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The Impact of Financial Incentives on Decision Making: Further Evidence

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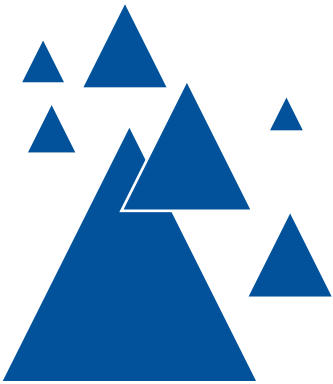

The Impact of Financial Incentives on Decision Making: Further Evidence

by

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FOREWORD

Do financial incentives improve decision making and motivate performance? The use of financial incentives is widespread and believed to work but what evidence is there to support their continued use? This research furthers the work undertaken by the authors on financial incentives published by ICAS in 2006. The project takes a laboratory experiment approach to test the theory that formal performance related rewards increase the use made by decision-makers of valuable information and, in turn, lead to improved decision-making. The study varied from the 2006 work in three ways: earnings were denominated directly in money; there was no time cost involved; and participants were randomly assigned different incentives.

The experiment found that profit performance was strongly and significantly related to the level of profit-related incentives. The research highlights the importance of individuals understanding the structure of incentives for incentives to be effective. The research also identified that, irrespective of incentives, individuals perform better when they have an intrinsic interest in something or wish to please. Therefore, recruitment of the 'right' individual for the job remains a key factor in performance. Although the results of this study are based on an experiment or "business game", they have wider implications for the business community.

This project was funded by the Scottish Accountancy Trust for Education and Research (SATER). The Research Committee of the Institute of Chartered Accountants of Scotland has also been happy to support this project. The Committee recognises that the views expressed do not necessarily represent those of ICAS itself, but hopes that this

project will contribute to the understanding of the impact of financial incentives on decision making performance and the associated design of employment and executive contracts.

David Spence
Convener, Research Committee
January 2008

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We would also like to express our appreciation to participants at the British Accounting Association (BAA) Northern Group Conference, at Northumbria University, September 2005, for helpful comments in the preparation for this study, to staff in the Accounting and Finance subject area at Newcastle Business School for comments at staff seminars and for acting as 'guinea pigs' for the early development work, and to Chris Woodford for advice on issues that arose in the use of the UNIX system and also program development. Anthony Miller would also like to express his personal appreciation to David Oldroyd, Reader in Accounting and Finance at Newcastle University, for his consistent encouragement and support.

Executive Summary

This project involves extension work to an earlier ICAS funded project entitled 'The Impact of Financial Incentives on Decision Making.' This executive summary first gives a summary of the earlier project and its findings; it then outlines the nature of the extensions involved in the present work. Following this the key findings and conclusions flowing from this further work are discussed.

The original project, Dobbs and Miller (2006a) was concerned with using the 'experimental approach' to study whether financial incentives 'work' in complex decision-making environments. The hypothesis was that, if a participant's earnings were related to performance, this would induce the participant to use information systems more fully and efficiently, to learn faster and to take decisions more seriously and hence to turn in higher levels of performance.

The participants in the original experiments played in essence a stylised 'business game', involving a sequence of decisions over time. Individuals could choose to access information about their performance so far, and this information could be used to help inform and improve decisions for the future. Accessing information was costly, but intelligent use of the information could translate into higher levels of profit performance for the firm, and hence, given a positive level of incentive pay, into higher earnings for the individual.

The participants in the original experiments were Newcastle Business School year two or year three undergraduates, or postgraduates including those on MA, MSc and MBA programmes. Participants were divided into two groups: an 'incentive group' in which an individual's earnings depended on the quality of their decision-making, and a control group for whom earnings were independent of their decisions. The

study then examined the use of information and the ensuing level of performance for individuals in these two groups, also looking at how performance was affected by individual characteristics. The individual characteristics measured were; (i) the individual's latest university grade point average (GPA); (ii) age; (iii) gender; (iv) nationality; and (v) whether the participant was undergraduate or postgraduate. The original experimental findings were:

1. that profit-related pay had little impact on performance; it improved it slightly, but not with any statistical significance;
2. individual characteristics matter to some extent; GPA, being a postgraduate, being male, being a UK student, all tended to unilaterally increase performance, though none of these impacts were particularly strong.

The failure to find significant support for the hypothesis that incentives improve the use made of information was a result at variance with the conclusions of a related experiment reported by Sprinkle (2000). Reviewing possible reasons for the discrepancy, the role of chance, of systematic differences in individual characteristics across samples, and of weaknesses of research design were all considered. The present authors felt that, on the basis of chance alone, the original Dobbs-Miller finding should be considered more reliable than that of Sprinkle. Systematic differences might arise because Sprinkle's participants were drawn from an American academic institution, whilst those in the latter experiment were drawn from a UK academic institution. These appear most unlikely to be material, however, particularly given the international character of the student population in the Newcastle University Business School, and given that the experiment also controlled for personal characteristics. Three potential issues associated with the research design employed in Dobbs-Miller, at least two of which affected the study by Sprinkle, were considered as possible explanations: a lack of salient incentives; a

failure to fully communicate a potentially over-complicated experimental environment to participants; and implementation of a procedure for 'inducing' in participants specific attitudes to risk which might be theoretically and practically problematic.

The objective of the current study remains the same as that of the earlier Dobbs-Miller project: '…to test the theory that formal performance-related rewards (PRR) … increase the use made by decision-makers of valuable information and, in turn, lead to improved decision-making', though in the new experiments the performance-related rewards employed are permitted to vary more widely. The new work makes three contributions. First, it provides additional evidence on the role, in performance measurement and decision-making, of information, of which the managerial accounting function constitutes an important special case. Second, by exploring the impact of variation in the level of rewards on experimental outcomes, the current work provides evidence relevant to the design of experiments. Third, we develop a new approach to statistical modelling for the basic environment in Sprinkle (2000) and Dobbs and Miller (2006a).

The 'laboratory' experiment used to generate data for this study was similar to the original project:

Individual participants, acting independently, were each requested to perform an identically structured, timed, decision-making task, earning an outcome labelled in the experiment as 'profit'. The task was designed as a multi-period decision problem, incorporating a significant role for feedback information to enhance profit performance (Dobbs and Miller, 2006a).

Participants were university students, on various postgraduate and undergraduate degree programmes, all with a business, economics and accounting focus, at the University of Newcastle upon Tyne during the academic year 2005-06.

The current work differs from the original project as follows:

1. Earnings were denominated directly in money. By contrast, in the original study by Sprinkle, and in our early ICAS study, a participant's earnings were generated through a two stage process. The initial 'earnings' arising out of a participant's decisions were denominated in lottery 'tickets' to second stage lotteries. Getting more lottery tickets gave the individual a greater chance of winning in these second stage lotteries, each of which involved fixed money prizes. The fact that the incentive effect observed in Dobbs and Miller (2006a) was weak might be explained by the complexity of this two stage process, since this complexity might have contributed to a reduction in effectiveness when communicating the experimental environment to participants. Introducing a direct linkage might overcome this to some extent; the pros and cons of this modification are discussed in chapter two below.

2. In order to simplify the experimental environment further, explicit money cost to participants of spending time on the decision task, in Dobbs and Miller (2006a) averaging one penny for every ten seconds used, was set at zero in the new experiments. The analysis of the post-experiment questionnaire returns in Dobbs and Miller (2006a) clearly demonstrated that participants consistently over-estimated this cost, and this may have led to undue 'hurrying' of decision-making. Given that the focus of the experiment was on the impact of incentives on decision-making, not on the speed of decision-making, it made sense to simplify this aspect by setting the cost to zero.

3. The most significant difference between the current and earlier experiments, however, is in the manipulation of the magnitude of incentives. In both Sprinkle (2000) and Dobbs and Miller (2006a), just two sets of reward parameters were employed, but in the new series of experiments, participants were randomly assigned to nineteen distinct sets of reward parameters.

Statistical modelling of the underlying data generating process was then undertaken to estimate the impact of incentives on variation in response variables; specifically, profit performance, the frequency of accessing information and time spent on the decision-making task. The findings on the determinants of these variables will now be discussed in turn.

Profit performance is strongly and significantly related to the level of profit-related incentives. For every halfpenny increase in the money incentive per unit of profit earned for the firm, on average profit increased by 12 units in every trial of the experiment. A substantial impact is thus achievable through increasing the level of payment by results. Increasing the costs of accessing information reduced profit performance, but not significantly. Other things equal, male participants did better at earning profit than females; the quantitative effect was almost identical to the corresponding finding in Dobbs and Miller (2006a).

Increases in the money reward per profit point significantly increased the use made of information, while increases in the cost of accessing information significantly reduced its use. It is unclear why the reduced use of information as costs increase does not lead to a significantly lower profit performance for the firm. Participants with an Asian nationality used information significantly more than British participants.

Money rewards and costs did not influence decision-making time at all. Maturity, either measured by age or graduate status, was associated with an increase in time spent on the task.

Individuals in these experiments, even in the absence of incentives, tended to use information in order to increase firm profits. Such behaviour is normal and commonplace in organisational contexts; in the experimental context it may arise out of interest in puzzle solving per se, but also because of a general spirit of cooperation. The issue then is whether introducing incentives creates an incremental effect on performance. The present study finds that real money incentives can indeed be used to affect subject behaviour and performance in

experiments, confirming a result in Sprinkle (2000). Thus, when information that is valuable for enhancing profit is also costly, the efficiency with which it is used can be increased if individuals are given profit-related incentives. Further this is true even when the incentives are really quite modest, in terms of the absolute level of reward. Statistical analysis of the post-experiment questionnaire in conjunction with the performance data reveals that these incentive effects are most pronounced for those who best understood the experimental environment. This is logical and reassuring; after all, if one has a poor understanding of the structure of incentives, or indeed whether incentives exist at all, one is less likely to be responsive to those incentives.

To sum up, the research identifies that irrespective of incentives, individuals perform better when they have an intrinsic interest in something, or when they 'wish to please' but that individuals do respond to incentives, and they respond to incentives better, the better these are understood. This emphasises the importance of individuals understanding the full structure of the incentive schemes they face if these incentives schemes are likely to be effective. In a business context, this translates to the observation that recruitment of individuals who manifest intrinsic levels of commitment may remain important, but that providing appropriate incentive structures is likely to remain a key factor in performance.

1 Experimental Research on Information and Incentives

Introduction

This report sets out additional empirical results relating to an earlier ICAS-funded project entitled 'The Impact of Financial Incentives on Decision Making', by Dobbs and Miller (2006a). In this introductory chapter, the background to Dobbs and Miller (2006a) is reviewed, followed by a discussion and assessment of the initial results; a necessary starting point for understanding the contribution made by the additional work described in the present report. The chapter continues with an outline of the research objectives, and methods adopted, for the new work. A brief guide to the overall structure of the current report, indicating its general content, concludes the chapter.

Background to the project

The current project extends earlier empirical work in Dobbs and Miller (2006a), where a controlled experiment was used to test whether individuals with profit-related rewards (PRR) would be motivated to make better use of information and to enhance profit performance, compared to a control group of individuals earning a 'flat rate' or fixed reward (FR), unrelated to profit performance. The objective of the test was to:

> *…contribute to a greater understanding of the role of information, including accounting information, in performance measurement and decision-making.* (Dobbs and Miller (2006a), p.6)

Participants were drawn from university students of business, economics and accounting, attending the University of Newcastle upon Tyne in the academic year 2004-05. The results provided information on the motivational characteristics of individuals in an employment group of potential importance to UK accounting firms. The experimental design in Dobbs and Miller (2006a) was based on a previously-published paper by Sprinkle (2000), entitled 'The Effect of Incentive Contracts on Learning and Performance'.

In both Sprinkle (2000) and Dobbs and Miller (2006a), the data suggested that PRR participants made more use of information than FR participants, and that their use of information resulted in better profit performance. However, whereas in Sprinkle (2000) the effects were strongly significant, in Dobbs and Miller (2006a) they were weak and insignificant. Indeed, in Dobbs and Miller (2006a), FR participants did nearly as well as PRR participants. Perhaps the discrepancy between the significant effect in Sprinkle (2000) and the insignificant effect in Dobbs and Miller (2006a) is not surprising, given that real incentives appear to enhance performance in only about half of the published experimental investigations (Sprinkle, 2003). However, the lack of consistent results in this literature pertains to a wide variety of experimental environments, so these other environmental factors might explain the variation in findings. In the present case, by contrast, Dobbs and Miller (2006a) and Sprinkle (2000) have very similar, albeit not identical, research designs. Hence, it seems likely that either the strongly significant finding in Sprinkle (2000) is a 'false positive', or the weak effect found in Dobbs and Miller (2006a) is a 'false negative'. Given the similarity in the experimental environment, the discrepancy between Sprinkle (2000) and Dobbs and Miller (2006a) results might be attributed to:

(i) chance;

(ii) cohort effects – differences in individual characteristics within the samples; and/or

(iii) weakness in research design.

The operation of chance alone would tend to cast greater doubt on the conclusion in Sprinkle (2000), since it was based on a substantially smaller sample size and a less general implementation of the basic experimental design than Dobbs and Miller (2006a). Cohort effects were not documented or measured in Sprinkle (2000). In Dobbs and Miller (2006a), they had some impact, though probably too small to plausibly explain the discrepancy in the results in the two studies. Turning to research design, three sufficient conditions for the valid testing of quantitative predictions about market behaviour are relevant (Plott, 1982):

a) participants should fully understand the experimental environment they face;

b) participants should have no intrinsic preference regarding performance of the experimental task, either positive or negative; and

c) participants should prefer more money to less.

Additionally, when considering decision-making under uncertainty, some attention should be given to individual attitudes to risk. In this respect, the experimental design in Dobbs and Miller (2006a) followed Sprinkle (2000), by incorporating a lottery procedure designed to 'induce' in all participants a neutral attitude to risk (Berg *et al.*, 1986). For this to be effective, Dobbs and Miller (2006b) show that two more conditions need to be added to the three mentioned above; roughly speaking, these can be described as:

d) participants should possess a certain facility for handling probability data; and

e) when participants perform a sequence of decision-making tasks, the lottery procedure leading to a participant's earnings should be applied only to independent tasks.

More precise statements of these two conditions are given in Dobbs and Miller (2006b). The above five conditions may not all be necessary, particularly as the PRR/FR research hypothesis involved only a qualitative prediction; that PRR participants make more use of information and earn more profit for the firm than FR participants. However, if they are not satisfied, the validity of hypothesis tests are then cast into some doubt. Before considering the research design within the framework of the conditions above, it should be noted that Dobbs and Miller (2006a) was intended to replicate the strongly significant results in Sprinkle (2000). Hence, there seemed to be some merit in using the same design in Dobbs and Miller (2006a) as in Sprinkle (2000), despite reservations expressed in Dobbs and Miller (2006a). Any design weaknesses are, therefore, likely to be common to both Dobbs and Miller (2006a) and Sprinkle (2000). Nevertheless, there were differences between the two studies. Firstly, documentation of the experimental environment in Sprinkle (2000) was, in various places, incomplete, so there were some points where the replication might have been only approximate. Further, there were methodological reasons for Dobbs and Miller (2006a) explicitly deviating from exact replication: to avoid repeating a deception practised in the original work by Sprinkle (2000), whereby the environment described to participants in the instructions was not faithfully implemented, but was replaced instead by a different, albeit similar, environment. The deception had been employed in order to enhance experimental control when generating sample data. Its avoidance in Dobbs and Miller (2006a) led to a faithful implementation of the experimental environment described in both Sprinkle (2000) and Dobbs and Miller (2006a).

These observations aside, Dobbs and Miller (2006a) identified three possible weaknesses in research design, at least two of which, despite

silence in Sprinkle (2000) on design weaknesses, are shared in common by the two studies. Firstly, it was noted, both from experimental data and from observation of behaviour, that FR participants made significant use of costly feedback information and made significant efforts to increase profit levels, despite the fact that their explicit financial rewards gave them a disincentive to behave in this manner. A similar propensity for FR participants to co-operate was reported in Sprinkle (2000), although as already noted, in that study the effect did not preclude observation of a significant difference in behaviour between PRR and FR participants. Nevertheless, these observations suggest that condition (b) above was not satisfied in either study; that is, participants had personal preferences in favour of taking decisions that would increase profit for the firm, independently of explicit money rewards they could earn. In the written instructions given to, and also read out to, experimental participants in Dobbs and Miller (2006a), it was explicitly stated that participants should try to maximise profit. The participants in Dobbs and Miller (2006a) were then given, separately for PRR and FR participants, their own private reward structures, which included an average cost of £0.001 per additional second of time taken to complete the decision task, and an average cost of £0.008 for each request for information, both intended to act as disincentives to co-operating with the explicit goal announced in the written instructions. PRR participants were additionally offered an average individual reward of £0.004 per unit of profit earned; whereas the reward given to FR participants was unaffected by profit. In summary, PRR participants were given a reinforcing real incentive to co-operate with the written instructions, whereas FR participants were given a real disincentive. The magnitude of these money incentives were selected to approximately match those used in Sprinkle (2000). With the benefit of hindsight, it was suspected that the Dobbs and Miller (2006a) levels of financial disincentives were simply not salient enough to dominate intrinsic personal preferences in favour of co-operation and therefore to induce measurable differences between PRR participants and FR

participants. Hence, in Dobbs and Miller (2006a), one suggestion for further experimental work was to increase:

> *…the saliency of incentives through larger money payments, so that, whilst PRR participants still have an incentive to maximise profits, the cost to FR participants of behaving co-operatively from private motives is sharply increased. (*Dobbs and Miller (2006a), p.52)

Secondly, analysis of a post-experiment questionnaire used in Dobbs and Miller (2006a) indicated that the lottery procedure, incorporated into the design to 'induce' risk-neutral behaviour in participants, made communication of the experimental environment problematic (see Selten *et al.*, (1999), for a discussion of similar findings in other contexts). In particular, participants found the costs associated with both accessing information and taking time to complete a task obscure. Moreover, many FR participants did not appear to realise that their financial rewards did not depend upon profit earned. These findings call into question conditions a) and d) above, for participants did not fully understand the environment they faced and did not have the required sophistication in assessing probabilities. In Sprinkle (2000), prior to beginning the experiment, participants 'answered a brief quiz regarding the instructions' (Sprinkle (2000), p.309), but no other details are provided in Sprinkle (2000) about the quiz or what it revealed.

Finally, the manner in which the lottery procedure was implemented in Sprinkle (2000) and in Dobbs and Miller (2006a) is inconsistent with condition(e) above. In the concluding section of Dobbs and Miller (2006a) it was recommended that more:

> *…experiments, dispensing with the lottery procedure, could be undertaken to clarify whether or not the effects of performance-related incentives are stronger when made clearer to participants.* (p.52)

The above discussion provides a motivation for additional experiments, departing somewhat from the original intention in Dobbs and Miller (2006a) of replicating the research design in Sprinkle (2000), in order to re-examine the role of financial incentives in improving use of information and of decision-making. The current work reports the results of these new experiments, designed in particular in response to the potential violation in Dobbs and Miller (2006a) and Sprinkle (2000) of sufficient conditions (a), (b), (d) and (e) above, with the lottery procedure dropped from the experimental design, and with systematic variation of the saliency of real money incentives.

Objectives and research methods employed

The objective of the current study remains the same as that of the earlier Dobbs and Miller (2006a) project:

> *...to test the theory that formal performance-related rewards (PRR) … increase the use made by decision-makers of valuable information and, in turn, lead to improved decision-making.* (Dobbs and Miller (2006a), p.5)

In the new experiments in this study the performance-related rewards employed are permitted to vary more widely. The new work makes three contributions. First, it provides additional evidence on the role, in performance measurement and decision-making, of information, of which the managerial accounting function constitutes an important special case. However, the intention is to keep the experimental environment and information function deliberately simple, analytically straightforward, and as far as possible, consistent with the domain in which the theory of incentives operates, rather than attempt to simulate a complex naturally-occurring environment and a complete managerial accounting function, where consistency with the theory's domain cannot

easily be verified. Second, by exploring the relation between saliency of participant rewards and experimental outcome, the current work provides general evidence relevant to the methodology of experimental design. Third, we develop the statistical modelling for the basic environment in Sprinkle (2000), as well as its attempted replication in Dobbs and Miller (2006a), in order to take account of data issues, some of which are common to all three studies.

The 'laboratory' experiment used to generate data for this study was similar to the original project:

> *Individual participants, acting independently, were each requested to perform an identically structured, timed, decision-making task, earning an outcome labelled in the experiment as 'profit'. The task was designed as a multi-period decision problem, incorporating a significant role for feedback information to enhance profit performance.* (Dobbs and Miller (2006a), p.6)

Participants were university students, on various postgraduate and undergraduate degree programmes, all with a business, economics and accounting focus, at the University of Newcastle upon Tyne during academic year 2005-06.

The current work differs from the earlier Dobbs and Miller (2006a) project in the following respects:

- Both Sprinkle (2000) and Dobbs and Miller (2006a) featured a rewards lottery, in which a participant's earnings were denominated in 'tickets' in a subsequent lottery involving a fixed money prize. The complexity of this process was suspected to have contributed in Dobbs and Miller (2006a) to a reduction in effectiveness when communicating the experimental environment to participants. Accordingly, in the present study, participant earnings were denominated directly in money.

- In order to simplify the experimental environment further, the explicit money cost to participants of spending time on the decision task was set to zero. In Dobbs and Miller (2006a), this cost averaged one penny for every ten seconds used; analysis of the post-experiment questionnaire returns in Dobbs and Miller (2006a) clearly demonstrated that participants consistently over-estimated this cost. However, the focus of the experiment is on the trade-off between the costs and benefits of using information to increase profit for the firm, and not on 'time pressured decision-making'. Given this focus, the cost of time is clearly an unnecessary distraction and an unnecessary complication; setting this cost to zero is thus regarded as a useful simplification of the original experimental environment.
- The most significant difference between the current and earlier experiments, however, is in manipulation of the magnitude of incentives. In Dobbs and Miller (2006a), just two sets of reward parameters were employed, but in the new series of experiments, participants were randomly assigned to up to nineteen distinct sets of reward parameter values. Statistical modelling of the underlying data generating process was then undertaken to estimate the impact of variation in incentive levels on the response variables; specifically, profit performance, the frequency of accessing information and time spent on the decision-making task.

Structure of the report

The report is divided into four chapters. Chapter two reviews the experimental environment used in Dobbs and Miller (2006a) and the present study, outlines procedures used to collect sample data, and describes the sample used for estimation of the statistical model. Chapter three discusses issues concerning the statistical modelling of response variables; profit earned for the firm, number of information requests, and time spent on the task. It also contains the results and discusses

their meaning. Chapter four ends with a summary and some comments regarding implications for future research.

Summary

This study extends work contained in Dobbs and Miller (2006a), examining the impact of financial incentives on the use made of information in decision-making. The previous work had failed to find significant support for the hypothesis that incentives improve the use made of information, a result at variance with the findings of a related experiment reported in Sprinkle (2000). Reviewing possible reasons for the discrepancy, the role of chance, of cohort effects, and of weaknesses of research design have all been considered. The present authors feel that, on the basis of chance alone, the Dobbs and Miller (2006a) finding should be considered more reliable than that of Sprinkle (2000), and that cohort effects do not appear to be material. Three potential problems in the research design employed in Dobbs and Miller (2006a), at least two of which affect the study by Sprinkle (2000), were discussed as possible explanations: a lack of salient incentives; a failure to fully communicate an over-complicated experimental environment to participants; and implementation of a procedure for 'inducing' in participants specific attitudes to risk, in a context that has subsequently been found to be inappropriate for that procedure. The present study simplifies the experimental environment used in Dobbs and Miller (2006a) and Sprinkle (2000), and explores the issue of saliency by examining a wider range of incentives. The study will contribute to understanding of the role of incentives and information in decision-making, provide evidence on the general importance to experimental method of careful design of participant reward levels, and develop statistical modelling pertinent to the experimental environment created in the three studies.

2 Implementation of the Experimental Environment

Introduction

To be able to reliably interpret experimental data on human behaviours, it is first necessary to document fully the experimental environment in which the behaviours are generated. Relevant features of the environment include recruitment of participants, operating procedures followed by instructors, tasks undertaken by participants, and arrangements for rewarding participants for their participation. These matters are the subject of the present chapter. A detailed account of the decision-making task required of participants is described first, followed by a discussion of certain properties of the available feedback information that made it useful for improving participants' decisions. The main treatment variables manipulated for this study were the two kinds of money incentives provided to participants; rewards for improved decision-making, and costs to accessing feedback information. They are discussed next, followed by a statement of recruitment and operating procedures. The use made of questionnaire analysis, to evaluate the integrity of sample observations, and to 'filter' data used in tests of the research hypotheses, is then explained. Various tables on sample characteristics are presented throughout the chapter. Since a number of features of the environment are unchanged from Dobbs and Miller (2006a), some abridged materials from that study are included at various places in this chapter.

Decision making task required of experimental participants

The decision-making task was based on Table 2.1.

Table 2.1 The Relationship between Participant Decisions and 'Profit' Outcomes

		Column choices											
		1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**
Rows	**1**	5	0	0	0	0	0	0	0	0	0	0	0
	2	5	5	0	0	0	0	0	0	0	0	0	0
	3	5	5	10	0	0	0	0	0	0	0	0	0
	4	5	5	10	20	0	0	0	0	0	0	0	0
	5	5	5	10	20	20	0	0	0	0	0	0	0
	6	5	5	10	20	20	30	0	0	0	0	0	0
	7	5	5	10	20	20	30	30	0	0	0	0	0
	8	5	5	10	20	20	30	30	30	0	0	0	0
	9	5	5	10	20	20	30	30	30	45	0	0	0
	10	5	5	10	20	20	30	30	30	45	45	0	0
	11	5	5	10	20	20	30	30	30	45	45	60	0
	12	5	5	10	20	20	30	30	30	45	45	60	60
	13	5	5	10	20	20	30	30	30	45	45	60	60
	14	5	5	10	20	20	30	30	30	45	45	60	60
	15	5	5	10	20	20	30	30	30	45	45	60	60
	16	5	5	10	20	20	30	30	30	45	45	60	60
	17	5	5	10	20	20	30	30	30	45	45	60	60
	18	5	5	10	20	20	30	30	30	45	45	60	60
	19	5	5	10	20	20	30	30	30	45	45	60	60
	20	5	5	10	20	20	30	30	30	45	45	60	60

Table 2.1 The Relationship between Participant Decisions and 'Profit' Outcomes (Continued)

		Column choices							
		13	14	15	16	17	18	19	20
Rows	1	0	0	0	0	0	0	0	0
	2	0	0	0	0	0	0	0	0
	3	0	0	0	0	0	0	0	0
	4	0	0	0	0	0	0	0	0
	5	0	0	0	0	0	0	0	0
	6	0	0	0	0	0	0	0	0
	7	0	0	0	0	0	0	0	0
	8	0	0	0	0	0	0	0	0
	9	0	0	0	0	0	0	0	0
	10	0	0	0	0	0	0	0	0
	11	0	0	0	0	0	0	0	0
	12	0	0	0	0	0	0	0	0
	13	60	0	0	0	0	0	0	0
	14	60	80	0	0	0	0	0	0
	15	60	80	80	0	0	0	0	0
	16	60	80	80	95	0	0	0	0
	17	60	80	80	95	95	0	0	0
	18	60	80	80	95	95	95	0	0
	19	60	80	80	95	95	95	100	0
	20	60	80	80	95	95	95	100	100

Both columns and rows in Table 2.1 are labelled 1-20. The numbers in the cells are the profit outcomes associated with particular combinations of row and column. For example, the profit associated with column 14 and row 19 is found, at the intersection of that column and row, to be 80. The rows used were randomly selected, by computer, and so were wholly beyond the control of the participant. Moreover, when a row was chosen for the participant, this choice of row was not disclosed to the participant. Following this choice of row by the computer, the participant was required to make a choice of column. Thus, in contrast to the row, choice of column was within the full control of a participant. Given this structure, whenever the participant chose a column, this decision was taken without the participant knowing for sure the profit

that would result. However, the participant was given Table 2.1 to work with, and was told that rows would be selected randomly by computer. Participants were informed that the computer had been programmed so that each row was equally likely to be selected, and also that the row for one participant was selected independently of the row selected for any other participant participating in the experiment at the same time, and independent of all the column choices they themselves had made up to that point in the experiment. The column headed 'Count – G0 (Trials 1-12)' in Table 2.2 gives the actual frequency with which each row occurred in the full experiment. The G0/G3 labelling of the columns refers to different partitions of the sample. G0 indicates the full sample and G3 is a sub-sample of participants who correctly answered all the questions in a post-experiment questionnaire. Likewise, since there were twelve trials making up the experiment, the first two columns of data also refer to different partitions of the sample; one for trials 1-12, the other for trials 7-12. The significance of each of these partitions will be discussed in more detail later in this chapter.

Table 2.2 Frequency counts of randomly generated rows

Row	Count – G0 (Trials 1-12)	Count – G0 (Trials 7:12)	Count – G3 (Trials 7:12)
1	45	20	9
2	56	24	10
3	66	35	15
4	68	34	15
5	62	33	11
6	64	35	21
7	46	27	7
8	40	19	9
9	55	28	10
10	59	27	14
11	46	21	11
12	40	18	6
13	57	31	15
14	72	30	10
15	55	29	11
16	59	29	16
17	72	29	16
18	55	38	18
19	54	29	13
20	57	28	15
Total	**1128**	**564**	**252**

As a check on the reliability of the computer program written to randomly assign rows to participants, a sample of 200 million independently-generated rows was produced and tested using the Chi-squared statistic. A null hypothesis that each row had the same probability of being randomly assigned could not be rejected at conventional significance levels; see appendix seven, Table A7.1.

Each of the twelve trials of the experiment was subdivided into five periods, making in all sixty periods. For each period of the trial, the participant had two kinds of decision to take. First, they were asked to select a column. Second, they were asked whether or not they wished to learn the profit associated jointly with their choice of column and the unknown row. They were given the objective of maximising total individual profit earned during the whole of the experiment.

The information contained in profit feedback

Each column of the profit function of Table 2.1, except column 1, has two possible profit outcomes; either zero profit or a positive level of profit. Moreover, the design of the profit function is such that, for any given column, these two possible profit levels partition that column into two contiguous non-overlapping sets of rows. For example, for column 13, rows 1 to 12 generate zero profit and rows 13 to 20 generate a profit of 60. Thus knowledge of the profit outcome provides additional information about the undisclosed row relative to knowledge of column choice alone. Suppose, as above, in the first period of the experiment a participant chose column 13. If, following this, the participant does not choose to learn the resulting profit outcome, then the uncertainty about the row selected will be the same as it was before the choice of column; that is the row could be any of rows 1 to 20. But if the participant does choose to learn the resulting profit outcome, and it turns out to be zero, then the participant for certain can rule out all of rows 13 to 20 as possibilities. By contrast, if the request for information revealed that profit was 60, then the participant for certain can rule out all of rows 1 to 12 as possibilities. Hence, whatever is reported, the participant can for certain learn, after this first period, something they did not know before. The only case where this is not true is for column 1, which has a constant profit of 5, whatever the selected row. In this one case, knowledge of the row does not give any information over and above

the column choice. Considering the 20 columns as a whole, the design provides the participant with a means of receiving either no information, in the case of column 1, or unambiguously better information concerning which row had been selected, in the case where any other column has been selected.

Although profit outcomes are clearly informative, it has not yet been demonstrated that this information has incremental value, in terms of maximising profits for the firm. The information would have value if it led to improved decisions, and that would be the case if past profits had some relation to future profits. Hence, the system for randomly generating row numbers for each participant was designed so that in any given trial the row selected was held constant for all five periods of that trial. A new row number was only generated at the beginning of each new trial, and then it was determined independently of the row numbers used in all previous trials. Participants were informed of these facts before the experiment commenced. As implemented, the features mean that whatever a participant learns from a profit report after the first column choice will reduce uncertainty about the undisclosed actual row they face for the remainder of the column choices required in that particular trial. Pursuing the previous hypothetical example a little further, if the first-period profit outcome following choice of column 13 was reported, on request, to the participant, and turned out to be zero, then the participant knows for certain that the same column choice or indeed any higher-numbered column will produce exactly the same zero profit for the remaining four periods of the trial. The participant wishing to maximise profits for the firm would then rationally choose a lower-numbered column in the second period. Even if the reported profit was 60, the knowledge still might provide extra profit to the participant, for it might encourage the participant to increase the column number chosen next period in the search for even higher profits: particularly since the participant can always 'retreat' back to column 13, should a higher-numbered column choice subsequently produce a zero profit.

It is also worth noting that there is never any decision-facilitating value to learning fifth-period profit, since at that point the trial is completed and the participant either finishes the experiment or moves onto a new trial, with a new independently generated row number. If accessing information is costly, requesting information at the end of the fifth period will always entail a value loss.

The personal costs and benefits to participants of maximising profits for the firm

In Dobbs and Miller (2006a) and Sprinkle (2000), there was evidence that, despite being provided with explicit disincentives against maximising profit for the firm, FR participants nevertheless, from 'private' motives such as getting satisfaction from a 'job well done', performed the decision-making task almost as well as PRR participants facing profit-related payments. It was concluded that the sufficient condition, stipulating that participants have no intrinsic preference towards performing the experimental task, either positive or negative, was not satisfied in either Dobbs and Miller (2006a) or in Sprinkle (2000) (see sufficient condition (b), discussed previously on page 3). Moreover, for the Dobbs and Miller (2006a) experiment, the average money consequences had been set at £0.008 per information request and, for PRR participants, £0.004 per unit of profit earned. It seemed that these levels were so low, they were not salient enough to dominate intrinsic preferences, resulting in a failure to find any significant role for performance-related pay in determining behaviour and performance. Although, after allowing for the effects of inflation and converting to a common currency, the average payments in Sprinkle (2000) and Dobbs and Miller (2006a) were similar, their saliency cannot be assumed to be the same for UK and US students, given the different funding contexts faced by these groups.

In the present study, it was decided to re-examine the role for performance-related pay when participants were provided with a wider

range of incentives and disincentives, in order to explicitly assess whether saliency is indeed a factor affecting performance. The money reward to participants per unit of profit earned was therefore varied across participants, with five levels considered: £0.00, £0.005, £0.01, £0.015, £0.02. The money cost to participants per information request was also varied, with five levels: £0.01, £0.05, £0.10, £0.15, £0.20. To minimise the possibility participants might end up with negative money earnings from the experiment, the upper bound for the variation of cost per information request was reduced when money reward per unit of profit earned was small. When the money reward per unit of profit was zero, participants were given a fixed money reward for each column choice made, independent of profit earned. With these ranges of rewards and costs, average participant earnings from the experiment turned out at £19.50, compared to an average of £9.85 in the earlier Dobbs and Miller (2006a) study. Panel A, headed 'Count – G0', in Table 2.3 below shows the sample frequencies of each assigned combination of profit reward and information cost for all 94 participants in the experiment. In Panel A of Table 2.3, for example, 6 participants faced the reward structure of £0.05 Cost Per Information Request and £0.01 Reward Per Profit Point.

Table 2.3 Frequency of participants with given reward/cost incentive conditions

Panel A: Count – G0 (The entire sample)

		Reward per profit point					Totals
		£0.000	£0.005	£0.010	£0.015	£0.020	
Cost per information request	£0.01	4	5	6	4	4	23
	£0.05	7	6	6	4	4	27
	£0.10	7	-	4	5	3	19
	£0.15	4	-	-	5	5	14
	£0.20	6	-	-	-	5	11
Totals		28	11	16	18	21	94

Panel B: Count – G3 (The sub-sample of participants who completed correctly the post-experiment questionnaire)

		Reward per profit point					Totals
		£0.000	£0.005	£0.010	£0.015	£0.020	
Cost per information request	£0.01	2	2	3	1	2	10
	£0.05	6	2	3	2	0	13
	£0.10	0	-	2	3	1	6
	£0.15	3	-	-	2	2	7
	£0.20	3	-	-	-	3	6
Totals		14	4	8	8	8	42

As mentioned above the G0/G3 labelling of Panels A and B refers to different partitions of the sample. G0 refers to the full sample and G3 to a sub-sample of participants who answered all questions on a post experiment questionnaire correctly; the relevance of the G3 group is discussed later. The aim was to allocate roughly the same number of participants to each incentive parameter set; some variation in cell frequency occurs in Panel A because of essentially ad hoc reasons; for example, if a PC in the cluster was malfunctioning, the parameter set allocated to that machine would not get used.

There were two other changes in the arrangements for rewarding participants, compared to Dobbs and Miller (2006a) and Sprinkle (2000). As discussed in Dobbs and Miller (2006a), and chapter one of the present study, there was evidence in Dobbs and Miller (2006a) that many participants failed to understand the experimental environment they faced, particularly the true cost of taking time to complete the task and the manner in which their incentives were linked to a lottery procedure, meant to induce neutral attitudes towards risk. This evidence appeared to suggest violation of the sufficient conditions (discussed on page 3) which respectively stipulate that participants fully understand the experimental environment they face, and that they possess a certain facility handling probability data. In order to simplify the environment and improve the clarity of instructions, it was therefore decided to abandon both the lottery procedure and the pecuniary costs of time spent on the task. In the new sessions, rewards were denominated directly in money rather than lottery tickets, and the cost of time spent on the task was fixed at zero.

It is possible to argue that, in the absence of specific controls for variation across participants in their risk attitudes, the results of the present study are confounded. However, it can be argued that the variation in money payments for this experiment is not sufficiently large for risk attitudes to play a significant part in determining behaviour, particularly as there are no negative rewards, and the range of positive rewards is fairly modest. That is, it may be possible to characterise each participant's attitude to risk as approximately 'neutral', as long as random variation in rewards is sufficiently small; see Arrow (1970) and Rabin (2000) for arguments along these lines. As for the cost of time spent, at £0.001 per second in Dobbs and Miller (2006a), it was in any case at most a trivial addition to any pre-existing intrinsic motivation felt by participants towards completing the experiment as quickly as possible. Indeed, Sprinkle (2000) reported that his results were robust with respect to the presence or absence of time-based opportunity costs.

Recruitment and operating procedures

Four experimental sessions were conducted at the University of Newcastle upon Tyne during the spring and early summer of 2006. A total of 94 participants provided potentially useable data. Volunteer participants were drawn from various degree programmes involving business, economics and accounting, with a mix of undergraduate, postgraduate, overseas, domestic, male and female students. Two sessions exclusively involved undergraduate participants, and two sessions were exclusively made up of postgraduates. All those available for the session dates and times were accepted for participation. Retrospectively, it was discovered that one participant had actually taken part in the Dobbs and Miller (2006a) laboratory experiment, as part of a previous experiment. This participant therefore had experience with the decision-making task. However, he had not been debriefed about either the hypothesis examined in Dobbs and Miller (2006a) or in the subsequent experiments reported in the current study. Although not reported here, the analysis presented below was conducted both with this individual included and with the individual excluded, with there being no material difference in results.

Volunteers were randomly assigned to incentive conditions, except for the set of conditions without profit-related rewards. These conditions were assigned only in the first session, involving a proportion of the accounting undergraduates; all participants in this session faced a fixed money reward for each column choice made, regardless of profit performance, with information costs systematically varied over the full range.

The column headed 'G0' in Table 2.4 presents descriptive information concerning the full sample of volunteers, whilst that headed G3, gives this information for the sub-sample of participants who answered a post-experiment questionnaire wholly correctly; as previously remarked, the significance of this group is discussed below.

Table 2.4 Sample descriptive statistics

Participants	G0	G3
% Male	56	60
% Postgraduate	35	14
Average grade (%)	61	63
Average age (Years)	22	21
% British	62	88
% Non-British European	7	2
% Asian	22	10
% Other	7	0

Some comments are perhaps worth making on the descriptive statistics reported in Table 2.4. The G0 figures indicate the composition of the overall cohort of participants. By contrast, the G3 group are a sub-set of the G0 group, consisting of individuals who answered all questions in the post experiment questionnaire correctly. The G3 group features proportionately more undergraduates and fewer postgraduates, and proportionately more British and fewer overseas students. The G3 group also has a slightly higher GPA and are on average, slightly younger. The impact of individual characteristics on performance is discussed in more detail in chapter three below.

For each session, an instructor attended one of the target group's lectures, handed round copies of a 'Personal Details Form', and asked for volunteers to participate in the experiment. The 'Personal Details Form' provided details on gender, nationality, age, degree course and contact email for those wishing to volunteer, as well as a consent declaration allowing the researchers to use data on end-of-year grades in subsequent statistical analysis. One end of year grade was not available because the student did not attend the examinations; the grade for this participant was set equal to the average for the remaining participants. The conclusions reported here were not affected by inclusion or exclusion of this participant in the sample. The 'Personal Details Form' is included

here as appendix one. Appendix two presents a typical email used to inform participants of the arrangements for sessions. Due to physical capacity limitations in the computer cluster used for the experiments, for three of the sessions, volunteers were split into two groups. However, the starting times for the two groups overlapped in such a way as to ensure each group was isolated from the other until the experiment was complete. Each group first received instructions in a classroom, then moved to the computer cluster, where each member of the group sat at an individual terminal to undertake the experiment.

In the classroom, an instructor read out, word for word from a pre-prepared document, instructions implementing the experimental environment. Participants could follow the instructions from their own individual hard copies, provided just before the session began but only after initial rules of communication were explained. These instructions appear here as appendix three, for participants without profit-related rewards; and appendix four, for participants with profit-related rewards. The instructions were drafted and the sessions arranged so that no participant knew the variation in treatment faced by any other participant, and no participant had information about hypotheses under examination; the instructions in appendices three and four carried only a neutral heading, without an indication of treatment, and the different treatments were disclosed only when participants were physically separated at computer terminals and about to begin the experiment. Hard copies of Table 2.1 were also distributed to all participants.

A computer program was specially written to create the experimental environment. A selection of screen shots is reproduced in appendix five to illustrate what each participant with profit-related rewards would have seen on the screen at various points during the experiment. Screen shots were broadly similar for the group without profit-related rewards, and so have not been reported.

Post-experiment questionnaire analysis

Evidence concerning each participant's understanding of the experimental environment was provided by a questionnaire, which had to be completed by every participant after they had finished the experiment. The questionnaire, adapted from Dobbs and Miller (2006a) to take account of the simplifications introduced into the present study, is included as appendix six. Deficiencies of understanding represent a breach of sufficient condition a), from chapter one of the present study. To ignore evidence of this problem and proceed by pooling all response data, without adjusting for different levels of understanding of the experimental environment, risks contaminating subsequent statistical analysis. To put the issue at its most extreme, response data derived from participants with no conception whatsoever of the experimental environment cannot be attributed to that environment; such participants were responding to something other than the created experimental environment.

Some studies attempt to avoid the problem of imperfections in understanding by means of compulsory ex-ante testing of individuals, followed as required by reinforcement of instructions; see, for example Fisher *et al.* (2002) and Fisher *et al.* (2003). Typically, details of the test are not included in published documentation accompanying the results, making it difficult to assess the extent to which the procedure is effective, either in particular cases or more generally. Other studies have relied on ex-ante opportunities for individuals to pose their own questions to an instructor; see Sprinkle (2000) and Plott (1982). The conventional wisdom is that all questions about the experimental environment should be met with repetitions of the relevant portions of the written instructions, thus reducing the possibility of an instructor being drawn into unwittingly revealing clues about expected or desired responses from participants, particularly using unscripted remarks that may then be difficult to document and therefore replicate (see Rosenthal, 1963). The

ability or confidence of confused participants to frame suitable questions, either in private instruction or openly during group instruction, is open to doubt, however, as is the general effectiveness of an instructor who responds to questions only by repeating the very instructions that gave rise to the participant's confusion in the first place.

The approach in the present study and in Dobbs and Miller (2006a) was to strictly confine ex-ante instruction to what could be documented, here reproduced as appendices three and four, so that the environment was recorded as fully as possible, but then to test participant understanding ex-post, when particular questions could not influence responses during the experiment, either by provision of focal points or otherwise. This approach has recently been employed by Davis *et al.* (2006) who, on the basis of an ex post test of participant understanding, subsequently reported results that excluded 6% of their sample data; although they reported that their findings were not affected if the results were based on the full sample. Hence, in the present study, prior to analysing the data generated during the experiment, an attempt was made to develop a taxonomy of questionnaire responses, with the resulting classifications then being used to immunise the body of data against potential bias arising from breaches of sufficient condition a). The method adopted and its use in the data analysis is described next.

The post-experiment questionnaire consisted of five questions. Questions 1 and 2 tested understanding of aspects of the profit table; respectively, whether a participant could correctly ascertain profit from a given row and column, and could infer the range of unobserved rows consistent with both a given column and profit report. A total of 70 participants correctly answered both questions 1 and 2. This group was assigned a code of 'G1', whereas the full sample was coded 'G0'. Question 5 tested understanding of the individual cost to a participant of accessing feedback information. A total of 62 participants answered all of questions 1, 2 and 5 correctly. This group, a proper subset of group 'G1', was coded 'G2'. Finally, questions 3 and 4 dealt with individual participant rewards and their relation, if any, to earned profit. Only 42

participants correctly answered all of questions 1 through 5. This group, a proper subset of group 'G2', was coded 'G3'. Hence the participants were 'filtered' into 4 nested groups, 'G0', 'G1', 'G2' and 'G3'. The classification scheme is summarised in Table 2.5.

Table 2.5 Classification of participants according to responses to questionnaire

Code	Questions correctly answered	Sub-sample size
G0	-	94
G1	1, 2	70
G2	1, 2, 5	62
G3	1, 2, 3, 4, 5	42

If membership of 'G3' had been the sole test of adequate understanding, implementation of the approach adopted in Davis *et al*. (2006), excluding participants from the analysis when they did not understand the experimental environment, would have resulted in a wastage rate for the current study of 55% of the sample. The more complex classification scheme in Table 2.5 was applied in order to avoid such a high level of wastage, allowing potentially heterogeneous response models, according to the four levels of understanding, to be distinguished from one another using standard statistical techniques. Of course, it is possible to conceive of many other classification schemes; for instance, with five questions it would have been possible to divide participants into 32 separate non-nested classifications. But a proliferation of groups would have resulted in uneconomically large estimation demands on the available data, or produced models that proved difficult to interpret. The idea behind the nested classifications thus adopted was to order the participants in terms of increasing understanding. The main results presented in this study are for the 'G3' group, in which every participant answered all five questions of the questionnaire correctly. Table 2.2 and Panel B of Table 2.3 give sample frequencies for the assignment of profit

table rows and incentive conditions to participants in the 'G3' group, whilst Table 2.4 presents the corresponding descriptive statistics. Note the final two columns of Table 2.2 indicate profit table row frequencies only for the final six trials, 7 to 12, of the experiment; a point to be discussed in chapter three.

Classification of participant understanding by means of a questionnaire is not claimed to be perfect, for assessment was based upon a small number of questions as well as subjective appraisal of the significance of apparent error. Moreover, completion of the questionnaire was not linked to any money consequences for participants, so the extent to which it was taken seriously, apart from through a desire to co-operate and to display achievement, might be doubted. A conservative approach to analysing the questionnaires was adopted, whereby all errors were treated identically, as failures to understand; this somewhat robust approach results in maximum loss of participants from the 'G3' group. If questionnaire errors have been incorrectly assessed, it is therefore more likely to have resulted in inappropriate exclusion rather than inappropriate inclusion. Hence the consequence is likely to be loss of efficiency, due to smaller sample size, in estimating the statistical model for 'G3' participants, rather than bias arising from inappropriate pooling of heterogeneous participants. That is, tests of research hypotheses would be unbiased but have less statistical power for detecting significant results. This outcome constitutes a potential problem only in the absence of significant support for the research hypotheses. From the point of view of inference to the general population, selective use of data also means that generalisation of the results in this study is limited to that part of the population capable, after some experience, of understanding the decision task and incentives.

Summary

In this chapter and its appendices, the experimental environment created for participants, and also the recruitment and operating procedures performed by the experimental instructors, have both been described. The cognitive task required of participants involved a sequence of decisions, where quality of decision-making could be significantly improved by use of feedback information on past profits earned. The treatment variable randomly assigned to participants was the combination of participant reward per unit of profit earned and participant cost per request for feedback. The use, prior to performing statistical analysis, made of a post-experiment questionnaire for classifying participants by their levels of understanding of the experimental environment was discussed in some detail. This classification scheme permitted an ex-ante evidence-based analysis of data integrity, leading to exclusion of data of doubtful relevance to the research hypotheses. To the best of the authors' knowledge, such analysis is a novel feature for experimental work with complex multi-dimensional environments, and serves as an alternative to other existing approaches to the problem of participant understanding of experimental environment.

3 Statistical Analysis, Results and Conclusions

Introduction

This chapter explains and discusses the statistical analysis undertaken on the experimental data, and presents results concerning the main research questions. The chapter begins with preliminary descriptive analysis of participants' decision-making; this analysis reveals some sophistication in participants' recognition of 'rational' decisions. A statistical modelling approach is then used to test the effect of incentives on profit performance. The approach is described in some detail, compared with the approach adopted in both Dobbs and Miller (2006a) and Sprinkle (2000), and issues affecting its validity are discussed. Following this, the main research hypotheses are presented and a testing procedure outlined. The findings of these tests suggest a significant role, in the use of information and in decision-making performance, for profit-related rewards. What might seem somewhat surprising, however, is that, although the costs of accessing information significantly reduce participants' use of feedback information in decision-making, the effect on profit performance is not significantly different from zero.

Preliminary analysis of participants' decision making

Evidence on the degree of 'rationality' exercised by participants in their actual column decisions can be examined in a number of ways. One approach, relying on the notion of 'dominated' columns, an analysis previously undertaken in both Dobbs and Miller (2006a) and Sprinkle (2000), is presented next. Reference to Table 2.1 reveals that eleven of

the twenty columns available for selection by participants are 'dominated' by adjacent columns, in the sense that each 'dominated' column offers the same potential profit as the column to its left, but for fewer rows, and therefore less probability. Columns 2, 5, 7, 8, 10, 12, 13, 15, 17, 18 and 20 are all 'dominated'. An individual who chooses columns at random would, on average, choose a 'not-dominated' column 9/20=0.45 of the time. By contrast, a participant who recognises this feature of the profit table, and is motivated to behave perfectly rationally, would choose 'not-dominated' columns with probability 1. It follows that the proportion of 'not-dominated' columns actually selected can be viewed as an indicator of comprehension and/or rationality. Panel A of Table 3.1 below presents the sample proportions observed in the present study, by both group coding and experience with the experiment, whilst panel B focuses on trials 7-12 and compares the results obtained in this study with those from Sprinkle (2000) and Dobbs and Miller (2006a). Note that periods in which participants were timed out without making a column choice are not included, nor are the column decisions of participants without profit-related rewards; such participants are rewarded for any column selected, whether or not 'dominated'. The probabilities of observing each sample proportion, when in fact participants choose columns randomly, are shown in parentheses below each proportion. Appendix seven discusses the calculation of these probability values.

Table 3.1 Proportions of selected columns passing the rationality test

Panel A: Present study; by group coding and experience (probability values in parentheses)

	Trials 1-6	Trials 7-12	Trials 1-12
G0	0.64 (0.00)	0.70 (0.00)	0.67 (0.00)
G3	0.70 (0.00)	0.78 (0.00)	0.74 (0.00)

Panel B: Comparative results across studies, for Trials 7-12 (probability values in parentheses)

	Present study: G3	Present study: G0	Dobbs and Miller (2006a)	Sprinkle (2000)
Trials 7-12	0.78 (0.00)	0.70 (0.00)	0.57 (0.00)	0.81 (0.00)

To interpret Table 3.1 panel A, for PRR participants in the G0 group for example, 67% of all column choices made, over all trials 1-12, were 'not-dominated' in the sense described above. The chance of this happening if these individuals were choosing columns randomly is given in parentheses as less than 1% (i.e. 0.00 to two decimal places). For PRR participants in the G3 group who answered the post experiment questionnaire entirely correctly, over trials 7-12 for example, 78% of all column choices were not-dominated, with again the chance of this happening 'by chance', virtually zero. Panel B of the table compares the performances of the G0 and G3 groups in the present study with those in Dobbs and Miller (2006a) and Sprinkle (2000).

Table 3.1 shows not only that the proportion of 'not-dominated' columns selected is significantly higher than the 0.45 that would arise under pure random choice, but also that there is both a trial and a post-experiment questionnaire effect. That is, as participants proceed through the experiment, and as they are sorted into higher-numbered groups via their performance in the questionnaire, their score on 'comprehension/rationality' improves. Notice also, in Panel B, that the Dobbs and Miller (2006a) proportion of 'not-dominated' columns compares unfavourably with both the G0 and G3 groups of the present study and with Sprinkle (2000), reinforcing the idea that the weak Dobbs and Miller (2006a) results were due, at least in part, to lack of comprehension of the experimental environment. The extent to which the participants in Sprinkle (2000) were 'trained up' to behave rationally, through instructors' answers to their pre-experiment questions is a moot point, since this aspect of the Sprinkle (2000) environment is not fully documented.

A model of the process generating the experimental data

The validity of any statistical hypothesis test rests on the descriptive power of its underlying assumptions about the nature of the data used; in general, the more powerful is the test, in discriminating between competing hypotheses, the stronger the underlying data assumptions need to be for that test to be validly applied. Hence, it is useful to establish a plausible account of the general process governing data generation before deciding on the particular tests to be applied in any specific context. This notion of first characterising the data generating process was glossed over somewhat in Sprinkle (2000), and in the Dobbs and Miller (2006a) replication of Sprinkle (2000). The current study makes an initial effort to fill the gap and provide a more concrete basis for procedures by which the research hypotheses are tested.

In what follows, three response variables are studied: specifically these are:

i) Actual profits earned for the firm by a participant;

ii) The number of information requests made by a participant; and

iii) The time taken to complete the task.

Given the structure of the profit table, Table 2.1, it is clear that trial profit is determined solely by the row randomly selected by the computer, indexed by '*row*', and the five column decisions made by a participant. In turn, the research hypotheses postulate that a participant's column decisions will be influenced by the personal financial incentives faced. These financial incentives are described by two variables; the participant's financial *reward per unit of profit earned* for the firm, and the financial *cost incurred per information request*. An individual participant, faced with these incentives, will adopt some strategy for selecting columns. This strategy may differ across individuals, according to various personal characteristics such as age (*age*), gender (*mf*), graduate status (*upg*), nationality (*nat*) and intellectual ability, the latter proxied by the most recent grade point average (*gpa*). A participant's strategy may also change during the course of the experiment as experience, indexed by trial number, is gained with the decision-making task. Adding a random term, denoted *u*, to account for variables not specifically articulated in the model, it is possible then to specify a 'reduced form' regression model for each of the above response variables. For actual profits earned for the firm by a participant, the model is given by equation 3.1, where *f* denotes the idea that earned profit 'is a function of' the explanatory variables in parenthesis:

Equation 3.1

Profit earned = f (row, reward per unit profit earned, cost per information request, age, mf, upg, nat, gpa, trial) + u

Equation 3.1 as yet has an unspecified functional form; however, in the basic modelling approach discussed in appendix seven, a simple linear functional form is adopted. The same set of explanatory variables is then used in the models for the other two response variables.

It is possible to simplify this general framework along the lines undertaken in previous work by Sprinkle (2000) and Dobbs and Miller (2006a). In those studies, there were just two treatments to be compared, and this meant it was feasible to divide up the sample by profit table row or trial, and then conduct pairwise tests of the main research hypotheses. In the present study, however, Panel B of Table 2.3 shows seventeen different treatments for the G3 group: this makes a partitioning of the sample into profit table rows or trials before conducting pairwise tests covering all the seventeen treatments both impractical and uneconomic, given the sample size available. Instead, therefore, the approach adopted in the present study is to pool all the 'G3-coded' data into a single estimation equation and then conduct a single test of each of the main research hypotheses. An extended discussion of issues concerning the level of aggregation can be found in appendix seven. Specifically, the profit response variable is taken as the total profit earned for the firm by a participant over trials 7-12 of the experiment. The effect on equation 3.1 is to remove trial as a determinant of the profit response variable, leaving a pure cross-section of aggregated profit observations to be explained by the eight remaining variables. Aggregation over trials also effectively sidesteps discreteness and potential non-normality issues discussed in appendix seven. The appendix also shows that when trials 7-12 are aggregated, instead of '*row*' being a determinant, the appropriate determinant becomes a *weighted row average*; this is computed as the average of the frequencies of rows occurring in trials 7-12, with weights equal to row numbers. Table 3.2 below gives some information on the key variables involved.

Table 3.2 Range of values for key variables

Definition	Range of values
Response variables	
Aggregate profit earned for the firm by participant in Trials 7-12	0-3000
Number of information requests made by participant in Trials 7-12	0-30
Total time (seconds) taken by participant to complete tasks in Trials 7-12	0-1080
Determinants	
Weighted sum of profit table row numbers faced by participant in trials 7-12	6-120
Money reward paid to participants per unit of profit earned (pounds)	0-0.02
Money cost to participants per information request (pounds)	0.01-0.20
Age of participant (years)	18 upwards
Gender of Participant (male or female)	Female: $mf = 1$ Male: $mf = 0$
Graduate status of participant (undergraduate or postgraduate)	Postgraduate: $upg = 1$ Undergraduate: $upg = 0$
Nationality group of participant (British, non-British European, Asian or other)	British: $nat2=nat3=nat4=0$ European non-British: $nat2=1$, $nat3=nat4=0$ Asian: $nat3=1$, $nat2=nat4=0$ Other: $nat4=1$, $nat2=nat3=0$
Most recent grade point average (per cent)	0-100

The main research hypotheses predict that, other things being equal, increasing a participant's money reward per unit of profit earned will increase time spent on the task, increase the number of information requests made, and increase profit performance for the firm. By contrast, increasing a participant's money cost per information request will reduce time spent on the task, reduce the number of information requests made and reduce profit performance. For all other variables, there are no particular expectations concerning their impacts, except in respect to profit earned, where weighted row average and grade point average are both expected to have positive impact; the former because a higher average row number in the trials faced by the individual can be expected to translate into higher profit performance; the latter because higher *GPA* might be expected to translate into a higher level of understanding of the structure of the problem.

Results

A standard 'general to specific' methodology is employed, in which all the variables are considered as explanatory variables for profit performance, followed by a process of 'testing down' in which variables which have limited explanatory power are sequentially dropped from the specification.

Table 3.3 gives the results and, where appropriate, the probability values in parentheses below each estimate that indicate the probability of obtaining each non-zero estimate purely by chance. The probability values are one-tailed when the directional impact is predicted by the research hypothesis; otherwise they are two-tailed. The lower the probability value, the more likely it is that the research hypothesis is true. For easy reference, significant results are indicated by asterisks, with strengths *, **, ***, denoting probability values less than 0.1, 0.05 and 0.01 respectively (10%, 5% and 1% significance levels).

The first column of Table 3.3 gives the variable definitions, column two gives the results for the estimation of the relation between these

explanatory variables and 'Profits earned for the firm', column three for the relationship between these variables and 'total information requests', and column four for the relationship between these variables and 'time taken'. For the latter variable, the logarithm of time taken was used as the explanatory variable rather than simply 'time taken' primarily for statistical reasons, as discussed in appendix seven. It is perhaps worth emphasising that the primary interest lies in column two, as it is concerned with the direct impact of the variables on the profits earned by the firm. Column three is of some indirect interest, since it reveals the extent to which individuals appear to behave rationally or not, in response to changes in incentive pay and in the cost of information. The final column which is concerned with time is of more tangential interest, as the experiment did not focus on 'how fast' individuals completed the task in hand, and participants were not rewarded at all for completing the task more quickly.

Other things equal, the average 'Profits earned for the firm' increases by 13,438 for every £1 increase in the rate of incentive pay. An increase of £1 in the cost of an information request decreases the profit earned by, on average, 386.76.

Table 3.3 Initial estimates of profit, number of information requests, and time equations

Explanatory variables	Profits earned for the firm	Total information requests	Log time taken
Constant	-48.56	19.25	4.50
Weighted Row Average	19.53 (0.00)***	-0.11 (0.19)	0.01 (0.06)*
Participant earnings per unit of firm profit	13,438.60 (0.01)***	453.61 (0.00)***	-5.18 (0.823)
Cost per information request	-386.76 (0.28)	-29.46 (0.05)**	0.19 (0.61)
age (in years)	-8.23 (0.57)	0.13 (0.74)	0.02 (0.20)
mf (Male=0,Female=1)	-124.44 (0.20)	-1.83 (0.47)	-0.19 (0.05)**
upg (undergrad=0, postgrad=1)	103.72 (0.57)	0.36 (0.94)	0.24 (0.18)
nat2 (non-British European)	-190.74 (0.49)	-4.83 (0.50)	0.19 (0.48)
nat3 (Asian)	-132.84 (0.48)	9.32 (0.06)*	0.04 (0.82)
nat4 (Other)	0.00 (1.00)	0.00 (1.00)	0.00 (1.00)
gpa (grade point average)	-0.69 (0.54)	-0.07 (0.67)	0.00 (0.49)

Focussing on the probability values in parentheses, the smaller these are, loosely speaking, the more significant the variable is in explaining the response variable. Thus in column two for example, the estimate for participant earnings has the only small probability value, apart from 'row', indicating that it is the only variable that explains profitability apart from 'row'. This suggests that an individual's earnings does impact on the profit earned by a company. By contrast, column three shows that information requested is influenced by earnings, the cost of obtaining that information, and to a lesser extent, whether an individual is Asian or not. The final column shows that the main influence on the time taken to do the experiment relates to the gender of the individual.

A standard 'testing down' procedure, discussed further in appendix seven, involves sequentially dropping insignificant variables in order of their level of 'insignificance', as measured by probability value, and re-estimating the relevant equation. The most important factors then become earnings, cost of information and male/female, and just these three variables are then used. Thus, in the table 3.4 regression, *nat2*, *nat3*, *nat4*, *upg*, *gpa* and *age* have been dropped using this procedure, as they do not offer any significant explanatory effect. Probability values of each estimate are again included in parentheses underneath the appropriate estimates, with asterisks indicating the strength of the results. The final column gives the expected sign of the effect. Thus, for example, in Table 3.4, the expected effect of increasing the rate of incentive pay on the profits earned for the firm is expected to be positive (+), whilst an increase in the cost of information is expected to have a negative effect (-). Where there is no prior rationale for expecting a positive or a negative effect, this is indicated with a question mark (?).

Table 3.4 Regression results for profits earned for the firm

Response variable: *Profits earned for the firm*		
Explanatory variables	**Estimated impact on profits earned for the firm**	**Hypothesis concerning expected impact on profits earned for the firm**
Constant	-237.33	?
Weighted Row Average	19.20 (0.00)***	+
Participant earnings per unit of firm profit	14,855 (0.00)***	+
Cost per information request	-533.64 (0.19)	-
mf	-145.78 (0.06)*	?

The table shows that there is a negative effect of substituting a female participant for a male participant; it lowers expected profit in trials 7-12 by about 145 units of profit, or about 24 units of profit per trial, a finding similar to the corresponding result of 20 in Dobbs and Miller (2006a). The main result, however, is the strongly significant effect of increasing the level of participant earnings per unit of firm profit earned, confirming the main research hypothesis. The 14,855 marginal effect is equivalent to 12 extra units of profit per trial *for every halfpenny increase* in incentive pay rate, a substantial amount of leverage achieved through increasing the rate of payment by results.

Table 3.5 Regression results for total information requests made

Response variable: *Total information requests made*		
Explanatory variables	**Estimated impact on total information requests**	**Predicted impact on total information requests**
Constant	10.08	?
Participant earnings per unit of firm profit	465.12 (0.00)***	+
Cost per information request	-41.75 (0.00)***	-
nat3	9.32 (0.00)***	?

Table 3.5 derives from the regression reported in Table 3.3 column three, dropping non-significant variables, thus *nat2*, *nat4*, *upg*, *gpa*, *age*, *mf* have been dropped as they do not offer any significant explanatory effect. The regression in Table 3.5 shows that, as determinants of the number of information requests made, there are three significant explanatory variables; the incentive pay variable, the cost of information, and being Asian or not (*nat3)*. Thus, increasing the money reward from earning profit increases the frequency with which information is accessed, and increasing the money cost per information request reduces the frequency with which information is accessed. There is also a tendency, other things equal, for participants with an Asian nationality (*nat3*) to use information more frequently.

Table 3.6 Regression results for log total time taken per experiment

Response variable: *Log total time taken*		
Explanatory variables	**Estimated impact on log total time taken**	**Predicted impact on log total time taken**
Constant	4.78	?
Weighted Row Average	0.01 (0.03)*	?
age	0.02 (0.20)	?
mf	-0.18 (0.02)*	?
upg	0.22 (0.16)	?

Table 3.6 derives from the regression reported in Table 3.3 column four, where *nat2*, *nat3*, *nat4*, *gpa* have been dropped. The regression in Table 3.6 shows that, as determinants of log time taken, there are 4 significant explanatory variables, namely the *weighted row average, age, mf and upg*. The two determinants, *age* and *upg*, have been retained in the log time regression, despite their apparent lack of significance. Dropping either determinant leads to the retained determinant becoming strongly significant, so it is suspected that both determinants are proxying for the same underlying factor; perhaps 'maturity' (see also Table A7.4 in appendix seven). Significantly, neither money incentive nor cost per information request appears to influence time spent.

4 Conclusions and Suggestions for Further Analysis

Introduction

The capacity to replicate research findings is fundamental to scientific enquiry. Dobbs and Miller (2006a) had attempted to replicate earlier published results by Sprinkle (2000) demonstrating, in a laboratory experiment, the importance of profit-related incentives in the use of information and in the quality of decision-making, issues of considerable importance to managerial accounting. However, although Dobbs and Miller (2006a) obtained similar results to Sprinkle (2000), their results were not statistically significant. The authors speculated that the complexity of the experimental environment, as well as the perhaps inadequate incentives provided to participants, reduced the effectiveness of the research design. Furthermore, they identified some statistical issues that merited more attention than had previously been afforded. Each of these matters is addressed in the current report.

Policy implications

The experimental environment adopted in the present study departed from the attempted replication in Dobbs and Miller (2006a) by reducing its complexity. Participant comprehension of the environment was measured after the experiment and assisted the researchers in partitioning the sample by level of comprehension and in performing different analysis of the resulting partitions. The results indicate that comprehension of the experiment materially affects the results; that is, those who understand the environment less well, are also less responsive to that environment.

The present study finds that real money incentives affect participant behaviour and performance in experiments, confirming the result in Sprinkle (2000). Thus, when information that is valuable for enhancing profit is also costly, its effective use by individuals can be increased if individuals are given, even quite modest, profit-related incentives. The saliency of individual money consequences appears to be important in driving this result. That is, even when the incentive pay is zero, and when there are positive costs associated with accessing information, participants still endeavour to use information *to some extent* to enhance profit; this arises perhaps in part because of an intrinsic interest in 'solving the problem' *per se*, and perhaps also in a spirit of co-operation with those running the experiment. However, performance is enhanced over and above this as the rate of profit-related pay is increased. Saliency may also interact with the complexity of the environment to affect behaviour. As yet no convincing explanation can be offered for a curious result in the present study that whilst, as one would expect, increasing the cost of information leads to significantly less use of that information, this reduced use does not in turn significantly lower profit performance. It should be noted that the presence or absence of this latter effect could not be observed in the earlier studies, Sprinkle (2000) and Dobbs and Miller (2006a), for the simple reason that the cost of accessing information in those studies was a fixed datum that never varied.

The importance of the magnitude and form of money incentives goes beyond a business context to touch on the methodology of experimentation. The calibration of money incentives, provided in experiments to align participant preferences with underlying assumptions of a research hypothesis, is not a well-documented feature of published work. When participants have unobserved intrinsic preferences regarding the experimental task, then there are usually no compelling theoretical reasons for choosing one set of incentive levels over another. It follows that the appropriate levels to set for incentives in experiments in itself requires empirical investigation. As a general point, and in the interests

of research dissemination, details of the use of pilot sessions undertaken to inform calibration should be included with published evidence, both to avoid unnecessary duplication of effort, and to help in the evaluation of selected results drawn from a larger data set.

Some policy implications that could be drawn from this study are:

- Individuals that better understand the environment perform better. Thus, businesses need to ensure that their workforce understand the goals and missions of their organisation and that transparency concerning information and incentives will ensure better performance.
- Profit-related incentives or rewards related to performance can be used to increase motivation.
- Individuals perform better when they have an intrinsic interest in something or where they wish to please or gain a favourable impression.
- The cost of obtaining information significantly reduces the use of such information. Organisations need to ensure that their ERP system and the culture of their organisations ensure that information is disseminated swiftly and easily.
- The use of experiments can enhance business understanding. However, the precise nature of such experiments needs to be disclosed, such that readers understand exactly the nature of the study.

Future research

Within the experimental environment considered in this report, a number of additional questions could be explored. These include the extent of learning or experience effects from trial to trial, as well as within trials; the nature of 'rules of thumb' or heuristics used by experimental subjects to understand the connections between feedback information and future performance; and the impact, if any, of risk attitudes on subjects' use of reward-sensitive information. Such research would help to identify additional factors affecting the positive impact of incentives on information use and behaviour. There are also opportunities to extend the research beyond the particular environment employed here to investigate whether and how the role of incentives is changed when subjects are evaluated in teams, or in an agency context, or when the environment is framed in less abstract, more accounting-relevant terms. There is clearly great scope for additional policy-relevant research in this area.

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APPENDIX 1

Personal details form

Project No. RES0371 07209

(Sponsored by The Institute of Chartered Accountants of Scotland)

Personal Details

In order to undertake a statistical analysis of the results of the experiments, we need to know the following personal details:

Date: ..... Gender: M or F (Please circle as appropriate)

Full Name (please print): …….....

Degree Course: …….....

Age (in years): …….....

Nationality: …….....

Email Address: …….....

We intend to undertake a statistical analysis to determine whether the experimental data supplied by volunteers is related in any way to personal characteristics such as the above and also measures of academic attainment such as end-of-term examination results. Findings will be published only in anonymous form. In view of data protection requirements, we need you to sign to indicate your consent for this. Note that your signature is also required for you to be able to participate in the experiment.

Signature: ……

We will inform you by email when we have assigned you to a time and venue for the experiment.

APPENDIX 2

Email inviting volunteers to session

Notes for Participants in Experiments (those who completed Personal Details Forms at lecture…)

Thank you for agreeing to participate in these research experiments. They will take place on…. We have organised volunteers into two groups. Please look for your name in the lists below in order to find out which group you are in and the time you should arrive for the experiments.

Both groups should come to Room….. Please bring with you your smart card because we need to collect your student number for the purpose of accounting for payments. You might also bring a calculator. Everything else you need will be provided for you.

Please arrive promptly since the experiments cannot start until everyone in a given group is present.

If you completed a 'Personal Details' form at the lecture, but your name does not appear on either of the two lists, it is because you were unavailable at the times selected for the experiments.

We look forward to seeing you on…. and we hope you enjoy the experiment.

APPENDIX 3

Instructions for participants without profit related rewards

Materials

A copy of instructions (this document), a table of numbers, a pen, and paper are provided. Check you have these items now. If anything is missing please raise your hand and I will attend to you. These materials should be returned to the instructor supervising in the computer cluster at the end of the experiment.

Rules of Communication

Our purpose in conducting this experiment is to gain information about individual behaviour rather than group behaviour. For this reason, we must insist on the following rules concerning communication:

(a) No communication - there must be no verbal or other forms of communication between those taking part until everyone has completed the experiment,

(b) No questions – the only clarifications you can seek are those concerning the use of software in the PC room (if you have difficulty using the software, raise your hand and wait for assistance from the instructor).

Introduction

You will be taking part in a timed computerised experiment designed to investigate the nature of individual decision-making under conditions of uncertainty. You will be paid for your participation. Your earnings will depend upon how you perform the experimental task, and can be anything up to £12. Earnings will be calculated and reported to you today, privately – they are then paid in cash, privately, in a few days time. I will first explain the experimental task and then how you earn money from performing this task.

The Experimental Task

Please look at your profit table. The rows are labelled 1 to 20 in the left hand column. The columns are labelled 1 to 20 in the top row. In essence you will be playing a series of 'games' against the computer in which the computer first randomly picks a row, which you are unable to observe; and then you choose a column. The chosen row and column will determine, where they cross in the profit table, the profit earned.

To illustrate how the game works, suppose one of you plays 'the computer' [SELECT SOMEONE TO PLAY THIS ROLE – NAMED 'X' BELOW]. X should write down a row number on a piece of paper, without revealing it to the group, and then pass the piece of paper to me. Now, consider what happens when you make a choice of column. [ASK A DIFFERENT RESPONDENT 'Y' FOR A CHOICE OF COLUMN: THIS IS THEN ANNOUNCED SO EVERYONE CAN HEAR THE CHOICE]. The profit result is determined by the intersection of the row and column. In fact the row chosen was …. ('X' CONFIRMS THE CHOICE OF ROW). Hence the profit is ….(EXPLAIN THE RESULT VIA AN OVERHEAD).

The experiment is organised as follows:-

(a) There are 12 trials, for each of which there are 5 decision periods, altogether making 12 x 5=60 column decisions for you to take. You identify your column choice each time by inputting your chosen column's number at your computer terminal when prompted. The number of the column you want to choose can be found in the top row of the profit table

(b) For each trial, the computer will select a row, which is then FIXED for all periods in the trial.

(c) You then choose a column in period 1 of this trial.

(d) The computer will ask you whether or not you wish to learn the profit earned by your choice in (c) above. If you agree, it will be reported immediately (but note there is a cost associated with this - see below). If you do not, the profit earned will not be reported until the end of the trial.

(e) The process in (c) and (d) is then repeated for periods 2,..,5 of the trial.

(f) At the end of 5 periods, the profits earned in each period are revealed to you without cost (see information costs below), and your money earnings are calculated and also revealed.

(g) The process now starts again with a new trial – the computer again chooses a row at random which is then fixed during the trial, and you repeat the process (c)-(f), (and so on, until all trials have been completed)

<u>In taking part in the experiment, we would like you to try to earn as much profit as you can</u>.

Notice, in the profit table that, since the chosen row will not be known to you when you select your column, you will generally not know for certain the profit that will be earned by your column choice. However, you *do* know that the row is fixed for the 5 periods of each trial, so that whenever you choose a column, not only does your choice determine profit, jointly with the selected row, but if you make an information request, it also provides information about the unobserved row being used in the current trial. Look again at the profit table. ['Y' who previously chose the column] chose column … If an information request is made, it would be discovered that profit from the decision was …. And one can reason that the computer has selected a row between row… and row… [ILLUSTRATE THIS ON OVERHEAD] With this knowledge, one may (or may not) wish to change the choice of column in the next period (and so on).

Additionally, the following information is available about how the computer has been programmed:

- The rows 1 to 20 are all equally likely to be randomly selected by the computer.
- The rows selected for you by the computer in a given trial will generally vary randomly from trial to trial, and will also differ across other individuals taking part in the experiment.
- The selection of a row by the computer in each trial is not affected by your choices of columns in the current or previous trials.

Money Earnings

Your earnings are (i) positively related to the number of column choices you make, irrespective of the profits these column choices earn, and (ii) negatively related to the number of times you request profit information

during a trial. The money gain per column choice, and the money loss per information request will be notified to you at the start of the experiment.

You have 180 seconds to complete the five column choices expected in each trial. If you run out of time before completing the five column choices, you will only be paid for those you actually complete. The clock will start anew at the beginning of each trial. Whether or not you requested information on earned profits *during* each trial, you will also be informed (at no extra cost) of the total profits earned at the end of each trial

We recommend that, before you begin the experiment, you spend some time examining the structure of the profit table. Do not be influenced by the pace at which other respondents work, whether slower or faster than you. Your money rewards will be based solely on *your* performance of the task and no one else's. We have allocated plenty of time, an hour, for you to complete the entire experiment.

Payments & Completion of the Experiment

When you have finished the experiment, please raise your hand and an instructor will attend to the final procedures, including the issuing of a short questionnaire which you should complete immediately and hand to the instructor. A summary of your earnings appears on the computer screen at the end of the experiment. Arrangements for payment of your earnings will be emailed to you in the next few days.

Post-Experiment Briefing

This experiment is funded by the Institute of Chartered Accountants of Scotland, who will publish a report on the findings of the experiment in

due course. All published data collected will, of course, be anonymous. In order to preserve the integrity of the ongoing experiment, we are unfortunately unable to offer any feedback on its nature or your role in it until the whole experiment is complete. However, we will email every participant in the experiment when the report is available to let them know where they can obtain a copy

APPENDIX 4

Instructions for participants with profit related rewards

Materials

A copy of instructions (this document), a table of numbers, a pen, and paper are provided. Check you have these items now. If anything is missing please raise your hand and I will attend to you. These materials should be returned to the instructor supervising in the computer cluster at the end of the experiment.

Rules of Communication

Our purpose in conducting this experiment is to gain information about individual behaviour rather than group behaviour. For this reason, we must insist on the following rules concerning communication:

(a) No communication - there must be no verbal or other forms of communication between those taking part until everyone has completed the experiment,

(b) No questions – the only clarifications you can seek are those concerning the use of software in the PC room (if you have difficulty using the software, raise your hand and wait for assistance from the instructor).

Introduction

You will be taking part in a timed computerised experiment designed to investigate the nature of individual decision-making under conditions of uncertainty. You will be paid for your participation. Your earnings will depend upon how you perform the experimental task. Earnings will be calculated and reported to you today, privately – they are then paid in cash, privately, in a few days time. I will first explain the experimental task and then how you earn money from performing this task.

The Experimental Task

Please look at your profit table. The rows are labelled 1 to 20 in the left hand column. The columns are labelled 1 to 20 in the top row. In essence you will be playing a series of 'games' against the computer in which the computer first randomly picks a row, which you are unable to observe; and then you choose a column. The chosen row and column will determine, where they cross in the profit table, the profit earned.

To illustrate how the game works, suppose one of you plays 'the computer' [SELECT SOMEONE TO PLAY THIS ROLE – NAMED 'X' BELOW]. X should write down a row number on a piece of paper, without revealing it to the group, and then pass the piece of paper to me. Now, consider what happens when you make a choice of column. [ASK A DIFFERENT RESPONDENT 'Y' FOR A CHOICE OF COLUMN: THIS IS THEN ANNOUNCED SO EVERYONE CAN HEAR THE CHOICE]. The profit result is determined by the intersection of the row and column. In fact the row chosen was …. ('X' CONFIRMS THE CHOICE OF ROW). Hence the profit is ….(EXPLAIN THE RESULT VIA AN OVERHEAD).

The experiment is organised as follows:-

(a) There are 12 trials, for each of which there are 5 decision periods, altogether making 12x5=60 column decisions for you to take. You identify your column choice each time by inputting your chosen column's number at your computer terminal when prompted. The number of the column you want to choose can be found in the top row of the profit table

(b) For each trial, the computer will select a row, which is then FIXED for all periods in the trial.

(c) You then choose a column in period 1 of this trial.

(d) The computer will ask you whether or not you wish to learn the profit earned by your choice in (c) above. If you agree, it will be reported immediately (but note there is a cost associated with this - see below). If you do not, the profit earned will not be reported until the end of the trial.

(e) The process in (c) and (d) is then repeated for periods 2,..,5 of the trial.

(f) At the end of 5 periods, the profits earned in each period are revealed to you without cost (see information costs below), and your money earnings are calculated and also revealed.

(g) The process now starts again with a new trial – the computer again chooses a row at random which is then fixed during the trial, and you repeat the process (c)-(f), (and so on, until all trials have been completed)

In taking part in the experiment, we would like you to try to earn as much profit as you can.

Notice, in the profit table that, since the chosen row will not be known to you when you select your column, you will generally not know for certain the profit that will be earned by your column choice. However, you do know that the row is fixed for the 5 periods of each trial, so that whenever you choose a column, not only does your choice determine profit, jointly with the selected row, but if you make an information request, it also provides information about the unobserved row being used in the current trial. Look again at the profit table. ['Y' who previously chose the column] chose column … If an information request is made, it would be discovered that profit from the decision was …. And one can reason that the computer has selected a row between row… and row… [ILLUSTRATE THIS ON OVERHEAD] With this knowledge, one may (or may not) wish to change the choice of column in the next period (and so on).

Additionally, the following information is available about how the computer has been programmed:

- The rows 1 to 20 are all equally likely to be randomly selected by the computer.
- The rows selected for you by the computer in a given trial will generally vary randomly from trial to trial, and will also differ across other individuals taking part in the experiment.
- The selection of a row by the computer in each trial is not affected by your choices of columns in the current or previous trials.

Money Earnings

Your earnings are (i) a fixed proportion of profit earned, but are also (ii) negatively related to the number of times you request profit information during a trial. Your money reward per unit of profit earned, and the

money loss per information request will be notified to you at the start of the experiment.

You have 180 seconds to complete the five column choices expected in each trial. If you run out of time before completing the five column choices, you will only be paid for those you actually complete. The clock will start anew at the beginning of each trial. Whether or not you requested information on earned profits during each trial, you will also be informed (at no extra cost) of the total profits earned at the end of each trial

We recommend that, before you begin the experiment, you spend some time examining the structure of the profit table. Do not be influenced by the pace at which other respondents work, whether slower or faster than you. Your money rewards will be based solely on your performance of the task and no one else's. We have allocated plenty of time, an hour, for you to complete the entire experiment.

Payments & Completion of the Experiment

When you have finished the experiment, please raise your hand and an instructor will attend to the final procedures, including the issuing of a short questionnaire which you should complete immediately and hand to the instructor. A summary of your earnings appears on the computer screen at the end of the experiment. Arrangements for payment of your earnings will be emailed to you in the next few days.

Post-Experiment Briefing

This experiment is funded by the Institute of Chartered Accountants of Scotland, who will publish a report on the findings of the experiment in due course. All published data collected will, of course, be anonymous.

In order to preserve the integrity of the ongoing experiment, we are unfortunately unable to offer any feedback on its nature or your role in it until the whole experiment is complete. However, we will email every participant in the experiment when the report is available to let them know where they can obtain a copy

APPENDIX 5

Selected computer screen shots for participants with profit-related rewards

(a) Initial screenshots – Entering personal information

Welcome to Experimental Research.

Prepare yourself with pen/paper etc. This will be a Timed Experiment < The clock is not running yet > You will receive a message when the clock starts.

We need your name and student number so that we pay the right people the right amount of money!

Please enter your full name and student number (on one line but separated by spaces) and then press 'Enter'

a n other xxxxx

You entered your name as a n other

If this is correct, please type 1 and press 'Enter'

Otherwise type '2' (or anything, other than '1') and press 'Enter'

1

Experiment Log In Time: 8:31:40

Log In Date: 27/11/2006

Remember that you are NOT in competition with other participants. When they start, when they finish, and what they do has NO bearing on the games you are playing against the computer, or on the rewards you can earn.

Your Earnings

_____

1) You earn 0.50 pence for every unit of profit you make for the firm.

2) You lose 1.00 pence for every information request you make

(b) Screenshots from the beginning of the start of play

You can start whenever you want. To start the game enter '1' and hit 'Return'. However, only do this when you feel you are ready to commence the game.

1

Trial 1 begins now.

You have 180 seconds to complete the trial

The clock starts now!

A row of the pay-off table has just been selected by the computer at random. This row will remain the same for all periods in THIS trial.

Trial 1 Period 1 (of 5) starts NOW.

What decision would you like to choose in this period? Type a number from 1-20 to identify the column you wish to choose, and then press 'Return'

11

Time Update: Time elapsed so far = 18 seconds (out of the overall allocation of 180 seconds).

In Trial 1, period 1, you chose column 11.

You now have an opportunity to discover the profit earned by your column choice. If you take this opportunity, the information request will cost you 1 penny. If you do not take this opportunity, this period's profit will not be revealed until the end of the trial.

Do you wish to discover the profit earned by your decision?

Type '1' for YES, and '2' (or anything else you like) for NO and hit 'ENTER'

1

Time Update: Time elapsed so far = 44 seconds (out of the overall allocation of 180 seconds).

Period	Your choice	Profit earned for the firm
1	11	0

Trial 1 Period 2 (of 5) starts NOW.

What decision would you like to choose in this period? Type a number from 1-20 to identify the column you wish to choose, and then press 'Return'

Please note - you are no longer being timed - time taken between trials does NOT affect your Earnings in any way.

At the end of a trial:

In Trial 1

You used 95 seconds.
You made 5 information Requests

Period	Your choice	Profit earned for the firm
1	11	0.00
2	6	0.00
3	4	0.00
4	1	5.00
5	1	5.00

Total Profit earned for the firm = 10.00

Your Profit related pay = 0.05 pounds
Deductions for Information Requests = 0.05 pounds

Overall Earnings from Trial = 0.00 pounds

Note: if column choice is 0 in the above, it is because you ran out of time in the trial.

Type '1' and hit 'return' to proceed.

1

Type '1' and hit 'return' to proceed to the next trial

(c) **Screenshots from the end of the experiment:**

Congratulations - You have now completed the Whole Experiment.

Please type '1' and hit 'Enter' to continue. **1**

A summary of the results you obtained now follows. Please type '1' and hit 'Enter' to continue. **1**

Trial	Net Earnings
1	0.000
2	2.180
3	0.420
4	2.005
5	0.870
6	1.720
7	0.020
8	0.270
9	1.470
10	0.870
11	0.795
12	2.180

Your Total Earnings from the Experiment were 12.80 pounds.

CONGRATULATIONS!!

This concludes the experiment. We thank you for participating, and hope you enjoyed the experience.

APPENDIX 6

Post-experiment questionnaire

Project No. RES0371 07209

(Sponsored by the Institute of Chartered Accountants of Scotland)

Full Name (Print)…….

1. Suppose, for a given trial, the row was fixed at row 15. Suppose you selected column 18 in the first period. What profit would you earn for the firm in that period?

   Your answer ……..

2. Suppose you begin a new trial. You do not know the row selected by the computer. If in the first period you choose column 6, request profit information, and learn that profit was zero, what do you conclude about the row number selected by the computer?

   Your answer ……..

3. Suppose, after a choice of column, you earned 0 for the firm. How much do you earn for *yourself*?

   Your answer ……..

4. Suppose, after a choice of column, you earned 80 for the firm. How much do you earn for *yourself*?

   Your answer ……..

5. Suppose, after a choice of column, you make an information request. How much does it cost you?

   Your answer ……..

APPENDIX 7

Statistical modelling

This appendix covers in more detail the statistical modelling and testing undertaken for this report, including some of the more technical aspects of that analysis.

Chi-Squared test of row-generating program

As a check on the programming of the way rows were randomly generated, the row-generation sub-routine was set up to generate 200 million observations for 'row'. Given that 'row' is supposed to be a uniformly distributed random variable, this means that the expected frequency with which each row is observed should be 10 million. It is thus possible to conduct a χ^2 test of this as follows.

Table A7.1 Frequency count of randomly generated test rows and chi-squared test

Row	Count – Test Data
1	9,995,552
2	9,998,974
3	9,998,094
4	9,996,097
5	10,001,881
6	10,006,066
7	10,006,344
8	9,997,908
9	10,002,099
10	10,005,799
11	10,001,396
12	9,996,667
13	9,995,413
14	10,001,096
15	9,995,945
16	10,001,992
17	9,997,967
18	10,002,487
19	9,998,777
20	9,999,446
Total Frequency	200,000,000
Expected Frequency	10,000,000
χ^2_{19}	23.053
Null probability	> 0.20

Calculation of probability values for Table 3.1

This section discusses the computation of the probability values reported in Table 3.1. The starting point for this is the frequency counts of dominated/not-dominated choices made by participants, as per Table A7.2 below.

Table A7.2 Frequency counts of 'not-dominated' column choices

Trials 1-6	**Group G0**	**Group G3**
Total number of column choices made	1,943	833
Total Number of 'Not-Dominated' Column Choices Made	1,239	586
Trials 7-12		
Total number of column choices made	1,979	840
Total number of 'not-dominated' column Choices made	1,386	658
Trials 1-12		
Total number of column choices made	3,922	1,673
Total number of 'not-dominated' column choices made	2,625	1,244

Under the null hypothesis of random column selection, the expected value and variance of the number of 'not-dominated' column choices is $0.45n$ and $(0.45)(0.55)n$ respectively, where n is the number of column choices, excluding timed-out periods and all choices by participants with no profit-related rewards. The distribution of the number of 'not-dominated' choices is approximately Normal by the Central Limit Theorem. An illustrative calculation of the probability value follows: for Group G0 in Trials 1-6, n = 1,943; hence under the null hypothesis of random selection of columns, the distribution of the number of 'not-dominated' columns is N(874.35, 480.8925). The observed number, 1,239, is thus 16.6 standard deviations from the mean, with a

probability value of approximately zero. The continuity correction has negligible impact.

The probability values for Panel B of Table 3.1 are calculated similarly, but ignoring any timed-out periods. For Dobbs and Miller (2006a), n = 1710; and for S, n = 600.

Technical issues and further details of the regression modelling reported in chapter three

Many, though by no means all, of the standard parametric hypothesis tests that could be performed on specific versions of equation 3.1 in the report, rely on an assumption that u is a linearly independent, Normally distributed random variable, with constant variability. This is equivalent to an assumption that the response variable, given its nine determinants, is a linearly independent, Normally distributed variable, with constant variability. For certain specifications of equation 3.1 in the report, the validity of these assumptions is open to doubt.

For example, in equation 3.1, profit earned for the firm refers to a single trial. Since the profit table row is held constant for the duration of each trial, this means that to appropriately apply standard parametric tests based on Normal distribution theory, the distribution of trial profit must be Normal for every individual profit table row, for arbitrary values of the other eight determinants. Table A7.3 below lists feasible values for profit per trial, for each profit table row.

Table A7.3 Feasible values for trial profit

Profit table rows	Range of values for profit	Number of different discrete values for profit
1-2	0 - 25	6
3	0 - 50	11
4-5	0 - 100	20
6-8	0 - 150	29
9-10	0 - 225	42
11-13	0 - 300	55
14-15	0 - 400	72
16-18	0 - 475	85
19-20	0 - 500	90

Clearly, profit per trial is a discrete variable, with a row-dependent upper bound and a lower bound of zero. In contrast, a Normally distributed variable is continuous and unbounded. The problem is most severe for low-numbered rows, where there is likely to be a high proportion of observations at the lower bound, potentially affecting the tests in both Sprinkle (2000) and Dobbs and Miller (2006a). Although some writers claim that standard tests are robust to departures from Normality, see for instance Clinch and Keselman (1982), and Tan (1982), others have opposing views; see Bradley (1978), Glass, Peckham and Sanders (1972), Wilcox (1995) and Wilcox (1998). A second issue to address in equation 3.1 is its panel data, or repeated measure, aspect. That is, there are twelve profit observations for each of 42 'G3' participants. Ignoring this aspect may produce linear dependence between profit observations; see Maddala (2001).

Whilst it is possible to employ more complex, non-standard approaches that take explicit account of the issues of bounded and discrete variables, as well as the panel data structure, a simpler approach, adequate for the present study, is to aggregate the response data across

trials. If the profit response variable is re-defined as total profit earned for the firm by a participant over trials 7-12 of the experiment, then each observation of the new profit variable will be determined by a random collection of 6 profit table rows. The result is to shift the empirical distribution of observed profit away from the lower bound of zero, and also to increase the number of feasible values, attenuating its non-Normality. Trials 1-6 were discarded for the main analysis in this study, as they were in Dobbs and Miller (2006a), in order to reduce possible experience effects on the response variables; see Panel A of Table 3.1 in the main report. The effect on equation 3.1 is to remove trial as a determinant of observed profit leaving a pure cross-section of aggregated profit observations to be explained by the eight remaining variables.

A linear estimating equation, albeit accommodating some non-linear effects via the use of 'dummy' variables, was used for testing hypotheses. The estimation method was ordinary least squares multiple regression.

The role of the control variable for profit table rows requires some elaboration. This control variable, referred to in chapter three as the 'weighted sum of profit table row numbers faced by participant in trials 7-12' is denoted as *CR* in what follows. According to equation 3.1, only two determinants of a participant's earned profit are varied during the experiment: profit table rows and trials. Focusing only on the latter half of the experiment, it is plausible to assume that any learning effect on a participant's decisions will be exhausted by then, so that the trial effect might be absent for trials 7-12. With no trial effect, aggregate profit over trials 7-12 depends only on the set of rows occurring in those trials. In the absence of learning effects, one would expect that if a particular row occurred twice, then the individual would go through the same set of decisions, and hence earn the same profit. This suggests it is reasonable to expect aggregate profit to be linear in the number of times a row occurs. At the same time, again holding participant decisions constant, the higher the row number, the more profit that can be expected from

the task. Hence, other things equal, profit would then be a positive monotonic function of row number. A more formal derivation of the count variable *CR* is discussed in the next section.

Relation between weighted average row control CR and 19 'count' row controls

Denote the number of times profit table row *j* occurs by R_j. Then the weighted average row control variable, *CR*, is defined as:

$$CR \equiv \sum_{j=1}^{20} jR_j \qquad \text{(A7.1)}$$

Suppressing other variables for simplicity of exposition, a linear model of aggregate profit, $\Sigma\pi$, with a constant term, would have 19 row controls, R_j:

$$\Sigma\pi = \alpha_1 + \sum_{j=2}^{20} \alpha_j R_j \qquad \text{(A7.2)}$$

whilst a linear model with the *CR* omnibus variable is:

$$\Sigma\pi = \beta_1 + \beta_2 CR\,. \qquad \text{(A7.3)}$$

By definition:

$$R_1 = 6 - \sum_{j=2}^{20} R_j \qquad \text{(A7.4)}$$

the overall sum of the 6 trials minus the number of times each of rows 2-20 occurred. So,

$$\beta_1 + \beta_2 \sum_{j=1}^{20} jR_j = (\beta_1 + 6\beta_2) + \sum_{j=2}^{20} \beta_2 (j-1) R_j \qquad \text{(A7.5)}$$

Hence imposing the restrictions $\alpha_1 = \beta_1 + 6\beta_2$, $\alpha_2 = \beta_2$ and $\alpha_j = \beta_2 (j\text{-}1)$ for j=3... 20 transforms a model with 19 count variables into a model with just the single *CR* variable. There are 18 restrictions, since given an estimate of α_2, all α_j (j=3,… 20) are then determined by the above linear condition. On the other hand, α_2 can be freely estimated, as can α_1 since it is not fully determined by $\alpha_2 = \beta_2$.

Using *CR*, the weighted sum of row numbers, with weights equal to the number of times each row occurred in trials 7-12, is a relatively parsimonious form of row control. It turns out that, for all three response variable models, restricting the model to one in which the aggregate response is a function of *CR* is statistically acceptable. That is, starting from a general model in which there are individual row count variables, R_j, it is statistically acceptable to test down to a model that merely features *CR*. Accordingly, given its statistical performance, the variable *CR* was used as the 'row' explanatory variable throughout the main analysis reported in chapter three.

Diagnostic tests from the standard regression output were examined at every stage of the analysis, covering issues of linear independence, Normality, homoscedasticity and equation specification. When time was the response variable, the diagnostic tests indicated significant heteroscedasticity of u across the sample. To correct this problem, the estimating model for time used was successfully modified by a simple logarithmic transformation of the response variable. Since the logarithmic function is a positive monotonic transformation, the predicted signs of the main research hypotheses were unaffected.

The initial estimated relationships are listed in equations (A7.6), (A7.7) and (A7.8), with k^+, k^- denoting the reward per profit point and cost per information request respectively.

$$\Sigma\pi = \beta_1 + \beta_2 CR + \beta_3 k^+ + \beta_4 k^- + \beta_5 age + \beta_6 mf + \beta_7 upg + \beta_8 nat2 + \beta_9 nat3 + \beta_{10} nat4 + \beta_{11} gpa + u \quad \text{(A7.6)}$$

$$\Sigma IR = \gamma_1 + \gamma_2 CR + \gamma_3 k^+ + \gamma_4 k^- + \gamma_5 age + \gamma_6 mf + \gamma_7 upg + \gamma_8 nat2 + \gamma_9 nat3 + \gamma_{10} nat4 + \gamma_{11} gpa + u \quad \text{(A7.7)}$$

$$\ln \Sigma s = \delta_1 + \delta_2 CR + \delta_3 k^+ + \delta_4 k^- + \delta_5 age + \delta_6 mf + \delta_7 upg + \delta_8 nat2 + \delta_9 nat3 + \delta_{10} nat4 + \delta_{11} gpa + u \quad \text{(A7.8)}$$

The three dependent variables in the above are defined as:

$\Sigma\pi$ = sum of profits earned for the firm in trials 7-12 by the participant.
ΣIR = sum of information requests made in trials 7-12 by the participant,
$1n \Sigma s$ = log sum of time taken in trials 7-12 by the participant,
β_i, γ_i, δ_i, i =1, ...11 are the coefficients to be estimated.

The main research hypotheses predict that, other things equal, increasing the money reward per unit of profit earned for the firm will increase time spent on the task, number of information requests made, and profit performance; whilst, other things equal, increasing the money

cost per information request will reduce time spent on the task, number of information requests made and profit performance. More formally, the null and alternative hypotheses, respectively H_0 and H_1, state that, in the population

$$H_0 : \beta_3 = 0, H_1 : \beta_3 > 0$$
$$H_0 : \beta_4 = 0, H_1 : \beta_4 < 0$$
$$H_0 : \gamma_3 = 0, H_1 : \gamma_3 > 0$$
$$H_0 : \gamma_4 = 0, H_1 : \gamma_4 < 0$$
$$H_0 : \delta_3 = 0, H_1 : \delta_3 > 0$$
$$H_0 : \delta_4 = 0, H_1 : \delta_4 < 0$$

For all other coefficients, there are no particular expectations concerning their signs, except perhaps in profit equation (A7.6), where β_2 and β_{11} are both expected to be positive; the former because a larger value of *CR* signifies a higher average row number in the trials faced by the individual and, referring to table 2.1, this can be expected to translate into higher profit performance; the latter because higher GPA might be expected to translate into a higher level of understanding of the structure of the problem.

Care needs to be exercised in using the 'testing down' procedure when there are significant linear relationships between potential determinants. Very strong linear relationships can lead to apparently insignificant estimates for affected determinants merely because the regression approach is unable to disentangle the *separate* effects of each related determinant. Mistakenly omitting a significant determinant, when it is strongly related to a determinant retained in the equation, can lead to biased estimation of the *separate* effect of the retained determinant. Since incentives and rows are randomly assigned to participants, any problem of linear dependence between determinants will be confined to personal characteristics, such as *age* and *upg*. Table A7.4 sets out all the pairwise correlation coefficients for personal characteristics,

measuring the strength of linear dependence on a scale of -1 to +1, with the extremes indicating perfect dependence, negative or positive. Only the correlation between *age* and *upg*, at 0.72, appears to be large, given a sample size of 42.

Table A7.4 Matrix of Pearson Product-Moment correlations of personal characteristics

	age	*mf*	*upg*	*nat2*	*nat3*	*gpa*
age	1.00					
mf	0.01	1.00				
upg	0.72	0.08	1.00			
nat2	0.00	-0.13	-0.06	1.00		
nat3	0.40	0.39	0.56	-0.05	1.00	
gpa	-0.38	0.28	-0.30	0.14	-0.23	1.00

A more comprehensive estimation model for the full sample

As explained in chapter two, with the results reported in chapter three, the modelling of incentive effects was conducted using the sub-sample of participants who had completed entirely correctly the post-experiment questionnaire. This section examines a more general model and shows that the results reported in chapter three are essentially unchanged if the full sample is used in the analysis.

The objective in building this more general model was to let all the data inform the estimation procedure but allow for differential effects of the four sub-samples, 'G0', 'G1', 'G2' and 'G3', on the constant term, the omnibus row control and the two incentive determinants. Recall that 'G0' denotes the whole sample, 'G1' the sub-sample that answered questions on the profit table correctly, 'G2' were those who, in addition, also answered questions on their information costs correctly, and finally 'G3' is the subset who answered all these questions and the questions on

their profit-related pay correctly. Thus these are nested sub-samples; $G3 \subset G2 \subset G1 \subset G0$. Modelling for sub-sample effects used four 'dummy' variables. Specifically, for m = 0, 1, 2, 3, let the dummy variable Gm = 1 if participant is a member of sub-sample 'Gm'; and 0 otherwise. Then the beginning model for profit, corresponding to equation (A7.6), takes the form

$$\begin{aligned}\Sigma\pi = &\sum_{m=0}^{3}\left(\beta_{1m}\times Gm\right)+\sum_{m=0}^{3}\left(\beta_{2m}CR\times Gm\right)+\sum_{m=0}^{3}\left(\beta_{3m}k^{+}\times Gm\right)+\sum_{m=0}^{3}\left(\beta_{4m}k^{-}\times Gm\right)\\ &+\beta_5 age+\beta_6 mf+\beta_7 upg\\ &+\beta_8 nat2+\beta_9 nat3+\beta_{10} nat4\\ &+\beta_{11} gpa+u\end{aligned}$$

(A7.9)

The use of multiplicative dummies implies that, for example, the coefficient of k^{+} applicable to sub-group 'G0' is β_{30}; the coefficient of k^{+} applicable to sub-group 'G1' is $\beta_{30} + \beta_{31}$; the coefficient of k^{+} applicable to sub-group 'G2' is $\beta_{30} + \beta_{31} + \beta_{32}$; and the coefficient of k^{+} applicable to sub-group 'G3' is $\beta_{30} + \beta_{31} + \beta_{32} + \beta_{33}$. The usual tests can then reveal, for each affected determinant and for the constant term, whether sub-groups differ amongst themselves, or are homogeneous and therefore can be pooled, saving degrees of freedom in estimation. Testing down from (A7.9), for example, reaches a final model of the form

$$\begin{aligned}\Sigma\pi = &\underset{(0.92)}{22.88}G0-\underset{(0.13)}{424.27}G1+\underset{(0.00)}{15.03}\left(CR\times G0\right)+\underset{(0.01)}{11.64}\left(CR\times G1\right)-\underset{(0.01)}{4.88}\left(CR\times G2\right)\\ &+\underset{(0.00)}{14{,}202.90}\left(k^{+}\times G3\right)-\underset{(0.11)}{667.94}\left(k^{-}\times G3\right)-\underset{(0.19)}{74.26}mf-\underset{(0.02)}{168.69}nat3-\underset{(0.00)}{331.53}nat4\end{aligned}$$

(A7.10)

Here, the 'testing down' found β_{12}, β_{13} not significantly different from zero. Thus all participants in groups G1-G3 can be pooled for the purposes of estimating the constant term. Whereas for k^{+}, the 'testing

down' has shown that all of β_{30}, β_{31}, β_{32} are not significantly different from zero. Thus there is an incentive effect only for sub-group 'G3'. This result means that for participants outside the 'G3' group there are no significant incentive effects.

The same procedure can be applied for the other dependent variables Σ *IR,* ln Σs, starting in each case with a general model with the same explanatory variables as (A7.9). Equation (A7.11) below provides the 'tested down' equations which result from starting with this more comprehensive model using all sample data, not only the 'G3' group, for the dependent variable $\sum$ *IR*.

$$\Sigma IR = \underset{(0.74)}{1.77}\,G0 - \underset{(0.04)}{5.72}\,G2 + \underset{(0.05)}{12.28}\,G3 + \underset{(0.03)}{0.09}\left(CR \times G1\right) - \underset{(0.06)}{0.18}\left(CR \times G3\right) + \underset{(0.00)}{338.36}\left(k^{+} \times G0\right) - \underset{(0.00)}{31.22}\left(k^{-} \times G0\right) + \underset{(0.06)}{0.40}\,age - \underset{(0.00)}{8.94}\,nat4 \qquad \text{(A7.11)}$$

Comparing equations (A7.10) and (A7.11) with Tables 3.4 and 3.5, it is clear that the results on the main hypotheses are broadly similar for the 'G3' coding and for the more comprehensive 'G0' model, which uses all the sample data but also uses an estimation procedure designed to discriminate between differential effects for the sub-groups.

Problems with diagnostic tests cast doubt on the testing down procedure for the model for ln $\sum s$, so the results for this case are not reported.

The Impact of Financial Incentives on Decision Making: Further Evidence

Do financial incentives improve decision making and motivate performance? The use of financial incentives is widespread and believed to work but what evidence is there to support their continued use? This research furthers the work undertaken by the authors on financial incentives published by ICAS in 2006. The project takes a laboratory experiment approach to test the theory that formal performance related rewards increase the use made by decision-makers of valuable information and, in turn, lead to improved decision-making.

The experiment found that profit performance was strongly and significantly related to the level of profit-related incentives. The research highlights the importance of individuals understanding the structure of incentive schemes for them to be effective. The research also identified that irrespective of incentives, individuals perform better when they have an intrinsic interest in something or wish to please. Therefore recruitment of the "right" individual for the job remains a key factor in performance.

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