.444
	(2.375)	(2.787)
gender	4.412*	7.774***
	(2.434)	(2.856)
income	1.489*	-0.653
	(0.798)	(0.936)

, ** and * respectively indicate significance in terms of two-tailed p-values at the 10%, 5% and 1% levels. Standard errors are in parentheses.*

4.5 Main Results: The Impact of a Deadline

Our main question is whether a deadline induces some cost or inefficiency. We present this analysis in the first subsection below. A related question is whether the presence of a deadline has an impact on decision times. We present this analysis in the next subsection. Finall, we study whether the presence of a deadline affects allocations. We present this analysis in the final subsection.

4.5.1 Deadline Paralysis and Inefficiency

For the purpose of examining whether a deadline imposes any cost or inefficiency, we study data from the two treatment conditions only. Subjects faced a deadline in the second phase of these conditions. The decision of a subject in any of these conditions can be said to be inefficient if she failed to complete decisions for all 20 payoff-yielding problems within the deadline given.

We found that 6 subjects in **TH** and 4 subjects in **TL** completed less than 20 problems in phase 2. Proportions tests for either condition revealed that the proportion of subjects completing less than 20 problems was statistically less than 1 (one-tailed p-values < 0.0001). These proportions however did not vary across conditions, indicating that a deadline did not induce differential impact depending on the risk level as far as the number of subjects diplaying ineffiency is concerned.

For the purpose of testing hypotheses, we study the number of problems completed in phase 2. A null hypothesis of efficiency indicates that the number of problems completed on average is 20. We posit inefficiency as an alternative hypothesis, i.e., the average number of problems completed is less than 20.

The mean number of problems completed in phase 2 was 19.16 in **TH** and 19.42 in **TL**. The average number of problems completed by subjects completing less than 20 problems was 13.67 for **TH** and 13.5 for **TL**. The minimum number of problems completed by such subjects was 11 for either condition, while the maximum was 16 for either condition. For either condition t-tests and Snedecor-Cochran sign tests rejected the null hypothesis: one-tailed p-values were 0.009 and 0.0156 respectively for **TH** and 0.0278 and 0.0625 respectively for **TL**. A t-test as well a MW test revealed no difference in the number of problems completed across **TH** and **TL**, suggesting the level of inefficiency induced by a deadline did not depend on the level of risk.

What affects the likelihood of a subject completing all problems within the deadline, or the number of problems actually completed within the deadline? In particular does either the likelihood of completing all problems or the number of problems actually completed increase in the deadline faced? If so, this would reinforce the idea that a deadline imposes a constraint on decision-making, a possibility indicated by the findings reported above.

To address these questions, we conducted two regressions on data pooled across the two conditions, one a probit regression, and the other a linear regression. The dependent variable for the first was a dummy coded as 0 if a subject completed less than 20 problems within her deadline, and 1 otherwise. For the second, the dependent variable was the number of problems completed. Independent variables for either regression were a condition dummy (riskcode, coded as 1 for **TH** and 0 for **TL**), the average investment by the subject across the 20 problems in phase 1, the deadline she faced, as well as gender and income level. Results are presented in Table 4.4 below.

The results show that the likelihood of a subject completing all 20 problems within her deadline as well as the number of problems actually completed by any subject are increasing in the deadline faced (p-values were 0.034 and 0.028 respectively). For the probit regression,

Table 4.4: Inefficiency: Regression results

	probit	linear
riskcode	-0.266	-0.357
	(0.387)	(0.224)
avginv	0.01	0.008
	(0.012)	(0.013)
deadline	0.089**	0.069**
	(0.042)	(0.031)
gender	-0.063	0.145
	(0.403)	(0.473)
income	0.251*	0.204
	(0.149)	(0.147)

** and ** respectively indicate significance in terms of two-tailed p-values at the 10% and 5% levels. Standard errors are in parentheses.*

income was also significant (weakly), with higher income making it more likely that a subject would complete all problems within her deadline. However, income was not significant in the linear regression. No other variable assumed significance in any regression.

We also conducted equivalent regressions on each condition separately, without pooling. No variable other than the deadline showed up as significant in any of these regressions. Further, the deadline was insignificant in the regressions using data from **TL** only, though the coefficient had a positive sign. The deadline was significant (with a positive sign) for both the probit as well as the linear regression using data from **TH** only, though weakly (p-values were repectively 0.092 and 0.077).

Overall, our conclusion is therefore a deadline does act as a constraint on decision-making in the environment considered, and potentially can induce an inefficiency. This tendency does not strongly depend on the level of risk, but may be somewhat more pronounced when risk level is high.

4.5.2 Deadline Acceleration

Does the presence of a deadline affect the time taken to decide? Table 4.5 gives average time taken per problem in seconds for phase 2. The means for all subjects are given in the left two columns. The third column gives the means for subjects in the treatment conditions completing all 20 problems, while the fourth column gives corresponding means for subjects in the treatment conditions completing less than 20 problems.[7]

Table 4.5: Mean Time taken: Phase II

	All subjects		Subjects in treatment conditions	
	C	**T**	Completing 20 problems	Completing less than 20 problems
H	18.68	15.84	14.81	22.55
L	18.49	17.03	16.09	26.67

We already know from Section 4.5.1 that some subjects in the treatment conditions take considerably more time (6 in **TH** and 4 in **TL**) in the presence of a deadline, so much so that they fail to complete all problems and thereby lose some payoff-earning opportunities. These subjects can be said to be suffering from some form of deadline paralysis. The analysis in this section therefore restricts attention only to those subjects in the treatment conditions who completed all problems within the deadline in phase 2.

For the comparison involving subjects in the high-risk conditions, a t-test as well a MW test showed significant difference in phase 2 time taken across treatment and control (p-values: t-test = 0.0683, MW = 0.014). However, no significant difference was recorded by either test for the corresponding comparison involving subjects from the low-risk conditions. These results suggest that the presence of a deadline can potentially accelerate decision-making, with the effect pronounced in high-risk settings.

Multivariate results from regression analysis presented below in Table 4.6 confirm and strengthen the univariate results presented above. We ran three linear regressions, each with average time taken in phase 2 as the dependent variable, and a condition dummy (codecondition, which took value 0 for treatment and 1 for control) as the main independent variable. One was a pooled regression using data from both the high-risk and the low-risk conditions. Other regressors used in this specification were riskcode (0 for low risk and 1 for high risk), average time taken in phase 1 (avgtt), average investment in the safe option in phase 1 (avginv), and gender and income dummies. The other two regressions were disaggregated, one using data from only the high-risk conditions, and the other from only the low-risk conditions. These

[7]For both columns 2 and 4, the mean is computed only for problems actually completed for subjects in the treatment conditions completing less than 20 problems.

specifications used the same additional regressors, except that riskcode was absent.

Table 4.6: Time Taken Regressions: Phase II

	Pooled	High-risk	Low-risk
codecondition	4.202***	4.947***	3.445**
	(1.183)	(1.723)	(1.668)
avgtt	0.296***	0.279***	0.319***
	(0.032)	(0.043)	(0.051)
avginv	0.059	0.068	0.056
	(0.038)	(0.069)	(0.048)
riskcode	-2.006*	-	-
	(1.199)	-	-
gender	-0.291	0.928	-1.376
	(1.248)	(1.825)	1.762
income	0.468	0.359	0.504
	(0.399)	(0.592)	(0.555)

Standard errors are in parentheses.
** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$*

We found that the variable codecondition was positive and significant in all the regressions, indicating that subjects in the treatment conditions completing all problems in phase 2 took decisions faster in phase 2 relative to their counterparts from the corresponding control conditions. An important difference from the univariate analysis was that this acceleration in decision-making was evident for the low-risk condition as well. The acceleration effect appears slightly stronger for the high-risk condition, as can be seen from the negative sign and (weak) significance of the riskcode variable in the pooled regression. The results further showed that time taken in phase 2 was highly correlated with time taken in phase 1.

4.5.3 Risk Preference

Does the presence of a deadline impact the allocation decision? Table 4.7 below gives the mean amounts allocated to the safe option out of 100 in phase 2. The means for all subjects are given in the left two columns. The third colmn gives the means for subjects in the treatment conditions completing all 20 problems, while the fourth column gives corresponding means for subjects in the treatment conditions completing less than 20 problems.[8]

[8]For both columns 2 and 4, the mean is computed only for problems actually completed for subjects in the treatment conditions completing less than 20 problems.

Table 4.7: Mean Allocations: Phase II

	All subjects		Subjects in treatment conditions	
	C	**T**	Completing 20 problems	Completing less than 20 problems
H	51.88	47.2	46.58	51.23
L	45.51	41.79	42.02	39.41

A comparison of Tables 4.1 and 4.7 shows that mean allocations (for all subjects taken together) fall in phase 2 relative to phase 1 in all conditions, with the fall sharper in the treatment conditions. Since mean allocations for phase 1 were statistically indistinguishable across treatment and control, given risk level (see Tables 4.2 and 4.3), an appropriate univariate comparison involves testing whether phase 2 allocations in the treatment conditions differ from phase 2 allocations in the corresponding control conditions. A significant difference would indicate that a deadline leads to more risk-taking behavior.

Classical t-tests as well MW tests showed insignificant differences in phase 2 allocations across treatment and control for both the high-risk and the low-risk conditions when the comparison involved all subjects from the treatment conditions. The same results obtained when attention was restricted only to those subjects completing all 20 problems in the treatment conditions. For the high-risk conditions, the results were similarly unchanged when attention was instead restricted to only those subjects from the treatment conditions completing less than 20 problems. For the equivalent tests for the low-risk conditions however, while the MW delivered the same results, the t-test showed that the difference was significant at the 10% level (p-value = 0.0704). This appears to be an exceptional case however, since there were only 4 subjects in **TL** who completed less than 20 problems, and hence we conclude from the univariate comparisons that a deadline did not significantly impact the allocation decision.

We now move to a multivariate comparison. We ran three pooled linear regressions, each with average investment in the safe option in phase 2 as the dependent variable, and a condition dummy (codecondition, which took value 0 for treatment and 1 for control) as the main independent variable. Other regressors were riskcode (0 for low risk and 1 for high risk), average investment in the safe option in phase 1 (avginv), average time taken in phase 1 (avgtt), and gender and income dummies. The three regressions differed in the set of subjects considered from the treatment conditions: the first included all subjects, the second only those subjects completing all 20 problems, and the third only those subjects not completing all 20 problems. The results are given Table 4.8.

Table 4.8: Allocation Regressions: Phase II

	All subjects	Subjects completeing 20 problems	Subjects not completeing 20 problems
codecondition	3.814	4.217*	1.394
	(2.311)	(2.394)	(5.085)
avginv	0.304***	0.359***	0.452***
	(0.074)	(0.078)	(0.105)
avgtt	0.013	0.025	-0.116
	(0.063)	(0.065)	(0.108)
riskcode	3.574	2.575	3.124
	(2.336)	(2.426)	(2.999)
gender	7.412***	8.241***	6.409**
	(2.435)	(2.526)	3.143
income	0.452	0.635	1.232
	(0.779)	(0.807)	(1.045)

, ** and * respectively indicate significance in terms of two-tailed p-values at the 10%, 5% and 1% levels. Standard errors are in parentheses.*

Results from the multiavariate analysis did not fully confirm the univariate findings reported above. The variable codecondition assumed insignificance when all subjects from the treatment conditions were considered. However, in a divergence from univariate results, the variable was weakly significant and with a positive sign for the second regression, where only those subjects from the treatment condition were considered who had completed all 20 problems, indicating that the deadline produced more risk-seeking behavior. The results further showed that allocation in phase 2 was highly correlated with allocation in phase 1, and that females tended to invest more in the safe option, with the latter finding confirming resuls from phase 1 (see Table 4.3).[9]

[9]These results remained more or less unchanged when data were disaggregated by risk level. In particular, the variable codecondition became insignificant when high and low risk conditions were considered separately in all cases.

4.6 Conclusion

The main question addressed in this chapter is whether a deadline can act as a constraint on decision-making, in the context of choice under risk. We found that imposition of a deadline caused some subjects (about 10%) to display deadline paralysis; these subjects failed to complete all potential problems in the presence of a deadline and hence forewent some payoff-earning opportunities. The impact emerging from such paralyzed subjects was large enough to manifest itself statistically, with the result that the average number of problems completed across all subjects was less than the maximum possible. The presence of this inefficiency suggests therefore that a deadline can act as a constraint on decision-making, and is costly.

For the remaining subjects a deadline caused an acceleration in decision-making. This points to a potential benefit of deadlines, as such subjects could in principle have used the time saved due to accelerated decision-making to pursue alternate productive opportunities. Our design however is unable to capture or provide any measure of such potential benefits, as subjects completing the task within the deadline were not given the opportunity to use the time saved to approach alternative tasks. The incorporation of the possibilities of both costs as well as benefits of deadlines in a single framework is left for future work.

Subjects who completed the task within the deadline tended to be more risk-seeking in their investment choices. This was found to be statistically significant in the multivariate analysis, but not in the univariate. A possible explanation for this finding is that information processing may be impaired by time-pressure, with decision-makers systematically failing to correctly assess underlying risk, or apply appropriate prudence.

As discussed earlier, a feature of our design which separates us from all prior work on deadlines is the endogeneity of the deadline set, with a subject facing a deadline based on an assessment of the 'normal' time taken by her to complete the task. We approached this element through our two-phase design, with the time taken by a subject in the first phase being used as her deadline in the second. A problem with this approach is that subjects tended to be much quicker in task completion in the second phase, even without a deadline, possibly because of familiarity effects. This suggests that first phase times may not be the most appropriate measure of normal times. An alternative approach would be to set an imputed deadline, by first estimating the relationship between first- and second-phase times taken for control subjects, and then applying first-phase data for treatment subjects to the estimated equation in order to generate an imputed deadline to be used for treatment subjects in the second phase. Such a correction is left for future work.

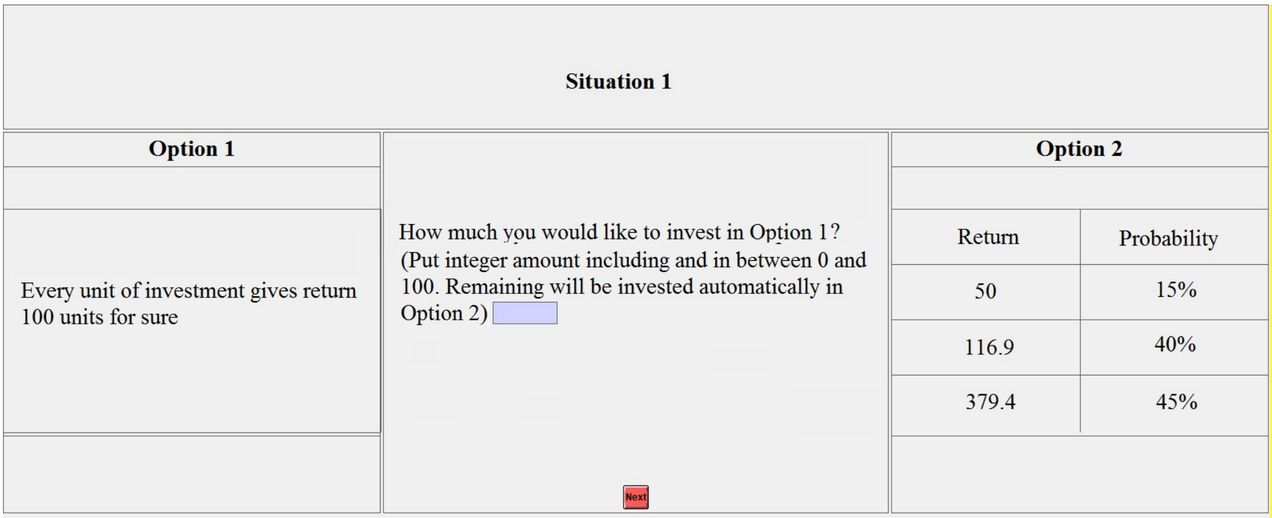


Figure 4.1: Phase 1 Problem 1 (high-risk control condition)

Appendix

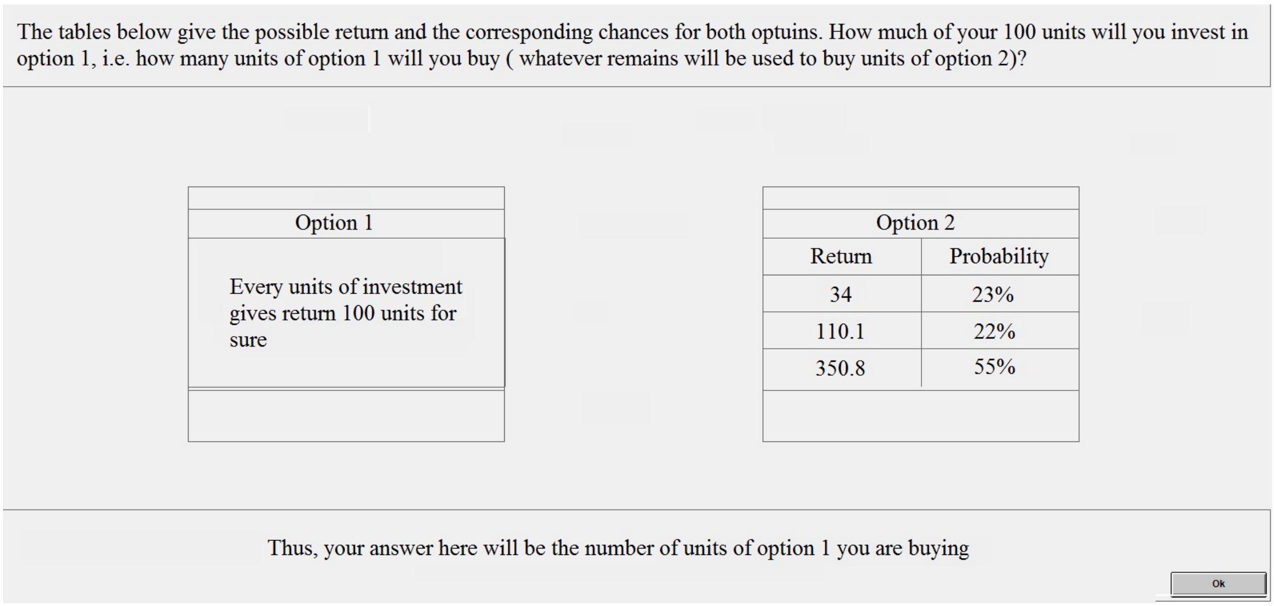


Figure 4.2: Example Screen 1

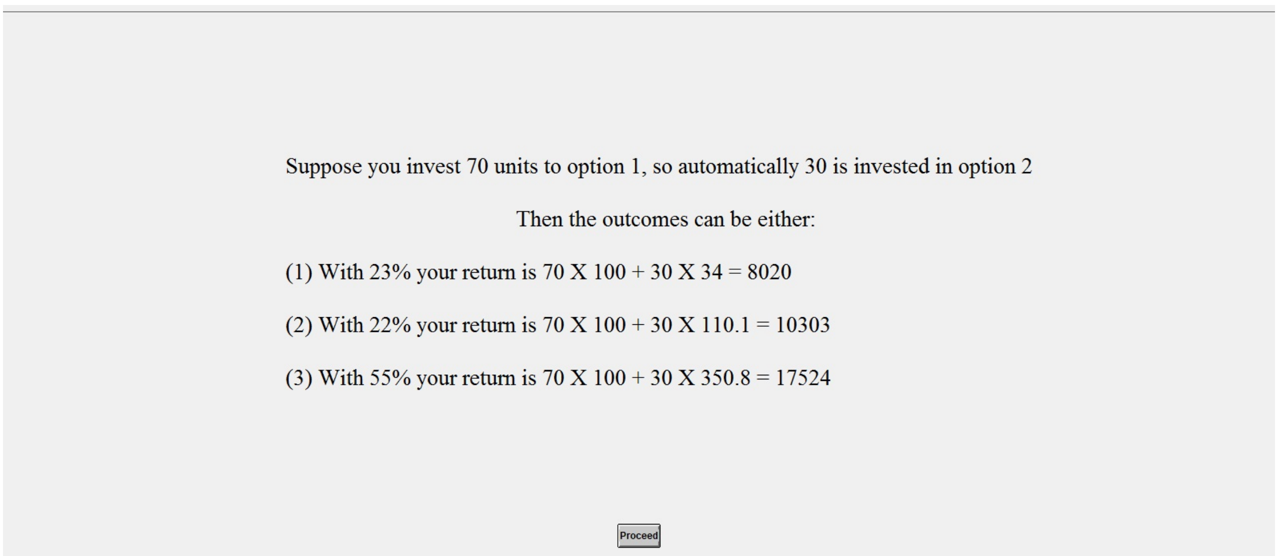


Figure 4.3: Example Screen 2

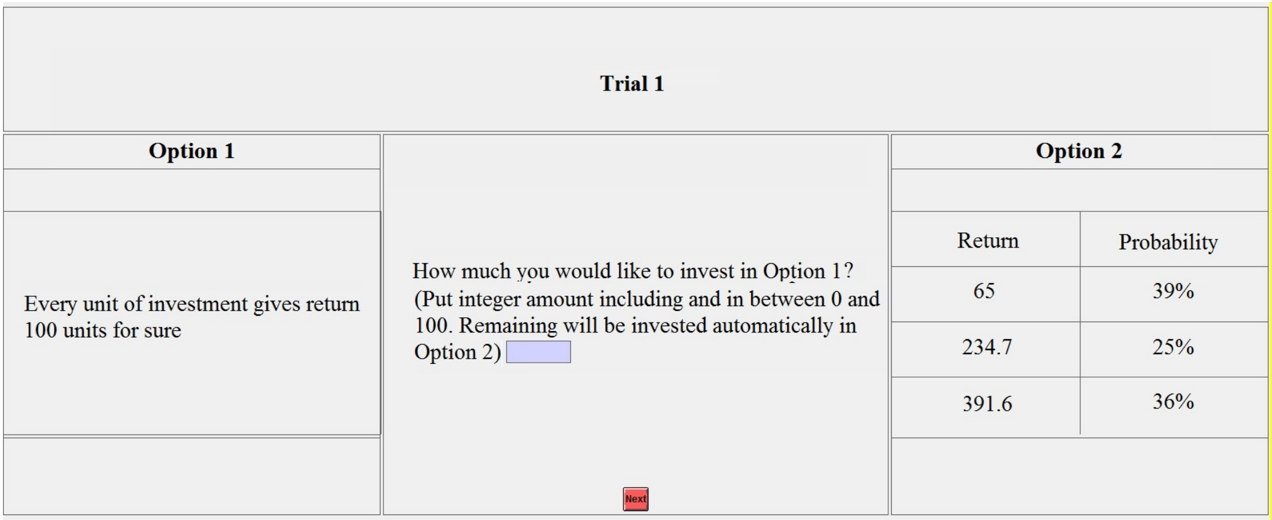


Figure 4.4: Phase 1 Trial Problem 1

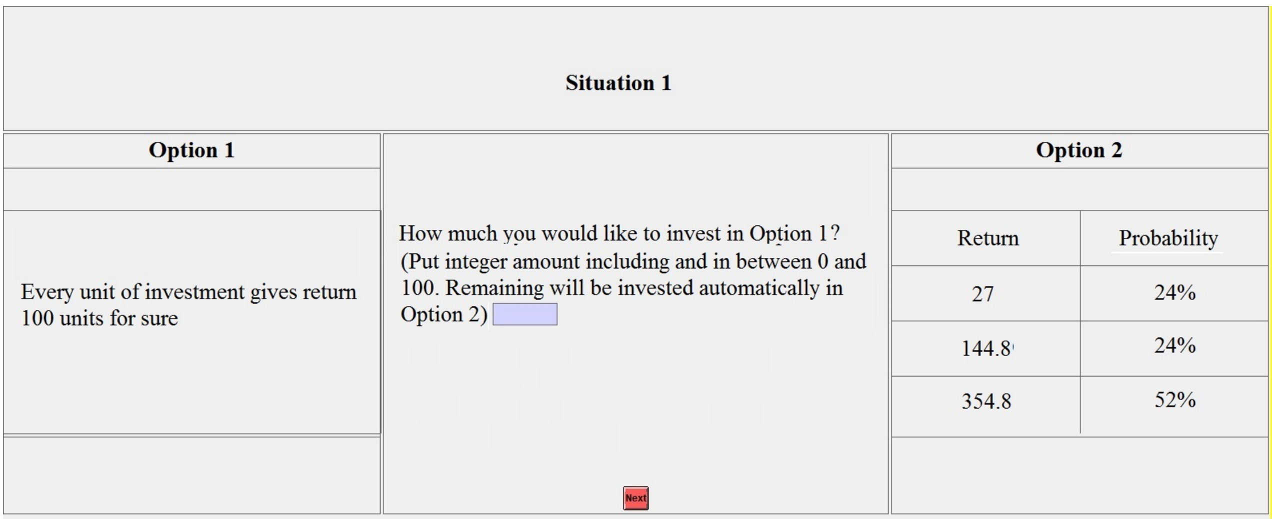


Figure 4.5: Phase 2 Problem 1 (high-risk control condition)

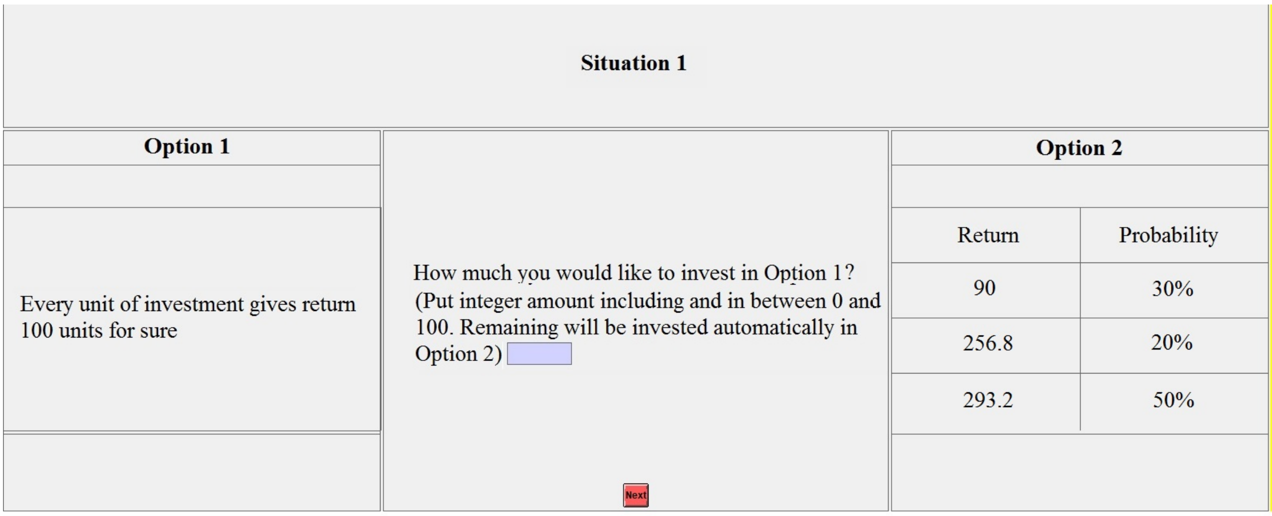


Figure 4.6: Phase 1 Problem 1 (low-risk control condition)

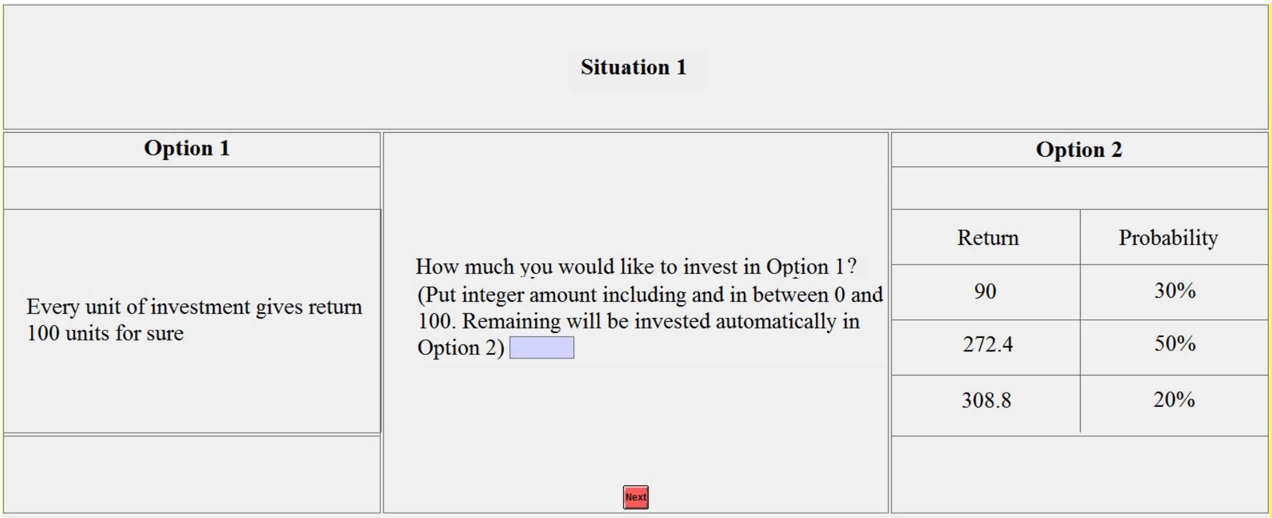


Figure 4.7: Phase 2 Problem 1 (low-risk control condition)

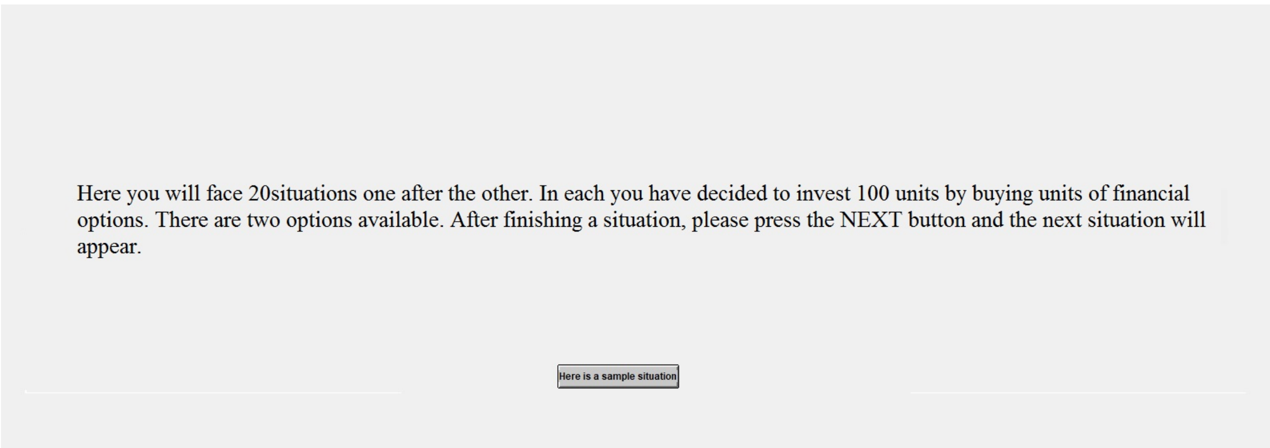


Figure 4.8: Phase 1 Welcome Screen

Figure 4.9: Phase 2 Welcome Screen (control)

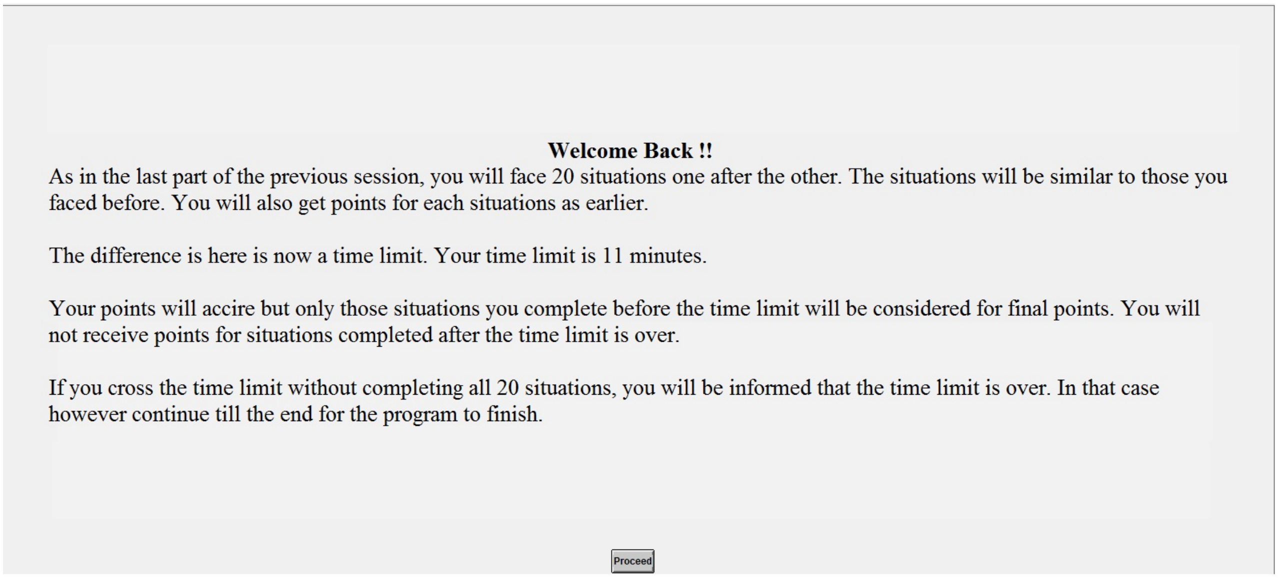


Figure 4.10: Phase 2 Welcome Screen (treatment)

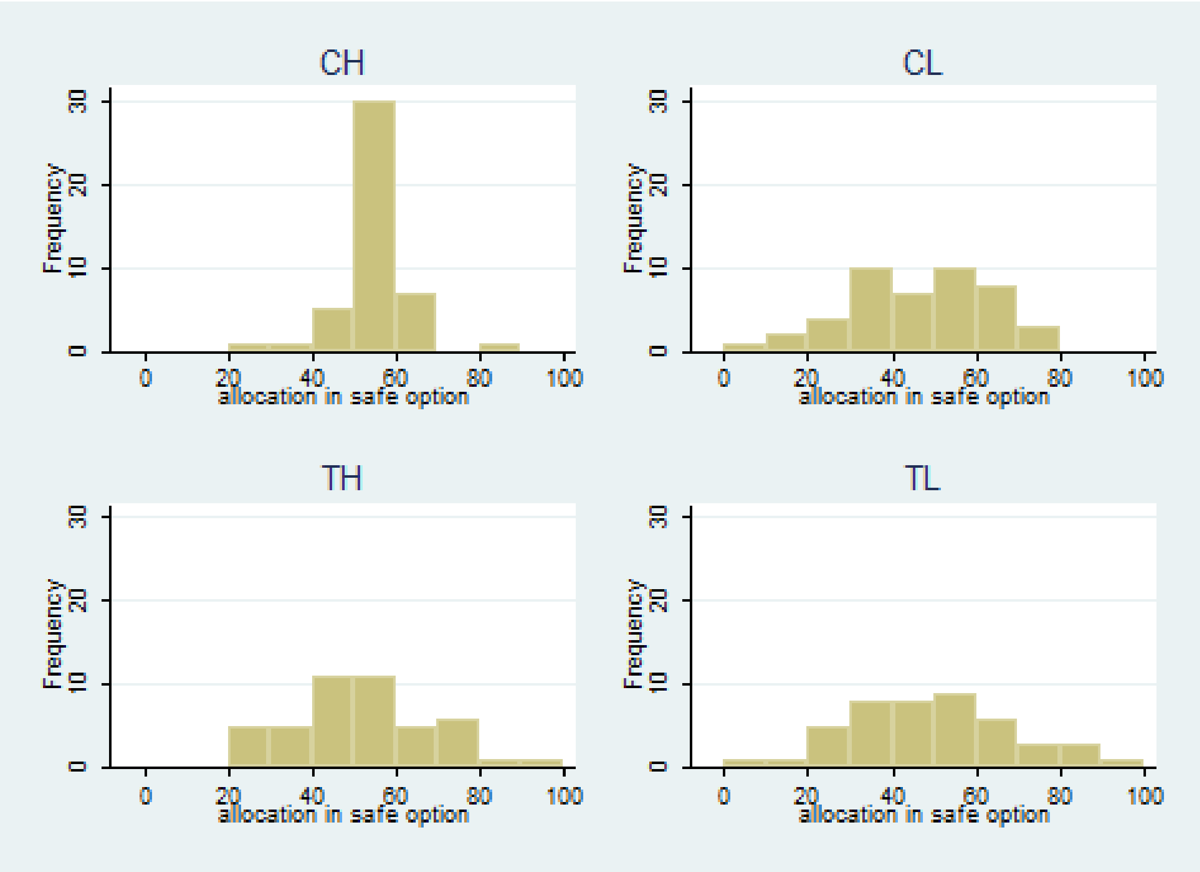


Figure 4.11: Average allocations in safe option: Phase 1

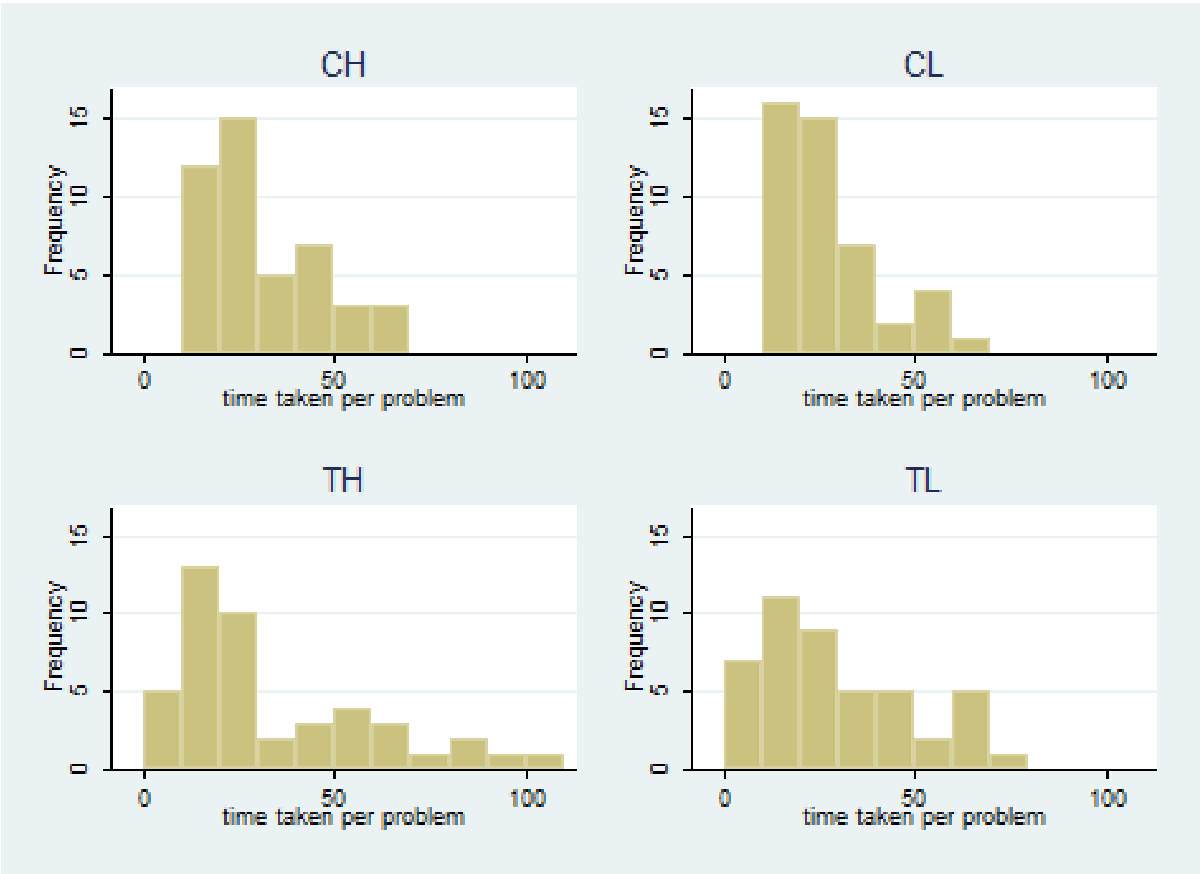


Figure 4.12: Average time taken to complete 20 problems: Phase 1

Chapter 5

Rational Imitation under Cognitive Pressure

5.1 Introduction

Observational learning is a form of social learning that occurs through observing the behavior or choices of others (see e.g. Bandura [11] and Shettleworth [88]). An important type of observational learning is imitation, which is presumed to have occurred when an observer copies (mimics) or follows or is influenced by an action or that he has observed, with the matching behavior not explicable by an alternative mechanism (see e.g. Heyes [50] and Hughes [52]).

Interest in social observational learning, herding or imitation has rapidly grown in economics in recent decades. These or similar forces have been proposed as factors driving or potential causes for a variety of economic phenomena such as job search in labor markets, adoption of new products and technologies, failure of optimal technological shifts, financial crises etc (see e.g. Mobius and Rosenblat [71]).

The main line of experimental economics research on these topics has followed a Bayesian framework with incomplete information being the driver of learning or imitation (see e.g. Anderson and Holt [3] and Hung and Plott [53]). Another line has followed the insight that cognitive pressure or environmental complexity may generate imitation (see e.g. Baddeley [10] and Borghans, et. al. [15]). In this view, the complexity of the decision environment can trigger imitative behavior as a heuristic response to save cognitive effort and decision cost (see e.g. Tversky and Kahneman [96] and Gigerenzer and Selten [38]). Our chapter is situated within this line.

Specifically, we ask first if cognitive pressure can be a causal basis for imitative behavior, i.e., is the presence of cognitive pressure associated with imitative behavior and the absence associated with the lack of such tendencies? Our second question is whether any identified imitation constitutes pure mimicry, where imitation is an end in itself, or whether it can be described as purposive or rational (see e.g. Dewey [28] and Matthey [68]).

We pursued these questions through a field experiment using ordinary citizens as volunteer subjects. Subjects faced two decision problems sequentially, in each of which a budget had to be split across two options. The return functions were deterministic, and was linear for one of the options, and non-linear for the other (the complex option). All subjects decided independently for the first problem faced. For the second problem, half the subjects saw the response of one other subject before deciding.

We varied the degree of non-linearity of the return function for the complex option in an attempt to manipulate the degree of cognitive pressure, i.e., the level of cognitive pressure imposed by the structure of problem was used as a treatment variable. We found that our manipulation was effective, with average independent subject performance higher when the return function was 'less non-linear' (in a sense to be made precise below). We then tested for the presence of imitative behavior by analyzing the extent to which decisions for the second problem by subjects observing another's prior response to the same problem tended to be influenced positively by the observed prior choice. We found no imitation when the problem was cognitively relatively simple, and heavy imitation when the problem was more complex and hence likely to generate substantial cognitive pressure. Imitation in the complex environment was extreme, with the decision-maker being more influenced by the observed prior response than her own response to the first problem, which was structurally identical to the second (in a sense to be made precise below). We also found that cognitive pressure in an of itself did not affect individual learning behavior, with the dependence of the response to the second problem on the response to the first for independently deciding subjects invariant to the degree of cognitive pressure. Finally, for the more complex environment where imitation was detected, we found that such imitation could be characterized as rational. This conclusion was facilitated as our results showed that subjects tended to be more influenced or imitative when the observed response of another subject was qualitatively superior, and that more imitative subjects tended to obtain higher payoff.

We depart from prior research (see e.g. Huck, et. al. [51], Bosch-Domènech, et. al. [16], Offerman, et. al. [77], Selten and Apesteguía [86], Kirchkamp and Nagel [58], Apesteguía, et. al. [6], Duersch, et. al. [33] and Friedman, et.al. [36]) on this topic in two main ways, apart from ours being a field experiment. First, all earlier papers investigating imitation under full information have used some model of strategic competition, typically the Cournot, to identify the presence of imitation and its effects. A problem with the strategic environment is that it is difficult to identify whether any observed imitation is arising due to a failure to compute best-responses given conjectured behavior, or due to a failure to form conjectures. By contrast, we use an individual decision environment where any purposive imitation can only arise due to a failure to compute the payoff function. Second, prior papers have not identified cognitive pressure as a causal basis for imitation, as they do not vary cognitive pressure through variation in the structure of problems faced, and concomitantly do not demonstrate the non-existence of imitation in the absence of cognitive pressure. Our approach by contrast establishes a causal role for cognitive pressure.

The rest of this chapter is organized as follows. The next section lays out our design and procedure, while Section 5.3 presents our results. Section 5.4 concludes.

5.2 Design and procedure

The experiment was hand run. Potential subjects at the time of recruitment were told they would be participating in a study on financial decision making which would take a few minutes and may involve some calculations, and that they would have to make choices for two problems. Subjects were paired randomly following recruitment, and one of them was randomly allocated the role of member 1, the other taking the role of member 2. Subjects then faced two allocation problems sequentially, both members in any pair facing the same problems in the same sequence.

Members of any pair were not told they would face the same problems. However, on commencement, both members were given their instruction and decision sheets for problem 1 simultaneously (see Figure 5.1 for a sample instruction and decision sheet - all decision and instruction sheets were identical in structure, except that the specific problem could vary). These sheets were also collected simultaneously, after both members had announced completion of decision. The members were then given their instruction and decision sheets for problem 2, again simultaneously. They were told at this time to announce when they were ready to decide, and not to write on the sheet until then. When both members had announced they were ready, member 1 was asked to record her decision. This sheet was then collected and given to member 2, who was then asked to return the sheets after recording her decision on her own sheet.

Member 2 thus made her single choice for problem 2 while being aware of member 1's choice for the same problem.[1] Member 1 however made her choice for problem 2 independently. Both members made their choices for problem 1 independently. In all cases, we chose subjects for a given pair such that they were seated within visual range of each other, but were obviously unacquainted. Each subject in any pair could thus observe the other making decisions. But sufficient distance was maintained between them such that no subject could observe the actual choices of or communicate with the other. We additionally took care to ensure that subjects in any pair were never aware of who else was participating in the study.

All problems involved allocating a budget of 20 across two options, each with deterministic returns. One option was linear and gave a fixed return of 5 units per unit invested. The other option in any problem had a non-linear and monotonically increasing return function. Subjects were provided a figure as well as a table showing the return function for the non-linear option.

There were two types of non-linear options for either problem 1 or problem 2. One of these involved *non-cognitive* options, with piecewise linear return functions. The other involved *cognitive* options, with return functions which had smoother curvature and were not piecewise

[1] An alternative design could have been to allow member 2 to record her preliminary decision for problem 2 independently, and then allow her to change her decision after providing her with the decision sheet for member 1. We used the design as described to be consonant with the normal interpretation of social learning, where the potential learner is aware of the decision of the person potentially being learned from before making any explicit choice.

linear. Our a priori conjecture was that the problems with cognitive options, as defined above, would generate more cognitive pressure than those with non-cognitive options.

Within any pair, both members faced options of a single type only for either problem, i.e., a subject pair faced either non-cognitive options or cognitive options for both problems. We denote problems with non-cognitive non-linear options as problem N1 for the first problem faced and problem N2 for the second problem faced (see Figure 5.2), and problems with cognitive non-linear options correspondingly as problem C1 and problem C2 (see Figure 5.3).

All problems, whether first or second, were constructed to induce an aggregate payoff function with a unique maximum. Moreover, all problems had the maximizer at 7 units of investment in the linear option and a maximand of 328 (see Figures 5.4 and 5.5).[2] Subjects were not provided any figure or table for the aggregate payoff function for any problem.

Subjects were passengers on trains. Half the subjects were passengers on luxury trains leaving from or going toward Kolkata, West Bengal, India. These subjects were of relatively high income and wealth status. The other half were passengers on local or suburban trains in the Kolkata area. These subjects were of relatively lower income and wealth status. The category of trains data were collected from thus acts as a proxy for average income and wealth of the subjects. For either income group, two-thirds of the pairs received problems C1 and C2, while one-third of the pairs received problems N1 and N2.

We thus had two major conditions, depending on whether subjects faced problems with non-cognitive (condition N) or cognitive (condition C) non-linear options. Within each of conditions N and C, there were two sub-conditions, depending on whether subjects were passengers on long distance luxury trains or local trains (i.e., were respectively of higher or lower income and wealth status). Within any condition, there were equal numbers of subject pairs for either sub-condition. The total number of subjects in condition N was 200 (50 subject pairs per sub-condition), and the total number of subjects in condition C was 400 (100 subject pairs per sub-condition).

Pairs took about 10 minutes to complete the study. Subjects were told at the time of recruitment that one of their decisions would be selected randomly, and payment would be given for

[2] We chose to make both problems faced by any pair thus 'structurally similar' to allow maximum scope for individual learning and thereby reduce the likelihood of imitation.

that decision at the rate of 1 rupee for every 5 points obtained. The average subject earned 60 rupees[3]. Payment was given in an envelope to a subject about 1 minute after the pair completed all decisions.

5.3 Results

Our study focuses on two questions. One, whether cognitive constraints can be a causal basis for imitation. Two, if so, whether such imitation can be identified as rational or purposive in any sense. A key issue is therefore the identification of the propensity to learn socially, which centres around a comparison of the responses for problem 2 across the members. Before addressing this issue, we first establish that the population constituting the members 1 is the same as that constituting the members 2. This allows us a basis for the main comparison, designed to study the issue of imitation.

To establish population equivalence, we compare performance in problem 1 across the members within each condition, using regression approaches. This strategy has its justification in that the two members in any pair faced the same problem 1, and chose their responses to it independently without knowing conditions under which responses would be generated for problem 2. We use three measures of performance: (a) points earned, (b) the distance of the actual choice in the linear option from the payoff-maximizing level, and (c) whether the payoff-maximizing response was chosen or not (recall, aggregate payoff was maximized at 7 units of investment in the linear option). We carry out these comparisons within each condition because of the expectation on a priori grounds that average performance should differ across N and C. An additional reason for comparing within condition only, related to the first measure of performance we use, is that aggregate payoff structures differ across N and C away from the maximizer (see Figures 5.4 and 5.5). The Table 5.1 and Table 5.2 show the summary statistics of points earned or the payoff obtained by the subjects, conditions and stage-wise.

We ran three regressions for each condition, each with a different dependent variable, and a common set of independent variables. The first two were linear specifications, while

[3]The purchasing power parity exchange rate between the Indian Rupee and the US Dollar for 2010 was 16.84 rupees to a dollar according to the Penn World Tables (see Heston, Summers and Aten [49]).

Table 5.1: Summary Statistics for pay-off : Condition *C*

	Stage 1							
	Member 1				**Member 2**			
	Max.	Avg.	Min.	S.D.	Max.	Avg.	Min.	S.D.
High income	328	296.98	100	54.61	328	296.15	100	51.21
Low income	328	283.49	100	61.49	328	290.04	100	41.72
	Stage 2							
	Member 1				**Member 2**			
	Max.	Avg.	Min.	S.D.	Max.	Avg.	Min.	S.D.
High income	328	312.93	240	23.13	328	314.32	245	21.05
Low income	328	305.44	240	26.92	328	307.02	240	24.97

Table 5.2: Summary Statistics for pay-off : Condition *N*

	Stage 1							
	Member 1				**Member 2**			
	Max.	Avg.	Min.	S.D.	Max.	Avg.	Min.	S.D.
High income	328	304.86	260	26.4	328	298.04	145	36.54
Low income	328	305.22	260	22.21	328	306.14	260	22.54
	Stage 2							
	Member 1				**Member 2**			
	Max.	Avg.	Min.	S.D.	Max.	Avg.	Min.	S.D.
High income	328	281.42	130	68.77	328	276.6	130	65.32
Low income	328	266.02	130	64.52	328	286.84	130	53.18

the third was a probit specification. The dependent variables in the first (1), second (2) and third (3) were respectively points earned (this corresponds to performance measure (a) above), distance of actual choice from the maximizer (this corresponds to performance measure (b) above), and a dummy variable which took the value 1 if the response was payoff-maximizing, and 0 otherwise (this corresponds to performance measure (c) above). The main independent variable was a dummy (*member*) taking value 1 for member 1 and 2 for member 2. Our hypothesis is that the coefficient on this variable is insignificant. The other independent variable was a dummy, (*income*), indicating whether the pair were from the lower (income = 0) or higher (income = 1) income group. Results are given in Table 5.3.

•variable description:

- P_i: Points earned.
- dum_M_i: Member dummy.
- dum_I_i: Income dummy.
- D_i: Distance of actual choice from the maximizer.
- $(dum\_Inv)_i^{P_{max}}$: A dummy variable which took the value 1 if the response was payoff-maximizing, and 0 otherwise.

The table shows that the coefficient estimate for the variable member was insignificant in all the regressions. Hence the null hypotheses of population equivalence cannot be rejected, leading us to conclude for either condition that for any pair, both members are drawn from the same population. The variable income was significant in two of the regressions, one each in N and C, but the pattern was not consistent.

5.3.1 Cognitive pressure across the conditions

The distinction maintained so far between the conditions is predicated on our a priori assessment, that the problems faced in N are easier than those faced in C, being correct. If this is inappropriate, then no difference in the degree of cognitive pressure can be expected across the

Table 5.3: Population equivalence

	control (N)		
	(N1) (P_i)	(N2) (D_i)	(N3) ($(dum\_Inv)_i^{P_{max}}$)
member (dum_M_i)	-2.95 (3.895)	0.04 (0.641)	-0.032 (0.198)
income (dum_I_i)	-4.23 (3.895)	-0.58 (0.641)	0.499* (0.199)
constant	310.11*** (6.459)	6*** (1.063)	-0.946** (0.223)
R^2	-0.001 (adjusted)	-0.006 (adjusted)	0.029 (pseudo)
No. of obs.	200		
	treatment (C)		
	(C1) (P_i)	(C2) (D_i)	(C3) ($(dum\_Inv)_i^{P_{max}}$)
member (dum_I_i)	2.86 (5.271)	0.055 (0.344)	-0.182 (0.163)
income (dum_M_i)	9.8 (5.271)	-1.105*** (0.344)	0.23 (0.164)
constant	282.475*** (9.724)	4.618*** (0.571)	-1.017*** (0.264)
R^2	0.004 (adjusted)	0.02 (adjusted)	0.011 (pseudo)
No. of obs.	400		

, ** and * respectively indicate significance in terms of two-tailed p-values at the 5%, 1% and 0.1% levels. Standard errors are in parentheses.*

For the relevant regression equations see section 5.5.1 conditions. This would in turn imply that even if differences were identified in the propensity to learn socially across the conditions, such differences would not be attributable to differences in the degree of cognitive pressure. This would hence lead to a failure to draw any causal link between cognitive pressure and imitation.

In this section we therefore attempt to assess whether subjects found the problems in *N* to be more tractable than those in *C*. To do this we used regression analysis to a) compare subject performance for problem 1 across the conditions, and b) compare performance of members 1 for problem 2 across the conditions. The comparison for problem 2 was done for members 1 only as members 2 did not take independent decisions for that problem.

We used the proportion of subjects achieving the payoff-maximizing response as the performance measure. For problem 1, across all members, this proportion was 23.5% for condition *N*, and 12.3% for condition *C*. The corresponding proportions for problem 2, members 1, were 23% and 13.5% respectively. Probit regressions were conducted for the comparisons. The dependent variable was always a dummy which took the value 1 if the response was payoff-maximizing, and 0 otherwise.[4]

The main independent variable in all the regressions performed was a dummy (*expcode*) taking value 0 for *N* and 1 for *C*. Our hypothesis is that the variable expcode will have a negative and significant coefficient in all cases, indicating that a move from *N* to *C* will lead to a lower performance, in terms of the proportion of subjects achieving the payoff-maximizing response. Income, defined earlier, was another independent variable common to all the regressions.

The other independent variable for the first comparison (1), relating to problem 1, is member, defined earlier. A secondary hypothesis for this comparison, following from the analysis earlier, is that this variable will have an insignificant coefficient estimate.

For the second set of comparisons, for problem 2 and members 1, we report three regressions, (2), (3) and (4). All specifications included a control for subjects' performance in problem 1 (neither specification included the variable member as this comparison restricted attention to member 1 only). The three regressions used different controls. The control variable

[4]We do not use points earned or distance from the maximum as performance measures. This is because, as discussed earlier, aggregate payoff structures differ across *N* and *C*, for both problems, away from the maximizer, rendering the use of these measures as dependent variables inappropriate.

for (2) was the points earned for problem 1. A secondary hypothesis for this specification is that the control variable should have a positive and significant coefficient estimate, indicating that more points earned for the first problem should increase the likelihood of achieving the payoff maximizing response for the second problem. The control variable for (3) was a dummy which took value 1 if the subject achieved the payoff-maximizing choice for problem 1, and 0 otherwise. A secondary hypothesis for this specification is that the control variable should have a positive and significant coefficient estimate, indicating that achieving the payoff maximizing response in the first problem should increase the likelihood of doing so for the second problem. The control variable for (4) was the distance of actual choice for problem 1 from the payoff-maximizing level. A secondary hypothesis for this specification is that the control variable should have a negative and significant coefficient estimate, indicating that a smaller distance from the maximum for the first problem should increase the likelihood of achieving the payoff maximizing response for the second problem.

•variable description:

- $(dum\_Inv)_i^{P_{max}}$: a dummy variable which took the value 1 if the response was payoff-maximizing, and 0 otherwise.
- dum_exp_i : condition dummy. 0 for N and 1 for C.
- P_i^{P1}: Points earned for problem 1(control variable for the equation (2)).
- $(dum\_Inv)_i^{P_{max}^{P1}}$: Dummy variable which took value 1 if the subject achieved the payoff-maximizing choice for problem 1 (control variable for the equation (3)).
- $(D\_Inv)_i^{P_{max}^{P1}}$: The distance of actual choice for problem 1 from the payoff-maximizing level(control variable for the equation (4)).
- dum_M_i: Member dummy.
- dum_I_i: Income dummy.

Regression results are presented in Table 5.4. They show that the variable expcode is negative in all regressions. It is significant for the first comparison (1), as hypothesized. It is also

Table 5.4: Differential cognitive pressure across N and C

	problem 1	problem 2, members 1		
	(1)	(2)	(3)	(4)
	$(dum\_Inv)_i^{P_{max}}$	$(dum\_Inv)_i^{P_{max}}$	$(dum\_Inv)_i^{P_{max}}$	$(dum\_Inv)_i^{P_{max}}$
expcode	-0.443***	-0.329	-0.221	-0.754***
(dum_exp_i)	(0.128)	(0.187)	(0.223)	(0.235)
member	-0.124	-	-	-
(dum_M_i)	(0.126)	-	-	-
control	-	0.014***	2.061***	-0.362***
	-	(0.004)	(0.233)	(0.049)
income	0.337**	0.366*	0.553*	0.289
(dum_I_i)	(0.126)	(0.186)	(0.219)	(0.219)
constant	-0.719***	-5.116***	-1.701***	0.375
	(0.221)	(1.097)	(0.242)	(0.26)
pseudo R^2	0.038	0.115	0.365	0.372
No. of obs.	600	300		

, ** and * respectively indicate significance in terms of two-tailed p-values at the 5%, 1% and 0.1% levels. Standard errors are in parentheses. For the relevant regression equations see section 5.5.1*

significant for specification (4) relating to the second comparison, but not for specifications (2) or (3).[5] Overall, the results tend to support the validity of our a priori differentiation of the conditions. Results also show that the variable member is insignificant in (1) (for problem 1), validating our secondary hypothesis, and confirming findings from above. Finally, the secondary hypotheses for the variable control are upheld in all regressions, (2), (3) as well as (4).

5.3.2 Differential imitation

The analysis uptil now has established two things. First, that members 1 and 2 of any pair are drawn from the same population. We established this through examination of their responses to problem 1. This suggests that in the absence of any possibility for social learning, we would expect them to choose similarly for problem 2 as well. Second, that the problems faced in N are easier and hence less likely to pose a cognitive challenge than those faced in C. These allow us to frame the broad hypotheses that (a) for members 2, imitation is less likely in N than in C,

[5]The variable was significant at the 10% level for (2): p-value = 0.078.

and (b) for members 1, dependence of the choice with respect to problem 2 on own choice with respect to problem 1 should be similar across N and C.

We investigate these broad hypotheses through regression analyses of the two members' responses to problem 2. Below, we outline our analytical strategy and the specific hypotheses to be explored. Let the investment in the linear option by member i for problem j be denoted as r_j^i. Consider two linear regressions, one each for N and C (which we respectively call regressions $1N$ and $1C$) with r_2^2 (member 2's response to problem 2) as the dependent variable, and r_1^2 (her own response to problem 1) and r_2^1 (member 1's response to problem 2), and also income, as independent variables.

By design, member 2 knows her own response to problem 1, as well as her paired member 1's response to problem 2, at the time of choosing allocation in problem 2. It is natural to expect that member 2's response to problem 2 should be correlated with her own response to problem 1, i.e., the coefficient of r_1^2 in the regression models specified above should be positive and significant. This is because a member's cognitive ability, as reflected in her responses, should display some stability across problems. If no imitation is taking place, then the coefficient of r_2^1 in the regressions above should be insignificant. A positive and significant coefficient for r_2^1 is expected however in the presence of imitation.

Our first step in the investigation of the presence of imitation therefore is the performance of regressions $1N$ and $1C$ as specified above to observe whether the coefficients for r_2^1 are positive and significant. Our hypothesis in this respect, which we denote **hypothesis 1**, is that r_2^1 is positive and significant for $1C$, but insignificant for $1N$.

If imitation is detected, then a question is which component is more important in driving member 2's response to problem 2. That is, does member 1's reponse to problem 2 have a role greater or lesser than member 2's response to problem 1 in determining member 2's response to problem 2? Our first approach to this question is to compare the coefficients of r_2^1 and r_1^2, within each of the above regressions. We do this by performing regressions $1N$ and $1C$, and then testing post estimation whether the coefficient for r_2^1 is greater than that for r_1^2. Our hypothesis in this respect, which we denote **hypothesis 2**, is that the coefficient for r_2^1 is greater than that for r_1^2 in $1C$, with the ordering reversed in $1N$.

Our second step is to perform two more linear regressions, one each for N and C (which we respectively call regressions $2N$ and $2C$), identical to regressions $1N$ and $1C$ respectively except that r_2^1 does not appear as an independent variable. Regressions $2N$ and $2C$ therefore analyze member 2's response to problem 2 through the implicit assumption that it is independent of member 1's response to problem 1. Our second approach to the question of relative importance is to compare the performance of the models specified through regressions 1 and 2, within each condition. We do this by conducting regressions $1N$, $2N$, $1C$ and $2C$, and then using likelihood ratio tests post estimation to determine whether $1N$ (resp $1C$) is a better model than $2N$ (resp $2C$). Our hypothesis in this respect, which we denote **hypothesis 3**, is that regression models 1 and 2 will be comparable in performance for N, but $1C$ will constitute a better model than $2C$.

Our third step is to perform two more regressions, one each for N and C (which we respectively call regressions $3N$ and $3C$), with r_2^1 (member 1's response to problem 2) as the dependent variable, and r_1^1 (her own response to problem 1), along with income, as independent variables. These regressions, together with the earlier ones, allow us to develop two more approaches to the question of relative importance delineated above.

The first of these involves a comparison of regressions 1 and 3, within each condition. As articulated above, we expect to detect no imitation in N. This suggests that the response patterns for members 1 and 2 to be the same for N. For C however, imitation is expected. This indicates that when choosing response for problem 2, dependence on her own choice for problem 1 should be less for member 2 than for member 1. Our hypothesis in this respect, which we denote **hypothesis 4**, is therefore that the coefficient for r_1^2 in $1N$ should be the same as the coefficient for r_1^1 in $3N$, while the coefficient for r_1^2 in $1C$ should be less than the coefficient for r_1^1 in $3C$. We test this hypothesis by first performing the four regressions and then using seemingly unrelated post estimation procedures.

Our final approach surrounds the behavior of members 1. These members take their decisions for problem 2 without the opportunity to learn. If the possibility of imitation is what drives the difference in members 2's response patterns across conditions, this suggests that the level of cognitive pressure should not affect members 1's individual learning behavior with regard to the decision for problem 2. Hence our hypothesis in this respect, which we denote **hypothesis 5**, is that the coefficients for r_1^1 should be the same across regressions $3N$ and $3C$. We test these hypotheses by first performing regressions $3N$ and $3C$ and then using a seemingly unrelated post estimation testing procedure.

• **Variable description:**

- r_j^i: Investment in the linear option by member i for the problem j.
- dum_I_i: Income dummy.

The output from the six regressions, three each for either condition, is given in Table 5.5, with the upper panel for the control and the lower for the treatment. For hypothesis 1, the results show that the coefficient for r_2^1 is positive and significant for $1C$, but insignificant for $1N$, thus upholding the hypothesis. For hypothesis 2, the results show that the coefficient for r_2^1 is greater than that for r_1^2 in $1C$, with the ordering reversed in $1N$. The differences are quite large in either case, and post estimation means comparison tests generate significance with p-values indistinguishable from 0, upholding the hypothesis therefore. For hypothesis 3, the results show that the coefficient for r_2^1 in $1C$ is greater than that for r_1^2 in $2C$, while the coefficient for r_1^2 is smaller in $1C$ than in $2C$. This suggests that $1C$ constitutes a better model than $2C$. For N, the coefficient for r_2^1 is insignificant in regression 1, while the coefficient for r_1^2 is the same across regressions 1 and 2. This suggests that $1N$ and $2N$ are comparable models. Post estimation likelihood ratio tests confirm these indications and uphold hypothesis 3: the tests show that $1N$ and $2N$ are indeed comparable (p-value = 0.1098), while $1C$ nests $2C$ (p-value = 0). For hypothesis 4, visual observation of the results show that the coefficient for r_1^2 in $1N$ is approximately the same as the coefficent for r_1^1 in $3N$, while the coefficient for r_1^2 in $1C$ is less than the coefficient for r_1^1 in $3C$. Post estimation seemingly unrelated means comparison tests uphold the hypothesis: the coefficient estimates are found to be indistinguishable across $1N$ and $3N$ (p-value = 0.643), and distinguishable across $1C$ and $3C$ (p-value = 0). Finally, for hypothesis 5, visual observation yields that coefficients for r_1^1 are comparable across regressions $3N$ and $3C$. A post estimation seemingly unrelated means comparison test indeed shows the two are insignificantly different (p-value = 0.2118), hence upholding the hypothesis.

These results thus confirm our broad hypotheses. Our subject members 2 are engaging in social learning or imitation when choosing for problem 2 in the treatment condition, where they are under cognitive pressure, but not in the control condition, where they face limited cognitive pressure. The extent of imitation in the treatment is intense, with member 1's choice for problem 2 being more important as a determinant for member 2's choice for problem 2 than her own choice for problem 1. Members 1 however, who face no opportunities to learn socially, do not behave differently across treatment and control, with the dependence of the choice for problem 2 on earlier choice for problem 1 being the same across the conditions.

5.3.3 Rationality of imitation in the treatment

Results from the previous section indicated the presence of imitation in the cognitively challenging environment, as we found that member 2 in any pair in the treatment condition was heavily influenced by member 1's choice when choosing investment allocation for problem 2. There was no such influence in the control condition indicating that cognitive pressure is a necessary condition for imitation. While it is not unnatural that individuals may be prone to learning from anothers' choice for solving a problem which imposes cognitive demands, our results point to a surprisingly high degree of imitation, as we found that the influence was very strong. Indeed, findings reported in Table 5.5 indicated that member 2's choice for problem 2 was considerably more influenced by member 1's choice for the same problem than her own choice for problem 1 even though the two problems had by construction the same general structure, the same maximizer and the same maximand.

This high degree of imitation in the treatment condition raises the question as to whether the imitation uncovered in our environment can be identified as rational in any sense. In other words, are decision-makers in our environment merely imitating others due to an urge for pure mimicry, or is their decision to learn from others driven by a judgement that such imitation will increase payoff?

We approach this question as follows (for the treatment condition only). First, we investigate whether the degree of imitation by member 2 is positively correlated with the quality of the choice of member 1. An affirmative answer would be indicative of rational imitation.

Table 5.5: Regressions: response analysis for problem 2

	control (N)		
	$1N$	$2N$	$3N$
dependent variable	r_2^2		r_2^1
r_2^1	-0.068 (0.043)	- -	- -
r_1^2	0.765*** (0.041)	0.767*** (0.041)	- -
r_1^1	- -	- -	0.721*** (0.063)
income (dum_I_i)	0.287 (0.592)	0.289 (0.597)	-1.255 (0.909)
constant	1.758** (0.813)	1.174* (0.689)	3.197*** (0.785)
adjusted R^2	0.788	0.785	0.564
No. of obs.	100		
	treatment (C)		
	$1C$	$2C$	$3C$
dependent variable	r_2^2		r_2^1
r_2^1	0.712*** (0.031)	- -	- -
r_1^2	0.193*** (0.035)	0.318*** (0.066)	- -
r_1^1	- -	- -	0.825*** (0.025)
income (dum_I_i)	-0.185 (0.259)	-0.909 (0.488)	0.051 (0.233)
constant	0.971 (0.519)	7.196*** (0.836)	1.788*** (0.319)
adjusted R^2	0.758	0.128	0.846
No. of obs.	200		

, ** and * respectively indicate significance in terms of two-tailed p-values at the 5%, 1% and 0.1% levels. Standard errors are in parentheses.*

For the relevant regression equations see section 5.5.1

Following this, we analyze whether higher imitation has any beneficial influence on payoff, given the relationship between the degree of imitation and the quality of the choice of member 1. A positive answer with respect to the second issue, in addition to one with respect to the first, would provide more conclusive evidence in favor of some rationality in imitation. In our analysis we found that, imitation didn't occur in control condition (N). So, the analysis which is applicable for treatment condition (C) in this subsection 5.3.3, is not applicable for the control condition (N).

To implement our approach, we first construct a simple measure of imitation, the absolute difference for problem 2 between the responses of the two members, which we denote by d. The measure has its justification in that ceteris paribus higher imitation by member 2 would push her choice for problem 2 closer to that of member 1 for the same problem. A lower value of the absolute difference d would then be indicative of greater imitation.

We then pursue the first issue, whether the degree of imitation by member 2 is positively correlated with the quality of the choice of member 1. To do so, we conduct three linear regressions, all with d as the dependent variable. The main independent variable in all three is the distance between member 1's actual investment in the linear option for problem 2 and 7, the maximizer, which we denote by $\tilde{r}_2^1$. This distance is our measure of the quality of member 1's response to problem 2. We would expect a positive and significant coefficient on this variable under the hypothesis of rational imitation, with a lower distance, indicative of a higher quality response by member 1, leading to more imitation and hence greater closeness or a smaller absolute difference between the two members' responses. Each regression had four other independent variables. Income was common to all three. Another control variable, common to the three regressions, is r_2^2, member 2's response to problem 2, as a control for member 2's choice for the problem under consideration. The final independent variable was a control for member 2's cognitive ability ($control_1^2$), as reflected in her performance with respect to problem 1. The three regressions differed in terms of which control was used. The first (1) uses points earned by member 2 from problem 1, the second (2) uses the distance of member 2's actual choice for problem 1 from the payoff-maximizing one, and the third (3) uses a dummy which took the value 1 if member 2 achieved the payoff-maximizing choice for problem 1, and 0 otherwise.

• **Variable description:**

- d_i: Measure of imitation : the absolute difference for problem 2 between the response of the two members.

- $\tilde{r}^1_{2i}$: The distance between member 1's actual investment in the linear option for problem 2 and "7", the maximizer.

- P^2_{1i}: Points earned by member 2 from problem 1(control variable for equation (1)).

- $D^{2,P_{max}}_{1i}$: The distance of member 2's actual choice for problem 1 from the payoff-maximizing one(control variable for equation (2)).

- $(dum\_Inv)^{P^{2,P1}_{max}}_i$: A dummy which took value 1 if member 2 achieved the payoff-maximizing choice for problem 1 and 0 otherwise(control variable for equation (3)).

- r^2_2: Investment in the linear option by member 2 for the problem 2.

- dum_I_i: Income dummy.

The output from these regressions is reported in Table 5.6. The coefficient for $\tilde{r}^1_2$ is positive and significant in all three, as hypothesized, showing that better quality of choice by member 1 for problem 2 leads to a smaller distance between the two members' responses to problem 2, i.e., more imitation, after controlling for member 2's response to problem 2, and her cognitive ability. No other variable assumes significance.

We now analyze the second issue, whether higher imitation has any beneficial influence on payoff, given the relationship between the degree of imitation and the quality of the choice of member 1. To do this we follow three two-stage linear regression procedures, one for each of the regressions reported in Table 5.6. The first stage for each is any one of the regressions conducted above. We use the estimated absolute difference ($\hat{d}$) obtained from this regression as the independent variable in the second stage, with points earned by member 2 for problem 2 as the dependent variable. The coefficient on this independent variable is expected to be negative and significant under the hypothesis of rational imitation, with higher imitation or a

Table 5.6: Regression: rational imitation

	(1)	(2)	(3)
	d_i	d_i	d_i
$\tilde{r}_2^1$	0.099*	0.096*	0.096*
	(0.044)	(0.044)	(0.044)
control$_1^2$	-0.003	0.019	-0.157
	(0.003)	(0.038)	(0.393)
r_2^2	0.041	0.047	0.052
	(0.042)	(0.043)	(0.042)
income	0.418	0.423	0.424
(dum_I_i)	(0.239)	(0.242)	(0.243)
constant	1.256	0.199	0.243
	(0.922)	(0.412)	(0.405)
adjusted R^2	0.063	0.057	0.056
no. of obs.		200	

** indicates significance in terms of two-tailed p-values at the 5% level. Standard errors are in parentheses.*

For the relevant regression equations see section 5.5.1

lower absolute difference leading to more points earned.

- **Variable description:**

  - $\hat{d}_i$: Estimated absolute difference obtained from the regressions in Table 5.6

  - P_{2i}^2: Points earned by member 2 from problem 2.

The regression output is reported in Table 5.7, and shows a negative and significant coefficient for $\hat{d}$ in each of the three cases, as hypothesized. We conclude therefore that the imitation uncovered in our environment, the presence of which was established above, is rational and also payoff-improving: member 2 in any pair chooses to learn more if her matched member 1 provides a higher quality response for problem 2, and this learning process is beneficial in the sense that it improves payoff for the learner.

Table 5.7: Regression: second stage

	(1) P_{2i}^2	(2) P_{2i}^2	(3) P_{2i}^2
$\hat{d}$	-37.884***	-39.684***	-39.362***
	(2.052)	(2.094)	(2.144)
constant	361.434***	363.847***	363.415***
	(2.923)	(2.974)	(3.044)
adjusted R^2	0.63	0.64	0.63
no. of obs.	200		

**** indicates significance in terms of two-tailed p-values at the 0.1% level. Standard errors are in parentheses.*

For the relevant regression equations see section 5.5.1

5.4 Conclusions

This chapter shows that cognitive pressure can be a cause for social learning or imitation. Our field experimental subjects faced deterministic and fully described budget allocation problems. They showed no tendency to imitate when the problems they confronted were comparatively easy. However, the presence of comparatively hard problems, likely to induce greater decision cost and cognitive pressure, generated substantial imitative behavior, to the extent that imitation was given preference over native decision. There are some literature where they documented the phenomenon of social learning in events with stochastic outcomes (See Arthur and Lane [9]) and Narduzzo and Warglien [74].

The chapter also shows that the imitative behavior identified under conditions of cognitive pressure can be described as rational. Imitating subjects tended to imitate those whose decisions were better, with the resultant process tending to improve payoff. This finding is incompatible with imitation being akin to pure mimicry, and suggests purposivity.

Explanations for herd behavior and information cascades in the extant literature tend to be articulated in terms of informational incompleteness. Imitation takes place in this view because a) decision makers are imperfectly aware of the relation between choice and outcome due to some aspect of the problem under consideration being not fully described, and b) prior decision makers possess some relevant information which is reflected in their decisions. Our study by contrast analyzes an environment where problems are fully described. Imitation emerges here due to the cognitive pressure produced when the problem under consideration is comparatively hard, which results in an inability to perfectly comprehend the relation between choice and outcome. Our investigation thus simultaneously shows that cognitive pressure can be a cause for imitation, and that informational imperfection resulting from incomplete description of problems is not necessary for imitation. At the same time, the extant view leads to the possibility of decision makers getting trapped in incorrect cascades by following erroneous earlier decisions. Our results by contrast suggest that imitation generated through cognitive pressure tends to be selective, and does not lead to payoff-reducing herd behavior.

5.5 Appendix

5.5.1 Regression equations:

• **Regression equations for control condition (N) for the Table 5.3:**

$$P_i^N = \alpha_1^N + \alpha_2^N.dum\_M_i + \alpha_3^N.dum\_I_i + \varepsilon_{1i}^N, \; \forall i = 200 \tag{N1}$$

$$D_i^N = \beta_1^N + \beta_2^N.dum\_M_i + \beta_3^N.dum\_I_i + \varepsilon_{2i}^N, \; \forall i = 200 \tag{N2}$$

$$(dum\_Inv)_i^{P_{max},N} = \gamma_1^N + \gamma_2^N.dum\_M_i + \gamma_3^N.dum\_I_i + \varepsilon_{3i}^N, \; \forall i = 200 \tag{N3}$$

• **Regression equations for treatment condition (C) for the Table 5.3:**

$$P_i^C = \alpha_1^C + \alpha_2^C.dum\_M_i + \alpha_3^C.dum\_I_i + \varepsilon_{1i}^C, \forall i = 400 \tag{C1}$$

$$D_i^C = \beta_1^C + \beta_2^C.dum\_M_i + \beta_3^C.dum\_I_i + \varepsilon_{2i}^C, \forall i = 400 \tag{C2}$$

$$(dum\_Inv)_i^{P_{max},C} = \gamma_1^C + \gamma_2^C.dum\_M_i + \gamma_3^C.dum\_I_i + \varepsilon_{3i}^C, \forall i = 400 \tag{C3}$$

• **Regression equations for the Table 5.4:**

$$(dum\_Inv)_i^{P_{max}} = \alpha_1 + \alpha_2.dum\_exp_i + \alpha_3.dum\_M_i + \alpha_4.dum\_I_i + \varepsilon_{1i}, \; \forall i = 600 \tag{1}$$

$$(dum\_Inv)_i^{P_{max}} = \beta_1 + \beta_2.dum\_exp_i + \beta_3.P_i^{P1} + \beta_4.dum\_I_i + \varepsilon_{2i}, \; \forall i = 300 \tag{2}$$

$$(dum\_Inv)_i^{P_{max}} = \gamma_1 + \gamma_2.dum\_exp_i + \gamma_3.(dum\_Inv)_i^{P_{max}^{P1}} + \gamma_4.dum\_I_i + \varepsilon_{3i}, \; \forall i = 300 \tag{3}$$

$$(dum\_Inv)_i^{P_{max}} = \zeta_1 + \zeta_2.dum\_exp_i + \zeta_3.(D\_Inv)_i^{P_{max}^{P1}} + \zeta_4.dum\_I_i + \varepsilon_{4i}, \; \forall i = 300 \tag{4}$$

• **Regressions equations for Control condition(N) for the Table 5.5:**

$$r_{2i}^2 = \alpha_1^N + \alpha_2^N.r_{2i}^1 + \alpha_3^N.r_{1i}^2 + \alpha_4^N.dum\_I_i + \varepsilon_{1i}^N, \; \forall i = 100 \tag{1N}$$

$$r_{2i}^2 = \beta_1^N + \beta_2^N.r_{2i}^1 + \beta_3^N.dum\_I_i + \varepsilon_{2i}^N, \; \forall i = 100 \tag{2N}$$

$$r_{2i}^1 = \gamma_1^N + \gamma_2^N.r_{1i}^1 + \gamma_3^N.dum\_I_i + \varepsilon_{3i}^N, \; \forall i = 100 \tag{3N}$$

• **Regressions equations for Treatment condition(C) for the Table 5.5:**

$$r^2_{2i} = \alpha^C_1 + \alpha^C_2.r^1_{2i} + \alpha^C_3.r^2_{1i} + \alpha^C_4.dum\_I_i + \varepsilon^C_{1i},\ \forall i = 200 \tag{1C}$$

$$r^2_{2i} = \beta^C_1 + \beta^C_2.r^1_{2i} + \beta^C_3.dum\_I_i + \varepsilon^C_{2i},\ \forall i = 200 \tag{2C}$$

$$r^1_{2i} = \gamma^C_1 + \gamma^C_2.r^1_{1i} + \gamma^C_3.dum\_I_i + \varepsilon^C_{3i},\ \forall i = 200 \tag{3C}$$

• **Regression equations for the Table 5.6:**

$$d_i = \alpha_1 + \alpha_2.\tilde{r}^1_{2i} + \alpha_3.P^2_{1i} + \alpha_4.r^2_2 + \alpha_5.dum\_I_i + \varepsilon_{1i},\ \forall i = 200 \tag{1}$$

$$d_i = \beta_1 + \beta_2.\tilde{r}^1_{2i} + \beta_3.D^{2,P_{max}}_{1i} + \beta_4.r^2_2 + \beta_5.dum\_I_i + \varepsilon_{2i},\ \forall i = 200 \tag{2}$$

$$d_i = \gamma_1 + \gamma_2.\tilde{r}^1_{2i} + \gamma_3.(dum\_Inv)^{P^{2,P1}_{max}}_i + \gamma_4.r^2_2 + \gamma_5.dum\_I_i + \varepsilon_{3i},\ \forall i = 200 \tag{3}$$

• **Regression equations for the Table 5.7:**

$$P^2_{2i} = \alpha_1 + \alpha_2.\hat{d}_i + \varepsilon_{1i},\ \forall i = 200 \tag{1,2,3}$$

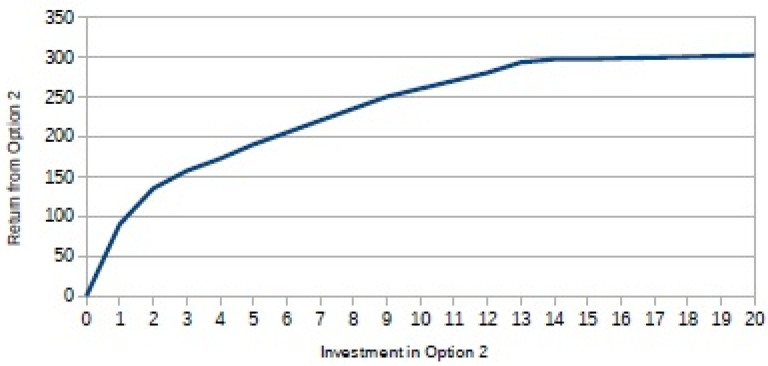
5.5.2 Figures

You have to allocate 20 across 2 investment options (integer allocation only, between and including 0 and 20). Please choose your investment in option 1. Whatever remains will be automatically invested in option 2.

Option 1: 1 unit of investment will give 5 units of return.

Option 2: See the table and figure

investment in Option 2	return from opton 2
0	0
1	90
2	135
3	157
4	172
5	190
6	205
7	220
8	235
9	250
10	260
11	270
12	280
13	293
14	297
15	297
16	298
17	299
18	300
19	301
20	302



How much do you want to invest in option 1?

Figure 5.1: Sample instruction and decision sheet (C1)

Table for N1

investment in option 2	return from option 2
0	160
1	170
2	180
3	190
4	200
5	210
6	220
7	230
8	240
9	250
10	260
11	270
12	280
13	**293**
14	295
15	298
16	301
17	304
18	307
19	310
20	313

Figure for N1

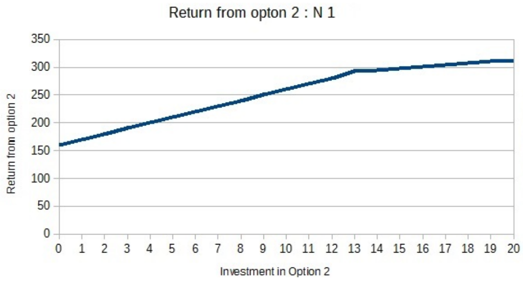


Table for N2

investment in option 2	return from option 2
0	30
1	50
2	70
3	90
4	110
5	130
6	150
7	170
8	190
9	210
10	230
11	250
12	270
13	**293**
14	294
15	298
16	302
17	304
18	306
19	308
20	310

Figure for N2

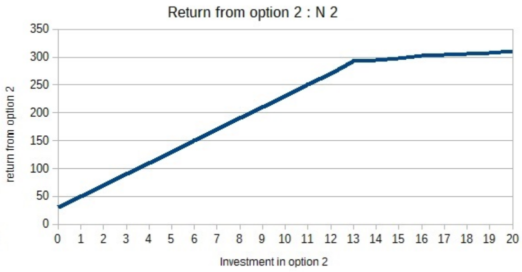


Figure 5.2: Non-cognitive options

Table for C1

investment in Option 2	return from opton 2
0	0
1	90
2	135
3	157
4	172
5	190
6	205
7	220
8	235
9	250
10	260
11	270
12	280
13	293
14	297
15	297
16	298
17	299
18	300
19	301
20	302

Figure for C1

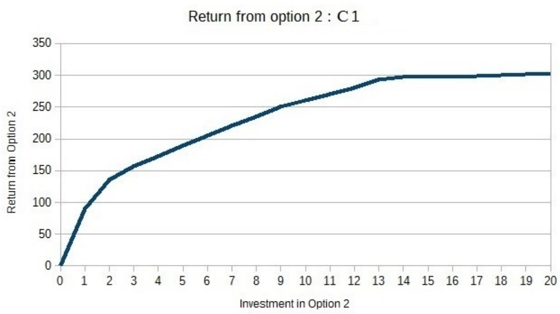


Table for C2

investment in option 2	return from option 2
0	140
1	150
2	159
3	172
4	185
5	200
6	215
7	230
8	245
9	261
10	272
11	280
12	287
13	293
14	295
15	296
16	297
17	298
18	299
19	301
20	303

Figure for C2

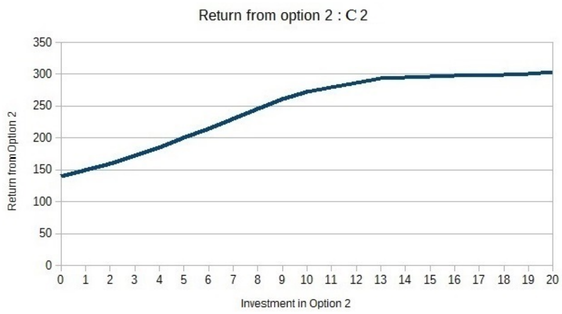


Figure 5.3: Non-linear cognitive options

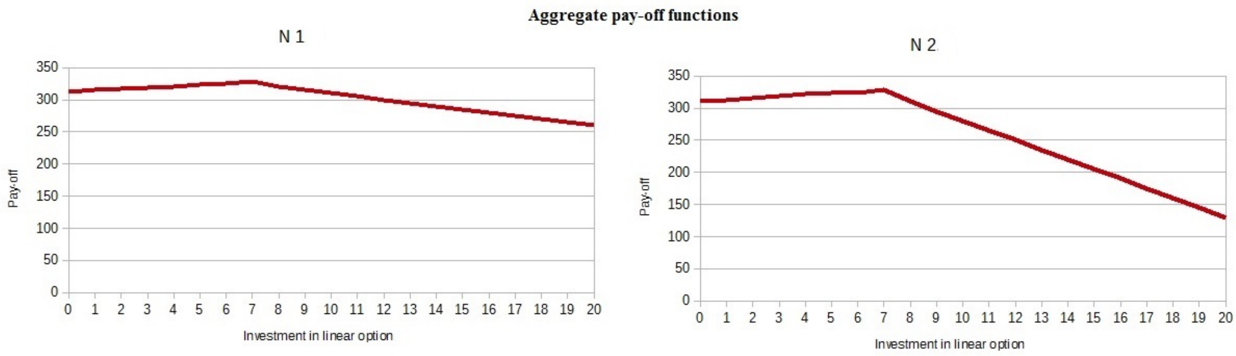


Figure 5.4: Aggregate pay-off functions for non-cognitive problems

Figure 5.5: Aggregate pay-off functions for cognitive problems

Chapter 6

Concluding Remarks

This thesis falls within the confines primarily of descriptive analysis, focussed around explaining how decisions are made through the analysis of observed behavior. The main aim has been to examine the descriptive power of RCT. Each chapter takes an implication or implicit assumption of RCT, and examines its empirical validity under experimental conditions. The results in general show numerous failures of RCT criteria, and strengthen the idea of BRCT, one of the principal alternatives to RCT.

In chapter 2 we asked whether contingent plans are consistent, i.e., whether irrelevant information matters for decision making, in an environment of risky choice. Our approach involved two laboratory experiments. Subjects in either case had to make choices with and without alternative contingencies, where each alternative contingency acted as a potential source of irrelevant information. We found that consistency was more likely to obtain when problems were complete, but unlikely to hold in more complex settings. Our results suggested that any failure of consistency is due to the use of irrelevant information in decision making. Since consistency is fundamentally associated with rationality, our findings indicate rationality in some strong forms may not always be empirically realistic.

In chapter 3 we investigated whether performance in cognitively demanding financial tasks can depend on prior outcomes from independent and unrelated financial tasks. We tested this directly through a two-part laboratory experiment. The first part of the experiment generated prior outcomes endogenously in the laboratory through a series of lottery choice problems and the second part consisted of a cognitively challenging yet deterministic budget allocation problem. We found that subjects who faced a positive financial outcome history in the first part tended to perform better than those who faced a negative outcome history. Some subjects, who were cognitively weaker performers, tended to be immune in their choices to prior personal experience or history. The results establish a link between economic condition and quality of financial choice, and support the idea that economic condition can affect cognitive performance, which can impact decision quality.

In chapter 4 we examined the effect of a deadline in a risky decision environment through a two phase laboratory experiment. In both phases subjects faced 20 risky investment choice problems. Phase 1 was without a deadline. In the control conditions, phase 2 was an exact repeat of phase 1. Comparison across phases of the control conditions allowed us to record behavior when the task was being faced a second time (without a deadline). Phase 2 of the treatment condition involved a deadline. The deadline was endogenously set at the level of the subject: the time taken by the subject in the first phase was the deadline in the second. We found some subjects tended to accelerate decision making when faced with a deadline, while a minority tended to display paralysis. Overall the effect of the minority group was large enough to generate an aggregate inefficiency. This suggests that a deadline can act as a binding constraint in risky environments. Our main contribution to the literature studying risky decision making in the presence of a deadline is methodological, as prior papers have only used arbitrary deadlines. Our exploration confirms many prior results derived under arbitrary deadlines, such as decreased risk aversion in the presence of a deadline, and therefore suggest some robustness to existing findings.

In chapter 5 we aimed to check whether individuals tend to imitate others in their choices in cognitively demanding environments and, if so, whether such can imitation can be characterized as rational. We conducted a field experiment with paired subjects who both faced the same 2 problems. The first problem was approached independently by each subject. The second problem was approached independently by one subject, while the other subject saw the response of her partner before deciding. We found cognitive pressure can be a causal basis for imitation: subjects in the cognitively demanding condition tended to be heavy imitators, while subjects in the cognitively undemanding (control) condition tended not to imitate. To understand the possible rationality of imitation, we analyzed whether imitation in the treatment condition had an impact on payoff. We found imitation was quality sensitive, with better decisions more likely to be imitated, and tended to be payoff-improving. This finding suggests imitation was purposive and hence rational, and cannot be described as pure mimicry.

The findings of the thesis may yield policy implications, some of which we note briefly in conclusion. Development policy often involves provision of insurance and other financial products to poorer individuals on subsidized bases. The results of Chapter 2 suggest that it may be better if such policies were decentralized, and the mix of products offered allowed to be modified on the basis of information regarding an individual's problem set. The results of Chapter 3 indicate that standard charges based on financial outcomes, such as income and wealth taxes, can have effects on future performance through variations in effective cognitive capacity. More research is required on this topic to arrive at a determination with regard to the overall effect of such taxes on cognitive capacity and hence aggregate performance. Results of Chapter 4 show that the aggregate effect of a deadline can depend on the extent of presence of those liable to be paralyzed, with a deadline inadvizable in settings where a high proportion of the population is susceptible. In reverse scenarios however, a deadline can be beneficial in accelerating activity and releasing time for other productive opportunities. Finally, results of Chapter 5 indicate that consumer choice may not always be optimal in cognitively demanding situations, such as choosing an investment plan. Feedback from previous customers can be a useful service providers can offer potential customers in such environments, enabling them to update calculations and improve choice.

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