DOCTOR OF PHILOSOPHY

Factors affecting the conscious control of movement

Oldham, Anthony Robert Hiron

Award date:
2001

Awarding institution:
Bangor University

Factors Affecting the Conscious Control of Movement

Anthony Robert Hiron Oldham

A thesis submitted in partial fulfilment of the requirements for the degree Doctor of Philosophy.
University of Wales – Bangor.

September 2001

Summary

This thesis pursues two strands of research: The first area involves fine motor control and conscious awareness of it. The second area involves conscious processing and how it interferes with performance under pressure. In Experiment one, participants were required to identify perturbations to a force production task. Results supported the notion that individuals have difficulty consciously identifying low level corrections to movements that they have made. Experiment two expanded on this by examining the effect of different display resolutions and different force outputs on the ability to identify change. Together, these two sets of findings indicate that people have a relatively poor perception of low level movement correction and that the threshold for perception of correction is constant across effectors and KR resolution. Furthermore, results also tentatively suggested that thresholds for the determination of change are linked to intrinsic variability associated with a given task.

The second area of investigation tried to resolve a conflict between two replications of an experiment by Masters (1992) which offer contradictory results. One experiment supported a conscious control explanation (Hardy et al., 1996), another supported a task difficulty explanation (Bright & Freedman, 1998). It was argued that differences between the two lay with quantity of practice. Experiment three examines this suggestion by replicating the methods used by both investigators. This study failed to successfully replicate either method due to an ineffective stress manipulation. Experiment four revealed results supporting the learning based explanation of the difference between results. Overall these findings offered unique support to the conscious processing hypothesis through learning effects. The broad interpretation of these findings supports the view that conscious control of movement can prove problematic under stressful conditions. These problems might be explained by different languages of representation being employed at different levels of cognitive functioning.

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Acknowledgements

I would like to offer my sincere thanks to all those who have helped get to this point.

In particular I would like to thank:

My supervisors Lew Hardy and Mike Khan for assistance and support far beyond the call of duty.

John & Della Fazey for getting me started, giving me inspiration, pizza and "something useful to do".

Nicky, Jo, Tim, Sean, Tara, Ali, Yvonne, Amanda, Kieran, Simon, Dave, Samuelle, Becks, Kylie, Jane, Stuart and all the rest of "Team Bangor" who have put up with bleating, rudeness etc. from me for such a long time. Go Ali !

Special mention goes to Matt Cook for exceptional teamwork.

Thanks also to Pat and Katharine, two very under appreciated people.

Ian, David, John and the rest of my colleagues at SHU for finding me more than a bit of slack when I needed it.

Vic Brown, Dave Jones, Derek Collins, Chris Onslow and Pete Walker – good teachers are very hard to find.

Mark & Di.

Mum for teaching me to be tough and telling me I could do anything (what a choice !)

Very special thanks go to Catherine Bacon.

This candidate acknowledges the contribution of Peter Smith.

Chapter 1

General Introduction

Chapter 1

General Introduction

Background

Recent performance models and popular sporting history illustrate how the competitive environment can bring about sudden and sometimes catastrophic changes in performance. Often these changes are brought about by excessive pressure leading to competitive anxiety. The ability to deal with this anxiety appears to be crucial in achieving the highest levels of performance (Orlick & Partington, 1988). An important challenge for sport psychologists therefore, is to try and explain why at important times performers fail to do just that, perform. A number of theories have been developed to account for the relationship between anxiety/arousal and performance. Examples are Drive Theory (Spence & Spence, 1966), Inverted U hypothesis (Yerkes & Dodson, 1908; Oxendine,1970,1984), Individualised Zones of Optimal Functioning (Hanin, 1997), Multidimensional Anxiety Theory (Martens, Vealey & Burton, 1990) and Catastrophe Models (Hardy, 1990). All of these models make some attempt to describe the changes in performance that come about as a result of changes in anxiety, arousal or both. What these models do not do, is explain how anxiety or arousal actually influences performance and what changes in performance might be expected as a consequence. Two recent theories that begin to address this shortcoming are Processing Efficiency Theory (Eyesenck & Calvo, 1992) and the Conscious Processing Hypothesis (Masters, 1992). Processing Efficiency Theory (Eyesenck & Calvo, 1992) is based on the way cognitive resources are used and their limitations in terms of processing. Processing Efficiency Theory predicts that anxiety or worry will have two effects on performance. The first effect is that in response to worry, performers will increase effort and the use of conscious strategies in order to facilitate performance.

This additional effort expenditure will be at the expense of overall performance efficiency though not necessarily performance outcome, and may also depend on the depth of cognitive resources available to the performer. The second predicted effect is that worry in itself will specifically reduce the capacity of working memory and as a consequence limit resources further, to the detriment of performance. Conscious Processing Hypothesis on the other hand is based around ideas of automaticity and regression of learning. Specifically, it predicts that under anxious conditions, highly practised performers with a large pool of explicit knowledge accumulated across learning, will attempt to use that knowledge to control the task at hand.
This "re-investment" of explicit knowledge, it is suggested, will cause a "de-automatisation" or regression in performance from automatic to controlled processing. Controlled processing is thought to be less efficient and more like that found earlier on in learning. Interestingly, support has been found for both of these models using putting tasks (Masters, 1992; Mullen & Hardy, 2000; Mullen Hardy & Tattersall, under review). Consequently a complete single explanation of these results remains elusive. In spite of this, a compelling feature of both these theories is the notion of additional effort expenditure and the desire to "tell oneself what to do" when anxious. This should perhaps be set in direct contrast to conditions described by Csikzentmihalyi (1990), for flow or peak performances which identify effortlessness, abstract attention and a detached sense of control. It would seem that effort, attention and control can be misallocated at the expense of performance. This indicates a need to explore the relationship between, anxiety, allocation of effort and task control.

Further to the examination of changes in psychological behaviour resulting from anxiety, it is important to consider the actual changes that take place in the movements themselves. More specifically, if a performer exceeds resource limits or starts to re-invest, what changes in movement occur that bring about such dramatic changes in outcome ? Are these changes specific in that they only affect certain aspects of movement, or are they global and have an effect on all aspects of the movement task ?

Bernstein (1967) presents a model of control which highlights a fundamental difficulty associated with effortful control of movement. A principle feature of the model is that consciousness does not have access to all levels of the motor system. Bernstein divides the motor system into four broad levels: The level of tone, the level of synergies, the level of space and the level of action. The level of tone might generally be thought of as the level at which force is generated within specific muscles.
At the level of synergies, groups of muscles are linked together in a stable and reproducible manner. Above this, is the level of space which deals with movement in the space adjacent to the body, for example reaching and locomotion. At the top of this hierarchy is the level of action involving sequences of actions that require adaptive changes and creative solutions, such as changing a light bulb. Within this model, the levels of space and action are described as leading levels, whereas the levels of tone and synergies are designated as supporting levels. This model assumes a degree of cooperation between levels within the hierarchy. Whilst upper levels plan and exercise control (leading), lower levels construct the actual movements involved (supporting) (cited in Beek 2000). At supporting levels of control, explicit knowledge seems redundant as it has to correspond with specific muscle groups and forces. Equally, detailed information from supporting levels is unlikely to prove particularly comprehensible or useful at leading levels. The whole purpose of skilled activity in the scope of this model is to delegate or distribute responsibility, not to accumulate responsibilities in one place.

An important feature of this model is that according to Bernstein, lower levels and particularly the level of synergies are responsible for using sensory information in order to make "background corrections". These background corrections ensure automaticity, and are highly task specific. The broad suggestion might be that the level of action may find a solution, but the implementation and learned continuity of that solution are ensured at lower levels. As Beek (2000) points out, automatization occurs when leading and subserving levels of control are optimized to achieve a task goal. De-automatization occurs when organisation is overridden and control is switched to new unusual levels of control. The notion that different levels of control refer to different categories of information is borne out by the studies of Annett (1991), in which adequately describing a process such as tying a bow is quite difficult without actually doing it or making gestures. Furthermore, descriptions, actions and actual movements can differ from each other quite substantially.

It is reasonable to suggest that anxious performers through their desire to take complete control of a task, bring about switches between levels of control. This may in turn bring about a change in the "language" of control and attention to only particular, sometimes inappropriate, aspects of the task. This might then be characterised as a freezing of degrees of freedom (Verijken, Van Emmerik, Whiting & Newell, 1992), or indeed a regression. Mullen et al. (2000) offer tentative support for this view by showing that anxious golfers stiffen the distal joint of the wrist whilst putting.
It may also be speculated at this point, that switching between levels of control and only effectively taking control of a small portion of the task, has further consequences for the allocation of attention. Whilst attention is focussed on specific aspects of a task, it may not be usefully deployed elsewhere or where it is needed for adequate specification of that task. This has implications for task control and general processing efficiency.

Overall the problem of performance breakdown under pressure or “choking” (Baumeister, 1984) as it is popularly termed, offers questions both in terms of motor behaviour and traditional sports psychology. An important motor related issue hinges on the real limits of conscious knowledge and control of movement. An important sport psychology related issue is linked to the theories employed to explain changes in response to anxiety. It is against this background that the current thesis is set. Just why it is that anxious people mess up and what happens when they do ?

The history and development of this thesis

The intention behind the development of this thesis was to explore both the limits of conscious knowledge about movement and the influence of top down control on low level automated processes. With this in mind two series of experiments were undertaken. The first series addressed the issue from within a traditional motor control framework. Chapters two and three seek to identify limitations on the conscious knowledge of movement production and in particular responses to small changes in task specification. To put it another way, whilst most people are aware of the success or outcome of a movement, detailed knowledge of what actually was done to achieve these aims remains difficult to identify. A force control task was devised, whereby participants had to accurately control force output in order to make a bar chart display on a VDU reach a specific value. During testing, changes were made in the signal gain for the output display and participants were questioned about whether they thought changes had taken place. Independent variables explored were effector combinations, levels of force output and display characteristics. These variables were manipulated in order to examine both the robustness of the findings and their generality.

The second series of experiments covered in chapters four and five were designed to examine the effects of anxiety on performance. Of specific interest was how high levels of anxiety dispose certain individuals to consciously control their movements to the detriment of performance. Masters (1992) study on anxiety and putting performance, provided a useful framework through which to examine these effects. It identified a method and a potential line of reasoning as to why performers breakdown under pressure. The conscious processing hypothesis derived from this research proved both compelling and robust, particularly after replication by Hardy, Mullen and Jones (1996), had corrected for a potential artefact in the original method of Masters (1992). It was originally thought that the next logical step would be to record and investigate the actual changes in movement that occur as a consequence "re-investment" of explicit knowledge in a learned task. However, a paper published in 1998 by Bright & Freedman called into question the original findings of Masters (1992) and contradicted the findings of Hardy et al. (1996). With this in mind, it was felt necessary to attempt a further replication of both the Hardy et al. (1996) and Bright & Freedman (1998) studies.

Chapter 2

Differences between detection and response to perturbations of a shoulder push and a finger press task.

Chapter 2

Introduction

Within cognitive and motor psychology, a number of models have been proposed that suggest different levels of processing. An underlying feature of these models is that particular aspects of processing are apparent to consciousness while others are not. Models employing this idea have included: Procedural/Declarative (Anderson, 1982), Implicit/Explicit (Reber, 1989) and Automatic/Non Automatic (Schnieder & Schiffrin, 1977). This idea is neatly summed up by Annett (1991) who uses the example of tying shoelaces. He points out that, whilst there is no doubt that we are fully aware of what we are doing, we have relatively little explicit knowledge about the actual finger movements. In fact the majority of this knowledge is gained by watching ourselves doing the task, rather than analysing internal sources of information such as kinaesthesia. An important feature of the distinctions outlined here, is that consciousness does not appear to have access to all aspects of movement control. Of particular interest to the current investigation is the limit of conscious knowledge beyond visual resources. In circumstances where vision is absent or unreliable, just how useful is kinaesthesia or movement information fed back to consciousness from within the body ?

An early experiment that highlights the distinction between what we do and what we know about our attempts to control movement was performed by Henry (1953). Whilst blindfolded participants were required to control the position of a pad placed at shoulder height. In one of three conditions, participants were requested to maintain constant pressure on the pad while the pad moved (constant pressure task). In another condition, participants were required to maintain a constant hand/body position in response to the same pattern of changes (constant position task). In a third condition, participants were requested to maintain pressure on the pad and hold the arm and upper body immobile whilst reporting perceived changes in pressure (perception task). Participants were able to respond to changes in the constant position task that were one twentieth (5%) of the force magnitude that they were able to detect in the perception task. Thus, participants were able to respond to very small variations in force but were not aware of those responses taking place. Both Henry (1953) and Schmidt (1988) suggested that this effect is the result of corrections taking place at the spinal level and which were not available to conscious awareness. Whilst such an explanation is suitable for a blindfolded task, it would be difficult to make the same explanation for results obtained from tasks involving vision.

When determining the correctness of a given movement, vision when available appears to have a high priority. This has been illustrated in studies of agency, where an alien hand is substituted for a participant's own hand during the production of hand gestures. These studies record a significant number of instances where gestures made by an alien hand were mistaken for a participant's own hand (Neilsen, 1963; Daparati et al., 1997). Being unable to distinguish between personal actions and those of a surrogate, raises issues about the ability to perceive movement without the help of vision. Is there a lack of appropriate internal movement information or a failure to make use of available information when vision is present? Further insight into this issue is offered by Fourneret & Jeannerod (1998) who perturbed the visual display of movement information during a line drawing task. In this study, participants were requested to trace lines on a graphic tablet which were displayed on a screen in front of them. The display of these lines would either correspond directly with the actual movement, or randomly deviate towards the left or right by varying amounts. In one condition, participants were asked to estimate the degree of perturbation; in a second condition they were required to draw the perceived direction of their hand while blindfolded. The study showed that participants either grossly underestimated the deviation made, or failed to recognise any deviation at all. It was concluded that participants were not aware of the internal signals generated by their own movements. Taken together, these studies suggest that during visually controlled tasks, the internal movement information generated is either somewhat impoverished or not generated at all.

Jeannerod and his colleagues discuss several potential explanations for these results (Fourneret & Jeannerod, 1998; Georgieff & Jeannerod, 1998). One is that proprioceptive information itself is limited or "weakened". This it is argued, is unlikely given the success of corrections to the perturbations and the accepted importance of proprioceptive information to successful movement control. Deafferentation studies suggest that proprioception has an important part to play in fine motor control, therefore any "weakening" would be to the detriment of successful completion of the task. Another explanation refers to issues of visual dominance. In this case "seeing is believing" and the presence of apparently congruent visual information on the screen, caused participants to overlook or ignore any other movement information, discrepant or otherwise. The final explanation offered argues that, internal information, such as proprioception or action related signals were generated and that these signals were sufficient to permit successful control and correction of the movement. However, the signals generated were not made available for conscious monitoring. This latter case is favoured by Jeannerod and colleagues and to some extent fits with the findings of Henry (1953). In support of the suggestion that somehow action related signals are not conveyed to consciousness, Jeannerod and colleagues cite evidence by Wann & Ibrahim (1992) on "proprioceptive drift". This evidence implies that conscious position sense and its contributory elements such as kinaesthesia, rapidly degrade from memory as soon as a new position is taken. Thus an opportunity to compare existing internal movement information with a template for correctness requires recall of both instances. Such comparison may prove difficult if not impossible, especially if other movements have to be performed in the meantime.

Georgieff & Jeannerod (1998) take this notion a little further by suggesting a "double coding of action related information". By this it is meant that signals generated for the control of a movement are separate from those employed to make conscious judgements about that movement. Indeed, this idea may be extended to suggest that different information from that used for control is generated and fed to consciousness after the original action. This explanation is inviting for several reasons. Primarily, it addresses the problem that arises between what makes up our knowledge of movement (electrocortical patterns and relative joint positions) with actual conscious experiences of movement. Moreover, this approach suggests that action related information used to control movement, is separate and probably different from that fed to consciousness. If consciously perceived movement information is in some way different, this raises questions about the nature of the difference, for example the boundaries for the perception of error.

From the evidence presented so far it would appear that consciously perceived movement information lacks the necessary detail to inform the performer about all aspects of the movement being performed. As a result of this lack of detail, performers may experience misattribution of agency (ownership) of a movement and also fail to perceive low-level changes (Fourneret & Jeannerod, 1998; Daparati et al., 1997). However the evidence does suggest that conscious information is sufficient to allow discrimination of large discrepancies between what is seen and what is felt. An important question here would seem to be how large this discrepancy needs to be.

The starting point for this study is to examine the threshold for detection of a mismatch between what is seen and what is felt.

The present experiment is similar to that of Fourneret & Jeannerod (1998) in that it involves a perturbed visual display. However, a force production task rather than a line drawing task has been employed. Participants were requested to repeatedly apply a target force to a dynamometer in time with an auditory time signal. Visual feedback regarding force output was provided in the form of a bar presented on a Visual Display Unit. The target goal was a marked height on the bar chart display. Perturbations were produced by altering the amount of force required to reach the target goal on the display. Control and Perturbed (test) trials were alternated concurrently. On control trials, participants were told there would be no perturbation. On the test trials, participants were informed that there might be change in gain between the force output and the display on the monitor and were asked to detect if such a perturbation was present. Perturbations varied randomly in 5% increments from 0-25%. In this way it was hoped to obtain an estimate of the threshold for the detection of discrepancy between what participants saw on the screen and what they felt. This experimental adaptation afforded a wider range of perturbations to be used than those available for a line drawing task. Also, the task was employed using two different effector systems: finger pressing and pushing from the shoulder. This allowed different postures and effector systems to be compared. Given that there is a threshold for detection, the hypothesis tested here was that the threshold would change according to the scale of force and effector system used.

Method

Participants

13 female and 10 male participants completed the experiment. All participants were undergraduate sport science students. Participants were randomly allocated to one of two tasks; finger press or shoulder push.

Apparatus and task

For the finger press, a dynamometer was fixed to the desk in front of the participant. This produced an output that could be converted to a digital value between 0 and 1500. The force required to produce an output of 1000 points was 11.74N. An output of 1.174N was required to hit the goal target of 100 points in this task.

For the shoulder push, data acquisition was performed using a dynamometer set at shoulder height. This was a standard load cell that produced output that could be converted to a digital value between 0 and 400. The force required to produce an output of 400 points was 56N. Therefore a force change of 0.14N would cause a displacement of one scale point. An output of 14N was required to hit the goal target of 100 points in this task.

Visual feedback for both tasks was provided using a computer monitor placed 40-50cm in front of the participant. The display was a staged bar chart where the height of the bar represented the magnitude of force. The bar was non-linear giving a displacement of 8cm for the first 80 points and 4cm for the following 20.

The finger press required participants to produce a single downward force on the dynamometer every 500ms for 11s paced by a timing signal. This was done with the distal portion of the index finger on the preferred hand. This resulted in participants making 21-22 discrete responses across the 11s period. These actions constituted the completion of 1 trial. The shoulder push required participants to grasp a handle set facing them at shoulder height, in such a way that the angle at the shoulder and elbow was 90 degrees, when their feet were set in a parallel position horizontal to the handle. From this position they were required to apply forward pressure to the handle. In both cases pilot testing suggested that the force required to achieve the target goal was less than one fifth of maximum output for all participants.

All participants received each of six perturbations, which were 0,5,10,15,20 and 25% increases in gain introduced via the VDU. The value that was supposed to be displayed on the screen was multiplied be a predetermined distortion factor. The gain was introduced by multiplying the original signal by a decimal value corresponding to the change in gain required. Thus a 5% gain was implemented using the original signal multiplied by 1.05. The net effect was an increase in signal gain to the VDU. Consequently it appeared that less force was required to achieve the target goal during perturbed trials. Each participant received all levels of the perturbation variable five times. Control (unperturbed) and test (perturbed) trials were presented alternately. The schedule of presentation was controlled using a randomised block method. Although 21-22 responses were collected in each trial, only the first twenty viable responses were retained for analysis.

Procedure

Participants performed a 20 trial practice phase in order to familiarise themselves with the task. As stated earlier 21-22 discrete presses on the dynamometer across the 11 second period constituted 1 trial. A normal trial required participants to hit the target of 100 points on the screen. The force required to do this was 1.174N for the finger press and 14N for the shoulder push. All 20 practice trials were performed without perturbation, therefore requiring the same amount of force to be produced in each trial. The practice was followed by a 60 trial test phase which consisted of 30 control trials (the same as in practice) and 30 "test" trials (during which perturbations were administered). Overall participants were presented with 30 pairs of trials (one control, one test). The random allocation of perturbations was not known to the experimenter at the time of data collection.

All participants were given instructions on how to perform the task followed by three demonstrations necessary to clarify the task requirements. The experimenter answered all relevant questions at this time. Specific instructions provided by the experimenter included:

i) Participants should try their hardest at all times and should not attempt a change of strategy during the course of the experiment.

ii) Participants should avoid changing hand or finger orientation during the experiment.

iii) Participants were told that the priority of the task was not to synchronise with the time signal, but to reach the target output with as much accuracy as possible.

iv) That the important priority was to get as close to the 100 target as possible at all times.

An additional set of instructions were provided during the perturbation phase. Participants were informed that this was to be a test of discrimination, in which it was possible that on specified "test" trials, it might be that less force than normal would be required to reach the 100 target. Participants were informed that this change was randomly allocated between the test trials and knowledge of this allocation was not available to the experimenter. At the end of each "test" trial participants were required to indicate whether they thought that the test trial had been changed or not. Participants could answer with a yes or no only; these verbal responses were coded as a difference or no difference detected contingency.

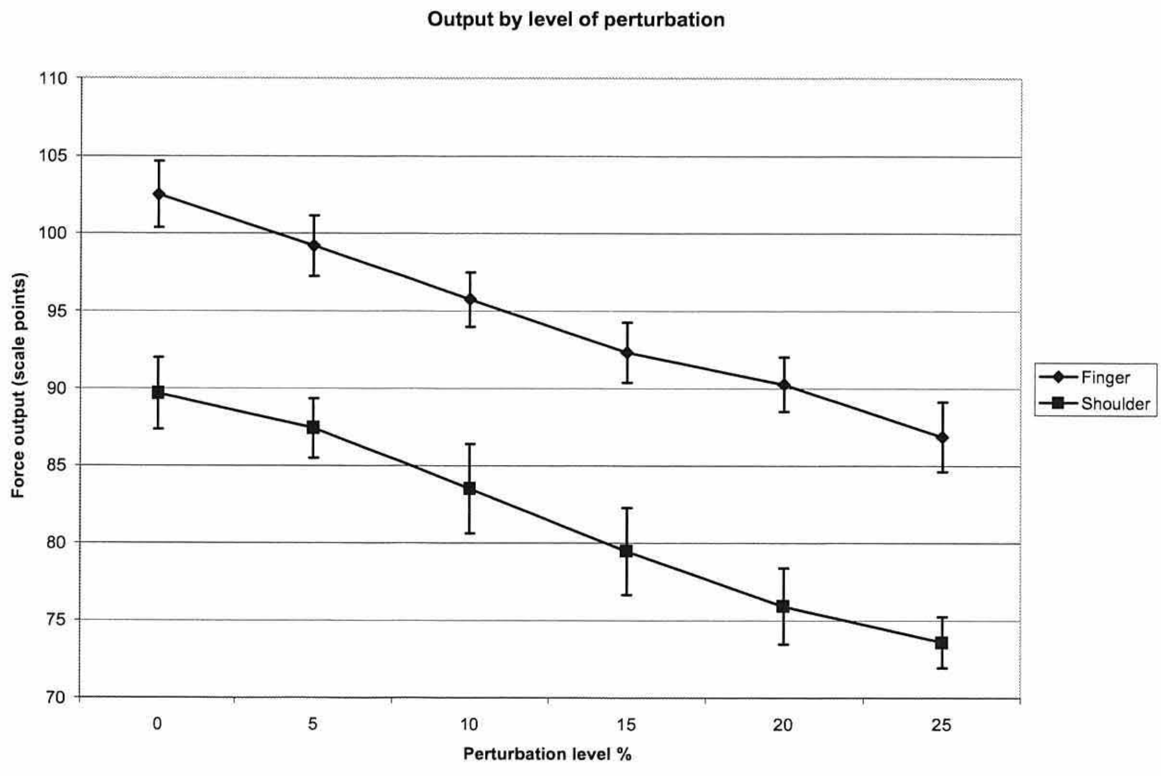
Results

For all Analyses of Variance, Huynh-Feldt adjusted probabilities are reported where sphericity assumptions were violated. In the event of a severe violation of these assumptions i.e. $\varepsilon < 0.75$ Greenhouse Geisser adjustments are used (Stevens, 1988).

Force output analysis

Participants performed five trials at each level of the perturbation condition, which were then averaged to provide one force output score at each level of perturbation per participant. ANOVA with repeated measures on the trial scores 2(Task) x 6(Perturbation), revealed a significant task main effect ($F(1,22) = 75.15$ $p < 0.001$), and a significant main effect for perturbation ($F(5,110) = 128.86$ $p < 0.001$). Tukey's HSD revealed significant differences between all levels of perturbation at the 5% level. The data are summarised in the graph that follows.

Figure 2-1. Graph of output by perturbation levels.



The significant results here indicate that participants in the perturbation conditions were capable of producing statistically distinct output at each level of perturbation. This would imply that for each level of perturbation participants made a corresponding correction.

Participants verbal responses during the perturbation phase

The aim was to search for differences in ability to identify change at different levels of perturbation and to see if the ability to identify change varied according to the movement used. Subsidiary to this, was to what extent the frequency of responding differed from that expected by chance. Participant's verbal responses across the six perturbation levels were summarised to provide a total number of correct identifications at each level of perturbation. Initial analysis was performed using a repeated measures 2(task) x 6(perturbation level) ANOVA with Tukeys' HSD pairwise comparisons as follow up tests. As there were five trials at each level of perturbation, participants could be expected to estimate the presence or absence of a perturbation on average 2.5 times by chance alone.

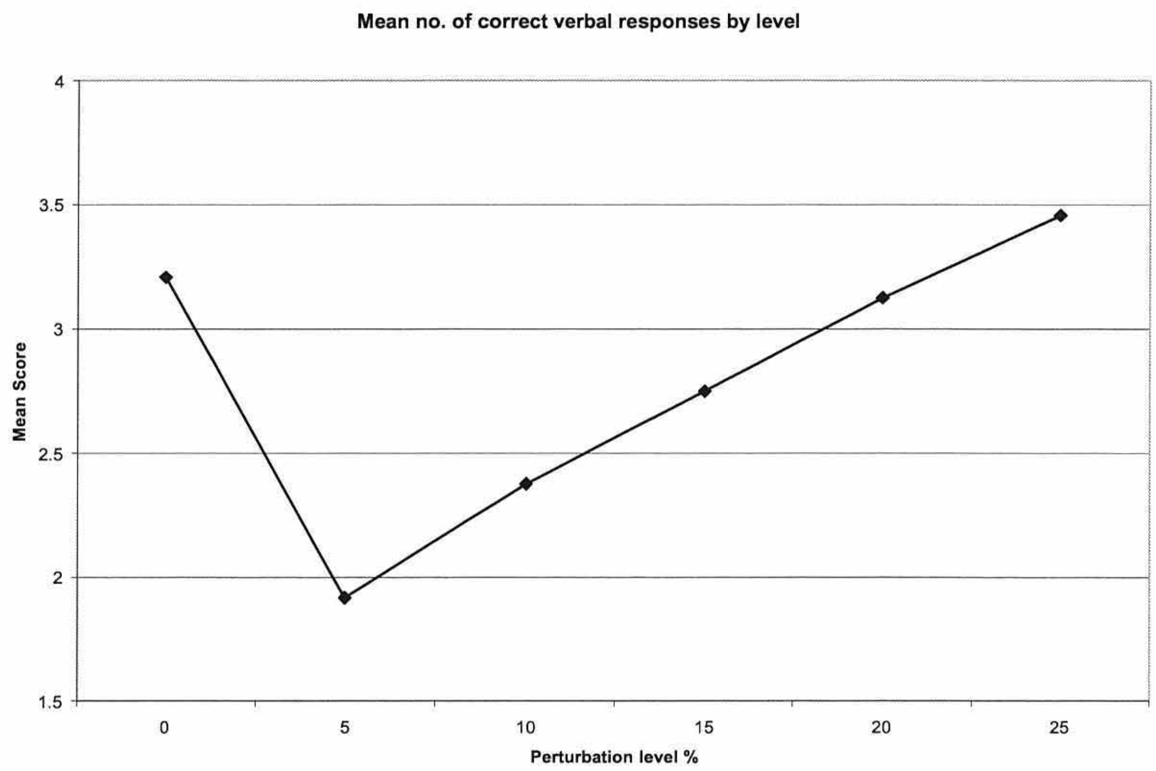
A 2(Task) by 6(Perturbation) ANOVA with repeated measures on perturbation level revealed a main effect for perturbation level only ($F(5,110) = 7.09$ $p < 0.001$). Tukey's follow up tests with α set at 0.05 showed a significant difference between the 5% level of perturbation and the 20, 25 and 0% levels of perturbation. This was due to the very low frequency of correct responding. Identification of perturbations at the 5% perturbation level that was somewhat less than 2.5 (chance). This would be the result of participants believing that a 5% level of perturbation was the same as 0%. The data are summarised in the table below.

Table 2-1. Mean number of correct verbal responses by perturbation level.

	Mean number of correct verbal responses by perturbation level					
Level %	0	5	10	15	20	25
Mean	3.208	1.917	2.375	2.75	3.125	3.458
S.D.	1.215	0.929	0.924	1.294	0.9	1.318

Note that the direction of the difference at 5% and 10% perturbation is opposite to all the others, this would correspond with a tendency to describe perturbations of 5%and 10% as no change. The particularly low frequency of correct responses at the 5% level of perturbation would suggest a very definite willingness to describe %5 perturbation as none at all. Means for correct scores around 2.5 would suggest an ability to correctly identify change no better than guessing. Thus the data seems to present an area of certain identification at the 0% and 5% levels, followed by a range of uncertainty (10-15%) returning to a degree of certainty at the 20% level. This is more clearly illustrated by the diagram presented figure 2-2.

Figure 2-2. Graph of correct verbal responses by perturbation levels.



Discussion

The data here presents strong support for the proposition that participants have limited awareness of their own motor performance. There is a clear mismatch between conscious detection of perturbation and responses to those perturbations. It seems that participants were unable to feel much of what they were doing when they had visual information available to them. Participants, although unaware that change was taking place in this experiment, exhibited the capacity to produce very accurate output in response to perturbation. Evidence for this is provided by comparison between the changes in force output brought about by each level of perturbation (fig.2-2), and the frequency of correct identifications at each level of perturbation (table 1). At the 5% perturbation level participants succeeded in producing a force output statistically distinct from that at the 0% and 10 % level, but did not appear to recognise this level of output as different from normal responding. Indeed the mean number of correct identifications at the 5% level, indicates that participants tended to believe 5% perturbation was the same as none at all.

An indication of how certain participants were in identifying perturbation was assessed by comparing the frequency of verbal responses to that of chance (2.5). This would suggest that beyond the 5% level of perturbation there follows an area of uncertainty in identifying discrepancy (10 & 15%), where the mean scores fall very close to 2.5 (chance). This is followed by increased certainty (20 & 25%) as mean scores move away from chance. These results do not support the idea of a definite threshold for identifying mismatch between what is seen and what is felt in this task.

Given substantial difference between the values at the 5% level of perturbation and higher levels, it is tempting to suggest that there is more than one important factor influencing decisions about discrepancy. A distinction might reasonably be drawn between those changes that were not detectable at the 5% level and those changes that were overlooked, or assumed to be part of natural output variability. This would offer support for two of the three proposed explanations for the results found by Fourneret & Jeannerod (1998); these are visual dominance explanation and the double coding explanation. Double coding suggests that information used to control and correct movement at a lower level is not made available to consciousness. In the context of this experiment, this would correspond with correct adjustments at the 5% level of perturbation going unidentified by participants. Participants responded to perturbation but consciousness remained uninformed. Visual dominance on the other hand, suggests that information about correction may well enter consciousness but is ignored as the visual display of activity remains largely consistent with expectations. Here, this may be seen as the uncertainty regarding change expressed by participants at the 10-15% of perturbation. In effect conscious information about movement may have been sufficient to identify discrepancy at 10% and 15% levels of perturbation, but it became critical or noticeable at a 20% level of perturbation. Given sufficient practice at the task, it is possible that participants would be able to make more accurate judgements at the 10% and 15% levels of perturbation.

An alternative to the visual dominance interpretation can be offered by looking at the transitory nature of movement information held in the working memory.
The verbal response portion of the task required the comparison of proprioceptive information from two separate trials, which would require an instance of both to be held in working memory. This is contrary to the general notion of working memory as a temporary resource. In this view memories for previous movements are overwritten by current movements in the short term memory. In support of this Georgieff & Jeannerod (1998) discuss evidence suggesting that memory for the previous instances of a task

would degrade in the presence of other information. This would make comparison difficult under the constraints of the task used here. Thus the area of uncertainty in discrimination shown at the 10% & 15% levels of perturbation might remain in spite of continued practice.

From the evidence presented, it seems clear that participants are able to make very accurate adjustments in response to perturbation. There are clear statistical differences between mean force outputs at all of the different levels of perturbation (figure 2-2). Correction to perturbation is most likely to take place in the early stages of responding. Bearing in mind that each trial consists of a string of ballistic presses on a dynamometer, participants when starting a new trial, have to make their first response based on the memory of previous responses. Any feedback or correction necessary can only be implemented in the formulation of a next response, due to the time constraints of the task. Therefore, it would be expected that perturbed trials would result in an initial overshoot on the first attempt at responding in a new trial, which could then be consciously or automatically corrected. When making the first response in a string, an initial overshoot would result in a biased mean output i.e. the mean output should be consistently larger that the target value. This would be true for both perturbed and normal trials, though perturbed trials should show an increasing bias as the perturbation gets larger. This in turn would mean that the scores for the perturbed trials should tend towards the normal state of responding (100) and that the mean for normal responding should rest just above the expected target. Data presented in figure 2 for the finger press task supports this idea. Instead of data points being distributed at 5% increments from 100 to 75 as might be expected, the data is biased towards the 100 mark. Although the data from the shoulder push task does not fall towards the 100 mark it still shows a bias in line with this notion.

No evidence was found to support a hypothesised difference between shoulder push and the finger press task in terms of ability to identify perturbation. The only difference shown in figure 2-2 is how close participants could get to the target goal. The participants performing the shoulder push seemed to consistently fall short of the target goal across all levels of perturbation. This may have been due to differences between the two types of force cell used in the experiment. It would seem that the larger force cell used for the shoulder push in this experiment was less compliant (springy) than that used for the finger press task. The lack of compliance may have caused difficulties with respect to meeting the time constraints of the task. In response many participants appeared to adopt a push and pull strategy. This strategy may have had a net effect on VE and RMSE which caused participants to consistently undershoot the target while responding. In spite of this, the overall implication is that changes in the scale of effector system had no statistical effect on the ability to detect discrepancy.

It is tempting to suggest that participants when performing a task of this type do not receive information about very small changes to goal movements. Whereas larger changes though detected may be ignored or overlooked until they reach a certain magnitude. The data presented here is not quite sufficient to support this view, however the results of Henry (1953) do offer some support to this notion. It is not clear if this latter situation would change as a consequence of practice. The fact that both a finger press and a shoulder push task showed a similar pattern of results indicates that this finding may generalise to other tasks of this type.

Chapter 3

The effect of display resolution and force output on the ability to detect change.

Chapter 3

Introduction

Experiment one (Chap 2) presented evidence indicating that participants have difficulty consciously identifying perturbations to a visually guided force production task, even though they are capable of accurately responding to those perturbations. Two possible interpretations arise from this finding. First, low level movement information does not enter or is not made available to consciousness for changes below a certain magnitude. Second, when such information is available to consciousness, discrepancy between what is seen and what is felt has to be quite large before the difference is acknowledged.

Experiment one involved participants producing a stream of paced periodic responses (20 responses of 1.17N or 14N at 0.5s intervals) in time with an auditory signal. Feedback about force output was provided through a Visual Display Unit (VDU), the signal gain of which was changed on certain trial blocks. Participants were required to identify in which trials they thought the gain had changed. Relatively small changes in gain (0-25% in 5% increments), led to correspondingly small changes in output. However, when participants were asked to identify trial blocks which had changed, they were unable to reliably identify changes of less than 15%. Indeed they reliably described changes of 5% as no change at all, in spite of making a compensatory response to the gain change. It would appear that in this situation participants have limited awareness of their own motor performance beyond the visual domain.

As stated before, reasons for this outcome have focused around two ideas, "double coding" of movement information and visual dominance. Double coding of movement information suggests that actual low level information used for the control of a movement, is not made available to consciousness, either during or after completion of the movement (Fourneret & Jeannerod, 1998). Indeed as the term suggests, potentially different and limited information about activity is fed into consciousness as a consequence of movement or corrections being made.

Visual dominance on the other hand, suggests that visual information about a task is used in preference to other internal sources of information which are available to consciousness. This preference for sometimes contrary information continues up to a critical limit, when conflicting information becomes difficult to ignore. Studies by Henry (1953) on blindfold positioning and Fourneret & Jeannerod (1998) using line drawing, support a double coding explanation in that they illustrate a level of movement correction that appears to be impervious to consciousness. Further support was found for either the double coding hypothesis or the visual dominance effect in Experiment one. It appears that when correcting for the 5% level of perturbation, information about change does not permeate consciousness. A corresponding correction is made, but the frequency of correct identification of change is so low as to suggest a substantial lack of awareness of perturbation. This is not the case at the 10-15% levels of perturbation. Here, corrections are made, but the number of trials where change is identified rises to a level close to chance, suggesting some uncertainty or a degree of conscious awareness. Beyond perturbation levels of 15%, the number of correct identifications of change rises to a level of some certainty. Thus it seems that above a certain level of perturbation, information about larger changes does find its way into consciousness, but the mismatch between what is seen and what is felt has to be quite large before change is identified with any certainty.

Two important questions arise from the existing findings. First, to what degree is the ability to detect change determined by the nature of the task ? Second, is the capacity to make decisions about change affected by the quality of the visual information available ? Given that there is some evidence for graded levels of detection in terms double coding (unconscious) and visual dominance (partly conscious) it may be possible to examine for differential effects resulting from changes in selected independent variables. For example, a change in force output requirement for the task may bring about a change in the threshold for unconscious change (low level 5%). Alternatively, changes in available feedback may alter the capacity to determine change at the 10-15% of perturbation, as more pertinent/reliable information is made available.

At a theoretical level, it is important to examine whether the results of Experiment one will generalise to other conditions. One set of conditions relates to whether the amount of force employed in the movement has any influence on the ability to detect change. Some evidence already exists from Experiment one to suggest that between tasks there is no difference in ability to detect perturbation. As well as being different movements, both of these tasks required different absolute levels of force output. Experiment one used both a finger press and a shoulder push as a movement. The shoulder push required 14N of output and the finger press only required 1.17N of output, which represents a substantial difference in force output. Analysis showed no difference between tasks in terms of the ability to detect perturbation. This would imply that thresholds for detection remain relatively stable across tasks and across force levels. This approach is unlikely to be particularly reliable, as the possibility remains that there may be differences in ability to detect perturbation within tasks. A reasonable question at this point, would be to examine the ability to consciously identify perturbation between two different levels of force output within a single task. Therefore, the question is not whether detection thresholds are linked to levels of MVC (relative force output), but whether they are linked to absolute changes in force output within a given task. Using the finger press task from Experiment one, a suitable test would involve two groups of participants completing the same protocol as in Experiment one, at different target forces.

With respect to thresholds of detection and changes in force output, Carlton and Newell (1993) showed that output variability rises in proportion to increases in force output. This result is supported across a range of values from as little as 1N to maximum voluntary contraction (MVC). The rise is said to obey a non-linear function with increases in output magnitude showing progressively smaller increases in variability. In other words, as the force increases the rate of change in for intrinsic output variability decreases. It can be argued that the threshold for detection of change rests somewhere above the level of intrinsic output variability for a movement. Hence as the force increases and intrinsic variability decreases, a smaller percentage perturbation would lead to identification of change. If force and intrinsic variability were linked in this way, then doubling force output should see some change in conscious detection threshold. In effect, an expected threshold for conscious identification of change would get smaller as output magnitude increased. This would be shown in a corresponding increase in the number of correct identifications of force change at low levels of percentage perturbation.

Alternatively, a more traditional view might argue that ability to identify change obeys Weber's Law. Weber's law when applied here would suggest that ability to detect change, alters in direct proportion to the magnitude of force output. That is to say, a difference threshold would be the same percentage of output across the entire range forces a participant is capable of producing. Studies of force perception support this view, indicating that increases in force magnitude cause a proportional increase in just noticeable difference (jnd) Jones (1986). The larger the force, the larger the comparison force has to be in order for a jnd to be recognised. In the context of the current investigation, this would suggest that the threshold for the detection of change would remain at the same percentage of output regardless of force magnitude. A best estimate of the Weber fraction for Experiment one would lie somewhere between 0.05 and 0.2 at force outputs of 1.17N. This shows some congruence with direct studies of force perception that have shown values between 0.09 and 0.13 for 0.54N (Ross & Reshke, 1982, cited in Jones, 1986), 0.3 (Engen, 1971, cited in Jones 1986) to 0.12 (Victor Raj et al., 1985, cited in Jones, 1986) at forces greater than 1.5N. Weber's law describes a linear function and if conscious detection thresholds obey this function, then there would be no change predicted in ability to identify perturbation at differing levels of force output.

As suggested earlier, the nature of visual information used by participants could have an effect on their ability to make discriminations. Specifically, physical limitations of the display such as viewable height and feedback resolution are of obvious importance. A VDU is a limited device with respect to displaying movement information in real time. This issue can be considered in terms of the actual space available on screen to illustrate change (height) and the way in which change is illustrated (resolution/refresh rate of screen). With respect to viewable height of display, it is possible that rather than the degree of deviation from the target being indicative of perturbation, it was how close the display came to the top of the scale. Thus, if the display scale disappeared out of the top of the viewing box on screen, a large perturbation was indicated and hence a positive identification made. Feedback resolution on the other hand, deals with the appearance of unit increments on screen. An increase in resolution has a similar effect to a magnifying glass. Small units which are indicative of low resolution, might cause difficulty when detecting small changes (10% perturbation or less). Thus adjustments to the scale in terms of range and feedback resolution, would test for the likelihood of either of these effects. Fortunately, both of these issues can be addressed by adjusting the number of display increments shown on screen to participants. An increase in display increments will lead to a smaller range of force being displayed in the same viewing space. One consequence would be, that the display scale is more likely to disappear out of the top of the viewing box. If participants were identifying change in this way a shift in identification threshold should match the shift in the point of disappearance. However, an effect that corresponds with display resolution is likely to be seen as improvement in ability to detect changes lower than this point.

Evidence concerning changes to feedback resolution/precision, presents an equivocal picture with respect to its effect on performance (Rogers, 1974; Smol, 1972; Gill, 1975; Newell & Kennedy, 1978; Salmoni, 1980). A particular problem with this area of research is the way in which feedback resolution has been manipulated. Whilst some experiments double or halve resolution, others change resolution by orders of magnitude e.g. 1/10th to 1/100th (Salmoni, 1980). Jumps in orders of magnitude seem most likely to have an all or nothing effect on outcome, whereas smaller manipulations are more likely to show a graded effect. As a consequence, potential experimental effects may have been lost in the large differences between manipulations used. The current approach would involve doubling of a target force, therefore some gradual change may be detected. A change in feedback resolution, implemented using the equipment employed in Experiment one will also test for any display-ceiling effects, as the ceiling will get lower with increases in resolution. This is because more information will be forced into the same linear space. The effect should be identifiable as different from the general effect for resolution, as it will correspond with a specific point on the output scale.

The current experiment seeks to determine whether conscious detection thresholds are affected by changes in force output requirements and feedback resolution. This will be accomplished by adopting the method used in Experiment one but increasing the required force and visual feedback resolution. It is hypothesised that changes in force output and feedback resolution will not alter thresholds for the conscious detection of change, regardless of appropriate changes in actual response. Any change in thresholds for detection will be indicated by differences in the number of perturbed trials correctly identified in the differing treatments.

Method

Participants

23 female and 20 male participants completed the experiment. All participants were undergraduate sport science students.

Design

Participants were randomly allocated to one of four groups. The groups reflected two treatments; Force level – high force 2.34N or low force 1.17N and Display scale - large scale $0.117Ncm^{-1}$ or small scale $0.0585Ncm^{-1}$. The groups were described as Low force-large scale (LL), High force-large scale (HL), Low force-small scale (LS), High force-small scale (HS).

Apparatus and task

Data acquisition was performed using a dynamometer fixed to the desk in front of the participant. The task required participants to produce a single downward force on the dynamometer with the distal portion of the index finger on their preferred hand, every 0.5 of a second for 11 seconds. Participants had to keep pace with a 0.5s timing signal, making 21-22 discrete responses across the 11 second period. These actions constituted the completion of 1 trial. Each single response within a trial produced an output that could be converted to a digital value between 0 and 1500. The force required to produce an output of 1000 points was 11.74N. In the low force condition participants were required to produce an output of 100 points (1.174N), while in the high force condition participants were required to produce a force output of 200 points (2.348N).

Visual feedback for the task was provided using a computer monitor placed 40-50cm in front of the participant. The display was a staged bar chart where the height of the bar indicated the magnitude of force. The bar was non-linear, giving a changing display of magnitude that varied according to experimental condition. For the LS (Low/Small) group this was 8cm for the first 80 points and 8cm for the following 40, for the LL (Low/Large) group this was 8cm for the first 90 points and 8 cm for the next 20. For the HS (High/Small) group the display was 8cm for the first 160 points and 8 cm for the next 80, for the HL (High/Large) group the display was 8 cm for the first 180 points and 8cm for the next 40.

All participants received each of six perturbation conditions, which were 0,5,10,15,20 and 25% increases in gain introduced via the VDU. The gain was introduced by multiplying the original signal by a decimal value corresponding to the change in gain required. Thus a 5% gain was implemented using the original signal multiplied by 1.05. The change in gain was designed to proportionally reduce the amount of force required to achieve the target goal by the set percentage in each case. The net effect was an increase in signal gain to the VDU that gave the required level of percentage perturbation at the goal force. Thus it appeared that less force was required to achieve the target goal during perturbed trials. Each participant received all levels of the perturbation variable (perturbation) five times. Overall participants were presented with 30 pairs of trials (one test, one normal), so that 60 trials were completed overall. Thus participants were "tested" 30 times 25 of which were perturbed trials.

The schedule of presentation for perturbation levels was controlled using a randomised block method. Although 21-22 responses were collected in each trial, only the first twenty viable responses were retained for analysis.

Procedure

For all groups the experiment was divided into a 20 trial practice phase and a 60 trial perturbation phase. The perturbation phase consisted of 30 "normal" trials (the same as in practice) and 30 "test" trials (during which perturbations were administered). The specification of the perturbed trials was according to a random schedule not known to the experimenter at the time of the data collection. All participants were given instructions on how to perform the task followed by 3 demonstrations necessary to clarify the task requirements. The experimenter answered all relevant questions at this time. Specific instructions provided by the experimenter included:

i) Participants should try their hardest at all times.

ii) They should not attempt a change of strategy during the course of the experiment.

iii) They should maintain the same hand/finger position throughout the experiment.

iv) Participants were informed that the priority of the task was not to synchronise exactly with the time signal but to try and keep reasonable pace.

v) The important priority was to get as close to the 100 target as possible at all times.

An additional set of instructions were provided during the perturbation phase in which participants were informed that this was a test of discrimination, where on specified "test" trials (alternate with normal trials) it might be that less force than normal would be required to reach the 100 target. Participants were informed that this change was randomly allocated between the test trials and knowledge of this allocation was not available to the experimenter. At the end of each "test" trial participants were required to indicate if they thought that test trial had been changed or not. Participants could answer with a yes or no only and these verbal responses were coded as a difference or no difference detected contingency.

Results

For the purpose of comparison output values at the higher level (200) are halved. Where significant violations of sphericity assumptions occurred, Greenhouse-Geisser corrected values are reported for $\varepsilon < 0.75$ and Huynh-Feldt values are for $\varepsilon > 0.75$.

Force output

Analysis of force output was performed using planned contrasts and a 2 (Force Level) x 2 (Resolution) x 6 (Perturbation) ANOVA with repeated measures on the last factor. The planned contrasts were to determine that for each level of perturbation there was a statistically distinct level of response made by participants and that the pattern of responses fitted a linear model. This was to determine that participants effectively responded to each level of perturbation.

Table 3-1. Planned contrasts for output between each level of perturbation

DISTLEV	df	F	Sig.
Level 1 vs. Level 2	(1,41)	154.116	p < 0.001
Level 2 vs. Level 3	(1,41)	61.104	p < 0.001
Level 3 vs. Level 4	(1,41)	153.125	p < 0.001
Level 4 vs. Level 5	(1,41)	74.132	p < 0.001
Level 5 vs. Level 6	(1,41)	43.562	p < 0.001

Results indicate that participants were able to produce statistically distinct responses at each level of perturbation. The data was best fitted to a linear model $F\ (1,41) = 682.350\ p < 0.0001$. Linear regression of all groups output against distortion level, revealed a significant positive regression (Rsq = 0.714, Beta = 0.845, $F(1,118) = 294.927\ \ p < 0.001$). This would indicate participants were able to make corresponding corrections to all levels of perturbation.

The only significant other effect shown was a Resolution by Perturbation interaction, this effect was marginalised as a consequence of using a Greenhouse-Geisser correction ($p = 0.057$). Observation of the means suggested that patterns of responding diverged at the highest level of perturbation. This would correspond with a display ceiling effect i.e. the display box was too small for the changes taking place at levels of 25% perturbation. Output data is summarised in the graphs that follow.

Figure 3-1. Graph of output by perturbation levels for the low force tasks.

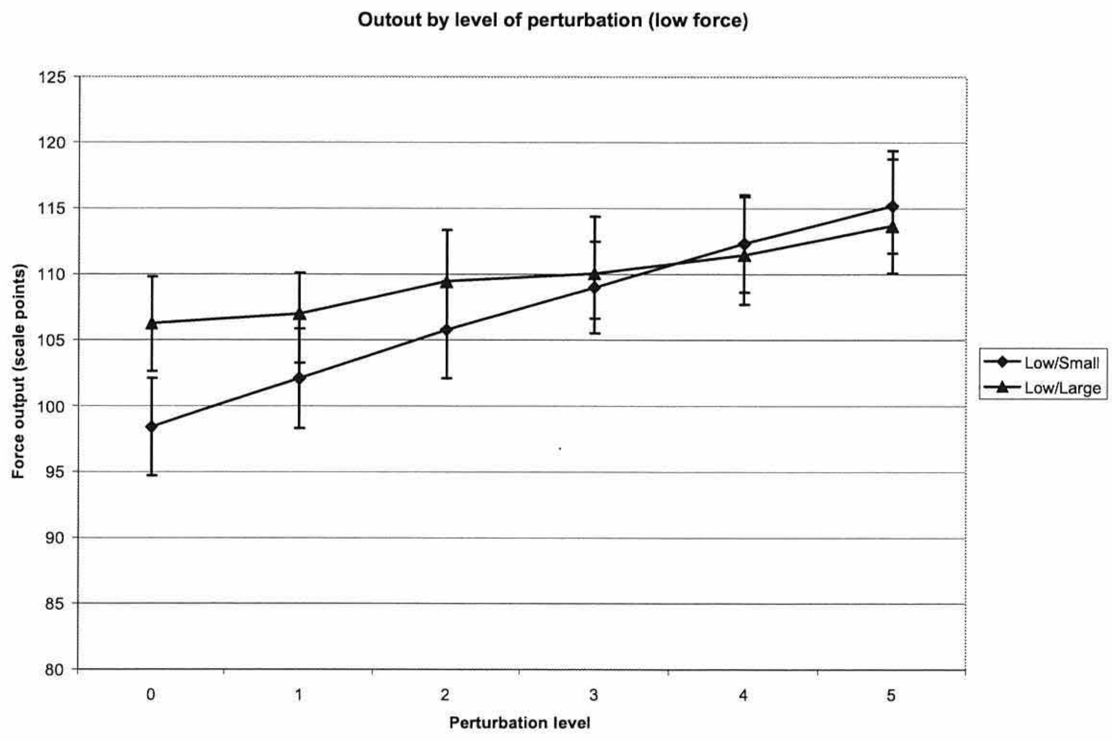
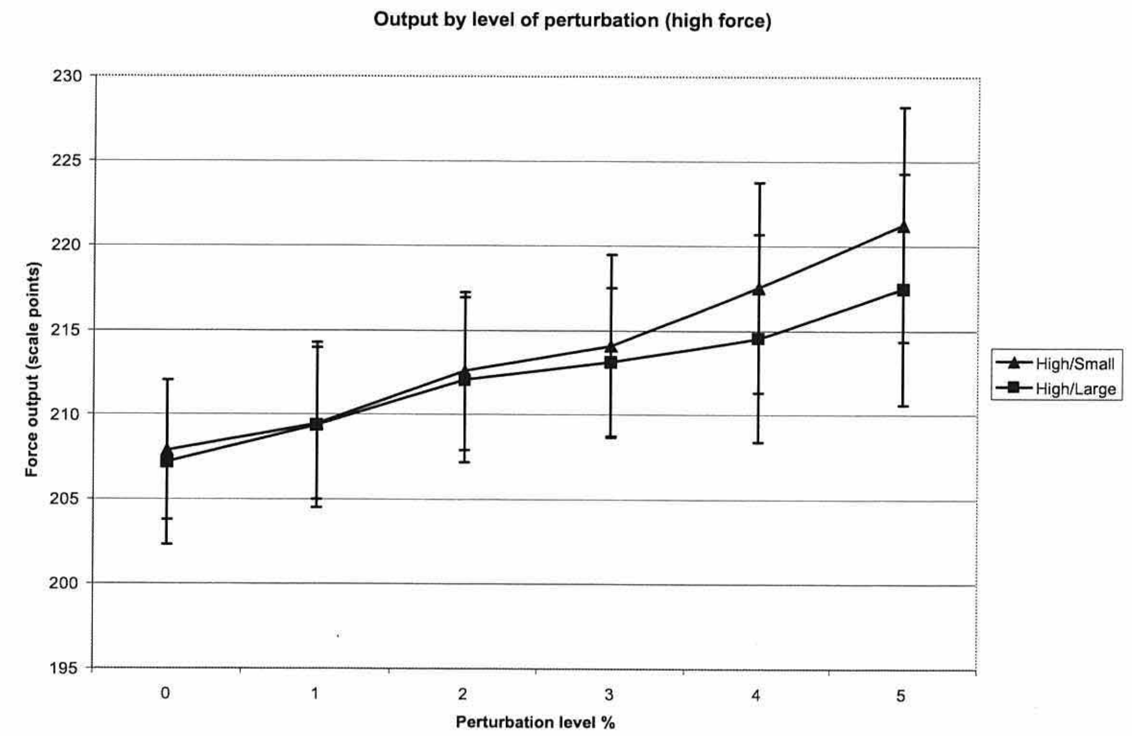


Figure 3-2. Graph of output by perturbation levels for the high force tasks.



In order to examine for changes in variability due to differences in force output requirements and display resolution a two way 2 (Force Level) x 2 (Resolution) was performed on Coefficient of Variation scores. The analysis revealed a significant main effect for force output $F(1,3)= 62.782$, $p< 0.0001$ and for display resolution $F(1,3)= 7.204$, $p< 0.011$. The main effect for force output an inspection of means confirms the expect increase in variability at higher levels of force output in line with Carlton & Newell (1993).

Participant's verbal responses during the perturbation phase

The main hypothesis to be tested in this experiment was whether different levels of force output and different resolutions of display gave rise to changes in the extent to which participants could correctly identify change. Participant's verbal responses across the six perturbation levels were summated to provide a total number of correct identifications at each level of perturbation. Initial analysis was performed using a 2 (Force Level) x 2 (Resolution) x 6 (Perturbation Level) ANOVA with repeated measures on the last factor. Tukeys' HSD ($p < 0.05$) pair-wise comparisons were used as follow up tests. The analysis revealed only a main effect for perturbation level, $F(5,210)=30.519, p< 0.001$. No other effects were significant. Follow up tests revealed significant differences between number of correct identifications at the 5% level and the 0%,15%, 20% & 25% levels and between the 10% and 25% levels of perturbation. The difference between the 5% level of perturbation and other levels was due to the very low frequency of correct identification. Identification of the 5% perturbation level was less than 2.5 (chance) unlike all the others which were greater than chance. This would be the result of participants believing that a 5% level of perturbation was the same as 0%. The difference between the 10% level of perturbation and the 25% level, can be taken to express increasing certainty regarding detection of a difference. The data seem to present an area of certainty at the low level (0-5%) followed by a range of uncertainty (10-15%) returning to a degree of certainty (20-25%) with respect to identification. This is not altered by changes in display resolution or output magnitude. Although no other significant effects were found, the test for a force main effect approached significance ($p = 0.09$) with participants in the higher force condition showing a larger number of correct identifications.

Table 3-2. Mean number of correct verbal responses by perturbation level.

	Mean number of correct verbal responses by perturbation level					
Level %	0	5	10	15	20	25
Low/Small	3 (1.35)	1.69 (0.75)	2.53 (0.78)	3.07 (1.19)	3 (1.08)	3.38 (1.33)
Low/Large	3.64 (1.03)	1.36 (0.81)	2.64 (1.29)	2.82 (1.17)	3.45 (1.04)	4.09 (0.83)
High/Small	3.27 (0.96)	1.18 (1.13)	2.09 (1.14)	2.64 (1.24)	3.09 (1.26)	3.90 (1.13)
High/Large	4.18 (0.87)	1.18 (0.87)	2.73 (0.79)	2.90 (1.09)	3.73 (1.35)	4 (0.77)
Overall	3.5 (1.15)	1.37 (0.88)	2.5 (0.94)	2.87 (1.41)	3.30 (1.17)	3.83 (1.06)

Discussion

In this experiment, the effect of changing feedback resolution and force output on the ability to detect perturbation was investigated. When participant's verbal responses were analysed, no main effects or interactions involving either force output or feedback resolution were reported. This evidence indicates that in line with current suggestions, thresholds for detection of perturbation remain constant across different feedback resolutions and force outputs. This supports earlier evidence from Experiment one, that thresholds also show consistency across effectors. The absence of changes in capacity to detect perturbation at different force levels suggests that thresholds for detection are a relative percentage of existing force output and effectively obey Weber's law. However this conclusion is not indisputable. It is not clear from these results whether or not doubling the force would bring about a sufficient change in output magnitude, to elicit a correspondingly large change in output variability, i.e. one that would be detectable using the current method. Indeed, studies by Carlton & Newell (1993) covered a much broader range of forces than those involved in the current experiment. The fact that an effect for force approached significance might support such an argument. The direction of change for identification is in the correct direction to support detection thresholds decreasing as force increases within task.
Therefore, these results are unable to distinguish between the Weber function and the threshold predicted by the force variability function described by (Carlton & Newell, 1993). The thresholds might be expected to be quite similar at lower levels of force output but not if one were at a lower level and one as a higher proportion of MVC. What is realistically required, is examination of this effect at progressively higher percentages of MVC. Whether or not conscious detection thresholds are found to obey a linear or curvilinear function, does not detract from the suggestion that differences

between the ability to consciously detect change, and the ability to respond to change are likely to occur across a range of force outputs and effectors.

The lack of variation in conscious thresholds resulting from changes in feedback precision, permits a more detailed evaluation of arguments proposed in Experiment one. The two main resources for making decisions about whether or not a movement has been perturbed, are visual information acquired during the task, and comparison with earlier correct trials of kinaesthetic/internal information from the current trial.
It was suggested that participants may acquire information about when a change has taken place, from the extent to which they overshoot the target in the early stages of a perturbed trial. Given that increased feedback resolution would magnify this tendency, a dependence on this information should have increased the ability to identify change at certain levels of perturbation. The fact that changes in the visual information presented had no impact on the conscious thresholds for detection, would indicate a limited role for information acquired in this way. This would seem to place more emphasis on judgments based around kinesthetic information. With practice, the situation may change and visual information may grow in importance for making decisions.

To conclude that very precise feedback is redundant in tasks may be misleading, as this particular experiment deals only with conscious detection.
As Experiment one illustrates, the ability to make use of precise information at an unconscious level is definitely present. Furthermore, as Newell and McDonald (1994) have shown, the capacity to exploit increased feedback precision is limited by the properties of the effector system involved. In particular, the limitation appears to be linked with the number of available degrees of freedom. If thresholds for conscious detection are independent of changes in feedback precision but output is not, a valid test of this would be to check for changes in output variables such as CV and RMSE when

manipulating feedback precision. It would be expected that the output variables would change but conscious detection thresholds would remain the same. This might in turn contribute to an argument describing the relative independence of information used for control and the kinaesthesia received at the conscious level.

In light of these results, it seems reasonable to suggest that the difference between conscious detection thresholds and actual output is not the result of feedback properties or absolute thresholds of force output. Furthermore, this effect can be generalised across effectors when performing the same task. It is probable, that this effect will be maintained across a wide range of forces regardless of whether it can be described by a linear or curvilinear relationship. It is evident that activity at a lower level continues with some accuracy in the absence of conscious recognition, but why this is the case is not obvious.

Chapter 4

Knowledge and conscious control of motor actions under stress: A re-examination.

Chapter 4

Introduction

Investigation of performance breakdown under pressure or "choking" (Baumeister, 1984), has recently focused on issues of skill acquisition. In particular a number of papers have looked at the differential effects of learning a skill either implicitly or explicitly, and how this influences performance under anxious conditions (Masters, 1992; Hardy, Mullen & Jones, 1996; Bright & Freedman, 1998). Masters (1992) original study, suggested that performers who learned a task implicitly will not suffer the same performance decrement under pressure as those who learn the task explicitly. He argued that this effect was due to the explicit learning group having a pool of verbalisable knowledge available to them, which they consciously "re-invested" in the task when anxious. Re-investment is defined as using explicit knowledge acquired early on in learning a task to control present performance. This act of re-investing explicit knowledge in the task was thought to be to the detriment of performance by causing a regression to conscious forms of control.

The original Masters (1992) study involved participants learning a putting task under differential conditions. Amongst the learning conditions was one in which participants learned the task with the aid of a script (explicit) and another where participants had to practice whilst performing an articulatory suppression task (AST) during practice (implicit). The AST was intended to prevent the acquisition of explicit knowledge about the putting task. Both groups were tested under stress conditions and the performance of the implicit group continued to improve, whilst the explicit group did not. Unfortunately there are problems with the interpretation of this result, as the implicit group did not have to perform the AST during competition. This problem makes it unclear whether the results are the product of differential learning, or the removal of a cognitive load (AST) during competition for the implicit group.

Two papers have re-examined the original work of Masters (1992) with this issue in mind. The first was by Hardy et al. (1996), who replicated the method of Masters (1992), with the addition of an extra implicit learning group. The new implicit learning group had to perform both the putting task and the AST under stressed conditions. The requirement to perform the AST whilst putting in the stressed condition made no significant difference to the performance of the extra implicit learning group. Thus Hardy et al. (1996) found support for Masters (1992) original interpretation, that putting when learned implicitly is resistant to the effects of stress. This resistance may be explained by participant's being unable to re-invest explicit knowledge in the task when anxious, because they have none to re-invest. This resistance is to the advantage of implicit performers despite the additional dual task load under the high stress condition. However, in a further attempted replication, Bright & Freedman (1998) failed to confirm the original Masters (1992) finding.

Bright & Freedman (1998) suggested that any differences in performance were the consequence of the AST being present during the learning phase of the experiment but not during testing under stress for the implicit learning group. Support for this was presented in an interaction effect, whereby the only improvement in performance was between the final practice trials and the test phase for the IL group (AST removed). There was no significant improvement for the implicit group that had to perform the AST under the stress condition. This appears to be the opposite of the Hardy et al. (1996) results, where significant improvements were found for both the implicit groups regardless of the presence of the AST. It is notable that in neither case was there a significant increase or decrease in performance made by the explicit groups during the same test period. The only support for re-investment theory, rests with improvements for implicit groups but no matching improvements for explicit groups. This is contrary to obvious expectations of a decline in performance for the explicit groups, however such a decline might only be expected when large improvements due to learning effects no longer take place.

Unlike the replication of Hardy et al. (1996), the approach of Bright and Freedman (1998) was not a complete replication of Masters' (1992) method. There are three potentially important differences in the method used by Bright & Freedman (1998) that might explain the difference between the two replication studies: One is the amount of practice that participants received before testing. Another is the structure of that practice and finally there is an issue of participant screening or recruitment.

In line with the original study by Masters (1992), Hardy et al. (1996) gave participants 4x100 training trials and a 100 trial test. Bright & Freedman (1998) gave participants 4 x 40 training trials and a 40 trial test. Although some learning would have occurred after 160 trials, it is reasonable to suggest that substantially more learning would have taken place after 400 trials. In support of this point, it should be noted that participants in the Hardy et al. (1996) and Masters (1992) study continued to improve after some 400 trials of practice. When performance in a near identical task is still improving after twice the amount of practice, it seems reasonable to examine what difference this would make on the outcome of these studies. Previous studies involving implicit learning have used similar numbers of trials, if not more than Masters (1992), most often over a number of days. Wulf & Schmidt (1997), Magill & Hall (1987), Pew (1974) all used 14 days of 24 trials, Green & Flowers (1991) used 800 trials over 5 days, Cohen, Ivry & Keele (1990) used 10 x 100 in a day. Automaticity studies are known to use as many as 2000 trials over several days (Schiffrin & Schneider, 1977).

In this light, the 160 trials in one day used by Bright & Freedman (1998), might be viewed as insufficient to imply substantial learning or indeed effective implicit learning.

According to "stages of learning" models such as that proposed by Fitts & Posner (1967), learning is thought to proceed from a Verbal, to Associative, to Autonomous stage. An important consequence of transition from verbal to autonomous processing is the lower cognitive demands made by the tasks during performance. Following a relatively small amount of practice, it is likely that participants are still at a verbal stage, which it is argued is quite demanding on general cognitive resources. If it can be accepted that one of the consequences of learning is a decrease in demand on general (working memory) and verbal-cognitive (articulatory loop) resources, then the impact of an AST (verbal task) on performance is likely to be less following 400 trials of learning, than it is at 160 trials. This reflects not only increased proficiency at the task to be learned, but also increased proficiency at performance of the AST itself. Progress towards autonomous processing is more likely, given the 400 trials of practice undertaken by participants in the experiments by Hardy Mullen & Jones (1996) and Masters (1992). Hence, release from the demand of having to perform the AST at 160 trials of practice, is likely to have far more benefit for performance than a similar release at trial 400. The change in impact of the AST is likely to be due to performers being less proficient at both tasks in the earlier stages of learning. In turn, this would explain the increase in performance for the IL group during testing in the Bright & Freedman (1998) experiment, an effect not found in either Masters (1992) results or those of Hardy et al. (1996).

To reinforce their point, Bright & Freedman (1998) performed a second experiment in which two groups had to learn the putting task under differential AST loadings, one group was required to call out letters at a faster rate than the other.

Between the last practice session and a test session where participants did not have to call out letters, there was a significant interaction between the groups. The high demand group, which had been performing less ably than the low demand group, significantly improved in performance sufficient to surpass the low demand group. It was clear from this that the AST does have a significant impact on performance. Notwithstanding, the experiment was once more completed with a 160 trial practice phase and a 40 trial test phase, so is still unclear if results would have been the same after 400 trials of practice. What is worth noting, is that as a precautionary measure both Masters (1992) and Hardy et al. (1996) chose to increase the load on the AST across learning in the experiment. This was to compensate for improvements in performance of the AST and the associated reduction in cognitive load. The additional load was introduced after 300 trials of practice. This action seems justified, as it does not appear to have caused a significant loss of performance when introduced. Though still effective at blocking acquisition of explicit information, the AST by this stage in learning appears not to present as large a cognitive load as it would earlier in learning. Therefore, it would seem quite important in this light to compare the effect of the AST on the same group after both 160 and 400 trials of practice.

Another consequence of extended practice for the IL groups is that it should result in a more automated, robust and less resource-consuming mode of movement control for putting (Reber, 1993; Green & Flowers, 1991). This state should render the movement less susceptible to interference resulting from changes in the demands of the AST or the stress condition. For the EL groups the result of extended practice should be that more explicit knowledge about the task is acquired. As a consequence, there ought to be a larger pool of explicit knowledge for participants to "re-invest" under stressful circumstances. Therefore more practice should increase the likelihood of the EL group suffering a decrement in performance because more knowledge is available to re-invest. It can be argued that 're-investment' relies on the proposition that some, if not all verbal knowledge, has been 'abandoned' in the transfer to an autonomous mode of control. There are some grounds from Masters' (1992) and Hardy et al.'s (1996) studies to suggest that this abandonment has not been complete after 400 trials of practice. Therefore, the evidence would seem even less convincing after the 160 trials prescribed by Bright & Freedman (1998). For EL groups, it is quite possible that early on in practice participants have not got beyond a verbal-associative state. In this case they will still be using explicit verbal information to control many aspects of the task. Therefore the notion of re-investment of explicit knowledge seems somewhat redundant, as this information may currently be in use. In this way, without any re-investment of knowledge, no decrement in performance can be predicted for the EL groups. This may explain the lack of significant decline in performance, not only for the EL group in the Bright & Freedman (1998) study but also in those by Masters (1992) and Hardy et al. (1996).

As noted earlier, amongst the differences between the two replications were the volume of practice and its structure. Hardy et al. (1996) administered trials and testing over five days, Bright & Freedman (1998) completed the entire experiment all in one day. With respect to the distribution of practice, it would appear from what evidence exists, that practice distributed over several days would lead to better learning of the task (Baddely & Longman, 1978; Bourne & Archer, 1956; Murphy, 1916 cited in Schmidt& Lee, 1999). In the context of the current discussion, this is likely to have the same consequences as quantity of practice discussed in the previous paragraph i.e. that removal of an AST after 160 trials of practice will have more impact than when removed at 400 trials of practice.

In the final training session of the Hardy et al. (1996) experiment the percentage of putts holed was close to 40% for the EL group and around 30% for the IL groups. In comparison, the EL group in the Bright & Freedman (1998) experiment achieved no better than 25% and the IL groups appeared to hole around 12% of putts attempted. These figures would seem to indicate that the amount and method of practice did make a difference to participants' ability to successfully complete the task, supporting the argument above. Given that the ability to perform the task is affected by the structure and volume of practice, it is reasonable to surmise that responses to stress (testing) and changes in cognitive load (removal of AST) will also change as a result.

A further issue raised by Maxwell, Masters and Eves (2000) in a discussion of the two replications, questions the method of participant selection by Bright & Freedman (1998). They suggest that Bright & Freedman (1998) may have used lax criteria for selecting participants to take part in their investigation. The result of this change was that participants with some putting experience were allowed to participate in the experiment. This would have brought about a change in the expected effects of the experimental protocols by reducing the effect of the AST and increasing the pool of verbalisable knowledge that could be re-invested by the IL groups. The consequences of this confound are not made clear. In support of their argument, Maxwell et al (2000) draw attention to the differences in the mean number of explicit rules generated by the IL groups in Masters (1992) and Bright & Freedman (1998): 1 vs. 3.5. It is perhaps worth noting that some of the evidence to support this argument does not appear in the original Masters (1992) publication and that the mean number of explicit rules cited in Maxwell et al. (2000) was not matched in the replication by Hardy et al. (1996), whose study revealed an average of 2.75 rules for the IL group and 4.88 rules for the IL plus AST group. Perhaps an equally valid explanation for this difference would be that there was some disparity in the questioning and the judging criteria used for assessing accretion of explicit rules in these experiments.

Although Bright & Freedman (1998) do present some evidence that may be argued as challenging to the notion of differential effects for implicit and explicit learning, it is not clear to what extent their results could also be explained in terms of different the methodologies employed by themselves and Hardy et al. (1996). With this in mind, the current experiment seeks to complete a replication of both methods using the same groups on each occasion. The general goal will be to determine if the differences between the two sets of results can be explained in terms of the amount and distribution of practice. Critical to this argument, would appear to be an expected interaction between groups that have to perform the AST during practice but not during testing. It is suggested that a non-AST group should get significantly better than an AST group in the first test session but not in the second.

Method

Participants

Thirty-two undergraduate volunteers were randomly allocated to one of four treatment groups: Explicit Learning (EL), Implicit Learning (IL), Implicit Learning with AST 1 test (ILAST1), Implicit Learning with AST 2 tests (ILAST2). Participants were right-handed first year undergraduate sport science students with no previous golfing experience. Only participants with no previous psychological skills training or experience of psychology courses were selected.

Apparatus

A putting surface of an identical design to that employed by Masters (1992) and Hardy et al. (1996), was used in the current study. This consisted of an Astroturf putting surface with 1.5m between the start line and the hole. There was a 1m long 25% gradient between the two points which commenced 30cm from the start line. The hole itself was 10.8cm in diameter, in accordance with United States Professional Golf Association rules. Regulation white golf balls (4.27cm in diameter) and a standard putter (88.9cm in length with a standard angle of lie and loft) were used by all participants. Heart rate was measured by means of a Polar Electro Sport Tester PE3000 heart rate monitor. Use of the PE3000 heart rate monitor requires that a chest transmitter and a wrist-based receiver be used. This type of heart rate monitor was chosen in preference to devices that could be attached to the ear or the fingers, as these methods were thought to be more invasive. Heart rates were recorded at 5s intervals for 180s in total, for each measurement period. Heart rates were recorded for download twice when the stress manipulation was delivered. The heart rate monitor was worn for all sessions throughout the experiment. An electronic metronome was used with a 1.5s time-base as a prompt for the Articulatory Suppression Task.

Design

The experiment was spread over 5 days and consisted of two phases: Phase one was 4 sessions of 40 putts practice separated by a short rest followed by a stress test/competition of 40 putts, this was all completed on day 1. Phase two was 2 sets of 50 putts separated by a short rest on days 2-4 followed by a stress test/competition on day 5 consisting of a further 2 sets of 50 putts under stressed conditions. Phase one allowed for some 160 trials of practice before test 1, Phase two allowed some 500 trials of practice before test 2 if trials on day 1 were counted as practice. All but the ILAST1 group were tested under increased stress conditions on days 1 and 5. For the ILAST1 group only, the first test phase was omitted and treated as a further 40 trials of practice. The ILAST1 group was included to control for possible carry-over effects resulting from the stress manipulation in test 1. All sessions took place on consecutive days at approximately the same time of day.

Each group was required to perform according to one of four separate learning conditions

EL group – Before each practice session began participants in the explicit learning groups were requested to read carefully a set of instructions on how to putt. These instructions were identical to those used by Masters (1992) and Hardy et al. (1996) which were compiled using two reputable coaching sources (Saunders & Clark, 1977; Stirling, 1985; appendix a). These instructions were chosen over those used by Bright & Freedman due to their availability and perceived reliability. It was impressed upon participants that they should read these instructions carefully and follow them as closely as possible. The instructions were not presented during the final stress test.

ILAST groups – Participants in the ILAST groups were given no instructions on how to putt before starting the task. Instead they were required to perform an Articulatory Suppression Task (AST) whilst putting. This was the same AST used by Masters (1992) and Hardy et al. (1996), which was in turn based on a procedure first outlined by Baddely (1966). Participants were required to call out a random letter each time an electronic metronome "clicked". For the first 400 trials in the experiment, clicks were timed to occur every 1.5s after this, subsequently clicks sounded every second. Participants were required to prioritise random letter generation, which meant that rather than stop generating letters they should stop putting. The importance of the generated letters being random was emphasized to the participants. The reduction in the time interval between clicks was designed to maintain the difficulty of the AST, thus continuing the suppression of explicit knowledge throughout the skill acquisition phase. In line with earlier studies, it was assumed that an inter-click interval of 1-1.5s would be sufficient to suppress any acquisition of explicit information about the putting task.

The ILAST1 and ILAST2 differed only insofar as the ILAST1 group received only the second of the two stress treatments and the earlier treatment period was used as practice. The ILAST1 group was introduced to the study in order to check for possible carry over effects resulting from the first stress manipulation.

IL Group – The IL group like the ILAST groups, was required to learn the task whilst performing the AST. The only difference was that during both of the stress treatments they were not required to perform the AST.

Procedure

Prior to each acquisition or treatment period, participants were fitted with a heart rate monitor. They were then requested to sit quietly for five minutes to allow baseline measures to be obtained. During this time participants were given written instructions concerning the nature of their participation and what was expected of them during the session (appendix b,c). Standard instructions included the request that they not think about or rehearse the task while away from the experiment and that they were not to discuss the experiment with any other party. They were informed that the purpose of the experiment was to examine the effect of different practice conditions on learning, that their participation was entirely voluntary and they could withdraw their consent to participate at any time. No time constraints were imposed on participants.
Group specific instructions were also administered at this time (appendix d). Participants' heart rates were then monitored for a period of three minutes in the baseline and stress sessions. In the high stress sessions, specific stress inducing information was given to the participants in the middle portion the three minute heart rate monitoring period (appendices e, f). The putting sessions were broken up into sets of 40 putts, separated by a 5 min rest on the first day and 50 putts separated by 5 min on subsequent days. The first set of putts commenced at the end of the 3 min rest period. The primary dependent variable was the number of putts holed.

Stress intervention

At the start of the treatment phases of the experiment, participants were informed that they were to participate in a competition for a given prize. They were told that although there was only one prize, winning involved the efforts of the entire group. It was explained that the best performer in the best group would get a major prize and the rest of the group would receive minor prizes to be administered at the end of the experiment. With the exception of the ILAST1 group, participants were informed that the competition was cumulative across two sessions during the experiment.

In addition to this information a video camera was set up "to record their activities for evaluation". The camera was switched on but the taped material was never used.

This is a departure from the methods used by both Masters (1992) and Hardy et al. (1996) and is in some ways more like a modification of the intervention employed by Bright & Freedman (1998). However, the stressor does include a combination of financial incentive, competition and social evaluation as in previous studies.

The stressor information was delivered during the middle 60s of the 180s heart-rate monitoring period, participants were given a standard statement to read outlining the competition conditions. In this way, stress was induced by a combination of incentives and evaluation. It was hoped that the use of a team prize would maintain motivation in spite of poor performance by individuals. As in the Masters (1992) and Hardy et al. (1996) studies, this was thought to be necessary as a defensive measure against participants feeling that their performance was so poor it was pointless to continue making an effort. Because heart rate was being measured throughout this period, it was possible to obtain an index of the physiological response to the intervention by comparing the initial 60s period, with the final 60s of the monitoring period.

A significant increase in heart rate was accepted as indicating an increase in performance apprehension. Following the measurement period the stress trials began.

Stress measures

Unlike Masters (1992) and Bright & Freedman (1998), the psychological tool for measurement of stress response was not the State-Trait Anxiety Inventory (STAI; Spielberger, Gorsuch & Lushene, 1970) but the Competitive State Anxiety Inventory - 2 (CSAI-2; Martens, Burton, Vealey, Bump & Smith, 1990). This was in line with arguments presented by Hardy et al. (1996) to the effect that this was a sport specific inventory that measured particular separate subcomponents of competitive anxiety and was therefore more informative as to the nature of the stress response. The CSAI-2 also seems most appropriate given the nature of the stress treatment employed here, as the task involved is putting and the stressor involved explicit competition. Internal consistency for the CSAI-2 has been shown to be of adequate standard with Cronbachs alpha coefficients ranging form 0.70 to 0.90 (Gould, Petchlikoff & Weinberg, 1984). Concurrent validity has also been confirmed by Martens et al (1990) who obtained predicted relationships between the CSAI-2 and an assortment of trait measures.
The CSAI-2 was administered pre and post-stress intervention, to assess each participant's levels of cognitive and somatic anxiety. The CSAI-2 was administered during the inter-trial interval between sessions three & four and following the stress intervention before testing on day 1. The CSAI-2 was also administered during the inter-trial interval on day 4 and following the stress intervention on day 5 before first putting session. The wording of the CSAI-2 was slightly modified to alternate the term practice or competition to match respective baseline and stress measures. A modified version of the CSAI-2 was used with the word practice substituted for the word competition in the rubric, when baseline measures were taken during practice.

In line with Masters (1992) and Hardy et al. (1996), time to completion was measured for each putting session. This measure was included as it was originally hypothesised by Masters (1992), that time to completion would increase as participants took extra time to prepare for each putt. Time to completion provides an indirect test of the Processing Efficiency Theory (Eyesenck & Calvo, 1992) in that it can be used as an index of effort, whereby increased time taken to complete a putt is indicative of additional effort expenditure.

Verbal Protocols

In order to test the prediction that the EL group should have acquired more explicit knowledge about the task than the other groups, it was necessary to assess the extent of participants' explicit knowledge using a verbal protocol. This was in line with the notion that individuals learning implicitly, would accumulate less explicit knowledge than those learning using explicit rules. After completing each of the test sessions participants were required to list all of the information that they thought was relevant to making a successful putt. They were asked to use information that they had become aware of over the five putting sessions. In line with all other studies of this type, the written protocols were scored by summing the number of explicit rules each participant wrote down. Explicit rules were defined as those statements that related to the instruction set used, or mechanical/procedural aspects of the task. Statements outside of these criteria such as those relating to feelings, were excluded from analysis. The credibility of the explicit rules identified was checked by "Investigator triangulation" (Lincoln & Guba, 1985). This was done in line with the modification used by Hardy et al. (1996) to increase the validity of this measure. The method involved two researchers analysing the protocols independently using the same criteria. Checking one list against the other then allowed corroboration of the explicit rules elicited.

Results

Verbal protocols

In line with Hardy, Mullen & Jones (1996) a priori or planned contrasts were employed to test expected directional effects. Testing for these effects has been identified in previous research as necessary to verify the effectiveness of the stress intervention and the learning manipulation. In these circumstances further exploration of the data or examination of other effects was deemed unnecessary hence planned contrasts were used.

It was predicted that the EL group should have a significantly larger pool of explicit knowledge than the IL, ILAST1 and ILAST2 groups together, and all groups would increase their explicit knowledge across the two test sessions. In line with the role of the ILAST1 group as a control group, no explicit rule measures were taken following the first test session. Therefore, the ILAST1 group was excluded from this analysis. A two way Group(4) by Test(2) analysis of variance with test as the repeated measured revealed a significant main effect for group ($F(2,21) = 11.980$ $p < 0.001$), but the test variable only approached significance ($F(1,21) = 3.628$ $p = 0.071$). A planned contrast of the group effect showed the EL group to have significantly more explicit knowledge than the other groups ($F(1,21) = 23.96$ $p < 0.001$).

Table 4-1. Mean (SD) number of explicit rules reported after each stress test.

Group	Test 1	Test 2
EL	8.1 (2.7)	9.5 (2.1)
ILAST1		5.0 (2.3)
ILAST2	3.6 (2.6)	4.3 (3.0)
IL	4.0 (2.1)	4.0 (1.9)
Total	5.3 (3.2)	5.7 (3.2)

Stress Intervention

Analyses of two separate variables was undertaken in order to determine the success of the stress intervention: CSAI-2 scores, heart rate. Each variable was subject to analysis of variance and a priori contrasts where appropriate. It was hypothesised that the groups would show an increase in scores on the Somatic and Cognitive anxiety subcomponents of the CSAI-2 as a result of the stress intervention, when compared to a baseline measure taken during practice. This, it was hypothesised, would be accompanied by an increase in heart rate as a result of the stress intervention. A further hypothesis to be tested, was that in line with the predictions of Masters (1992) and processing efficiency theory, time to completion for putting sessions under stressful conditions would increase. Scores from the self-confidence scale of the CSAI-2 were not analysed in this study as no relevant hypotheses were identified.

CSAI-2 scores

Table 4-2. Mean (SD) cognitive anxiety scores for pre and post intervention for both test sessions. Planned contrasts of the cognitive anxiety scores pre and post test showed no significant differences for either test 1 ($F(1,28) = 2.016$ $p > 0.05$) or test 2 ($F(1,28) = 2.462$ $p > 0.05$).

	Cognitive Anxiety			
Group	Baseline 1	Test1	Baseline 2	Test2
EL	14.25 (3.73)	15.38 (5.60)	15.13 (4.26)	16.13 (5.59)
ILAST1	15.50 (7.54)	15.63 (7.69)	17.88 (4.67)	18.75 (5.42)
ILAST2	17.25 (4.13)	19.63 (7.21)	15.25 (3.69)	15.75 (4.23)
IL	14.75 (4.65)	15.63 (6.25)	14.25 (3.73)	14.88 (5.17)
Total	15.44 (5.10)	16.56 (6.65)	15.63 (4.14)	16.38 (5.09)

Table 4-3. Mean (SD) somatic anxiety scores for pre and post intervention for both test sessions. Planned contrasts of the somatic anxiety scores pre and post test showed no significant differences for either test 1 ($F(1,28) = 0.342$ $p > 0.05$) or test 2 ($F(1,28) = 1.911$ $p > 0.05$).

	Somatic Anxiety			
Group	Baseline 1	Test1	Baseline 2	Test2
EL	15.50 (6.28)	16.50 (7.15)	13.00 (4.14)	14.63 (5.76)
ILAST1	15.75 (5.87)	15.13 (5.84)	14.38 (5.58)	15.25 (3.81)
ILAST2	18.38 (2.62)	19.50 (5.10)	12.63 (2.77)	12.88 (2.90)
IL	14.88 (2.80)	14.88 (4.64)	13.25 (4.06)	13.50 (4.81)
Total	16.13 (4.67)	16.50 (5.79)	13.31 (4.10)	14.06 (4.33)

Heart rate

Table 4-4. Mean (SD) heart rates pre and post stress intervention for both test sessions. Planned contrasts showed no significant increases in heart rate between the pre and post measures for both test 1 ($F(1,28) = 1.101$ $p > 0.05$) and test 2 ($F(1,28) = 1.596$ $p > 0.05$).

	Heart Rate			
Group	Baseline 1	Test1	Baseline 2	Test2
EL	80.4 (12.6)	81.7 (11.3)	92.6 (4.6)	89.9 (5.1)
ILAST1	77.9 (13.1)	78.1 (12.4)	81.8 (12.4)	83.3 (10.4)
ILAST2	82.1 (13.7)	82.5 (12.6)	79.5 (16.4)	82.3 (13.4)
IL	77.1 (13.4)	78.4 (13.7)	77.4 (12.3)	80.0 (12.2)
Total	79.4 (12.7)	80.2 (12.1)	82.8 (13.0)	83.9 (10.9)

Table 4-5. Mean (SD) putt completion times before and after the stress intervention for both test sessions. Planned contrasts showed that times to completion increased between the pre and post measures for both test 1 and test 2: Test 1 $F\,(1,28) = 6.860\; p < 0.05$, Test 2 $(1,28) = 5.430\; p < 0.05$.

	Average time per putt (secs)			
Group	Baseline 1	Test1	Baseline 2	Test2
EL	9.1 (2.5)	9.5 (2.3)	9.1 (3.1)	9.1 (4.0)
ILAST1	7.7 (2.9)	9.0 (3.4)	8.4 (2.1)	9.8 (3.6)
ILAST2	6.9 (1.8)	7.6 (2.5)	6.8 (2.6)	7.6 (2.4)
IL	6.2 (2.5)	6.55 (2.2)	6.6 (2.1)	6.6 (1.4)
Total	7.5 (2.5)	8.2 (2.8)	7.7 (2.4)	8.3 (3.2)

Analysis of performance scores

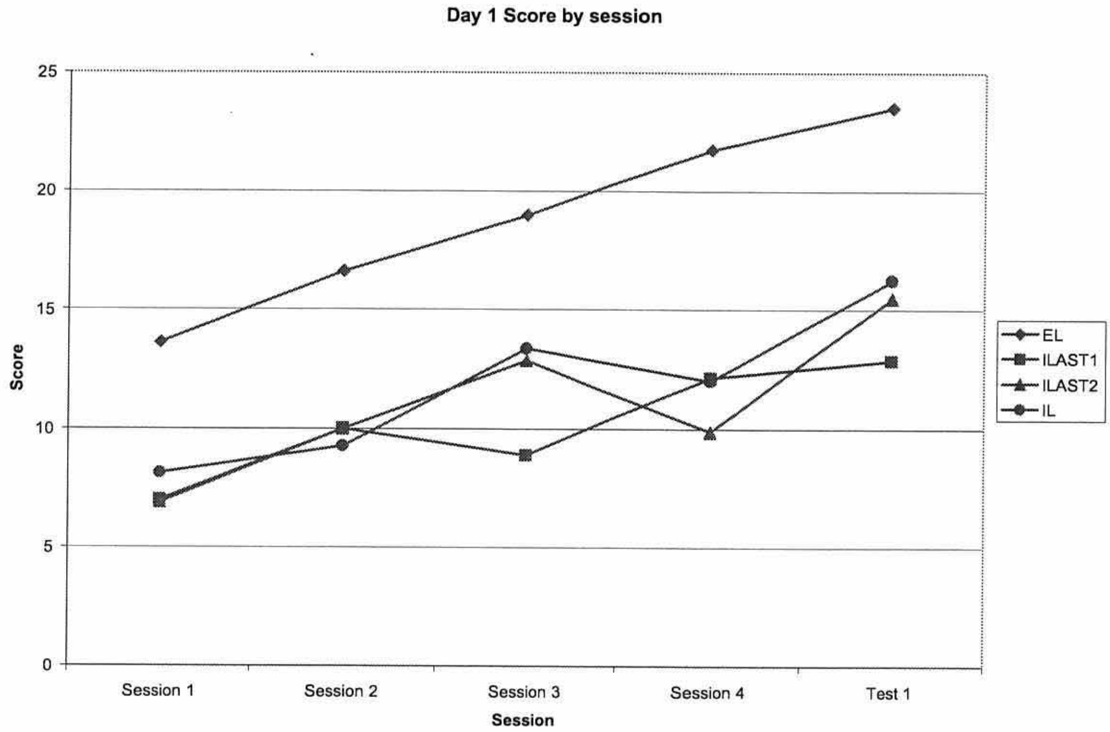
The primary hypothesis of this experiment was that pattern of scores obtained in the test phase on day 1, would be different that obtained in the test phase on day five. These two tests reflect the protocols used by Bright & Freedman (1998) and Hardy et al. (1996) respectively. With this in mind, the results from day 1 and the rest of the week were analysed in separate mixed model ANOVAs.

In addition a one way ANOVA (group by score) of the scores from the first 10 putting attempts, was run to test for homogeneity of groups i.e. that the groups all had the same level of putting skill at the start of the experiment. Analysis showed there to be no significant difference between groups across the first 10 attempts ($F(3,31) = 0.170$ $p > 0.05$) indicating a similar level of skill for all groups at the start of the session.

Day 1 - Bright & Freedman (1998) method

A two factor (4 groups by 5 sessions) analysis of variance with session as the repeated measure was performed on the data from day 1. Analysis revealed a main effect for group ($F(3,28) = 4.234$ $p < 0.025$), and a main effect for session ($F(4,112) = 23.224$ $p < 0.001$), but no significant interaction ($F(12,112) = 1.299$ $p > 0.2$). Observed power for the interaction was 0.696. Follow up analyses of means using Newman-Keuls tests on the group main effect revealed that the EL group performed significantly better than all other groups ($p < 0.05$). No other differences were significant. Follow up analysis of the sessions main effect revealed significant differences between session 1 and sessions 3, 4 and 5 which would imply a significant learning effect ($p < 0.05$). No other differences were significant. The data are summarised in the graph below.

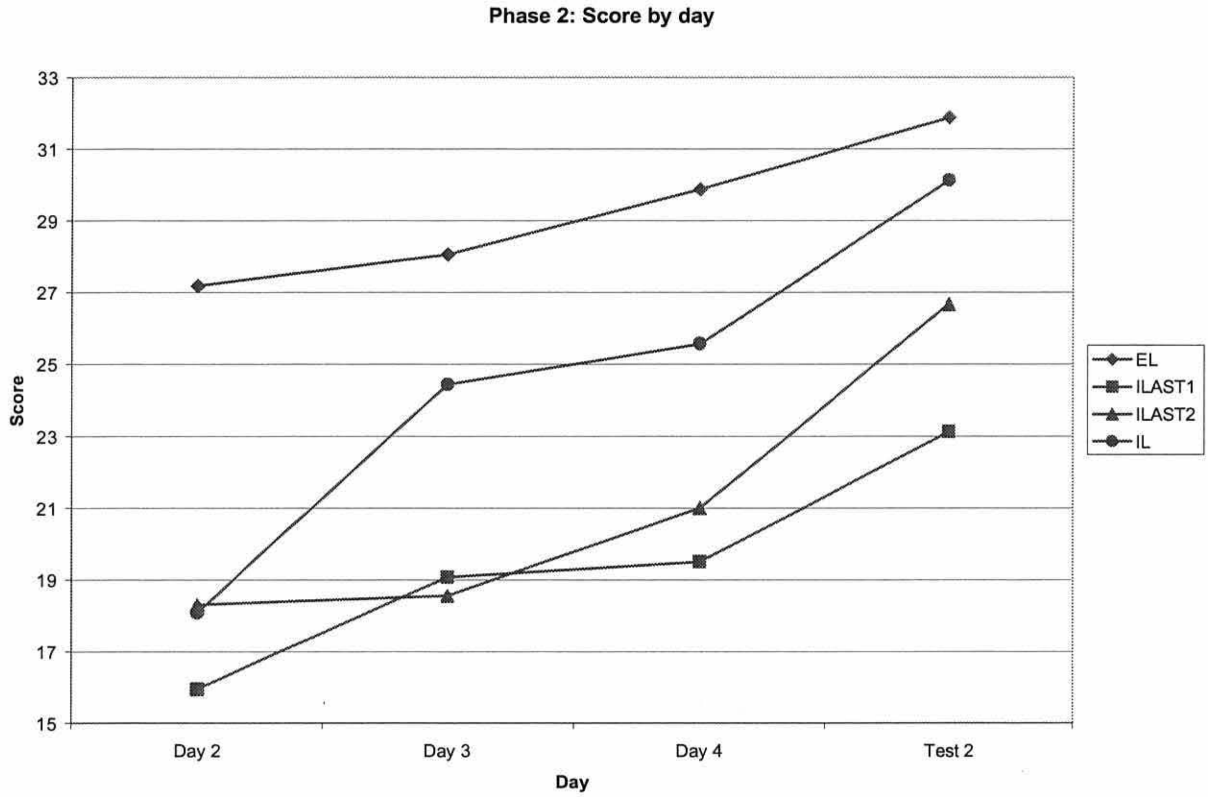
Figure 4-1. Graph for experimental phase 1: Scores by session for the first day to test1.



Days 2 to 5 – Hardy et al. (1996) method

A two factor (4 groups by 4 days) analysis of variance with days as the repeated measure was performed on the data from days 2 - 5. Analysis revealed a main effect for group ($F(3,28) = 3.664$ $p < 0.025$), a main effect for days ($F(3,84) = 27.025$ $p < 0.001$), but no significant interaction ($F(3,84) = 1.460$ $p > 0.05$, observed power = 0.658). Follow up analyses using Newman-Keuls tests showed the following: Group main effect - the ILAST1 and ILAST2 groups performed significantly worse than both the EL and IL groups ($p < 0.05$). Days main effect – All groups performed significantly better on day 5 than on day 2, significant differences were found between days 2-4 and day 5 ($p < 0.05$). No other differences were significant. This would suggest significant learning and continued improvement throughout the experimental period.

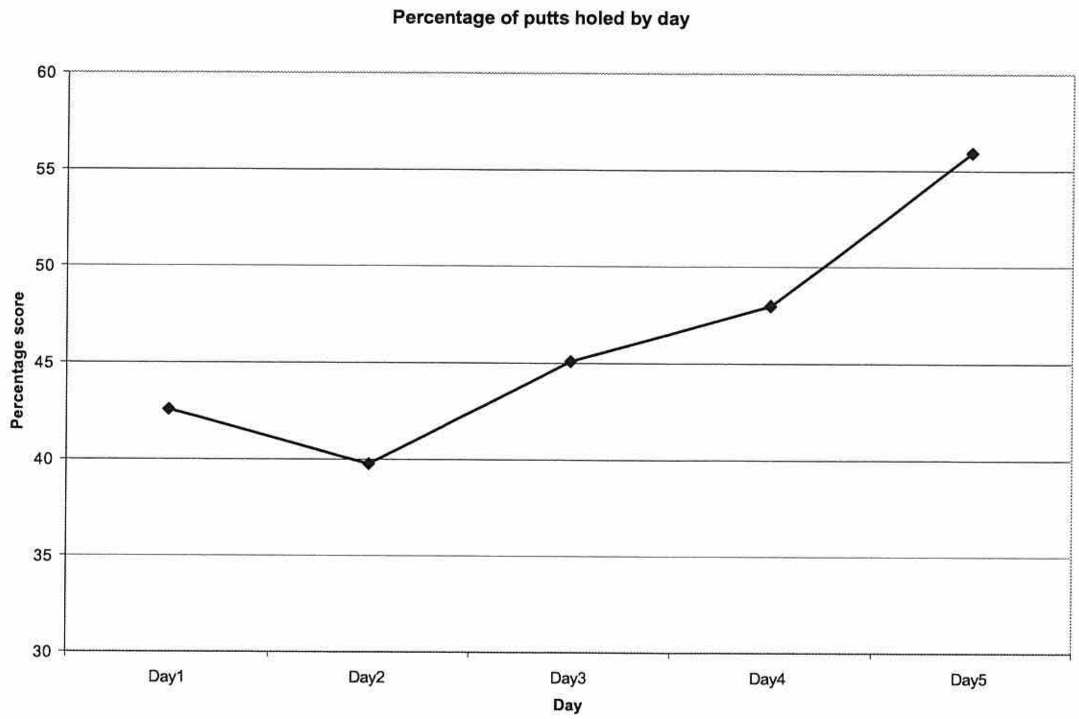
Figure 4-2. Graph for experimental phase 2: Scores by session days 2-4 and test 2.



Analysis of learning

A further post hoc test comparing percentage of putts holed for the last session on day1 and days 2-5 was performed in order to determine if performance on day 5 was better than that on day 1. One way analysis of variance with day as the repeated measure on percentage of putts holed revealed a significant main effect ($F(4,124) = 17.096$ $p > 0.001$) using epsilon corrected values. Newman Keuls follow up tests revealed significant differences between all means ($p < 0.05$). This would indicate significant learning across the experimental period, demonstrated by a clear improvement from day1 to day 5. Data are summarised in the graph below.

Figure 4-3. Percentage of putts holed by day for all groups.



Discussion

The most obvious explanation for the lack of interactions is revealed by analysing the validity measures taken for both of the stress sessions. For neither test 1 nor test 2 did CSAI-2 scores or heart rate scores reveal any significant difference pre and post stress intervention. The participants simply failed to get anxious on either occasion.[1] This may be interpreted in terms of "stress", "strain" and reported anxiety. In accordance with Hardy & Jones's (1989) definition, a stressor is a demand placed upon an individual. Strain will only result when perceived resources are exceeded, and feelings of anxiety will increase along with the resulting strain. Therefore if the stress condition used in the current experiment was not perceived to exceed general or personal resources by participants, no feelings of anxiety would have been reported. A stressor being within the scope of personal resources would perhaps have been interpreted positively, used as a motivator and not invoke an anxiety response when participants were questioned.

However some support is offered for the experimental hypothesis. Post hoc analysis of the percentage of putts holed showed a significant improvement in performance from day 1 to day 5. This is clearly indicative of further learning, but more importantly it supports the notion that participants in the Bright & Freedman (1998) experiment, were unlikely to have achieved a sufficient level of automaticity. Without sufficient progress towards automaticity any regression towards controlled processing would be all but impossible, as participants were still at a stage of highly controlled processing.

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[1] During piloting the anxiety manipulation was not tested fully i.e. the whole experiment was not run in full. As a consequence allowance was not made for participants become familiar with the experimenter over time. This my have been compounded by inconsistent behaviour on the part of the experimenter whose presence was crucial to the success of the anxiety manipulation.

Interestingly, task completion times for test 1 and test 2 were significantly longer than in practice. This result, though not relevant to the Bright & Freedman (1998) study is of interest when compared to the results of both Masters (1992) and Hardy et al. (1996). Analysis of task completion times for both of these experiments showed a significant increase in the Masters (1992) study and a marginally significant increase in the Hardy et al. (1996) study. Overall this would suggest that performers do take more time over putting in the test conditions.

Increased completion times for the putting task in the test conditions might be taken as indicative of greater effort expenditure. This is in line with the findings of Mullen & Hardy (2000), who found increases in self reported effort for putting under stressed conditions. Taken together, these findings are consistent with Eysenck & Calvo's (1992) Processing Efficiency Theory. Processing Efficiency Theory predicts differential effects for anxiety on processing efficiency and performance outcomes. It suggests that when anxious, performers will endeavour to invest more effort in a task in order to improve or maintain performance. Evidence for increased effort expenditure can be seen in participants taking more time over each shot. This increased effort would result in a net decrease in overall processing efficiency. Increased effort expenditure may well decrease overall performance efficiency, but provided demands do not exceed resource limits, increased effort will not lead to corresponding decreases in performance outcome. In essence, participants may try harder and go slower as a result, but provided levels of anxiety are not too high, performance is likely to improve or go unchanged.

According to one of the hypotheses tested in this experiment, an interaction should still have taken place. The explanation provided by Bright & Freedman (1998) for the performance of the IL groups in the Masters (1992) study, allows for improvement of the IL group in both tests, regardless of the level of anxiety induced.

If removal of the AST for the test sessions gives participants in the IL groups less to do, then an improvement above that of the ILAST group should take place regardless of anxiety. This prediction is not tenable according to the re-investment explanation offered by Masters (1992) or the Processing Efficiency view supported Hardy et al. (1996), as both predict that anxiety should predominantly influence the performance of the EL group. This view relies on the notion that participants get anxious and respond negatively to it. Improvement for the IL group was not shown to be significantly above that of the ILAST groups and unsurprisingly in view of the anxiety data, the EL group did not appear to suffer any decrement in performance. Indeed, the pattern of means in the graphs, offers little to suggest that the IL group was performing any better than the ILAST2 group. This might be taken to tentatively support the views of Masters (1992) and Hardy et al. (1996).

The lack of detected interactions might be due to effect size and group numbers. In line with Hardy et al. (1996) and Masters (1992) the current experiment involved only eight participants in each group, whereas the replication by Bright & Freedman (1998) involved some 16 participants in each group. The overall effect of the larger groups is likely to be a magnification of any experimental effects (Cohen, 1978).
It is possible that the effect measured by Bright and Freedman (1998), was so small that it would not show up with only eight participants in each group. This would explain the failure to detect an interaction that was expected at test 1 regardless of the anxiety level. Once again it may be of some concern that the basic pattern of means during test1 barely supports this contention and the effect size for the interaction is only 0.122

The question remains as to why the stress manipulation used in this experiment was insufficient to bring about an appropriate anxiety response. Two minor deviations from previous approaches might account for the lack of anxious participants. The first has to do with the participants themselves and the second is to do with the anxiety manipulation.

The participants recruited for the current experiment were first year undergraduate sport science students. Although these students were screened for task related knowledge and knowledge about psychology of performance, this screening may not have proved sufficient. It might reasonably be expected that sport science students have at least some experience of regular sporting competition and this could have rendered them more resistant to the sort of laboratory stressor used in this experiment. As a consequence of previous sporting activity, the participants recruited for this experiment could also reasonably be expected to have had some strategies in place to combat the effects of competitive stress. Alternatively, participants may indeed have chosen sport and competition as well as this experiment, precisely because they enjoy this experience ! In all of the above cases it is likely that large amounts of stress would be needed to bring about a negative anxiety response in this particular group of participants. It should be noted as a counterpoint to this view that Mullen & Hardy (2000), managed to obtain significant stress effect with experienced golfers. This clearly puts focus on the viability of the stress manipulation.

The most important issue here then, deals with differences between the stress interventions employed to bring about an expected anxiety response. Masters (1992) and Hardy et al. (1996) used identical stress interventions that involved financial incentive with the potential loss of a sum of money, and test evaluation facilitated by the presence of a "Golf Professional". Bright and Freedman (1998) appear not to have made any use of financial incentives but they did make use of a Golf Professional, a video camera and the element of surprise. The present experiment made use of a video camera and a golf professional as a judge of the video material recorded. This, it was hoped would be equivalent to suggesting with the help of taped coughing, that a Golf Pro was sat behind a one-way mirror, as in the Bright & Freedman (1998) experiment. The current experiment introduced a team prize which suggested that each player was part of a team and that during test sessions their scores would determine if the team won a prize. The intention was to increase pressure on performers as they were no longer just playing for themselves. The shortcoming of this approach, is that at no point were participants subjected to the feelings of direct personal evaluation i.e. the threat of a significant individual in the room with them judging their performance. It may be critical that an additional body with some authority or importance was in the room during testing, as this is most likely to provoke motives of achievement or more likely failure avoidance (Atkinson & Litwin, 1960; Atkinson, 1964). Furthermore, it seems likely that participants being unable to meet or see other members of their team might have diminished their sense of team affiliation. Indeed, being an anonymous member of a team probably diffused personal responsibility rather than increased it (Hardy & Latane, 1988; Latane, Williams, Harkins, 1979). Finally, incentives such as prizes can have a motivational effect or a disruptive effect, either way they appear to be most effective when tied directly to personal performance (Eyesenck, 1983).

Hardy et al. (1996) & Bright & Freedman (1998) sought to replicate and modify the findings of Masters (1992) with the same aim in mind i.e. to correct for a potential confound in the treatment of the implicit learning group involved in the study.
The confound was that an implicit learning group was required to perform an Articulatory Suppression Task during practice, but not during a test phase under

stressed conditions. Therefore, any changes between practice and testing could be attributed either to the stress manipulation or to the removal of the AST.

The replications surprisingly came up with widely different results. The present experiment sought to determine if a difference between two replications could be explained by differences in the methodologies employed. In particular, this study sought to examine whether extended practice influenced the degree of change experienced as a result of removing the AST. The present study failed to replicate the results of Hardy et al. (1996) or Bright & Freedman (1998), because the stress manipulation used failed to bring about any significant reportable changes in anxiety.

In conclusion, the original hypothesis and rationale suggested at the start of this chapter is still viable. What is required is a more effective stress manipulation, in order to examine fully hypothesised effects fully and to complete replications of earlier studies.

Chapter 5

Knowledge and conscious control of motor actions under stress: A further re-examination.

Chapter 5

Introduction

The experiment described in the previous chapter sought to resolve a conflict between two replications of the same experiment which revealed widely different results. Both were based on work by Masters (1992), examining the differential response to stress made by participants who had learned a putting task either implicitly or explicitly. Masters (1992) contention was that those who learned the task implicitly did not suffer performance decrements when tested under stressful conditions. Support for this notion came from his results, which showed the Implicit Learning continuing to improve during the stress – test period, and the Explicit Learning group to failing to improve in the same period. However Masters' experimental method contained an artefact involving the Articulatory Suppression Task (AST; Baddeley, 1966), which was used to prevent implicit learning groups acquiring explicit knowledge about the task. Whilst the AST was performed by the implicit learning (IL) group during practice, it was not performed during the test period under stress. Therefore it was not clear if any changes in performance for the IL group in the stress test were due to the stress manipulation, or the release from performing the AST whilst putting.

Attempts to correct for this artefact were made by, Hardy, Mullen & Jones (1996) and Bright and Freedman (1998). The correction used was to replicate Masters (1992) original experiment with the introduction of an additional group which had to perform the AST during testing. Unfortunately the two approaches revealed different results. Hardy et al. (1996) found no difference between the performance of the original IL group and the new IL plus AST group when tested, thus supporting Masters (1992). Bright and Freedman (1998) found improvements for the IL group but not the IL plus AST group in testing. They proposed on the basis of this result that there was no support for differential effects on performance under stress for implicit and explicit learning. It was proposed in the previous chapter that the main reason for the difference between the two results was the amount of practice afforded groups before testing. The approaches of Masters (1992) and Hardy et al. (1996) allowed 400 trials of practice, more than double the 160 trials used by Bright and Freedman (1998). The quantity of practice it was argued would have two effects: The first was that more practice would increase the likelihood of the EL group suffering some decrement to performance during the stress test. More practice should lead to greater probability of participants automating some if not all of the task. Therefore, if under stressed conditions participants were to engage a mode of conscious, controlled processing this should bring about a corresponding regression in performance. Thus EL groups should cease to improve, if not get worse under stressed conditions. This is in contrast to the IL groups which ought to improve in the same period. This hypothesis is consistent with the conscious processing hypothesis and the findings of Masters (1992) and Hardy et al. (1996). Secondly additional practice should also cause a different response to the removal of the AST during testing for the IL group. After 400 trials, participants were deemed more proficient at both the putting task and the AST, therefore removal of the AST was unlikely to have a substantial effect on performance. In light of this, any specific gains made by the IL group following release from the AST after 160 trials of practice were less likely after 400 trials. In order to test these propositions the previous experiment devised a method whereby groups completed both the protocol used by Bright and Freedman (1998) and the protocol used by Hardy et al. (1996). This was done in the hope that the results of Bright and Freedman (1998) would be replicated in the first test session under stress and the results of Hardy et al. (1996) would be replicated in the second. Unfortunately the stress manipulation of the previous experiment failed to bring about sufficient changes in anxiety for the test sessions and as a consequence neither of the approaches was replicated. Though some implications were discussed, there was the need to complete the previous experiment with a better stress manipulation.

The stress manipulation used in the previous chapter involved team affiliation, a shared prize and the threat of filming/evaluation by a "golf professional". It seems likely that the issue of team affiliation did not work in the absence of other team members during testing or indeed during practice. Even if a team was identified, the presence of others watching is likely to be the only successful countermeasure against social loafing (Hardy & Latane, 1988; Latane, Williams, Harkins, 1979). Having team members present for competitions is likely to have the desired effect but may cause other problems. For example it is unlikely that participants could be kept from communicating with each other. This problem is particularly pertinent where repeated testing is involved. For the current experiment, the team idea was abandoned in favour of participants performing individually. Performing only in the presence of the experimenter alone without the golf pro apparently in the room most probably had the effect of reducing failure avoidance motives for the participants (Atkinson & Litwin, 1960, Atkinson, 1964). With only the implied presence of a golf pro, a one way mirror, basic test anxiety and no material prizes, Bright & Freedman (1998) managed to introduce an apparently successful stress intervention. This would seem to underline the need for the presence of an "alien" observer implied or otherwise. Unfortunately obtaining walk on/walk off golf pros and one way mirrors is somewhat difficult, so it was considered useful to contrive a method that replaced these elements for any future experiments of this type. With this in mind two experimenters acted as judges for the tests. This was the only time there was be two experimenters in the room. The video and further evaluation by a golf pro remained so that in the stress condition participants believed their performance would be observed by three people. This it was hoped would increase the likelihood of failure avoidance behaviour. In the previous experiment, winning a prize was not directly contingent on personal performance but on team performance, this may not be the most effective use of an incentive (Eyesenck, 1983). As a result, cash prizes were awarded for the top three scores and a style prize was also awarded. The style prize was introduced in order to maintain motivation for participants who were performing poorly, it also necessitated that both experimenters needed to be sat in the room in order to complete their share of the judging. There were two competitions run, one at the first test and one at the second. It was explained to participants that these competitions were independent. In the sense that rewards are directly related to personal performance, which is more in keeping with the interventions used by Masters (1992) & Hardy et al (1996).

The present experiment therefore, seeks to use the protocol outlined in Experiment three with a modified stress intervention. Consequently, the hypotheses to be tested remain the same. Specifically they are that: During the first test, analysis of scores will show a greater increase in performance for the IL group than the IL plus AST group and that the EL group will continue to improve. In the second test period performance for both the IL and IL plus AST group should improve similarly, but performance of the EL group will not improve.

Method

Participant's

Thirty- six volunteers (18 male, 18 female) were randomly allocated to one of four treatment groups: Explicit Learning (EL), Implicit Learning (IL), Implicit Learning with AST 1 test (ILAST1), Implicit Learning with AST 2 tests (ILAST2). Participants were right-handed with no previous golfing experience. Only participants with no previous psychological skills training or experience of psychology courses were selected.

Apparatus

A putting surface of an identical design to that employed by Masters (1992) and Hardy et al. (1996) was used in the current study. This consisted of an Astroturf putting surface with 1.5m between the start line and the hole. There was a 1m long 25% gradient between the two points which commenced 30cm from the start line. The hole itself was 10.8 cm in diameter, in accordance with United States Professional Golf Association rules. Regulation white golf balls (4.27 cm in diameter) and a standard putter (88.9 cm in length with a standard angle of lie and loft) were used by all participants. Heart rate was measured by means of a Polar Electro Sport Tester PE3000 heart rate monitor. Use of the PE3000 heart rate monitor requires that a chest transmitter and a wrist-based receiver be used. This type of heart rate monitor was chosen in preference to devices that could be attached to the ear or the fingers, as these methods were thought to be more invasive. Heart rates were recorded at 5s intervals for 180s in total for each measurement period. Heart rates were recorded for download twice when the stress manipulation was delivered. The heart rate monitor was worn for all sessions throughout the experiment. An electronic metronome was used with a 1.5s time-base as a prompt for the Articulatory Suppression Task.

Design

The experiment was spread over 5 days and consisted of two phases: Phase one was 4 sessions of 40 putts practice separated by a short rest followed by a stress test/competition of 40 putts, this was all completed on day 1. Phase two was 2 sets of 50 putts separated by a short rest on days 2-4 followed by a stress test/competition on day 5 consisting of a further 2 sets of 50 putts under stressed conditions. Phase one allowed for some 160 trials of practice before test 1, Phase two allowed some 500 trials of practice before test 2 if trials on day 1 were counted as practice. All but the ILAST1 group were tested under high stress conditions on days 1 and 5. For the ILAST1 group only, the first test phase was omitted and treated as a further 40 trials of practice.
The ILAST1 group was included to control for possible carry-over effects resulting from the stress manipulation in test 1. All sessions took place on consecutive days at approximately the same time of day.
Each group was required to perform according to one of four separate learning conditions
EL group – Before each practice session began participants in the explicit learning groups were requested to read carefully a set of instructions on how to putt.
These instructions were identical to those used by Masters (1992) and Hardy et al. (1996) which were compiled using two reputable coaching sources (Saunders & Clark, 1977; Stirling, 1985; see appendix a). These instructions were chosen over those used by Bright & Freedman due to their availability and perceived reliability. It was impressed upon participants that they should read these instructions carefully and follow

them as closely as possible. The instructions were not presented for the final stress test.

ILAST groups – Participants in the ILAST groups were given no instructions on how to putt before starting the task. Instead they were required to perform an Articulatory Suppression Task (AST) whilst putting. This was the same AST as used by Masters (1992) and Hardy et al. (1996) which was in turn based on a procedure first outlined by Badddely (1966). Participants were required to call out a random letter each time an electronic metronome "clicked". For the first 400 trials in the experiment clicks were timed to occur every 1.5 seconds, after this subsequently clicks sounded every second. Participants were required to prioritise random letter generation, this meant that rather than stop generating letters they should stop putting. The importance of the letters generated being random was emphasized to the participants. The reduction in the time interval between clicks was designed to maintain the difficulty of the AST, thus continuing the suppression of explicit knowledge throughout the skill acquisition phase. In line with earlier studies, it was assumed that an inter-click interval of 1-1.5s would be sufficient to suppress any acquisition of explicit information about the putting task.

The ILAST1 and ILAST2 differed only insofar as the ILAST1 group received only the second of the two stress treatments and the earlier treatment period was used as practice. The ILAST1 group was introduced to the study in order to check for possible carry over effects resulting from the first stress manipulation.

IL Groups – The IL group like the ILAST groups was required to learn the task whilst performing the AST. The only difference was that during both of the stress treatments they were not required to perform the AST.

Procedure

Prior to each acquisition or treatment period, participants were fitted with a heart rate monitor. They were then requested to sit quietly for five minutes to allow baseline measures to be obtained. During this time participants were given written instructions concerning the nature of their participation and what was expected of them during the session (appendices b,c). Standard instructions included the request that they not think about or rehearse the task while away from the experiment and that they were not to discuss the experiment with any other party. They were informed that the purpose of the experiment was to examine the effect of different practice conditions on learning, that their participation was entirely voluntary and they could withdraw their consent to participate at any time. No time constraints were imposed on participants.
Group specific instructions were also administered at this time (appendix d). Participants' heart rates were then monitored for a period of three minutes in the baseline and stress sessions. In the high stress sessions specific stress inducing information was given to the participants in the middle portion the three minute heart rate monitoring period (appendices e,f). The putting sessions were broken up into sets of 40 putts separated by a 5-minute rest on the first day and 50 putts separated by 5 mins on subsequent days. The first set of putts commenced at the end of the three-minute rest period. The primary dependent variable was the number of putts holed.

Stress intervention

At the start of the stress manipulations participants were informed that they were to participate in a competition for a cash prize. The prizes ranged from £25 pounds for the winner, £15 for second place and £10 for third. Participants were informed that there was an additional £10 prize for the best technique as judged by two experimenters and video analysis by a golf professional. This information was delivered during the second minute of the 3 minute heart rate recording phase. In addition to this information a video camera was set up "to record their activities for evaluation" in view of the participant. The camera was switched on but the taped material was never used. When the putting session commenced the two experimenters sat down with clip boards and score sheets, one sat slightly behind and the other sat near the hole in full view of the participant. The experimenter sat in view maintained a blank face throughout the test period. The stressor information was delivered during the middle 60s of the 180s heart-rate monitoring period, during which time participants were given a standard statement to read outlining the competition conditions. Because heart rate was being measured throughout this period, it was possible to obtain an index of physiological response to the intervention. For analysis the first 60 seconds before the intervention was compared to the last 60 seconds following the delivery of the stress instructions only the final 60 seconds of each measurement period was used for analysis. Following the measurement period the treatment the test trials began.

Stress measures

Unlike Masters (1992) and Bright & Freedman (1998), the psychological tool for measurement of stress response was not the State-Trait Anxiety Inventory (STAI ; Spielberger, Gorsuch & Lushene, 1970) but the Competitive State Anxiety Inventory - 2 (CSAI-2 Martens, Burton, Vealey, Bump & Smith, 1990). This was in line with arguments presented by Hardy et al. (1996) to the effect that, this was a sport specific inventory that measured particular separate subcomponents of competitive anxiety and was therefore more informative as to the nature of the stress response. The CSAI-2 also seems most appropriate given the nature of the stress treatment employed here, as the task involved is putting and the stressor involved explicit competition. Internal consistency for the CSAI-2 has been shown to be of adequate standard, with Cronbach's alpha coefficients ranging form 0.70 to 0.90 (Gould, Petchlikoff & Weinberg, 1984). Concurrent validity has also been confirmed by Martens et al (1990), who obtained predicted relationships between the CSAI-2 and an assortment of trait measures. The CSAI-2 was administered pre and post-stress intervention, to assess each participant's levels of cognitive and somatic anxiety. The CSAI-2 was administered during the inter-trial interval between sessions 3 & 4 (baseline 1) and following the stress intervention before testing on day 1 (test 1). The CSAI-2 was also administered on day 5 before first putting session (test 2). The wording of the CSAI-2 was slightly modified to alternate the term practice or competition to match respective baseline and stress measures.

In line with Masters (1992) and Hardy et al. (1996) time to completion was measured for each putting session. This measure was included as it was originally hypothesised by Masters (1992) that time to completion would increase as participants took extra time to prepare for each putt. Time to completion provides and indirect test of the Processing Efficiency Theory (Eyesenck & Calvo, 1992) in that it can be used as an index of effort, whereby increased time taken to complete a putt is indicative of additional effort expenditure.

Verbal Protocols

In order to test the prediction that the EL group should have acquired more explicit knowledge about the task than the other groups, it was necessary to assess the extent of participants' explicit knowledge using a verbal protocol. This was in line with the notion that individuals learning implicitly would accumulate less explicit knowledge than those learning using explicit rules. After completing each of the test sessions participants were required to list all of the information that they thought was relevant to making a successful putt. They were asked to use information that they had become aware of over the five putting sessions. In line with all other studies of this type, the written protocols were scored, by summing the number of explicit rules each participant wrote down. Explicit rules were defined as those statements that related to the instruction set used, or mechanical/procedural aspects of the task. Statements outside of these criteria such as those relating to feelings were excluded from analysis.

The credibility of the explicit rules identified was checked by "Investigator triangulation" (Lincoln & Guba, 1985). This was done in line with the modification used by Hardy et al. (1996) to increase the validity of this measure. The method involved two researchers analysing the protocols independently using the same criteria. Checking one list against the other then allowed corroboration of the explicit rules elicited.

Results

Verbal protocols

In line with Hardy, Mullen & Jones (1996) a priori or planned contrasts were employed to test expected directional effects. Testing for these effects has been identified in previous research as necessary to verify the effectiveness of the stress intervention and the learning manipulation. In these circumstances further exploration of the data or examination of other effects was deemed unnecessary hence planned contrasts were used.

It was predicted that the EL group should have a significantly larger pool of explicit knowledge than the IL, ILAST1 and ILAST2 groups together and that regardless of condition all groups would increase their explicit knowledge across the two test sessions. A two way analysis of variance (group by test) with test as the repeated measured revealed a significant effect for group ($F(3,32) = 3.959$ $p < 0.05$) and for test ($F(1,32) = 21.397$ $p < 0.0001$). An a priori contrast of the group effect showed the EL group to have significantly more explicit knowledge ($F(1,32) = 10.233$ $p < 0.01$). It is worth noting from the means table supplied below, that as in Hardy, Mullen & Jones (1996), the ILAST2 group in spite of performing the AST on all days acquired more explicit knowledge than the other IL groups.

Table 5-1. Mean (SD) number of explicit rules reported after each stress test.

Treatment	Test 1	Test 2
EL	8.67 (2.83)	10.44 (3.05)
ILAST1	4.67 (2.65)	6.56 (3.71)
ILAST2	6.00 (2.18)	8.22 (3.77)
IL	5.33 (3.12)	6.11 (1.96)
Total	6.17 (3.02)	7.83 (3.52)

Stress intervention

Analyses of two separate variables was undertaken in order to determine the success of the stress intervention: CSAI-2 scores, Heart rate. Each variable was subject to analysis of variance and a priori contrasts where appropriate. It was hypothesised that the groups would show an increase in scores on the Somatic and Cognitive anxiety subcomponents of the CSAI-2 as a result of the stress intervention, when compared to a baseline measure taken during practice. This, it was hypothesised, would be accompanied by an increase in heart rate as a result of the stress intervention. A further hypothesis to be tested was that in line with the predictions of Masters (1992) and processing efficiency theory, time to completion for putting sessions under stressful conditions would increase. Scores from the self confidence scale of the CSAI-2 were not analysed in this study as no relevant hypotheses were identified.

CSAI-2 scores

Table 5-2. Mean (SD) Cognitive anxiety scores for baseline measures and both test sessions. A planned contrast of the cognitive anxiety scores showed significant differences between the baseline measure and both the stress measures: Baseline vs. test 1, F (1,32) = 33.995 $p < 0.0001$, baseline vs. test 2, F (1,32) = 10.593 $p < 0.01$.

	Cognitive Anxiety		
	Baseline 1	Test 1	Test 2
EL	14.22 (4.38)	18.78 (5.09)	17.67 (4.06)
ILAST1	14.44 (2.19)	14.22 (2.59)	15.78 (2.77)
ILAST2	16.44 (5.90)	20.89 (5.88)	17.85 (6.46)
IL	13.33 (5.24)	17.00 (6.60)	15.44 (4.03)
Total	14.61 (4.59)	17.22 (5.61)	16.69 (4.47)

Table 5-3. Mean (SD) Somatic anxiety scores for baseline measures and both test sessions. Comparison of somatic anxiety scores revealed a significant difference only between the baseline measure and test 1 (F (1,32) = 12.026 $p < 0.01$). Baseline vs. test 2 did not reach significance.

	Somatic Anxiety		
	Baseline 1	Test 1	Test 2
EL	13.44 (3.84)	15.89 (3.14)	14.89 (3.41)
ILAST1	12.50 (3.67)	14.89 (3.14)	15.22 (3.56)
ILAST2	16.25 (4.28)	17.78 (5.26)	15.67 (5.10)
IL	13.44 (3.57)	14.67 (3.87)	13.44 (2.35)
Total	13.91 (3.95)	15.80 (3.98)	14.80 (3.67)

Heart rate

Table 5-4. Mean (SD) heart rates before and after the stress intervention for both test sessions. Planned contrasts showed that heart rates increased between the pre and post measures for both test 1 and test 2: Test 1, $F(1,24) = 6.993$ $p < 0.01$, Test 2, $F(1,24) =$ 7.578 $p < 0.01$.

	Heart Rate			
Group	Baseline 1	Test1	Baseline 2	Test2
EL	84.91 (7.29)	86.57 (6.77)	77.66 (11.88)	80.56 (14.28)
ILAST1			74.59 (9.53)	77.37 (8.89)
ILAST2	81.92 (9.82)	83.66 (11.38)	84.88 (6.33)	86.03 (4.45)
IL	78.15 (3.61)	81.94 (4.84)	79.37 (12.94	82.19 (31.31)
Total	81.66 (7.61)	84.06 (8.06)	79.12 (10.72)	81.54 (10.94)

Time to completion for putting

Table 5-5. Mean (SD) completion times per putt before and during the stress intervention for both test sessions. Planned contrasts showed that times to completion increased significantly between the pre and post test measures for test 1 $F(1,32) = 7.934$ $p < 0.01$. Test 2 approached significance $F(1,32) = 4.067$ $p = 0.052$.

	Average time per putt (secs)			
Group	Baseline 1	Test 1	Baseline 2	Test 2
EL	10.0 (4.4)	11.3 (5.6)	10.0 (5.5)	10.6 (5.0)
ILAST1	6.4 (2.1)	6.6 (1.9)	7.1 (1.7)	8.0 (3.4)
ILAST2	6.4 (1.2)	7.0 (1.8)	7.0 (1.8)	7.9 (2.6)
IL	6.9 (2.3)	7.7 (2.2)	7.4 (2.1)	7.4 (1.5)
Total	7.4 (3.0)	8.1 (3.7)	7.9 (3.3)	8.5 (3.5)

In spite of the lack of difference between somatic anxiety scores in test 2, overall the data would suggest that the stress intervention was effective. Significantly elevated heart rates in test 2 would suggest that participants though physiologically aroused did not perceive this as a change in somatic anxiety. This would be in line with criticisms of the relationship between physiological arousal measures and somatic anxiety scores discussed by Woodman & Hardy (2001).

Analysis of performance scores

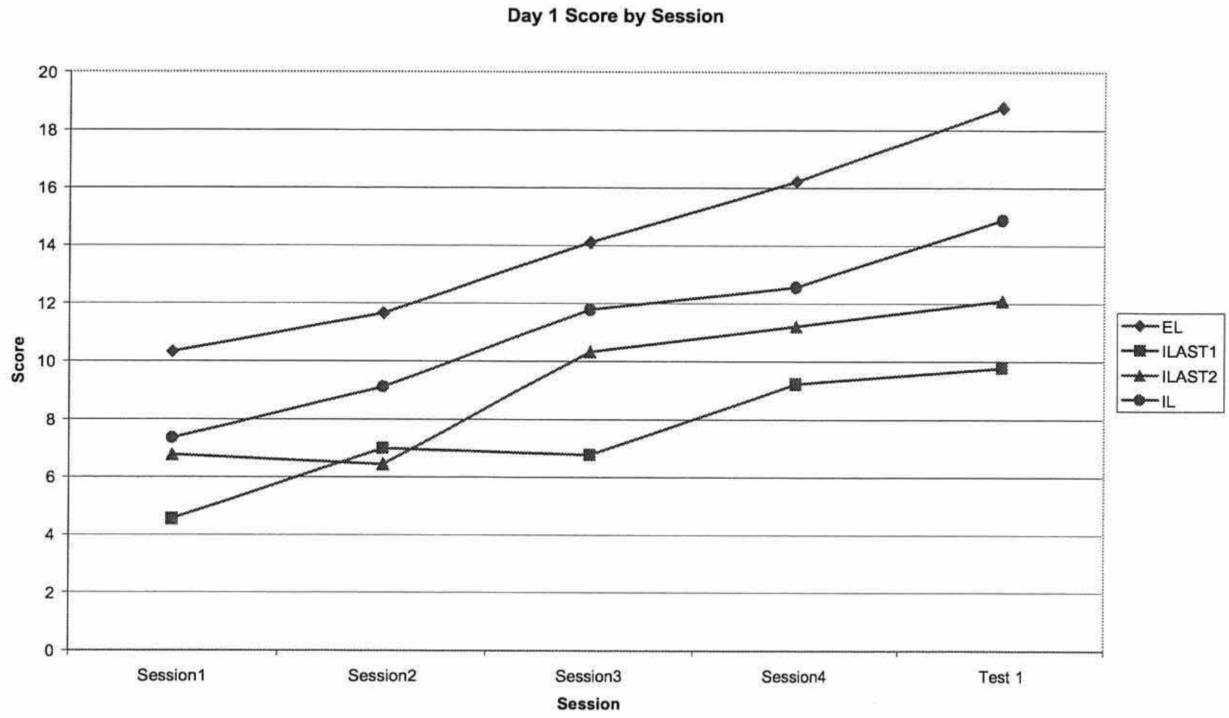
The primary hypothesis of this experiment was that pattern of scores obtained in the test phase on day 1 would be different to that obtained in the test phase on day 5. These two tests reflect the protocols used by Bright & Freedman (1998) and Hardy et al. (1996) respectively. With this in mind the results from day 1 and the rest of the week were analysed in separate mixed model ANOVA's.

In addition a one way ANOVA (group by score) of the scores from the first 10 putting attempts was run to test for homogeneity of groups i.e. that the groups all had the same level of putting skill at the start of the experiment. Analysis showed there to be no significant difference between groups across the first 10 attempts ($F(3,35) = 0.007407$ $p > 0.05$).

Day 1: Bright & Freedman (1998) method - Test1

A two factor (4 groups by 5 sessions) analysis of variance with session as the repeated measure was performed on the data from day 1. Analysis revealed a main effect for group ($F(3,32) = 3.631$ $p < 0.025$), a main effect for session ($F(4,128) = 22.706$ $p < 0.001$) there was no significant interaction ($F(12,128) = 0.629$ $p > 0.8$) observed power for the interaction was 0.347, effect size was 0.056. Follow up analyses of the main effects using Newman-Keuls tests revealed differences between sessions 1 and 3-5 ($p < 0.01$) indicting a significant learning effect and that the EL group performed significantly better than the ILAST1 group ($p < 0.05$). No other effects were significant. The data are summarised in the graph that follows.

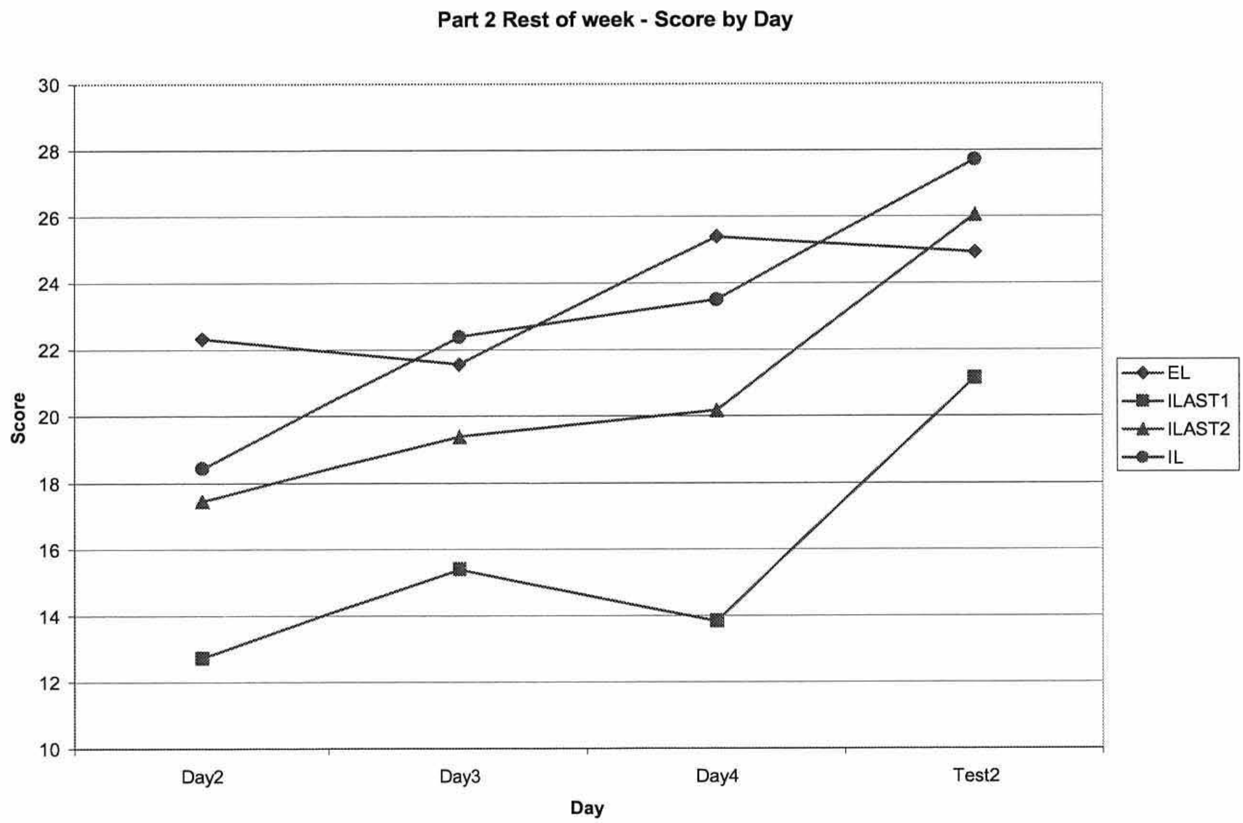
Figure 5-1. Graph for experimental phase 1: Scores by session for the first day to test1.



Days 2 to 5 – Hardy, Mullen & Jones (1996) method

A two factor (4 groups by 4 days) analysis of variance with days as the repeated measure was performed on the data from days 2 - 5. Analysis revealed a main effect for group ($F(3,32) = 3.862$ $p < 0.018$), a main effect for days ($F(3,96) = 30.849$ $p < 0.001$) and a significant interaction ($F(9,96) = 2.367$ $p < 0.05$). Follow up analyses using Newman-Keuls tests showed the following: Group main effect - the ILAST1 group performed significantly worse than all the other groups ($p < 0.05$). Days main effect – All groups performed significantly better on day 4 and test 2, there were significant differences between day 2 and days 3&4 ($p < 0.05$), indicating continued improvement throughout the experimental period. Group by days interaction – The important interaction rests with the differences between the final practice session (day4) and the final test session (test 2). From day 4 to test 2 Neuman Keuls tests revealed a significant improvement for the ILAST 1 and ILAST 2 groups ($p < 0.01$) and a significant improvement for the IL group ($p < 0.05$), that was marginalised when epsilon corrected scores were used. There was no significant improvement for the EL group ($p > 0.05$) despite a significant improvement from day 3 to day 4 ($p < 0.05$). The data are summarised below.

Figure 5-2. Graph for experimental phase 2: Scores by session days 2-4 and test 2.



Discussion

Analysis of scores for test1 on day1 indicated no significant interaction but significant practice effects and group effects. Therefore the findings of Bright and Freedman (1998) were only partially replicated by this study. However analysis of scores for days 2-4 and test 2 did show a significant interaction. Further analysis revealed a pattern of results that matched those of Hardy et al. (1996): The ILAST2 and ILAST1 groups both showed significant improvement between day4 and test2 whereas the EL group failed to improve within the same period. The IL group showed a marginal improvement following the use of epsilon corrected values for the analysis. Subsidiary analysis showed the stress intervention to be effective, however it can be seen from both the CSAI-2 scores and the heart-rate scores that the response in the second test period was not as high as that in the first. Indeed somatic anxiety scores for the second test were not significantly different to baseline scores. This is most probably due to measurement difficulties associated with the somatic anxiety scale of the CSAI-2 (Woodman & Hardy, 2000), though a stress inoculation effect cannot be ruled out.

Insofar as there was an expected interaction at test2 and not at test1, the contention that the difference between the findings of Bright & Freedman (1998) and Hardy et al. (1996) was due to quantity of practice is tentatively supported. From these results some support is found for differential effects on performance under stress resulting from the use of implicit and explicit learning methods. These findings are in keeping with Masters (1992) and garner support for the conscious processing hypothesis (re-investment) hypothesis. Although a lack of interaction at test 1 does not support Masters (1992) the presence of a supporting interaction at test 2 does and the only difference between the two points is the amount of practice that the groups have had. It is worth noting that the interaction in test 2 emerged despite participants having been exposed to the test situation once already. When examining the role of the ILAST1 group as a control it can be seen that the group underperforms at test 1 and makes great improvements at test 2. The underperformance of the ILAST1 group can be seen as a trend in the data up until test 2. There are two possible explanations for this: The first agues that the first test in some way facilitated learning for groups other than ILAST1. The second is that across an entire week of practice period the ILAST1 group did not benefit from motivational effects resulting from the first test. Of the two the latter explanation is preferred as it would explain the exceptional improvements made by the ILAST1 group in the second between day 4 and the final test to found in the pattern of means. As stated earlier there is limited evidence to support as an expected stress inoculation effect from the first to the second test. It would seem participants were getting used to the laboratory and the test situation. However this issue is countered by ILAST 1 group results which showed both somatic anxiety and cognitive anxiety rose between test 1 and test 2 unlike all other groups.

It is of concern that once more the interaction found by Bright & Freedman (1998) could not be replicated here. Inspection of the means from day 1 show the correct pattern for an expected effect, but the interaction result and the effect size (0.056) offer little evidence that increased group numbers would add any value to this pattern. Comparing the results of this experiment and the previous one it is clear that a successful stress intervention had no effect on the likelihood of the expected interaction occurring. An important difference between the current method and that of Bright & Freedman (1998) that might explain the absence of the hypothesised effect is task difficulty. In line with Masters (1992) and Hardy et al. (1996) the current experiment used a ramp with 1.5m from start line to hole with an upward gradient in between, Bright & Freedman (1998) used a flat ramp with 3m from start line to hole. If scores at test 1 for the current experiment are compared with those at the same point in the Bright & Freedman (1998) study, what appears to be a large difference emerges. The scores in the present experiment are more than twice those reported by Bright & Freedman (1998) at the same point. If this lack of scores is indicative of high task difficulty, then it is probably the case that participants in the Bright & Freedman (1998) experiment had particular difficulty in completing the task and the AST at the same time. Removal of the AST in this case would have a more beneficial effect than that expected in the present study. This situation would seem only to underline the importance of further practice, particularly as Maxwell, Masters & Eves (2000) showed that IL groups can successfully learn to make the 3m flat putt given sufficient practice, albeit not as well as their EL counterparts.

An exceptional feature of the results uncovered by the current study can be found in the pattern of means shown in the second test. Unlike the results of Masters (1992) and Hardy et al. (1996), the performance of the implicit groups actually begins to exceed that of the EL group in the test phase. Although this effect only approached significance, it is important as it provides further support for both the learning hypothesis proposed here, and the conscious processing hypothesis of Masters (1992). This difference may be explained in two ways: Firstly the degree of anxiety experienced by groups in the second test was higher than that experienced in previous experiments. The additional anxiety in turn brought about a greater degree of breakdown under stress. This explanation seems unlikely, as comparison of anxiety scores between Hardy et al. (1996) and the current experiment, showed the current experiment to be lower with respect to both cognitive and somatic anxiety in the final test. The second route to explore in explaining the performance of the implicit groups is to discuss possible learning effects. It should be noted that in the period leading up to test 2 participants in the current experiment received the equivalent of 500 practice trials rather than the 400 trials employed by Masters (1992) & Hardy et al. (1996). Also the structure of practice was somewhat different in delivering five sets of forty on the first day and two sets of forty on the subsequent days. In general it can be stated that participants in this experiment received additional practice. As a consequence of the additional practice and possible changes in practice structure participants achieved a higher level of automaticity than in previous studies. Greater automaticity according to conscious processing hypothesis should have led to higher probability of decrement for the EL group and more robustness for the implicit groups. This effect is only to be expected under anxious conditions and is not inconsistent with the notion that during normal learning, implicit groups appear unable to match the performance of explicit groups (Maxwell al., 2000).

The important considerations that arise from the current experiment are: That further support for differential effects on performance under stress for implicit and explicit learning has been provided. Perhaps of equal importance is that these effects appear to be contingent on quantity of practice. Additionally it is evident that laboratory based interventions seem to be successfully facilitated by the threat of observation by strangers or unfamiliar parties.

In line with Masters (1992) conscious processing hypothesis the two key ingredients that are thought to increase the likelihood of choking are high state anxiety and a sufficiently large pool of explicit knowledge. Given the findings of the current experiment it might tentatively be suggested that degree of choking or breakdown is to some extent determined by quantity of practice at a task. So whilst anxious performers with a large pool of explicit knowledge are more likely to lapse into conscious processing, the decrement resulting from this lapse is likely to be greater for those who have practised most. This would be in line with anecdotal evidence and well publicised examples of choking which appear to portray larger decrements in performance than those reported here and elsewhere. In light of this, there would appear to be three sets of issues that need to be considered when addressing this field of research: Firstly are issues dealing with likelihood of breakdown such as state anxiety or amount of explicit knowledge acquired. Secondly are those issues related to the magnitude of breakdown such as amount of learning and type of explicit knowledge. Finally there is a need to address issues such as; what actually happens when we do go wrong ?

A broad interpretation of findings to date might argue that implicit learning is a good thing and explicit learning is a bad thing. However, with such a dichotomy comes the danger that "the baby will be thrown out with the bathwater". There are clearly some disadvantages to be associated with implicit learning. For example no study to date has shown performance for the IL groups to have actually exceeded that of the EL groups in practise. This would suggest that either the dual task constraints applied for IL groups make learning slower and less efficient, or that implicit learning is in itself a more difficult process. An extension of this argument might suggest that those who learn a task implicitly will never achieve the same standard of performance as their explicit counterparts. There is some evidence to support this argument. Maxwell et al.(2000) performed a longitudinal study of implicit and explicit methods for learning a putting task. Over some 3000 trials participants were required to acquire the task either with (IL) or without (EL) a secondary tone counting task. Across the 3000 trials of practice the EL group outperformed the IL groups, at the end of which a delayed retention test tantalisingly failed to show a clear decrement for the IL groups. Notwithstanding the fact that explicit knowledge may hinder task acquisition, it would seem that either the dual task load or the nature of implicit learning is more of a hindrance. Furthermore, it is worth remembering that the current experiment suggests that the opposite may be true under anxious conditions, if sufficient automaticity has been achieved.

It should be noted that the EL group in the Maxwell et al. (2000) experiment although allowed to acquire explicit knowledge about the task, was in effect and explicit discovery learning group insofar as they were given no explicit instruction. This is not the normal progression of explicit learning. It does mean that both groups had to learn the task without guidance. There are difficulties with learning a task in this way. Taking Newell's (1991) argument about task mapping and outcome relationships the following points can be made. It is apparent that seeing the ball miss tells the learner nothing about why the shot went wrong. Only exploration of mappings between different attempts at the task and outcome will solve this problem. However this exploration is slow and prone to error, particularly when the task involved is complex involving a number of degrees of freedom. It is possible that given more than one viable solution to a movement problem, the wrong one will be selected. This may provide improvement in the short term, but prove problematic later on in transfer to novel situations or when higher levels of performance are sought. An example of this may be found in the search for the correct throwing/racquet swing technique made by children. From a standing position a step forwards with either foot will provide a better throw/swing force or distance. However, only the same hand same foot solution will provide more long term benefits to performance. Newell's (1991) answer to this problem is to provide a highly constrained task environment that includes augmented information and verbal instruction. This can mean that the situation in which a task has to be performed in some way forces adoption of the correct solution. An example is the order of hand placement when learning to cartwheel: Learners have a habit of trying to follow the first foot with the opposite side hand, as opposed to first foot same side hand. When required to perform this act off the end of a bench, balance considerations force adoption of the correct method. Clearly not all movement problems can be solved in this way. However Newell advocates consideration of task, environment and organism, between these considerations rests constraints. In supplement to a directly constrained task environment Newell (1991) suggests the use of augmented information and verbal instruction. Verbal instruction in this sense can be seen as a necessary resource to be used sparingly.

Being an implicit performer is also more of a problem than at first it might appear, as the success of implicit learning models may rely on continued implicit performance. This suggests that performers would have to continue performing and practicing in this way indefinitely. It seems unlikely that golfers would choose to call out letters throughout a golf round. A point made by Seger (1994) is that although implicit processing shares similarities with automaticity it does show evidence of processing demand, therefore possible capacity limitation. This implies a finite robustness against anxiety and other processing demands. Such attentional demand also begs the question, would an implicitly learned task remain robust against continued cognitive inspection following acquisition. It appears then, that implicit learning also has its disadvantages particularly with respect to successful skill acquisition. With this in mind it seems more reasonable to suggest that the use of both implicit and explicit need to be explored carefully in order to avoid their respective shortcomings.

In trying to resolve a conflict between of Hardy et al. (1996) and Bright & Freedman (1998), the current experiment has also dealt tentatively with the relationship between practice and the conscious processing hypothesis. The current results suggest that more practice will lead to greater decrement in performance for explicit learning groups under pressure and further increases in robustness for implicit groups. These findings are consistent with the conscious processing hypothesis (Masters 1992). Though this provides evidence to support an avoidance of explicit methods for learning, counter arguments advocate the need for a more careful examination of both implicit and explicit learning methods.

Chapter 6

General Discussion

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Summary of results

This thesis addresses two distinct areas with respect to movement control. The first area investigated fine motor control and conscious awareness of it. The second area examines explicit knowledge or conscious processing and how it may interfere with a highly practised task. In Experiment one participants produced a series of force responses paced by an auditory signal. Force output was presented in the form of a variable bar chart on a visual display unit, the gain of which was changed on selected trial blocks. Participants were asked to discriminate between those trials they thought had changed and those they thought had not. The results revealed that participants were only able to reliably identify changes of 20% or above. However participants were able to make corresponding and distinct changes in output to changes as small as 5%. Indeed the frequency of responding showed that 5% changes were most often reported as no change at all by participants. No difference in capacity to identify change was found between a finger press and a push from shoulder height. This evidence was taken to support the notion that individuals have difficulty consciously identifying low level corrections to movements that they have made during performance. The findings were in line with other similar research (Fourneret & Jeannerod 1998, Georgieff & Jeannerod 1998) which supported a 'double coding' of movement information, whereby information delivered to consciousness about a movement was different to that used in actual movement control. In addition to this some evidence was found to support a visual dominance effect. In this case, even when suitable information is delivered from internal sources, discrepancy between that information and visual information has to be quite large before the discrepancy is acknowledged.

Experiment two was designed to examine the effect of different display resolutions and different force magnitudes on the ability to identify change using an identical paradigm. This was done using the finger press task, which was modified in order to perform a two way comparison: The first independent variable doubled available display resolution, the second doubled force output requirement.

The evidence from this experiment suggested that changes in display resolution had no effect on the ability to identify perturbations to the task. With respect to doubling force output it appeared that the relatively small changes employed were not sufficient to bring about changes in identification of perturbation. It was not clear from the results if larger changes in force magnitude would bring about similar changes in capacity to identify change. This was indicated by a marginally significant force effect, showing an increase in ability to detect change with greater force magnitude. These results may imply that detection thresholds are in some way linked to intrinsic output variability for the task. A model proposed by Carlton and Newell (1993) predicts that decreases in intrinsic variability will accompany increases in magnitude of force output. An increase in number of correct identifications of perturbation would correspond with the predicted decrease in force variability. Speculatively it might be suggested, that conscious detection of change can only take place at some margin above the intrinsic variability of the task.

Taken together these two sets of findings indicate that people have a relatively poor perception of low level movement correction and that the threshold for perception of correction is constant across effectors and KR resolution. It is also tentatively suggested that thresholds for the determination of change are in some way linked to intrinsic variability associated with the task. Support is found for a 'double coding' explanation which suggests that, conscious information about the performance of a task is different from the movement information actually used in the control of a task (Fourneret & Jeannerod, 1998; Georgieff & Jeannerod, 1998). In addition to this, there is some evidence to imply that a visual dominance effect i.e. believing what is seen rather than what is felt, may contribute to the high thresholds for detecting change. Given that this evidence seems to advocate a level of action which remains beyond the reach of normal conscious monitoring, tentative support is provided for motor control models such Bernstein's (1967) model; which argues for lower levels of control that operate outside the realm of consciousness.

The second area of investigation addressed by this thesis attempts to validate findings in support of differential effects on performance under stress for tasks learned by implicit and explicit means (Masters, 1992; Hardy al., 1996). Explanations of these effects in terms of the conscious processing hypothesis (Masters 1992) and processing efficiency theory (Eyesenck & Calvo, 1992), have been challenged by evidence presented by Bright & Freedman (1998) that supports a task difficulty explanation. Experiment three in this series endeavoured to replicate the methods used by Masters (1992), Hardy et al. (1996) and Bright & Freedman (1998). The aim was to show that quantity of learning is an important feature of examining the differential effects of implicit and explicit learning. This is of importance as other authors (Hardy & Mullen 2000) have argued that the task acquisition period used by Bright & Freedman (1998) was insufficient to support a successful replication of earlier methods. Experiment three failed to successfully replicate either method due to an insufficient stress manipulation which was crucial to the experimental test periods. It was decided to modify the stress manipulation used and attempt a further replication in Experiment four.

This was thought necessary because further research in the area was contingent on an interpretation of Masters (1992) and Hardy et al. (1996) that relied on differential learning rather than task difficulty. Experiment four replicated the results Bright & Freedman (1998) with the exception of a predicted improvement in performance for the IL group over the ILAST group. While this predicted improvement was important in supporting the task difficulty explanation of Bright & Freedman (1998), it did not militate against a learning based interpretation of results. Consistent with the learning based hypothesis that an increase in the performance for IL groups under testing was due to lack of practice, the second test phase in Experiment four revealed results matching those of Masters (1992) and Hardy et al. (1996). The fact that the Masters (1992) and Hardy et al. (1996) results were reproduced after more learning than was administered by Bright & Freedman (1998) supports the learning hypothesis that was proposed to explain the differences between the two sets of results. Interestingly, the results of Experiment four did not match those of Masters (1992) and Hardy et al. (1996) perfectly. For the first time in the series of experiments conducted using this paradigm the pattern of means suggested that the performance of the IL groups actually exceeded the performance of the EL group. This is entirely in keeping with the predictions of the conscious processing hypothesis. Overall these findings offer further support to the conscious processing hypothesis and introduce an important factor for future consideration: That of quantity of practice and its contribution to differential learning effects. At a more general level, variations in laboratory stress manipulations have been explored and the need for <u>exact</u> replications in future has been underlined.

Theoretical implications

A general implication to be drawn from the first pair of experiments is that in line with models of motor control such as that suggested by Bernstein (1967), there is a level of control to which consciousness has at best limited access. The second pair of experiments in the broad sense, suggest that following extended practice, "telling ourselves what to do" is somehow problematic. Successful skill acquisition in itself suggests that we acquire some degree of mastery or control over the lower levels of the motor system. It would seem that very accurate, fluid control resulting from practice appears to be achieved in an abstract manner. The extent of this abstract relationship becomes apparent when attempts are made to achieve the same ends through explicit means. A compelling issue is why this should be the case; some recent conceptions of motor learning and control point tentatively towards an answer.

A standard view of learning proposes that explicit/declarative information is changed into implicit/procedural information across a period of learning or that controlled processing is transferred to automatic systems (Anderson, 1982; Fitts & Posner, 1967; Schneider & Schiffrin, 1977; Logan, 1988). A good example of this is Anderson's (1982) ACT theory, which describes a process through which information for the completion of a task is broken up, chunked and converted into procedural information. This implies that information starts in one state and is then converted to another, therefore original information used in the control of a task may not remain intact after extended practice. This latter proposition is problematic as the conscious processing hypothesis relies on the notion that over anxious individuals access explicit information that is acquired earlier on in learning. It would seem for motor tasks at least, that explicit knowledge is retained and information for low level control is derived from it in effect leaving the explicit information intact. However, such a qualification would not be needed if it were the case that explicit and implicit learning were seen as separate processes. By this it is meant that explicit and implicit information are accumulated independently and in parallel across learning, in the support of separate but interacting systems for the control of movement.

A similar point of view is offered by Keele, Davidson & Hayes (1998) in their treatment of sequence learning effects. In an extensive review of sequence learning evidence, they propose that skill acquisition is supported by two separate systems attentional and non-attentional. These systems develop and acquire information about movement separately and in parallel. Keele et al. (1998) suggest that there is no direct link between the two systems. This has interesting implications, the non-attentional system being entirely separate, is always automatic, has its own language and by inference is impervious to conscious inspection. Automatisation in this case is not about transferring processing demands from one resource to another, but of acquiring sufficient knowledge in the non-attentional system to allow dependence on the attentional system to be reduced. Explicit or attentional systems therefore can be used to drive and construct a framework from which a non-attentional system can accrue sufficient task relevant knowledge. Once sufficient knowledge has been acquired by the non-attentional system, its efficiency at supporting a task should match or exceed that for the attentional system and therefore demands on the attentional system are reduced. Keele et al. (1998) support this idea of separate parallel learning systems with a considerable quantity of behavioural and neurological evidence, with this evidence come other useful implications. For example, a motor representation describes either where targets occur in space or how a response should move through space. Thus the explicit system should at the outset of learning describe at least some or all of the paths/endpoints involved. Explicit and attentional processes are according to Keele et al. (1998) different and separable. This is important as it suggests that changes in demand on an attentional system need not be reflected by changes in explicit knowledge. In the broad sense, the allocation of attention or effort may be seen as independent of explicit knowledge acquired.

The findings of this thesis have a number of implications that are consistent with the scheme proposed by Keele et al. (1998). Firstly is the lack of conscious knowledge that individuals appear to have about certain types of task that they perform. The implementation of tasks such as the force task used in the current thesis or the line drawing task used by Jeannerod & colleagues mat reasonably be argued to be for the most part implemented via the non-attentional system (the how). The target endpoints or outcome may along similar lines be argued to be the only portions of the task explicitly known (the what). Thus basic movement information about how movement goals are achieved, will remain outside the realm of consciousness. This of course will present difficulties in identifying when the respective tasks have been perturbed, as corrections are also likely to take place using the non attentional system. A strong view of the separation of attentional and non-attentional systems would suggest that no internal information can be gleaned about a task via a non-attentional system. This is clearly not the case as participants are certainly able to detect large perturbations as shown by the results of experiments 1&2. Additionally Georgieff & Jeannerod (1998) suggest some information may be necessary in order to establish the attribution of agency Although for the most part 'seeing is believing', to be certain that they are the ones who have initiated and controlled a movement, individuals need to have some feeling of the action that corresponds with what they see. Put another way, a person may see the outcome of a motor act that they believe to be conducted by themselves, but to be sure that act was actually carried out by them, it is necessary to detect a corresponding internal change (to feel it). Hence there is a need for "double coding" of movement information (Fourneret & Jeannerod, 1998; Georgieff & Jeannerod, 1998), which reflects both the actual state of the non-attentional system and offers enough "feeling" to permit an attribution of agency and control. More importantly as the questioning of agency rarely occurs, visual information about task outcome is usually accepted without question. This would perhaps help explain the visual dominance effect found in the force studies reported in experiments 1&2.

Issues concerning learning, de-automatisation, explicit information and the conscious control hypothesis can also be addressed through the framework proposed by Keele et al. (1998). To begin with, practice as stated earlier, facilitates the accumulation of information in the non-attentional system with the goal of decreasing reliance on the attentional system. This decrease in expected demand on attentional systems, would allow for greater capacity to perform a secondary task, therefore a secondary task may no longer constitute a burden that exceeds existing capacity later in learning.
A decrease then, predicts greater impact for the removal of a secondary task such as an AST (Articulatory Suppression Task) earlier in learning, than later in learning.
This assumes some degree of capacity limitation. Therefore removal of the AST after 160 trials of learning as in Bright & Freedman (1998) would lead to improvement in performance as a large amount of attention can then be devoted to the remaining task (putting). This is because both tasks have high attentional demand at this early stage of learning. The removal of the AST after 400 trials has less impact because the attentional demands of both the AST and the putting task will be lower following additional practice. This latter case is consistent with the findings of Experiment four, Hardy et al. (1996) and Masters (1992) which showed no additional benefit for IL groups despite the removal of the AST during the final test session.

De-automatisation may be seen as a switch from control at a non-attentional level to control at the attentional level along with associated penalties in terms of fluidness and efficiency. This may take place in parts or as a whole, but is likely to be predicted by greater quantities of explicit information held by an individual and the desire to feel in control of what they are doing. Note that changes in the effect of an AST on performance can be seen to operate independently of switches in control mode therefore separately from changes in anxiety.

The approach of Keele et al. (1998) is also reflected in Willingham's (1998) control based learning theory (COBALT). COBALT is based on three principles of motor control; neural separability, disparate representation, and dual modes of operation. Neural separability suggests that anatomically distinct parts of the brain subserve different cognitive processes. Disparate representation advocates that these different parts use different forms of representation (language of control). Finally, the dual mode principle proposes that motor acts can be executed in a conscious and unconscious mode. This appears to operate in a similar way to the attentional non-attentional distinction established by Keele et al. (1998). Although there are similarities between the two approaches, what makes Willingham's (1998) approach worthy of note is that he specifically addresses the problem of choking.
Willingham (1998) describes choking as non-optimal use of the conscious mode, proposing that neither arousal nor attention is central to choking. For Willingham (1998), it is the increased desire to perform well, facilitated by rewards or audiences that drive performers to use the conscious mode of control. Choking is use of the conscious mode of control instead of the unconscious mode. This is not only effortful, but also the conscious mode lacks the benefits of learning accrued at lower levels of the system, which are only available to the unconscious mode. Benefits contained at lower levels of

the system are described as "tuning" which reflect task optimisation resulting from practice. In this way the decrements associated with choking are likely to increase the more a task is practised. This view is entirely in line with the findings of Experiment four as the performance of the IL groups matched and nearly significantly exceeded that of the EL group.

As stated earlier, the success of the IL groups in Experiment four is unique due to the exceptional performance of the IL group during testing. This success it may be argued is due to the additional practice given to groups in this experiment. Unlike Hardy et al. (1996) or Masters (1992), participants in Experiment four received the equivalent of 100 additional trials of practice. The additional practice and the extra test are thought to have contributed to the exceptional performance of the IL groups in Experiment four.

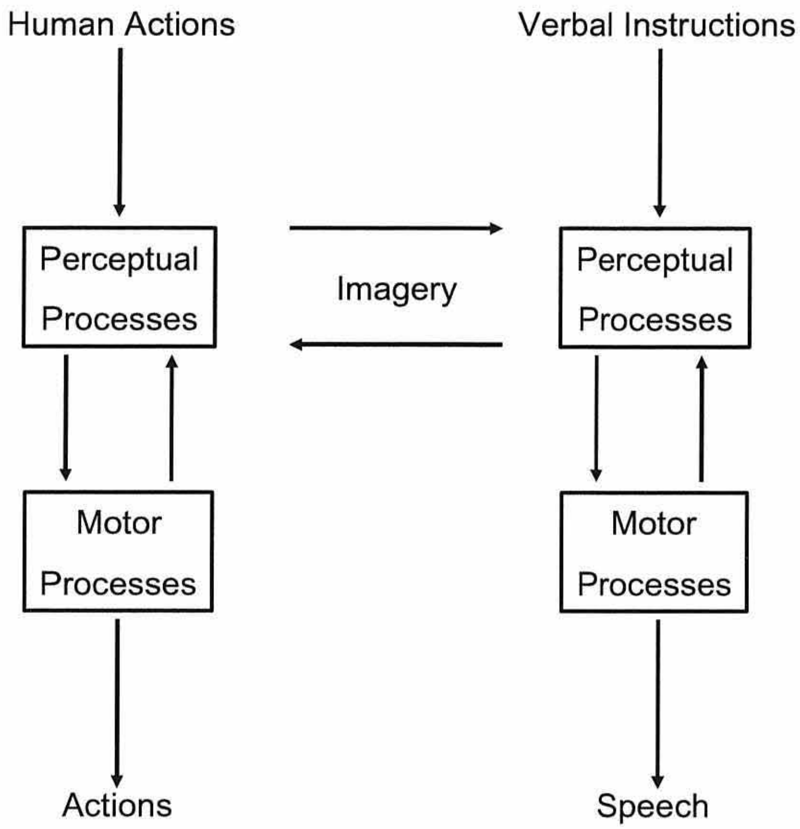
Models that propose distinct parallel learning processes such as those proposed by Keele et al. (1998) and Willingham et al (1998) have strength in addressing issues such as lack of conscious awareness in movement control and decrements associated with explicit control of movement. Perhaps worth noting is that whilst the models to some extent predict increased magnitude of performance breakdown with practice, neither is specific enough to address the nature of that breakdown.

Applied Implications

The generalisation that conscious attentional control using explicit knowledge is "a bad thing" could be made at this point. However, as discussed in the previous chapter there is a danger that the "the baby will be thrown out with the bathwater". By this it is meant that, explicit knowledge though problematic for performers under stress, is not to be avoided at all costs, as potential benefits for skill acquisition to be gained from explicit methods may be overlooked. A better question might be: If the use of higher order conscious or attentional modes of control has associated penalties, how might such penalties be reduced ? One answer may be to manage and identify limitations for the use of explicit information in the acquisition and control of a task (Masters 2000, Liao & Masters 2001, McMahon & Masters in press).

Verbal information as a form of explicit knowledge is a central feature of task acquisition or control and a necessity if the highest levels of performance are to be achieved. The first point to clarify is that explicit knowledge though verbalisable, may not in its raw form amount to words alone. Explicit knowledge could also amount to imagery or the ability to reproduce *and then* describe spatio temporal patterns associated with successful performance. Annett (1991, 1995) illustrates this point amply with his treatment of tying a shoelace. The point is made, that without actually tying a shoelace or making gestures to that effect, rendering a complete description of the process is quite difficult. Even when gestures are used, they tend to exaggerate certain aspects of the movement at the expense of others, differing considerably from the actual movement itself. A suitable model which encompasses this view, and works within the general framework of the models by Keele et al. (1998) and Willingham (1998) is presented by Annett (1991). This model is useful in that it deals specifically with the role of verbal information in the control of a movement task. A feature of this model is that Annett (1991) prescribes different action pathways for human action and verbal instruction. Crucially Annett (1991) describes imagery as being the link between the two, this is a central feature of the model. See figure below.

Figure 6-1. The 'action - language bridge' diagram modified from Annett (1991).



In line with neurological evidence, verbal and action pathways are separated, as are systems of representation and production. In this way both the representation and production of speech is not directly integrated into systems for action and movement. Verbal instruction is accordingly translated into imagery propositions which may be fed into the action system. This separation of verbal and action systems is echoed by Reber (1992), who agues that verbal, analytical and explicit systems arrived later in evolution than implicit systems. An implication of this would seem to be that verbal systems are less well integrated and have undergone a separate evolution to motor systems. The key issues to acknowledge here are the separation of processing into verbal or motor streams

and the role of imagery in facilitating communication between the two. With separate supporting systems, information relevant to each stream can evolve at a different rate and use a different language of representation. Thus verbal instruction cannot be translated directly into action information, but needs translation into an imagery propositions first if control is to be achieved by verbal means. The idea that translation of verbal information into propositional information comes at some cost to the system is entirely conceivable and in line with previous models by Keele et al. (1998) and Willingham (1998). Annett's (1991) approach also raises some additional issues worthy of consideration. It appears possible according to Annett's (1991) model that each verbal instruction would need its own corresponding imagery proposition. Therefore the greater numbers of individual verbal instructions, the more imagery propositions are needed. Clearly this has implications for cognitive capacity and performance. In this way instructions that can be translated into propositions and contain large amounts of movement information are likely to prove very useful.
It might also be speculated that certain types of instruction would invoke certain types of imagery and that some types of imagery may be more advantageous than others.

In line with Annetts' (1991) model, it seems reasonable to suggest that the cost of making use of verbal information for the control and acquisition of a task is likely to be highest when it refers directly to the dynamics of that task. In particular, most disruption is likely to take place if verbal information has to be translated into action propositions while the task is being performed. In situations where use of verbal information is unavoidable, minimal information that is readily translated into perceptual propositions would have an advantage. This notion is supported in the ideas of metaphor or analogue learning (Masters, 2000; Liao & Masters, 2001). Metaphors or analogues can be seen as a single item of information which can be used to convey a large quantity of technical instruction as a single item such as "hold your racquet like an axe". Another alternative may be to use demonstrations or imagery, as they do not require translation from words in to action (Annett, 1991,1995).

Verbal knowledge used in the preparation of movement is also less likely to incur meaningful penalty, as any translation for action takes place before movement commences. This is in line with the findings of Jackson & Wilson (1998) which showed that participants, who used self set or given set up instructions, showed no evidence of choking when compared to those given movement related instructions. This view is consistent with other lines of existing thought within the literature, for example: Gentiles (1998) distinction between processes that describe the functional relationship between the performer and environment, and those that determine the dynamics of movement. Processes that describe the functional relationship between the performer and the environment are likely to benefit from coaching statements such as "watch the ball", because these statements direct attention towards perceptual variables used in control of movement. Whereas, statements such as "get your wrist over the ball" in a forehand tennis stroke are more likely to be problematic because they interfere with existing descriptions of movement dynamics. These issues would substantiate the need for verbal information to channel attention towards particular perceptual variables (watch the ball) and for get set behaviours (feet 18 inches apart). Some support is garnered for this point of view by the findings of Wulf and her colleagues (Wulf, Luaterbach & Toole, 1999; Shea & Wulf, 1999; Wulf, Hoss & Prinz, 1998) who indicate that an external focus of attention during learning is more beneficial than an internal one.

At an applied level Singer, Lidor, Cauraugh (1994) prescribe a ready, image, focus, execute and evaluate approach to attentional focus in movement control. In this process: Ready sets up the body, image causes the imaging of the movement to be performed, focus directs attention to a useful perceptual variable, execute instantiates the movement, and evaluate focuses attention on the movement as it was performed ready for comparison with other outcome information. It should be noted that this approach predominantly advocates training in the allocation of attention.

Training in the allocation of attention fits in with the implications drawn form Keele et al.'s (1998) model as it directs attention to the resources which permit the non-attentional system to acquire information. In terms of control, it is worth noting that Singer et al's (1994) procedure shifts attention away from movement related areas to perceptual areas. For example, attention is focussed on the ball just before executing a tennis serve. The important feature here is that execution of the movement does not appear to demand more than a minimum of attention, thus interference with movement dynamics is avoided. The external focus attention employed by Wulf et al., (1999;1998) also fits in with this view. However such simple distinctions may prove deceptive as a subsequent study by Wulf, McNevin, Fuchs, Ritter & Toole (2000) showed advantages for focus of attention on the ball leaving the racquet rather than its approach as beneficial. In the same study focus on the action of the club rather than the trajectory of the ball appeared to have the advantage. Though attention away from movement dynamics is still supported, different foci may need to be developed for different tasks.

From these guidelines a taxonomy of instruction for the control and acquisition of movement that focuses attention away from movement during execution can be explored. This is likely to prove quite challenging as certain types of verbal information may have influence in more than one area and the differences between helpful and unhelpful instructions could prove quite subtle.

Future Research

Based on the findings within this thesis and the issues discussed above, useful future lines of investigation may be drawn. Some of these might address theoretical issues and the robustness of existing findings alongside practical applications. A logical extension of experiments one and two would be to explore; what sort of changes if any take place in the ability to identify perturbations when the finger press task used in Experiment two is highly practised. To examine the relationship between levels of control it would seem necessary to devise a means of comparing attempts at control of a well learned task using conscious, explicit or attentional control with attempts using un-conscious, implicit, non-attentional methods. From here it would seem important to examine whether specific types of information can be linked to different types of performance breakdown. To date Mullen et al. (under review) has provided the only measure of movement changes that result from performance breakdown by showing that anxiety caused a stiffening in the distal portion of the wrist joint in a putting task. Clearly then an important shift in focus, is not to look at changes in outcome measures such as scores but to look at the movements themselves. At a more practical level there is a need to examine methods of learning that do not involve direct movement instruction such as metaphor or analogy methods. Other directions include approaches that encourage performers to allocate attention in specific ways, in particular away from movement dynamics during performance. As it stands, the success of "swing thoughts" (Jackson & Wilson, 1998), and process goals (Kingston & Hardy, 1997) in facilitating performance under pressure is likely to prove difficult to interpret, as the terms described seem too vague to match the issues and directions discussed here. There may also be some latitude for a shift in focus away from stress and anxiety per se towards issues relating to the allocation and effects of "effort" on performance. It is clear that effort provides a link between conscious processing, processing efficiency, stress and performance breakdown. This issue has been addressed by a small number of researchers so far, for example; Mullen & Hardy (2000) have used self report measures of effort and Mullen et al. (under review) used both spectral analysis of heart rate and self report measures.

Limitations of the Thesis

It may be argued that the research described here lacks ecological validity, though as Heuer (1988) points out, this is not in itself a problem for the building and testing of theory. However some caution should be expressed before generalising these findings to other areas. In particular there is some concern about anxiety manipulations and tasks used in the experiments. So far the majority of research cited with respect to conscious processing hypothesis has used golf putting as the task to be examined. There is clearly the need to find at least one other task that can be manipulated in the same way, particularly as the majority of other related evidence is based around cognitive rather than motor tasks (Baumeister, 1984; Langer & Imber, 1978; Reber, 1992). The only other motor related task investigated in this way, was the computer based catching task used by Green & Flowers (1992). Perhaps less important in this respect are the tasks used in experiments one and two, because Jeannerod & colleagues have already offered similar findings using line drawing rather than force production. With regard to anxiety, Bennett (2000) points out that the stress manipulations discussed here show little correspondence with those encountered on the field, particularly with respect to their magnitude and impact. It might be speculated that more stress would lead to bigger breakdowns and some anecdotal evidence does support this view. However, ethical difficulties are likely to restrict comprehensive exploration of this topic both in the laboratory and in the field.

Strengths of this thesis

As suggested in the introduction this thesis has provided opportunities to address research questions from the perspective of both motor control and social psychology of sport. As a consequence experience has been provided in theory and methods relevant to both fields. In this way it provides a valuable platform from which to launch a research programme and some useful questions have been identified as a result.
In pursuing two strands of research, both a novel issue in motor control was addressed and a contribution was made to an ongoing commentary in sport psychology.

Summary and conclusion

The main purpose of this research programme was to begin to address the question; How and why do people mess up when under pressure ? This question was addressed through two research strands. The first strand dealt with force control and the conscious perception of change. Results indicated that there is a level of correction employed for certain tasks that operates outside the scope of conscious perception.
It appears that we know less about our actions than we think ! The second strand dealt with the effect of stress on motor performance and how implicit and explicit learners respond to that stress. Results found support for the conscious processing hypothesis of Masters (1992). In addition results also supported the suggestion that more practice leads to greater decrements for explicit learners and more benefits for implicit learners. Taken literally this might suggest we should all be learning skills implicitly and learn nothing explicit in order to avoid "telling ourselves what to do". Taken together

though, the results as a whole might be taken to support the a more parsimonious viewpoint that; as learners, performers and actors we should tread carefully between controlling what we do and letting ourselves get on with it !

In the pursuit of learning, every day something is acquired.
In the pursuit of Tao, every day something is dropped.

Less and less is done
Until non-action is achieved.
When nothing is done nothing is left undone.

The world is ruled by letting things take their course.
It cannot be ruled by interfering.

Lao Tsu, 600 BC.

Appendices

Appendix a: Putting instructions presented to the EL groups in experiments 3 & 4.

The following instructions in golf putting are borrowed in a slightly modified form from books entitled 'Golf: The Skills of The Game' (Stirling, 1985) and 'The Young Golfer' (Saunders & Clark, 1977). You will receive them at the beginning of each session. Always read them carefully and follow them strictly, as they will show you the correct way to putt a golf ball.

Set the clubface behind the ball with the face at right angles to the hole, and have all of the sole of the putter on the ground.

Use the reverse overhand grip. This means that the forefinger of the left hand overlaps the little finger of the right hand. (Reverse this if you are left-handed). There is a feeling of more control with the dominant hand when using this hold, and this is an essential feeling to have when trying to roll a ball along a line to a specific point.

Stand with the distance between the heels in the region of ten to twelve inches (25-30 cm)...The alignment of the shoulders, hips, knees and feet should be parallel to the ball-to-target line.

The body should be bent over until the eyeline is directly over the ball-to-target line. Ideally, the person should feel that a balanced stillness can be maintained in the body as the arms and putter make the stroke.

Ball position is also critical, as the ideal point of contact is when the putter is traveling at the lowest point of its swinging arc. At this stage, it is square and traveling through to the target....Placing the back of the ball at a point opposite the inside of the left heel gives the best opportunity of achieving ...this.

The actual swing of the putter is made by the arms with the hands serving as the connecting link. Because of the forward bending at address, the arms bend slightly and it is vitally important to maintain this degree of bend throughout the stroke.

Another very important feeling is that of moving the top of the shaft and the putter head back and through together. This cancels out wrist action....

In a good putting stroke, the putter should move back and through smoothly; with the putter very low to the ground.

The most important thing about the putting stroke is to take a fairly short backswing so that you can push the club firmly at the hole. Never take a long backswing with a putt so that you have to slow down. Short back; firm through.

Keep your head absolutely still. This will help you make the club travel in a perfectly straight line. If you move your head, you will find the putter travels off its line in the throughswing and is pulled in towards your feet. Never look at the ball as it reaches the hole....Always keep your head perfectly still until you hear the ball drop in. Only look up once you hear it drop, or once you're sure it's missed!

Appendix b: General instructions given to participants on day 1

Please read these instructions carefully.

You are required to complete 200 putts. These can be completed in your own time. After completing the first 40 putts there is a rest period, after which you will complete the next 40 putts until the 200 putts are completed. Simply try and get as many putts as possible into the hole.

You are reminded that you have been requested not to talk to anyone about your task until after the study has been completed, in Jan 2001 *Please* adhere to this request.

You are also requested not to think about, rehearse or practice putting while away from the experiment.

Thank you for your help.

Appendix c: General instructions given to participants on days 2-4.

Please read these instructions carefully.

You are required to complete 100 putts. These can be completed in your own time. After completing the first 50 putts there is a rest period of 5 minutes duration, after which you will complete the next 50 putts. Simply try and get as many putts as possible into the hole.

You are reminded that you have been requested not to talk to anyone about your task until after the study has been completed, in Jan 2001 ***Please*** **adhere to this request.**

You are also requested not to think about, rehearse or practice putting while away from the experiment.

Thank you for your help.

Appendix d: Instructions given to the IL& ILAST groups

The following instructions are to be carried out while you are putting. You will receive them at the beginning of each session. Always read them carefully and follow them strictly.

Imagine drawing letters of the alphabet from hat one at a time, calling the out and replacing the in the hat. You will need to call a letter out every time you hear the beep. Any of the 26 letters will be equally likely to be drawn from the hat. A sequence of letters like this would be expected to be all jumbled up and random. It would be very unlikely that the sequence would consist of any words or sequences such as ABC or XYZ. Nor would groups of letters be likely to be repeated regularly. You must call out a randomly generated letter each time you hear a click.

Appendix e: Stress inducing instructions for EL & ILAST Groups.

THIS SESSION IS A COMPETITION

The forthcoming session is a competition. You will be permitted 40 putts only. Your performance in this competition will determine if you are eligible for a further reward. If you come first you will receive £25, if you come second you will get £15, third £10 and fourth £5. A placing below fourth will not entitle you to receive a reward. A further prize of £10 will be awarded to the competitor showing the best putting technique overall. The experimenter and a professional golfer who will watch a video recording of your performance will judge your technique

Appendix f: Stress inducing instructions for IL Group.

THE NEXT SESSION IS A COMPETITION

The forthcoming session is a competition. You will be permitted 40 putts only. Your performance in this competition will determine if you are eligible for a reward. If you finish in first place you will receive £25, if you come second you will get £15, third £10 and fourth £5. A placing below fourth will not entitle you to receive a reward. A further prize of £10 will be awarded to the competitor showing the best putting technique overall. The experimenter and a professional golfer who will watch a video recording of your performance will judge your technique. During this session you will not be required to call out letters.

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