Investigating Cognitive Effort And Its Role In Control Over Pavlovian Bias

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Declaration

I, Hugo Alexander Fleming, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

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Abstract

Effort is a key determinant of cognitive performance, particularly for processes involving cognitive control – without it, performance may be slow, inaccurate or biased. Related to this, decreased ability to exert effort has been implicated in the symptoms of conditions including depression and anxiety. In this thesis I investigate the role of effort in the specific case of control over Pavlovian biases. In the first two experimental chapters I examine whether Pavlovian biases are in principle modifiable, a necessary precondition for demonstrating that they are also controllable. Following a simple programme of behavioural training, participants showed reduced influence of Pavlovian biases on behaviour, a result which is consistent with increased cognitive control. In the third experimental chapter, I present a new task for measuring cognitive effort sensitivity, suitable in particular for individual differences research. Subsequently, in the final experimental chapter, I use this task to test directly the hypothesis that the strength of Pavlovian bias is influenced by effortful cognitive control. I present initial evidence that indeed willingness to exert effort and the strength of Pavlovian biases seem to be negatively correlated, while effort also seems to be negatively associated with both depression and anxiety symptoms. Finally, in a standalone theoretical chapter, I discuss the rationale for effort costs, which currently are not well understood; I introduce and extend two existing ideas from outside of neuroscience which I think may be informative in this regard. Overall this thesis extends our understanding of the link between effort and control, suggesting in particular that the expression of Pavlovian biases can be framed in terms of effort-based decision-making. Additionally, by introducing fresh ideas about the basis of cognitive effort costs, it is hoped that this thesis will provide stronger foundations on which experimental research on cognitive effort can be built in the future.

Impact Statement

Impact within academia

The primary scientific significance of this thesis is in advancing and improving our understanding of the links between effort and cognitive control. The experimental and theoretical chapters of this thesis are intended to be published in scientific journals in the near future and in so doing the results contained in them will be disseminated for other scientists to read. Indeed it is my intention for these papers to be published in open access journals so they will in fact be able to be read by anybody, including members of the public.

Chapter 4, in which I present a new task to measure cognitive effort sensitivity, will be particularly useful for other researchers as it represents a significant advance in our ability to accurately measure individual differences in cognitive effort. This in turn opens up a number of new areas of study that were not able to be investigated previously due to the lack of tasks with appropriate controls.

Finally, as I mention at the end of Chapter 6, the theoretical ideas I discuss in this thesis are novel and somewhat speculative in their current state. I would like over the coming years to build collaborations with other researchers, particularly those from adjacent fields in mathematics and physics, who would be able to help develop these ideas into more specific predictions, which could then be tested experimentally. This has the potential to develop into a substantial programme of research and, if validated, would have fundamental implications for understanding both optimal decision making and the physiological constraints on the brain.

Impact for society generally

Beyond academia, this thesis is also relevant to society at large. In Chapters 2 and 3 I show that the strength of Pavlovian biases can be reduced following a programme of behavioural training, and I suggest that this has potential as a treatment for some of the symptoms of anxiety and depression (in which Pavlovian biases have been implicated). There is much more work to be done before this potential can be realised, but if successful then this would be an important step towards tackling the cognitive symptoms of common mental health conditions.

More generally, effort is an important feature of many aspects of cognition, contributing not just to disease but also to healthy variation in performance. Often there are times when we would like to be able to exert more effort than we feel able to – in the short to medium term, it is hoped that the work contained in this thesis will at least contribute to a better understanding amongst the public of *why* this is the case, and what factors contribute to the decision to exert (to a greater or lesser extent) cognitive effort. This impact can be accomplished by my taking opportunities to present the results in this thesis to public and non-specialist audiences over the coming years. In the longer term the ideas in this thesis could, as part of a much larger field of research, eventually contribute to a better understanding of how effort can be deliberately regulated and thus how we might seek to enhance human cognition.

Acknowledgements

I would like to thank my supervisors, Professors Oliver Robinson and Jonathan Roiser, without whom of course none of the work comprising this thesis would have been possible. Over recent months as I have been writing this thesis, Oli in particular has gone above and beyond, reviewing a huge volume of writing quickly and efficiently and providing invaluable feedback throughout – I'm hugely appreciative of his efforts.

The Neuroscience and Mental Health group have all been brilliant, but there are two names that I would like to mention in particular. I have been lucky to work with, and be informally mentored by, two brilliant postdocs, Vincent Valton and Alex Pike. Both have been endlessly helpful, providing advice and inspiration whenever I have run into modelling problems, entertaining my many speculative ideas even when they definitely had better things to be working on, and generally being extremely generous with their time and knowledge.

I am grateful to the Wellcome Trust, who not only funded my PhD studentship in the first place but also provided our cohort with a 6-month stipend extension when it became clear that COVID was going to be a significant issue. Being supported by an organisation that takes care of its students in such a generous fashion was hugely encouraging and made a big difference at a time that was disruptive, but could have been a lot worse without the support that the Wellcome Trust provided.

Speaking of COVID, it would be remiss of me not to mention my parents, who have given up so much over the years to support me; even so I do not think that when I 'temporarily' returned home in March 2020 they were quite anticipating I would stay for nearly two years! Nevertheless we survived and I am so grateful for all that they did during that time (and, indeed, in the twenty five years prior as well). Finally, I would like to thank my partner, Arabella, whom I first met right at the start of this PhD and who will now, finally, be celebrating with me at its conclusion.

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Note to examiners

The findings from Chapter 4 have previously been included in the following preprint report:

Fleming, H., Robinson, O., & Roiser, J. (2021). Measuring effort without difficulty. *PsyArXiv.* https://doi.org/10.31234/osf.io/rbwmp

Chapter 1. General Introduction

Effort is a ubiquitous feature of everyday life. Physical effort is perhaps the most obvious manifestation – lifting a heavy weight off the ground requires more effort than a lighter one, running takes more effort than walking, and throwing a ball as far as possible is more effortful than aiming for a shorter distance. In all of these cases, 'effort' describes the amount of resources that get devoted to a task (e.g. the force of muscle contractions), with more effort leading to greater performance, but also being accompanied by an aversive sensation, a subjective cost of exerting effort.

The same arrangement seems also to hold in cognition – while there are some cognitive activities that are entirely automatic and reflexive, most require some degree of conscious engagement, the extent of which can be varied. In colloquial terms, we say that we can decide how hard we 'try'. For example, mental arithmetic requires effortful attention, without which you are likely to take longer and could also make a mistake; so too does close reading; even everyday problem solving, like planning a meal or arranging to see friends, requires some degree of effort, without which it is hard to do these activities effectively (as in more severe cases of depression or schizophrenia; Perini et al., 2019; Kaneko, 2018). In common with the notion of physical effort is the sense that there is some continuous resource which can be deployed when carrying out a cognitive task, which is necessary in order to achieve desirable outcomes but which is accompanied by an unpleasant and aversive sensation that limits the amount of effort one is willing to exert. The examples above also highlight the overlap between cognitive effort and concepts like conscious attention, working memory and cognitive control.

The chief significance of cognitive effort is that it determines the level of performance that can be achieved on a task. If we consider there is a maximum potential we could achieve, effort determines the proportion of this that is actually expressed. In other words, effort is "the mediating factor between cognitive capacity, on the one hand, and performance on the other" (Shenhav et al., 2017, p.101). In this regard, effort is critical to a number of important cognitive processes, being associated with more rational reasoning and less use of heuristics (Shah & Oppenheimer, 2008; Toplak et al., 2011; Venkatraman et al., 2009), improved working memory (Westbrook et al., 2013) and greater facility for flexible task-switching (Koch et al., 2018). In wider life, although exertion is difficult to test rigorously, self-report measures suggest that greater disposition to engage in cognitively demanding activities also predicts better academic performance, employment status and even IQ score (Cacioppo et al., 1996; Duckworth et al., 2011; Tangney et al., 2004). Cognitive effort is therefore potentially key to understanding why people behave as they do and, moreover, how we might intervene to improve cognition. Effort is probably the most promising target for enhancing cognition given that, by definition, effort levels are not fixed but instead can be manipulated flexibly; in contrast, the cognitive capacity side of the equation is likely to be structural and so less easily changed.

Along similar lines, understanding how decisions about exerting effort are made will likely be key to understanding the cognitive symptoms of a number of mental health and neurological illnesses. For example, increased sensitivity to effort has been implicated in symptoms of anhedonia and apathy, which are an important feature of both depression and some neurological diseases such as Parkinson's (see Husain & Roiser, 2017, for a review). In schizophrenia impaired cognitive function is amongst the most debilitating symptoms (Green, 1996; Green et al., 2000; Tabarés-Seisdedos et al., 2008) and may be related to increased sensation or effects of cognitive effort (Fervaha et al., 2013; Gold et al., 2013; Gold et al., 2015). Finally, anxiety entails an increased influence of avoidance biases on behaviour (Krypotos et al., 2015; Mkrtchian, Aylward, et al., 2017; Mkrtchian, Roiser et al., 2017; Robinson et al., 2013), which can be framed as a question of cognitive effort, in so far as optimal responding may require greater control when you are anxious.

Focussing on Pavlovian bias and effort

The latter example of avoidance biases brings us to the particular focus of this thesis, namely the role of effort in Pavlovian biases. These biases are fixed responses to Pavlovian predictions of reward and punishment, and entail the invigoration of action when rewards are anticipated (termed an 'approach bias') and inhibition when punishments are expected ('avoidance bias'). Naturally these responses are, at least some of the time, suboptimal and will lead to negative outcomes (as for example when one needs to remain still in order to gain a reward, or interact with a stimulus to prevent a punishment). In these situations, cognitive control is thought to be able to regulate the balance between the Pavlovian and other action selection systems, by reducing the influence of the Pavlovian system on behaviour; this control, in turn, requires effort. Thus there appears to be a causal chain from exerting effort to increased cognitive control, to attenuated Pavlovian biases and finally to better, more appropriate behaviour (Figure 1.1). The role of cognitive control in Pavlovian biases has not, however, been investigated a great deal, and the link to effort specifically has not been studied previously at all.

Figure 1.1. The proposed relationship between effort, cognitive control and Pavlovian biases. Effort is required to exert greater cognitive control, which in turn is able to overcome and reduce the influence of Pavlovian biases.

The studies described in this thesis are therefore aimed at exploring this gap in our knowledge with regard to the role of effort in control over Pavlovian biases. This is not just important for its own sake but also because Pavlovian biases provide a relatively well defined context in which to investigate aspects of effort and control that will be relevant to other cognitive processes as well.

In the remaining sections of this introduction I will present and discuss the existing literature on these two issues, cognitive effort and Pavlovian biases, in turn. Subsequently, I will introduce the key questions that this thesis is aimed at tackling, followed by an overview of the individual chapters and their aims and hypotheses.

1.1 Cognitive effort

1.1.1 Defining cognitive effort

Cognitive effort can prove something of an elusive subject so it is helpful at this stage to set down precisely how I shall be using the term. If we imagine that we have some maximum cognitive capacity available to do work on a task, then cognitive effort is the process by which a proportion of this capacity is selected and employed. In this sense, as we have noted above, effort is a mediator, and it is the combination of capacity and effort (together of course with the inherent difficulty of the task) which determine how well some cognitive operation is carried out (Figure 1.2).

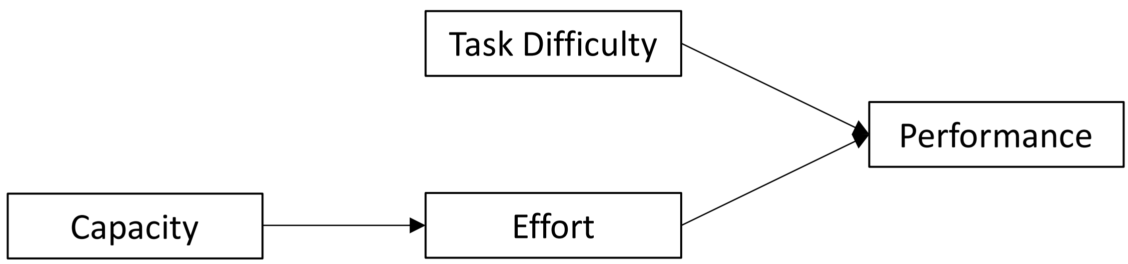


Figure 1.2. Defining cognitive effort. Cognitive effort describes the proportion of available cognitive capacity that gets employed on a particular task, and therefore contributes to successful performance.

Supplemental to this definition, it is important to distinguish effort explicitly from some of the concepts often associated, and even conflated, with it.

In the first place, much of the potential for confusion comes from the fact that, in everyday language, effort can mean both how much we 'try' and also how aversive a particular task is. We talk of 'giving' and 'putting in' effort to a task, meaning devoting some quantity of our available resources to it; on the other hand, if an activity is 'effortful' we mean it is unpleasant to do. The distinction between these two senses of effort is subtle but important. For the avoidance of doubt, the former (effort as the amount of cognitive work done) is the sense that shall be employed throughout this thesis and where I refer to the latter it will be qualified by the term effort costs.

Cognitive effort also needs to be distinguished from several other overlapping, but not completely redundant concepts (see Table 1.1). For instance, although the effort required by a task frequently covaries with its *difficulty*, the two are not exactly equivalent. The same task can demand very different levels of effort from different people – driving for example can be incredibly effortful for a learner but relatively easy for an experienced driver. Indeed any experienced driver was of course also a learner at some point, so even for one person the effort required by the same task can change over time. There is also the phenomenon of 'flow', where a task that usually requires effort can sometimes be carried out automatically, without much conscious awareness (Nakamura & Csikszentmihalyi, 2014). Although otherwise beyond the scope of this thesis, the existence of flow suggests that effort is not directly linked to objective features of a task like its difficulty; instead, it is a subjective phenomenon that is influenced also by other elements of psychology including motivation and the learned value of effort (Shenhav et al., 2013). Perhaps the best way to sum up the difference is to say that effort as a construct is used to account for the remaining variation in performance after having controlled for task difficulty.

Second, although effort frequently accompanies *attention* (to such an extent that Kahneman, 1973, conflates the two entirely), a distinction should be drawn between them. It is true that, in many ways, both attention and cognitive control (see below) capture something of what we mean by effort – namely, increased engagement with a task, and the deployment of greater resources to its completion. As both Kaplan and Berman (2010) and Westbrook and Braver (2015) note, however, only top-down, volitional attention is experienced as effortful; bottom-up attention is not, and we should be careful not to confuse the two.

Table 1.1. Concepts with which effort is often conflated.

Concept	**Summary**	**Relevant citations**
Task difficulty	Difficulty is a feature of the task itself, while effort is psychological and subjective. E.g. consider ‘flow’ states – the difficulty of a task stays the same, but the effort required seems to be much reduced.	Nakamura & Csikszentmihalyi, 2014; Shenhav et al., 2013
Attention	Conscious attention/focus is closely aligned with effort; but unconscious, bottom-up attention is not effortful at all.	Kahneman, 1973; Kaplan & Berman (2010); Westbrook & Braver (2015)
Cognitive Control	Some have suggested that cognitive control and effort are synonymous; ‘control’, however, entails a stronger commitment to particular cognitive mechanisms, whereas effort is broader and refers to any deployment of flexible cognitive resources. Control may be regarded as a specific implementation of effort.	Shenhav et al., 2013; Shenhav et al., 2017; Westbrook & Braver, 2015

This brings us finally to the notion of *cognitive control* (that is, the set of volitional, internally-directed processes which manage the resources of the brain). It is control processes which have been most closely associated with effort in the recent literature (Botvinick, 2007; Kool et al, 2010; Shenhav et al., 2013, 2017). Again, however, effort is not exactly synonymous with control, at least in part because, conceptually, the term cognitive control refers specifically to the balance between automatic and non-automatic cognitive processing, and entails a set of mechanisms involving monitoring and interacting with cognitive signals; effort, on the other hand, is a broader term connoting the deployment of cognitive resources more generally. Thus we might say that effort is expressed or implemented through cognitive control (Shenhav, 2017; Westbrook & Braver, 2015). Nevertheless we should acknowledge that, of all the concepts related to effort, cognitive control is probably the one that is most strongly related. I return to this issue in Section 1.2.5 below.

1.1.2 Measuring cognitive effort

Although, as I have said, it is important to distinguish between effort and effort costs, the latter are in fact what most cognitive effort measures focus on. This is partly due to expedience – effort costs are relatively overt and easy to measure compared with trying to infer effort from performance – and partly because effort costs and effort-based decision-making answer directly the question of *why* people behave as they do. All else being equal, effort costs are a demotivator; they encourage people to avoid exertion. This has been recognised scientifically for over a century, with Thorndike writing that "feelings of fatigue… serve as a sign to us to stop working long before our actual ability to work has suffered any important decrease" (quoted in Kurzban et al., 2013, p.665). In the 1940s, Hull stated as one of his 'Principles of Behaviour' that animals seek to minimise their work or exertion (Hull, 1943). More recently, this principle has provided the basis of modern methods of assessing cognitive effort, which focus on the economic decision-making aspect, the choice to exert effort (or not) rather than the performance of effort itself.

Most cognitive effort measures fall within the broad category of 'demand selection tasks'. These are tasks in which participants have a free choice whether to engage in some effortful activity or do something else. In the earliest examples of these tasks, participants simply chose between low-effort tasks and high-effort tasks with no specific incentives. For example they may have had to choose between task switching with different frequencies of switching, or making judgements based on information held in working memory versus simple perceptual judgements (Kool et al., 2010; Kool & Botvinick, 2014). These foundational studies were the first experimental test of Hull's principle stated above – they established that participants *do* systematically avoid tasks that demand greater cognitive effort when given a free choice (Kool et al., 2010), but can be enticed into selecting the higher effort-demanding option when monetary incentives are on offer (Kool & Botvinick, 2014).

One issue with these early experiments was that they only described a categorical relationship between effort costs and behaviour, namely that people prefer low effort to high. Subsequently, tasks were developed that allowed continuous measurement of effort costs. For example, Westbrook and Braver (2013, 2015) introduced a task paradigm they called 'Cognitive Effort Discounting', which titrates monetary rewards against different levels of cognitive demand (on the N-back working memory task) until they reach an indifference point; this allows the experimenter to quantify the subjective costs of cognitive effort. Similarly, Apps et al. devised the 'Rapid Serial Visual Presentation' (RSVP) task, in which participants first learn to perform an effortful task with several different levels, and then in a separate phase make a series of choices between a fixed low-effort/low-reward baseline option and a variable higher-effort/higher-reward option (Apps et al., 2015). This allows measurement of the effort discounting function – the way that the value of a choice progressively decreases as the offered effort level increases (Apps et al., 2015; Chong et al., 2016). Using these and similar tasks, researchers have shown for example that effort costs increase with age (Westbrook et al., 2013), are modulated by dopamine availability (Froböse & Cools, 2018; Westbrook, van den Bosch, et al., 2019) and are associated with activity in the dorsal Anterior

Cingulate Cortex (Shenhav et al., 2013, 2017), the prefrontal cortex and anterior insula (Chong et al., 2017).

Clearly a key component in these tasks is the effortful activity offered, and the manipulation of effort within this. There are a number of important design considerations in order to avoid potential confounding: for example, as the effort level is increased, the duration of a trial needs to be held constant so that the rate of reward also does not change; similarly, the difficulty of the task needs to be kept the same, so that the probability of success, and therefore of winning reward, is consistent. The latter is unfortunately an issue that has not previously been given much consideration in cognitive effort tasks, and is particularly a problem where these tasks are increasingly being used for individual differences research – if the task difficulty is not standardised and participants are not matched for baseline cognitive capacity, then differences between participants will be confounded by the probability of obtaining reward. Unfortunately, the most frequently used effort manipulations, such as levels of the N-back working memory task (Westbrook et al., 2013), response inhibition on Stroop-like tasks, and frequency of task switching (Kool et al., 2010; Kool & Botvinick, 2014) are indeed intrinsically more difficult as the effort level increases, because the amount of mnemonic resources has to be split between more items (on the N-back), or because there is a prepotent bias to be overcome (on the Stroop-like and task switching tasks). One exception to this is the RSVP task (Apps et al., 2015), in which the effort manipulation depends on the frequency of attention shifting from one stream of alphanumeric characters to another – there is no reason to think that attention shifting should intrinsically be associated with lower rates of success, so within participants at least there does not seem to be a confound. However the difficulty of the task is not standardised across participants (indeed Apps et al. found that participants' success rates during the training phase significantly predicted their subsequent choices) so comparisons between individuals are likely still to be confounded. There is therefore a clear need to devise a new task that addresses the confound of reward probability completely, and is therefore suitable for individual differences research – this is a requirement I will address in Chapter 4.

1.1.3 Theories and perspectives on cognitive effort

Early attempts to understand the nature of cognitive effort are often traced back to Kahneman's 1973 book 'Attention and Effort' which sought to muster over a decade's worth of research in service of the idea that, as the title suggests, effort can be directly equated with attention. Kahneman characterised attention as a limited capacity resource that is dynamically allocated according to task demands. Crucially, this resource is capacity limited at any one time but not depleted over time.

An alternative class of theory takes the opposite position on the nature of cognitive resources. The idea that tasks consume (metabolic) resources is intuitive and has a natural analogy in physical effort. Probably the most prominent example of this approach is the Ego Depletion Theory of Baumeister and colleagues, who presented declining performance during sustained cognitive exertion as evidence of the depletion of some resource, possibly blood glucose (Gailliot & Baumeister, 2007; Gailliot et al., 2007). Subsequent mixed results from replications and meta-analyses have, however, cast significant doubt over the Ego Depletion Theory specifically (Carter & McCullough, 2014; Lurquin et al., 2016) and on theories of resource depletion more broadly. Researchers have noted that while active processing no doubt requires energy, the difference in the rate of consumption of glucose compared with at rest is small (estimated at around 1% by Raichle & Mintun, 2006). That is, the so-called 'resting' dynamics of the brain are also rather metabolically costly (Kurzban, 2010; Kurzban et al., 2013). Preserving glucose supplies seems not to provide a rationale for cognitive effort.

Nevertheless, resource theories continue to be considered seriously, not least because the idea that cognitive work has metabolic consequences is the basis for the BOLD signal and, in turn, the whole field of neuroimaging. Instead what has been revised is the claim that effort reflects a global resource depletion. More recent theories have instead suggested that local metabolic changes may be involved, such as depletion of astrocytic glycogen stores (Christie & Schrater, 2015) or the accumulation of amyloid beta protein (Holroyd, 2016); but proposals such as these remain to be seriously tested. Ultimately, metabolic depletion theories offer an attractive explanation for cognitive effort, but with a physical resource yet to be identified, they remain speculative at this stage.

In contrast, limited processing capacity has become the consensus account in recent years, helped no doubt by the success of the neuroeconomic paradigm (which implicitly starts from the assumption that resources are *not* depleted over time). Crucially, researchers are now seeking to be more specific about the nature and source of effort costs – what is it about cognitive processing that is costly? I will briefly summarise the two main perspectives that have been offered so far, both of which identify cognitive effort with opportunity costs.

1.1.3.1 Opportunity costs

Utilising any limited resource entails an opportunity cost, in so far as that resource now cannot be used for any other purpose and so other sources of rewards have been foregone. Granting that cognitive processes are, ultimately, limited capacity, then the brain ought to take account of those opportunity costs when choosing how to allocate its processing resources (a principle known as Bounded Optimality). This model of effort costs is most often attributed to Kurzban et al. (2013) and Shenhav et al. (2013, 2017), with some distinctions between the two.

To borrow an example from Kurzban et al. (2013), one might imagine a participant performing a maths problem; with nothing else to do, other than daydream, they might not perceive the maths problem to be too effortful. But place their mobile phone on the table next to them and the opportunity cost model predicts that the maths problem will be perceived as more aversive, because the value of alternative options (e.g. the social reward available from responding to messages on their phone) has increased. Accordingly this will also be reflected in behaviour, as an increased tendency to switch away from the maths problem to the higher utility action of browsing one's phone.

The model presented by Kurzban et al. focusses on limited capacity for task-specific computations (this distinguishes their proposal from that of Shenhav and colleagues). Effort is therefore experienced only to the extent that two different tasks draw on overlapping processing resources, thus allowing for unimpaired dual-task performance under certain circumstances. Kurzban et al. also permit that the same set of processing resources may be shared between two different tasks, rather than there being a binary choice between processing one task or another. Put simply, resources may be split between different tasks provided that the marginal utility gained by devoting one unit of processing capacity to an alternative task exceeds that lost by reallocating that unit of capacity away from the original task. A final consideration is that Kurzban et al. are relatively agnostic about the specific economic calculations involved – they suggest that effort costs reflect the opportunity cost associated with the next best option only, but it is also possible that the cost instead reflects, say, the mean value of all of the available alternatives. These are not core assumptions of their model and could be tested and clarified by future research.

Shenhav et al. (2013, 2017) independently proposed another opportunity cost model of cognitive effort, but in this case aligning effort specifically with cognitive control. They suggest that the purpose of control is to prevent or ameliorate cross talk between concurrent signals within a particular processing system, which is achieved by intervening to favour one signal over the others. Controlled processes thus appear to be limited in capacity, though Shenhav et al. (2017) are keen to emphasise that this is precisely the purpose of cognitive control and not the result of some structural or physical constraint on resources. Having in any case established that controlled processing is limited, then according to the principle of Bounded Optimality, its allocation ought to be weighted by the relevant opportunity costs, reflected in the subjective experience of effort costs.

The model of cognitive effort proposed by Shenhav et al. forms part of a broader theory they term the Expected Value of Control (EVC) theory, and it is with this that they depart more significantly from the similar proposal of Kurzban et al. (2013).

Specifically, they propose that the costs associated with control incorporate not just the opportunity cost but also the intensity of control signal required – on the basis that computing and implementing a control signal apparently entails greater representational complexity compared with uncontrolled or automated processing. They do not go into further detail however as to why the magnitude of the control signal should be intrinsically costly. The likely answer is that their cognitive control model is based upon earlier models of optimal motor control (see Shadmehr & Krakauer, 2008, for a review), which also stated that the control signal was intrinsically costly; this was not fully justified in the original motor control models, however, and has now unfortunately been accepted uncritically in the cognitive control literature. These are fundamental issues that I will explore in more depth in this thesis as part of the theory-focussed Chapter 6.

1.1.4 Cognitive effort in disease

Cognitive effort is an important feature of everyday cognition so, as a corollary, we might think that it would be implicated in some of the cognitive symptoms of mental and neurological illnesses. Perhaps because a thorough study of cognitive effort has only begun relatively recently, however, this remains poorly understood and to some extent uncharted territory. I will briefly highlight below two potentially fruitful areas where cognitive effort may provide a useful perspective, and which I will focus on in this thesis.

1.1.4.1 Anhedonia and apathy

Anhedonia is defined as “a consistently and markedly diminished interest or pleasure in almost all daily activities” (Husain & Roiser, 2017, p2). It is one of the two core symptoms of Major Depressive Disorder (MDD) cited in the *Diagnostic and Statistical Manual of Mental Disorders* (APA, 2013), along with depressed mood, but can also figure in schizophrenia (as a negative symptom), eating disorders and substance use disorder (Husain & Roiser, 2017). Apathy is a similar construct, referring to a loss of motivation for at least two of: goal-directed behaviour, cognitive activity and emotion (Robert et al., 2009); in contrast to anhedonia, which has tended to be a psychiatric symptom, apathy has been associated with neurological conditions such as Parkinson's and Alzheimer's diseases and stroke. An emerging perspective, however, articulated by Husain and Roiser (2017), sees these as two overlapping constructs, potentially with some common and some distinct underlying mechanisms.

There is accumulating evidence that the trade-off between the costs and rewards of physical exertion is skewed in both apathy (Chong et al., 2015; Le Heron et al., 2018) and anhedonia (Treadway et al., 2013; Valton et al., 2018). Significantly, Valton et al. (2018) have taken this further and, using parameters derived from a computational model, specifically associated higher anhedonia scores with increased sensitivity to physical effort (as opposed to lower sensitivity to rewards, which was associated instead with lower mood/anxiety). So far, however, research into cognitive effort in disease is less advanced, with little research attempting to quantitatively relate cognitive effort to symptom severity. One exception is a study by Patzelt et al. (2019), who investigated the associations between effort avoidance (on the Demand Selection Task of Kool et al., 2010) and a range of 19 symptom scales. However, they found no correlation between effort preferences and either depression specifically or transdiagnostic anhedonia or apathy scores. One possibility is that this study was not sufficiently sensitive to the putative effort–anhedonia association (for example due to the categorical nature, noted previously, of the Demand Selection Task). This interpretation is supported by results of earlier studies which showed that patients with depression were impaired versus healthy controls on tasks demanding effortful attention but not automatic, stimulus-driven attention (e.g. visual search with one versus multiple distractors (Hammar, 2003; Hammar et al., 2003). This suggests that exploring the role of altered cognitive effort in depression, in particular by using newer, more sensitive tasks, could be a promising avenue for future research.

1.1.4.2 Anxiety disorders

Anxiety constitutes a normal and adaptive set of responses to prolonged, unpredictable threat, promoting cautious avoidance behaviour and heightened vigilance. When a state of anxiety becomes permanent, generalised or otherwise decoupled from genuine threat, however, it can become pathological (Robinson et al., 2013). One of the core features of anxiety is avoidance behaviour which, in excess, is thought to contribute to the inception and maintenance of pathological anxiety because, by avoiding a feared situation, one is unable to learn when the true outcome is not as bad as first thought (Krypotos et al., 2015). Conversely, addressing avoidance behaviour and so facilitating extinction learning constitutes one of the main psychological approaches for treating anxiety (Kaczkurkin & Foa, 2015).

Recent theoretical and empirical work has suggested that a major factor in avoidance behaviour is the negative Pavlovian bias – that is, a tendency to withhold or inhibit action when an aversive event is predicted (Boureau & Dayan, 2011; Guitart-Masip, Duzel et al., 2014). This Pavlovian bias can account for important findings, such as avoidance behaviour in anxiety even when there is no instrumental component (i.e. when actions do not affect the presence/absence of the stimulus; Krypotos et al., 2014). Additionally, computational modelling studies have found that when a contextual stressor ('threat of shock') is present, Pavlovian bias in patients with anxiety disorders is increased (Mkrtchian, Aylward et al., 2017; Mkrtchian, Roiser et al., 2017).

The above discussion raises the prospect that avoidance behaviour in anxiety could be framed in terms of cognitive effort: while threat-related biases in anxiety may not be a direct result of altered effort processing in itself, patients' ability to control these biases will depend on the extent to which they are able to exert the required effort. This hypothesis motivates the first study of this thesis, described in Section 1.3.1 below.

1.1.5 Cognitive Effort: Interim summary

To summarise what has been reviewed so far, there is widespread agreement that cognitive resources are limited and therefore that they ought to be treated as costly in order to ensure their efficient use and maximise the rewards available to the organism. Whether the constraint on resources reflects metabolic alterations, processing capacity or both remains a point of contention; but theories assuming a limited capacity and proposing opportunity cost as a key contributor to subjective effort have enjoyed success in recent years (Kurzban et al., 2013; Shenhav et al., 2013, 2017; Westbrook, Cools, & Braver, 2019).

Cognitive effort therefore constitutes a research field with a maturing theoretical basis, which raises a number of new and underexplored empirical questions. Concerning the nature of effort costs, it is still unclear whether these just reflect economic (opportunity) costs or may incorporate intrinsic costs of cognition as well. Similarly, what precisely is the relationship between effort and cognitive control – are they two sides of the same coin (as Shenhav et al., 2013, 2017 claim) or is effort a wider phenomenon involving any use of limited capacity resources (Kurzban et al., 2013)? Concerning the role of cognitive effort in disease, much more research needs to be done not simply to identify whether effort processing is affected in certain disease states, but to link this parametrically with symptom classifications and the phenomenology of the disease. Finally, there is a more practical question of whether one's subjective assessment of effort costs can be shifted and, if so, how? This would potentially offer a translatable route for treating effort-related symptoms of psychiatric and neurological diseases, and also a means of enhancing human cognitive performance in healthy individuals.

1.2 Pavlovian bias

As noted in Section 1.1.4 above, this thesis will in large part focus on the role that cognitive effort plays in the ability to exert control over Pavlovian biases. In the following section I will give a more detailed overview of what Pavlovian biases are, why they are thought to arise and how they are controlled. Finally I will discuss the reasons for thinking that effort and the strength of Pavlovian biases are linked.

1.2.1 Pavlovian and instrumental systems for action selection

Theories of learning have historically tended to make a distinction between two processes, Pavlovian and instrumental learning. The former is said to be concerned with stimulus-stimulus learning – for example, in the classic case, Pavlov trained dogs to learn that the ringing of a bell preceded the delivery of food. The latter process refers to learning action-outcome contingencies (Dickinson & Balleine, 2002) – for example, that pressing a button causes a light to switch on. There is however a wealth of evidence indicating that the two systems are not as distinct as often portrayed and in fact overlap and interact with one another significantly (Guitart-Masip, Duzel et al., 2014). In particular, it should be emphasised that the Pavlovian system is involved in more than just predictive learning alone, contributing also to action selection through the generation of conditioned (Pavlovian) responses – indeed these responses are the only way that Pavlovian learning is outwardly manifested (Dayan & Balleine, 2002; Dayan et al., 2006). Pavlovian responses are relatively fixed and are thought to be innate; broadly speaking, they invigorate action and promote approach towards reward-associated (appetitive) cues, and inhibit action in order to avoid contact with punishment-associated (aversive) cues (Dayan & Balleine, 2002). It is widely accepted that these responses are adaptive, at least on an evolutionary scale, yet as we shall see there are specific situations in which they fail.

The potential for conflict between these two action selection systems, Pavlovian and instrumental, has been recognised for many decades. An early report of what has since come to be called Pavlovian-Instrumental Transfer (PIT) was provided by Estes and Skinner (1941): they found that the rate of instrumental lever-pressing by rats was reduced when a tone, which had previously been associated with punishment, was played at the same time; conversely, lever pressing increased if the tone had been associated with food reward (Estes, 1943). A starker example is given by an experiment conducted by Hershberger (1986), who trained chickens first to learn that food was present in a particular cup; then, once the association had been acquired, placed the chickens and the cup on two treadmills set up such that, as the chicken approached the cup, the cup would move away from them at twice their speed; conversely if they moved away from the cup, it would be transported towards them at twice their speed. Hershberger found that the chickens were unable to learn that they needed to set off away from the direction of the cup in order to win the food reward – they were unable to overcome their prepotent Pavlovian approach response.

The reason for results such as these can be understood by considering action selection as a problem of optimal control. An optimal action is one that leads to the maximum possible reward and minimum possible punishment over the long term. There is really only one way to achieve this: one has to know and take account of the contingency between each action and its consequences, in order to select the action that will lead to the best outcome. In other words, one needs to know the instrumental value of an action; without this, one is more or less in the dark about the outcome to be expected following any particular action.

This is precisely the case with the Pavlovian system, which generates fixed responses to predictions of reward or punishment. One way of framing this is to say that the Pavlovian system implicitly encodes the belief that there is a positive contingency between approaching appetitive stimuli and reward, or avoiding aversive stimuli and preventing punishment (Guitart-Masip, Duzel et al., 2014). Because of this, the Pavlovian system is not guaranteed to be optimal even if Pavlovian learning is complete. This in turn can lead to conflict between the Pavlovian and the instrumental systems.

It is however only relatively recently that tasks have been developed to separate the effects of required action and valence on behaviour. One of these, featured prominently throughout this thesis, is called the Orthogonal Go/No-Go task (Guitart-Masip et al., 2011). It is so-called because it allows the required response to a stimulus (go or no-go) and the outcome valence (reward or punishment) to be varied independently. This gives rise to four distinct trial types (see Table 1.2): go to win reward, go to avoid punishment, no-go to win reward, no-go to avoid punishment. In two of these (go to win reward and no-go to avoid punishment) the Pavlovian and instrumental systems favour the same responses; in the other two (go to avoid punishment and no-go to win reward) they are in conflict. This is manifested in lower accuracy for the trial types where there is conflict. A related observation, not as frequently remarked upon, is that we nevertheless do not see perfect *in*accuracy for the go to avoid punishment/no-go to win reward trial types, which is what we would expect if Pavlovian biases were the only influence on behaviour. The fact that participants can learn these trials to some degree suggests that the brain is running both the Pavlovian and instrumental systems in parallel, and both are simultaneously able to affect behaviour.

Table 1.2. The four trial types of the Orthogonal Go/No-Go Task. Squares shaded dark are those for which the Pavlovian and instrumental systems produce conflicting responses.

	Reward	**Punishment**
Go	Go to Win Reward	Go to Avoid Punishment
No-Go	No-Go to Win Reward	No-Go to Avoid Punishment

This naturally raises the question of why the brain would go to the trouble of using both systems at once – why is there this semi-redundancy? It is often remarked that the Pavlovian system is computationally cheaper than the instrumental system (e.g. Swart et al., 2017, p.2), but this is not really advantageous if the instrumental systems are still kept running; and if the instrumental systems are available then why permit the possibility of systematic errors due to the Pavlovian system? I will return to these questions in Section 1.2.4, after first reviewing some of the proposed neural and other cognitive correlates of the Pavlovian and instrumental systems.

1.2.2 Neural mechanisms of Pavlovian and instrumental systems

Although in this thesis I will not be directly investigating the neural correlates of Pavlovian biases, it is worthwhile to briefly review this literature for two reasons: firstly, the primary evidence for the involvement of cognitive control in Pavlovian biases comes from studies of brain activity (fMRI and EEG); secondly, the lack of clarity in this literature perhaps indicates that we need to develop a better cognitive understanding of Pavlovian biases on which brain studies can then be based.

Using the Orthogonal Go/No-Go Task, and other tasks like it, a growing number of studies have contributed to a more complex assessment of the role of particular brain regions in Pavlovian and instrumental action selection. For example, areas in the ventral striatum and amygdala had previously been associated with the Pavlovian system, and in the dorsal striatum with the instrumental system (Liljeholm & Doherty, 2012). In rodents, ablation of the nucleus accumbens (NAcc) core has been found to abolish appetitive PIT (Cardinal et al., 2002), while in human fMRI studies, NAcc and amygdala BOLD activity was found to be positively correlated with the strength of the appetitive PIT effect (Talmi et al., 2008; Prévost et al., 2012). However, because these studies relied exclusively on appetitive PIT, they were not able to disambiguate the action and valence components fully, and therefore it is also possible that these results could be explained by the NAcc signalling action (go) value. Accordingly, subsequent studies with the Orthogonal Go/No-Go Task have reported that ventral striatum BOLD signal during the anticipatory phase of a trial (i.e. prior to the response being performed) tracked the action value and not the state value (Guitart-Masip et al., 2011, Guitart-Masip et al., 2012).

As a result of these findings, Guitart-Masip, Duzel et al. (2014) have proposed an alternative view of the Pavlovian and instrumental systems, according to which they are not completely segregated in the brain but instead may both be related to the direct ('go') and indirect ('no-go') pathways within the striatum. In order then to account for performance of the Pavlovian-instrumental conflict trial types (they single out no-go to win in particular), they propose the involvement of a top-down frontal controller, dependent on dopamine, which downweights the strength of Pavlovian representations in the striatum relative to the instrumental representations (through some as yet unidentified mechanism; Guitart-Masip, Duzel et al., 2014; Guitart-Masip, Economides et al., 2014).

There is indeed relatively good evidence for situating control over Pavlovian biases in the frontal cortices. A consistent result in fMRI studies has been that activity in the inferior frontal gyrus (IFG) is inversely correlated with the strength of Pavlovian bias (Ahn et al., 2013; Guitart-Masip et al., 2012; Gershman et al., 2021). One especially important study found that frontal theta-band EEG activity was associated with trial-by-trial changes in the strength of Pavlovian biases (Cavanagh et al., 2013); complementing the fMRI studies, this gives a much more fine-grained assessment that directly links frontal activity with dynamic changes in the strength of Pavlovian biases. Interestingly, another study reproduced this result when monetary rewards were used, but found that if social reward and punishment was used instead, it was frontal alpha band activity that was associated with (reduction in) Pavlovian bias instead (Thompson & Westwater, 2017).

In summary, these results paint a picture in which the Pavlovian and instrumental systems seem to be located in and around the basal ganglia, possibly overlapping with the direct and indirect pathways. As with much of our understanding of this part of the brain, however, the details are still not entirely clear, which maybe suggests that our theoretical understanding of these process does not perfectly match the computations that are actually being carried out. That said, the results that *are* consistent and straightforward to interpret are those relating to cognitive control over Pavlovian biases: this control is frontal in origin, modulated by dopamine and able to influence action selection on a trial-by-trial timescale. In the empirical chapters of this thesis I will explore the issue of control over Pavlovian biases in greater detail.

1.2.3 Pavlovian biases and mental health

Dayan and Huys (2008) have suggested that a core characteristic of affective disorders (both anxiety and depression) might be enhanced Pavlovian avoidance biases. Subsequently this prediction has been mostly borne out, certainly for anxiety. Mkrtchian et al. examined in two studies performance on the Orthogonal Go/No-Go Task while participants underwent an anxiety induction involving the threat of an electric shock. In the first study, participants were all healthy and did not have any preexisting mental health symptoms (Mkrtchian, Roiser et al., 2017), while in the second study, a sample of participants with anxiety and depression symptoms was compared against a healthy control group (Mkrtchian, Aylward et al., 2017). In both studies, participants showed enhanced Pavlovian avoidance biases when under threat of shock, compared with a 'safe' comparison condition; in addition to this, participants with anxiety or depression symptoms showed greater overall Pavlovian avoidance biases and a greater increase in this bias when under threat of shock, compared to the healthy controls. Finally, in a third study, Peterburs et al. (2022) also found that social anxiety symptoms were associated with enhanced Pavlovian avoidance bias. Together, these studies provide clear evidence that even state anxiety is associated with greater aversive Pavlovian biases, and that this connection is potentiated in the case of pathological, trait anxiety.

The situation is less clear with regards to depression; indeed, Dayan and Huys (2008) are themselves slightly more equivocal in linking Pavlovian avoidance biases to depression as well as anxiety. Two studies have looked at this question explicitly and reported opposite results. Nord et al. (2018) found that patients with major depressive disorder had enhanced aversive bias on a Pavlovian-instrumental transfer task, whereas Moutoussis et al. (2018) reported results from the Orthogonal Go/No-Go Task in which patients had no difference in Pavlovian avoidance bias compared with controls. Part of the reason for the discrepancy could of course be due to the difference in task used: Nord et al.'s aversive PIT task separates out the Pavlovian and instrumental learning phases from the actual transfer phase, while the Orthogonal Go/No-Go task tests acquisition and transfer at the same time. Thus while the two tasks are conceptually equivalent, it is nevertheless to be expected that they do not give identical results. Perhaps more important though is participants' medication status – in Nord et al.'s study patients were unmedicated, whereas the majority of Moutoussis et al.'s sample were prescribed antidepressants. Previous work has established that serotonergic drugs affect performance on the Go/No-Go task (Guitart-Masip, Economides et al., 2014), so it is very possible that medication status has substantially contributed to the differences between the two studies observed here.

Overall, therefore, there is a substantial literature supporting the idea that Pavlovian bias is related to symptoms of some mental health conditions, in particular anxiety and depression. In the wider context of this thesis, it will be important to address these specific associations so that our understanding of Pavlovian bias can benefit basic and clinical mental health research.

1.2.4 Pavlovian-instrumental interactions: The purpose of the two systems

In section 1.2.1 above I posed the question of what purpose the Pavlovian system really serves in action selection – why use the Pavlovian system at all if the instrumental system is guaranteed to be more accurate? An easy answer to this question would be simply to say that it is a vestigial cognitive structure; despite now having been superseded by more flexible instrumental systems, it has not yet been selected away because it continues to produce appropriate responses most of the time. However, this view misses the potential advantages of having both systems available.

The key characteristic of instrumental systems—their flexibility and ability to learn essentially any response to a stimulus—is also the source of significant costs (see Daw & Dayan, 2014 for a full discussion of this issue). For example, the two main algorithms for learning instrumental associations are called model-based and model-free (or cached). As the name suggests, the former involves learning the full network of causal relationships between stimuli, actions, and other stimuli (the model). In order to then select an action, one needs to generate simulations from this model, following the different possible causal paths in order to predict what the likely outcomes of an action will be. There is therefore a huge, potentially infinite, amount of information to be represented and processed, depending on how many layers of events one wishes to simulate. The model-free system, on the other hand, just entails saving the values of rewards and punishments experienced and apportioning them to actions taken previously – the value of an action in a particular context is therefore represented explicitly and does not require simulation, so the computational costs are not as great as those of the model-based system. Nevertheless they are still greater than those of the Pavlovian system, because the model-free system involves learning the value of combinations of actions and stimuli, rather than stimuli alone, so there is an extra dimension to the space of values to be learned.

Boureau and Dayan (2010) suggest that, beyond the computational costs, the primary issue with instrumental systems is the amount of training samples required. As noted above, the actions generated by the instrumental systems are guaranteed to be optimal only when learning is complete; prior to this point, because the instrumental systems do not suppose any relationship between actions and outcomes *a priori*, a decision-maker will inevitably have to make mistakes in order to learn. The effects of this are two-fold: there is an opportunity cost of having to go through the learning process before one can generate the correct responses; and there is also a more direct cost of making errors because, in an instrumental context, these have the potential to result in harm to the decision-maker. The Pavlovian system, on the other hand, is quicker to reach the point of complete learning, because one only needs to learn the values of stimuli, and not actions. It also does not entail potential harm to the decision-maker because Pavlovian learning is purely observational and so can be carried out at a distance. There is therefore an advantage to possessing both Pavlovian and instrumental systems because, although the computational costs overlap, the Pavlovian system fills a particular niche in generating approximately correct responses early on in the learning process, before the instrumental systems are fully trained. This does however introduce the potential for conflict, which needs to be overcome through cognitive control.

1.2.5 Cognitive control, effort and Pavlovian biases

The regulation of the Pavlovian and instrumental systems by cognitive control potentially allows for further optimisation of decision-making, by allowing the brain to make best use of these systems when they are advantageous and to scale them down when not. For example, the Pavlovian biases might be relied upon initially, during the critical early phase when the instrumental systems are not fully trained, in order to give the decision-maker a 'head start' and avoid too many potentially costly mistakes; then, later on, these biases could be selectively downweighted if and when they are predicted to lead to errors (perhaps based on detecting conflict between the Pavlovian and instrumental responses).

The evidence that cognitive control over Pavlovian biases does indeed take place is, however, not as strong as it could be. As reviewed already, a number of studies show a clear association between activity in frontal regions of the brain and reduced Pavlovian bias, but of course this requires making a backwards inference from brain region to function. Direct, cognitive evidence of control over Pavlovian biases is otherwise relatively lacking so far. Perhaps the one exception is that several studies have shown that the strength of Pavlovian biases is negatively associated with the controllability of the outcome (Gershman et al., 2021; Dorfman & Gershman, 2019; Csifcsák et al., 2020). This is interesting because it directly links control over Pavlovian biases to the Expected Value of Control theory (Shenhav et al., 2013). According to EVC, controllability is a key quantity because control is only valuable to the extent that it actually effects changes in the outcome; otherwise it is just unnecessary exertion. Thus, showing that the strength of Pavlovian biases is sensitive to controllability is an important, but insufficient, indication of the involvement of control.

In summary, by considering Pavlovian biases in the framework of effort-based decision-making, we are able to formulate a hypothesis about why Pavlovian biases are able to influence behaviour and why we do not just rely on the instrumental systems at all times: Pavlovian biases are controlled only to the extent that the effort required would be worthwhile. This in turn depends on quantities such as the value of any incentive for accurate responding, the efficacy of control and the cost of control. To prove this hypothesis, more research needs to be done, both to directly demonstrate that Pavlovian biases are subject to cognitive control, and to link this to effort processing in turn.

1.3 Thesis aims, hypotheses and predictions

My overall aim in this thesis is to investigate the factors involved in deciding when and how much effort to exert on a cognitive task. I specifically focus on the role of effort in control over Pavlovian biases because this is a phenomenon in which the purpose of control is relatively easy to define and conceptualise – control is required to regulate the balance between the Pavlovian and instrumental systems and resolve any conflicts that arise. In addition, Pavlovian bias has previously been implicated in symptoms of both anxiety and depression, which gives rise to a secondary aim of this thesis, to explore the role of effort in symptoms of common mental health conditions, specifically anxiety and depression. These are addressed in four empirical chapters.

My final aim in this thesis is to contribute to a stronger and more principled theoretical understanding of effort costs. Unfortunately this is currently lacking, without which much of the good experimental research carried out in recent years is built on foundations of unknown quality. By bringing in two new ideas from areas outside of neuroscience, I hope to reenergise this discussion. This aim is addressed in a single, final theory chapter.

Below I provide an overview of each of the remaining chapters in turn.

1.3.1 Chapter 2

In Chapter Two, we investigated whether participants could learn to overcome the deleterious effects of Pavlovian biases on behaviour through a programme of behavioural training. This was a blinded, sham-controlled study in which we assessed the performance of a sample of healthy participants on the Orthogonal Go/No-Go Task before and after a week of practising specifically on the high Pavlovian conflict trials. A number of previous studies have found that the strength of Pavlovian biases can be modified, for example as a function of acute stress (Mkrtchian, Aylward et al., 2017; Mkrtchian, Roiser et al., 2017) or pharmacological interventions (Guitart-Masip, Economides et al., 2014). Because Pavlovian biases themselves are thought to be fixed, it is generally assumed that changes in their strength can be attributed to changes in cognitive control, which in turn is dependent on effort. We therefore wanted to see whether we could train participants to exert more control over their Pavlovian biases on the Go/No-Go task – if so, we hypothesised that this would be because of greater willingness to exert effort.

It should be noted that one study has looked at the issue of modifying Pavlovian biases through training previously (Ereira et al., 2021). They found that this was possible only when using a modified semantic version of the Go/No-Go task (in which the stimuli and required actions were contextualised within a wider narrative) and not with the original task. We therefore focussed specifically on the original task, but using a simpler set of stimuli, so as to test whether Pavlovian biases could be controlled at all.

In addition to this we looked at transfer to two other tasks, the Affective Bias Task and the Risk-Taking Task, both of which involve cognitive biases that have to be overcome in order to perform accurately. Assuming the training worked by enhancing participants' willingness to engage in effortful, controlled behaviour, we predicted that those who did the active training would show less biased performance on these tasks than those in the sham training group. Finally, we also explored the association between Pavlovian biases and both anxiety and depression symptoms – we again predicted that, to the extent that the training was effective, this would be reflected in a decrease in symptom scores.

1.3.2 Chapter 3

In Chapter Three we made some changes to, and then replicated, the Pavlovian bias training study reported in Chapter Two. This allowed us to address several limitations identified in the earlier study: most notably, we substantially increased our sample size so that the experiment was better powered to detect smaller effects; we were also able to address a technical problem with the Go/No-Go Task which had previously prevented some of the data from being recorded; finally, this study was conducted entirely online, which facilitated not just the larger sample size but also allowed us to recruit a more diverse range of participants.

Our aims and hypotheses for this study were the same as before. We were looking firstly to test whether the active training led to enhanced control over not just the Pavlovian biases in the Go/No-Go Task but also the other biases in the Affective Bias and Risk-Taking tasks. Secondly we investigated whether there was a corresponding reduction in reported depression or anxiety symptoms.

1.3.3 Chapter 4

In Chapter 3 we had observed a significant change in Pavlovian bias after the active training, and we proposed that this was attributable to enhanced cognitive control. To explore this further we wanted to look at the role that sensitivity to cognitive effort played in exerting control over Pavlovian bias, but before we could do this we needed to design a task suitable for measuring effort sensitivity and for making individual differences comparisons in particular. The development and assessment of this task is described in Chapter 4.

Earlier cognitive effort tasks are susceptible to confounding by probability discounting, meaning that the difficulty of the task (and therefore probability of obtaining reward) is not adequately controlled or standardised. This is particularly a problem if these tasks are to be used to compare patient groups to healthy controls. To resolve these issues we designed a new task, the Number Switching Task, in which we aimed to ensure that the rates of success were both held constant across the different effort levels and could be standardised across participants. In this study we validated that this was indeed the case, while at the same time crucially ensuring that the effort manipulation itself was successful in that participants treated it as costly and avoided the higher effort levels. Finally we also conducted an exploratory analysis to examine the associations between effort sensitivity and a number of self-report mental health symptom scales and cognitive traits. Broadly, we anticipated increased effort sensitivity would be associated with higher scores on the depression and anhedonia scales in particular. The other associations were examined on a more exploratory basis.

1.3.4 Chapter 5

Previously in this Introduction I suggested that the strength of Pavlovian biases can be influenced by cognitive control. However, while there have been some studies published previously that support this idea, the evidence so far is relatively indirect. In this study we therefore sought to investigate and understand specifically the *cognitive* mechanism underlying control over Pavlovian biases. We reasoned that, if people are able to exert control over their Pavlovian biases, this should be dependent on exerting effort. Specifically, we tested whether individual differences in the strength of Pavlovian biases were associated with differences in sensitivity to cognitive effort (as measured by the new cognitive effort task, Chapter 4).

Any significant Pavlovian bias–effort sensitivity correlation would also help to inform our interpretation of the earlier training studies (described in Chapters 2 and 3) since, by virtue of being a flexible resource, cognitive effort is probably the most likely source of any improvement as a result of the behavioural training. Showing that effort sensitivity is associated with Pavlovian bias would help to provide further support for this hypothesis.

Finally, we also tested the associations between Pavlovian bias and effort sensitivity, and symptoms of both anxiety and depression. Previous research (Mkrtchian, Aylward et al., 2017) has shown that patients diagnosed with either anxiety or depression show greater Pavlovian avoidance biases than healthy controls (although note in the former case this was also dependent on the presence of 'threat of shock', an anxiogenic context). We were therefore interested in the extent to which the same association held for continuous symptom scores in a healthy sample of participants, and we anticipated seeing a significant correlation. Cognitive effort sensitivity, on the other hand, has not been explicitly tested before in relation to mental health symptoms (aside from in our own initial study using the Number Switching Task, in Chapter 4). Robust associations between physical effort sensitivity and depression scores have been reported previously (both in patients and in healthy participants) and we therefore anticipated seeing a similar result here with regards to cognitive effort; but either way this would be a new result and potentially significant with regards to understanding the cognitive aspects of anxiety and depression.

1.3.5 Chapter 6

Chapter 6 is a theory-focussed chapter in which I attempt to address the core problem of cognitive effort, namely that we still do not know why effort is costly. In this Chapter I put forward two complementary ideas that I hope will advance our collective thinking on this topic, one based on a recent attempt in economics to improve optimal decision theory (Ergodicity Economics; Peters, 2019), which I suggest can also be applied to effort-based decision-making; the other develops an idea from computer science called Landauer's Principle (Landauer 1961), with which I will show that there are obligatory energetic costs of attenuating noise in the brain that seem to map neatly onto effort costs. Overall I hope that these two different, but complementary ideas, will contribute to an enhanced debate with regards to the theory and rationale of effort costs.

1.3.6 Chapter 7

In Chapter 7, I present a general discussion of the previous four empirical chapters and one theoretical chapter. After briefly revisiting the aims and main results of each chapter in turn, I then provide a synthesis of the whole, discussing the overall implications of the thesis for understanding the links between effort, cognitive control and Pavlovian biases. I acknowledge the limitations of the components of this thesis, but then go on to propose further studies which could address these issues, as well as advance our understanding of cognitive effort in other respects.

Chapter 2. Learning to Overcome Pavlovian Biases

2.1 Abstract

Pavlovian biases are fixed patterns of responses that involve approaching stimuli associated with reward and avoiding those associated with punishment. These responses can sometimes conflict with those produced by other action selection systems, potentially giving rise to suboptimal behaviour (which may be particularly relevant to some of the symptoms of affective disorders like anxiety and depression; Dayan & Huys, 2008). It has previously been suggested that, to resolve this conflict, the brain is capable of exerting control over the Pavlovian system, selectively downweighting it as and when conflict is anticipated or detected (Cavanagh et al., 2013). Importantly, however, no-one has actually established whether there is behavioural evidence for control – are Pavlovian biases even modifiable? In this blinded, sham-controlled study we addressed this question through a behavioural training intervention: participants in the active training group repeatedly practiced trials of the Orthogonal Go/No-Go Task (Guitart-Masip et al., 2011) that evoke high Pavlovian-instrumental conflict. We reasoned that if participants could improve their performance on these trials this would indicate that it is indeed possible to overcome the influence of the Pavlovian system, at least in principle. Unfortunately, however, we found that there was no significant training effect, and further interpretation of this null result was hindered by limitations of the experiment. We also did not observe any significant transfer effects to secondary tasks or anxiety or depression symptom scales. We discuss the implications of these results for understanding control over Pavlovian bias, while also emphasising that further, improved studies are required to answer our questions satisfactorily.

2.2 Introduction

A repeating motif in cognitive research is the observation that people frequently behave in ways that seem to be suboptimal. For instance, we often rely on heuristics rather than reasoning in full (Shah & Oppenheimber, 2008), we treat losses differently to equivalent gains (Mrkva et al., 2019) and we sometimes engage in behaviour that we know does not align with our goals (de Wit et al., 2012). A key, high-level concern in cognitive neuroscience is to explain why such behaviour takes place.

In the context of action selection, an optimal action is that which leads to the best possible outcome. Therefore, when selecting an action, an optimal *strategy* requires assessing the contingency between each response option and subsequent (rewarding or punishing) outcomes. After taking an action, a decision-maker could then update their belief about the effects of that action based on the observed result, allowing them to adapt their behaviour to novel or changing environments. Although there are different ways of implementing this strategy (discussed below), all are grouped under the general heading of 'Instrumental Learning', because they involve learning about actions and their outcomes (Dickinson & Balleine, 2002).

In reality, decision-making is also affected by systematic biases, such as result from Pavlovian responses to stimuli associated with reward or punishment (Dayan & Balleine, 2002; Dayan et al., 2006). These responses generally involve invigoration of action when rewards are predicted (called an approach bias) and inhibition of action in the face of punishment (avoidance bias). Given these responses are innate and ubiquitous across species (Cavanagh et al., 2013), it seems likely that they tend to be adaptive, at least on an evolutionary scale. On shorter timescales, however, there is the problem that the Pavlovian system can sometimes promote actions that conflict with those of the instrumental systems, resulting in poor outcomes. This can be seen, for example, in the case of the ambush predator that starts its chase too early, allowing its prey to escape; or the proverbial rabbit in the headlights, that sees a car advancing towards it at speed, but freezes instead of running.

To put this in more concrete terms, consider a Go/No-Go task in which some of the stimuli are incentivised by reward and others by punishment (as first described by Guitart-Masip, 2011); this leads to four distinct trial types, set out in Table 2.1. A consistent finding with this task is that participants are more likely to make an error when they have to go to avoid punishment or no-go to win reward, reflecting the fact that, on these trial types, the reward/punishment associations of each of the stimuli elicit Pavlovian approach/avoidance responses that conflict with the actual actions required.

Table 2.1. The four trial types of the Orthogonal Go/No-Go Task. Squares shaded dark are those for which the Pavlovian and instrumental systems produce conflicting responses.

	Reward	**Punishment**
Go	Go to Win Reward	Go to Avoid Punishment
No-Go	No-Go to Win Reward	No-Go to Avoid Punishment

A natural question arising at this point is why Pavlovian biases are present at all – why not rely entirely on the instrumental systems, given these are in principle able to learn any set of responses required by a task? The most compelling answer is that the advantage of the instrumental systems, their flexibility, also entails significant costs (Daw & Dayan, 2014; Boureau & Dayan, 2011). As mentioned above there are different implementations of instrumental learning: One involves building up a model of the stimulus-action-outcome contingencies in the environment (hence this is referred to as model-based learning); The other is simpler and just involves caching the rewards and punishments as they are experienced and apportioning them to actions taken previously (this is referred to as model-free learning). Both forms of instrumental learning are reliant on large numbers of training samples, which carries direct costs, if the consequences of making a mistake are dangerous, as well as indirect opportunity costs. In addition, model-based learning is particularly computationally expensive, because the value of each action is only represented implicitly in the model, so a decision-maker has to generate simulations to predict each fork on the path from an action to its possible consequences.

The Pavlovian system is therefore a valuable adjunct to the instrumental systems, especially in novel environments, because it is computationally cheap and requires fewer training samples, as it has one less dimension (action) to learn and represent; in addition, the training that is required is only observational (not instrumental) so the decision-maker does not need to put themselves in harm's way. The Pavlovian system can therefore support decision-making from an earlier point in time, when the instrumental systems are still yet to be fully trained (Boureau & Dayan, 2011).

2.2.1 Pavlovian bias and cognitive control

This answer is still not entirely satisfying, however, because there remains the problem that the Pavlovian system will sometimes persistently favour responses that are suboptimal, which would seem to counter the benefit of being able to make decisions from fewer training samples. One possible solution is to consider whether Pavlovian biases are subject to cognitive control; in other words, although Pavlovian responses themselves are 'hard coded' and immutable, is it possible through top-down signals to selectively reduce their *influence* on behaviour? If so, this would allow the brain to make the most of both the Pavlovian and instrumental systems, for instance by running the two systems in parallel until such time as conflict is anticipated or detected, at which point control signals could intervene to inhibit the Pavlovian response (Shenhav et al., 2013). See Figure 2.1 for a high-level schematic of this arrangement.

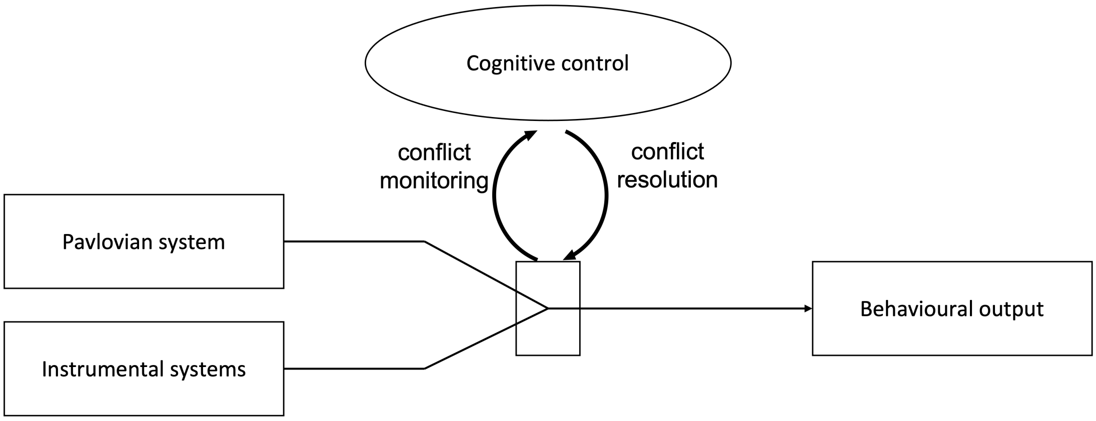


Figure 2.1. Schematic of the proposed relationship between the Pavlovian and instrumental systems, and cognitive control. A controller monitors the output streams of the Pavlovian and instrumental systems for conflict; when conflict is detected, this controller can then intervene to downweight the strength of the Pavlovian stream. The decision to engage control also depends on other factors such as the cost of the control signals required and the estimated efficacy of control.

This also reframes the issue of Pavlovian bias in terms of effort-based decision-making; in the wider literature on cognitive control, the decision to exert control is typically framed as an economic calculation that involves weighing up the relative benefits (and the likelihood of obtaining them) against the costs (Shenhav et al., 2013, 2017; Westbrook & Braver, 2015). In this sense, the presence of Pavlovian biases may reflect not the fixed limits of cognition, but an active choice to accept errors in order to maximise net rewards.

Supporting this hypothesis, it has indeed been shown that the balance between Pavlovian and instrumental influences on behaviour is not static but seems to be regulated dynamically. For example, the strength of Pavlovian biases is decreased following L-DOPA administration (Guitart-Masip, Economides et al., 2014) but increased by acute stress (Mkrtchian, Aylward et al., 2017; Mkrtchian, Roiser et al., 2017). One particularly suggestive study found that performance on the Orthogonal Go/No-Go Task was best explained by a computational model in which the Pavlovian component could be upweighted or downweighted trial by trial in proportion to EEG mid-frontal theta power (Cavanagh et al., 2013). This was interpreted as indicating the presence of a cognitive control mechanism of a kind like that suggested above. Of course, this inference relies upon identifying mid-frontal theta signals with cognitive control, an assumption that was not directly tested within this study.

2.2.2 Can control over Pavlovian biases be increased through training?

In the present study we decided to take these ideas further and asked whether participants can be taught to increase their control over Pavlovian biases through training. As outlined above, in this scheme a control system would first have to recognise situations of Pavlovian-instrumental conflict, then make an economic choice about the value of exerting control or not. We reasoned that participants might be able to improve at both stages of this process, i.e. by getting better at detecting conflict and also learning that they are able to control their biases (enhancing their belief about the efficacy of exerting control; *cf.* the Expected Value of Control (EVC) model, Shenhav et al., 2013, 2017). We suggest that if indeed we can show successful training of the ability to overcome Pavlovian biases, this would be further evidence of a cognitive control mechanism regulating the Pavlovian system.

Moreover, the training could also have potential as a clinical treatment, with the aim of enhancing cognitive control in conditions where it otherwise seems to be lacking. In depression and anxiety for example, we know that patients are affected by cognitive symptoms and in particular may have difficulties with exerting cognitive control (Robinson et al., 2013; Grahek et al., 2019). Separately, several studies have shown that patients with these conditions also tend to exhibit greater Pavlovian biases (Mkrtchian, Aylward et al., 2017; Nord et al., 2018). It seems likely that these two findings are linked – that to some extent avoidance behaviour in depression and anxiety might result from a reduced tendency to exert control – and this in turn might be able to be ameliorated through training. Here, we examined this directly by looking at the association between Pavlovian bias (before and after the training) and depression and anxiety scores.

2.2.3 Hypotheses and expectations for this study

We had two related aims for this study. First, we set out to test whether control over Pavlovian biases can be enhanced through behavioural training. Specifically, this training consisted of selectively practicing the conflicting trials of the Orthogonal Go/No-Go Task (go to avoid punishment, no-go to win reward). We hypothesised that, when tested on the full Orthogonal Go/No-Go Task before and after the training, participants who received this training would show a greater reduction in Pavlovian bias than participants who did a sham training intervention instead (consisting of practice on the non-conflicting go to win and no-go to avoid trial types). Important to note is that we tested participants with the same stimuli on which they had trained, because at this stage we were primarily looking to prove the principle that control is amenable to training. Encouragingly, a previous study involving training of a negative facial interpretation bias has shown successful results with a similar protocol to ours (Peters et al., 2017), giving promising grounds to think we might be able to effect changes in Pavlovian biases as well.

In addition to looking at Pavlovian biases on the Go/No-Go task itself, we also included a Risk Taking task (Rutledge et al., 2015) in which these biases have also been shown to influence the decision to gamble; we used this task to assess the transfer of any training effects to other contexts.

Our second aim was to examine the implications of any change in control for symptoms of depression and anxiety. We included both self-report measures of symptoms (the Beck Depression Inventory and the State-Trait Anxiety Inventory; Beck et al., 1996; Spielberger et al., 1983) and a more targeted cognitive task measuring affective bias (Aylward et al., 2020), which has previously been shown to be associated with depression in both case-control and continuous designs. If training is successful in reducing Pavlovian biases, and the mechanism involves enhancing cognitive control, then we expect this would result in reduced affective biases too, potentially also accompanied by reduced symptom scores.

2.3 Methods

2.3.1 Preregistration

This study was preregistered on the Open Science Framework (https://osf.io/ax5b4/?view_only=6bac5fafce044b21b1374219484c3ba9). The method described below did not deviate from the preregistration.

2.3.2 Participants

In a pilot study we found that the size of the training effect was approximately *d* = 1.2. To be conservative, we halved this to *d* = 0.6 and, using α = 5% and power of 90% (one-tailed), we calculated in G*Power (Faul et al., 2007) a minimum sample size of N=52, which we then rounded up to 60 (30 in each group) to give our target sample size.

In total 71 participants were recruited, using a notice posted on the website ‘Call for Participants’. All participants reported no history of neurological or psychiatric illness, and had not taken part in a study using the Orthogonal Go/No-Go Task with our lab before. Of the 71, three did not complete all of the online training sessions as required, and eight failed to attend the Follow-up session in the laboratory, leaving us with the required 60 participants (30 in each group).

Participants were paid £7.50 for each lab-based session they attended, plus a further £5 bonus if they completed all five of the online training sessions. This study was approved by the UCL Research Ethics Committee (6198/001) and all participants gave written, informed consent prior to taking part.

2.3.3 Procedure

The study comprised three phases, taking place over eight days. This is shown in Figure 2.2.

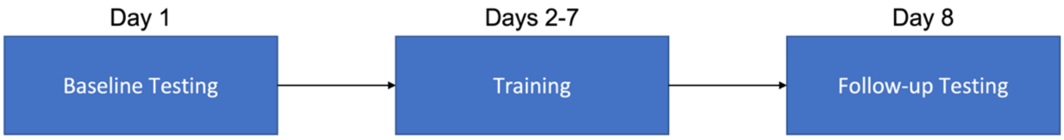


Figure 2.2. The timeline of the study.

First, participants attended the laboratory to complete a baseline set of cognitive tasks – the Orthogonal Go/No-Go Task, the Affective Tone Task, the Risk Taking Task, the Beck Depression Inventory and the State-Trait Anxiety Inventory. These are described in detail in Section 2.3.4 Measures and Tasks below. At the end of this session they were provided with a personalised link to the website Gorilla (www.gorilla.sc), where the training then took place.

At the start of this second phase, Gorilla randomly allocated participants to the active or sham training groups. The participants however were not told which group they were in nor what the training was for. Participants were instructed to complete at least five online training sessions over the next six days.

Finally, on the eighth day of the study, participants returned to the laboratory where they repeated the same battery of tasks that they had done on the first day of testing.

2.3.4 Measures and Tasks

This section describes the three cognitive tasks and two mental health symptom scales that comprised the battery of tests used in both the Baseline and Follow-up Testing sessions. In between the two laboratory-based sessions, participants completed daily online behavioural training, which is also described at the end of this section.

2.3.4.1 Orthogonal Go/No-Go Task (Guitart-Masip et al., 2012)

The procedure for this task is set out in Figure 2.3. A trial consisted of three events, each displayed for 1000ms with a 250ms inter-stimulus interval: first an initial fractal cue was shown in the centre of the screen; then a circle target was displayed on either the left- or right-hand side, to which participants chose whether to make a key-press response; finally the outcome of their decision was given.

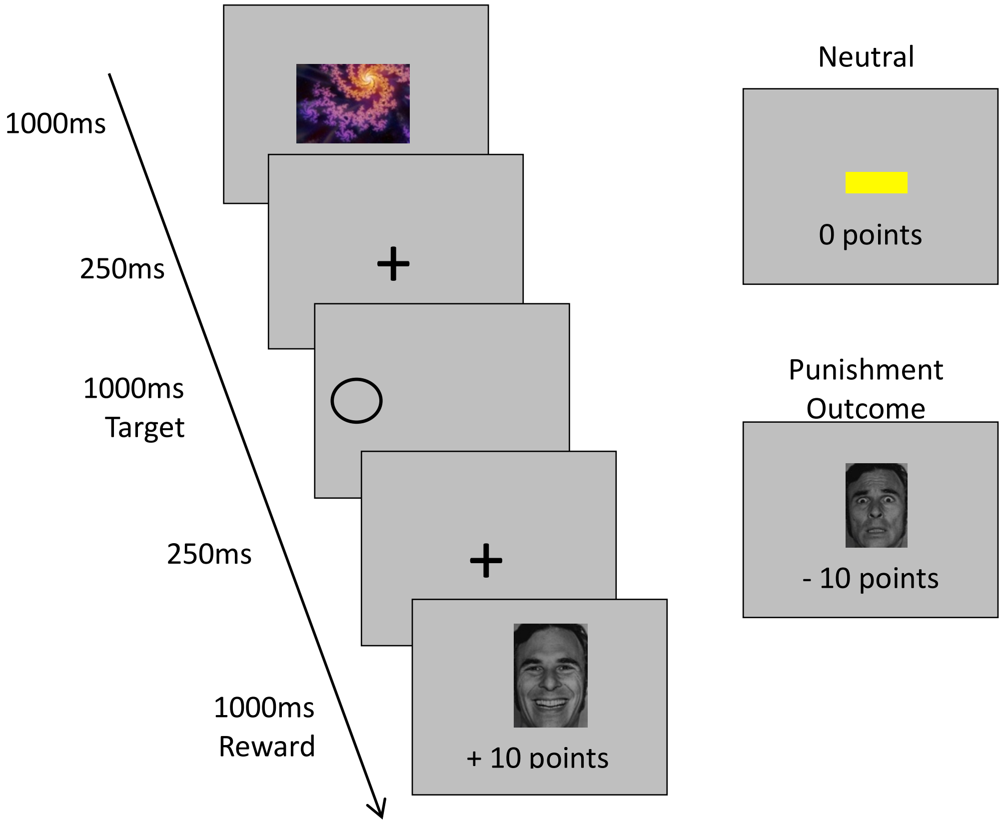
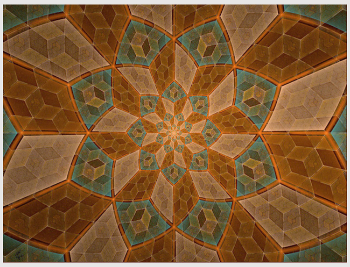
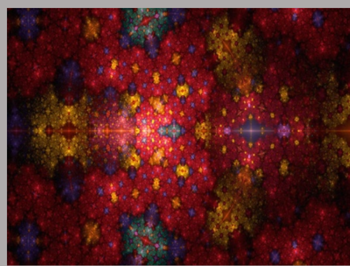
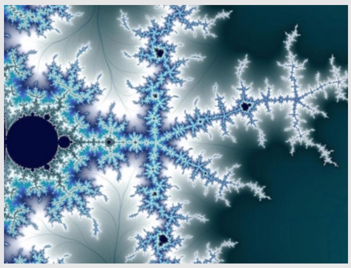


Figure 2.3. Procedure for the Orthogonal Go/No-Go Task. Participants were shown an initial fractal cue, which indicated whether they should go or no-go when the circle target was subsequently shown, and whether they could expect to be rewarded or punished (dependent on their performance); The target was then shown for participants to respond to; Finally the outcome for that trial was displayed. In this case reward is shown in the main sequence and examples of the neutral and punishment stimuli are given on the right.

The fractal indicated whether participants would have to make or withhold a key-press response (go or no-go) on that trial. It also specified the possible outcomes for that trial, contingent on participants' responses – these could (for correct/incorrect responses respectively) be either reward/no reward or no punishment/punishment. This created four distinct trial types, as laid out in Table 2.2: 'go to win reward', 'go to avoid punishment', 'no-go to win reward' and 'no-go to avoid punishment'. There were likewise four possible fractals, one for each response-outcome combination. Participants were not explicitly told their associations – they had to work these out through trial and error.

Table 2.2. The four trial types of the Orthogonal Go/No-Go Task, with stimuli. Note that this builds on Table 2.1, additionally including an example allocation of fractals to the different trial types. In the study the fractal allocation was randomised at the Baseline testing session.

	Reward	**Punishment**
Go	 Go to Win Reward	 Go to Avoid Punishment
No-Go	 No-Go to Win Reward	 No-Go to Avoid Punishment

Next, a circle was presented either on the left or right side of the screen, which provided the target to which participants could respond using the 'S' or 'L' keys. Participants were instructed that, if they chose to make a response here, they had to press the key that was on the same side as the target – otherwise their response was classed as incorrect.

Finally, the outcome for that trial was shown, consisting of a happy/neutral/sad face and the words '+10 points/0 points/-10 points' for reward/neutral/punishment outcomes respectively. These outcomes were probabilistic, such that on 20% of 'reward' trials a correct response in fact led to a neutral outcome, while an incorrect response led to reward; similarly on 20% of 'punishment' trials a correct response led to punishment while an incorrect response avoided it.

After an initial set of practice rounds using a different set of stimuli, the main phase of the task consisted of 100 trials, 25 per condition, with the different trial types presented in a random order. At the start of the Baseline testing session, the fractal allocation was randomised for each participant, who then kept the same allocation throughout the rest of the study (i.e. for both the training and Follow-up testing sessions).

2.3.4.2 Auditory Affective Bias Task (Aylward et al., 2020)

In the first phase of this task, participants had to correctly identify a random sequence of high (1000Hz) and low (500Hz) tones. Each tone was presented for 1000ms, during which time participants could press the 'Z' or 'M' keys to indicate whether they thought the tone sounded high or low (the tone-key mapping was randomised across participants). If they responded correctly, they were shown a message that lasted 750ms stating they had won a (virtual) monetary reward: one of the tones was associated with a £4 reward, the other with a £1 reward, with the exact mapping between the tones and the reward values again being randomised across participants. On the other hand, if participants responded incorrectly (or failed to respond in time) they were shown a message stating "Timeout for incorrect [/late] response ”, which lasted 3250ms. This acquisition phase comprised 20 trials, with 10 low and 10 high tones, presented in random order.

In the second phase of this task, participants were told that, as well as the high and low tones that they had practiced on, they would also hear tones at other pitches. They were now instructed to respond by indicating whether the tone they heard sounded more like the original high tone or low tone. In fact, the new, ambiguous tones were always 750Hz (i.e. exactly halfway in-between the originals). For half of the trials, this mid-tone was treated as if it were closer to the high tone (and so led to the same reward if correctly identified) and on the other half of trials it was treated as if it were closer to the low tone. There were 240 trials in this phase of the task, with 80 trials each of high, low and ambiguous stimuli, presented in a random order. The principle measure of interest was whether participants rated the intermediate tone as being closer to the original high- or low-rewarded tones, with this quantifying their positive (or negative) affective bias.

2.3.4.3 Risk Taking Task (Rutledge et al., 2015)

In this economic decision-making task, participants chose between either a certain outcome or a 50-50 gamble which might improve their position, but could also worsen it. On gain trials, participants chose between a certain gain (20–60 pence) and a gamble returning either £0 or a larger gain (determined as a multiple of the certain gain amount, between 0.78 and 2.1 times). In loss trials, participants chose between a certain loss and a gamble returning either £0 or a larger loss (determined by the same multipliers as used for the gain trials). There were 200 trials in total, 100 gain trials and 100 loss trials, presented in random order. There was no time limit for responding, but after the response was made, the unchosen options disappeared and, after a delay of 2 seconds, the outcome was shown on the screen. There was a 1 second interval between a participant giving their response and the start of the next trial.

2.3.4.4 Beck Depression Inventory (BDI; Beck et al., 1996)

The Beck Depression Inventory II is a questionnaire that asks about symptoms relevant to depression. It ordinarily contains 21-items, however we opted to remove one question that asks about thoughts of suicide due to the safeguarding risks this presented. Each item was scored from 0-3, and we report the total score across all items.

2.3.4.5 State-Trait Anxiety Inventory (STAI; Spielberger et al., 1983)

The STAI is a 40-item questionnaire comprising two subscales, one that asks questions about feelings of anxiety "in general" (trait) and the other about feelings "at this moment" (state). We report the total score for each subscale separately.

2.3.4.6 Pavlovian Bias Training

Participants were trained on a variant of the same Orthogonal Go/No-Go Task described above, but with just a subset of conditions: those in the active training group practiced the 'go to avoid punishment' and 'no-go to win reward' trial types only, whilst the sham group were trained on the 'go to win reward' and 'no-go to avoid punishment' trial types only. They trained with the same fractal allocations as used for the laboratory testing. Each cue was shown 24 times, for a total of 48 trials per training session (in a random order).

The training was administered via an online platform, Gorilla (www.gorilla.sc/about). At the end of the first laboratory testing session, participants were provided a unique login to the platform, allowing them to complete the training remotely, in their own time. They were instructed to complete one session per day and a minimum of five sessions overall, before they returned to the laboratory for their Follow-up session seven days after the Baseline session. To encourage compliance with the training, the Gorilla system sent out an automated reminder email to participants each day and, after a training session was completed, blocked participants from starting a new session until the next day.

2.3.5 Analysis

Our primary analysis throughout was an ANOVA. The dependent variables for each of the tasks are listed below:

- *Orthogonal Go/No-Go Task:* accuracy (proportion correct)
- *Affective Bias Task:* bias (proportion of responses to the ambiguous trials that matched the stimulus shown to the high- or low-reward exemplars). Values > 0.5 indicate a positive bias, and < 0.5 refer to a negative bias.
- *Gambling Task:* proportion of gambles chosen

Across all tasks, training condition (active or sham) was the only between-subjects independent variable, while timepoint (Baseline vs Follow-up) constituted a within-subjects variable. The specific ANOVAs carried out were therefore as follows:

- *Orthogonal Go/No-Go Task:* training condition X timepoint X action (go vs. no-go) X outcome valence (reward vs. punishment)
- *Affective Bias Task:* training condition X timepoint
- *Gambling Task:* training condition X timepoint X framing (gain vs. loss)

In addition we also assessed performance in the training sessions. Here the dependent variable had to be the average accuracy across trial types, because participants in different groups completed different trial types. The ANOVA therefore contained only training condition X timepoint groups.

Throughout these analyses, we further investigated any significant effects indicated by the ANOVAs using *post hoc* simple effects ANOVAs and *t*-tests as appropriate.

2.4 Results

2.4.1 Orthogonal Go/No-Go Task

Unfortunately we were unable to analyse the data for this task due to a technical issue that prevented participants' responses from all being recorded.

2.4.2 Pavlovian Bias Training

There was a significant interaction between training condition and timepoint on the average accuracy during online training, $F(4, 232) = 3.9$, $p = .004$, $\eta^2_{partial} = 0.06$. Participants in the sham training group improved over the course of training (*post hoc* one-way ANOVA: $F[4,116] = 11.9$, $p < .001$, $\eta^2_{partial} = 0.29$), whereas those who did the active training did not ($F[4,116] = 0.63$, $p = .64$).

Specifically, those in the sham training group improved between sessions one and two only, $t(29) = 3.28$, $p = .01$, $d = 0.6$, with no significant improvement thereafter (for the remaining sequential comparisons: $p = .50$, 1 and 1 respectively, using the Bonferroni correction for multiple comparisons).

There was also a significant main effect of timepoint, $F(4, 232) = 5.21$, $p < .001$, $\eta^2_{partial} = 0.08$, but no main effect of training condition, $p = .20$.

Descriptive statistics are provided in Table 2.3 below and results are plotted in Figure 2.4. Note that for extra detail we have split the plots by trial type, although the analyses reported above were actually conducted on the average accuracy across the two trial types completed by each group.

Table 2.3. Descriptive statistics for the Pavlovian bias training.

Training condition	Session number	Accuracy Mean (*SD*)
Sham	1	0.73 (0.23)
	2	0.82 (0.22)
	3	0.86 (0.20)
	4	0.88 (0.20)
	5	0.88 (0.19)
Active	1	0.75 (0.22)
	2	0.79 (0.19)
	3	0.79 (0.20)
	4	0.78 (0.21)
	5	0.75 (0.21)

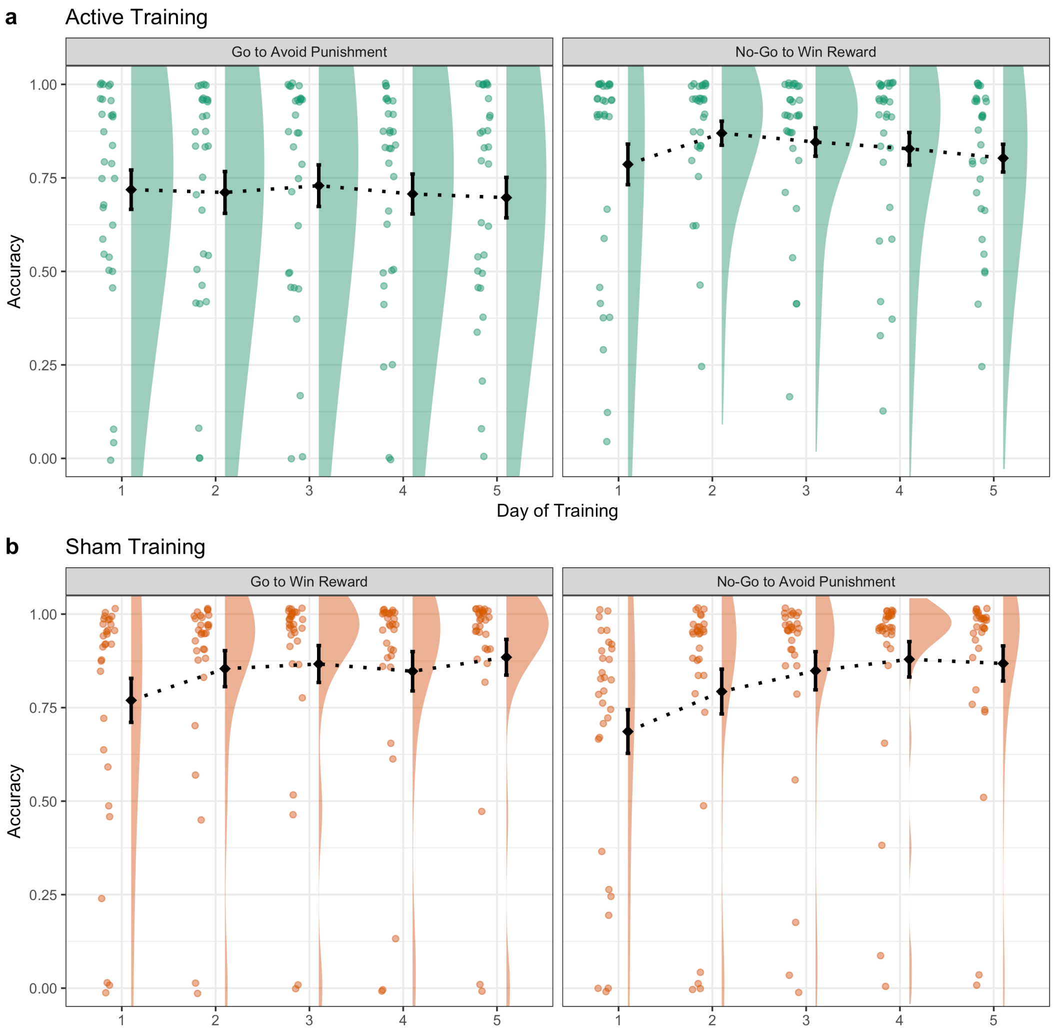


Figure 2.4. Performance on the Pavlovian bias training, split by trial type. Average accuracy in the active training group did not improve over the course of training, whereas accuracy did improve for the sham group. Plots show individual data points and distributions (colour) and means±SE (black).

2.4.3 Affective Bias Task

One participant in the sham training group was excluded from this analysis as their Baseline score did not record. There was no interaction between timepoint and group, $F(1,57) = 0.72$, $p = .40$, nor were there any significant main effects of timepoint or group individually, $F(1,57) = 1.22$, $p = .27$, and $F(1,57) = 0.70$, $p = .41$ respectively. Descriptive statistics are given in Table 2.4 and plotted in Figure 2.5.

Finally, we also examined the associations between affective bias (averaged across the two sessions) and scores on each of the mental health symptom scales. These were not significant: $r = -0.24$, p = .07 for the correlation with BDI; $r = 0.06$, $p = .64$ for the correlation with state anxiety; and $r = -0.22$, $p = .10$ for the correlation with trait anxiety.

Table 2.4. Descriptive statistics for the Affective Bias Task.

Training Group	**Timepoint**	**Mean bias (*SD*)**
Active	Baseline	0.46 (0.18)
	Follow-up	0.50 (0.16)
Sham	Baseline	0.45 (0.17)
	Follow-up	0.45 (0.18)

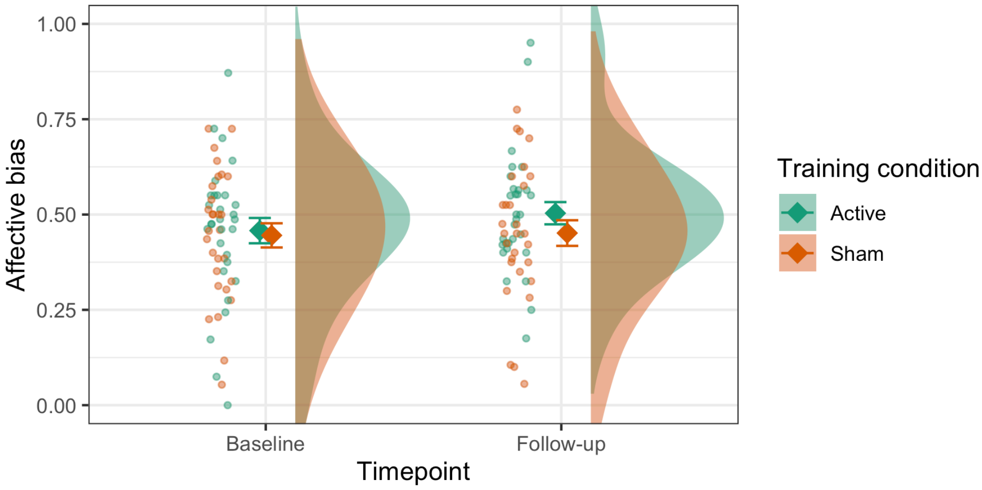


Figure 2.5. Affective bias before and after training. Affective bias is measured by the proportion of responses matching the ambiguous stimuli to the high-reward exemplar (a value of 0.5 is neutral, <0.5 is a negative bias and >0.5 is a positive bias). There were no significant differences between timepoints or training groups. Plot shows individual data points (left), mean$\pm$SE (centre) and distributions (right).

2.4.4 Risk Taking Task

The only significant effect was that of the framing, $F(1,58) = 36.1$, $p < .001$, $\eta^2_{partial} = 0.38$, with more gambles being chosen in the gain frame ($M = 0.57$, $SD = 0.20$) than the loss frame ($M = 0.37$, $SD = 0.21$). This result is illustrated in Figure 2.6.

All other effects were non-significant, namely: the main effects of timepoint and training group, $F(1,58) = 0.87$, $p = .35$, and $F(1,58) = 2.94$, $p = .09$ respectively; the interactions between training group and timepoint, $F(1,58) = 0.05$, $p = .82$, training group and framing, $F(1,58) = 0.36$, $p = .55$, and timepoint and framing, $F(1,58) = 2.42$, $p = .13$; and the three-way interaction between training group, timepoint and framing, $F(1,58) < 0.001$, $p = .97$.

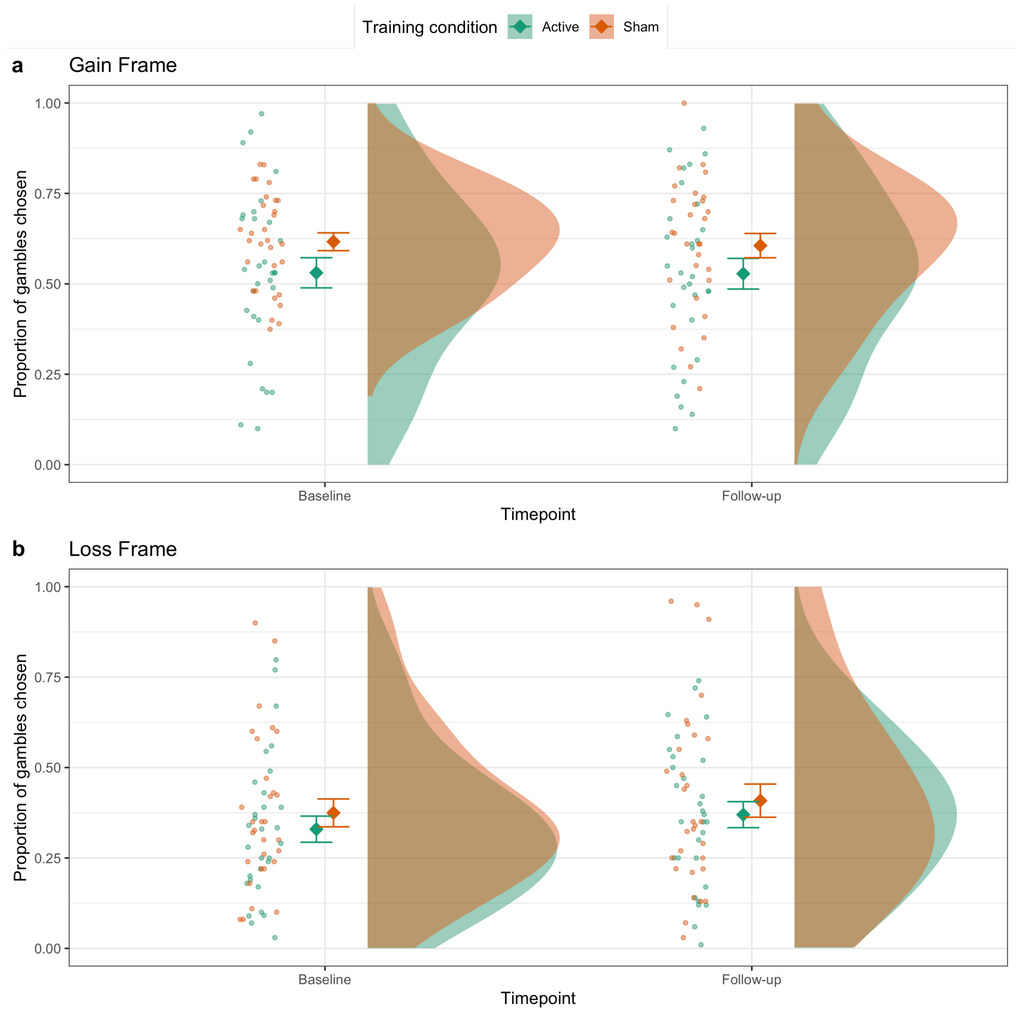


Figure 2.6. Risk Taking Task: Proportions of gambles chosen. There was a significant overall effect of framing – participants gambled more often when the gamble was framed by a certain gain as opposed to a loss. Plots show (left to right) individual data points, mean±SE and distributions.

2.4.5 BDI

One participant in the sham group was excluded from this analysis as their pre-training score did not record. There was a significant main effect of timepoint, $F(1,57) = 6.63$, $p = .01$, $\eta^2_{partial} = 0.10$, with mean BDI scores reducing from 5.63 ($SD = 5.09$) in the Baseline session to 4.66 ($SD = 4.02$) in the Follow-up session. This result also remained significant ($p = .01$) if we excluded the outlier (participant with Baseline BDI score > 20).

There was no significant effect of training group, F(1, 57) = 0.61, $p = 0.44$, nor of the interaction between timepoint and group, $F(1,57) = 0.13$, $p = 0.72$. These results are illustrated in Figure 2.7.

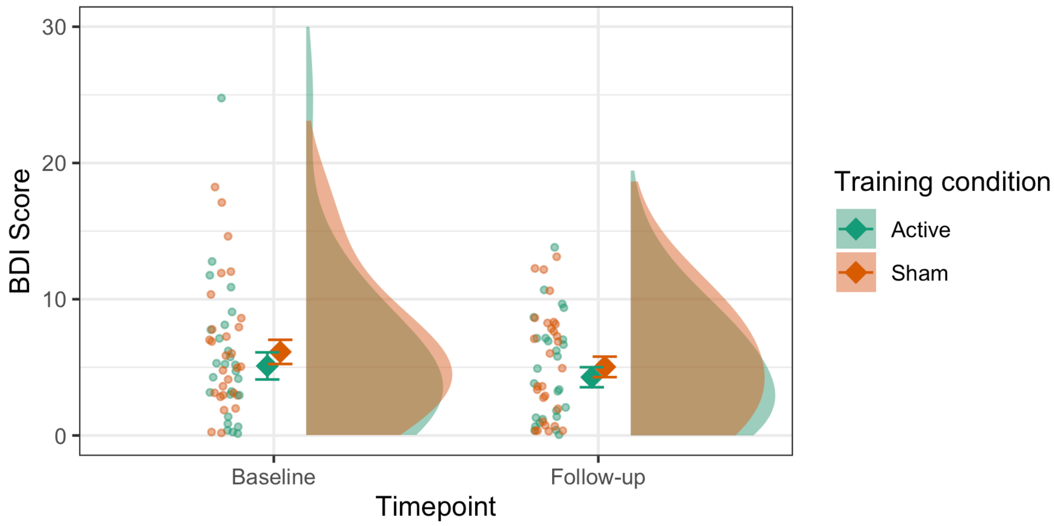


Figure 2.7. Beck Depression Inventory scores. Scores decreased significantly from Baseline to Follow-up, for both training groups. Plot shows individual data points (left), mean±SE (centre) and distributions (right).

2.4.6 STAI

There were no significant effects on either the state or trait subscales of the STAI. For the state subscale, the results were: training group, $F(1,58) = 0.38$, $p = 0.54$; timepoint, $F(1,58) = 3.51$, $p = .07$; timepoint x group interaction, $F(1,58) = 1.07$, $p = 0.31$. For the trait subscale, the results were: training group, $F(1,58) = 3.53$, $p = 0.07$; timepoint, $F(1,58) = 1.55$, $p = 0.22$; timepoint x group interaction, $F(1,58) = 0.07$, $p = 0.80$. These are plotted in Figure 2.8.

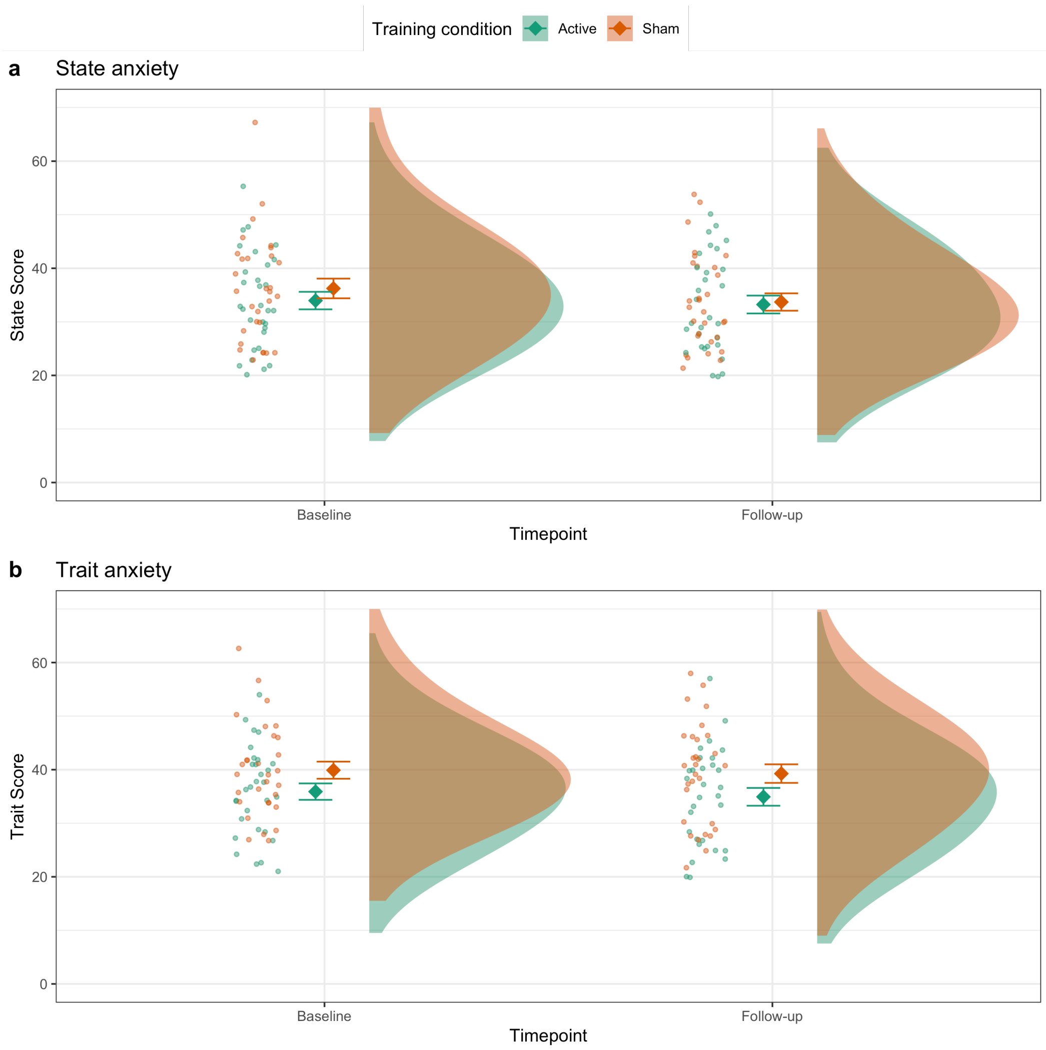


Figure 2.8. State-Trait Anxiety Inventory scores. There were no differences between either timepoints or training groups. Plots show individual data points (left), mean±SE (centre) and distributions (right).

2.5 Discussion

In this study we examined whether control over Pavlovian bias is amenable to behavioural training. Unfortunately, a technical problem with the Orthogonal Go/No-Go Task meant we were not able to directly compare performance between the Baseline and Follow-up sessions on this task. Over the course of the training sessions, however, we found there was no significant improvement in performance. We likewise did not observe a significant change between timepoints in affective bias, propensity to gamble, or state or trait anxiety. Regarding the Risk Taking Task, we did see a main effect of gamble framing across timepoints and groups, reproducing results from previous studies (e.g. Rutledge et al., 2015). Finally, we observed a significant overall reduction in BDI between the Baseline and Follow-up sessions across both groups.

In the Orthogonal Go/No-Go task, the lack of a significant change in performance across the training sessions strongly suggests that the training had no effect. If so this could indicate that Pavlovian biases are fixed and unable to be changed, which in turn suggests that they are not affected by cognitive control. Alternatively, it is also possible that Pavlovian biases are variable in principle, but we just did not succeed in this particular study. On reflection this is perhaps the more likely given the study was powered to detect effects of $d = 0.6$ or greater (for the Baseline–Follow-up comparison), which is typically regarded as a moderate-large effect size in cognitive science (Dienes, 2008) and means the training could have had a small-moderate effect that we did not have the power to detect.

In an unexpected result, participants who received the sham training did show an improvement over the course of training. This cannot be explained in terms of changes in cognitive control, since the sham group practiced only on the trials that did not require control. Instead, this result could indicate that participants had not fully learned the parameters of the task during the Baseline session; if so, then, when the training began, those in the sham group (experiencing only the easier trials) would likely have found the task more engaging and so learned more quickly, whereas those in the active group (training with only difficult trials) may have experienced more failures and found the training dispiriting. In economic models of cognitive control (e.g. EVC, Shenhav et al., 2013), one of the key parameters is control efficacy—it makes sense to exert control only when that control is able to affect the outcome—which itself is learned from experience. It may be that in this case participants in the active group, because they made more frequent mistakes and received more negative feedback during training, came to believe that their control had low efficacy.

In the absence of a significant active training effect, we expected not to see any training effects on the secondary tasks either, and indeed this was the case. There was however a small, significant decrease in depression scores common across all groups between the Baseline and Follow-up sessions; this probably represents either a placebo effect from the training, or an artefact of repeated testing more generally.

The lack of correlation between affective bias and depression and anxiety symptoms was somewhat surprising. Previous studies looking at these associations, however, have either used a case-control design (Aylward et al., 2020), or those that have examined continuous symptom scales in the general population have done so with much larger sample sizes (Daniel-Watanabe et al., 2022). Nevertheless, our measured correlation of $r = -0.22$ between affective bias and depression scores is of similar size to that found by Daniel-Watanabe et al., which is encouraging for future research – with greater statistical power we may be able to detect a significant association.

2.5.1 Limitations

The difficulty recording usable response data for the Orthogonal Go/No-Go Task was clearly a significant problem, so we decided to repeat this experiment again (with some modifications), which will be reported in the next chapter. As well as addressing the technical issue, we also judged that the sample size would need to be significantly increased – the present study was powered to detect an effect size of *d* = 0.6, which seemed eminently achievable given our pilot results, yet was too large to allow the null result we observed here to be fully interpretable. Given the present study points towards the active training having had no effect, any replication would need to be powered to detect a smaller effect size so as to allow a null result to be more definitive.

In addition, another issue raised in the discussion above was whether all participants fully understood the kinds of contingencies they needed to learn prior to starting the training. Otherwise, those in the active and sham groups may have experienced differing efficacy of control over the outcomes, contributing to differences in performance between these two groups (Shenhav et al., 2013, 2017). Although it has been difficult to verify whether this was a problem with this study specifically, it would nevertheless be useful in future experiments to include comprehension checks prior to the commencement of the main phase of this task (and indeed, the secondary tasks as well), to eliminate this problem as much as possible.

2.5.2 Conclusion

Interpretation of our results has to be tempered by the acknowledgement that we were not able to assess performance on the full Orthogonal Go/No-Go task due to the technical issue with this task. Nevertheless, the lack of improvement over the course of the online training sessions strongly suggests that the training was not successful – indeed, surprisingly, the group receiving the sham training showed a significant improvement even while the active group did not. In order to resolve some of the limitations raised above and complete this line of work, we undertook to repeat this study following some improvements to the design. This is described in Chapter 3.

Chapter 3. Learning to Overcome Pavlovian Biases: Online Replication

3.1 Abstract

In the previous chapter we investigated whether participants could learn to overcome their Pavlovian biases through a regime of behavioural training on the Orthogonal Go/No-Go task (Guitart-Masip et al., 2011). We found that they were apparently unable to do so, but we also identified several limitations to the study that needed to be improved in order to fully interpret and understand this null result. In the present chapter we therefore report the results of a replication in which, after having made the required changes (in particular, increasing the sample size, and therefore statistical power, substantially), we looked again at whether participants could reduce the strength of their Pavlovian biases through training. This time we found that the training *was* effective, which we suggest shows that people are capable of controlling their Pavlovian biases. As before, however, there were no transfer effects either to the secondary tasks or to the depression and anxiety symptom scales that were also included. We discuss in more detail the possible reasons for the difference with the previous study, as well as more broadly what this means for the interaction between control and Pavlovian biases, which we frame in terms of effort-based decision-making.

3.2 Introduction

Action selection is thought to be governed by two distinct types of learning system. The instrumental system (which can be further broken down into model-based and model-free implementations) learns the associations between actions and their consequences, and can thereby select an appropriate response that will maximise reward or minimise punishment (Dickinson & Balleine, 2002). The second system is the Pavlovian system, and although this is primarily concerned with learning the contingencies between stimuli, it also drives action: the Pavlovian system tends to invigorate responses to stimuli associated with reward (called an approach bias) and inhibit those to stimuli associated with punishment (avoidance bias; Dayan & Balleine, 2002; Dayan et al., 2006). In other words, the main distinction between the two is that the Pavlovian system encodes a fixed set of responses to different situations, whereas the instrumental system has to learn its responses from scratch, and in so doing is more flexible.

In general, the instrumental and Pavlovian systems support one another – in most cases, rewarding stimuli need to be approached and engaged with, whereas punishing stimuli should be avoided. Sometimes, however, the Pavlovian system can interfere with optimal responding, such as when one needs to take action in a potentially dangerous environment or, conversely, ignore immediate rewards. This interference is termed Pavlovian-instrumental conflict and, in these cases, the approach or avoidance biases introduced by the Pavlovian system can lead to errors and suboptimal behaviour (Boureau & Dayan, 2011; Guitart-Masip, Duzel et al., 2014).

In the previous study (Chapter 2) we introduced the question of whether the strength of Pavlovian biases can be modified using behavioural training. Specifically, we hypothesised that while the Pavlovian responses themselves are fixed, the extent to which they are allowed to influence behaviour is subject to cognitive control. If participants can be taught to overcome their Pavlovian biases, this would be further evidence that cognitive control is to some extent responsible for their presence (or otherwise) in behaviour. Moreover this would have potentially important clinical applications, in particular for understanding and treating anxiety and depression, two conditions in which enhanced Pavlovian biases and decreased cognitive control are known to feature (Dayan & Huys, 2008).

While the results of the previous study pointed towards there being no effect of training, this was not conclusive, principally because the study had not been powered to detect small effect sizes and because of a technical problem with one of the tasks. Therefore, in the present study, we sought to re-examine the Pavlovian bias training using an optimised design. Specifically, we used this opportunity to make a number of changes, the largest of which was moving the entire study (including recruitment and both Baseline and Follow-Up sessions) completely online. Online studies have been a growing feature of cognitive research over the past decade, with the recent COVID-19 pandemic (and attendant need to conduct research remotely) contributing to their much wider recognition and uptake. Online research has a number of advantages over traditional in-person studies: the sample sizes that can be obtained are several orders of magnitude higher than in the laboratory; the participants themselves are more diverse (both more international and with a wider range of ages); and, if experiments are coded in languages like HTML and javascript, study materials can be shared between researchers much more easily, supporting open science initiatives.

As mentioned in the discussion section of Chapter 2, one of the limitations of the earlier study was the fact that the tests had been powered to detect a training effect size of $d \geq 0.6$. This made it harder to interpret the null result, as we could not distinguish between the possibilities that either the training indeed had no effect, or that the effect was smaller than $d = 0.6$ but we lacked sufficient statistical power. To resolve this, in the present study we reduced the assumed effect size to $d \geq 0.25$, allowing us to make a stronger claim about the effect of the training in the event of a null result: given that in the previous study (Chapter 2), the standard deviation of the corresponding effect between the first and last training sessions was 0.18, an effect size of $d = 0.25$ implies that the active training improves performance 4.5% above the sham training; we suggest that this is a reasonable, minimally interesting effect size. This required a sample size of 800 participants (see Section 3.3.2 Participants below for the power calculation), a number that would have been impractical in the laboratory.

As before, the primary aim of the study was to assess the effect of a regime of behavioural training on control over Pavlovian biases. We specifically hypothesised that the active training group would show a greater reduction in Pavlovian bias (as indicated by both computational and model-agnostic analyses) between Baseline and Follow-Up sessions. We also had several secondary hypotheses. We expected that, in the Go/No-Go Task, we would see a significant interaction effect between required action and valence in the Baseline session, indicating the presence of Pavlovian biases. In addition, we predicted that, if the training was successful, we would see transfer to the other tasks, in that the active training group would show a greater reduction in bias on the Affective Bias task, and in Pavlovian bias on the Risk Taking Task, as well as reductions in BDI and STAI scores.

3.3 Methods

3.3.1 Preregistration

This study was preregistered on the Open Science Framework (https://osf.io/7msvw/?view_only=f0f9edee61c94c7e8e1804d1939df68c).

3.3.2 Participants

Participants were recruited through the online platform Prolific. The study was advertised only to participants who met the following inclusion criteria: aged 18-60, fluent in English and no history of psychiatric or neurological disorders. Participants also had to use a computer – smartphones or tablets were not allowed.

In our preregistration we decided to power this study for an effect size of d = 0.25. Following the previous study we anticipated that the true effect of the training may be small; we therefore considered d = 0.25 a reasonable effect size that would allow us to interpret a non-significant result meaningfully. Assuming α = 5% and power = 90% (two-tailed), we calculated a required sample size of 676 participants (338 per training group). We then rounded this up to a total of 800 participants, to allow for attrition and exclusions. Of these, 110 were subsequently excluded, leaving 690 participants whose data was included in the final analysis. A detailed breakdown of the reasons for exclusion is given in Section 3.3.3.1 below.

3.3.3 Procedure

The procedure was very similar to that of the earlier Pavlovian Bias Training experiment (Chapter 2). On the first day participants completed a Baseline testing session in which they completed the Orthogonal Go/No-Go Task (Guitart-Masip et al., 2011), an Affective Bias Task (Daniel-Watanabe et al., 2022), the Risk Taking Task (Rutledge et al., 2015) and the two self-report symptom scales (the Beck Depression Inventory, Beck et al., 1996; and the State-Trait Anxiety Inventory, Spielberger et al., 1983). Then, over the subsequent six days participants had to complete five online training sessions (with no more than one session per day permitted). Finally, on the eighth day of the study, there was a Follow-Up session containing the same battery of tasks as at Baseline.

However, unlike the earlier experiment, all of the sessions in this experiment were conducted online. For each session, participants signed up on Prolific and were then redirected to Gorilla (www.gorilla.sc) where the study was hosted. Initially (at Baseline) the study was available to all participants who met the inclusion criteria; for subsequent sessions, links to take part were sent out only to those participants who had fully completed the rest of the study up to that point. At the end of each session, Gorilla redirected participants back to Prolific via a unique link, which allowed us to verify that the participant had completed that session of the experiment. Payment for the study was withheld until after the final session, in order to incentivise full completion – those who did the full study were paid £15 (approximately £7.50/hr), while those who dropped out or were excluded part-way through received an equivalent amount *pro rata*.

While the tasks themselves were substantially the same as in the earlier study, there were some specific changes which are described in Section 3.3.4.

3.3.3.1 Participant Exclusions

A detailed schedule of the reasons for exclusion is provided in Table 3.1 and the criteria are also described in more detail under the relevant subheading in Section 3.3.4. All of the reasons for exclusion were preregistered except one, which excluded participants who, on the Go/No-Go task, responded on more than 12/80 trials with keys that were not in the response set (S or L). Participants with such a large number of wrong key responses were considered to have either forgotten the task instructions or been consistently very careless, in both cases invalidating their data. This criterion led to the exclusion of three participants; no other participants made close to so many wrong-key responses (the maximum among the other participants was 3/80). When excluded, participants' data was removed from the whole experiment.

Table 3.1. Schedule of exclusions. GNG=Go/No-Go Task, Aff. Bias=Affective Bias Task, STAI=State-Trait Anxiety Inventory. All criteria were preregistered except one, the criterion for the Go/No-Go task: '> 15% keys pressed not S/L'. See main text for details.

Timepoint	**Reason**	**N excluded**	**N remaining**
			800
Baseline Testing	Did not complete baseline session	11	789
	GNG: go to win reward accuracy < 65%	9	780
	GNG: left/right accuracy < 65%	1	779
	Aff. Bias: unambiguous accuracy < 60%	29	750
	Aff. Bias: no response on > 15% of trials	2	748
	STAI: failed attention check	2	746
Training	Did not complete 5 training sessions	46	700
Follow-Up Testing	Did not complete follow up session	1	699
	GNG: go to win reward accuracy < 65%	2	697
	GNG: wrong-key responses >15%	3	694
	Aff. Bias: unambiguous accuracy < 60%	1	693
	Aff. Bias: no response on > 15% of trials	2	691
	STAI: failed attention check	1	690

3.3.4 Measures and Tasks

3.3.4.1 Orthogonal Go/No-Go Task (Guitart-Masip et al., 2011)

This task was identical to that used in the earlier study (Chapter 2), with the exceptions that:

- The technical problem that had affected the recording of participants' responses in the previous study was now resolved
- The instructions and practice rounds were more detailed, and participants then had to pass a short, multiple-choice comprehension test (e.g. 'what keyboard keys should you use during the task?') in order to proceed to the main phase
- The number of trials in the main phase was reduced to 80 (20 trials per condition), in order to compensate for the increased length of the instructions and keep the length of the Baseline/Follow-Up session to no more than an hour

In our preregistration we set out three exclusion criteria for this task. Participants were excluded if:

- They failed the comprehension test 5 times
- Their accuracy on the go-to-win trials during the practice phase was less than 65%
- Less than 65% of their go responses matched the same side as the target circle

In addition, as noted in Section 3.3.3.1 above, we also had to exclude three participants who had made a large number of responses (more than 12 trials out of 80, i.e. >15%) using keys that were not S or L. These clearly either completely forgot or ignored the task instructions, invalidating their data. No other participants made nearly so many wrong-key responses (the maximum among the others was 3 trials out of 80).

3.3.4.2 Visual Affective Bias Task (Daniel-Watanabe et al., 2022)

This task differed more substantially from that used in the earlier study. We now used visual rather than auditory stimuli because in a remote context we can be more confident that these are being presented consistently to all participants.

Specifically, rather than high- and low-pitched tones, participants were shown large- and small-sized black circles. Structurally it was otherwise identical to the earlier task: there was an initial acquisition phase, during which participants learned to identify the two example stimuli (each of which was paired with a different reward amount); then subsequently, during the main phase, an intermediate, ambiguous stimulus was also introduced and participants had to decide which of the exemplar stimuli it was most similar to.

The other difference was the number of trials: while the acquisition phase again contained 20 trials, as before, the main phase now contained a reduced number of trials, 120 in total (40 each of the large, small and intermediate circles)

There were two exclusion criteria for this task which were again preregistered:

- Participants were excluded if they made no response on >15% of trials
- They were also excluded if they incorrectly identified >40% of the unambiguous stimuli during the main phase of the task

3.3.4.3 Risk Taking Task (Rutledge et al., 2015)

This task differed from that used in the earlier study in that:

- A new 'mixed frame' trial type was introduced, in which a gain or a loss were both possible outcomes of choosing to gamble, while the certain option was set at zero (previously only single-valence 'gain' or 'loss' trials had been shown)
- The number of trials was reduced to 150 in total, 50 of each trial type

There were no exclusion criteria for this task.

3.3.4.4 Beck Depression Inventory (BDI; Beck et al., 1996)

This was unchanged from the earlier study.

3.3.4.5 State-Trait Anxiety Inventory (STAI; Spielberger et al., 1983)

This was unchanged from the earlier study. However, we did add a catch question ("Press the very much so button") at the end of the questionnaire to detect inattentive participants without interfering with the STAI's psychometrics. Participants who failed this question were excluded.

3.3.4.6 Pavlovian Bias Training

This was unchanged from the earlier study. Participants had six days between the Baseline and Follow-Up testing sessions to complete five training sessions (and were not able to do more than one session per day). As set out in our preregistration, they were excluded if they did not complete the training on schedule.

3.3.5 Preregistered Analyses

To test our primary hypothesis that the training would enhance control over Pavlovian biases in the Orthogonal Go/No-Go task, we planned two related analyses, one that was model agnostic and another that used computational modelling.

3.3.5.1 Model Agnostic Analyses of the Orthogonal Go/No-Go Task

For the model agnostic analysis, we calculated a measure of Pavlovian bias for each participant in each session. This was defined as the sum of the accuracies for the two high Pavlovian-instrumental conflict trial types (go to avoid punishment and no-go to win reward) minus the sum of the two low conflict trial types (go to win reward and no-go to avoid punishment). We then computed a training effect, which was the change in this metric between the Baseline and Follow-Up sessions. Finally we tested (using an independent samples *t*-test) whether there was any difference in this change between the Active and Sham training groups.

We also had a secondary hypothesis for this task: we predicted that, in the Baseline session, there would be an interaction effect between required response and outcome valence, indicating the presence of Pavlovian biases. We assessed this by means of a 2 X 2 repeated measures ANOVA on the accuracy data from the Baseline session, followed by four planned paired-samples *t*-tests comparing accuracy in both the go to avoid punishment and no-go to win reward conditions with each of the go to win reward and no-go to avoid punishment conditions.

3.3.5.2 Computational modelling of the Orthogonal Go/No-Go Task

In parallel we also tested our primary hypothesis using a computational modelling approach. All models were fitted in Stan (Gabry & Češnovar, 2021) using the Variational Bayes method; once fitted, we generated 1000 samples from each model, which were then analysed as described below.

The first stage involved identifying the model that best explained the observed data. We started with the winning model of Guitart-Masip et al. (2011), which describes behaviour on each trial as a function of a Rescorla-Wagner learning process with participant-specific reward and punishment sensitivities, a noise component, a go bias and a Pavlovian bias – henceforth we refer to this as the 'Base' model. We then extended this model in three increments (see Section 3.3.5.2.3), gradually adding complexity and examining how this affected the model fit.

3.3.5.2.1 Description of the Base model

In the Base model, each trial was modelled as a two-step process, beginning with response generation and then, after the outcome of that action was observed, a learning step.

During response generation, the tendency to make a go response depended on the difference between the values assigned to the go and no-go options (q_{go} and q_{nogo}); on the first trial these were initialised at 0. To this, the participant's go bias and a Pavlovian component were both added to give the decision weight for making a go response on that trial. Specifically, the Pavlovian component contained the associative value of the stimulus shown, scaled by a Pavlovian bias parameter. The stimulus value was coded such that negative values indicated expected punishment, and positive values expected reward – therefore the model generated the classic Pavlovian pattern of responses (go to reward, no-go to punishment) whenever the Pavlovian bias parameter was positive. These components are set out in Equation 3.1 below.

Decision weight

$$w(s_t) = q_{go}(s_t) - q_{nogo}(s_t) \; + \; GoBias_{subject} + \; Pavbias_{subject} \times value(s_t) \tag{3.1}$$

Subsequently the decision weight was put through a logistic function (augmented by an additional noise component, $\xi_{subject}$, which shifted pGo_t towards 0.5) to give the probability of making a go response on each trial. Finally a response (go or no-go) for each trial was generated by sampling from a Bernoulli distribution with this probability. This is summarised in Equation 3.2 below.

Response generation

$$pGo_t = (1 - \xi_{subject}) \times logistic\big(w(s_t)\big) + \; \xi_{subject} \times \frac{1}{2} \tag{3.2a}$$

$$response_t = Bernoulli(pGo_t) \tag{3.2b}$$

The second set of steps involved updating the learned values of the response that had been chosen (q_{go} or q_{nogo}), as well as the associative value of the stimulus shown ($value(s_t)$). These updates were implemented by Rescorla-Wagner update rules, with separate sensitivities to reward and punishment outcomes. This is summarised in Equations 3.3 and 3.4 below.

Instrumental Learning

$$\begin{aligned} q_{response,t+1}(s_t) &\\ &= q_{response,t}(s_t) \\ &+ \ LearningRate_{valence,subject} \ \times (Sensitivity_{valence,subject} \\ &\times outcome - \ q_{response,t}(s_t)) \end{aligned} \tag{3.3}$$

Pavlovian Learning

$$\begin{aligned} value_{t+1}(s_t) = value_t(s_t) &\\ &+ LearningRate_{valence,subject} \times (Sensitivity_{valence,subject} \\ &\times outcome - value_t(s_t)) \end{aligned} \tag{3.4}$$

3.3.5.2.2 Choice of priors

The participant-level parameters were passed through appropriate link functions and then given hierarchical (population-level) priors which were determined through a process of prior predictive checking. These are set out in Equations 3.5 and 3.6 and plotted in Figures S3.1–S3.5.

Link Functions

$$\begin{aligned}
\xi_{subject} &= \Phi\left(raw_\xi_{subject}\right) \\
LearningRate_{subject} &= \Phi\left(raw_LearningRate_{subject}\right) \\
Sensitivity_{Reward,subject} &= e^{raw_Sensitivity_{Reward,subject}} \\
Sensitivity_{Punishment,subject} &= e^{raw_Sensitivity_{Punishment,subject}}
\end{aligned}$$

(3.5)

Priors

$$\begin{aligned}
GoBias_{subject} &\sim Normal(\mu_{GoBias}, \sigma_{GoBias}) \\
PavBias_{subject} &\sim Normal(\mu_{PavBias}, \sigma_{PavBias}) \\
raw_\xi_{subject} &\sim Normal\left(\mu_{\xi}, \sigma_{\xi}\right) \\
raw_LearningRate_{subject} &\sim Normal(\mu_{LR}, \sigma_{LR}) \\
raw_Sensitivity_{Reward,subject} &\sim Normal\left(\mu_{Reward_sens}, \sigma_{Reward_sens}\right) \\
raw_Sensitivity_{Punishment,subject} &\sim Normal(\mu_{Punishment_sens}, \sigma_{Punishment_sens})
\end{aligned}$$

$$\begin{aligned}
\mu_{GoBias} &\sim Normal(0,1.5) & \sigma_{GoBias} &\sim Exponential(0.8) \\
\mu_{PavBias} &\sim Normal(0,2) & \sigma_{PavBias} &\sim Exponential(0.5) \\
\mu_{\xi} &\sim Normal(0,0.5) & \sigma_{\xi} &\sim Exponential(1) \\
\mu_{LR} &\sim Normal(0,1) & \sigma_{LR} &\sim Exponential(1) \\
\mu_{Reward_Sens} &\sim Normal(0,0.3) & \sigma_{Reward_Sens} &\sim Exponential(1) \\
\mu_{Punishment_Sens} &\sim Normal(0,0.3) & \sigma_{Punishment_Sens} &\sim Exponential(1)
\end{aligned}$$

(3.6)

3.3.5.2.3 Other models

We gradually extended the Base model in three stages: we included distinct Pavlovian approach and avoidance biases; we included separate learning rates for reward and punishment (but not approach/avoidance biases); finally we included both reward/punishment learning rates and approach/avoidance Pavlovian biases in the same model.

3.3.5.2.4 Modelling of experimental conditions

The Baseline data was fitted with a single model regardless of participants' group membership, because we know *a priori* that the active and sham groups were identical at Baseline because participants were allocated at random (avoiding the so-called Table 1 fallacy). Subsequently, when analysing the data from the Follow-up Sessions, the model was fitted separately for the two training groups, since at that point there could be a difference between groups. Fitting the groups separately leads to more accurate and less biased parameter estimates (according to parameter-recovery simulations; Valton et al., 2020).

3.3.5.2.5 Assessing the models

We compared these models using the Widely-Applicable Information Criterion (WAIC; Watanabe, 2010), which estimates the leave-one-out predictive accuracy of a model; in so doing, WAIC provides both a point estimate and standard error, allowing us to quantify our uncertainty. We selected the best performing model according to their WAIC values, and then examined the posterior estimates of participants' Pavlovian biases according to this model. Our preregistered analysis, as with the model agnostic approach described above, was to compute the change in Pavlovian bias between Baseline and Follow-Up for each posterior sample, calculate the mean change for each participant and then test (using an independent samples *t*-test) whether there was any difference in this change between the Active and Sham training groups.

In addition to this, we also probed the model with several exploratory analyses: first we examined the posterior predictions from the model and compared them with the empirical data; then, looking at the posterior estimates of the parameters, we plotted the population-level parameters and assessed both the changes in these parameters following the training and whether there were any group differences in these changes.

3.3.5.3 Affective Bias Task

To test our hypothesis for this task, we computed the within-subjects change in affective bias (the change in proportion of responses matching the high-reward stimulus) and then compared this change across the two training groups using an independent samples *t*-test.

3.3.5.4 Risk Taking Task

We analysed the proportion of gambles chosen using a 3 X 2 X 2 ANOVA (framing: loss vs. mixed vs. gain X timepoint: pre- vs. post-training X training group: active vs. sham). Further analyses were conducted on an exploratory basis.

3.3.5.5 Computing environment and packages

Analyses were conducted in R version 3.5.3 (R Core Team, 2019). We used the R package 'rstatix' (0.6.0; Kassambara, 2021) to conduct the frequentist statistics and the Bayesian models were fitted in Stan using CmdStanR (0.3.0, Gabry & Češnovar, 2021).

3.4 Results

3.4.1 Orthogonal Go/No-Go Task

3.4.1.1 Preregistered model agnostic analyses

Beginning with the data for just the Baseline session (Figure 3.1, left panel), we see as expected there was a significant action by valence interaction, $F(1, 689) = 709$, $p < .001$, $\eta^2_{partial} = 0.51$, indicating the presence of Pavlovian biases. Specifically, accuracy on the 'go to win reward' trials was greater than on the 'go to avoid punishment' trials, $t(689) = 18.4$, $p < .001$, $d = 0.70$; conversely accuracy on the 'no-go to win reward' trials was lower than on the 'no-go to avoid punishment' trials, $t(689) = 23.9$, $p < .001$, $d = 0.91$. We also observed significant main effects of action, $F(1, 689) = 1680$, $p < .001$, $\eta^2_{partial} = 0.71$, and valence, $F(1, 689) = 97.3$, $p < .001$, $\eta^2_{partial} = 0.12$. Accuracy was higher when participants were required to make a go response ($M = 0.76$, $SD = 0.17$) compared with no-go ($M = 0.40$, $SD = 0.25$), indicating the presence of an overall 'go bias'; and it was also higher when the incentive involved avoiding punishment ($M = 0.61$, $SD = 0.15$) as opposed to gaining reward ($M = 0.55$, $SD = 0.36$).

In the Follow-Up session (also plotted in Figure 3.1, right panel), there appears to be a clear difference between the active and sham groups. We tested this by looking at the change in the model agnostic measure of Pavlovian bias (details of which were given in Section 3.3.5.2 above); by this measure, the active and sham groups differed significantly in the change in Pavlovian bias experienced, $t(685) = 11.9$, $p < .001$, $d = 0.91$. Specifically, Pavlovian bias in the active group decreased after training, $t(344) = 9.90$, $p < .001$, $d = 0.53$, whereas in the sham group it increased after the training, $t(344) = 6.86$, $p < .001$, $d = 0.37$. Descriptive statistics are given in Table 3.2 and plotted in Figure 3.2.

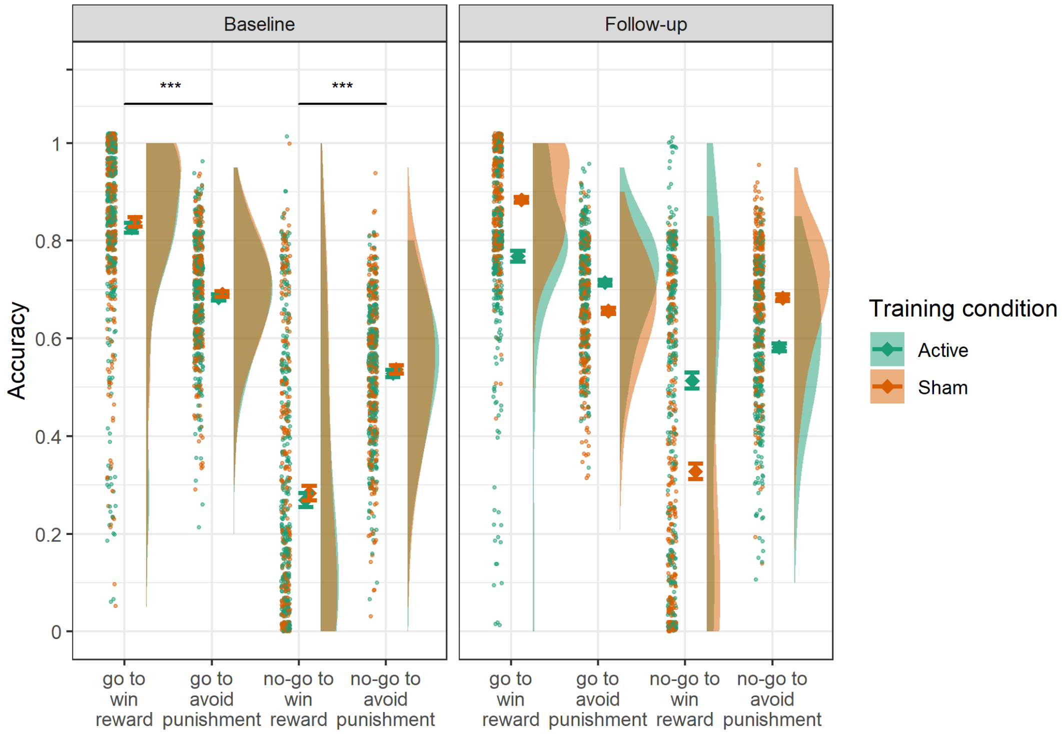


Figure 3.1. Performance on the Orthogonal Go/No-Go Task. Plot shows the average accuracy in each condition. Both groups were closely matched at Baseline and show clear signs of Pavlovian bias (*** shows significant ANOVA interaction, $p < .001$). At Follow-up, Pavlovian biases were decreased in the active training group and increased in the sham group (see Figure 2 for explicit test of change in Pavlovian bias).

Table 3.2 Model agnostic Pavlovian bias measure in each condition.

Training condition	**Timepoint**	**Pavlovian Bias Mean (*SD*)**
Sham	Baseline	0.40 (0.39)
	Follow-Up	0.12 (0.48)
Active	Baseline	0.40 (0.40)
	Follow-Up	0.58 (0.38)

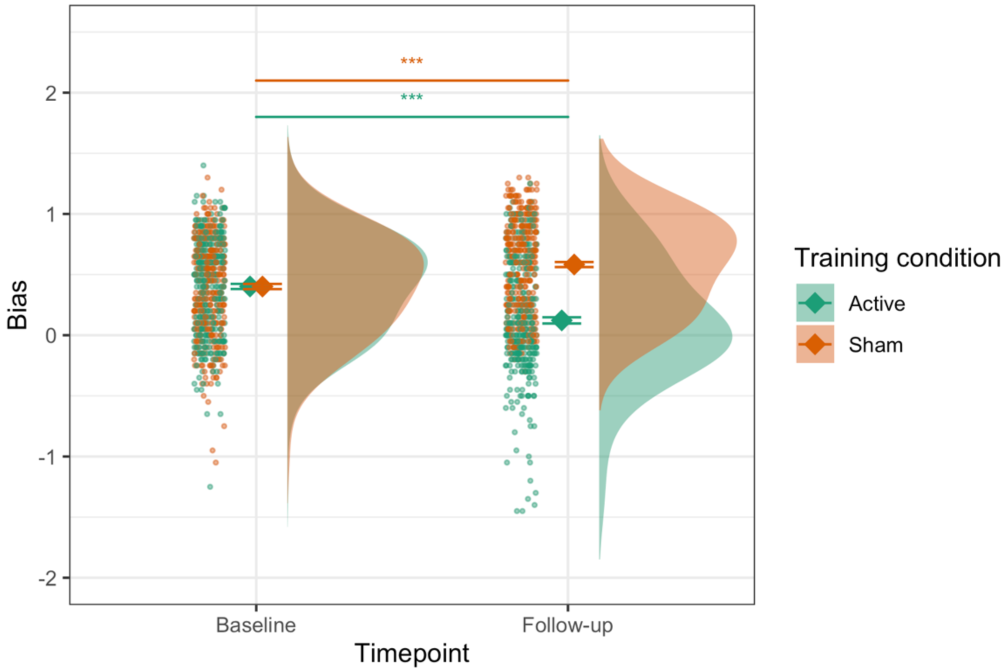


Figure 3.2. The model agnostic measure of Pavlovian bias. Participants in the active training group showed decreased bias after training, whereas those in the sham group showed increased bias. Plot shows the individual data points, mean$\pm$SE and distribution for each timepoint and training condition.

3.4.2 Pavlovian Bias Training

3.4.2.1 Exploratory analysis

Descriptive statistics for performance across the training sessions are given in Table 3.3 and plotted in Figure 3.3. There was a significant interaction between training condition and timepoint on the mean accuracy per session during training, $F(4, 2752) = 63.0$, $p < .001$, $\eta^2_{partial} = 0.08$. Specifically, whilst both training groups significantly improved their accuracy over the course of training, the active group improved by a greater amount: the average improvement from the first to the fifth training sessions for the active training group was 0.16 ($SD = 0.18$), and for the sham group was 0.05 ($SD = 0.09$), $t(491) = 9.91$, $p < .001$, $d = 0.76$.

Regarding the sequential differences between training sessions, the active training group improved significantly between each of the first, second, third and fourth sessions ($p < .001$, $d = 0.61$; $p < .001$, $d = 0.36$; $p < .001$, $d = 0.27$ respectively), but not between the fourth and fifth sessions ($p = .08$; tests were Bonferroni-corrected for multiple comparisons). By contrast the sham group improved significantly only between the first, second and third sessions ($p < .001$, $d = 0.50$; $p = .01$, $d = 0.17$ respectively) and not between the third, fourth and fifth sessions ($p = .50$ and .55).

Table 3.3. Mean accuracy across each of the five training sessions. Both groups improved significantly over the course of the training.

Training condition	**Session number**	**Mean (*SD*) accuracy**
Sham	1	0.93 (0.08)
	2	0.96 (0.04)
	3	0.97 (0.04)
	4	0.98 (0.03)
	5	0.98 (0.03)
Active	1	0.69 (0.21)
	2	0.78 (0.21)
	3	0.82 (0.20)
	4	0.85 (0.20)
	5	0.86 (0.20)

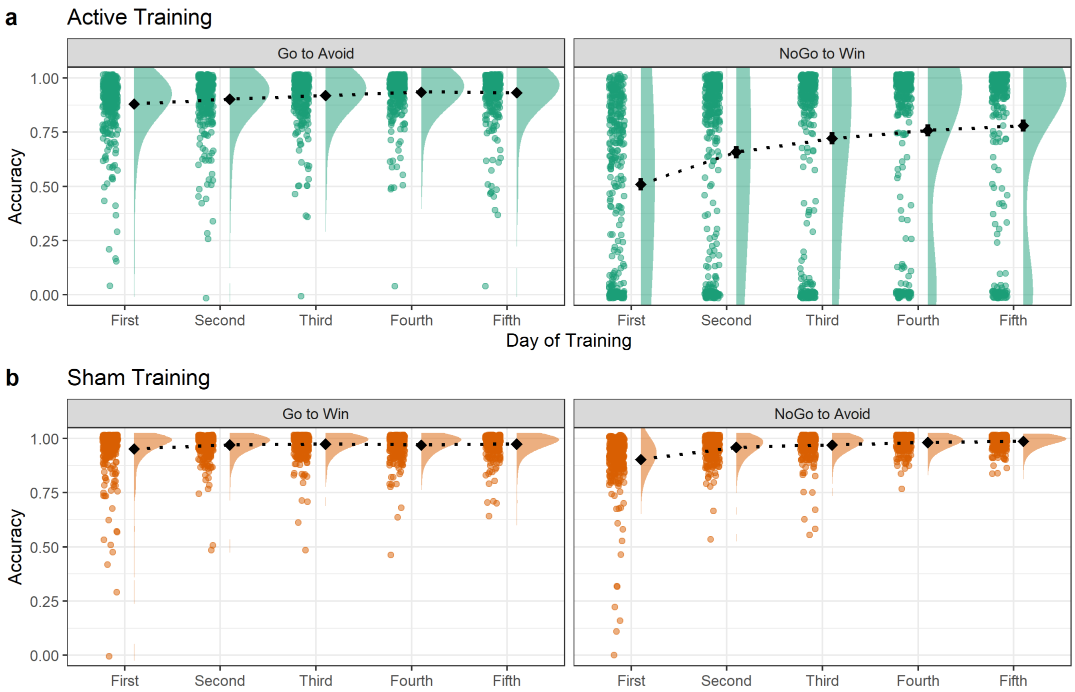


Figure 3.3. Performance on the Pavlovian bias training, split by trial type. Average accuracy in both groups improved over the course of training, but the improvement was greater in the active compared with the sham group. Plots show individual data points and distributions (colour) and means$\pm$SE (black).

3.4.3 Computational modelling of the Orthogonal Go/No-Go Task

3.4.3.1 Preregistered analysis

Next we fitted a number of computational models to the Orthogonal Go/No-Go Task data and examined the results. The models themselves were described in Section 3.3.5.2 above.

First the models were compared on the basis of their WAIC values. Figure 3.4 shows the estimated difference in WAIC (and standard error of this difference) between the best performing model and each model in turn. We found that the best model constituted the Base model plus two learning rates (for reward and punishment); the distance to the second best model is 3.8 times its standard error, so we can be reasonably confident that this model has better out-of-sample predictive accuracy than the other models considered.

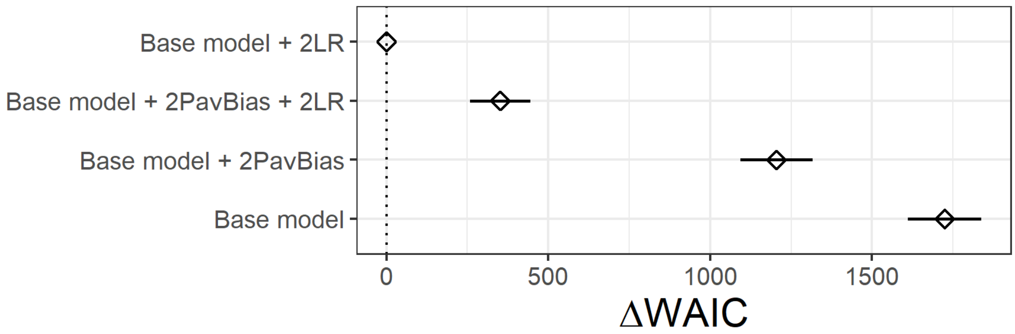


Figure 3.4. Model comparison results for the Orthogonal Go/No-Go Task. Plots show the difference in WAIC (and SE of this difference) between the best performing model, indicated by the vertical dotted line, and each of the models in turn. The best model by nearly four standard errors of difference constituted the Base model plus two learning rates (for reward and punishment).

Next we examined the trial-wise posterior predictions from the winning model. These are plotted in Figure 3.5, along with the empirical data for comparison. We see that the model generates predictions that are mostly well matched to the empirical data, including the greater variability in participants' performance on the no-go to win reward trials (to compare with the distributions of mean accuracy in Figure 3.1). However, it also seems that the model overstates slightly the accuracy in the high Pavlovian conflict trials – in the go to avoid punishment trials in particular, the predicted mean is consistently above the observed mean. This indicates that the model may have underestimated the strength of the Pavlovian biases slightly.

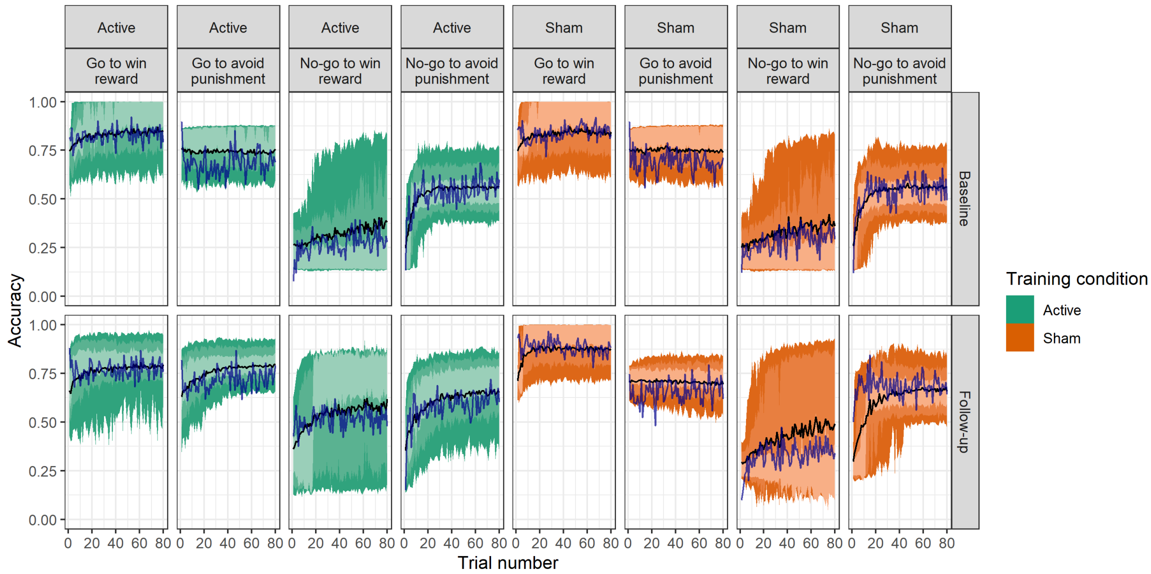


Figure 3.5. Posterior predictions from the winning model (Base model plus 2 learning rates). Plots show the mean (black line) and 50/80/95% highest density continuous intervals (HDCI) for the posterior predictions, as well as the empirical data (blue line) for comparison.

The final stage in our analysis of the model was to examine the posterior estimates of the parameters. First we conducted our preregistered analysis, which involved testing the difference between groups in the mean change of the participant-level Pavlovian bias parameters. These are plotted in Figure 3.6. As predicted, we found there were large differences between groups in the effect of training on Pavlovian bias, $t(552) = 41.9$, $p < .001$, $d = 3.19$. Specifically, those who received the active training showed a substantial decrease in Pavlovian bias ($M = 0.85$, $SD = 0.33$; $t(344) = 48.6$, $p < .001$, $d = 2.62$) whereas those in the sham group did not ($M = 0.00$, $SD = 0.19$; $t(344) = 0.33$, $p = .74$, $d = 0.02$).

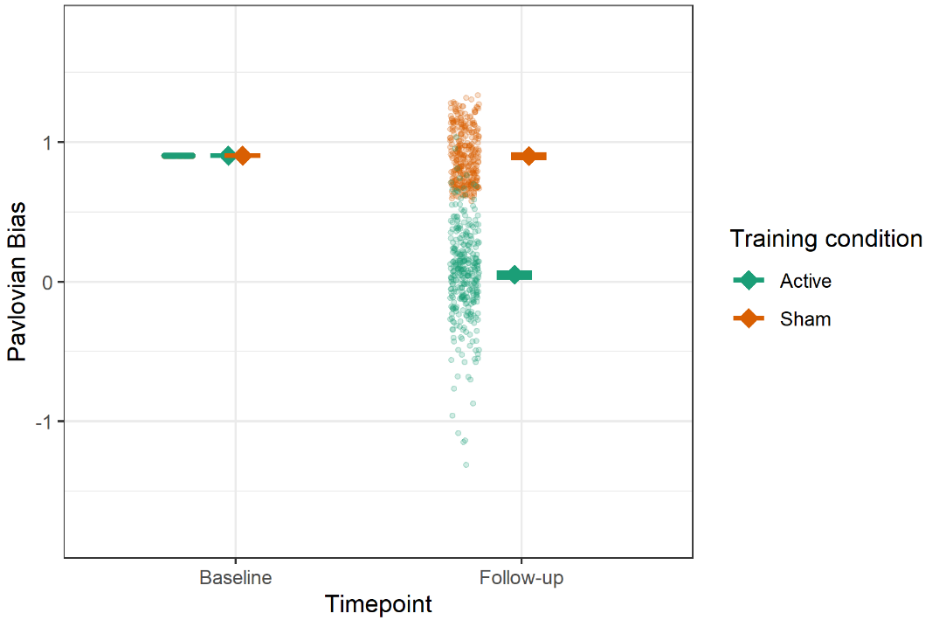


Figure 3.6. Participant-level estimates of the Pavlovian bias parameters, according to the winning model (Base plus two learning rates). The reduction in Pavlovian bias was significantly greater in the active training group compared with the sham group. Each plot shows the mean bias for each participant (i.e. averaging across samples) and the overall mean±SE.

3.4.3.2 Exploratory computational modelling

We then examined the posterior distributions of the population-level parameters. These are plotted in Figure 3.7a, and appear to show substantial training effects in every parameter. To test this more rigorously we computed the changes between sessions for each parameter, and also the differences in these changes between the active and sham groups, allowing us to infer whether any training effects present differed between groups. These are also plotted in Figures 3.7b and 3.7c respectively.

In line with our hypotheses, we found that the Pavlovian bias parameter decreased substantially after training in the active group only – from a value of approximately 0.82 at Baseline it was reduced to nearly zero at Follow-up. In the sham group the

95% HDCI of the change before and after training overlaps zero, suggesting Pavlovian biases remained the same in this group.

The go bias decreased fairly substantially after training in both the sham and active groups, from log-odds of approximately 1.8 at Baseline to 1.25 and 1.1 respectively (see Figure 3.7a; these changes imply a decrease in the probability of making a go response from 86% to 78% and 75% respectively after training). Figure 3.7b confirms that the go bias decreased by more in the active group, as the 95% HDCI for the difference in the two changes does not overlap zero.

There was a substantial difference between groups in the change in the noise parameter – in Figure 3.7a we see that noise decreased after training in the sham group and increased in the active group. This difference is confirmed by Figure 3.7b, which shows a clear, positive difference between the two groups. This may reflect the fact that not all participants in the active group responded to the training, which the model accommodated by increasing the noise.

In the remaining parameters—reward sensitivity, punishment sensitivity, reward learning rate and punishment learning rate—there were varying patterns of differences between the active and sham groups. The sham group consistently showed slight increases after training in all four of these parameters (note that in Figure 3.7a the 95% HDCIs for the changes in punishment sensitivity and reward learning rate do not overlap zero). In contrast, the active group showed substantial decreases in reward sensitivity and punishment learning rates, and increases in punishment sensitivity and reward learning rates. The plots in Figure 3.7b confirm that the two groups differed in the changes in all four of these parameters.

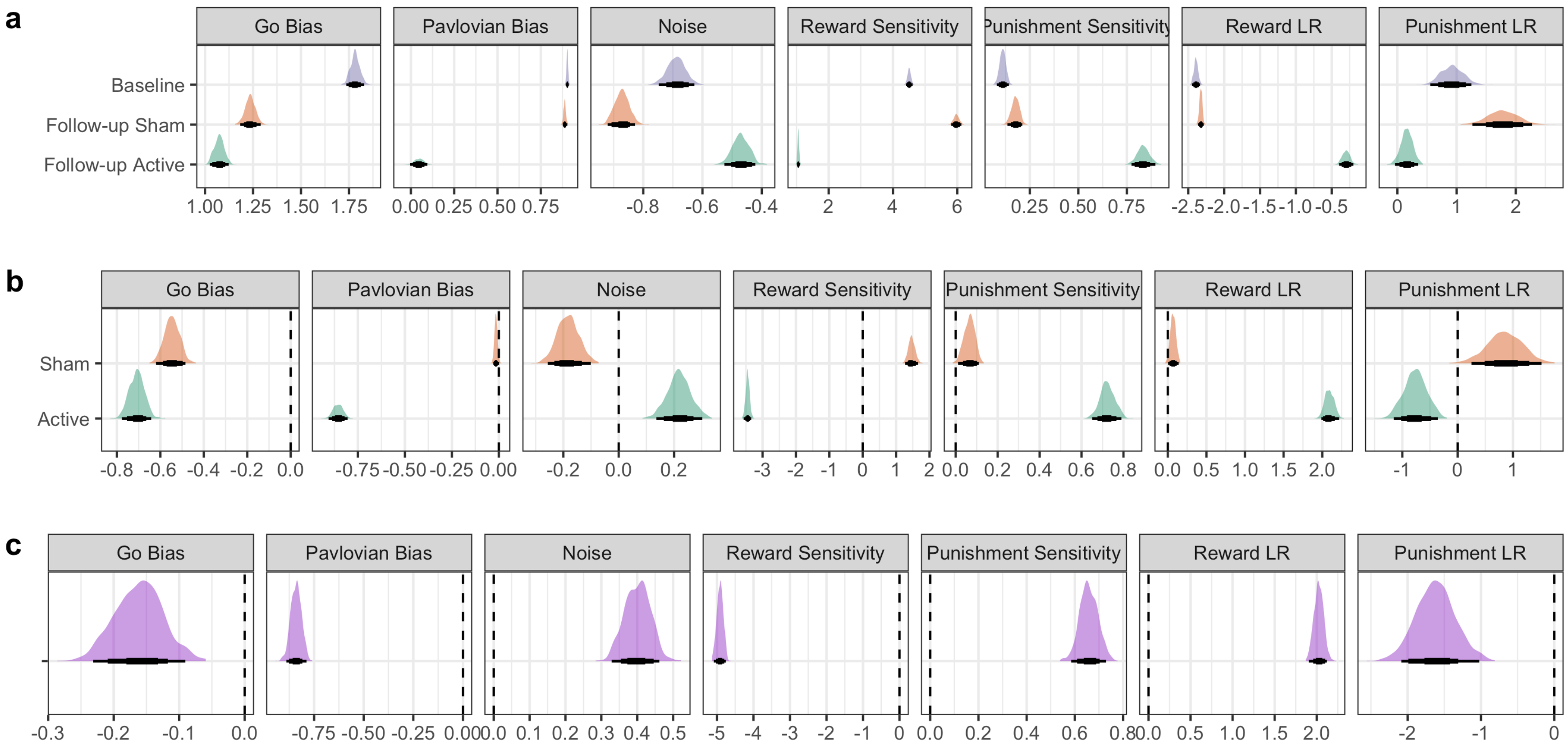


Figure 3.7. Posterior estimates of the population-level parameters according to the winning model (Base plus two learning rates). Each plot shows the posterior distribution and the 50/80/95% highest density continuous intervals (black lines) for the parameter shown. (a) The distributions of the parameters themselves; (b) Distributions of the change (Follow-up minus Baseline) within each group; (c) Distributions of the difference in the change (active group minus sham).

Interpreting these changes depends on first understanding the pattern of these parameters at Baseline: initially we see that reward sensitivity is relatively high and reward learning rate low; whereas punishment sensitivity is lower and punishment learning rate high. Together these values produce the slow, steady increase in accuracy seen in the no-go to win reward condition and the faster learning but low asymptote seen in the no-go to avoid punishment condition (see Figure 3.5; the same effects are less obvious in the go conditions because accuracy there is already saturated by the go bias). As a result of the training, the active group's accuracy in the no-go to win reward and no-go to avoid punishment conditions now follow much more similar trajectories, so the parameters in the model have shifted accordingly, such that the reward and punishment sensitivities and learning rates are now much closer to one another. In the sham condition, on the other hand, there was no obvious training effect in the observed data other than generally slightly improved performance across the board, and so the sensitivity and learning rate parameters have simply shifted upwards slightly.

3.4.4 Affective Bias Task

3.4.4.1 Preregistered analysis

There was no significant difference in the change in affective bias between the two training groups, $t(683) = 0.11$, $p = .91$. The affective bias in each of the conditions is plotted in Figure 3.8.

3.4.4.2 Exploratory analyses

A 2 X 2 (training condition x timepoint) ANOVA was conducted on the affective bias scores, and revealed a significant main effect of timepoint only, $F(1, 688) = 9.90$, $p = .002$, $\eta^2_{partial} = 0.01$. In the baseline session, participants on average showed a negative affective bias (the proportion of responses that equated the ambiguous stimulus to the high-reward exemplar was 0.39, *SD* = 0.16) but after training this bias became less negative (the proportion of 'high' responses increased to 0.41, *SD* = 0.19). The other effects—the main effect of training condition and the condition X timepoint interaction—were both non-significant, p = 0.74 and 0.91 respectively.

Finally we also examined the associations between affective bias (averaged across the two sessions) and scores on each of the mental health symptom scales. None of these correlations were significant (BDI: p = .33; STAI-state: p = .71; STAI-trait: p = .35).

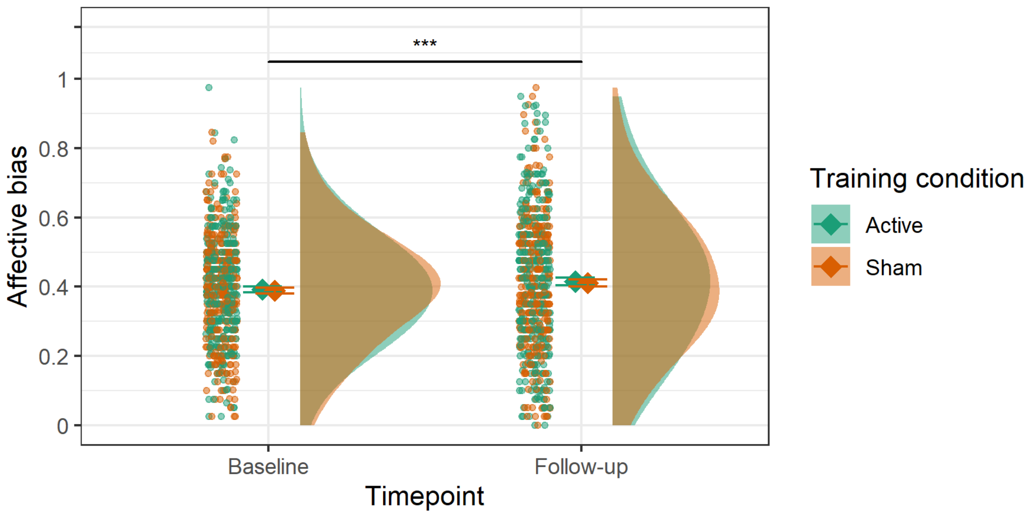


Figure 3.8. Affective bias before and after training. Affective bias is measured by the proportion of responses matching the ambiguous stimuli to the high-reward exemplar (a value of 0.5 is neutral, <0.5 is a negative bias and >0.5 is a positive bias). There was a significant main effect of timepoint only – the negative affective bias decreased significantly (moved closer to 0.5) after training. Plot shows individual data points (left), mean±SE (centre) and distributions (right).

3.4.5 Risk Taking Task

The proportion of gambles chosen in each condition is plotted in Figure 3.9. There was a significant interaction between gamble frame and timepoint, $F(2, 1376) = 10.6$, $p < .001$, $\eta^2_{partial} = 0.02$, with the proportion of gambles chosen reducing after training in the mixed and loss gamble frames only, $t(689) = 2.55$, $p = .01$, $d = 0.1$, and $t(689) = 5.84$, $p < .001$, $d = 0.22$ respectively; in the gain frame there was no change in gambling rates between the Baseline and Follow-Up sessions, $t(689) = 0.12$, $p = .91$. Full descriptive statistics are given in Table 3.4.

In addition there was a significant main effect of framing, $F(2, 1376) = 1030$, $p < .001$, $\eta^2_{partial} = 0.60$; participants chose to gamble significantly more often during the gain ($M = 0.69$, $SD = 0.24$) versus the mixed frame trials ($M = 0.48$, $SD = 0.24$), $t(1379) = 31.2$, $p < .001$, $d = 0.84$, which in turn was significantly more often than in the loss frame trials ($M = 0.25$, $SD = 0.23$), $t(1379) = 32.9$, $p < .001$, $d = 0.89$. Finally, there was also a significant main effect of timepoint, $F(1, 688) = 16.2$, $p < .001$, $\eta^2_{partial} = 0.02$, with the overall proportion of gambles chosen decreasing from 0.48 ($SD = 0.28$) at Baseline to 0.46 ($SD = 0.32$) at Follow-Up.

The remaining effects – the main effect of training group, the interactions between training group and timepoint, training group and framing, and between training group, timepoint and framing – were all non-significant (p = .82, .87, .52 and .70 respectively).

In a further analysis, we also examined the correlation at Baseline between the rates of gambling in the gain and loss frames and the Pavlovian biases. There was a significant correlation between Pavlovian bias and gambling in the loss frame, $r = 0.08$, $t(688) = 2.15$, $p = .03$, but not between bias and gambling in the gain frame, $r = 0.01$, $t(688) = 0.22$, $p = .82$.

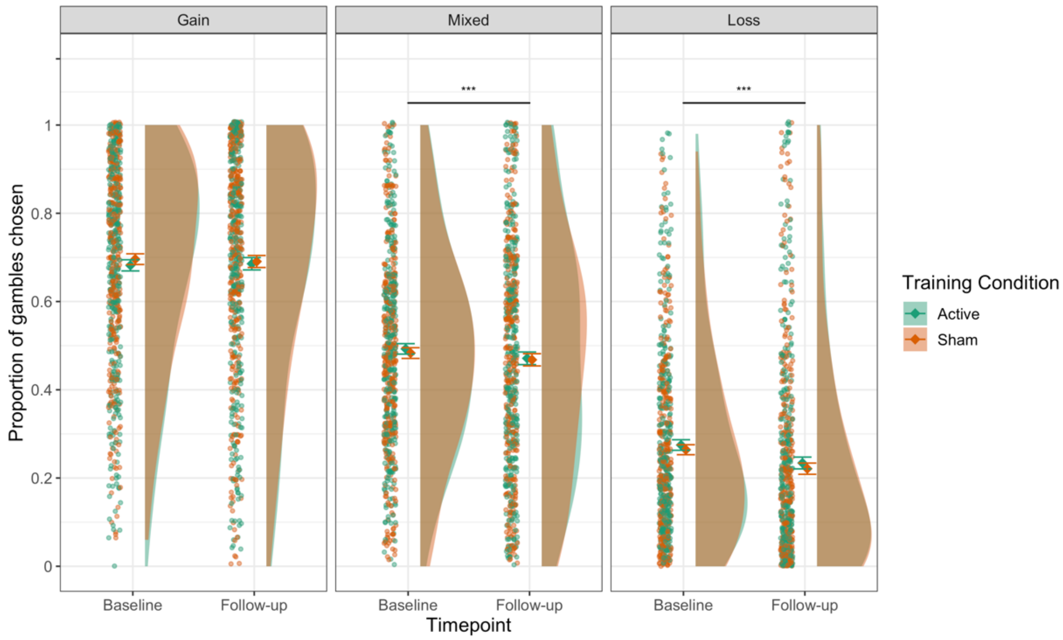


Figure 3.9. Risk Taking Task: Proportions of gambles chosen. Participants chose to gamble less often in the Follow-up session, but only in the mixed and loss frames. In addition, overall the rates of gambling were higher in the gain frame compared with the mixed frame, which in turn was higher than in the loss frame. Plots show (left to right) individual data points, mean±SE and distributions. *** $p < .001$

Table 3.4. Risk Taking Task: Proportion of gambles chosen in each combination of framing and timepoint.

Gamble framing	**Timepoint**	**Proportion of gambles chosen Mean (*SD*)**
Gain	Baseline	0.69 (0.23)
	Follow-Up	0.69 (0.26)
Mixed	Baseline	0.49 (0.22)
	Follow-Up	0.47 (0.26)
Loss	Baseline	0.27 (0.22)
	Follow-Up	0.23 (0.24)

3.4.6 BDI

There was a significant main effect of timepoint, $F(1, 687) = 5.32$, $p = .02$, $\eta^2_{partial} = 0.01$; average depression score decreased from 9.07 (*SD* = 8.19) to 8.69 (*SD* = 8.44) between the Baseline and Follow-Up sessions. However, the main effect of training condition and the training condition x timepoint interaction were both non-significant, *p* = .78 and .42 respectively. The BDI scores in each condition are plotted in Figure 3.10.

We also investigated whether there was any correlation between the change in BDI score and the change in the model-derived Pavlovian bias parameter. This was, however, non-significant, $r = -0.02$, $t(688) = 0.41$, $p = .68$.

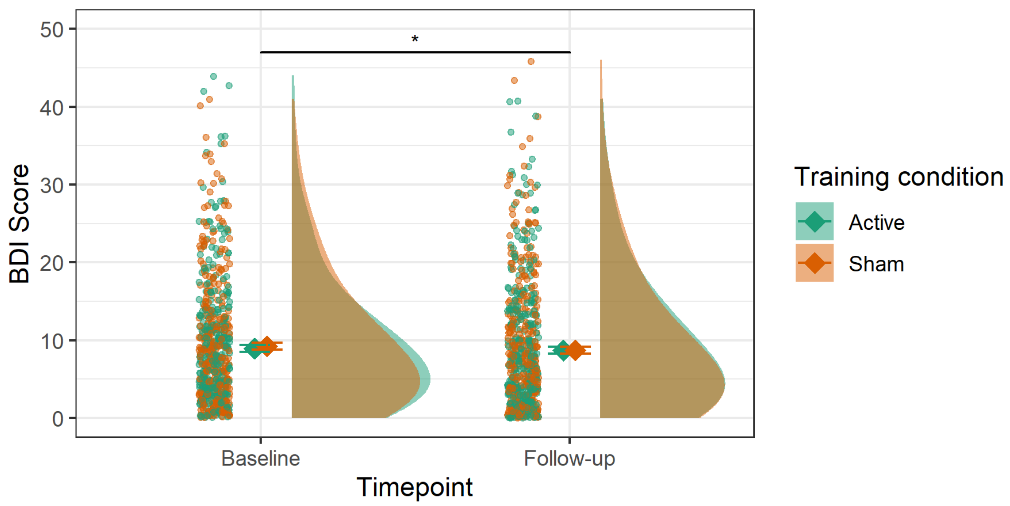


Figure 3.10. Beck Depression Inventory scores. Scores decreased significantly from Baseline to Follow-up, for both training groups. Plot shows (left to right) individual data points, mean$\pm$SE and distributions. * $p < .05$

3.4.7 STAI

There were no significant effects on either the state or trait subscales of the STAI. For the state subscale, the results were: training group, $F(1, 688) = 0.04$, $p = .84$; timepoint, $F(1, 688) = 0.35$, $p = .56$; timepoint x group interaction, $F(1, 688) = 0.26$, $p = .26$. For the trait subscale, the results were: training group, $F(1, 688) = 0.40$,

p = .53; timepoint, $F(1, 688) = 1.85$, p = .17; timepoint x group interaction, $F(1, 688) = 0.56$, p = .46. The STAI scores in each condition are plotted in Figure 3.11.

We also investigated the correlations between the change in the STAI scores and the change in the model-derived Pavlovian bias parameters. However, these were both non-significant: for state anxiety, $r = 0.02$, $t(688) = 0.58$, p = .56; and for trait anxiety, $r = –0.02$, $t(688) = 0.40$, p = .69.

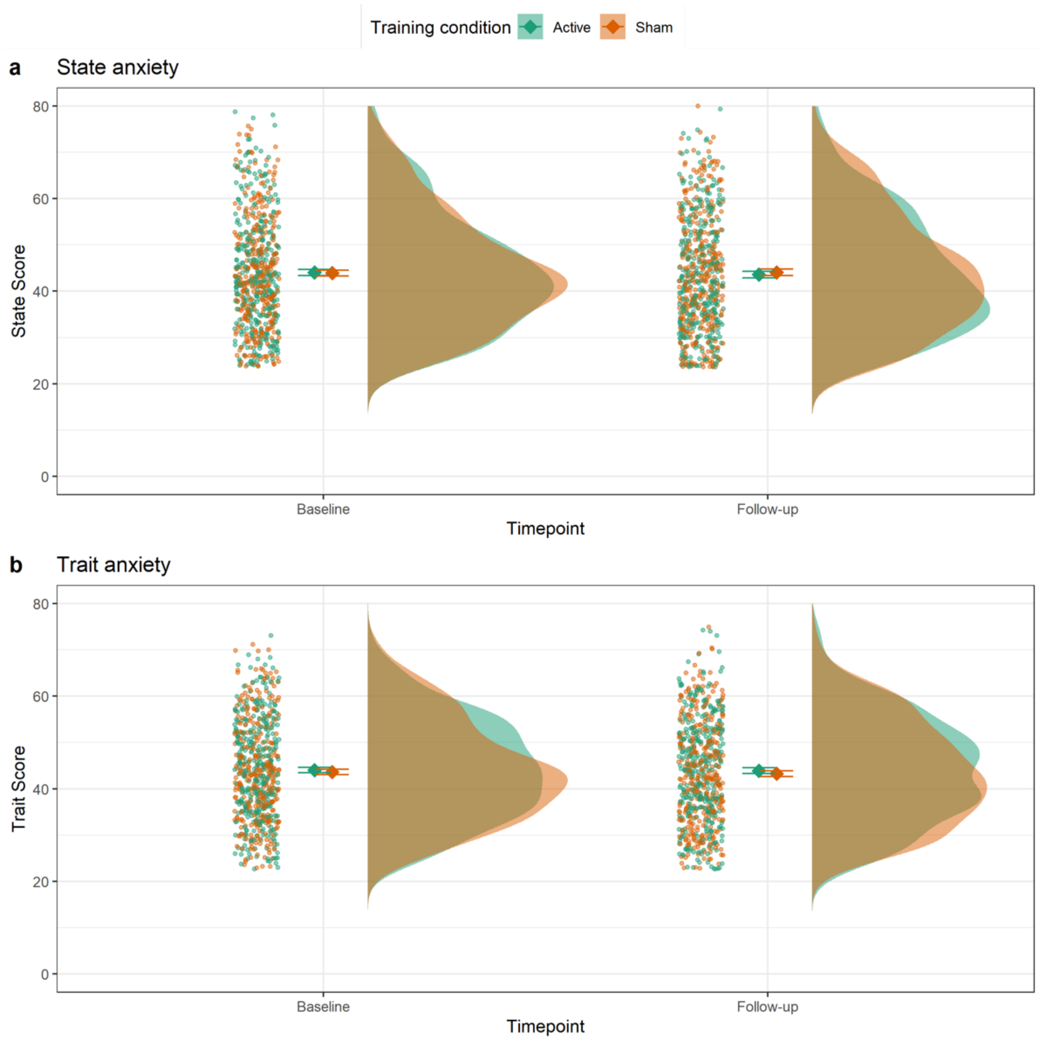


Figure 3.11. State-Trait Anxiety Inventory scores. There were no differences between either timepoints or training groups. Plots show (left to right) individual data points, mean±SE and distributions.

3.5 Discussion

In this preregistered study we examined whether control over Pavlovian biases could be enhanced through a regime of behavioural training. Once a day for five days, participants in the active training group practiced the high Pavlovian conflict trials of the Orthogonal Go/No-Go Task, while those in the sham training group practiced the low conflict trials. We found strong converging evidence from both model-agnostic measures and computational modelling that, compared with the sham training group, participants in the active group had reduced Pavlovian biases. This indicates not just that the training had worked as predicted but also, more broadly, that Pavlovian biases are subject to cognitive control and that the decision to engage control is to some extent malleable and able to be trained.

These results are tempered by the finding that there were no significant differences between training groups in any of our secondary measures. This suggests that the apparent enhancement of cognitive control seen in the Go/No-Go Task was not transferred to other tasks or contexts. Nevertheless, in exploratory analyses we did observe several significant effects common to both groups: there was a significant overall reduction in both negative affective bias and depression symptoms between timepoints, the latter matching a result seen in the previous study (Chapter 2); there was also a significant difference in risk-taking as a function of both timepoint and gamble framing. This reproduces the main result seen previously with this task (Rutledge et al., 2015) and demonstrates that the task manipulation itself worked as intended, although it was not sensitive to the training group participants were in.

The training effect we observed in the Orthogonal Go/No-Go Task was not just significant but also large (for the model-agnostic between-groups comparison it was d = 0.91, while the computational model estimated that the value of the Pavlovian bias parameter had been reduced all the way to zero), suggesting the active training was extremely effective. Reinforcing this, we observed that while both groups improved in performance over the course of the training sessions themselves, the active group improved more consistently and by a greater amount.

These results are of course difficult to integrate with those of the previous chapter, in which it seemed the training effect was either absent or relatively small. For the most part the two versions of the experiment (lab-based and online) were identical. However, there were several important differences which may have caused the discrepancy between the studies. First, the present study was conducted entirely online, and so it could be that the population we sampled from were different in some important way from those recruited for the lab-based study – they may have been more diligent in doing the tasks, for example. In support of this we note that in the previous iteration approximately 15% of participants had to be excluded for not doing all of the sessions, while in the current experiment this figure was just 5%. Second, in the present study we added a comprehension check before the start of the main phase of the Go/No-Go Task, as well as a number of attention checks elsewhere – this improved the quality of the data in this version of the experiment by allowing us to filter out participants who did not understand the instructions or, worse, responded at random. Finally, the current study had a much larger sample size. Although in the previous chapter's discussion we noted concerns about the possibility that the earlier study was underpowered, that was not the case with the current study. Thus, overall, we suggest that we can be more confident in the results of the later version of the experiment, and specifically in the conclusion that the training was effective.

3.5.1. Go/No-Go modelling: further interpretation and limitations

There were several aspects of the computational modelling which were unanticipated or otherwise worthy of more substantial discussion. First, it was surprising that the best performing model was found to be the 'Base plus two learning rates' model, as a previous study with the same task had found that the best performing model included separate Pavlovian approach/avoidance biases as well (Mkrtchian, Aylward et al., 2017). There are three main differences between that study and our own which may explain this discrepancy. The choice of model comparison measure could have been a factor – Mkrtchian, Aylward et al. compared their models using the Bayesian Information Criterion, which is known to penalise model flexibility less than WAIC (McElreath, 2016). Second, Mkrtchian, Aylward et al.'s experiment included a state anxiety manipulation (threat of electric shock), which may have potentiated the avoidance bias in particular; in addition, they included both healthy participants and patients with anxiety disorders in their study, and subsequently found that the patients had increased Pavlovian avoidance, but not approach, biases. Thus it may be that the need to distinguish between the approach and avoidance forms of Pavlovian bias reflects a particular feature of anxiety, and in healthy participants the difference between these two biases is not so critical.

In any case we should be careful not to overinterpret the results of the model comparison, particularly when making inferences about the underlying cognitive mechanism. Although we found here that WAIC clearly favoured the Base plus two learning rates model, this does not necessarily mean that the approach and avoidance biases are processed identically in the brain (see e.g. Guitart Masip et al., 2011, and Boureau & Dayan, 2011) – only that it was not necessary in this case to model them separately in order to make accurate predictions.

Regarding the posterior predictive plot (Figure 3.4), we noted in the results section above that the model has mostly captured the empirical data well, but seems to have slightly overestimated accuracy in the high Pavlovian conflict (go to avoid punishment and no-go to win reward) trial types at Baseline. It is difficult to attribute this to any single feature of the model because the parameters interact in complex ways; however one important aspect especially for the go to avoid punishment condition may be the contribution of the go bias. The population-level go bias of 1.8, together with the noise value of –0.7, translates to a 77% probability of making a go response (see calculation in section 3.6.2), which incidentally is the same as the predicted accuracy in this condition. This suggests that the model may be accounting for performance in this condition using just the go bias and noise parameters alone, i.e. without allowing instrumental learning or Pavlovian biases to have any significant influence. Whatever the precise reason for this, if the model overestimated the accuracy for these high Pavlovian conflict trial types then it may in turn have underestimated both the strength of Pavlovian bias at Baseline and therefore also the size of the training effect. While this has not impeded our ability to detect the training effect in this particular study, it does mean we need to be cautious about the precise size of the training effect obtained from the model.

In addition, the model highlights an interesting feature of the data for the no-go to win reward condition. At Baseline, in the observed data and the model predictions, there is both a modal trajectory which is steady at 15-20% accuracy and a long tail extending to higher levels of accuracy over time. This suggests that most participants were simply not able to learn this condition at all, but a minority of them did learn and improved over the course of the session. At Follow-Up, on the other hand, in the active group the model assigns most of its probability density to a trajectory that starts at approximately 25% and then after 20 trials shows a rapid phase of learning, resulting in an accuracy of around 75% for the remainder of the session. Re-examining the observed accuracy distribution for this condition in Figure 3.1, we see that there is both a main peak at around 70% accuracy and a smaller peak at 15%. This bimodality was perhaps too small to detect when looking at Figure 3.1 by itself, but taken together with the results from the model suggests there may be two distinct subgroups of participants within the active group: a majority who responded to the training and so showed rapid learning in the Follow-up session, and a minority who did not improve at all.

Given that our model was not written to distinguish explicitly between responders and non-responders, it is encouraging that it was still able to detect and highlight this pattern of effects. Nevertheless an important consequence is that the population-level training effect estimated by our model is the mean of a bimodal distribution and does not fully characterise either subgroup of participants. For our purposes this is not a problem as we are primarily interested in the average effect of the training, but in the future it may be interesting to consider models in which this bimodality is represented in the structure of the model, for example by including another level of hierarchy, or adding a mixture component. Not only would this permit better measurement of the training effect within each subgroup, but it would also allow estimation of the proportion of participants who respond to the training.

3.5.2. Secondary measures: the lack of transfer effect

Although it is widely acknowledged that it is often difficult to elicit transfer effects with cognitive or behavioural interventions, it is nevertheless remarkable that there was no effect of training allocation on any of the secondary measures here, given the size of the effect seen on the Go/No-Go task. In the introduction to the previous chapter, we suggested that control over Pavlovian biases could be located within a broader framework of economic decision-making; that the exertion of control depends on an assessment of the possible rewards and costs, as well as the efficacy of control, all of which has to be learned (Shenhav et al., 2013). The finding that we were able to train control within the Go/No-Go task itself but that this did not transfer to the other tasks suggests that whatever participants learned over the course of the training was stimulus specific. In other words, rather than improving their ability to detect and manage cognitive biases in general, participants instead learned to identify and respond to specific stimuli by exerting greater control. This of course brings into focus a broader question as to what extent the decision to exert control is explained by stable, trait-like factors, or by the stimuli themselves and one’s beliefs about them.

We would emphasise, however, that our primary aim with this study was proof of concept, as a result of which we opted to train and test participants on the same set of stimuli, with just one stimulus per trial type. Having shown that this is in principle possible, it may be worthwhile to investigate in a future study whether a modified training regime, such as one in which transfer to different stimuli and contexts is deliberately emphasised, is more effective.

3.5.3. Secondary measures: other effects

Although there were no significant effects of the training groups on the secondary measures, we did observe a number of significant effects of timepoint: affective bias decreased between the Baseline and Follow-Up sessions, in the Risk Taking Task there was a significant reduction in gambling (specifically in the mixed and loss frames), and there was also a decrease in depression symptoms, the latter reproducing a result we saw in the earlier study (Chapter 2). It is possible these results represent a general effect of engaging in a programme of training, perhaps because there is an inherent reward in diligently sticking to a task and improving over time. Equally likely is that they were driven by some aspect of repeat testing – for example, on the Affective Bias task participants may have realised by the Follow-Up session that the intermediate circle was in fact exactly halfway between the two exemplars, or on the BDI participants' responses may have been affected by the fact that they were closer to receiving the reward for the study.

The final effect observed in these secondary measures was the main effect of framing on the Risk Taking Task, which was a relatively large effect at $\eta^2_{partial}$ = 0.60. It has previously been established that this is a result of Pavlovian approach and avoidance biases operating in the gain and loss frames respectively (Rutledge et al., 2015), and it is encouraging that we were able to reproduce this result.

3.5.4 Future directions

We have already highlighted above the difficulties with achieving transfer effects to other cognitive tasks or domains, and consequently the need in future studies for the training programme to explicitly incorporate different stimuli and contexts. This will be particularly important if, as we hope, behavioural training of cognitive control might eventually be developed into a treatment for cognitive symptoms in conditions like anxiety and depression.

The issue of transfer to other tasks raises another question, however, which is whether participants really learned to enhance their control over Pavlovian biases, or whether instead they learned something more specific about the particular stimuli they trained and were tested on. Although we have framed our arguments, in this and the previous chapter, in terms of cognitive control over Pavlovian biases, there is a need for more research to examine the relationship between these two processes directly. In the remaining empirical chapters of this thesis we will explore this line of study, asking to what extent differences in willingness to exert control explain the strength of Pavlovian biases. We hope to show that the ability to overcome Pavlovian biases is critically dependent on the extent to which one is sensitive to cognitive effort. Since effort is by definition a flexible resource it would seem to be a likely mediator of the training effect we found in the current study. Finding a relationship between Pavlovian bias and sensitivity to effort would thus allow us to interpret the results in this chapter with greater certainty. In order to investigate this link, we first needed to design a suitable measure of cognitive effort sensitivity, which is the subject of the following chapter.

3.5.5 Conclusions

In this study we have shown that it is possible to reduce the influence of Pavlovian biases on behaviour through a regime of behavioural training. This is consistent with the idea that there is a cognitive control mechanism operating on Pavlovian biases, which was enhanced by the training. Moreover the training could have important clinical applications with the aim of enhancing cognitive control, if developed further. While this has to be tempered by recognition of the fact that we did not see transfer of enhanced cognitive control to the other secondary measures in this study, it is nevertheless noteworthy that we were able to achieve changes in the influence of Pavlovian biases at all. With modifications to the training to focus on the issue of transfer to other stimuli and contexts, future studies may be able to progress this method even further. In addition, more work is needed to directly investigate the links between the strength of Pavlovian biases and participants' willingness to exert control – this question is addressed by the remaining two empirical chapters of this thesis.

3.6 Appendix

3.6.1. Prior distributions for the Go/No-Go Models

Figure S3.1. Prior predictions for the go bias parameters, with distributions $\mu_{GoBias} \sim Normal(0,1.5)$, $\sigma_{GoBias} \sim Exponential(0,1.5)$ and $GoBias_{subject} \sim Normal(\mu_{GoBias}, \sigma_{GoBias})$. (a) and (b) show the analytical distributions of the population mean and standard deviation; (c) shows the prior prediction for the participant-level go bias (log-odds), and (d) the implied go bias probability.

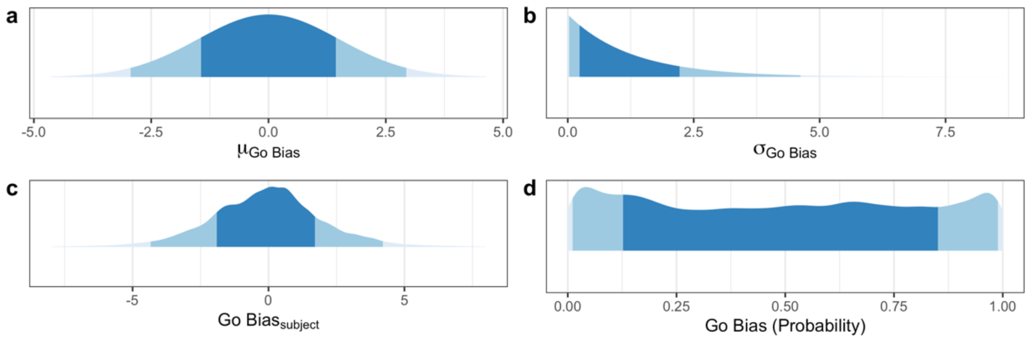


Figure S3.2. Prior predictions for the Pavlovian bias parameters, with distributions $\mu_{PavBias} \sim Normal(0,2)$, $\sigma_{PavBias} \sim Exponential(0.5)$ and $PavBias_{subject} \sim Normal(\mu_{PavBias}, \sigma_{PavBias})$. (a) and (b) show the analytical distributions of the population mean and standard deviation; (c) shows the prior prediction for the participant-level Pavlovian bias (dimensionless).

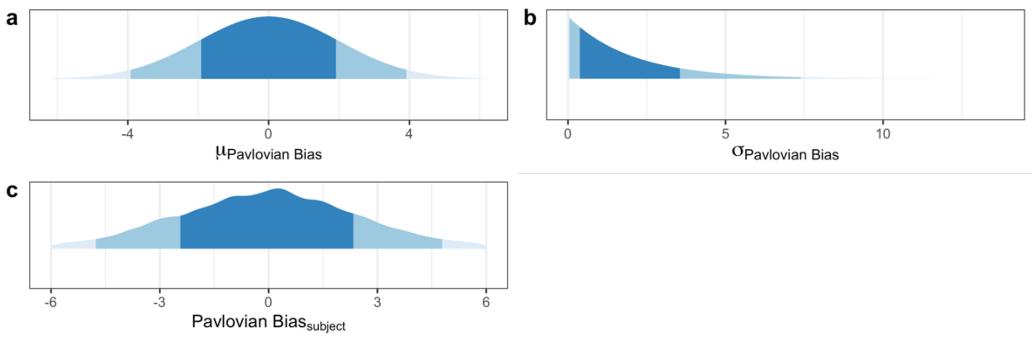


Figure S3.3. Prior predictions for the noise parameters, with distributions $\mu_\xi \sim Normal(0,0.5)$, $\sigma_\xi \sim Exponential(1)$ and $\xi_{subject} \sim Normal(\mu_\xi, \sigma_\xi)$. (a) and (b) show the analytical distributions of the population mean and standard deviation; (c) shows the prior prediction for the participant-level noise parameter (in probits), and (d) the implied noise proportion.

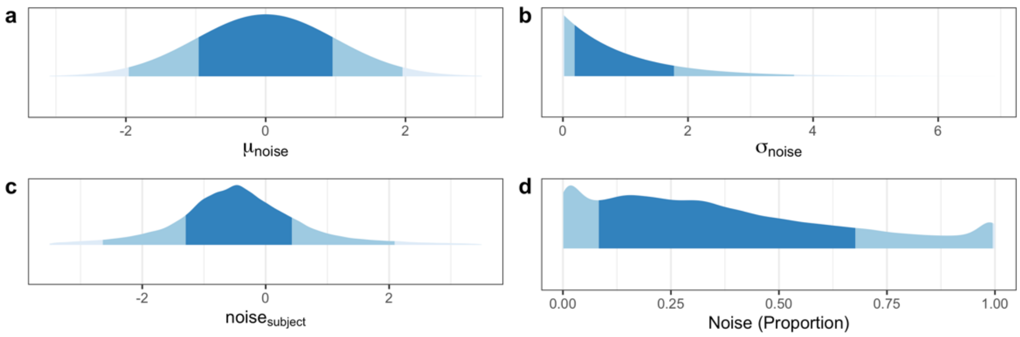


Figure S3.4. Prior predictions for the outcome sensitivity parameters, with distributions $\mu_{sensitivity} \sim Normal(0,0.3)$, $\sigma_{sensitivity} \sim Exponential(1)$ and $sensitivity_{subject} \sim Normal(\mu_{sensitivity}, \sigma_{sensitivity})$. (a) and (b) show the analytical distributions of the population mean and standard deviation; (c) shows the prior prediction for the participant-level outcome sensitivity parameters, and (d) the implied maximum possible instrumental accuracy (asymptote of learning).

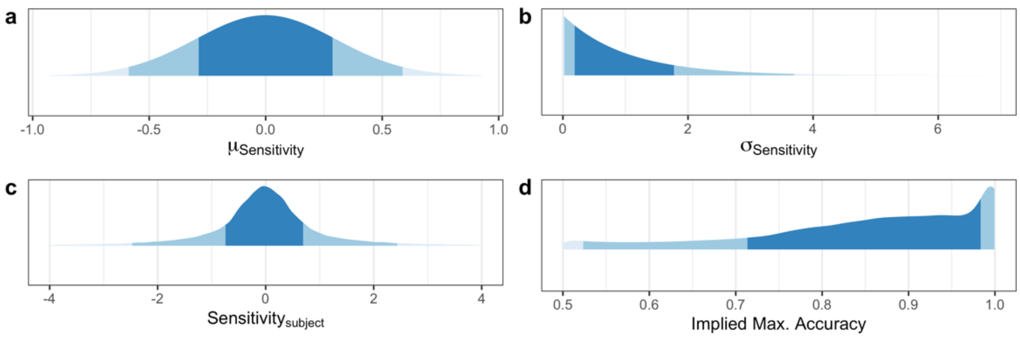
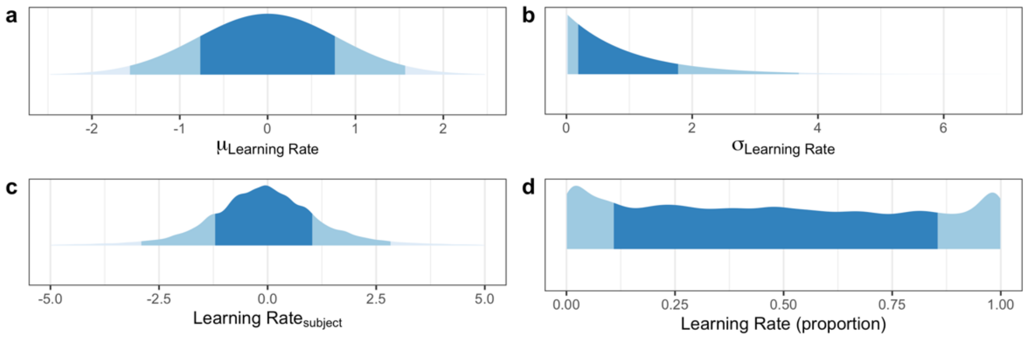


Figure S3.5. Prior predictions for the learning rate parameters, with distributions $\mu_{LR} \sim Normal(0,1)$, $\sigma_{LR} \sim Exponential(1)$ and $LR_{subject} \sim Normal(\mu_{LR}, \sigma_{LR})$. (a) and (b) show the analytical distributions of the population mean and standard deviation; (c) shows the prior prediction for the participant-level learning rate parameters (in probits), and (d) the implied maximum possible learning rate (relative to the outcome sensitivity).



3.6.2. Calculation of the initial Go probability

The population mean Go bias = 1.8 (log-odds). As a probability this equals 0.858.

The population mean noise = – 0.7 (probits). As a probability this equals 0.242.

Overall initial pGo: $0.858 \times (1 - 0.242) + 0.5 \times 0.242 = 0.77$

Chapter 4. Measuring Cognitive Effort Without Difficulty

4.1 Abstract

An important finding in the cognitive effort literature has been that sensitivity to the costs of effort varies between individuals, suggesting that some people find effort more aversive than others. It has been suggested this may explain individual differences in other aspects of cognition. We are particularly interested in a possible link to control over Pavlovian biases. However, there is a significant problem with existing measures of cognitive effort which is impeding this line of research, namely the confounding of effort and difficulty. This means that behaviour thought to reveal effort costs could equally be explained by cognitive capacity, which influences the frequency of success and thereby the chance of obtaining reward. To address this issue we introduce a new test, the Number Switching Task (NST), specially designed such that difficulty will be unaffected by the effort manipulation and can easily be standardised across participants. In a large, online sample we show that these criteria are met successfully and reproduce classic effort discounting results with the NST. We also demonstrate the use of computational modelling with this task, producing behavioural parameters which can then be associated with other measures, and report a preliminary association with the Need for Cognition scale. We believe this task will be an important tool for studying associations between individual differences in effort sensitivity and other cognitive functions in the future.

4.2 Introduction

Cognitive effort, our ability to vary the depth of our engagement with a cognitive task, influences a raft of fundamental cognitive processes including attention (Kahneman, 1973), working memory (Westbrook et al., 2013), cognitive control (Braver, 2012; Shenhav et al., 2013) and cognitive biases in decision-making (Ortega et al., 2015; Toplak et al., 2011). Consequently, there is substantial interest in both measuring cognitive effort and understanding the factors that determine when and how much effort is exerted in different situations. Unfortunately, cognitive effort is challenging to study for the very same reason – because it is so entangled with other processes, there is considerable potential for confounding, and attempts to measure cognitive effort must therefore be careful to isolate effort from other factors that may influence performance.

One particular problem—the conflation of effort and difficulty—has not to our knowledge been addressed. Current methods for studying cognitive effort involve assessing participants' preferences for different cognitive tasks: avoidance of more demanding tasks is interpreted as evidence of underlying effort costs, and these are quantified by examining how participants trade off the demand against rewards (a phenomenon termed effort discounting; see Westbrook & Braver, 2015). However, more demanding tasks may also have lower rates of success, and therefore of obtaining reward, giving rise to another form of discounting (this time by the probability of reward) that would cause avoidance of the more demanding tasks in exactly the same way.

Consider for example the N-back working memory task, which is frequently used in studies of cognitive effort (see e.g. Westbrook, Kester & Braver, 2013). Higher levels of the N-back feel more effortful, but they are also intrinsically more difficult to perform accurately, because with more items to hold in memory, the maximum precision with which each item can be maintained is lower (Bays et al., 2009). If we observe discounting of the value of the task as the N-back level increases, it is impossible to say to what extent this is due to the greater effort required or the lower probability of completing a trial successfully and gaining reward. Similar arguments can be made for other effort manipulations, such as response conflict tasks (e.g. McGuire & Botvinick, 2010; Schmidt et al., 2012). In order to dissociate these processes, it is essential that measures of cognitive effort hold difficulty constant when manipulating task demand.

It is important too that the difficulty of the task can be standardised across participants, as is usual in tasks manipulating physical effort (Chong et al., 2016; Husain & Roiser, 2017). Differences in cognitive abilities (including both cognitive capacity in a general sense and task-specific competencies) mean that the same task may be more or less difficult for different participants. This introduces further potential for confounding and renders comparison between individuals difficult. In order to conduct individual differences research, particularly in conditions such as depression and schizophrenia (in which cognitive impairment is a core symptom; Mesholam-Gately et al., 2009; Rock et al., 2014), we need to ensure that all participants are being tested at the same level of relative difficulty.

4.2.1 A new cognitive effort measure – the Number Switching Task

The purpose of the present study was to develop a task that distinguishes cognitive effort from difficulty. Specifically, we targeted two main criteria: the manipulation of effort demand should not affect the probability of success; and it should be possible to standardise the task difficulty by reference to each participant's baseline ability. Two further considerations were that the task should have several levels of effort demand, so that we can examine parametric responses to the manipulation across a reasonable dynamic range, and also that it should be optimised for use online, where it is possible to obtain much larger sample sizes more practically than through in-person testing.

We developed the Number Switching Task (NST), which involves categorising each digit in a nine-digit sequence as either even or odd. The frequency of switching between odd and even influences the effort level, but should not affect the intrinsic difficulty of the task, because it is only the order, not the content, of the trials that changes. Additionally, we can control the difficulty on the NST by calibrating the time participants have available to complete each sequence, allowing us to standardise the task across participants.

The primary aim of this paper was to validate the NST by testing the prediction that the effort manipulation will elicit the classic effort discounting effect without affecting the difficulty as measured by the rate of success. We also present some secondary analyses including computational modelling and an assessment of preliminary associations with cognitive traits relevant to depression and anhedonia.

4.3 Methods

4.3.1 Preregistration

This study was preregistered on the Open Science Framework (doi:10.17605/OSF.IO/8Y7P9). There were no deviations from this plan.

4.3.2 Participants

Participants were recruited through the online platform Prolific. The study was advertised only to participants who met the following inclusion criteria: aged 18-60, fluent in English, no history of a diagnosed psychiatric or neurological disorder, and did not take part in an earlier study in this series of experiments. Participants also had to use a computer – smartphones or tablets were not allowed.

In our preregistration we calculated a minimum required sample size of 259 participants in order to detect an effect of at least $r = 0.2$ with 90% power and alpha = .05 (two-tailed). To allow for withdrawals and exclusions, we initially recruited a larger sample, of whom 306 completed the whole study. Of these, three were excluded because they refreshed the web page part way through; nine were excluded because they repeatedly failed the familiarisation phase of the effort task; and four were excluded because they failed attention checks in the questionnaires. This left 290 participants with data included in the final analysis.

4.3.3 Procedure

From Prolific, participants were automatically directed to another website, Gorilla (www.gorilla.sc/), where the study was hosted. There they completed the Cognitive Effort Task, followed by eight questionnaires. At the end of the study, they were redirected back to Prolific via a unique URL, which allowed them to prove they had completed all the tasks; if instead they returned to Prolific manually (without this URL), their data was flagged and we checked whether they had actually completed all the tasks or not. On average, the entire study took approximately 45 minutes, from signing up to returning to Prolific, and participants were paid a flat rate of £5 plus a performance bonus of 1 pence per 3 points won on the effort task (on average participants won around £1.50 in bonuses).

4.3.3.1 The Number Switching Task

The structure of the task is shown in Figure 4.1. On each trial, participants were offered a reward (3, 6, 9 or 12 points, corresponding to 1, 2, 3 or 4p of real money, respectively) to complete an effortful task with a specified level of demand. If they accepted this challenge they had to complete the task successfully to win the reward; if they rejected it, they avoided performing the task, but won no points and, after a timeout of 2500ms, proceeded to the next offer.

The effortful task itself was to categorise the digits, in a random sequence of the numbers one to nine, as either odd or even. The subjective effort of this task scales with the frequency of switching between odd and even digits, allowing us to define four levels of demand: the lowest level, referred to in the task as 20%, contained either 1 or 2 switches; the next level (40%) 3 or 4 switches; the 60% level 5 or 6 switches; and the highest level, 80%, had 7 or 8 switches. On any given trial, the precise number of switches was determined at random to prevent the sequences becoming predictable.

Participants responded ‘odd’ or ‘even’ using the ‘f’ and ‘j’ keys (counterbalanced across participants). While the individual categorisations were self-paced, meaning the next digit did not appear on the screen until a response had been made to the current item, there was a time limit for completing the overall sequence. A trial was marked as ‘correct’ only if the sequence was finished within this limit and with no more than one wrong response. ‘Incorrect’ sequences were not rewarded.

Importantly, this allowed us to standardise the difficulty across participants: by calibrating the allowed time based on performance during an earlier familiarisation phase, we ensured that all participants had similar success rates on the task.

Phases of the task

Prior to embarking on the full task, participants progressed through several rounds of instructions and practice, followed by a longer familiarisation phase. This latter phase was important as it was used to calibrate the time limit for the sequences in the main phase of the task. It comprised 32 trials of just the odd/even categorisation task (i.e. without any offers of reward) – four trials of each of the eight possible numbers of switches, in a random order. In this phase there was no time limit, but participants were instructed to respond as quickly as possible while still trying to complete each sequence correctly.

To progress through the familiarisation phase to the main task, participants had to achieve at least 50% correct responses on the most difficult 8-switch trials; if they failed more than 50% of these trials they were given one opportunity to repeat this stage; if they failed again they were excluded.

For participants who passed the familiarisation phase, we calculated their time allowed for the main phase sequences as the median time to complete the hardest, 8-switch trials plus 500ms. During piloting we observed that this provided a good balance between providing sufficient time pressure to elicit the effort effects while ensuring that the task was possible within the maximum completion time for all participants.

Finally, participants completed the main phase of the task, which comprised a total of 80 trials – five trials of each of the 16 possible offer combinations, in a random order.

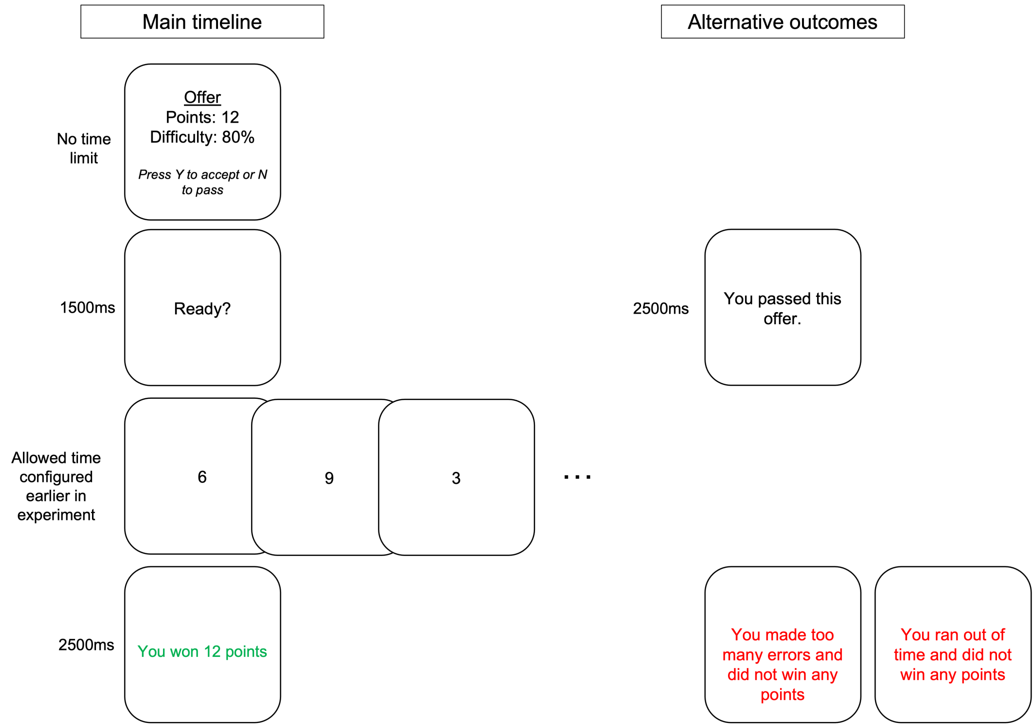


Figure 4.1. Number Switching Task trial structure. Participants chose whether or not to perform an effortful task depending on the points and effort level offered. If they accepted the offer, they were shown a random sequence of the digits 1-9 and had to indicate (by pressing the 'f' or 'j' keys) whether each of the digits was even or odd. Sequences with more frequent switching between odd and even were more effortful. To win the points on offer, participants had to categorise at least 8 of the 9 digits correctly and complete the sequence within the allowed time (which was calibrated to each individual). In the above figure, the 'alternative outcomes' show screens that participants saw if they passed an offer, or if they failed the trial owing to too many errors or timing out.

4.3.3.2 Questionnaire measures

Following the NST, participants completed a number of questionnaires. In all cases, participants gave their responses by moving a horizontal slider (which defaulted to the centre).

NASA Task Load Index. The NASA Task Load Index (Hart & Staveland, 1988), henceforth referred to as the 'Subjective Task Load', assesses subjective workload on six subscales: mental demand, physical demand, temporal demand, performance, effort and frustration. For each subscale and, in our case, for each level of effort, participants were asked to rate their experience of the task on a 21-point scale from 'very low' to 'very high'. The six subscales were presented in the same order as above, within which the questions about the different effort levels were randomised. We report participants' scores on each subscale for each effort level separately.

Cognitive Complaints Inventory. The Cognitive Complaints Inventory (Iverson & Lam, 2013) is a six-item questionnaire in which participants rate their problems with concentration, memory and thinking skills, on a four-point scale from 0 (not at all) to 3 (very much). We report total scores (where higher scores indicate more cognitive complaints).

We appended a catch question ("Select 'very much' for this question") to this questionnaire to identify participants who were not paying attention. We placed this at the end to avoid interfering with the psychometric properties of the questionnaire itself.

Fatigue Severity Scale. The Fatigue Severity Scale (Krupp et al., 1989) is a nine-item questionnaire in which participants rate their experience of fatigue and the impact fatigue has on their daily activities, on a scale from 1 to 7. We report total scores (where higher scores indicate more fatigue).

International Physical Activity Questionnaire Short Form. The International Physical Activity Questionnaire Short Form (IPAQ-SF; Lee et al., 2011) is a seven-item scale that measures self-assessed physical activity over the previous seven days. Participants are asked on how many days and on average for how long each day they spent engaged in vigorous activity, moderate activity, walking and sitting. These estimates are weighted by their estimated metabolic requirements and summed to generate an overall score (termed ‘MET-minutes’, a way of expressing activity relative to a resting metabolic rate), as follows:

- If necessary, bouts of activity are truncated at a maximum of three hours.
- Walking MET-minutes per week = 3.3 * walking minutes * walking days
- Moderate MET-minutes per week = 4 * moderate minutes * moderate days
- Vigorous MET-minutes per week = 8 * vigorous minutes * vigorous days
- Total MET-minutes per week = Walking Met-minutes + Moderate Met-minutes + Vigorous Met-minutes

Need for Cognition Scale (6-item version). The six-item Need for Cognition Scale (Coelho et al., 2018) measures the extent to which participants enjoy engaging in difficult cognitive activity. Participants rate each of six statements from 1 (not characteristic of themselves) to 5 (characteristic). We report participants’ total scores (where higher scores indicate greater enjoyment of cognitively demanding activity).

We added another catch question to the end of this questionnaire.

Temporal Experience of Pleasure Scale. The Temporal Experience of Pleasure Scale (TEPS; Gard et al., 2006) is an 18-item scale with two subscales: a 10-item anticipatory pleasure scale and an 8-item consummatory scale. Each item consists of a statement (e.g. “The smell of freshly cut grass is enjoyable to me”) which participants rate on a 6-point scale from ‘very false for me’ to ‘very true for me’. We report total scores (where higher scores indicate greater disposition to experience of pleasure or, equivalently, lower anhedonia).

Zung Depression Scale. The Zung Depression Scale (Zung, 1965) is a 20-item questionnaire in which participants respond to a series of statements about how they might feel on a 4-point scale from ‘a little of the time’ to ‘most of the time’. We report total scores (where higher scores indicate more depressive symptoms).

4.3.4 Statistical Analyses

4.3.4.1 Preregistered analyses

The main dependent variable on the NST was the proportion of offers accepted for each combination of reward and effort level. We also recorded participants' accuracy and completion times for the odd/even categorisation task – these were of course conditional on participants accepting the offer in the first place and, in the case of the completion times, completing the sequence within the time allowed and with no more than one mistake allowed.

Our primary analysis was a multilevel (mixed effects) ANOVA. This was used because multilevel ANOVAs can accommodate unbalanced designs, which arise in this task because participants could choose to accept or reject trials at will, resulting in secondary measures (success rate and completion time) with different numbers of trials from each participant. These ANOVAs contained fixed effects of reward and effort and their interaction, and varying intercepts across participants.

For analysis of the Subjective Task Load questionnaire, six multilevel ANOVAs were constructed, one for each of the constituent scales of the index, using a fixed effect of effort level and varying intercepts across subjects.

Throughout these analyses, we further investigated any significant effects indicated by the ANOVAs using *post hoc* simple effects ANOVAs and paired-samples *t*-tests as appropriate. Note that, unlike the multilevel ANOVAs, the *t*-tests require complete cases. This results in differing degrees of freedom across analyses, as some participants had to be excluded from specific *post hoc* analyses of success rates or completion times if they had not completed any trials at a particular reward or effort level.

4.3.4.2 Exploratory analyses

Computational Modelling

We considered eight models (listed in Table 4.1), all variations on a logistic regression. The characteristic mathematical form of these models is provided in Equation 4.1:

$$Y_{subject,trial} \sim \text{Bernoulli}(p_{subject,trial})$$
$$p_{subject,trial} = \text{logistic}(\alpha_{subject} + \beta_{reward,subject} reward_{trial} + \beta_{effort,subject} effort_{trial}) \tag{4.1}$$

where $y_{subject,trial} \in \{0,1\}$ is the choice of a particular subject on a particular trial to accept or reject the challenge. The underlying probability of accepting an challenge, $p_{subject,trial}$, is then calculated as a logistic function of a linear combination of a number of parameters, typically including an intercept, α, and one or more effects of reward and effort, β_{reward} and β_{effort} respectively.

Table 4.1. Specification of models fitted to the Number Switching Task.

	Intercept	Linear Reward	Linear Effort	Quadratic Effort
1	✓ (Fixed)	✓ (Fixed)	✓ (Fixed)	
2	✓(Varying)			
3	✓ (Varying)	✓ (Fixed)	✓ (Fixed)	
4	✓ (Varying)	✓ (Varying)	✓ (Varying)	
5	✓ (Fixed)	✓ (Fixed)	✓ (Fixed)	✓ (Fixed)
6	✓ (Varying)	✓ (Fixed)	✓ (Fixed)	✓ (Fixed)
7	✓ (Varying)	✓ (Varying)	✓ (Varying)	✓ (Varying)
8	✓ (Varying)	✓ (Varying)		✓ (Varying)

Equation 4.1 represents Model 4 – all of the other models can be constructed by modifying one or more components of this model. For example, here, the intercept and effects vary across subjects; however, as noted in Table 4.1, in some models these parameters were fixed instead, meaning all subjects took the same value.

The subject-level parameters were all given hierarchical priors which were determined through a process of prior predictive checking. Details are given in the Appendix, Section 4.7.

We standardised the values of the predictors (the reward and effort levels), for computational and arithmetical simplicity. Note that this affects the interpretation of absolute parameter values from the model.

The models were fitted using Markov Chain Monte Carlo sampling in Stan (Stan Development Team, 2021). The model was run across four chains each with 1000 iterations. Subsequent to fitting, we carried out the recommended standard diagnostics (Betancourt, 2018) and found no issues.

Structural Equation Modelling

We used confirmatory factor analysis (CFA) to fit several potential factor structures to the questionnaire data. We identified the best-fitting structure and inserted this into a structural equation model (SEM), with which we sought to predict the behavioural parameters estimated for each subject (intercept, reward and effort sensitivity) from their cognitive trait scores.

4.3.4.3 Computing environment and packages

Analyses were conducted in R version 3.5.3 (R Core Team, 2019). We used the R package 'lme4' (1.1-21; Bates et al., 2015) to fit the multilevel ANOVAs and 'rstatix' (0.6.0; Kassambara, 2020) to conduct the *post hoc* tests. Bayesian models were fitted in Stan using CmdStanR (0.3.0, Gabry & Češnovar, 2021). SEM was conducted in Lavaan (Rosseel, 2012).

4.4 Results

4.4.1 Preregistered Analyses

4.4.1.1 Number Switching Task

4.4.1.1.1 Proportion of Offers Accepted

The proportions of offers accepted at each level of reward and effort are plotted in Figure 4.2. These show a significant reward-by-effort interaction, $F(1, 4347) = 30.8$, $p < .001$, $\eta^2_{partial} = .04$, consistent with participants treating the effort level as an economic cost. Specifically, the value of a reward was progressively discounted as the effort required to obtain it increased, but this discounting was shallower when the reward offered was greater. Despite this flattening as reward increased, the effort effect was still significant at every reward level in *post hoc* ANOVAs (all *p*s < .001). The main effects of reward and effort were also both significant, $F(1,4347) = 108$, $p < .001$, and $F(1,4347) = 84.4$, $p < .001$ respectively. Full descriptive statistics are provided in Table S4.1.

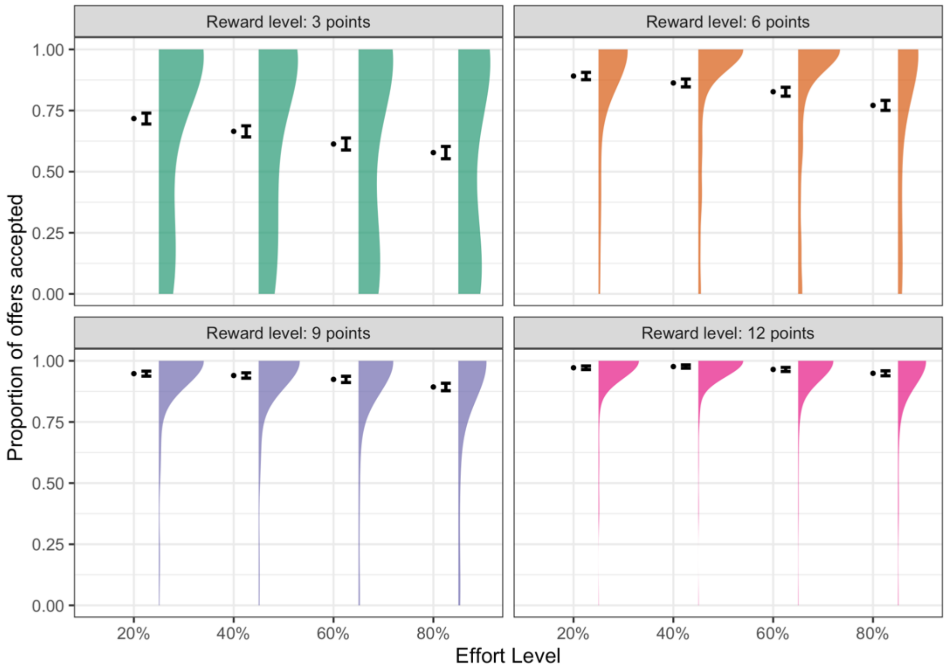


Figure 4.2. Number Switching Task: proportion of offers accepted. Plot shows the mean, standard error and distribution of the proportion of offers accepted for each combination of reward and effort level. There is a clear effort discounting effect which becomes shallower as the reward level increases. Plots show (left to right) mean, SE and distributions.

4.4.1.1.2 Success Rate

The success rate for each level of reward and effort is plotted in Figure 4.3. The only statistically significant effect was that of reward, $F(1, 4024) = 68.1$, $p < .001$, $\eta^2_{partial} = 0.08$, with participants more likely to complete the sequence correctly as the offered reward increased (Table 4.2), consistent with higher rewards being more motivating. *Post hoc t*-tests indicated that this was driven primarily by the increase in success rates between the 3 and 6 point reward levels, $t(272) = 3.01$, $p = .008$, $d = 0.18$, while the differences between 6 and 9 points, and 9 and 12 points did not achieve significance after Bonferonni-adjusting for multiple comparisons (*ps* = .10 and .07, and, *ds* = 0.13 and 0.14, respectively). Full descriptive statistics are provided in Table S4.2.

The effort level had no significant effect on the success rate, $F(1, 4024) = 2.18$, $p = .14$, and the reward-by-effort interaction was also non-significant, $F(1, 4024) = 0.380$, $p = .54$.

Importantly, the success rate varied relatively little across participants (overall mean = 0.90, *SD* = 0.11), suggesting the standardisation of difficulty had been successful.

Table 4.2. Number Switching Task: Descriptive statistics for the proportion of trials completed successfully (across reward levels).

P(Success)		
Reward (points)	**N**	**Mean (*SD*)**
3	273	0.86 (0.18)
6	273	0.89 (0.12)
9	273	0.91 (0.10)
12	273	0.92 (0.10)

Note. To be marked as correct, sequences had to be completed within the time limit and with no more than one error. These data only include complete cases, i.e. where participants attempted at least one trial for each level of reward.

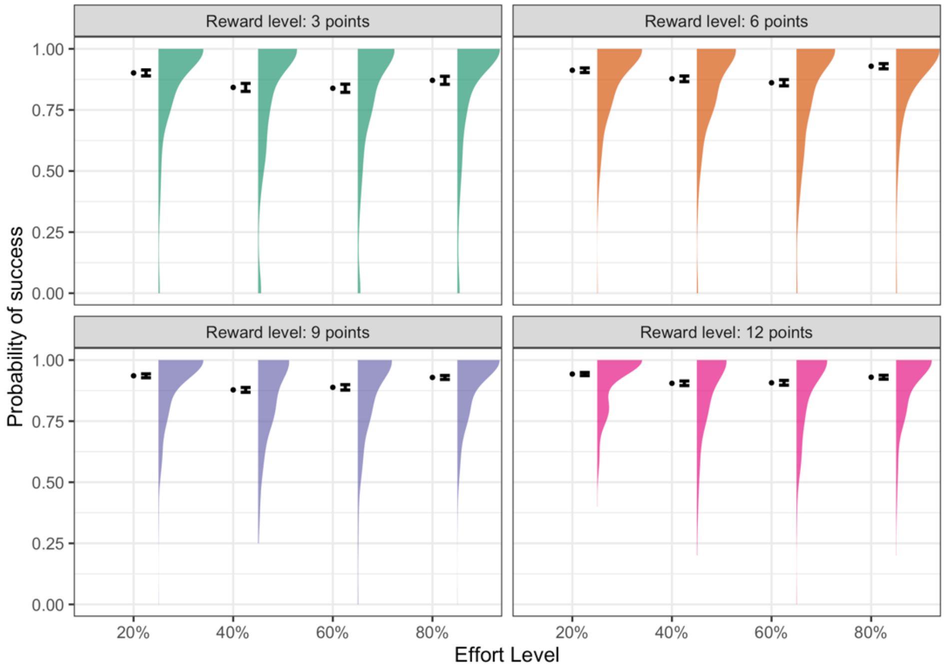


Figure 4.3. Number Switching Task: proportion of trials completed successfully. There was a significant reward effect only, driven specifically by the increase between the 3 and 6 points levels. Plot shows the mean, standard error and distribution for each combination of reward and effort level. Trials were marked as 'correct' if they were completed within the allowed time, with no more than one error. Plots show (left to right) individual data points, mean±SE and distributions.

4.4.1.1.3 Completion Times

Completion times, expressed as a proportion of each participant's allowed maximum time, are plotted in Figure 4.4. Full descriptive statistics are provided in Table S4.3. There were significant main effects of both reward, $F(1, 4014) = 10.1$, $p = .002$, $\eta^2_{partial} = 0.03$, and effort, $F(1, 4014) = 610$, $p < .001$, $\eta^2_{partial} = .52$. The interaction effect was non-significant, $F(1, 4014) = 0.56$, $p = .45$. We further investigated the two main effects with three *post hoc t*-tests for each factor. The p-values reported are Bonferroni-adjusted for multiple comparisons.

For the main effect of effort, we observed a non-linear pattern, with completion times lengthening progressively as the effort level increased between 20% and 60%, before decreasing again slightly for the 80% effort level (see descriptive statistics in

Table 4.3). The contrasts between adjacent effort levels were all significant (20% vs 40% effort: $t(286) = 19.7$, $p < .001$, $d = 1.16$; 40% vs 60% effort: $t(286) = 7.07$, $p < .001$, $d = 0.42$; and 60% vs 80% effort: $t(286) = 8.88$, $p < .001$, $d = 0.52$).

For the main effect of reward, the descriptive statistics (see Table 4.3) suggested that completion times decreased slightly with increasing reward level, although the *post hoc* comparisons between adjacent reward levels were all non-significant following Bonferroni correction (3 vs 6 points: $t(272) = 0.08$, $p = 1.0$; 6 vs 9 points: $t(272) = 1.11$, $p = .80$; 9 vs 12 points: $t(272) = 2.32$, $p = .06$).

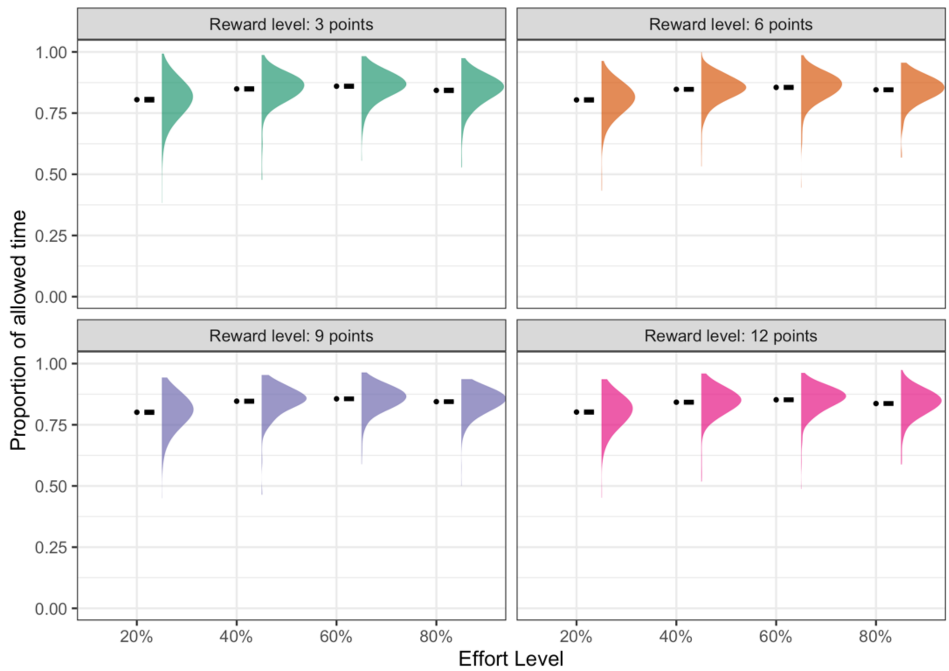


Figure 4.4. Number Switching Task: completion time. Figure shows the mean, standard error and distribution of the completion times (expressed as a proportion of each participant's allowed time) for each level of reward and effort level. Plots show (left to right) individual data points, mean±SE and distributions.

Table 4.3. Number Switching Task: Descriptive statistics for proportional completion time (across reward and effort levels).

Proportional Completion Time		
Reward (points)	**N**	**Mean (*SD*)**
3	273	0.84 (0.06)
6	273	0.84 (0.05)
9	273	0.84 (0.05)
12	273	0.83 (0.05)
Effort Level		
20%	287	0.80 (0.07)
40%	287	0.85 (0.06)
60%	287	0.86 (0.05)
80%	287	0.84 (0.05)

Notes. Times are expressed as a proportion of each participant's maximum allowed completion time.

These data only include complete cases, i.e. where participants recorded at least one trial for each level of reward or points.

4.4.1.2 Subjective Task Load

Participants' ratings of the subjective demand of each effort level are shown in Figure 4.5, with each scale of the index plotted in a separate panel. Participants reported that they found each effort level successively more demanding, which was confirmed statistically (all ANOVAs indicated a significant effect of effort, $ps < .0001$). *Post hoc t*-tests of the differences between sequential levels of effort are reported in Table 4.4. These comparisons were all significant (after Bonferroni correction), except for one: the comparison between ratings of perceived performance on the 60% and 80% effort.

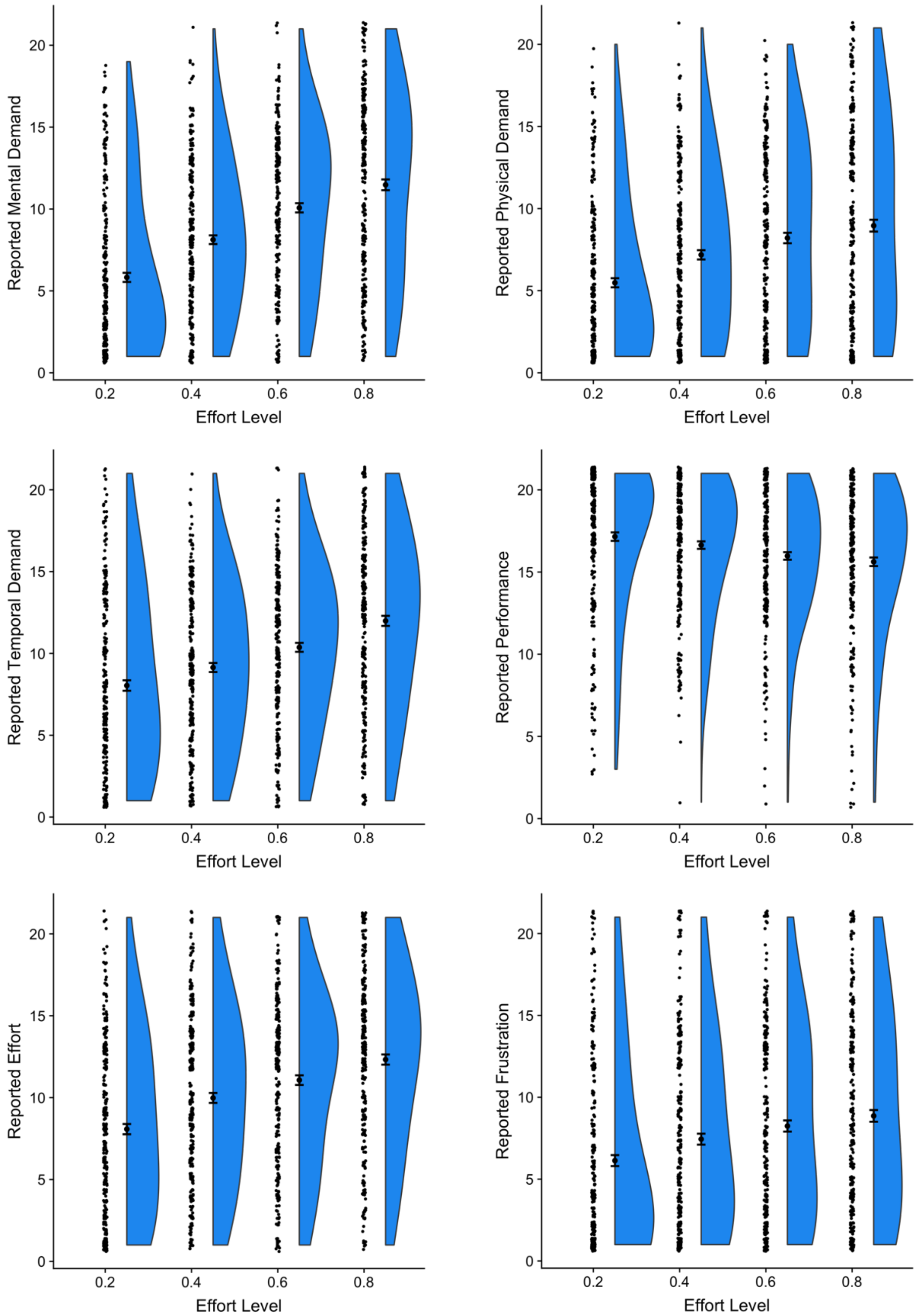


Figure 4.5. Subjective task load ratings for each effort level. Plots show (from left to right within each plot) the individual data points, the means and standard errors and the distributions of scores for each of the six scales of the index.

Table 4.4. Subjective Task Load: Post hoc t-tests and standardised effect sizes.

	Effort Level Comparisons								
	20% vs 40%			**40% vs 60%**			**60% vs 80%**		
	t	***p***	***d***	***t***	***p***	***d***	***t***	***p***	***d***
Mental Demand	10.2	< .001	0.60	10.1	< .001	0.59	5.65	< .001	0.33
Physical Demand	7.75	< .001	0.46	5.45	< .001	0.32	3.46	.002	0.20
Temporal Demand	4.50	< .001	0.26	6.26	< .001	0.37	7.15	< .001	0.42
Performance	2.48	.04	0.15	4.02	< .001	0.24	1.96	0.15	0.12
Effort	8.38	< .001	0.49	5.03	< .001	0.30	5.52	< .001	0.32
Frustration	6.13	< .001	0.36	5.05	< .001	0.30	2.93	.01	0.17

Note. P-values above are corrected for three multiple comparisons within each scale of the index. Degrees of freedom are 289 throughout.

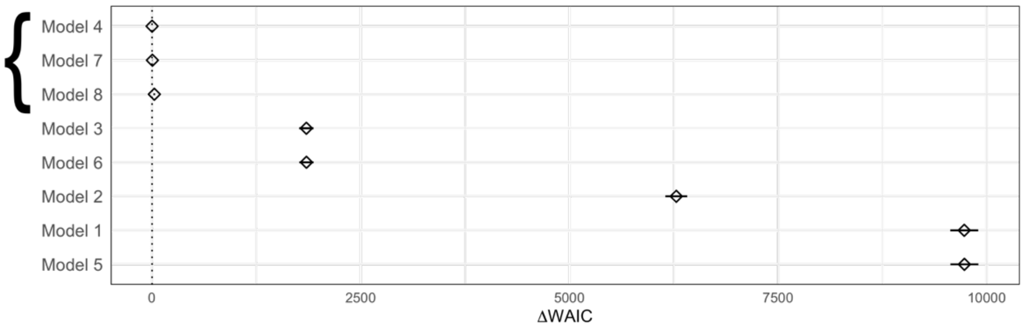
4.4.2 Exploratory Analyses

4.4.2.1. Model Comparison

We started by comparing the eight models using the Widely-Applicable Information Criterion (WAIC; Watanabe, 2010). WAIC estimates the out-of-sample predictive accuracy of a model, providing both a point estimate and standard error, enabling us to quantify uncertainty.

Models 4, 7 and 8 performed substantially better than the other five models, suggesting there is a significant benefit of allowing the effects to vary across subjects (see Figure 4.6a). Examining these three models by themselves (Figure 4.6b), we can also estimate with moderate confidence that models 4 and 7 would make better out-of-sample predictions than Model 8, which is 2.7 standard errors of difference worse. However, models 4 and 7 are probably too close to be separated on the basis of their predictions (the difference being just 1.5 standard errors). As a reminder, models 4 and 7 both contained a varying intercept and varying linear effects of reward and effort; in addition, Model 7 included a varying quadratic effect of effort.

a)



b)

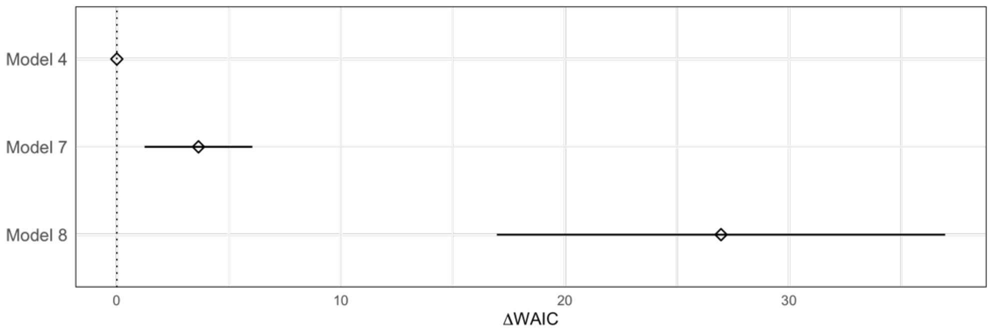


Figure 4.6. Differences in WAIC relative to the best performing model (Model 4). Plotted above is (a) the performance of the entire set of models, and (b) the performance of just the three best scoring models by themselves. Model 4, the best performing model, contained a varying intercept and varying linear effects of reward and effort; Model 7 contained the same plus a varying quadratic effect of effort as well.

This similarity in WAIC values implies that the quadratic term in Model 7 yielded no improvement in fit that could be distinguished from overfitting. Indeed, the posterior parameter estimates for Model 7 (see Figure 4.7) show that the quadratic sensitivity parameter both overlaps with zero and is highly colinear with the linear parameter. Therefore we did not consider this model further.

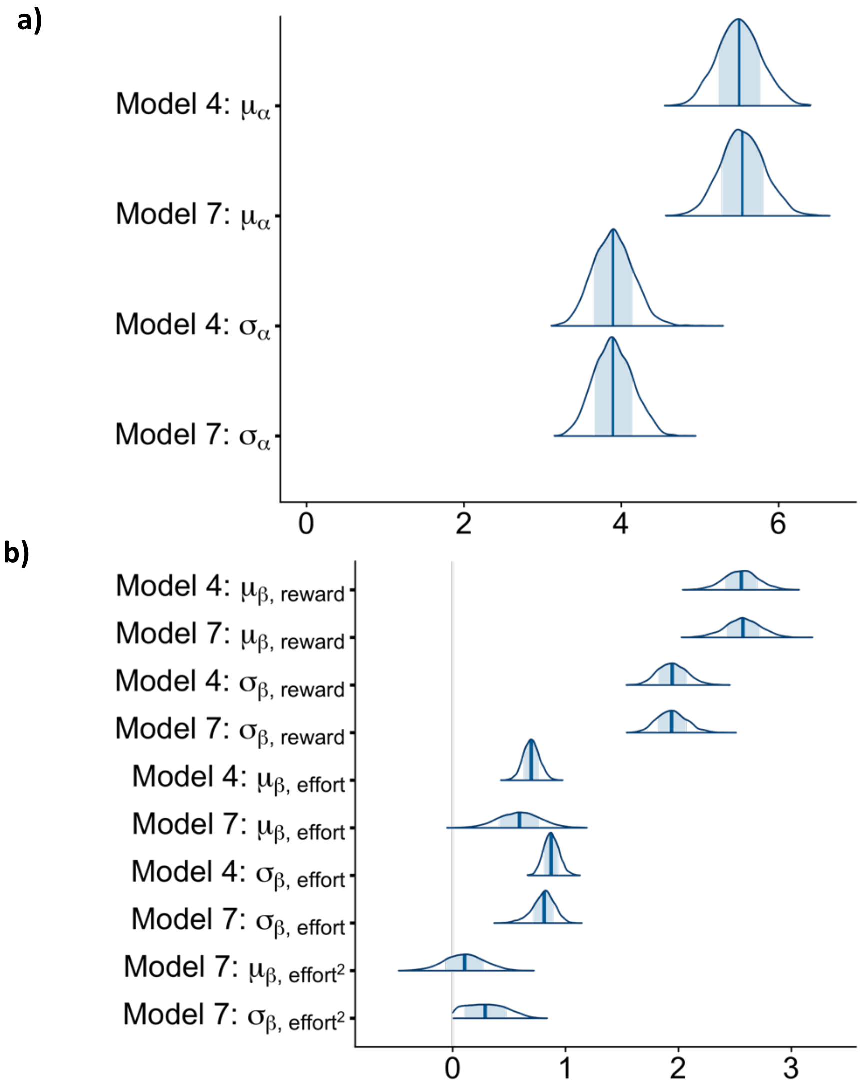


Figure 4.7. Posterior distributions of the population-level parameters in models 4 and 7. Plotted are (a) the intercept parameters, and (b) the reward and effort sensitivity parameters. The vertical line indicates the mean of each distribution, and the shaded region the 66% quantile interval. The quadratic effort sensitivity parameter (Model 7: $\mu_{\beta,effort^2}$) substantially overlaps 0 and, additionally, there is a very large negative correlation between the samples of this and the model 7 linear effort parameter, *r* = –0.92, *p* < .001, indicating substantial colinearity. See discussion in the paragraph above.

The posterior predictions of Model 4 are plotted in Figure 4.8. First, we see that the model predicts that the probability of accepting a challenge will decrease as a concave function of effort level and that this decline will be progressively shallower at higher levels of offered reward. This means that this model is able to reproduce not just the basic discounting of reward by effort, but also the specific shape of the discounting curves observed in the data. Second, the models also clearly show substantial uncertainty about the exact relationship between the probability of accepting a challenge and effort when it comes to predicting individual participant behaviour. In other words, the average population-level effect is clear but the model implies there is substantial variability between individuals.

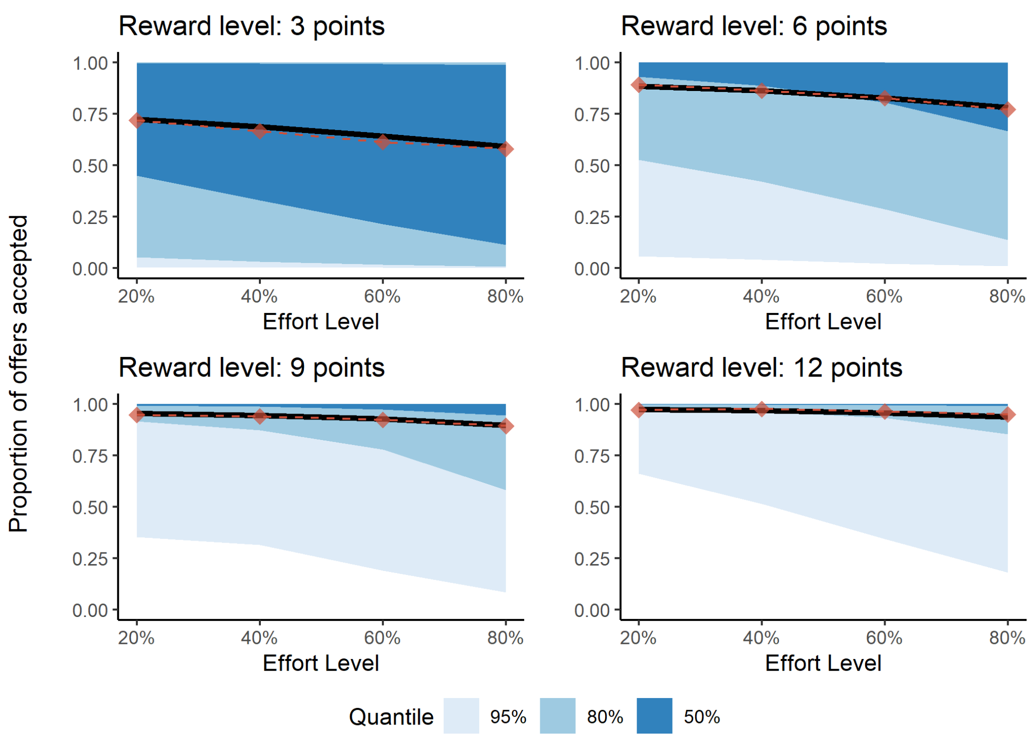


Figure 4.8. Posterior predictions for the model of the Number Switching Task. Plots show mean (black line) and posterior quantiles (95%, 80% and 50%) of the predicted probability of accepting an offer across each level of reward and effort. Also shown is the empirical data for comparison (red diamonds and dotted lines) Note that these are predictions for simulated new participants and therefore incorporate uncertainty not just about the average effect of the manipulations in the population, but also about the behaviour of individual participants.

Next, we examined the correlation between the participant-level effort sensitivity parameters and the probability of success on the task (see Figure 4.9). Importantly, this was not significantly different from zero, $r(288) = 0.10$, $p = .09$, suggesting effort sensitivity was not confounded by probability discounting.

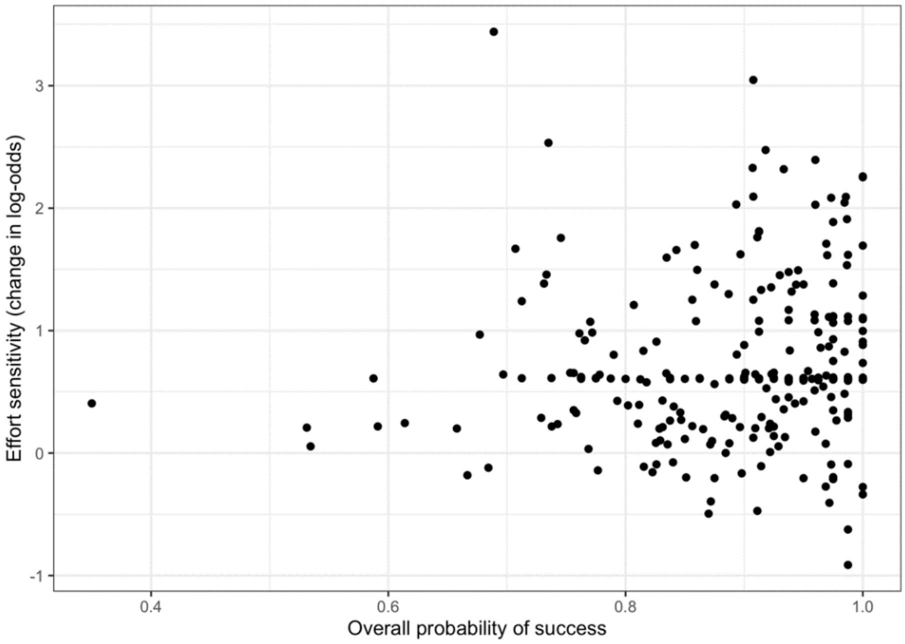


Figure 4.9. Relationship between the probability of success and effort sensitivity. The correlation was non-significant, implying that effort sensitivity is not confounded by probability discounting in this task.

Note 1. Effort sensitivity is coded such that positive values indicate that the likelihood of accepting an offer decreases as effort level increases.
Note 2. The extreme point on the left of the graph corresponds to a participant who accepted (and failed) only one trial overall. In a sensitivity analysis, removing this participant increased the *p*-value for the correlation from .09 to .14.

4.4.2.2 Structural Equation Modelling of the computational parameters and questionnaire measures

The purpose of this final stage of our analysis was to explore any possible associations between the traits assessed by our questionnaires and the subject-level parameters estimated in Model 4 above (*viz.* the intercept, reward and linear effort sensitivity).

First, we used confirmatory factor analysis to compare several possible factor structures which were devised *a priori*. The four structures considered were:

- One with a distinct latent factor for each questionnaire
- Another in which the all questions mapped onto a single latent factor, equivalent to a 'P' factor in psychiatry (Caspi et al., 2013)
- A structure in which they were grouped by broad cognitive domain
- Another in which the questionnaires directly relevant to mental health symptoms were grouped together

These are shown graphically in Figure 4.10a) – d) below.

We compared the relative accuracy of the fit of each model using three metrics: overall log likelihood, Akaike's Information Criterion and the Bayesian Information Criterion. The results are presented in Table 4.5 below. The 'Full' structure, with a distinct latent factor for each questionnaire, consistently fitted the data best across all three measures.

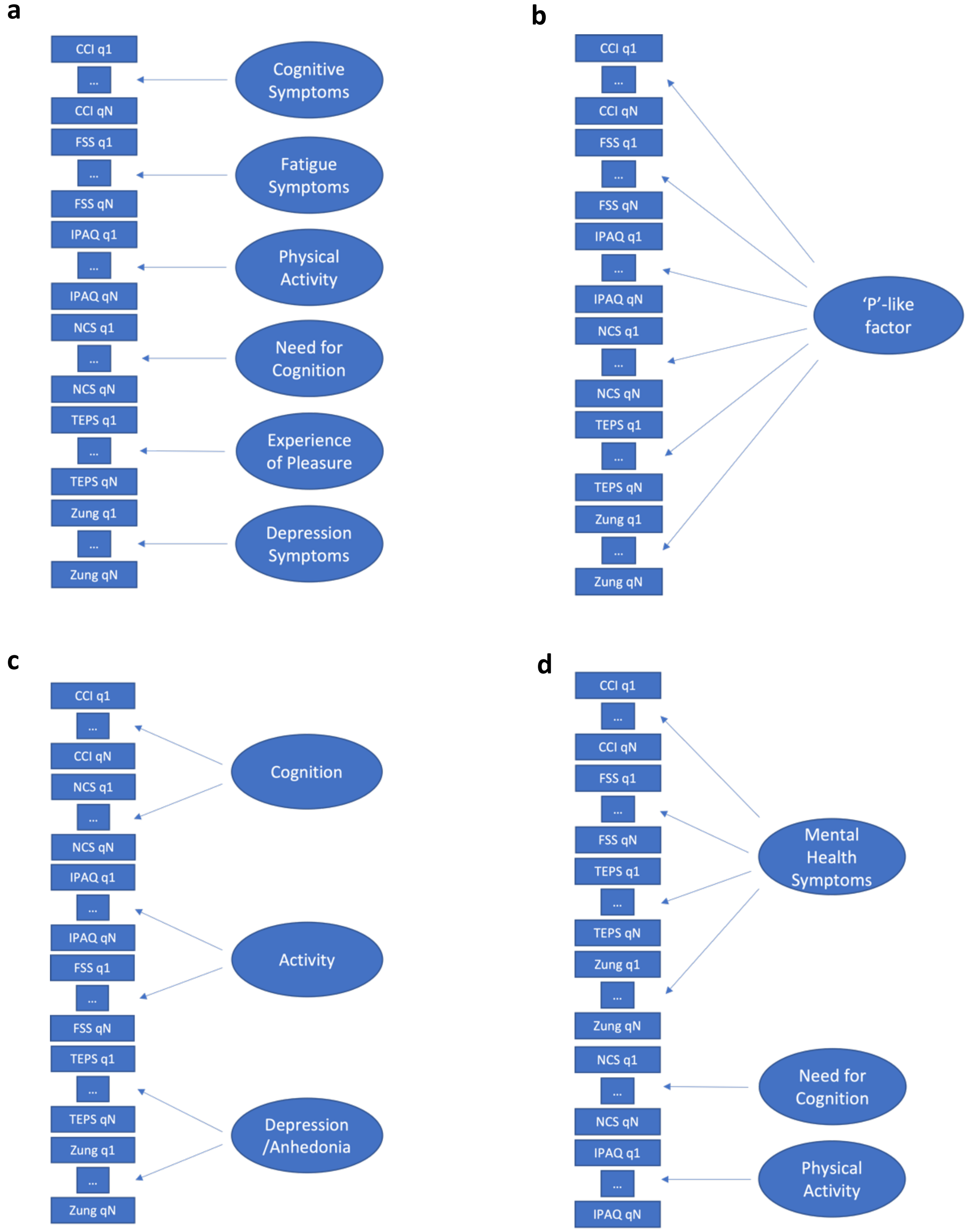


Figure 4.10. The four factor structures compared in the confirmatory factor analysis. (a) A full factor structure, with a distinct latent factor for each questionnaire. (b) A minimal factor structure, with just a single factor onto which all the questions loaded, corresponding to a 'p'-like factor. (c) An intermediate structure in which questionnaires were grouped by broad cognitive domain. (d) Another intermediate structure, in which the questionnaires directly relevant to mental health symptoms were grouped together.

Table 4.5. Results of model comparison for the confirmatory factor analysis.

Factor Structure	Log Likelihood	AIC	BIC
Full structure	-27237	54756	55273
MH symptoms grouped	-27741	55740	56213
Cognitive domain	-27827	55912	56386
'P'-like structure	-28281	56814	57277

We then conducted an SEM, using this winning factor structure as the measurement model, to predict the subject-level intercept, reward and effort sensitivity parameters obtained from Model 4 of the Number Switching Task. We found there was a significant positive association between Need for Cognition and reward sensitivity (standardised β = 0.21, p = .003). All other associations, however, were non-significant. Full results from the SEM are provided in Table S4.4.

4.5 Discussion

We have presented a new task, the NST, for measuring cognitive effort and demonstrated that it resolves one of the major shortcomings of existing measures, namely the confounding of effort by task difficulty. In our results, obtained from a large online sample, participants treated higher effort levels as more costly, despite being just as likely to win the offered reward. In other words, we were able to manipulate and measure cognitive effort without the problem of probability discounting.

A related concern was that we need to be able to standardise the difficulty of the task, otherwise comparisons across participants (for example between patient groups and healthy controls), may not be valid. In the NST this is achieved by tailoring the time allowed for completion of each sequence to participants individually. Encouragingly, the success rates were very consistent across participants, suggesting the standardisation procedure was successful.

The finding that completion times generally became longer as the effort level increased is consistent with these levels requiring more cognitive control (and therefore effort; Shenhav et al., 2017), but the small reduction in completion time at the highest level was unexpected. The most likely explanation is that half of the sequences at this level involved alternating on every digit (i.e. eight switches), which, even though participants could not be sure exactly how many switches they would be shown, may have permitted them to respond slightly faster. If so, this should be straightforward to address – future iterations of this task could use a ten- rather than nine-digit sequence, so that sequences which alternate on every digit are no longer possible. This would have the further advantage of requiring one digit to be shown twice, making it impossible for participants to work out which digits remain to be shown.

The Subjective Task Load results show clearly that participants reported finding each level of effort progressively more demanding. Curiously, in *post hoc* tests the only nonsignificant difference was on the performance subscale, between the two highest effort levels, which directly matches the behavioural result discussed above, that participants performed slightly faster at 80% than 60% effort. That participants were sensitive to this detail gives some reassurance that the Subjective Task Load results were accurate appraisals.

The remainder of our analyses were exploratory in nature and aimed principally at demonstrating how this task can be used for individual differences research. The most parsimonious model according to WAIC included linear effects of reward and effort which varied across participants. However, we should be clear that this model is linear on a log-odds scale only, implying non-linear effort costs on the outcome scale, consistent with other work (see Ritz et al., 2021). The model also indicated there was substantial variability in effort sensitivity across participants, which will be beneficial for individual differences research.

We used SEM to measure the association between several trait measures and the participant-level parameters from the behavioural model. The only significant association was between Need for Cognition, a construct representing participants' enjoyment of cognitively demanding activity, and reward sensitivity, the extent to which participants' choices changed in line with the offered reward. Possibly this is because participants who score higher on Need for Cognition pay more attention to the parameters of the task. While the lack of other associations was somewhat surprising, we emphasise that these analyses were exploratory, and it has been useful to demonstrate how this task can be applied to study symptoms of mental health conditions, in this case depression and anhedonia, even in a healthy population (in line with a dimensional view of psychiatry; see discussion in Husain & Roiser, 2017). Additionally, although the study was powered to detect reasonably small associations (r = 0.2) we could have missed weaker effects.

We are optimistic about further opportunities to use the NST in clinical research, but clearly more validation will be needed to support this. In addition, more work could be done to design more sophisticated models; those presented in this paper represent a starting point, from which natural extensions would be to explicitly model the correlations between the sensitivity parameters, or add a lapse component that acknowledges that on some trials participants may simply decide at random. Numerous other model variations can be devised and built, and would be interesting topics of study.

Having devised this new measure of effort sensitivity, our next goal is to apply it to the issue of understanding the role of effort in Pavlovian bias. Specifically, we suggested in earlier chapters that the ability to overcome Pavlovian bias is dependent on exerting cognitive control, which in turn is limited by effort costs. We can now investigate this by testing the association between participants' effort sensitivity, as assessed by the NST, and their Pavlovian bias on the Orthogonal Go/No-Go Task. We report results of such a study in the following Chapter 5.

In summary, we have presented a new task measuring cognitive effort, which resolves a longstanding problem of conflating the effort demanded by a task with its difficulty. Not only have we been able to manipulate effort without changing the difficulty of the task, but we can additionally standardise the difficulty across participants by tailoring the time allowed according to performance at an individual level. This is the first cognitive effort task in which such standardisation can be achieved and means individual differences research can be carried out without concerns around confounding from difficulty or ability.

4.6 Data and code availability

Code to run the Number Switching Task is deposited in the Gorilla Open Materials Repository, https://app.gorilla.sc/openmaterials/328049. All data and analysis scripts are provided at the Open Science Foundation repository, https://doi.org/10.17605/OSF.IO/X34KN.

4.7 Appendix

4.7.1 Full specification of the computational models

Model 1. Fixed Intercept and Fixed Linear Effects of Reward and Effort

$$Y_{subject,trial} \sim \text{Bernoulli}(p_{trial})$$
$$p_{trial} = \text{logistic}(\alpha + \beta_{reward} reward_{trial} + \beta_{effort} effort_{trial})$$
$$\alpha \sim Normal(0, 1.5)$$
$$\beta_{reward} \sim Normal(0, 1)$$
$$\beta_{effort} \sim Normal(0, 1)$$

(S4.1)

Model 2. Varying Intercept

$$Y_{subject,trial} \sim \text{Bernoulli}(p_{subject})$$
$$p_{subject} = \text{logistic}(\alpha_{subject})$$
$$\alpha_{subject} \sim Normal(\mu_\alpha, \sigma_\alpha)$$
$$\mu_\alpha \sim Normal(0, 1.5)$$
$$\sigma_\alpha \sim Exponential(2)$$

(S4.2)

Model 3. Varying Intercept and Fixed Linear Effects of Reward and Effort

$$Y_{subject,trial} \sim \text{Bernoulli}(p_{subject,trial})$$
$$p_{subject,trial} = \text{logistic}(\alpha_{subject} + \beta_{reward} reward_{trial} + \beta_{effort} effort_{trial})$$
$$\alpha_{subject} \sim Normal(\mu_\alpha, \sigma_\alpha)$$
$$\mu_\alpha \sim Normal(0, 1.5)$$
$$\sigma_\alpha \sim Exponential(2)$$
$$\beta_{reward} \sim Normal(0, 1)$$
$$\beta_{effort} \sim Normal(0, 1)$$

(S4.3)

Model 4. Varying Intercept and Varying Linear Effects of Reward and Effort

$$Y_{subject,trial} \sim \text{Bernoulli}\left(p_{subject,trial}\right)$$
$$p_{subject,trial} = \text{logistic}(\alpha_{subject} + \beta_{reward,subject} reward_{trial} + \beta_{effort,subject} effort_{trial})$$
$$\alpha_{subject} \sim Normal(\mu_{\alpha}, \sigma_{\alpha})$$
$$\mu_{\alpha} \sim Normal(0, 1.5)$$
$$\sigma_{\alpha} \sim Exponential(2)$$
$$\beta_{reward,subject} \sim Normal(\mu_{reward}, \sigma_{reward})$$
$$\mu_{reward} \sim Normal(0,1)$$
$$\sigma_{reward} \sim Exponential(2)$$
$$\beta_{effort,subject} \sim Normal(\mu_{effort}, \sigma_{effort})$$
$$\mu_{effort} \sim Normal(0,1)$$
$$\sigma_{effort} \sim Exponential(2)$$

(S4.4)

Model 5. Fixed Intercept, Fixed Linear Effect of Reward and Fixed Linear and Quadratic Effects of Effort

$$Y_{subject,trial} \sim \text{Bernoulli}(p_{trial})$$
$$p_{trial} = \text{logistic}(\alpha + \beta_{reward} reward_{trial} + \beta_{effort} effort_{trial} + \beta_{effort^2} effort_{trial}{}^2)$$
$$\alpha \sim Normal(0, 1.5)$$
$$\beta_{reward} \sim Normal(0, 1)$$
$$\beta_{effort} \sim Normal(0, 1)$$
$$\beta_{effort^2} \sim Normal(0, 1)$$

(S4.5)

Model 6. Varying Intercept, Fixed Linear Effect of Reward and Fixed Linear and Quadratic Effects of Effort

$$
\begin{gathered}
Y_{subject,trial} \sim \text{Bernoulli}\left(p_{subject,trial}\right) \\
p_{subject,trial} = \text{logistic}(\alpha_{subject} + \beta_{reward} reward_{trial} + \beta_{effort} effort_{trial} \\
+ \beta_{effort^2} effort_{trial}{}^2) \\
\alpha_{subject} \sim Normal(\mu_\alpha, \sigma_\alpha) \\
\mu_\alpha \sim Normal(0, 1.5) \\
\sigma_\alpha \sim Exponential(2) \\
\beta_{reward} \sim Normal(0, 1) \\
\beta_{effort} \sim Normal(0, 1) \\
\beta_{effort^2} \sim Normal(0, 1)
\end{gathered}
\tag{S4.6}
$$

Model 7. Varying Intercept, Varying Linear Effect of Reward and Varying Linear and Quadratic Effects of Effort

$$
\begin{gathered}
Y_{subject,trial} \sim \text{Bernoulli}\left(p_{subject,trial}\right) \\
p_{subject,trial} = \text{logistic}(\alpha_{subject} + \beta_{reward,subject} reward_{trial} \\
+ \beta_{effort,subject} effort_{trial} + \beta_{effort^2,subject} effort_{trial}{}^2) \\
\alpha_{subject} \sim Normal(\mu_\alpha, \sigma_\alpha) \\
\mu_\alpha \sim Normal(0, 1.5) \\
\sigma_\alpha \sim Exponential(2) \\
\beta_{reward,subject} \sim Normal(\mu_{reward}, \sigma_{reward}) \\
\mu_{reward} \sim Normal(0,1) \\
\sigma_{reward} \sim Exponential(2) \\
\beta_{effort,subject} \sim Normal(\mu_{effort}, \sigma_{effort}) \\
\mu_{effort} \sim Normal(0,1) \\
\sigma_{effort} \sim Exponential(2) \\
\beta_{effort^2,subject} \sim Normal(\mu_{effort^2}, \sigma_{effort^2})
\end{gathered}
$$

$$\mu_{effort^2} \sim Normal(0,1)$$
$$\sigma_{effort^2} \sim Exponential(2)$$

(S4.7)

Model 8. Varying Intercept, Varying Linear Effect of Reward and Varying Quadratic Effect of Effort

$$Y_{subject,trial} \sim \mathrm{Bernoulli}(p_{subject,trial})$$
$$p_{subject,trial} = \mathrm{logistic}(\alpha_{subject} + \beta_{reward,subject} reward_{trial} + \beta_{effort^2,subject} effort_{trial}{}^2)$$
$$\alpha_{subject} \sim Normal(\mu_\alpha, \sigma_\alpha)$$
$$\mu_\alpha \sim Normal(0, 1.5)$$
$$\sigma_\alpha \sim Exponential(2)$$
$$\beta_{reward,subject} \sim Normal(\mu_{reward}, \sigma_{reward})$$
$$\mu_{reward} \sim Normal(0,1)$$
$$\sigma_{reward} \sim Exponential(2)$$
$$\beta_{effort^2,subject} \sim Normal(\mu_{effort^2}, \sigma_{effort^2})$$
$$\mu_{effort^2} \sim Normal(0,1)$$
$$\sigma_{effort^2} \sim Exponential(2)$$

(S4.8)

4.7.2. Prior Predictive Checks

Below are plotted the distributions of all of the parameters used in the eight models, which are expressed and plotted on the log-odds scale. Additionally we include plots of the prior predictive distributions for the probability of accepting an offer for an individual subject, $p_{subject}$. These are on the probability scale. Where the plots show simulated, rather than analytical, distributions, these represent 1000 simulations with 100 hypothetical participants in each. In all cases, the shaded distributions are 66%, 95% and 100% quantiles.

Priors for the intercepts

Shown below are the prior distributions for the two types of intercept parameters, fixed (Figure S4.1) and varying (Figure S4.2) intercepts. In both cases we see that the priors chosen represent conservative predictions about the data we would expect to observe. Most importantly, a participant's probability of accepting an offer, $p_{subject}$, is constrained to be between 0 and 1. Within that range however the prior density is distributed fairly uniformly, save that it drops off below about 0.2 and above about 0.8. Overall these priors encode beliefs about participants' average acceptance rates that slightly downweight the likelihood of observing the most extreme values but otherwise are fairly agnostic.

Figure S4.1. Prior predictions for the fixed intercept parameter (featured in models 1 and 5), with distribution $\alpha \sim Normal(0,\ 1.5)$.
(a) shows the analytical distribution of the intercept α itself, whilst (b) shows the implied prior predictions for the probability of accepting an offer for an individual subject.

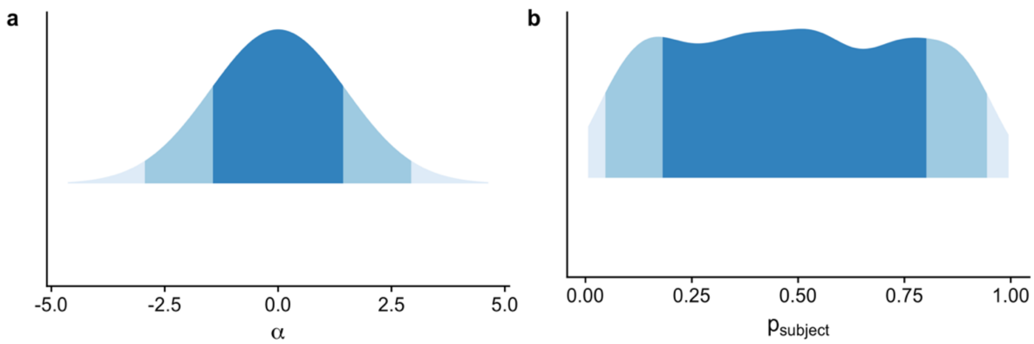
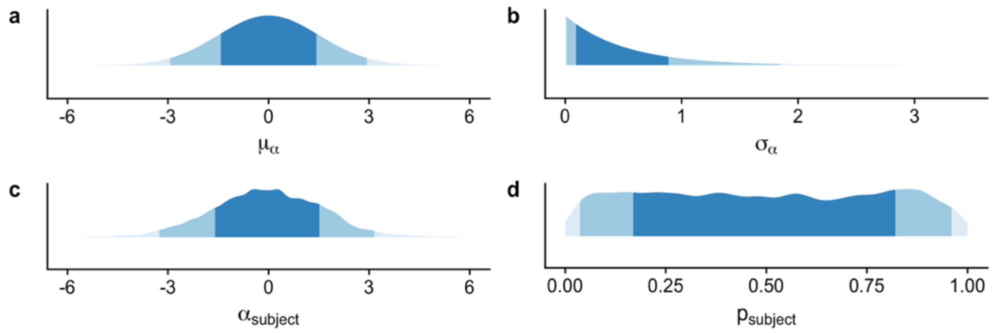


Figure S4.2. Prior predictions for the varying intercept parameters (featured in models 2, 3, 4, 6, 7 and 8), with distributions $\mu_\alpha \sim Normal(0, 1.5)$, $\sigma_\alpha \sim Exponential(2)$ and $\alpha_{subject} \sim Normal(\mu_\alpha, \sigma_\alpha)$.
(a) and (b) show the analytical distributions of, respectively, the population level average intercept and the standard deviation of this average; (c) shows the distribution of the (subject level) intercepts themselves; and (d) shows the implied prior predictions for the probability of accepting an offer for an individual subject.



Priors for the reward/effort effects

Shown below are the prior distributions for the reward/effort sensitivity parameters, for the fixed linear (Figure S4.3) and varying linear (Figure S4.4) cases and for the fixed and varying quadratic effort sensitivity parameter (Figure S4.5).

The most important plot for interpreting these priors is in the bottom right of each box, labelled b (or d in the case of Figure S4.4). This shows the prior on the effect of the reward/effort manipulation on the probability scale. Specifically, this is the predicted change in a participant's probability of accepting the offer when one of the manipulations is changed by one level. For example, moving from 3 to 6 points (or 6 to 9 points, etc.), while effort is kept constant, or vice versa moving from 80% to 60% effort (or 60% to 40% effort, etc.) while reward is kept constant.

In all cases the priors chosen encode conservative beliefs that the effects, if present, are expected to be approximately in the range 0 – 0.25, within which, because of the rightward skew, smaller effects are considered more likely than larger ones.

Figure S4.3. Prior predictions for the fixed linear reward and effort sensitivity parameters (featured in models 1, 3, 5 and 6), with distribution $\beta \sim Normal(0,\ 1)$.
(a) shows the analytical distribution of the sensitivity β itself, whilst (b) shows the implied predictions for the change in probability of accepting an offer as the reward or effort changes by one level (e.g. from 3 to 6 points, or 80% to 60% effort etc.)

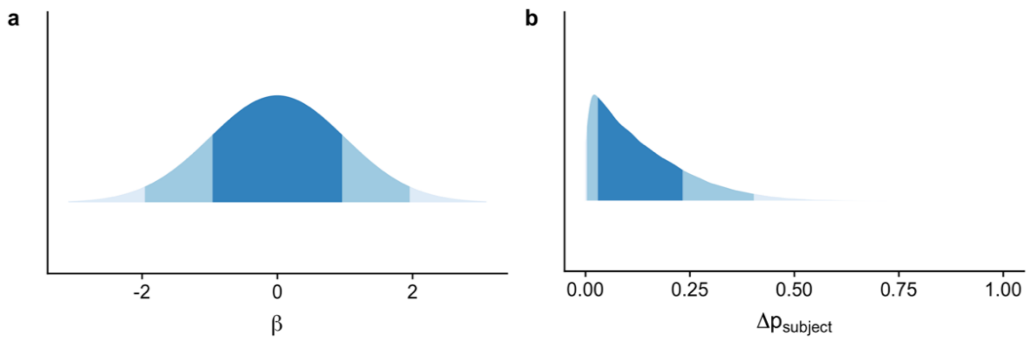


Figure S4.4. Prior predictions for the varying linear reward and effort sensitivity parameters (featured in models 4, 7 and 8), with distributions $\mu_\beta \sim Normal(0,1), \sigma_\beta \sim Exponential(2)$ and $\beta_{subject} \sim Normal(\mu_\beta, \sigma_\beta)$.
(a) and (b) show the analytical distributions of, respectively, the population level average sensitivity and the standard deviation of this average; (c) shows the distribution of the (subject level) sensitivity parameters themselves; and (d) shows the implied predictions for the change in probability of accepting an offer as the reward or effort changes by one level (e.g. from 3 to 6 points, or 80% to 60% effort etc.)

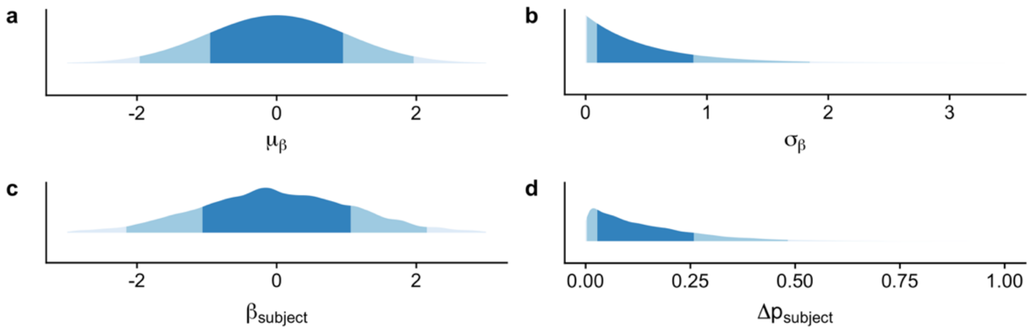
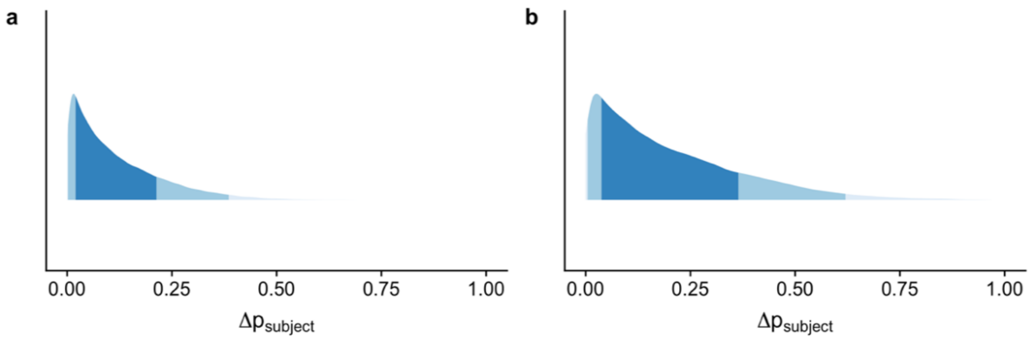


Figure S4.5. Prior predictions for the quadratic effort sensitivity parameters with distributions $\beta_{effort^2} \sim Normal(0, 1)$ (fixed effect in models 5 and 6) and $\mu_{effort^2} \sim Normal(0,1), \sigma_{effort^2} \sim Exponential(2)$ and $\beta_{effort^2,subject} \sim Normal(\mu_{effort^2}, \sigma_{effort^2})$ (varying effects in models 7 and 8).
The distributions of the sensitivity parameters themselves are the same as for the linear parameters plotted above, in Figure S3(a) for the fixed effect, and in Figure S4(a, b and c) for the varying effects. Below we plot the implied predictions for the change in probability of accepting an offer as the required effort changes by one level (e.g. from 80% to 60% effort etc.), for (a) a fixed parameter, and (b) varying parameters.



4.7.3. Supplementary results

Table S4.1. Number Switching Task: Proportion of offers accepted.

P(accept)		
Reward (points)	**N**	**Mean (*SD*)**
3	290	0.64 (0.37)
6	290	0.84 (0.27)
9	290	0.93 (0.19)
12	290	0.97 (0.12)
Effort level		
20%	290	0.88 (0.18)
40%	290	0.86 (0.19)
60%	290	0.83 (0.21)
80%	290	0.80 (0.25)
Reward: Effort		
3: 20%	290	0.72 (0.38)
3: 40%	290	0.66 (0.39)
3: 60%	290	0.61 (0.42)
3: 80%	290	0.58 (0.43)
6: 20%	290	0.89 (0.25)
6: 40%	290	0.86 (0.27)
6: 60%	290	0.83 (0.31)
6: 80%	290	0.77 (0.35)
9: 20%	290	0.95 (0.17)
9: 40%	290	0.94 (0.19)
9: 60%	290	0.92 (0.22)
9: 80%	290	0.89 (0.26)
12: 20%	290	0.97 (0.13)
12: 40%	290	0.98 (0.11)

12: 60%	290	0.96 (0.14)
12: 80%	290	0.95 (0.17)

Table S4.2. Number Switching Task: Proportion of trials completed successfully.

P(success)		
Reward (points)	**N**	**Mean (*SD*)**
3	273	0.86 (0.18)
6	287	0.89 (0.13)
9	289	0.91 (0.10)
12	289	0.92 (0.10)
Effort level		
20%	289	0.92 (0.10)
40%	288	0.88 (0.12)
60%	289	0.88 (0.15)
80%	287	0.92 (0.12)
Reward: Effort		
3: 20%	255	0.90 (0.20)
3: 40%	247	0.84 (0.26)
3: 60%	227	0.84 (0.25)
3: 80%	218	0.87 (0.24)
6: 20%	280	0.91 (0.16)
6: 40%	278	0.88 (0.19)
6: 60%	271	0.86 (0.21)
6: 80%	263	0.93 (0.17)
9: 20%	287	0.94 (0.13)
9: 40%	285	0.88 (0.17)
9: 60%	281	0.89 (0.18)
9: 80%	277	0.93 (0.14)

12: 20%	288	0.94 (0.11)
12: 40%	288	0.90 (0.16)
12: 60%	287	0.91 (0.17)
12: 80%	285	0.93 (0.13)

Table S4.3. Number Switching Task: Completion time.

Proportional completion time		
Reward (points)	**N**	**Mean (*SD*)**
3	273	0.84 (0.06)
6	286	0.84 (0.05)
9	289	0.84 (0.05)
12	289	0.83 (0.05)
Effort level		
20%	289	0.80 (0.07)
40%	288	0.85 (0.06)
60%	288	0.86 (0.05)
80%	287	0.84 (0.05)
Reward: Effort		
3: 20%	255	0.80 (0.08)
3: 40%	247	0.85 (0.07)
3: 60%	225	0.86 (0.06)
3: 80%	216	0.84 (0.06)
6: 20%	280	0.80 (0.07)
6: 40%	278	0.85 (0.06)
6: 60%	268	0.86 (0.06)
6: 80%	263	0.85 (0.06)
9: 20%	286	0.80 (0.07)
9: 40%	285	0.85 (0.06)

9: 60%	278	0.86 (0.05)
9: 80%	277	0.84 (0.05)
12: 20%	288	0.80 (0.07)
12: 40%	288	0.84 (0.06)
12: 60%	287	0.85 (0.06)
12: 80%	285	0.84 (0.06)

Table S4.4. Results of the structural equation model.

Path	Standardised Coefficient	*z*-score	*p*
Intercept →			
- Age	0.049	0.808	.42
- Education	-0.105	-1.727	.08
- Cognitive Symptoms	-0.004	-0.038	.97
- Fatigue Symptoms	-0.106	-1.269	.20
- Physical Activity	0.007	0.105	.92
- Need for Cognition	-0.058	-0.838	.40
- Experience of Pleasure	0.126	1.634	.10
- Depression Symptoms	0.058	0.507	.61
Reward sensitivity →			
- Age	-0.026	-0.444	.66
- Education	0.113	1.900	.06
- Cognitive Symptoms	0.117	1.135	.26
- Fatigue Symptoms	-0.047	-0.576	.57
- Physical Activity	0.138	1.826	.07
- Need for Cognition	0.210	3.001	.003**
- Experience of Pleasure	-0.149	-1.948	.05
- Depression Symptoms	0.004	0.039	.97

Effort sensitivity →			
- Age	–0.025	0.411	.68
- Education	0.070	-1.154	.25
- Cognitive Symptoms	0.019	-0.184	.85
- Fatigue Symptoms	0.062	-0.762	.45
- Physical Activity	0.025	-0.358	.72
- Need for Cognition	–0.128	1.840	.07
- Experience of Pleasure	0.071	-0.946	.34
- Depression Symptoms	0.079	-0.701	.48

Chapter 5. Does Cognitive Effort Explain Differences In Control Over Pavlovian Bias?

5.1 Abstract

Effort is a key factor in cognitive performance generally, and in particular in the ability to engage in careful, controlled processing. One example where control, and therefore effort, is required is in overcoming Pavlovian biases (fixed responses that promote approach towards reward and avoidance of punishments). In Chapters 2 and 3 of this thesis we showed that these biases are modifiable, supporting the view that the Pavlovian system is subject to control, but to make this connection stronger we want also to test whether the strength of Pavlovian biases is dependent on effort. If so this would allow us to situate Pavlovian bias within the framework of effort-based decision-making, describing the expression of Pavlovian biases as the product of a trade-off between the incentives for accurate responding and the costs of exerting control. In the present study we therefore investigated whether there was any association between the strength of participants' Pavlovian biases, as assessed by the Orthogonal Go/No-Go task, and their sensitivity to cognitive effort, measured on the Number Switching Task. The results were however somewhat equivocal: there was a significant correlation in our model-based analyses, but also some issues with model fit that suggest further validation work is likely to be required. In secondary, exploratory analyses we also found that there was a more reliable significant correlation between cognitive effort sensitivity and depression and anxiety symptoms, supporting hypotheses based in the physical effort literature that suggest links between effort and these conditions.

5.2 Introduction

The term cognitive effort refers to the proportion of our cognitive capacity that we choose to devote to a cognitive task. In everyday life we are all familiar with the sense that we can 'try' more or less hard at what we do and that this then influences the accuracy of performance we achieve (Shenhav et al., 2017, define effort as the "mediator" between capacity and performance). The reason we do not always exert maximal effort is because effort is accompanied by a subjective, aversive sensation believed to reflect the cost of deploying cognitive resources. Effort, and sensitivity to these effort costs in particular, is therefore an important factor determining cognitive performance and the ability to exert cognitive control in particular (Shenhav et al., 2013, 2017).

An example of this is control over Pavlovian biases. Briefly, Pavlovian biases are fixed responses to stimuli that predict reward and punishment, specifically promoting the invigoration of action when rewards are anticipated ('approach bias') and the inhibition of action when punishments are predicted ('avoidance bias'; Dayan & Balleine, 2002; Dayan et al., 2006). Clearly this can lead to behaviour that is maladaptive in certain circumstances. For example, impulsive behaviour resulting from the approach bias may mean one foregoes a larger reward that would have been available in the future; likewise, avoidance of stimuli that precede a negative event could mean that one misses the opportunity to intervene to prevent it from happening. We suggested in Chapters 2 and 3 that cognitive control may be capable of downweighting Pavlovian biases when they are likely to be inappropriate, in order to favour other responses that will lead to better outcomes. At the same time, however, exerting control is dependent on effort. This suggests that the strength of Pavlovian biases may depend on the extent to which people are willing to exert effort to control them. If this could be demonstrated empirically, it would significantly improve our understanding not just of the mechanism underlying the expression of Pavlovian biases, but also of the reason for differences between people in the strength of their biases.

For example, anxiety and depression have previously been found to be associated with enhanced Pavlovian avoidance biases (Mkrtchian, Aylward, et al., 2017, and Nord et al., 2018 respectively). Separately both conditions have also been linked to differences in effort-based decision making (see e.g. Grahek et al., 2019; Robinson et al., 2013; Valton et al., 2018). Hence, it is plausible that avoidance biases in anxiety and depression may to some extent be attributable to enhanced sensitivity to cognitive effort costs (Dayan & Huys, 2008).

In this study we therefore aimed to test the idea that individual differences in the strength of Pavlovian biases may be related to differences in willingness to exert effort. Thanks to the cognitive effort task we developed previously (the Number Switching Task, NST; see Chapter 4), this is now something we can quantify explicitly, using a computational model that estimates effort sensitivity (and reward and intercept) parameters for each participant. Importantly, this is the first cognitive effort task in which the difficulty can be standardised, making comparisons between individuals much easier.

Alongside our cognitive effort task, we measured the strength of participants' Pavlovian biases based on their performance on the Orthogonal Go/No-Go Task (Guitart-Masip et al., 2011), which was used previously in Chapters 2 and 3. In this task, the required action and the outcome valence are manipulated orthogonally to give four distinct trial types, in two of which the Pavlovian and instrumental systems are aligned and in the other two they are in conflict (see Table 5.1).

Table 5.1. The four trial types of the Orthogonal Go/No-Go Task. Squares shaded dark are those for which the Pavlovian and instrumental systems produce conflicting responses

	Reward	**Punishment**
Go	Go to Win Reward	Go to Avoid Punishment
No-Go	No-Go to Win Reward	No-Go to Avoid Punishment

This task allows us to isolate and measure the Pavlovian biases independently of other factors, such as differing sensitivity to reward and punishment. Our primary hypothesis in this study was that we would see a significant correlation between effort sensitivity on the Number Switching Task and the strength of Pavlovian biases on the Go/No-Go Task.

In addition we also wanted to test more explicitly the suggestion above that there may be a relationship between both Pavlovian biases and effort sensitivity and anxiety and depression symptoms (Dayan & Huys, 2008; Husain & Roiser, 2017). We therefore examined the associations between cognitive effort, Pavlovian bias and two self-report symptom scales, the Zung Depression Scale (Zung, 1965) and the State-Trait Anxiety Scale (Spielberger et al., 1983).

5.3 Methods

5.3.1 Preregistration

This study was preregistered on the Open Science Framework (https://osf.io/khws9/?view_only=b0b3c8e58d3d4b4280a7d7b4b0b7f11e). Note that the method below departs from the preregistration in two ways: we added another exclusion criterion, to remove participants who frequently responded on the Go/No-Go task with keys that were not in the response set (see Section 5.3.3.1); and we modelled the Number Switching Task and the Go/No-Go Task with just the winning models from Chapters 3 and 4 (i.e. rather than doing a full model comparison again) to ensure that the modelling results would be directly comparable across all chapters.

5.3.2 Participants

Participants were recruited through the online platform Prolific. The study was advertised only to participants who met the following inclusion criteria: aged 18-60, fluent in English, no history of psychiatric or neurological disorders, and did not take part in the studies reported in Chapters 3 and 4 in this thesis. Participants also had to use a computer – smartphones or tablets were not allowed.

In our preregistration we stated that a minimally interesting effect size for the correlation between the effort sensitivity and Pavlovian bias measures was r = 0.15. This is conventionally regarded as a small effect—it implies that effort sensitivity explains only 2.25% of the variance in Pavlovian bias—so we judged that if the true effect was in fact smaller than this, it would not be especially meaningful or useful. We then computed, using 90% power and α = 5%, a minimum required sample size of 571 participants, which we increased to 625 participants to allow for attrition and exclusions. Ultimately 45 participants were excluded, leaving 580 whose data was included in the final analysis. A detailed breakdown of the reasons for exclusion is given in Section 5.3.3.1 below.

5.3.3 Procedure

The entire study was completed online. Participants were recruited on Prolific, and then redirected to Gorilla, where the study itself was hosted. At the end of the study they were redirected back to Prolific again via a unique link, which allowed us to verify that they had indeed completed all of the tasks.

Participants completed two behavioural tasks, the online version of the Orthogonal Go/No-Go Task (Guitart-Masip et al., 2011) and the Number Switching Task. These tasks were as described in previous chapters (Chapter 3 Section 3.3.4.1 and Chapter 4 Section 4.3.3.1 respectively), except that the Go/No-Go Task now comprised 160 trials in the main phase, rather than 80. The order in which participants did these two tasks was counterbalanced. Following the tasks, participants then completed two self-report symptom scales, the Zung Depression Scale (Zung, 1965; described in Chapter 4, Section 4.3.3.2) and the State-Trait Anxiety Inventory (Spielberger et al., 1983; described in Chapter 2, Section 2.3.4.5).

5.3.3.1 Participant exclusions

A detailed schedule of the participants excluded at each stage and the reasons for exclusion, is given below in Table 5.2. Note that participants who were excluded were removed from the entire study, i.e. not just from the individual task.

For the Go/No-Go Task, we set out three exclusion criteria in our preregistration. Participants were excluded if:

- they failed the comprehension test 5 times
- their accuracy on the go-to-win trials during the practice phase was less than 65%
- less than 65% of their go responses matched the same side as the target circle

We also excluded participants for two reasons not included in the preregistration. As in Chapter 3, we had to exclude some participants who made a large number of responses (>15%) using keys that were not in the response set (not S or L). We also had to exclude participants who refreshed the webpage partway through and therefore repeated part of the task.

For the Number Switching Task, we set out one exclusion criterion in our preregistration. Participants were excluded if they failed the familiarisation phase (completed >50% of the highest effort level trials incorrectly) twice. Again, we also had to exclude some participants who refreshed the webpage partway through and therefore repeated the task.

Finally, for the STAI we included a catch question ("Press the very much so button") at the end of the questionnaire to detect inattentive participants, but without interfering with the questionnaire's psychometrics. Participants who failed this question were excluded. This was also noted in the preregistration.

Table 5.2. Schedule of exclusions. GNG=Go/No-Go Task, NST=Number Switching Task. All criteria were preregistered except one, the criterion for the Go/No-Go task: 'wrong key responses > 15%'. See main text for details.

Task	Reason	N excluded	N remaining
			625
Go/No-Go	GNG: go to win reward accuracy < 65%	11	614
	GNG: left/right accuracy < 65%	9	605
	GNG: refreshed during main phase	2	603
	GNG: wrong key responses >15%	6	597
Number Switching Task	NST: refreshed during main phase	8	589
	NST: failed training phase	3	586
STAI	STAI: failed attention check	6	580

5.3.4 Statistical analyses

5.3.4.1. Model agnostic analyses

For both the Go/No-Go Task and the Number Switching Task, we started by carrying out the same model agnostic analyses as in Chapters 3 and 4. For the Go/No-Go Task, we ran a 2 X 2 repeated-measures ANOVA with effects of action and outcome valence on accuracy. For the Number Switching Task, we carried out three multilevel (mixed effects) ANOVAs, with varying intercepts across participants and fixed effects of reward, effort and their interaction. These three ANOVAs examined the proportion of offers accepted, the success rates and the proportional completion times; of these, the ANOVA on the success rates was one that we had specifically preregistered and had predicted would not show a significant effort effect.

We followed any significant effects on the ANOVAs with *post hoc* simple effects ANOVAs and *t*-tests where appropriate. Note that the *t*-tests require complete cases and therefore, for the NST, some participants who had not completed any trials at a particular reward or effort level had to be excluded from the *post hoc* analyses of success rates or completion times.

Subsequently we then tested our primary hypothesis for this study, which was that there would be a significant correlation between the strength of Pavlovian bias and effort sensitivity. To test this, we first carried out a model agnostic analysis. For each participant, we computed the model agnostic measure of Pavlovian bias (see Chapter 3) by summing the accuracy in the two high Pavlovian-instrumental conflict trial types (go to avoid punishment and no-go to win reward) minus the sum of the two low conflict trial types (go to win reward and no-go to avoid punishment). Likewise we computed a model agnostic measure of effort sensitivity (not reported previously), which was the difference in the probability of accepting an offer between the highest (80%) and lowest (20%) effort levels, for each participant. We then calculated the Pearson correlation coefficient between the two, which we predicted would be significant.

We also examined the correlations between the model agnostic Pavlovian bias and effort sensitivity measures and our self-report symptom scales (the Zung Depression Scale and the State-Trait Anxiety Scale).

5.3.4.2 Computational modelling

To test our primary hypothesis about the association between Pavlovian biases and effort sensitivity, we also examined the parameters from computational modelling of the two tasks. We fitted the winning models from Chapters 3 and 4 to the data from the tasks. This was a slight departure from the preregistration, in which we had said we would repeat the model comparison process – instead, we decided to use the same models so that the analysis reported in this chapter would be directly comparable with those in the earlier chapters.

For the Go/No-Go Task this comprised the Base model plus two (reward/punishment) learning rates. The model is set out in Equations 5.1–5.5 below, in which $response_t$ denotes the response at time t (go or no-go), $\xi_{subject}$ is the amount of noise in participants' behaviour, $q_{response}(s_t)$ is the instrumental value of making a go or no-go response when stimulus s is presented at time t, and $value(s_t)$ is the associative (Pavlovian) value of the stimulus at time t.

Go/No-Go Model: Response

$$response_t = Bernoulli(pGo_t)$$
$$pGo_t = (1 - \xi_{subject}) \times logistic\big(w(s_t)\big) + \xi_{subject} \times \frac{1}{2}$$
$$\begin{aligned} w(s_t) = q_{go}(s_t) - q_{nogo}(s_t) \ + \ GoBias_{subject} \\ + \ Pavbias_{valence,subject} \times value(s_t) \end{aligned} \tag{5.1}$$

Go/No-Go Model: Instrumental Learning

$$
\begin{aligned}
q_{response,t+1}(s_t) \\
&= q_{response,t}(s_t) \\
&+ \; LearningRate_{valence,subject} \times (Sensitivity_{valence,subject} \\
&\times outcome - \; q_{response,t}(s_t))
\end{aligned}
\tag{5.2}
$$

Go/No-Go Model: Pavlovian Learning

$$
\begin{aligned}
value_{t+1}(s_t) = value_t(s_t) \\
&+ LearningRate_{valence,subject} \times (Sensitivity_{valence,subject} \\
&\times outcome - value_t(s_t))
\end{aligned}
\tag{5.3}
$$

Go/No-Go Model: Link Functions

$$
\begin{aligned}
\xi_{subject} &= \Phi\left(raw\_\xi_{subject}\right) \\
LearningRate_{valence,subject} &= \Phi\left(raw\_LearningRate_{valence,subject}\right) \\
Sensitivity_{Reward,subject} &= e^{raw\_Sensitivity_{Reward,subject}} \\
Sensitivity_{Punishment,subject} &= e^{raw\_Sensitivity_{Punishment,subject}}
\end{aligned}
\tag{5.4}
$$

Go/No-Go Model: Priors

$$
\begin{aligned}
GoBias_{subject} &\sim Normal(\mu_{GoBias}, \sigma_{GoBias}) \\
PavBias_{subject} &\sim Normal(\mu_{PavBias}, \sigma_{PavBias}) \\
raw\_\xi_{subject} &\sim Normal\left(\mu_{\xi}, \sigma_{\xi}\right) \\
raw\_LearningRate_{valence,subject} &\sim Normal(\mu_{LR}, \sigma_{LR}) \\
raw\_Sensitivity_{Reward,subject} &\sim Normal\left(\mu_{Reward\_sens}, \sigma_{Reward\_sens}\right) \\
raw\_Sensitivity_{Punishment,subject} &\sim Normal(\mu_{Punishment\_sens}, \sigma_{Punishment\_sens})
\end{aligned}
$$

$$\mu_{GoBias} \sim Normal(0,1.5) \qquad \sigma_{gobias} \sim Exponential(0.8)$$
$$\mu_{PavBias} \sim Normal(0,2) \qquad \sigma_{PavBias} \sim Exponential(0.5)$$
$$\mu_{\xi} \sim Normal(-0.5,0.5) \qquad \sigma_{\xi} \sim Exponential(1)$$
$$\mu_{LR} \sim Normal(0,1) \qquad \sigma_{LR} \sim Exponential(1)$$
$$\mu_{Reward\_Sens} \sim Normal(0,0.3) \qquad \sigma_{Reward\_Sens} \sim Exponential(1)$$
$$\mu_{Punishment\_Sens} \sim Normal(0,0.3) \qquad \sigma_{Punishment\_Sens} \sim Exponential(1)$$

(5.5)

Number Switching Task model

For the Number Switching Task we fitted Model 4 from our previous analysis, which contained varying intercepts and varying linear effects of reward and effort. This model is set out in Equation 5.6 below, where $y_{subject,trial} \in \{0,1\}$ is the choice of a particular subject on a particular trial to accept or reject the challenge, $\alpha_{subject}$ is a participant-level intercept parameter and $\beta_{reward,subject}$ and $\beta_{effort,subject}$ are participant-level reward and effort sensitivities respectively.

$$Y_{subject,trial} \sim \text{Bernoulli}(p_{subject,trial})$$
$$p_{subject,trial} = \text{logistic}(\alpha_{subject} + \beta_{reward,subject} reward_{trial} + \beta_{effort,subject} effort_{trial})$$
$$\alpha_{subject} \sim Normal(\mu_{\alpha}, \sigma_{\alpha})$$
$$\mu_{\alpha} \sim Normal(0, 1.5)$$
$$\sigma_{\alpha} \sim Exponential(2)$$
$$\beta_{reward,subject} \sim Normal(\mu_{reward}, \sigma_{reward})$$
$$\mu_{reward} \sim Normal(0,1)$$
$$\sigma_{reward} \sim Exponential(2)$$
$$\beta_{effort,subject} \sim Normal(\mu_{effort}, \sigma_{effort})$$
$$\mu_{effort} \sim Normal(0,1)$$
$$\sigma_{effort} \sim Exponential(2)$$

(5.6)

We then continued with the rest of our preregistered analysis: after initial diagnostic checks, examination of the models and their posterior predictions, we extracted the participant-level Pavlovian bias parameters from the Go/No-Go model, and the intercept, effort sensitivity and reward sensitivity parameters from the NST model, and computed the mean for each participant. Finally we analysed the Pearson correlations between the parameters from the two models. We had specifically preregistered only our hypothesis that there would be a significant association between the Pavlovian biases and effort sensitivity; the other correlations (involving the NST intercept and reward sensitivity) were therefore investigated on an exploratory basis.

The final preregistered hypothesis involved examining the associations between the Pavlovian bias parameters from the model of the Go/No-Go task and the two symptom scales (Zung Depression Scale and State-Trait Anxiety Scale). We predicted that there would be significant correlations between Pavlovian biases and symptom severity. We also looked at the associations between the other model parameters for both tasks and the symptom scales on an exploratory basis.

5.4 Results

5.4.1 Number Switching Task

5.4.1.1 Model agnostic analyses

The proportion of offers accepted at each level of reward and effort are plotted in Figure 5.1. As expected, these show a significant reward-by-effort interaction, $F(9, 5211) = 24.5$, $p < .001$, $\eta^2{}_p = 0.04$, with participants progressively discounting the value of an offer as the effort level increased, but this discounting becoming progressively shallower as the reward level increased. Despite this flattening, the effort effect was still significant at every reward level in *post hoc* ANOVAs (all *p*s < .001). The main effects of reward and effort were also both significant, $F(3,1737) = 343$, $p < .001$, $\eta^2{}_p = 0.37$, and $F(3,1737) = 100$, $p < .001$, $\eta^2{}_p = 0.15$ respectively. Full descriptive statistics are presented in Table S5.1.

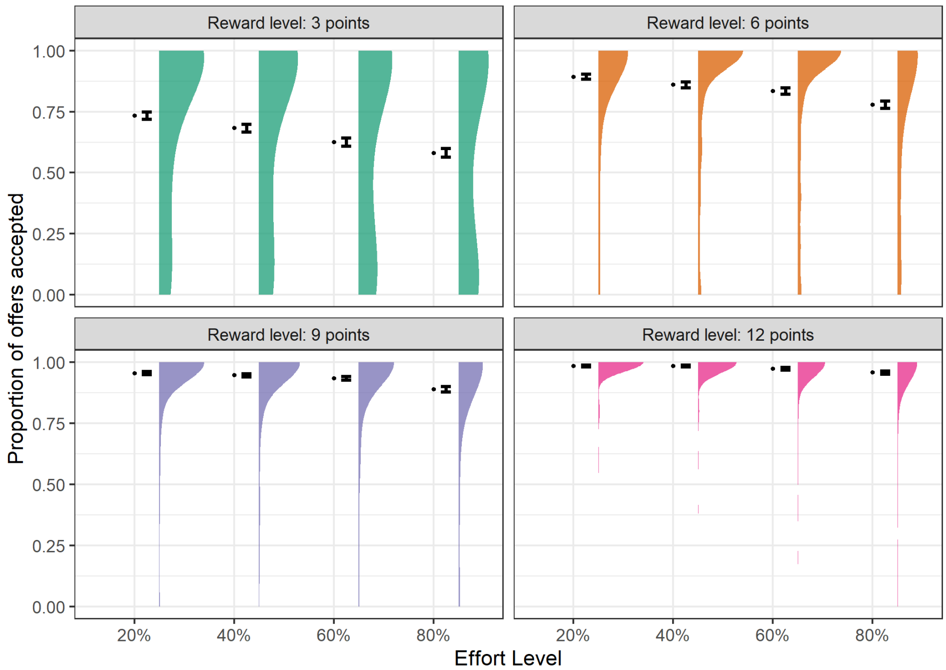


Figure 5.1. Number Switching Task: proportion of offers accepted. There is a clear effort discounting effect which becomes shallower as the reward level increases, replicating the effect seen in Chapter 4. Plot includes the mean, standard error and distribution of the proportion of offers accepted for each combination of reward and effort level.

Next we carried out an ANOVA on the success rates. We found there was a significant reward effect, $F(1, 8090) = 5.23$, $p = .02$, $\eta^2_p = 0.02$, with sequential *post hoc* *t*-tests on the complete cases data showing a significant increase in success rates from 3 points to 6 points, $t(536) = 3.01$, $p = .008$, $d = 0.13$, but not between 6 and 9, or 9 and 12 points (*p*s = 0.11 and 1 respectively; Bonferroni-adjusted for multiple comparisons). Descriptive statistics for this effect are given in Table 5.3.

Both the main effect of effort and the interaction between reward and effort were, however, non-significant, $p = .30$ and .66 respectively. This was in line with our preregistered hypothesis that the effort manipulation would not affect the rates of success on this task. These results are plotted in Figure 5.2 and full descriptive statistics are given in Table S5.2.

Table 5.3. Number Switching Task: Descriptive statistics for the proportion of trials completed successfully (across reward levels). Note these data only include complete cases.

	P(Success)	
Reward (points)	**N**	**Mean (*SD*)**
3	536	0.86 (0.19)
6	536	0.88 (0.14)
9	536	0.89 (0.13)
12	536	0.89 (0.13)

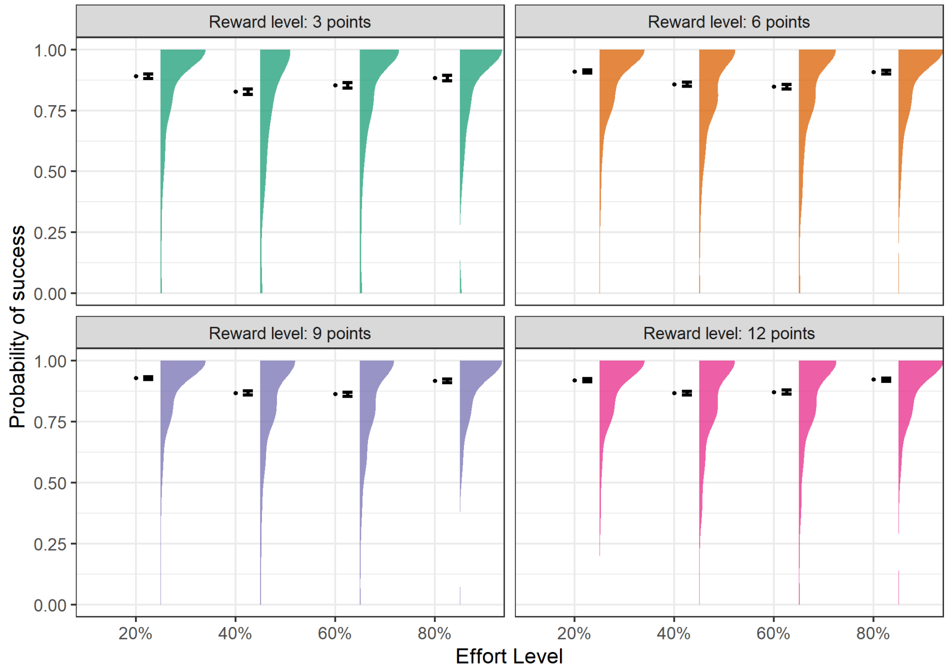


Figure 5.2. Number Switching Task: proportion of trials completed successfully. There was a significant reward effect only, which again replicates the effect seen in Chapter 4. Specifically, success rates increased between 3 and 6 points, but not between the other levels. Plot shows the mean, standard error and distribution for each combination of reward and effort level. Trials were marked as 'correct' if they were completed within the allowed time, with no more than one error.

Finally, we also examined the proportional completion times – these are plotted in Figure 5.3. We observed a significant main effect of effort level, $F(1, 8060) = 207$, $p < .001$, $\eta^2_p = 0.46$, with the completion times becoming consistently longer as effort level increased (all *ps* < .001, Bonferroni-adjusted for multiple comparisons, and *ds* = 1.2, 0.43, 0.48 respectively). Descriptive statistics for this effect are given in Table 5.4. The remaining main effect of reward level and the reward by effort interaction were both non-significant, *ps* = .60 and .31 respectively. Full descriptive statistics are given in Table S5.3.

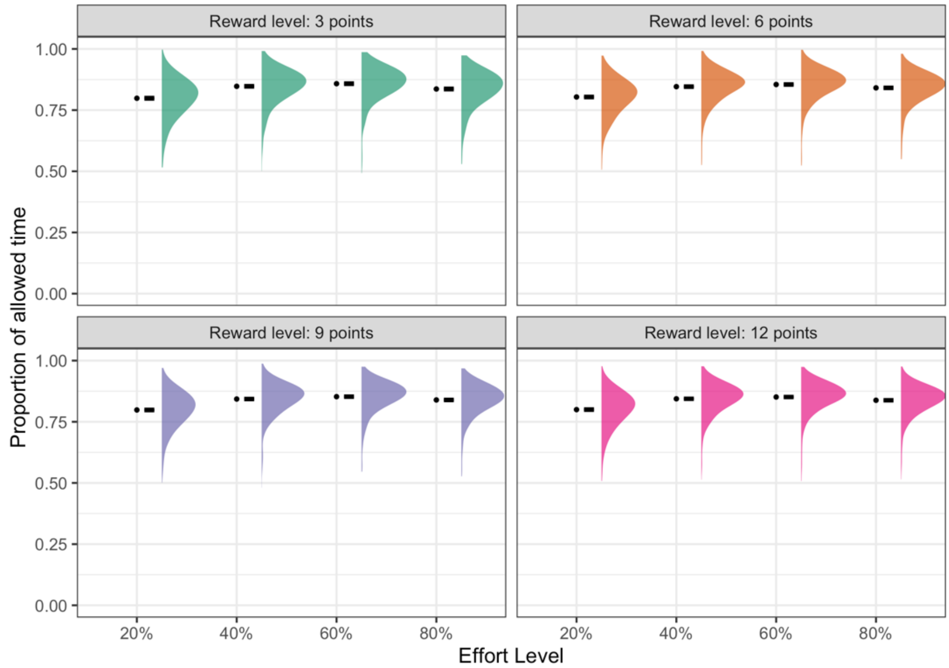


Figure 5.3. Number Switching Task: completion time. Participants completed the trials more slowly as effort level increased (replicating the result from Chapter 4), but there was no change with reward level. Plot shows the mean, standard error and distribution of the completion times (expressed as a proportion of each participant's allowed time) for each level of reward and effort level.

Table 5.4. Number Switching Task: Descriptive statistics for the proportional completion time (across effort levels). Note these data only include complete cases.

P(Success)		
Effort Level	**N**	**Mean (*SD*)**
20%	574	0.80 (0.07)
40%	574	0.84 (0.06)
60%	574	0.85 (0.06)
80%	574	0.84 (0.06)

5.4.1.2 Computational modelling

Next we fitted the computational model (see Section 5.3.4.2 above for description) to the Number Switching Task data. The posterior predictions of the model are plotted in Figure 5.4, alongside the observed data for comparison. The model provides a good fit to the data, capturing both the mean pattern of effects and the changes in the variability in the distribution of participants well (compare also with Figure 5.1, the observed proportion of offers accepted).

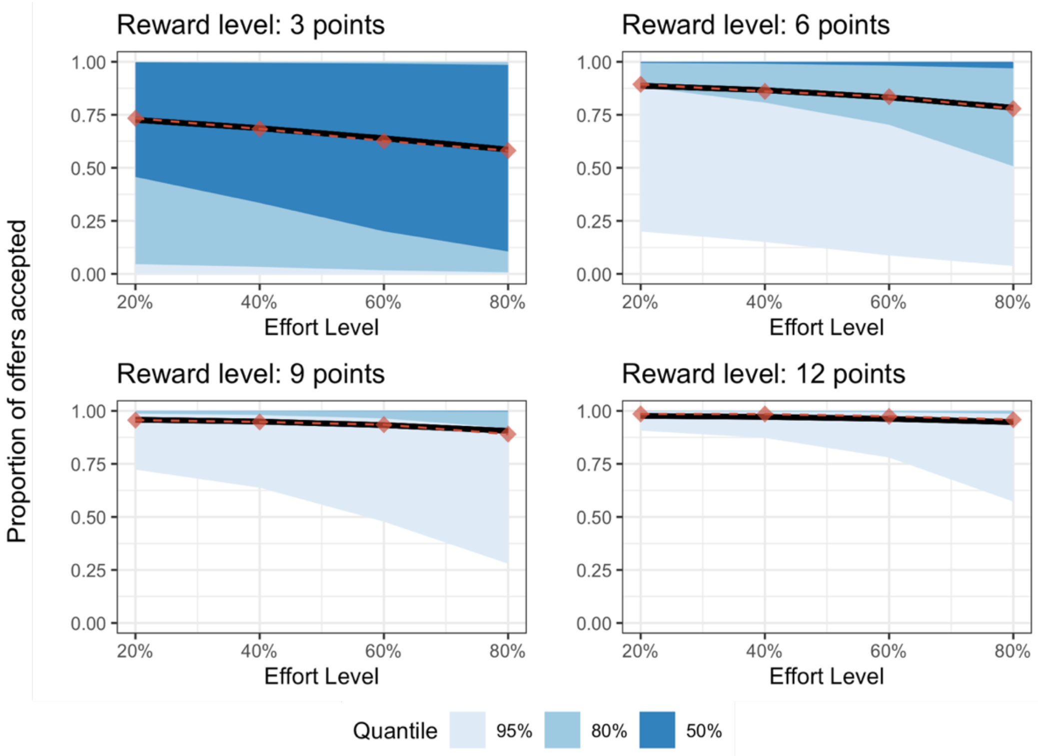


Figure 5.4. Number Switching Task: Posterior predictions of the proportion of offers accepted at each reward and effort level. The solid line shows the posterior mean and the shaded regions the highest density continuous intervals (95%, 80% and 50%), while the red diamonds and dashed lines show the empirical data for comparison. Note that these are predictions for simulated new participants and therefore incorporate uncertainty not just about the average effect of the manipulations in the population, but also about the behaviour of individual participants.

5.4.2 Go/No-Go Task

5.4.2.1 Model agnostic analyses

As expected, there was a significant action by valence interaction on the accuracy for the Go/No-Go Task, $F(1, 579) = 621$, $p < .001$, $\eta^2{}_p = 0.52$, indicating the presence of Pavlovian biases. Specifically, accuracy on 'go to win reward' trials was greater than 'go to avoid punishment', $t(579) = 23.6$, $p < .001$, $d = 0.98$; conversely accuracy on the 'no-go to win reward' trials was lower than 'no-go to avoid punishment', $t(579) = 20.0$, $p < .001$, $d = 0.83$. Descriptive statistics are provided in Table 5.5 and plotted in Figure 5.5.

We also observed significant main effects of action, $F(1, 579) = 771$, $p < .001$, $\eta^2{}_p = 0.57$, and valence, $F(1, 579) = 49.4$, $p < .001$, $\eta^2{}_p = 0.08$. Accuracy was higher when participants were required to make a go response ($M = 0.75$, $SD = 0.17$) compared with no-go ($M = 0.43$, $SD = 0.26$), indicating the presence of an overall 'go bias'; and it was also higher when the incentive involved avoiding punishment ($M = 0.61$, $SD = 0.15$) as opposed to gaining reward ($M = 0.57$, $SD = 0.35$).

Table 5.5. Accuracy across each of the four trial types, for the Baseline session only.

Trial Type	**Accuracy Mean (*SD*)**
Go to win reward	0.83 (0.18)
Go to avoid punishment	0.67 (0.29)
No-go to win reward	0.31 (0.13)
No-go to avoid punishment	0.55 (0.15)

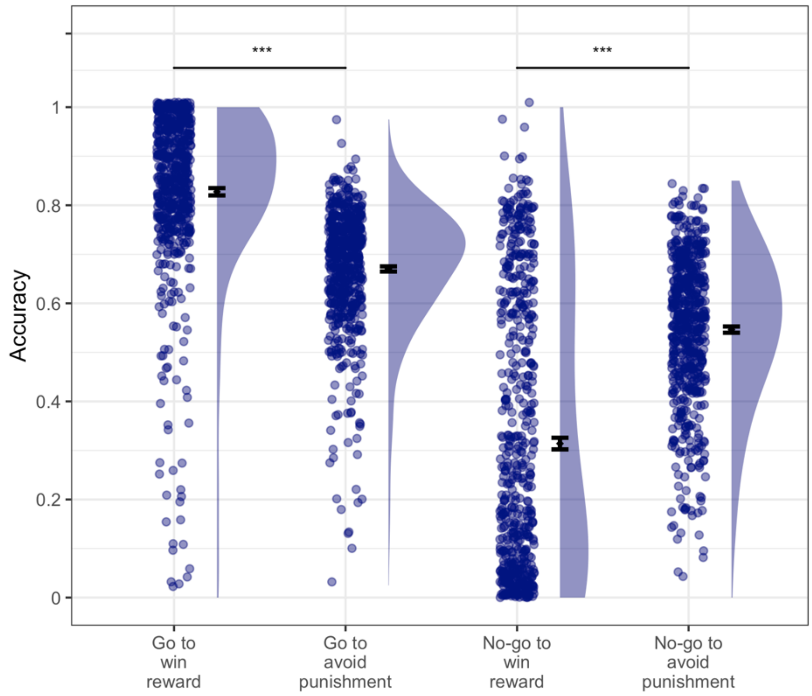


Figure 5.5. Performance on the Orthogonal Go/No-Go Task. The interaction between required action and outcome valence indicates the presence of Pavlovian biases, and replicates previous studies (see Chapters 2 and 3, and also Guitart-Masip et al., 2011) . Plot shows the individual performance, the mean±SE and the distribution for each trial type. (*** *p* < .001)

5.4.2.2. Computational modelling

Next we fitted the reinforcement learning model (described in Section 5.3.4.2 above) to the Go/No-Go data. The trialwise observed data and posterior predictions are plotted in Figure 5.6 below. Although the model roughly captures the trends in the data, it appears to be overestimating the probability of a go response (especially in the go to avoid punishment condition) and perhaps also underestimating the strength of the Pavlovian biases. In line with this, we note from the posterior parameter estimates (Figure S5.1) that the population-level go bias is estimated at a log-odds of approximately 5 (which equates to a go probability of 99%), while the Pavlovian bias parameter is apparently negative, at –0.3.

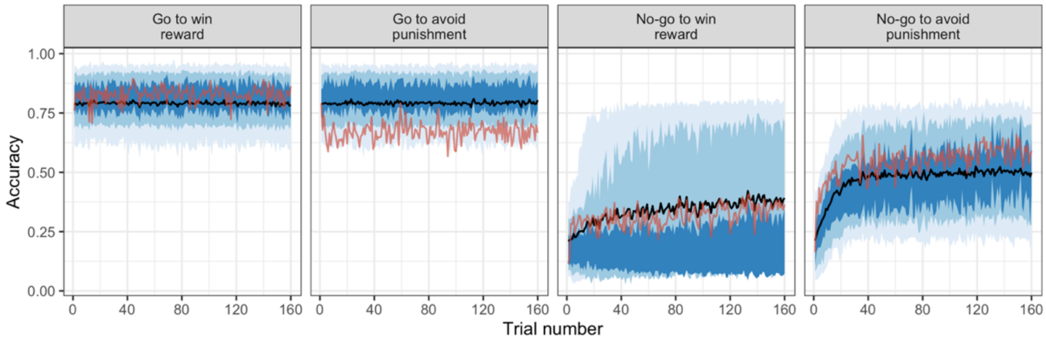


Figure 5.6. Go/No-Go Task: Posterior predictions of accuracy across each trial type. The black line shows the posterior mean and the shaded regions the posterior highest density continuous intervals (95%, 80% and 50%), while the red line shows the mean empirical data for comparison.

5.4.3 Correlation between Pavlovian bias and cognitive effort sensitivity

We found a significant positive correlation between the model-derived effort sensitivity and Pavlovian bias parameters, $r = 0.12$, $t(578) = 2.86$, $p = .004$; this is plotted in Figure 5.7a. Note that the negative values for the Pavlovian bias parameters imply that the probability of making a go response was increased when punishment was predicted and decreased when anticipating reward. This is however contingent on the values of the other model parameters, and therefore its absolute value is not necessarily meaningful in isolation. We discuss this issue in more detail in Section 5.5.3. Note also the subgroup of participants in Figure 5.7 with very similar effort sensitivity parameters, indicated by the vertical lines in the plots; we discuss this feature in the following section, 5.4.4. Finally, in contrast to the significant model-derived result, we found there was no significant association between the model agnostic measures of effort sensitivity and Pavlovian bias, $r = 0.01$, $t(578) = 0.19$, p = .85; see Figure 5.8.

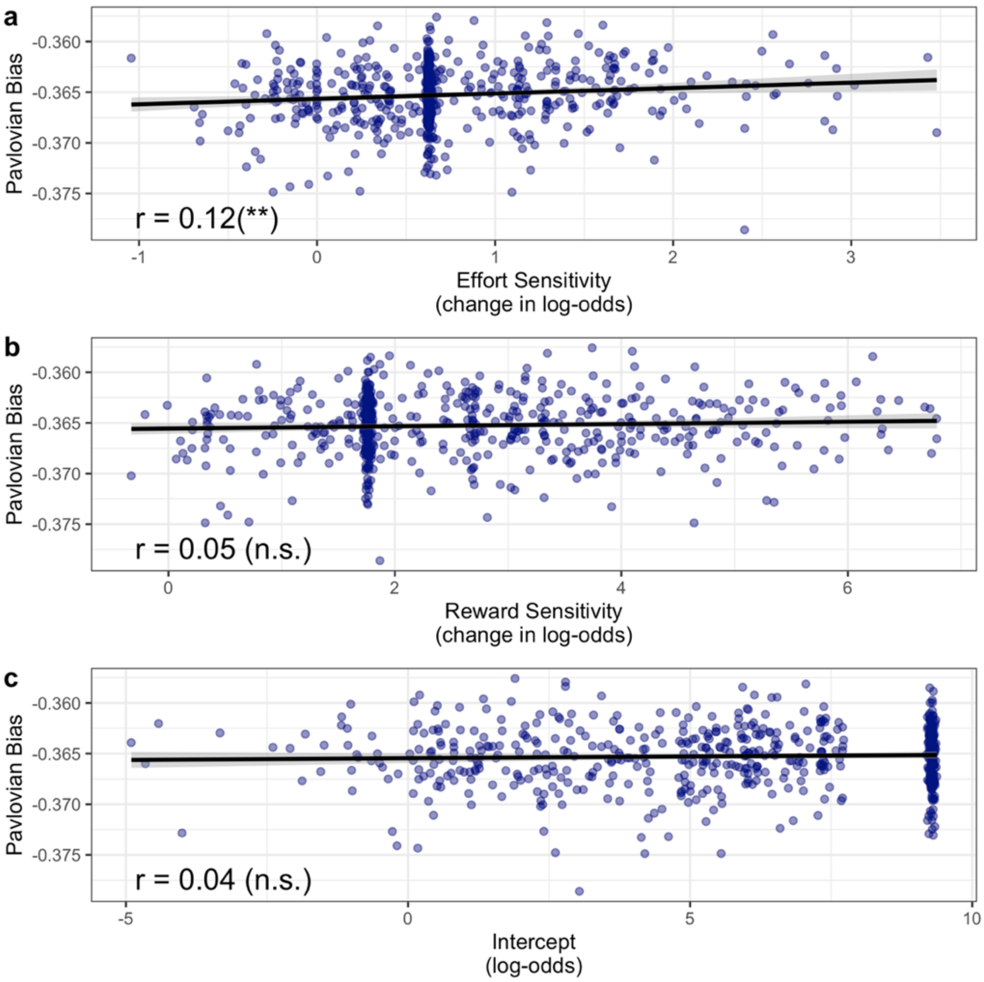


Figure 5.7. Correlations between the model derived measures of Pavlovian approach and avoidance biases and the effort sensitivity, reward sensitivity and intercept parameters from the Number Switching Task. Plots shows correlation lines with 95% confidence intervals.

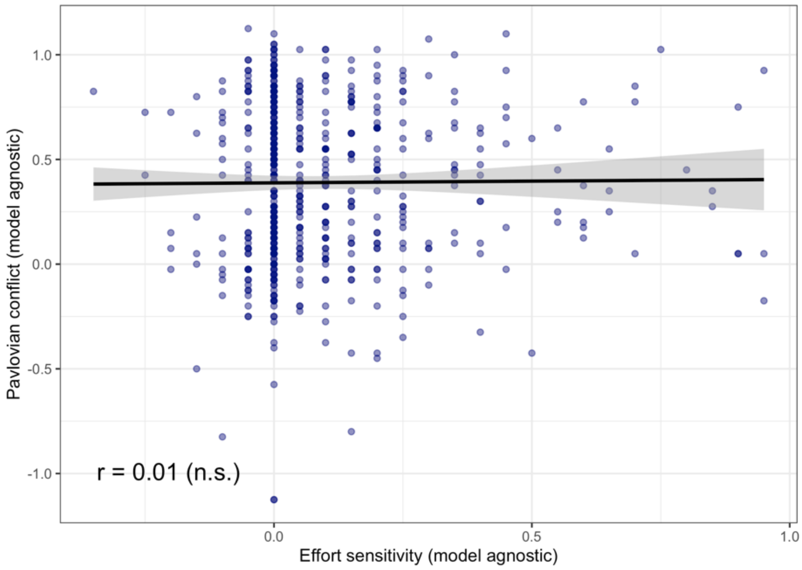


Figure 5.8. Correlation between the model agnostic measures of Pavlovian bias and effort sensitivity. Plot shows correlation line (*r* = 0.01, non-significant) with 95% confidence interval.

5.4.4 Exploratory correlations between Pavlovian bias and other NST parameters

We had no specific hypotheses about the other NST model parameters, but in a wider exploratory analysis we investigated their associations with Pavlovian bias as well. We found that there were no significant correlations either between reward sensitivity and Pavlovian bias, $r = 0.05$, $t(578) = 1.29$, $p = .19$, or the intercept parameter and Pavlovian bias, $r = 0.04$, $t(578) = 0.92$, $p = .36$. These are plotted in Figures 5.7b and 5.7c.

It is clear from looking at these plots that there seems to be a distinct subpopulation of participants who were all estimated to have the same parameters for the NST model (Figure S5.2 shows the same plot but with this subpopulation highlighted). These participants – approximately one third of the total sample – were all assigned very high intercepts, indicating they accepted essentially all of the offers regardless of reward or effort level (a log-odds of accepting an offer of 9 equates to a probability of 0.9999). To assess whether this affected the results, we conducted a sensitivity analysis in which these participants were removed. This change did not affect the significance of any of the tests, but did increase the size of the Pavlovian bias–effort sensitivity correlation slightly, to $r = 0.14$, $t[391] = 2.84$, $p = .005$.

5.4.5 Correlations between effort sensitivity and mental health symptom scales

There was a significant correlation between the model-derived effort sensitivity parameter and trait anxiety, $r = 0.09$, $t(578) = 2.15$, $p = .03$. The other correlations, between effort sensitivity and depression scores ($r = 0.06$, $t[578] = 1.53$, $p = .13$), and state anxiety ($r = 0.05$, $t[578] = 1.15$, $p = .25$), were however non-significant. These are plotted in Figure 5.9 (a, c & e). Regarding the model-agnostic effort sensitivity measure, we again found a significant association with trait anxiety, $r = 0.10$, $t(578) = 2.53$, $p = .01$, but here there was also a significant correlation with depression score, $r = 0.09$, $t(578) = 2.07$, $p = .04$. There was however no significant association between model-agnostic effort sensitivity and state anxiety, $r = 0.07$, $t(578) = 1.69$, $p = .09$. These are also plotted in Figure 5.9 (b, d & f).

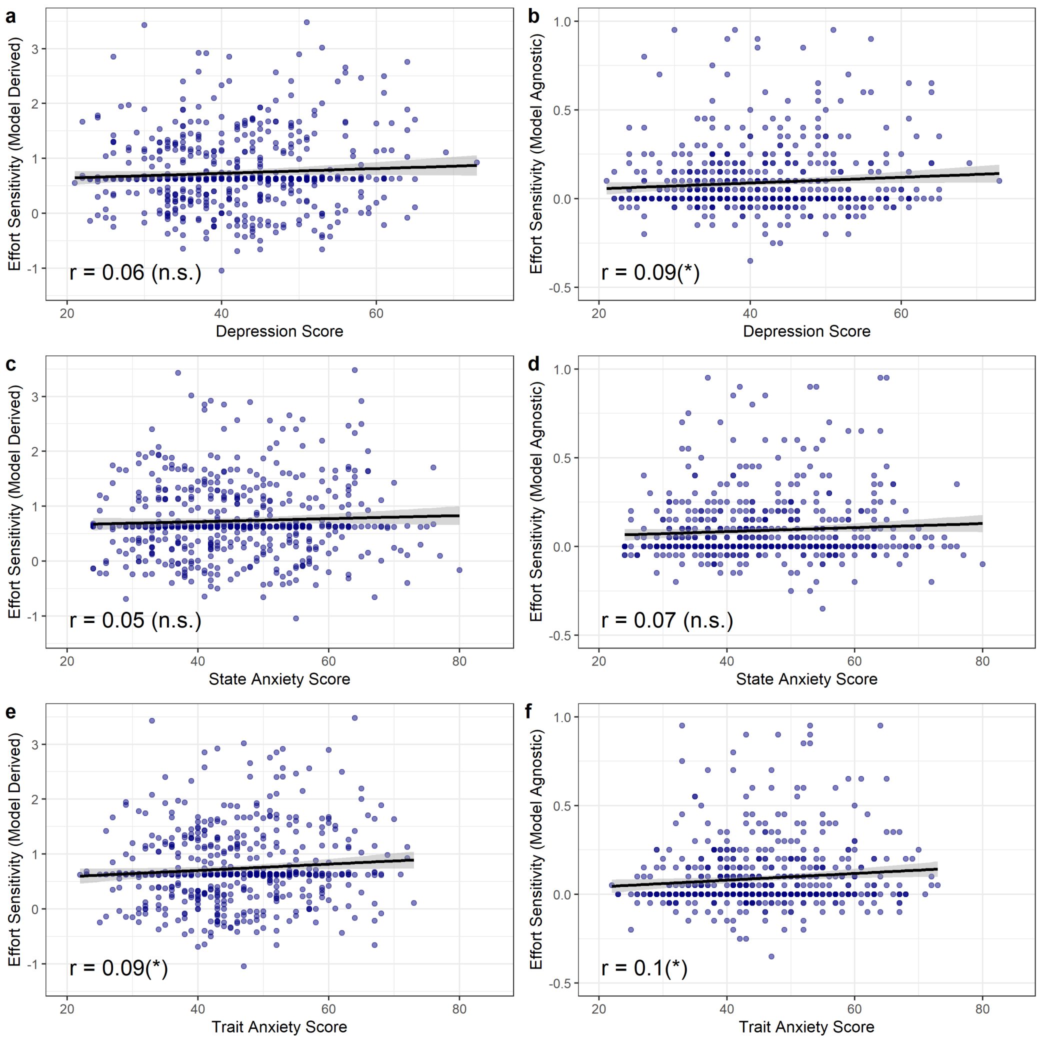


Figure 5.9. Correlations between the model-derived and model-agnostic measures of effort sensitivity, and the Zung depression scores, STAI state anxiety scores and the STAI trait anxiety scores. There were significant correlations between effort sensitivity and depression (model-agnostic only), and between effort sensitivity and trait anxiety (both).

5.4.6 Correlations between Pavlovian biases and mental health symptom scales

In our final preregistered analysis we found no significant correlations between the model-derived Pavlovian bias parameter and the depression and anxiety symptom scales. For the Zung Depression Scale, the correlation with Pavlovian bias was $r = 0.03$, $t(578) = 0.76$, $p = .45$; for state anxiety, it was $r = –0.02$, $t(578) = 0.58$, $p = .56$; and for trait anxiety it was $r = 0.01$, $t(578) = 0.12$, $p = .90$. These are plotted in Figure 5.10 (a, c & e).

In an exploratory analysis we also examined the correlations between the model-agnostic Pavlovian bias measures and the depression and anxiety symptom scales. These were again all non-significant. For the Zung Depression Scale, the association with the model agnostic Pavlovian bias measure was $r = 0.06$, $t(578) = 1.38$, $p = .17$; for state anxiety it was $r = 0.01$, $t(578) = 0.18$, $p = .86$; and for trait anxiety it was $r = 0.01$, $t(578) = 0.30$, $p = .76$. These are also plotted in Figure 5.10 (b, d & f).

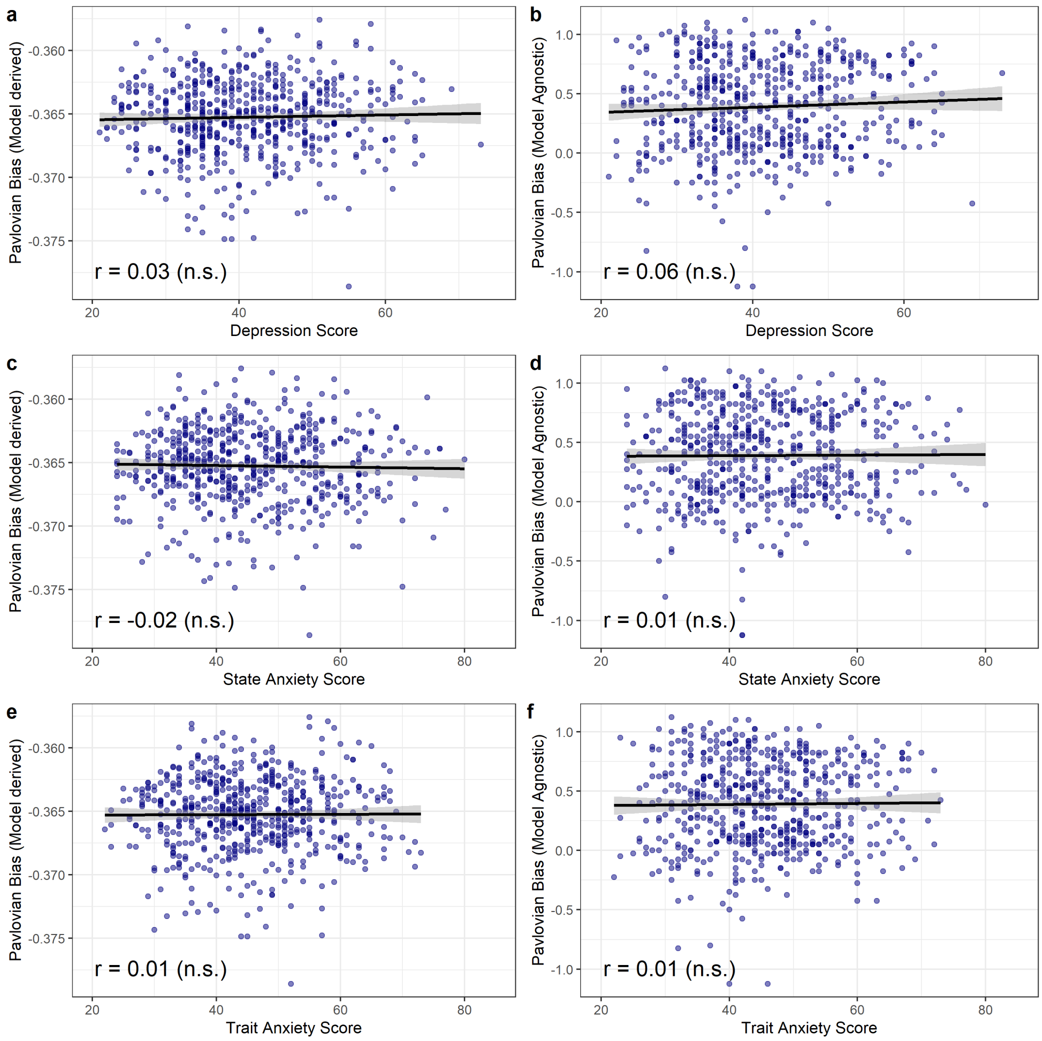


Figure 5.10. Correlations between the model-derived and model-agnostic measures of Pavlovian bias, and the Zung depression scores, STAI state anxiety scores and the STAI trait anxiety scores. There were no significant correlations.

5.5 Discussion

5.5.1 Primary hypothesis: The Pavlovian bias–effort sensitivity correlation

In this preregistered study we set out to investigate the association between Pavlovian bias and sensitivity to cognitive effort. In line with our primary hypothesis we found that there was a significant positive correlation between model-derived estimates of effort sensitivity and Pavlovian bias, indicating that participants who were more sensitive to cognitive effort had higher, i.e. less negative, Pavlovian biases (see further discussion of exactly how this result should be interpreted in Section 5.5.3 below). This is important because it is consistent with the suggestion that people can control their Pavlovian biases (Cavanagh et al., 2013), and that this control is dependent on exerting effort (Shenhav et al., 2017). This gives us a much better insight into the cognitive mechanism underlying the expression of Pavlovian biases, and in particular suggests we should think of Pavlovian biases in terms of effort-based decision-making: the strength of these biases depends not just on the Pavlovian learning system itself, but also economic consideration of the costs of trying to control and overcome them (Westbrook & Braver, 2015).

A further implication of this result is that the control mechanism must be to some extent domain general, in so far as effort sensitivity seems to have influenced performance on two tasks, the Go/No-Go Task and the NST. This may appear self-evident but nevertheless it is significant, in that it suggests that studying how Pavlovian biases are controlled may inform our understanding of control over other aspects of cognition and performance too. For example, future efforts to address the cognitive symptoms of conditions like anxiety and depression may benefit from targeting parameters which have effects across multiple cognitive processes, like effort sensitivity (Husain & Roiser, 2017).

Finally, this correlation between effort sensitivity and the strength of Pavlovian biases may also help to inform interpretation of our earlier study (Chapter 3). In that study we trained participants on the high Pavlovian-instrumental conflict trials and found that, after training, their Pavlovian biases were reduced compared with a group who did a programme of sham training. A plausible mechanism for this training effect is that participants learned that exerting control was worthwhile and therefore the expected value of their effort increased (*cf.* the Expected Value of Control theory, Shenhav et al., 2013), in turn meaning they were more willing to exert control.

Despite these positive results, we should also strike a note of caution, however, as the positive correlation between the effort sensitivity and Pavlovian bias model parameters was not replicated by the parallel model agnostic analysis. In theory we would expect the computational modelling to be more sensitive, and therefore it is not entirely surprising if the two do not agree; nevertheless this discrepancy indicates that further investigation is needed at least (see Limitations, Section 5.5.3 below).

5.5.2 Secondary hypotheses

In addition to our primary analysis of the relationship between effort sensitivity and Pavlovian bias, we also looked at the correlations between Pavlovian bias, the NST parameters and the three self-report symptom scales. Contrary to our hypothesis, we found that Pavlovian bias was not significantly associated with any of the self-report scales, whether using the model-derived or model-agnostic measures of bias. On the one hand this runs somewhat counter to results from previous studies which had found an association between Pavlovian avoidance bias in particular and both anxiety (Mkrtchian, Aylward et al., 2017) and depression (Nord et al., 2018); on the other, it matches the results of our earlier study reported in Chapter 3, in which we also found no correlation. As we also discussed there, two possibly significant differences between the previously published studies and our own are that we have been testing symptom scores in the healthy population, whereas the significant results were found in clinical samples; in addition, Mkrtchian et al. were also studying avoidance biases potentiated by threat of shock, a state anxiety manipulation. Possibly the enhanced avoidance bias only emerges in people meeting the clinical thresholds for diagnosis and/or in the presence of additional stress.

In exploratory tests we nevertheless found that there were significant correlations between effort sensitivity and both trait anxiety (consistent across the model-derived and model-agnostic measures) and also depression symptoms (model-agnostic only). This is an important result for two reasons: it demonstrates the utility of the NST (which had been developed explicitly for this kind of study), proving that the task is able to reveal meaningful variation between participants that can be related to other aspects of cognition; and more generally it supports the idea that effort-based decision-making is an important factor in the symptoms of a number of mental health conditions (Husain & Roiser, 2017). This latter point gives weight to another argument running through this thesis, that effort (and the cognitive mechanisms underlying it) should be a principal target of research aimed at treating the cognitive symptoms of conditions like anxiety and depression.

Finally, we also replicated some of the key results from our previous study of the Number Switching Task (see Chapter 4), most importantly that there was no significant effect of the effort manipulation on rates of success, even with roughly double the sample size of the earlier study. This again gives us further confidence in the NST by showing that the results reflect genuine effort discounting, without confounding by probability discounting.

5.5.3 Limitations

As noted above, the model-agnostic correlation between Pavlovian bias and effort sensitivity was non-significant, contradicting the results from the modelling analysis. Perhaps related to this, there were also some issues with the fit of the Go/No-Go model, specifically that it systematically overestimated the chances of making a go response, which in turn will have affected how well the Pavlovian bias parameter was estimated. As the plot of the posterior predictions shows (Figure 5.6), in the go to win reward and go to avoid punishment conditions, the model predicts that accuracy will be stable at around 75-80% from the very beginning of the session, with essentially no effects of learning – this is a consequence of the posterior estimates of the mean go bias and mean noise parameters of 5 and –0.2 respectively, which together give a go probability of 78%. It therefore seems that these two parameters are dominating the model and preventing the learning processes from having any influence on the outcomes.

The overestimation of the go bias and noise parameters seems in turn to have resulted in underestimation of the Pavlovian bias parameter, which was apparently not just small but negative. This value seems unreasonable given clear, positive Pavlovian bias is evident in the empirical data and model agnostic results (Section 5.4.2.1). It also complicates the interpretation of the positive effort sensitivity–Pavlovian bias correlation because it is unclear whether, as effort sensitivity increases, Pavlovian biases become stronger (since the value of the parameter is higher) or weaker (because the value of the parameter is closer to zero). Of these, the former interpretation is probably the more reliable one, because although the model has underestimated the Pavlovian bias parameters it is still possible that the difference between participants is meaningful, whereas it seems fairly certain that the absolute value is not correct. In any case this issue necessarily limits the confidence we can have in the effort sensitivity–Pavlovian bias correlation.

The underestimation of the Pavlovian bias parameters is particularly difficult to understand, given we can see in Figure 5.6 that the model would produce better predictions in three out of the four conditions (all except no-go to win reward) if the Pavlovian bias estimates were higher. In the no-go to win reward condition, the mode trajectory is below the empirical mean, but there is a long upwards tail, reflecting the observation (also noted in Chapter 3) that there is substantial variability in how well participants are able to learn this trial type, and possibly even a bimodal pattern of responses. This may provide an explanation for the issues identified here: the difficulty in fitting the no-go to win trial type may have led the model to a peculiar set of parameter estimates that may fit the data in aggregate, but is not necessarily cognitively meaningful. Despite these limitations, we decided that it was important that the models used in this analysis were directly comparable both with the earlier chapters in this thesis and also with the previously published literature, and so we still chose to use the *Base plus two learning rates* model for the Go/No-Go Task here. In future work it would perhaps be useful to study the two datasets (from this chapter and from Chapter 3) in parallel (rather than in series, as was the case here), in order to try to identify a common model that can accommodate both.

5.5.4 Future research

A priority for the future should be to try to better understand the modelling of the Go/No-Go Task; in particular, a concrete step would be to investigate whether new model structures, especially those which allow for bimodal distributions (so that some participants are able to learn over time and others are not) lead to improved model fit. This could be implemented through a mixture modelling approach, for example. With both improved understanding of the models that we have, and development of more sophisticated models in the future, we should be better placed to be more confident in the results of studies like this one.

More broadly, part of the motivation for this study was to demonstrate a link between the strength of Pavlovian biases and willingness to exert effort, in order to better understand the nature of the training effect we observed in Chapter 3. In this study, we were able to identify a possible association (bearing in mind of course the issues and caveats mentioned above), but we have not directly tested its role in mediating the training effect observed in the earlier study. This would be an important subject for a future study, particularly if we look to develop the behavioural training idea further with the aim of enhancing cognitive effort more generally.

Finally, the positive correlations between effort sensitivity and the depression and anxiety symptom scales were results of an exploratory analysis. Further replication is therefore needed to strengthen the inferences we can draw from these results.

5.5.5 Conclusions

In this study we identified a potential association between the model-derived measures of effort sensitivity and Pavlovian bias, in line with our hypothesis that overcoming and reducing the strength of these biases depends on effortful cognitive control. We should however be careful not to overstate this point given some of the difficulties we identified with the fit of the model – further validation of this result is likely to be required. In addition we also observed a number of significant associations between cognitive effort sensitivity and depression and anxiety symptoms which are more dependable; subject to the need for further replication, they are consistent with the emerging view that effort-based decision-making is an important component in depression and anxiety. Overall, while taking our caveats into account, these results are consistent with our hypotheses about the links between Pavlovian bias, effort and anxiety and depression symptoms.

5.6 Appendix

5.6.1. Supplementary results

5.6.1.1 Number Switching Task: Descriptive Statistics

Table S5.1. Number Switching Task: Proportion of Offers Accepted.

P(accept)		
Reward (points)	**N**	**Mean (*SD*)**
3	580	0.66 (0.37)
6	580	0.84 (0.26)
9	580	0.93 (0.16)
12	580	0.98 (0.09)
Effort level		
20%	580	0.89 (0.17)
40%	580	0.87 (0.18)
60%	580	0.84 (0.20)
80%	580	0.80 (0.24)
Reward: Effort		
3: 20%	580	0.73 (0.36)
3: 40%	580	0.68 (0.39)
3: 60%	580	0.63 (0.41)
3: 80%	580	0.58 (0.43)
6: 20%	580	0.89 (0.25)
6: 40%	580	0.86 (0.28)
6: 60%	580	0.84 (0.30)
6: 80%	580	0.78 (0.34)
9: 20%	580	0.96 (0.15)
9: 40%	580	0.95 (0.17)
9: 60%	580	0.93 (0.19)

9: 80%	580	0.89 (0.25)
12: 20%	580	0.99 (0.07)
12: 40%	580	0.99 (0.09)
12: 60%	580	0.97 (0.12)
12: 80%	580	0.96 (0.16)

Table S5.2. Number Switching Task: Proportion of trials completed successfully.

P(success)		
Reward (points)	**N**	**Mean (*SD*)**
3	537	0.86 (0.19)
6	571	0.88 (0.15)
9	578	0.89 (0.13)
12	580	0.90 (0.12)
Effort level		
20%	580	0.91 (0.12)
40%	580	0.86 (0.15)
60%	580	0.86 (0.17)
80%	575	0.91 (0.14)
Reward: Effort		
3: 20%	516	0.89 (0.21)
3: 40%	491	0.83 (0.26)
3: 60%	463	0.85 (0.24)
3: 80%	426	0.88 (0.22)
6: 20%	560	0.91 (0.17)
6: 40%	550	0.86 (0.20)
6: 60%	545	0.85 (0.23)
6: 80%	526	0.91 (0.17)

9: 20%	574	0.93 (0.14)
9: 40%	575	0.87 (0.20)
9: 60%	571	0.86 (0.20)
9: 80%	555	0.92 (0.17)
12: 20%	580	0.92 (0.15)
12: 40%	578	0.87 (0.19)
12: 60%	578	0.87 (0.20)
12: 80%	572	0.92 (0.15)

Table S5.3. Number Switching Task: Completion time.

Proportional completion time		
Reward (points)	**N**	**Mean (*SD*)**
3	536	0.83 (0.07)
6	570	0.84 (0.06)
9	578	0.83 (0.06)
12	580	0.83 (0.06)
Effort level		
20%	580	0.80 (0.07)
40%	580	0.85 (0.06)
60%	579	0.85 (0.06)
80%	575	0.84 (0.06)
Reward: Effort		
3: 20%	514	0.80 (0.08)
3: 40%	488	0.85 (0.07)
3: 60%	459	0.86 (0.07)
3: 80%	422	0.84 (0.07)
6: 20%	559	0.80 (0.07)

6: 40%	548	0.85 (0.07)
6: 60%	543	0.85 (0.07)
6: 80%	525	0.84 (0.06)
9: 20%	574	0.80 (0.07)
9: 40%	575	0.84 (0.07)
9: 60%	571	0.85 (0.06)
9: 80%	554	0.84 (0.06)
12: 20%	580	0.80 (0.07)
12: 40%	577	0.84 (0.06)
12: 60%	576	0.85 (0.06)
12: 80%	572	0.84 (0.06)

5.6.1.2 Go/No-Go Modelling: Population-level parameter estimates

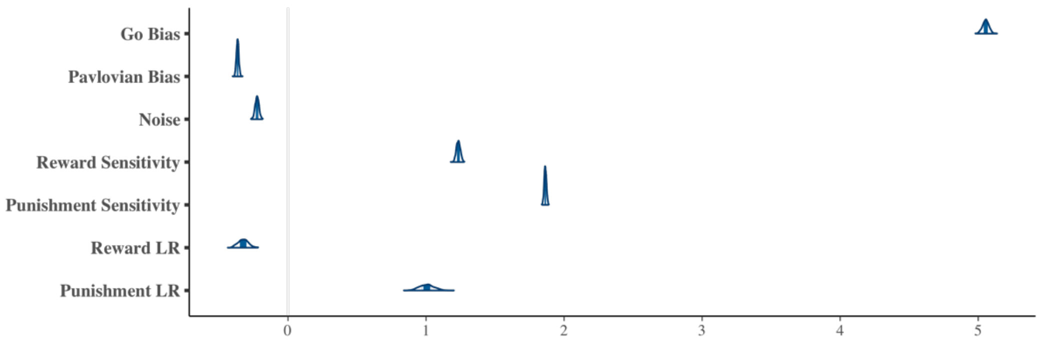


Figure S5.1 Go/No-Go Model: Posterior estimates of the population-level mean parameters. Plots show distributions and shaded 50% intervals.

5.6.1.3 Pavlovian Bias–Effort Sensitivity Correlation: Sensitivity analysis

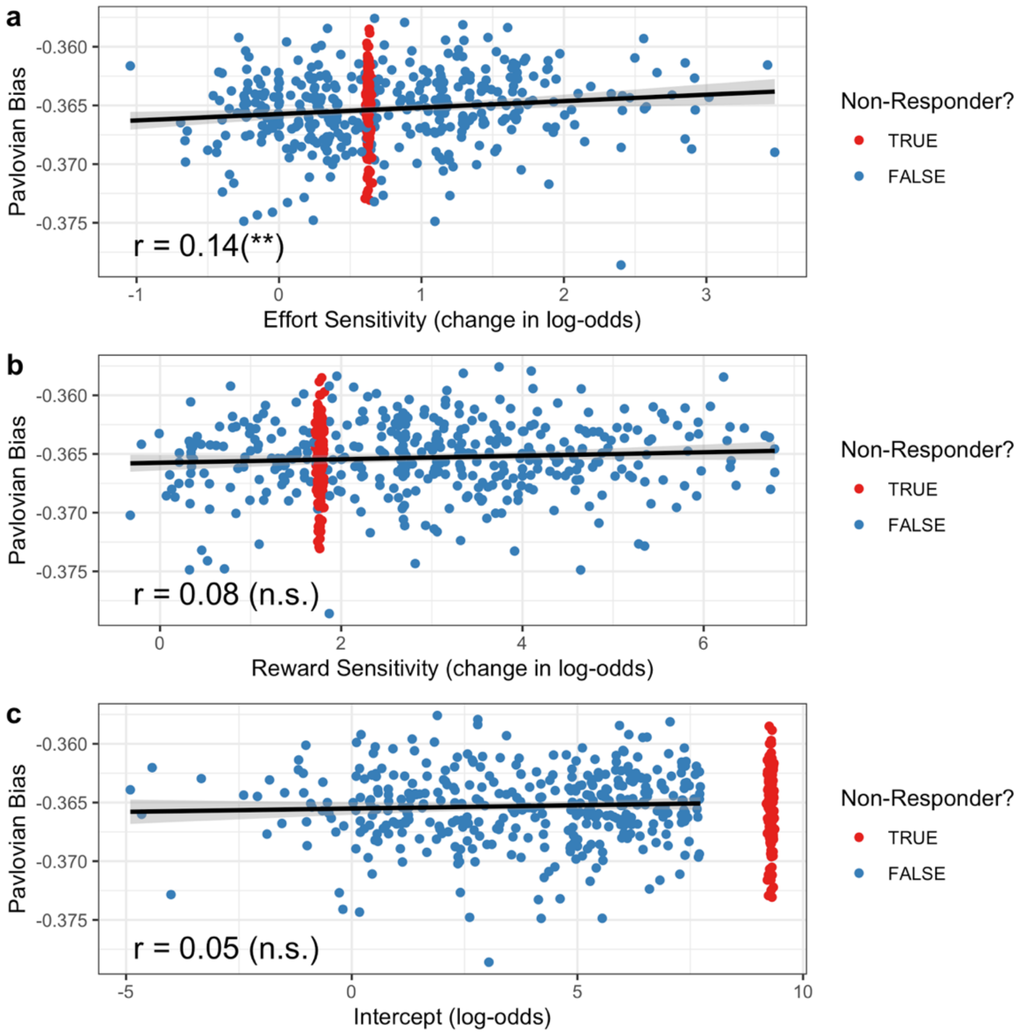


Figure S5.2. Correlations between the model derived measures of Pavlovian approach and avoidance biases and the effort sensitivity, reward sensitivity and intercept parameters from the Number Switching Task. Here, a distinct subpopulation of non-responders – those with very high intercepts – has been identified in red. Plots show correlation line after the non-responders are removed, with 95% confidence intervals. There were no changes in the significance of any of the correlations, but the size of the effort sensitivity – Pavlovian bias correlation increased slightly.

Chapter 6. Why Is Effort Costly?

6.1 Abstract

Effort costs are an attempt to describe a ubiquitous behaviour: people routinely make choices that fail to maximise reward. We see this not only in laboratory experiments but also in wider life: people often procrastinate and seek to avoid important tasks, even those they explicitly value and want to succeed at; equally, when they do engage in an activity, they do not necessarily perform to the best of their abilities, instead seeming to hold some measure of capacity in reserve. What is happening when people fail to perform to the best of their abilities? Why would they not try as hard as possible at all times? These questions indicate the central motivating problem of cognitive effort – people systematically make choices that appear to be suboptimal, in the sense that they do not lead to the maximum expected rewards. The orthodox solution to this problem is to introduce the notion of effort costs (e.g. Kool & Botvinick, 2010; Shenhav et al., 2013). It is suggested that actions and thoughts must have a hidden cost, and that this cost trades off with the overt reward. In this sense, it is argued, people's observed behaviour is not maladaptive after all, and is consistent with people making choices that maximise their expected *net* reward (Westbrook & Braver, 2015). However, precisely what is costly about cognition, or even action, is not fully known, meaning in a sense the central problem identified above has still not been satisfactorily answered. In this chapter I will discuss and apply two separate ideas—Ergodicity Economics (Peters, 2017) and Landauer's Principle (Landauer, 1961)— originally developed outside of neuroscience, which I nevertheless think have significant potential to explain apparent cognitive effort costs. On the one hand, Ergodicity Economics suggests that a number of effort-like phenomena can be accounted for by optimal decision-making, without recourse to intrinsic effort costs; on the other hand, Landauer's Principle suggests there is one cost that *is* obligatory, that of dissipating energy whenever information is erased. Together these ideas challenge and extend our existing understanding of cognitive effort.

6.2 Introduction

Cognitive effort is a common experience – many day to day cognitive tasks do not happen automatically, but instead require some degree of exertion. Colloquially, effort refers to 'how hard we try', whether we seek to do our best on a task, coast through, or anything in between. Shenhav et al. (2017, p. 101) put it more precisely: cognitive effort is "the mediating factor between cognitive capacity, on the one hand, and performance on the other".

The defining feature of cognitive effort, which makes it such an intriguing object of study, is that we do not always exert ourselves maximally. By definition, the presence of a mediator between potential and achieved performance means that we are able to choose to operate at a lower level of performance on a task than we are capable of, and this seemingly runs counter to the deeply-held assumption that humans make decisions in order to maximise expected rewards. The usual solution to this problem is to assert that cognitive effort has some hidden cost, which increases as a function of effort intensity, making high levels of effort aversive (e.g. Manohar et al., 2015; Shenhav et al., 2013).

The idea of effort costs is supported principally by empirical data which has shown that people exhibit effort discounting – as a cognitive task becomes more demanding, participants treat it as subjectively less valuable (see Westbrook et al., 2015, and Ritz et al., 2022 for reviews; see also Chapter 4 of this thesis), suggesting that rising effort demand is increasingly costly. However, these studies say nothing about the source or nature of these costs. This means we do not currently have a principled understanding of effort costs – despite a number of potential explanations having been put forward, including (amongst others) depletion of blood glucose (Gailliot & Baumeister, 2007; Gailliot et al., 2007), opportunity costs (Shenhav et al., 2017; Kurzban et al., 2010, 2013), and preventing interference from using shared computational resources (Sagiv et al., 2018; Musslick & Cohen, 2021), none has made both precise predictions and been supported by strong evidence. This is a significant problem as, at present, effort costs are regarded as being both hidden and unconstrained – in computational models of behaviour, they are typically coded as free parameters that can take any value. The result is that much of the existing cognitive effort literature relies on a circular logic: in order for participants' behaviour to be consistent with maximising expected value we assume that there are effort costs; but in order to measure these effort costs we then have to assume that participants maximise expected value. There needs instead to be a more principled way of determining, or at least constraining, estimates of effort costs.

An important further point is that cognitive effort research is increasingly taking inspiration from earlier computational work on physical effort. Two recent examples include papers from Ritz et al. (2022) and Manohar et al. (2015), both of whom present models of cognitive control, derived from optimal motor control models, in which they assume without question that costs are a quadratic function of the control signal. It should be emphasised, however, that even with regards to motor control a normative account of effort costs is still yet to be fully worked out.

There are two main explanations put forward for physical (motor) effort costs: first, movement consumes energy, and this increases with the strength of the motor signal (Shadmehr & Krakauer, 2008; Walton et al., 2006; Rigoux & Guigon, 2012); and second, larger motor signals result in greater movement noise, and specifically endpoint variability, which is costly because it increases errors (Harris & Wolpert, 1998). Neither of these explanations gives a principled rationale for assuming specifically quadratic costs, however. Harris and Wolpert, for example, use movement endpoint *variance* (σ^2) as the effort cost, but this is justified on empirical grounds only, not theoretical, so they do not address why it is variance specifically that is costly. In principle, for instance, endpoint standard deviation (σ) could also have been considered, implying linear costs of noise. There are thus still significant gaps in our understanding of effort costs, particularly in terms of identifying the exact biological or computational constraints that give rise to these costs. This is the case for both cognitive and physical costs, and we should be careful not to assume that the latter is totally understood.

In this theory focussed chapter I will be presenting a synthesis of two ideas, drawn from fields adjacent to cognitive neuroscience, which I think may help to provide a better account of cognitive costs than we have currently. First I will discuss the implications of 'Ergodicity Economics' (Peters, 2019), a new perspective on optimal decision theory, for understanding effort. The key development that Peters and colleagues have accomplished is to show that maximising expected value is not universally optimal – in fact, quite the opposite, maximising expected value is only optimal in one special case (when the change in one's wealth as a result of a decision can be represented by a stationary random variable, i.e. is ergodic). This implies that behaviour that is apparently indicative of effort costs could instead be explained by the particular reward dynamics of the environment. After providing the necessary mathematics to prove this result, I will present some specific applications of this framework to topics in cognitive neuroscience.

In the second half of this chapter, I will then describe one source of cognitive costs which *is* obligatory – heat dissipation whenever information is erased from memory. This is based on an older idea from computer science called Landauer's Principle (Landauer, 1961), which has nevertheless not (to my knowledge) been applied to cognitive effort before.

6.3 Ergodicity Economics and cognitive effort costs

Our concern in this section is whether effort costs are necessary to explain people's observed behaviour. Is it possible to account for people's failure to maximise expected rewards in any other way? More to the point, is there any sense in which not maximising expected rewards may in fact be optimal?

To illustrate the ultimate answer to these questions, it is instructive to consider a simple example. Imagine you are asked to play a game in which you gamble on a random outcome, like a coin flip. Your opponent says they will pay you back your stake plus 50% if you call heads or tails correctly, and your stake minus 40% if you are wrong. Conventional decision theory suggests that this is an easy choice: if you start with, say, £100, the expected outcome is a gain of £5[1]; thus if you choose to play you apparently stand to make a positive return compared with the alternative of not playing and winning nothing. Yet you may also have the sense that, in the real world, this is not a gamble that many people would want to make.

This is not unlike the situation we observe with effort discounting – a participant is offered the choice between doing a cognitive task, for which they may win a reward, and doing nothing, for which they are certain to win nothing. On the face of it this is a straightforward decision, as the expected value of attempting the task will always be better than doing nothing – and yet, if the 'effort demand' of the task is high enough, people often consistently choose the latter.

Table 1 shows the outcome of making the gamble proposed above. Since the probabilities of heads and tails are equal and independent over time, we can assume that in the long-time limit we will observe an equal number of each. Note that when we take this limit the order of the outcomes then does not matter. As Table 6.1 shows, despite the fact that every gamble has a positive expected value, in the long run you are guaranteed to lose money. It is optimal therefore not to

[1] You could also describe the expected outcome as a gain of +5%, but this would also be misleading about the value of the decision (though for a slightly different reason than if you computed the expected monetary gain).

choose to play this game – or, in other words, this is a situation where it is optimal to select the option with the lower expected value.

Table 6.1. Outcomes you can expect to receive if you accept the gamble

	Trial 1	Trial 2	Trial 3	Trial 4	Trial 5	Trial 6	Trial 7	Trial 8
Initial wealth	+50%	-40%	+50%	-40%	+50%	-40%	+50%	-40%
£100	£150	£90	£135	£81	£121	£73	£109	£66

In the following section I will discuss a new approach to optimal decision theory called Ergodicity Economics, introduced by Ole Peters and colleagues (Peters, 2019; Peters & Adamou, 2018), which will allow us to understand this example within a more formal mathematical framework. Peters et al. have used these ideas to solve a number of well-known problems in economics, such as hyperbolic temporal discounting and the St Petersburg Paradox. I will show that their ideas can also be usefully applied to cognitive neuroscience and in particular to solving the problem of effort. The points I will develop in particular are that:

1. Maximising expected value is not optimal behaviour in general – there are specific circumstances where it is, and others where it can be shown that it is optimal to maximise a different quantity.
2. Utility functions, which in psychology and cognitive neuroscience are typically regarded as idiosyncratic biases, in fact have a normative interpretation – for different contexts there is a specific utility function which is optimal. By extension this means that empirical utility functions can be regarded as beliefs about the environment.
3. Finally, in situations where the optimal utility function is concave, outcome noise is costly. Specifically, we can show that this cost has an approximately quadratic relationship with effort intensity, thus providing the missing justification for Harris and Wolpert's (1998) choice of endpoint variance in their account of motor effort costs, and for quadratic cognitive effort costs in other models (Ritz et al., 2022; Manohar et al., 2015).

6.3.1 Maximising expected value does not guarantee optimal decisions

Key to developing the example above into a more formal statement about optimal decision-making is stating explicitly what the objective of decision-making is and understanding exactly how the expected value of a decision relates to this.

Peters et al. start with the axiom that the goal of any economic decision-maker is to make choices that grow their wealth at the fastest rate possible (in cognitive neuroscience, terms like wealth, rewards etc. need not denote just monetary value – but it is convenient to use financial language because of the mathematical precision it affords). If one's wealth is denoted by $w(t)$, then a decision-maker should pick the option that will result in the greatest rate of change of their wealth[2], $\frac{\delta w}{\delta t}$.

In some deterministic cases it may be that the outcome of a decision can be known with certainty, in which case maximising $\frac{\delta w}{\delta t}$ is trivial. In general, however, the outcomes of decisions are random variables – you cannot know in advance whether a coin will land heads or tails up, just as you also cannot be sure whether putting more cognitive effort into a task will lead to greater rewards or not. More precisely, we say that the future changes in one's wealth, δw, follow a stochastic process (see Figure 6.1), and we do not know which realisation of this process we will experience. What is therefore needed is to condense the process δw into some scalar value representing the typical outcome that will be experienced (removing the randomness). We accomplish this by calculating an appropriate average of the process, with which we are then on firmer footing and can choose whichever option results in the better average change in wealth.

[2] This axiom justifies our use in the gamble example above of expected monetary outcome, rather than percentage outcome. We will see later that the percentage outcome would be misleading even if it were used.

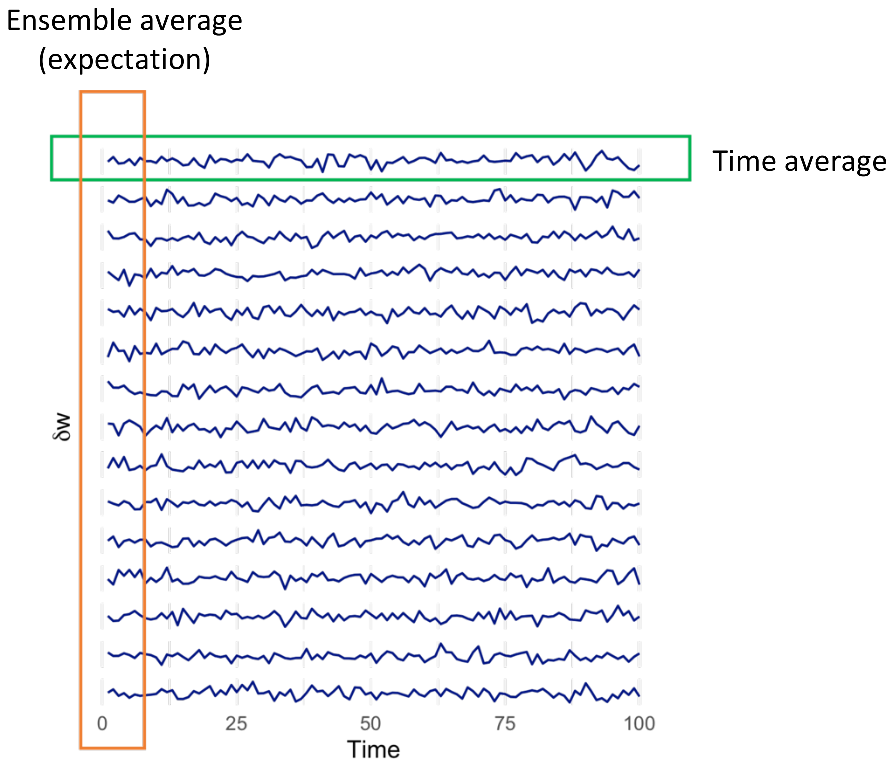


Figure 6.1. An example of a stochastic process, in this case the changes in wealth, δw, experienced over time. A stochastic process is a collection of random variables each associated with a different point in time. Because of this randomness, the process can unfold in many different ways over time – each of these possible trajectories is called a realisation of the process. There are then two different kinds of average we can take: we can average over the ensemble of different realisations at a single point in time (the ensemble average, or expectation) or we can average over time for a single realisation (the time average). Based on a figure in Peters and Adamou (2018, p.11)

There are two kinds of average of a stochastic process that we might choose to take, the time average and the ensemble average. The finite time average (green box in Figure 6.1) is the mean of a particular realisation of the process over some interval Δt; if we take the long-time limit $\Delta t \to \infty$, we then get the infinite time average (henceforth just called the time average). On the other hand, the finite ensemble average (orange box in Figure 6.1) is the mean at a single timepoint across some sample N of independent, parallel realisations of the process; if we likewise take the large-sample limit $N \to \infty$ we arrive at the infinite ensemble average, also called the expectation. As Peters and Adamou (2018) are keen to point out, the terminology here is somewhat misleading, as there is nothing about the ensemble average which means this is a value we should particularly ‘expect’ to see, since in the real world we only ever get to observe a single realisation of any stochastic process (indeed this linguistic confusion may be at least partly responsible for some of the problems with optimal decision theory that we have currently). The gamble suggested above is a good example of a case where, despite the expectation being positive, you should *actually* expect your wealth to decrease over time (Figure 6.2).

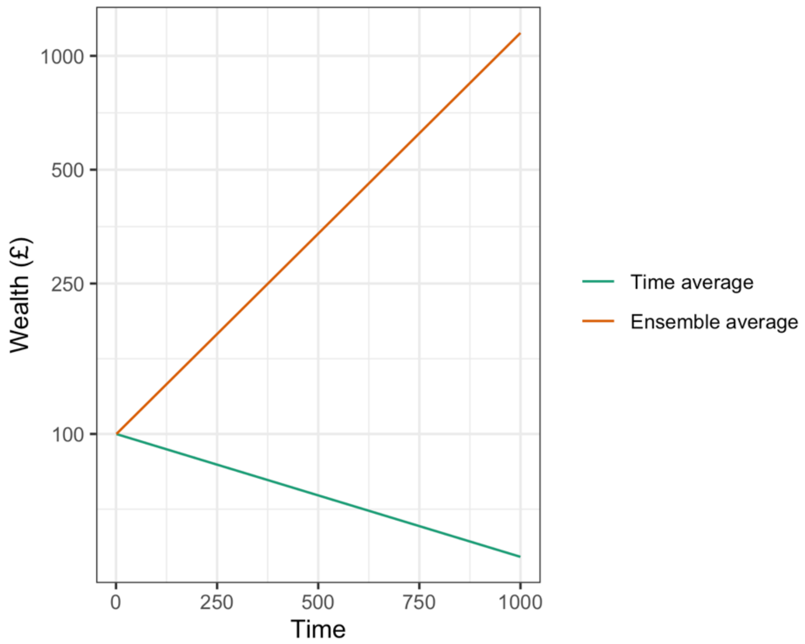


Figure 6.2. Comparison of the time average (green) and ensemble average (orange) trajectories of wealth. Any individual choosing the multiplicative gamble introduced at the start of Section 6.3 will, in the long-time limit, converge on the time average trajectory and so will experience a decrease in their wealth over time; conversely the ensemble average increases over time. Thus any decision-maker who bases their choices on the ensemble average (expected value) rather than the time average will be misled.

This brings us to the heart of Peters and Adamou's (2018) argument: they assert that the primary quantity of interest for a decision-maker should be the time average change in wealth, since this describes the average effect of a choice within a particular realisation; we do not get to experience multiple parallel realisations of the world, so the average across the statistical ensemble is *prima facie* irrelevant. Where it becomes useful, and the reason we talk about maximising expected value at all, is because in some cases the expectation and the time average of changes in wealth are equal – if so, we say that the change in wealth δw is ergodic, and this gives us a convenient means of calculating its time average. Specifically, the change in wealth δw is ergodic if its instances over time are independent realisations of a stationary random variable.

Consider a modified version of the coin-toss gamble proposed above, in which the outcomes (represented by z) are not dependent on the stake, but instead take absolute values; +£50 and –£40 for example. In this case, the increments of wealth δw are independent and stationary over time, and so are ergodic. Your wealth evolves after T increments of time according to:

$$w(t + T\delta t) = w(t) + \sum_{\tau}^{T} z(\tau) \tag{6.1}$$

and therefore the time average change in wealth (indicated by an overbar) is given by

$$\begin{aligned}\overline{\delta w} &= \lim_{T\to\infty} \frac{1}{T}\big(w(t + T\delta t) - w(t)\big) \\ &= \lim_{T\to\infty} \frac{1}{T}\sum_{\tau}^{T} z(\tau)\end{aligned} \tag{6.2}$$

This is identical to the expression for the expected change in wealth (indicated by angled brackets), save for the change of dummy variable:

$$\begin{aligned}\langle \delta w \rangle &= \lim_{N\to\infty} \frac{1}{N} \sum_{i}^{N} z(i) \\ &= \lim_{T\to\infty} \frac{1}{T} \sum_{\tau}^{T} z(\tau)\end{aligned}$$

(6.3)

Having established this equality, it is then straightforward to calculate the time average $\overline{\delta w}$ from the ensemble average, which can be computed as the weighted average of each of the J different possible outcomes:

$$\begin{aligned}\overline{\delta w} = \langle \delta w \rangle &= \sum_{j} p_j z_j \\ &= \frac{1}{2} \times 50 + \frac{1}{2} \times -40 \\ &= £5\end{aligned}$$

(6.4)

By simulating the coin toss gamble using the same process as before (Table 6.2), we can see that in this additive case the expected value is indeed a meaningful guide to the actual effects of choosing to gamble on one's wealth.

Table 6.2. Outcomes you can expect to receive if you accept the additive gamble

	Trial 1	Trial 2	Trial 3	Trial 4	Trial 5	Trial 6	Trial 7	Trial 8
Initial wealth	+£50	-£40	+£50	-£40	+£50	-£40	+£50	-£40
£100	£150	£110	£160	£120	£170	£130	£180	£140

Returning to the gamble as originally stated, in which the outcomes were expressed as a multiple, r, of the amount staked, we now have a better insight into why the expectation was not a useful guide to the actual effect of choosing to gamble: in this case the changes in wealth δw over time are not stationary, because they depend on time indirectly through wealth $w(t)$.

Specifically, in this version of the gamble, your wealth evolves according to:

$$w(t + T\delta t) = w(t) \cdot \prod_{t}^{T} r(\tau) \tag{6.5}$$

and therefore the time average of changes in wealth (indicated by an overbar) is given by

$$\begin{aligned}\overline{\delta w} &= \lim_{T\to\infty} \frac{1}{T}\big(w(t + T\delta t) - w(t)\big) \\ &= \lim_{T\to\infty} \frac{1}{T}\left(w(t) \cdot \prod_{t}^{T} r(\tau) - w(t)\right)\end{aligned} \tag{6.6}$$

It is immediately clear that there is no longer equality between the time average and the expectation, and so the latter cannot be used to compute the former. In other words, maximising expected value is not optimal decision-making in this example. This is the first key point when trying to understand cognitive effort.

To make the connection to cognitive effort more explicit, consider a typical effort discounting task, in which participants choose between engaging in some effortful activity or doing nothing. For simplicity we will continue to use the same gains and losses as in the gambles above, so participants are endowed with £100 and are initially offered +£50 if they complete the effortful activity successfully, –£40 if they make a mistake, and nothing if they choose not to do the effortful activity. Ordinarily, if we found that participants systematically chose to avoid doing the effortful task (e.g. Kool et al., 2010) we would conclude that this was evidence of intrinsic effort costs, because the expected value of the task is positive otherwise. But if the reward dynamic was really multiplicative (i.e. the +£50/–£40 incentive represented a +50%/–40% change in wealth, because the dynamic either of the experiment, or of the 'real world' in which the participant was responding, was multiplicative) then participants should choose not to do the task because, as we have already seen, they would typically lose money. Thus they would show behaviour that appears to be effort discounting, but that is not in fact driven by effort costs.

6.3.2 Ergodicity transformations and utility functions

To solve the problem of non-ergodicity, Peters and Adamou suggest applying what they call an ergodicity transformation, which is any monotonic function of wealth $v(w)$ whose increments $\delta v(w)$ are ergodic (independent instances of a stationary random variable). They then define what they call the growth rate, g, which is the change in this transformed wealth experienced over some interval of time:

$$g(\Delta t) = \frac{\Delta v(w)}{\Delta t} \tag{6.7}$$

The (time average) growth rate is then (by design) equal to the expected growth rate:

$$\begin{aligned} \bar{g} &= \lim_{\Delta t \to \infty} \left\{ \frac{\Delta v(w)}{\Delta t} \right\} \\ &= \frac{\delta \overline{v(w)}}{\delta t} \\ &= \langle \frac{\delta v(w)}{\delta t} \rangle \end{aligned} \tag{6.8}$$

The specification that the ergodicity transformation must be monotonic is important, because it preserves the order of values. Fundamentally, when making a decision, we are only looking to rank the different options available so that we can find the one that will typically lead to the biggest change in our wealth – the absolute value of the change in our wealth is not necessarily important. Because the transformation is monotonic, we know that the outcome of comparing two possible choices *a* and *b* on our wealth, $w_a > w_b$, will always give the same result as the comparison on our transformed wealth, $v(w_a) > v(w_b)$, even though the absolute values are not the same, $w_a \neq v(w_a)$. We can therefore rely on the time average growth rate of transformed wealth, $\bar{g}$, to make decisions that are optimal according to our decision axiom, even though we do may not know what the time average growth rate of wealth itself will be.

To put this in context, consider again the multiplicative gamble, which results in wealth given by Equation 6.5. An appropriate ergodicity transformation could be the logarithm

$$v\big(w(t+T\delta t)\big) = \ln\left(w(t)\cdot\prod_{t}^{T} r(\tau)\right) \tag{6.9}$$

because this results in increments that are independent and stationary over time:

$$\delta v(t) = \ln w(t+\delta t) - \ln w(t) = \ln r(t) \tag{6.10}$$

The time average growth rate is then:

$$\begin{aligned}\bar{g} &= \lim_{T\to\infty}\frac{1}{T\delta t}\Big(v\big(w(t+T\delta t)\big) - v\big(w(t)\big)\Big)\\ &= \lim_{T\to\infty}\frac{1}{T\delta t}\left(\sum_{\tau}^{T}\ln r(\tau)\right)\end{aligned}$$

$$
\begin{aligned}
&= \frac{\langle \ln r \rangle}{\delta t} \\
&= -0.05
\end{aligned}
\tag{6.11}
$$

Since this is negative, you should anticipate losing money if you choose to take this bet. Therefore compared with the null option of doing nothing (with time average growth rate equal to zero), gambling is the worse choice.

Decisions represented by other stochastic processes naturally require different ergodicity transformations. In the simplest case where outcomes are independent over time (which we have already seen in the additive gamble), the change in wealth is already ergodic, so the transformation can simply be the identity function, i.e. $v(w) = w$. Other more complex cases can also arise: for instance the outcome of a choice may combine both additive and multiplicative effects (in which case the ergodicity transformation would involve taking a root function of wealth); or the outcome may be a fraction, rather than a multiple, of current wealth (in which case the transformation is an exponential function). Fortunately, Peters and Adamou have produced a more general formulation of their decision theory, able to account for essentially arbitrary outcome dynamics, which I will discuss briefly in the following section.

For now, however, it may be helpful to summarise concisely what has been discussed so far. According to the Ergodicity Economics decision theory (Peters and Adamou, 2018; Peters, 2019):

- Optimal decision-making consists of making choices that maximise the growth of one's wealth over time. The primary quantity of interest for a decision-maker is therefore the time average rate of change of wealth, $\overline{\frac{\delta w}{\delta t}}$, because this describes what will actually happen to an individual over time (as opposed to what will happen to the statistical ensemble of realisations of the individual).

- However, if the changes in wealth as a result of making a particular decision are themselves time-dependent (non-ergodic), then the time average change in wealth may not be meaningful. Therefore find the ergodicity transformation, $v(w(t))$, that removes this time dependence, so that the growth rate of this transformed wealth,
  $g(\Delta t) = \frac{\Delta v}{\Delta t}$, becomes ergodic.
- Then calculate the time average growth rates, $\bar{g}$, either directly by taking the long-time limit or by making use of the equality $\bar{g} = \langle g \rangle$ (the latter typically being more convenient).
- Finally, compare the options and choose whichever has the highest time average growth rate, $\bar{g}$.

This method has an obvious parallel in Expected Utility Theory (EUT), according to which people transform their wealth into utility, then compute the expected change in utility under different options and choose whichever is the greatest. The key difference, however, is that EUT is an empirical explanation—it aims to describe, rather than predict, how people behave, with particular emphasis placed on measuring their utility functions—whereas Ergodicity Economics is normative. In other words, Ergodicity Economics asserts that there is an optimal ergodicity transformation that should be used, which depends on the dynamics of the environment, and which can be analytically calculated provided the dynamics are invertible. Maximising expected value is optimal only in the specific case where the change in wealth you experience is stationary over time and does not depend on what you won in the past. In other words one must assume ergodicity which, in the real world, is unlikely to be valid.

A corollary, not mentioned by Peters and Adamou but potentially important to cognitive neuroscience, is that empirical utility functions can also be interpreted as beliefs about the outcome dynamics. If we observe a logarithmic utility function we can infer that the participant believes that the changes in wealth they experience will be proportional to their current wealth (or more precisely to the amount they are willing to stake). More generally, Peters and Adamou have shown that it is possible to calculate the dynamic implied by essentially arbitrary utility functions (again provided the function is invertible). This is a powerful tool that allows us to go much further than the EUT interpretation of utility functions – quantities like risk aversion (or indeed risk seeking) are treated not as idiosyncratic biases but instead can be reframed as a belief about the dynamics of the environment. This is the second significant conclusion we can draw from the Ergodicity Economics work.

6.3.3 General dynamics and quadratic noise

In this final set-up section I will briefly set out the Ergodicity Economics framework for dealing with arbitrary outcome dynamics. This then provides a platform for discussing some of the more important results for the purposes of cognitive neuroscience and understanding cognitive effort in particular.

The aim is to model an arbitrary reward process (i.e. not just additive or multiplicative) as a type of stochastic differential equation (SDE) called an Itô process. Specifically, Peters and Adamou state that this Itô process should be of the form

$$dw = a(w)dt + b(w)dS \tag{6.12}$$

where dS is the Wiener increment (randomly distributed noise).

$$dS \sim N(0, dt) \tag{6.13}$$

This SDE allows us to model different dynamics through the choice of the coefficients $a(w)$ and $b(w)$.

For example an expression for the additive dynamic is obtained by setting $a = \mu$ and $b = \sigma$. This particular SDE represents Brownian motion.

$$dw = \mu dt + \sigma dS \tag{6.14}$$

Similarly the multiplicative dynamic is given by $a = \mu w(t)$ and $b = \sigma w(t)$, and is known as *geometric* Brownian motion:

$$dw = \mu w(t) dt + \sigma w(t) dS = w(t)(\mu dt + \sigma dS) \tag{6.15}$$

However the real power of this SDE is that it allows us to model arbitrary dynamics. For example,

$$dw = \mu w(t)^{\frac{1}{2}} dt + \sigma w(t)^{\frac{1}{2}} dS \tag{6.16}$$

encodes a dynamic that interpolates between the additive and multiplicative regimes.

To make the connection to the equations in the previous section explicit, note that what we have essentially done is invoke the central limit theorem to approximate the outcome variables z (in the case of the additive dynamic, Equation 6.1) or r (in the case of the multiplicative dynamic, Equation 6.5) by a normal distribution with expected outcome μ and standard deviation σ. The process for making a decision using this SDE is then the same as outlined in the previous section: you solve the differential equation, apply an appropriate ergodicity transformation to wealth, $v(w)$, then compute the time average growth rate, $\bar{g}$.

This leads us to the third significant result with regards to understanding effort: Peters et al. derive analytical expressions for the additive and multiplicative growth rates in terms of μ and σ (in the multiplicative case by Taylor-expanding the logarithm in $\overline{g_m} = \frac{\langle d \ln w \rangle}{dt}$).

$$\overline{g_a} = \frac{\langle dw \rangle}{dt} = \mu \tag{6.17}$$

$$\overline{g_m} = \frac{\langle d \ln w \rangle}{dt} = \mu - \frac{1}{2}\sigma^2 \tag{6.18}$$

What these equations prove is that, if changes in wealth are additive (or are believed to be so), outcome noise is irrelevant. However, if changes in wealth are (or are believed to be) multiplicative, the time average growth rate depends quadratically on noise. Indeed we can be more general than this: whenever the outcomes of a particular choice are non-ergodic, the time average growth rate of wealth will be affected by noise. If the required ergodicity transformation is concave, increasing noise results in a progressively lower growth rate, whereas if it is convex, noise increases the growth rate.

This is relevant to understanding effort in two important ways:

First, it provides a rationale for the use of endpoint variance as the cost function in physical effort (Harris & Wolpert, 1998), which had up until now been lacking. The endpoint of a movement determines the outcome that is received because, presumably, if a movement hits the target the reward will be received whereas if it misses the reward will be lower or zero (as an aside, the precise relationship between endpoint and reward does not matter in this framework, since both variables are being approximated with normal distributions). Therefore the time average change in wealth is ultimately a function of endpoint variance, provided the reward dynamic is multiplicative. On the other hand, *contra* Harris and Wolpert, in other dynamics the motor cost may depend on some other function of noise.

Second, it implies that outcome noise is a potentially significant confound in cognitive effort discounting tasks, since any intended manipulation of effort that also affects outcome noise may reduce the time average growth rate in a way that looks like effort discounting. Another way to put this that directly answers the question posed at the top of Section 6.3 is to say that effort costs are not the only explanation for people's apparent failure to maximise expected rewards – we also need to take account of their beliefs about the reward dynamic which, if non-additive, will mean that outcome noise provides a competing explanation.

For example, consider the N-back working memory task, which is sometimes used in cognitive effort tasks (e.g. Westbrook & Braver, 2013). I mentioned in Chapter 4 that it may be intrinsically more difficult at higher levels, but in addition to this we might also be concerned about whether responses, and therefore outcomes, are more variable at higher levels too. If so, Equation 6.18 shows that this outcome noise would subtract from the growth rate of wealth, constituting another confound that could also affect the inferences we draw from these tasks. We therefore need at least to be more aware and cautious of these risks when conducting cognitive effort experiments.

6.3.4 Specific applications of Ergodicity Economics to optimal control and cognitive costs

In the sections above I have reviewed the Ergodicity Economics framework and how it shows that maximising expected value is not universally optimal, and in particular that phenomena like effort discounting could result from an economic cost of noise. In this final section on Ergodicity Economics, I will now develop these ideas further with regards to cognitive neuroscience and costs of control in particular. I will focus on three types of control: control over the choice of response, the vigour of that response and its precision.

6.3.4.1 Control over choice of response

One of the most fundamental aspects of control is configuring the system for a particular task so that it produces optimal responses. In other words, defining in advance a mapping between input signals and outputs; for example, on the Stroop task this mapping is determined by the instructions – if these say to name the colour of the ink, then control is needed to ensure that responses are dictated by the colour part of the input signal, and not the semantic information.

A richer example is provided by working memory. Part of the role of control in working memory is to configure the mapping between memory inputs (the original stimulus) and memory outputs (recalled stimulus) so that we minimise errors (or more precisely, the cost of errors) subject to any constraints on memory capacity. In this section I will use the Ergodicity Economics framework to derive normative recall distributions for working memory under different outcome dynamics. I will show that these distributions fit the observed empirical data as well, if not better than, existing models which assume that subjective costs are a quadratic function of errors. This again means that Ergodicity Economics can provide a competing explanation of cognitive control that does not assume subjective costs.

As well as the Ergodicity Economics framework, I will also be making use in this section of a branch of Information Theory called Rate-Distortion Theory (Shannon & Weaver, 1949); its application to topics in working memory was first discussed by Sims (2012 and 2015). My own contribution in this section is in making the connection to Ergodicity Economics and specifically in demonstrating a method for converting between the two, i.e. for plotting the optimal loss function and working memory distribution given a particular reward dynamic.

6.3.4.1.1 Rate Distortion Theory – Key Results

The key parts of Rate-Distortion Theory needed for our present purposes are as follows.

A communication channel is defined as the mechanism by which information is encoded, transmitted from a sender to a receiver, and then decoded again. Furthermore a channel can exist in both space and time, so working memory can be understood as a kind of channel in which a message is sent from the present to the future. We model this working memory 'channel' as a conditional probability distribution, $Q = P(Y|X)$, that recalls a stimulus value Y given original input X. We treat X and Y as random variables since we are interested in configuring this channel in the general case where the stimulus to be remembered is unknown.

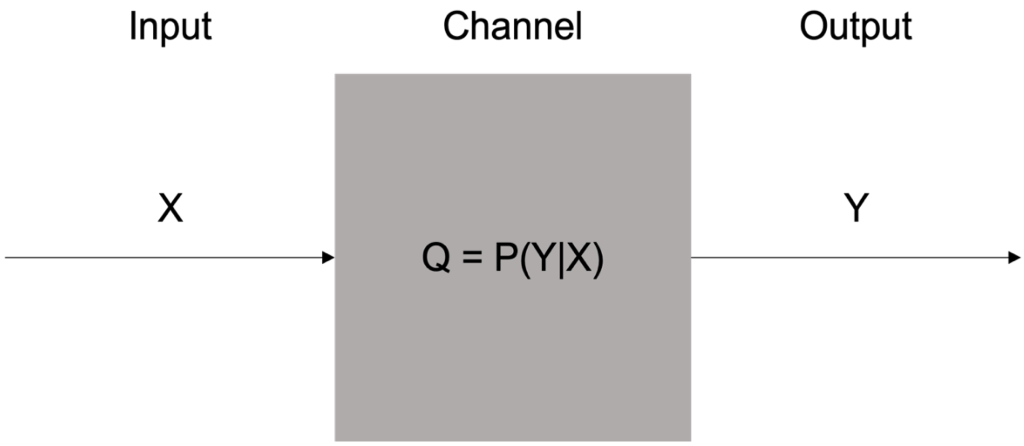


Figure 6.3. A schematic of a communication channel. A channel maps inputs to outputs through the conditional distribution $Q = P(Y|X)$. Usually we think of a channel as communicating information from one point in space to another, but in our case it can also be used to model memory, which I will treat as the communication of information from the present to a point in the future.

The capacity of a channel is the maximum amount of information it can convey (i.e. as a result of structural constraints). We start by defining the information rate of a particular configuration of the channel, $I(Q)$, which is the expected reduction in uncertainty about X given some observation $Y = y$ (also known as the mutual information between X and Y). The capacity of a channel is then the maximum achievable information rate.

$$I(Q) = \iint q(y|x)p(x)\log\left(\frac{q(y|x)}{p(y)}\right) dxdy \tag{6.19}$$

$$C = \max_{Q(Y|X)} I(Q) \tag{6.20}$$

We can also characterise a channel by its expected loss, or distortion. In the case of working memory channel, loss is defined as the cost of recalling y when the original stimulus value was really x, $\mathcal{L}(y,x)$. Distortion is then defined as

$$D_{\mathcal{L}} = \iint \mathcal{L}(y,x)q(y|x)p(x)\, dxdy \tag{6.21}$$

Finally, the goal of Rate Distortion theory—to find the best channel configuration, Q^*, subject to any constraints on capacity—is satisfied by

$$Q^* = \operatorname*{argmin}_{Q \in Q_C} D_{\mathcal{L}}(Q)$$

$$Q_C = \{Q : I(Q) \leq C\} \tag{6.22}$$

Using these equations we can compute the optimal channel configuration for four different loss functions and two different channel capacities. The working memory response distributions associated with each of these channels are plotted in Figure 6.4 (reproducing Figure 2 in Sims, 2015). For reference an empirical distribution is also plotted in Figure 6.5.

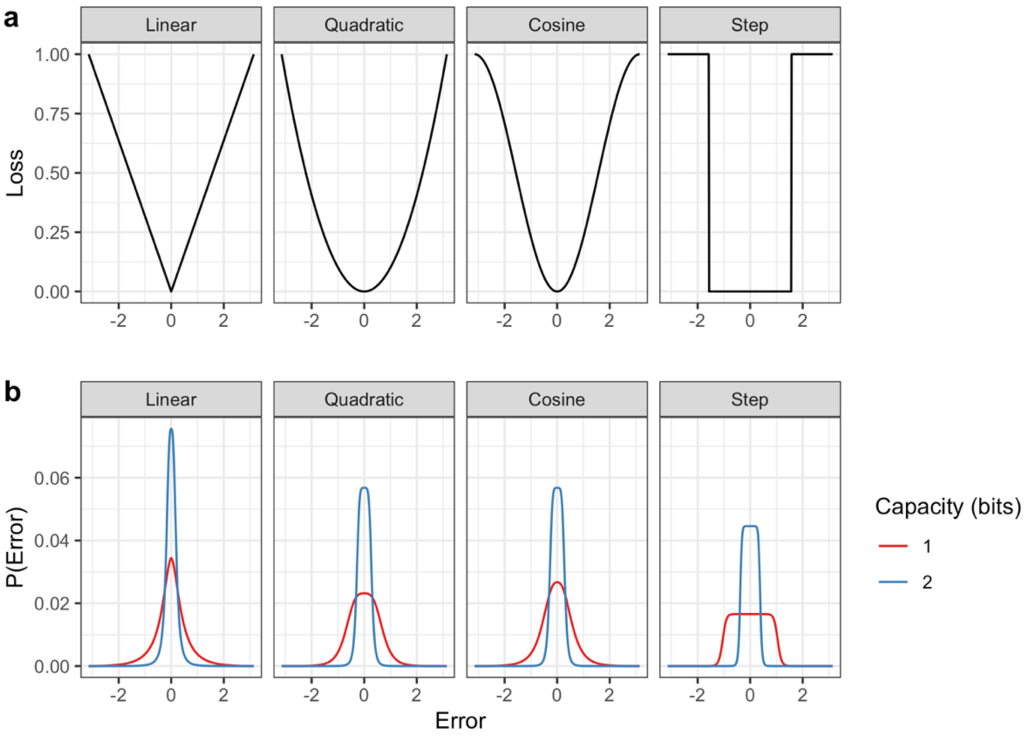


Figure 6.4. Optimal working memory response distributions according to Rate Distortion Theory. Panel (a) shows four different possible loss functions, and (b) shows the associated optimal recall distributions. This are shown as error distributions (i.e. response – target value) for ease of plotting. This figure reproduces Figure 2 in Sims (2015).

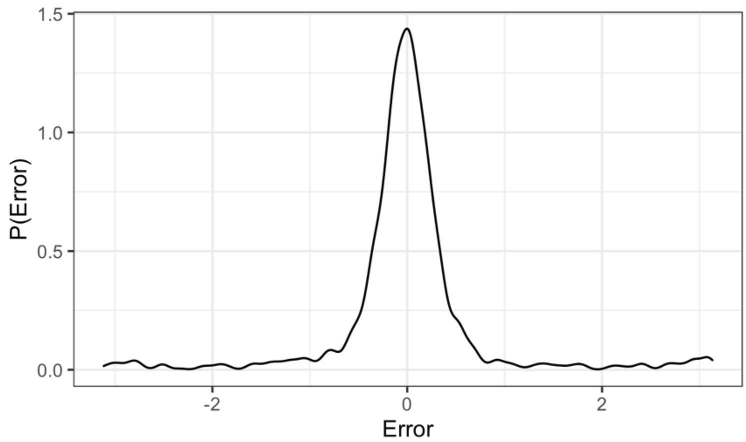


Figure 6.5. An empirical working memory distribution for comparison with Figure 6.4 (data collected by HF). This shows the characteristic von Mises-like distribution of recall errors.

6.3.4.1.2 Deriving normative working memory distributions

Loss functions can be of two types: they can be task-defined, meaning the reward contingency of the task dictates that a particular loss function is appropriate; or they can be empirical, in which case we are simply seeking to measure participants' subjective loss function, typically because we expect them to deviate from the task-defined function in some way. In the following sections I will restrict the analysis to the case seen most often in working memory experiments, where the outcome on each trial is linearly related to recall error, i.e. the task-defined loss function should be linear[3]. Sims et al. (2012, 2015) have shown that the empirical loss function across a number of previously published studies is nevertheless non-linear, which needs somehow to be explained.

As may already have been evident, empirical loss functions as treated by Rate Distortion Theory are equivalent to utility in the EUT framework, or the ergodicity transformations in Ergodicity Economics, save for the obvious distinction that loss refers to a negative change in wealth. I will likewise reinterpret the distortion of a channel as the expected (negative) change in transformed wealth, which is therefore equal to the time average negative growth rate of wealth.

$$\begin{aligned} D_{\mathcal{L}} &\equiv -\frac{\langle dv(w)\rangle}{dt} \\ &= -\bar{g}. \end{aligned} \tag{6.23}$$

This makes the connection with Ergodicity Economics clear: maximising the time average growth of one's wealth can be achieved by minimising the distortion of the channel, provided the loss function is an ergodicity transformation. We can use this knowledge to predict optimal working memory distributions across different reward dynamics according to Ergodicity Economics. I will focus on the two example cases of additive and multiplicative dynamics.

[3] Note this is not a critical assumption – in principle the model could be extended to accommodate more complex incentive structures, but this is beyond the scope of this chapter.

First, we need to state the incentive structure of the tasks, noting that we have restricted ourselves to the case where this is linear. In the additive case (where the outcome z is the additive change in wealth) and the multiplicative case (where the outcome r is the multiple on wealth), respectively:

$$\begin{gathered} z \propto -\epsilon \\ r \propto 1 - \epsilon. \end{gathered} \tag{6.24}$$

In both cases there is no change in wealth when error is zero, and as error increases the outcome decreases in linear proportion. By choosing an appropriate scale for ϵ we can also define the outcome when error is at its maximum (in this case I will scale ϵ by $\frac{1}{\pi}$ so that the loss is normalised between 0 and 1 when error is between 0 and π).

Let the loss, $\mathcal{L}(\epsilon)$, also be defined as the negative change in transformed wealth, $\mathcal{L}(\epsilon) \equiv -\delta v(w)$.

With these components in place it is then straightforward to show that the optimal loss function is linear when the reward dynamic is additive. The required ergodicity transformation is just the identity function, $v(w) = w$, and therefore $\delta v(w) = \delta w$. Substituting $\delta v(w) = -\mathcal{L}(\epsilon)$ and $\delta w = z = -\frac{1}{\pi}\epsilon$, we find:

$$\mathcal{L}(\epsilon) = \frac{1}{\pi}\epsilon \tag{6.25}$$

Finding the optimal loss function for the multiplicative dynamic is slightly more involved. The required ergodicity transformation is logarithmic, $v(w) = \ln w$. Then the change in transformed wealth is:

$$\delta v(w) = \ln w(t_0 + \delta t) - \ln w(t_0)$$
$$= \ln \frac{w(t_0 + \delta t)}{w(t_0)}$$
$$= \ln r$$
(6.26)

Substituting $\delta v(w) = -\mathcal{L}(\epsilon)$ and $r = 1 - \frac{1}{\pi}\epsilon$, we find:

$$\mathcal{L}(\epsilon) = \ln \frac{1}{1 - \frac{1}{\pi}\epsilon}$$
(6.27)

These loss functions, and the response distributions of the optimal channel configuration associated with them, are plotted in Figure 6.6 (together with those of the cosine loss function as well, for comparison). We can see that the distribution in the case of the multiplicative dynamic seems to interpolate between those of the additive dynamic (linear loss function) and the cosine loss function. Interestingly this matches empirical data reported by Sims (2015), who found that participants tend to have sharper recall distributions than he had predicted using a cosine loss function. Overall this suggests that cognitive control over the configuration of working memory responses could be explained not by quadratic costs, as suggested by e.g. Sims (2015) and Ritz et al. (2021), but instead simply by participants maximising the growth of their wealth given non-ergodic outcomes.

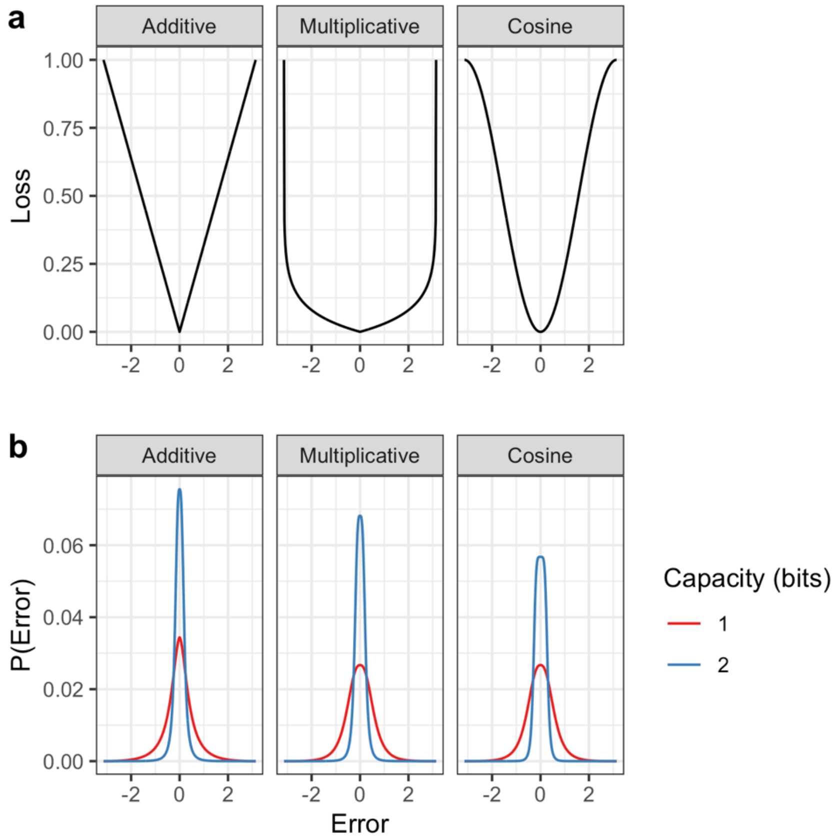


Figure 6.6. Optimal working memory response distributions in the Ergodicity Economics framework. Panel (a) shows the loss functions (ergodicity transformations) for the additive and multiplicative dynamics, as well as a cosine loss function for comparison; (b) shows the associated optimal recall distributions. The distribution in the multiplicative case seems to interpolate between those of the additive dynamic (linear loss function) and the cosine loss function.

6.3.4.2 Control of signal intensity

One of the most robust phenomena in cognitive neuroscience and psychology is the speed-accuracy trade-off (Heitz 2014; Shmuelof et al., 2012). This refers to the observation that, at faster reaction times, accuracy tends to be lower; vice versa, maximising accuracy tends to require reaction times that are longer. More specifically, this relationship appears to be approximately linear, so that doubling the speed of response halves the accuracy (Heitz, 2014).

Previously, the speed-accuracy trade-off has been thought to present something of a problem, since you can apparently produce the same rate of reward with an infinite number of different combinations of speed and accuracy – as a control problem, it is described as degenerate (Ritz et al., 2022; Manohar et al., 2015). Quadratic effort costs are then typically introduced as a solution to this problem because, in introducing a second trade-off that penalises vigour, they create a unique, optimum combination of speed and accuracy.

Once again however, this issue only arises if we assume that the change in wealth per trial is ergodic. If we do not make the ergodicity assumption, and we allow that a person's ability to access rewards varies over time, for example as a function of their current wealth, then the problem of control degeneracy dissolves. Instead, there is an optimum level of vigour that results just from maximising the time average growth of one's wealth, and we do not need to invoke effort costs at all.

First, consider a modified version of the SDE for a multiplicative dynamic (geometric Brownian motion; Equation 6.15).

$$dw = \ell w(\mu dt + \sigma dS) \tag{6.28}$$

Peters, 2010, originally derived this equation to solve the problem of how to distribute money among different assets in an investment portfolio, where ℓ represented the fraction of one's wealth invested. In the context of cognition, however, I suggest ℓ can instead be interpreted as controlling the amount of time elapsed (dt) for each increment of reward – in other words the speed or vigour of responding.

The time average growth rate, analogous to 6.18, can then be calculated:

$$\overline{g_m} = \lim_{\Delta t \to \infty} \frac{\Delta \ln w}{\Delta t} = \ell\mu - \frac{1}{2}\ell^2\sigma^2 \tag{6.29}$$

Note that this is similar in form to the equation given in Manohar et al. (2015, p. 1709 Eq. 1), except that their effort cost term has now been replaced by the outcome variance. Thus we can reproduce the same results as existing models, but without having to assume the existence of effort costs. In order to produce a specifically quadratic dependence on outcome noise we do have to assume that people believe the reward dynamic is multiplicative; however, we can relax this assumption and assume just that the change in wealth is non-ergodic, and still get the same basic result with a different functional dependence on noise.

Finally, the optimal vigour, $\ell_{opt} = \underset{\ell}{\operatorname{argmax}}\, \overline{g_m}$, can be found by setting $\frac{d\overline{g_m}}{d\ell} = 0$, and solving for ℓ:

$$\ell_{opt} = \frac{\mu}{\sigma^2} \tag{6.30}$$

These results—the calculation of the time average growth rate and of the optimal vigour—are plotted in Figures 6.7a and 6.7b respectively. Here again we see clear evidence that what is usually regarded as an indication of the presence of effort costs, namely the avoidance of high levels of response vigour, can actually be accounted for as a simple consequence of optimal decision-making, taking the non-ergodicity of the wealth dynamic into account.

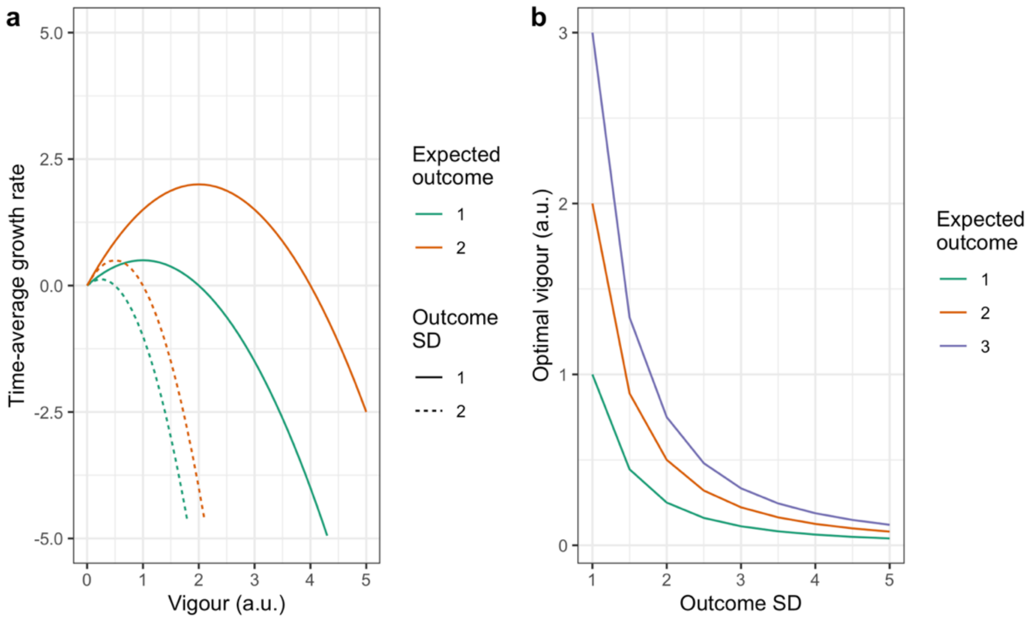


Figure 6.7. The relationships between vigour, expected outcome (μ), outcome noise (σ) and the time-average growth rate of wealth, g. (a) Both low and high levels of vigour lead to low or negative growth rates – instead the optimum vigour (the peak of the curve) is intermediate. (b) The optimal vigour decreases as outcome noise increases, but increases with greater levels of expected reward.

6.3.4.3 Control of signal precision

The final aspect of control that I will consider is control over precision. Several studies have shown that it is possible to attenuate noise independently of changes in response vigour, a phenomenon that Manohar et al. (2015) refer to as "breaking the speed-accuracy trade-off". Because this ability to control noise seems to depend on motivation and the amount of reward offered, they suggest that this form of control must also come at a cost. The question once again is whether this is strictly necessary – can this phenomenon be accounted for without invoking effort costs?

In short, the answer seems to be no. We can show this intuitively by considering what happens if we do add another control parameter to allow us to control noise. Consider once again equation 6.28.

$$dw = \ell w(\mu dt + \sigma dS) \tag{6.31}$$

Here response vigour, ℓ, multiplies the mean and noise terms (μdt and σdS) equally. In order to control noise we would need to add a second control parameter (say, $\frac{1}{k}$) which multiplies just the noise term alone. With these two parameters we then have full control over the process and no other parameters can be added that are not redundant with these two.

$$dw = \ell w(\mu dt + \frac{\sigma}{k} dS) \tag{6.32}$$

Computing the new time average growth rate we find:

$$\overline{g_m} = \ell\mu - \frac{\ell^2\sigma^2}{2k^2} \tag{6.33}$$

Because k is only included in the right hand term, there is no trade-off and the function has no maximum – as can be seen in Figure 6.8 below, the time-average growth rate approaches a horizontal asymptote at $\ell\mu$ as $k \rightarrow \infty$, and therefore the optimum precision, k_{opt}, is infinite. This implies that, on an economic basis, people should exert maximum precision at all times; since empirically we see they do not, there must be some other reason which, presumably, involves intrinsic costs of attenuating noise.

In the following section, I will pick up on this suggestion and show that there is indeed an obligatory energetic cost of controlling precision in the brain which, to my knowledge, has not been discussed previously in relation to cognitive effort.

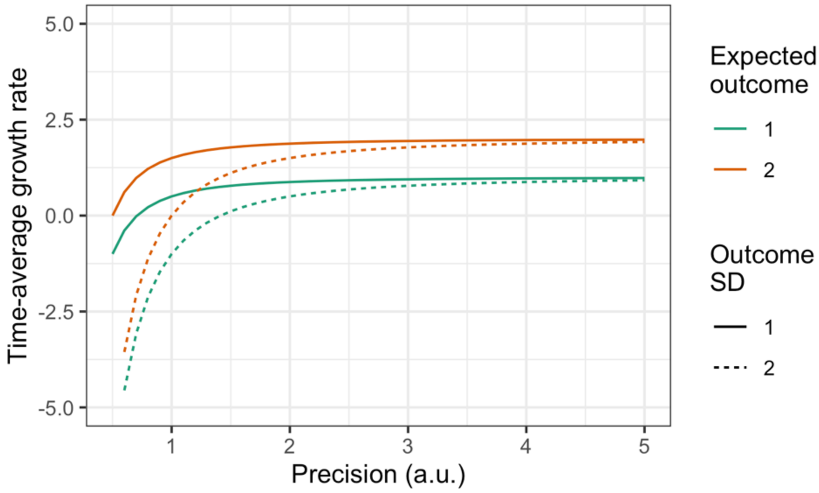


Figure 6.8. The relationship between precision and the time-average growth rate of wealth. Wealth grows more quickly as precision is increased, but there is no maximum – the function approaches an asymptote equal to $\ell\mu$ only in the limit where precision is infinite. This suggests we cannot account for effort costs associated with controlling precision within the framework of Ergodicity Economics.

6.4 Landauer's principle: A fundamental energetic cost of attenuating noise

A different approach to the question of cognitive effort might start by asking whether there are any fundamental, necessary costs involved in operating the brain. One answer comes from Landauer's Principle (1961), which concerns the energetic costs of computation. Specifically, the principle states that the erasure of n bits of information necessarily dissipates $nkT\ln 2$ joules of energy (where k is the Boltzmann constant and T temperature). In essence the principle is a reformulation and extension of the Second Law of Thermodynamics, which states that the entropy of a closed system cannot decrease. I suggest this may explain several features of cognitive effort, namely the fact that the cognitive processes which are effortful are invariably those which involve flexibly manipulating information in working memory.

Landauer's argument centres on the issue of logical reversibility, which refers to whether the output of a logical operation uniquely determines its inputs (see Figure 6.9). An operation is logically reversible if there is a one-to-one mapping between its input and its output; for example, the negation operation NOT is reversible – if the output is TRUE the input must have been FALSE and vice versa. Conversely, an operation is logically irreversible if there is a many-to-one mapping between inputs and outputs; for example, the conjunction operation AND is irreversible because, given the output is true, you cannot know whether the inputs were TRUE-TRUE or FALSE-FALSE (and similarly if the output is FALSE). Another way of framing this is to say that a logically reversible operation preserves information about the previous state of the system (even if just implicitly), whereas during an irreversible operation this information is discarded.

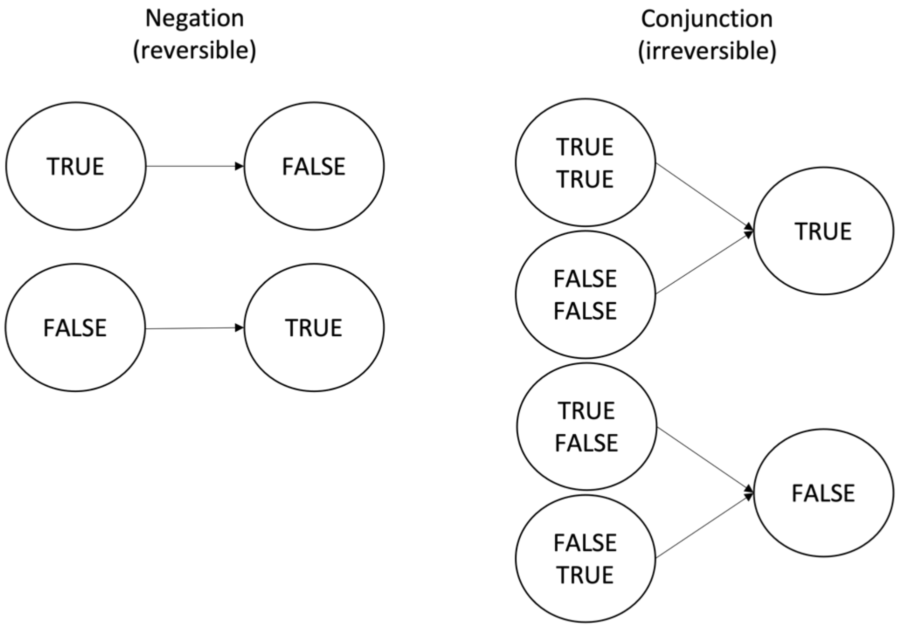


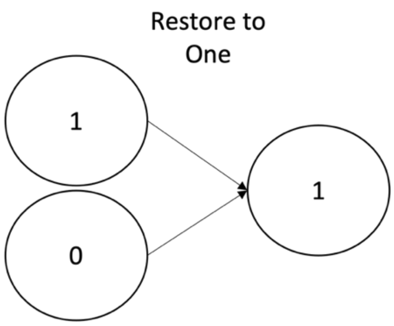
Figure 6.9. The logical (ir)reversibility of computations, in this case negation and conjunction. The former is reversible because the mapping of input to output is one-to-one; the latter is irreversible because the same mapping is many-to-one.

Landauer's second point is that information is encoded physically in a computer, in the states of its *information-bearing degrees of freedom* (IBDF; note that the rest of the computer and its environment make up the non-information-bearing degrees of freedom of the system). For example a bit of information could be encoded in the position of a particle, the charge state of a capacitor or, in the brain, the firing of a neuron.

The effect of a logically irreversible computation then—in fact, the defining feature—is that it reduces the number of logical, and therefore physical, states that the IBDF of the system could take. This reduction corresponds to a decrease in entropy, which has to be compensated for by an increase in entropy elsewhere (such as in the non-IBDF). This is Landauer's Principle.

Landauer (1961, p. 187) gives the example of an operation called 'Restore To One', which takes a binary input and, regardless of its value, outputs one (see Figure

6.10). This resets a unit of memory, discarding whatever information it previously held. If we imagine performing this operation on an ensemble of bits in thermal equilibrium (such that there are initially an equal number of ones and zeros) then the number of different states in the ensemble will be reduced from two to one, resulting in a reduction in the entropy of the ensemble of $kT\ln 2$ joules per bit (see Equation 6.34). This is the so-called Landauer Limit, the theoretical lower bound of energy dissipation for any computer carrying out irreversible operations.



Equation:

$$\begin{aligned}\Delta S &= S_{input} - S_{output} \\ &= kT\ln 2 - kT\ln 1 \\ &= kT\ln 2 \end{aligned} \tag{6.34}$$

Figure 6.10. Calculating the energy dissipation resulting from the operation 'Restore To One'. (left) 'Restore To One' maps any input, regardless of its value, onto one. In so doing, the number of states that can be occupied decreases from two to one (assuming inputs and outputs are binary); (right) The calculation of the minimum necessary energy dissipation

Landauer (1961) runs through a number of other examples, including more complex computations, situations where the entropy is computed over time rather than over a statistical ensemble, and where the compression of the output state space is not completed within a single cycle of computation. In all cases, the same result holds. Likewise Bennett (2003) discusses and refutes the main objections that have been raised against Landauer's Principle over the years, including most notably the claim that logically irreversible operations might be implemented in a thermodynamically reversible way. I refer the reader to these two papers for further details. In addition, it should be noted that in recent years significant empirical evidence has also been accumulating that validates the Landauer Limit of

$kT \ln 2$ joules per bit erased: most notably, Bérut et al. (2012) verified that shifting a charged particle trapped in a bistable potential well rightwards, regardless of its current position (i.e. carrying out the 'Restore to One' operation), dissipates an amount of energy that approaches the Landauer limit asymptotically as the movement time increases. For a review of this and similar experiments, see Lutz & Ciliberto (2015).

6.4.1 Landauer's Principle in cognition

In applying Landauer's Principle to cognition, my contention is that energy dissipation as a consequence of discarding information provides a strong rationale for cognitive effort costs. In particular I propose that energetic costs are of two types: first there are costs like those I have discussed already, that result from clearing or otherwise 'resetting' memory; second, there are more specific costs of removing noise and controlling precision. In this section I will discuss these two potential sources of costs in turn.

Three cognitive processes consistently identified as cognitively effortful are task switching, response inhibition, and updating the contents of working memory (see e.g. Shenhav et al., 2017, and Westbrook & Braver, 2015). All of these, I would argue, are instances of 'Restore To One' type operations, in the sense that they involve clearing part of the contents of working memory and replacing it with new information. In task switching for example, performing any controlled, non-automatic task depends on setting up and maintaining a representation of the rules and requirements of the task, known as the task set; task switching, and other processes like it (including attention shifting, rule switching and switching between different stimulus-response mappings), are then thought to require discarding this task set and setting up another one appropriate to the new task (Apps et al., 2015; Chiu & Lantis, 2009). This necessarily dissipates energy because the new task set is put in place regardless of what was previously held in memory. Likewise when a prepotent response is inhibited this means that the information encoded in the action signal must be discarded, and so again it gets dissipated to the environment as heat. Intuitively it seems plausible that this loss of energy should constitute a cognitive cost which the brain seeks to minimise.

Another cognitive process that appears to be effortful involves removing noise and therefore controlling the precision of cognitive signalling (Manohar et al., 2015). It is not so straightforward to identify whether there are energetic costs of removing noise, however, at least compared with the case where memory is being cleared completely. This is in part because there are some subtleties in the reasoning here, which will be important when I discuss some of the empirical results below.

In the first place, we are not looking to clear memory entirely, only to remove that portion of the signal which is noise. This is like trying to restore an ensemble of bits not simply to one, but to whichever value, one or zero, comprised the original signal before it was corrupted by noise; the challenge is to find a general operation that achieves this without knowing explicitly what the 'Restore to' target is (if we knew the target with certainty then of course we would already have access to a copy of the original signal). Nevertheless, by treating the task of removing noise as an example of a 'Restore'-type computation, it becomes clearer that what we are seeking to do is to reduce the entropy of the signal, which must come at an energetic cost.

One suggested mechanism by which noise might be attenuated in the brain is by averaging across an ensemble of independent neurons and then thresholding the result (as in e.g. Manohar et al., 2015). For example, imagine monitoring a simple ensemble of three neurons, with firing state represented by 0 or 1, that all encoded the same signal originally but have since been corrupted by noise. Assuming order does not matter, you could initially observe any of four possible states of the ensemble, 111, 110, 100, and 000. If you then take an average across the ensemble (and round up/down as appropriate so that the outputs are binary), you will observe one of only two possible output states, 111 or 000. The number of possible states has reduced by half, corresponding to a dissipation of $kT\ln 2$ joules. It can be shown that, as the size of the ensemble under consideration grows, the energy dissipated increases logarithmically.

Ultimately it is the thresholding operation here which is the cause of the reduction in the number of possible states, and it is worth examining this in more detail. We can model a neuron very simply as a particle trapped in a bistable potential well (see Figure 6.11): when the particle is in the left-hand well the neuron is silent; small, subthreshold changes in the neuron's membrane voltage do not affect the neuron's state, but a sufficiently large depolarisation will trigger sodium channels, inducing a transient current to the right that pushes the particle into the second well, corresponding to the firing of an action potential; simultaneously the sodium/potassium pump polarises the neuron, inducing a constant current that pushes the particle to the left.

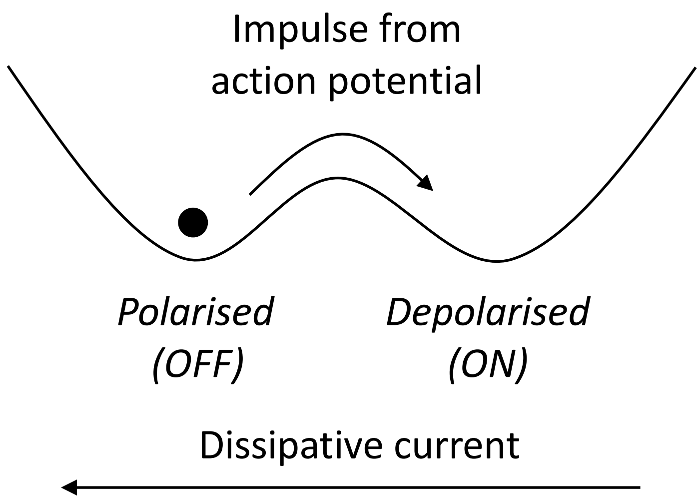


Figure 6.11. Modelling the removal of noise on the level of a single neuron. The state of the neuron is represented by a particle in a bistable potential well with two states, OFF (highly polarised) and ON (depolarised). Noise is controlled through the depth of the wells and the strength of the dissipative current, which prevent random fluctuations from influencing the state of the particle but increase the energy required when a signal is delivered.

The leftward current essentially implements a 'Restore To Zero' computation, resetting the neuron regardless of its current state. This erases any changes in membrane potential resulting from thermal noise before they are able to accumulate, but comes with two related costs: first, the by now familiar cost of erasing information, in the form of heat energy dissipated to the environment; secondly, the fact that any input signal to the neuron now needs to be stronger in order to overcome the leftward current and push the neuron above the threshold potential into the right-hand well. This means that, in order to maintain the neuron in the ON state—for example when trying to maintain information precisely in memory—we need to constantly supply a force to the particle equal to the force of the leftward current.

This arrangement defines a non-equilibrium steady state (NESS), a type of system in which there is constant, positive entropy transfer from the system to the environment over time. These systems are relatively well-characterised in physics, and it is generally possible to compute the entropy production (energy dissipation) rate exactly (Cocconi et al., 2020). The calculation is beyond the scope of this chapter, but this would be an important result to achieve because it would provide exact predictions of the power consumption of the brain when carrying out operations such as working memory. These could then be compared with empirical evidence of cognitive effort costs to allow us to test relatively directly my hypothesis that the former explains the latter. Similar studies have been conducted previously in other domains; e.g. Mehta and Schwab (2012) calculated the power consumption of a cell that uses energy to precisely measure external ligand concentrations, and Tu (2008) did similarly for power consumption of the flagellar motor of the E. Coli bacterium.

Finally, demonstrating that there is already some empirical support for the idea of intrinsic, energetic cognitive costs, I will briefly mention a study conducted by Padamsey et al., 2022. In it, they assessed visual coding precision in mice undergoing food restriction, while simultaneously conducting whole-cell recording in V1. They found that the neurons of mice that were food-restricted had lower

AMPA receptor conductivity (in Figure 6.11, weaker rightwards current), higher input resistance (shallower potential wells) and lower resting polarisation (weaker leftwards current) compared with mice that had had food *ad libitum*. We can interpret these results as a set of adaptations to the lower energy environment that reduce the energy dissipated during the encoding and maintenance of visual information, but at a cost of precision – in Figure 6.11, flattening the potential wells and reducing the strength of the polarising current reduces the energy dissipated during signalling but also means it is easier for thermal fluctuations to push the particle from one side to the other. Indeed, Padamsey et al. found exactly this, with lower rates of ATP use in mice in the food restriction group, but also lower signal-to-noise in the neuronal responses and worse behavioural (visual discrimination) performance. Thus this directly links control over precision with energetic costs, and in particular matches the mechanism suggested in Figure 6.11 above, in which noise is removed via a dissipative current which can be up or downregulated depending on energy requirements.

In summary, then, I suggest that there are fundamental energetic costs of cognition, and that these seem to map directly onto the kinds of processes that are traditionally described as effortful, namely task switching, response inhibition, working memory updating and maintenance of signal precision. These energetic costs are described by Landauer's Principle which states that there is an unavoidable cost of any computation which is logically irreversible, i.e. that discards information. This cost refers specifically to heat dissipated to the surrounding environment (or more precisely to the system's non-information-bearing degrees of freedom) and it is this that I suggest corresponds to effort costs.

6.5 Discussion

In this chapter I have advanced two conceptually different, but complementary ideas: first that a number of phenomena that appear to be evidence of cognitive effort costs are not necessarily so – instead, these can be accounted for without effort costs, if the per-trial change in wealth experienced by participants is non-ergodic. The one cost that cannot be accounted for in this way is the cost of controlling precision, which dovetails with the second part of this chapter, in which I discussed Landauer's Principle that there is an unavoidable energetic cost of computations that discard information from memory. Such computations include removing noise, and therefore I suggest that this energetic cost may underlie the subjective effort costs associated with control over precision.

The implications of this work as a whole are potentially significant, as there has been little progress in recent years on developing a theory of cognitive effort costs. In this chapter I have presented a relatively comprehensive account of a number of different aspects of subjective effort costs and effort-based decision-making, with which we should be in a position to make stronger predictions about behaviour, and therefore to better understand one of the key limiting factors in cognition and motivation. Even if the ideas presented in this chapter turn out to be incorrect, they will at least have introduced a fresh perspective which may help to kindle better theories in the future.

6.5.1 Implications and limitations of the Ergodicity Economics account of effort costs

There are a number of more specific implications of the Ergodicity Economics work. The core point is that maximising the expected value of one's choices is not universally optimal and depends on whether we can safely assume that the per-trial change in wealth is ergodic. If the ergodicity assumption is not satisfied then several aspects of motor and cognitive control immediately become possible to explain without recourse to intrinsic effort costs:

- In motor control, movement endpoint noise is costly because, over time, it results in a lower growth rate of wealth. Specifically, if the reward dynamic is multiplicative the cost will be proportional to endpoint variance, which agrees with the theory of Harris & Wolpert (1998).
- What appears to be effort discounting—valuing a task less as the effort demand increases—can also be explained if the effort manipulation affects response (and therefore outcome) noise. As above, in non-ergodic regimes, outcome noise subtracts from the growth rate of wealth and is therefore costly.
- By combining Ergodicity Economics and Rate-Distortion Theory we can generate normative working memory recall distributions. The distribution when the reward dynamic is multiplicative approximately reproduces empirical working memory performance (von Mises-like distribution of recall error).
- There is an optimum solution to the speed/accuracy tradeoff, provided the reward regime is non-ergodic. In agreement with previous empirical and theoretical work (Manohar et al., 2015), this solution is dependent on both the expected reward and outcome noise.

Of course the chief limitation of this account is that in most cognitive effort experiments the reward dynamic within the task is additive, so that the per-trial change in wealth is indeed ergodic. In this case strictly speaking most of the results derived above do not apply – optimal decision-making should then consist of simply maximising the expected change in wealth. However, there are three ways I would respond to this concern. First, we observe that people *do* behave as if outcome noise is costly – for example, empirically people tend to have approximately logarithmic utility functions (e.g. Groom & Maddison, 2018), so they at least behave as if they anticipate the change in their wealth to be non-ergodic. Second, ergodicity is a restrictive assumption that *a priori* is unlikely to hold very often in the real world – as a general rule, do we really expect that people have access to the same outcomes regardless of their current wealth, or instead are the outcomes they are able to achieve dependent in some way on what they received in the past? Third, and related to the previous point, although we specify what the reward dynamic is within the 'small world' of an experiment, participants who do our tasks of course live within a larger, real world in which the actual reward dynamic they experience may be different. Thus participants may use a non-linear ergodicity transformation even though we, from the point of view of the experimenter, may think they ought to use a linear one.

This in turn tends to shift the interpretation of Ergodicity Economics towards treating it as a descriptive theory (i.e. simply measuring how people behave), rather than the normative theory it was intended to be (i.e. prescribing how people *should* behave). Ultimately probably the only solution to this is careful experimentation. As an example, a study conducted recently by Meder et al. (2019) measured participants' empirical utility functions in different reward dynamics – they found that the utility functions themselves were not exact ergodicity transformations (i.e. they were not perfectly linear in the additive dynamic and logarithmic in the multiplicative dynamic), but nevertheless participants did adjust their utility function to the different dynamics, just as one would expect if the utility function is really an ergodicity transformation. This supports the idea that participants probably do employ ergodicity transformations, but that these take account of not just the parameters in the small-world of the experiment but also of other outside information as well.

6.5.2 Implications and limitations of applying Landauer's Principle to effort costs

The main significance of the section on Landauer's Principle is that it shows there are intrinsic energetic costs of cognition that seem to map fairly neatly onto those operations we know are subjectively effortful. More precisely, however, I proposed that these costs may reflect energy dissipation as heat. This is a different, and somewhat more exact, prediction compared with earlier (failed) attempts to link effort with intrinsic metabolic costs like glucose consumption (Gailliot & Baumeister, 2007; Gailliot et al., 2007). In particular, although one study mentioned above did find a significant relationship between energy consumption at the cellular level and behaviour that looks to be related to effort avoidance (Padamsey et al., 2022), it is unclear whether this relationship should be expected to hold on the scale of the whole human brain. Instead I think effort costs may reflect the need to avoid accumulating too much heat within the brain, either because of the direct damage this may cause or because this in turn increases the thermal noise in the IBDF which necessitates further noise removal. This is not an area that has been well-researched at all so far, but of what has been done it is interesting to note that brain temperature fluctuates quite substantially (by 3–4°C), and that it tends to be consistently hotter than arterial blood, so that at least part of the function of cerebral blood flow may be to cool the brain, not just to supply oxygen and nutrients (Kiyatkin, 2019). This implies that the accumulation of heat is at least a genuine problem that the brain has to manage and seek to minimise.

Probably the biggest limitation with this idea is that of course the Landauer Limit describes only the minimum energy dissipation required; the actual energy dissipated could be much more, while at the same time it is true that logically reversible operations could also be implemented in a way that is thermodynamically irreversible, and therefore dissipates energy. Thus the relevance of the Landauer Limit is initially unclear – this is ultimately an empirical question which will need to be resolved by measuring real energy dissipation in the brain and comparing it with predictions of the theory. Relatedly, I mentioned above that there is more detailed mathematical work that could be done to calculate the entropy production rate during different cognitive operations. This would be especially useful in providing precise predictions of the energy dissipation for comparison with empirical data.

6.5.3 Conclusion

In this chapter I have presented two complementary ideas looking at different aspects of cognitive effort costs, and exploring to what extent we can show that intrinsic costs do (or do not) exist. On the one hand, Ergodicity Economics suggests that a number of phenomena that are usually taken as evidence of intrinsic effort can also be accounted for by optimal decision-making in the absence of effort costs, provided the context is (or is believed to be) non-ergodic. On the other hand, controlling precision (removing noise) in the brain does necessarily dissipate energy, and I suggest that this transfer of heat to the surroundings may be treated as an intrinsic effort cost by the brain.

Chapter 7. General Discussion

This discussion section will provide a synthesis of the ideas and results presented over the course of the four experimental chapters and one theory chapter comprising this thesis. After giving a brief summary of the aims, hypotheses and results of each chapter, I will then discuss the ways that these relate to the overall aims of this thesis, which were to investigate the role of effort in Pavlovian bias, and the associations between effort, Pavlovian bias and symptoms of anxiety and depression. I will then discuss the implications and limitations of each chapter, leading to suggestions for future research. Finally I will end with a concluding section, summarising the findings of the thesis and the subsequent main points of this general discussion.

7.1 Summary of individual chapter aims and main results

The overall aim of this thesis was to investigate the relationship between effort and control over Pavlovian biases. In particular I was interested in exploring the potential mechanistic links between effort, Pavlovian biases and symptoms of anxiety and depression, which previous research has suggested are separately all associated with one another (Husain & Roiser, 2017; Dayan & Huys, 2008), but which have not been considered together before. I started by examining whether, through a regime of deliberate behavioural practice, participants could become better at overcoming their Pavlovian biases (Chapters 2 and 3). Subsequently, having identified that indeed they could, I then sought to test the hypothesis that this result was due to a cognitive control mechanism acting on Pavlovian biases. Specifically I focussed on the corollary of this hypothesis that the strength of Pavlovian biases should depend on one's willingness to exert effort (because cognitive control is dependent on effort; Shenhav et al., 2017). In order to pursue this idea I first had to develop a new measure of cognitive effort sensitivity that would be more suitable for individual differences research than previously existing measures – this became the Number Switching Task (Chapter 4). Then in Chapter 5, I set out to test whether there was a relationship between effort sensitivity (quantified using the Number Switching Task) and the strength of Pavlovian biases. Following these four empirical chapters, I presented a standalone theory chapter in which I attempted to address what I consider to be a significant problem at the core of cognitive effort research, namely that we still do not understand why cognitive effort appears to be costly (Chapter 6).

7.1.1 Chapter 2

The aim of Chapter 2 was to see whether the influence of Pavlovian biases on behaviour could be altered through a programme of behavioural training. Although Pavlovian biases themselves are generally conceived of as fixed responses to predictions of reward and punishment (Guitart-Masip et al., 2012), it has been suggested that they may be able to be overcome through the action of cognitive control. Most of the evidence for this is indirect, however, being based on neuroimaging results that show a correlation between frontal brain activity and reduced Pavlovian biases. By looking at whether the strength of Pavlovian biases could be deliberately changed, I hoped to be able to provide stronger evidence of the ability to control Pavlovian biases. I therefore conducted a blinded, sham-controlled study in which participants were trained specifically on the high-conflict trial types of the Orthogonal Go/No-Go task (Guitart-Masip et al., 2011). I compared their performance before and after the training to determine whether the active training group experienced any changes relative to the sham training group.

In addition, the idea of *enhancing* control is an important ambition in itself, both with regards to Pavlovian biases specifically (which have been linked to symptoms of mental health conditions like anxiety and depression; Dayan & Huys, 2008) and also in other areas of cognition more broadly. I anticipated that any enhancement of control on the Orthogonal Go/No-Go Task would also extend to control over cognitive bias in the secondary tasks (the Affective Bias task and the Risk Taking task; Aylward et al., 2020; Rutledge et al., 2015), as well as reduced symptoms of anxiety and depression.

On the contrary, however, I found that the active training appeared to have no significant effect. Unfortunately the interpretation of this result was hampered somewhat by an issue with the full version of the Orthogonal Go/No-Go task that prevented participants' responses being recorded. As a result I was only able to consider the change in performance during the five training sessions. I observed that the active training group showed no significant improvement over the training, which strongly suggests that it had no effect, but of course without the Baseline–Follow-up comparison we cannot be completely certain. Surprisingly there was a significant improvement over the course of training in the sham training group, which is superficially quite difficult to explain; however, it seems this effect may have been driven by relatively low accuracy in the first training session (which then immediately improved by the second session). The most likely explanation therefore is that, before the start of the first training session, all participants had to some extent forgotten the stimulus associations, but those in the sham group recovered quickly because their trials were easier than for those in the active group.

As was to be expected given the lack of a significant active training effect, there were no significant training group or timepoint effects on either of the secondary tasks. There was, however, a significant main effect of gamble framing on the Risk Taking Task, replicating a key result from previous studies with this task (Rutledge et al., 2015). There were also no training group effects on the anxiety and depression symptom scales, though surprisingly there was a small main effect of timepoint on depression symptoms, which I suggested in Chapter 2 was either a placebo effect or an artefact of repeated testing.

7.1.2 Chapter 3

This study was aimed at implementing a number of improvements to the previous experiment, namely: the study was moved entirely online, allowing us to recruit a much larger sample and therefore achieve greater statistical power; the issue with the full Go/No-Go task was fixed; I implemented a more comprehensive set of instructions, comprehension checks and exclusion criteria; additionally, the

Affective Bias task was altered to use visual, rather than auditory, stimuli (Daniel-Watanabe et al., 2022) – the former being more suitable for online testing. My predictions and hypotheses for this study nevertheless remained the same as in Chapter 2.

This time I did observe a significant training effect, with participants in the active training group showing a greater improvement between Baseline and Follow-up than those in the sham training group. This was reinforced by computational modelling results which indicated that the Pavlovian bias parameter was reduced all the way to zero in participants who completed the active training. Regarding the secondary measures, there were still no significant differences between the training groups, indicating that the improvement in control over Pavlovian biases did not transfer to other domains. However, there was again a significant main effect of timepoint on depression symptoms (which reduced after training in both training groups, active and sham). Similarly, affective bias became less negative after training in both training groups.

7.1.3 Chapter 4

In this third study I looked to develop a measure of cognitive effort sensitivity able to fulfil two main criteria: the difficulty of the task had to be able to be standardised across participants; and the manipulation of effort level within the task had to be achieved without affecting the probability of success (Chong et al., 2016). Both of these conditions related to the need to avoid confounding from probability discounting, which is where the value of a choice is affected by the probability of obtaining reward from it – even if the offered rewards are held the same, if the probability of achieving them is lower, then the expected value will be reduced. Probability discounting and effort discounting then become impossible to disentangle. My main aims in this study were therefore to explore the new task I had designed (the Number Switching Task), verify that it met these criteria and investigate a number of potential associations between effort sensitivity and other self-report cognitive and behavioural measures.

The most important result was that the task did seem to work as anticipated. I found clear evidence of effort discounting, while at the same time rates of success across the different effort levels were held constant. The standardisation of the task was also successful, as I observed relatively little variability in success rates between participants. There was a significant effect of effort level on completion times, suggesting that, as one might expect, the more demanding levels of the task required greater control (Shenhav et al., 2017). Finally with regards to validation, I found with the Subjective Task Load measure that participants consistently rated the effort levels as being progressively more demanding, except on the dimension of performance; here, they correctly reported that performance was unchanged by the effort manipulation. This provides further reassurance that the results on the Number Switching Task reflected genuine effort discounting and not just experimenter demand effects. Finally I then demonstrated the use of a computational model of the task for individual differences research, from which I extracted participant-level effort sensitivity, reward sensitivity and intercept parameters to then be correlated with other measures of interest. There were, however, no significant associations with effort sensitivity in this study; the only significant relationship was between reward sensitivity and Need for Cognition, a construct representing participants' enjoyment of cognitively demanding activity.

7.1.4 Chapter 5

In this final empirical chapter, my aim was to investigate further the role, suggested in Chapter 3, of cognitive effort in exerting control over Pavlovian biases. My specific hypothesis was that overcoming Pavlovian biases may to some extent depend on exerting sufficient effort, and therefore differences between people in their Pavlovian biases may reflect alterations in effort-based decision-making, such as in sensitivity to effort costs. In carrying out this study I made use of the cognitive effort task described in the previous chapter. Specifically I looked at whether there was any correlation between the decision-making parameters measured by the NST and Pavlovian biases measured by the Go/No-Go task. In addition I also examined whether there was any relationship between both effort processing and Pavlovian biases, and anxiety and depression symptoms.

The results of this study were, however, relatively mixed. On the one hand I found that there was, as predicted, a significant positive correlation between model-based measures of effort sensitivity and Pavlovian bias, suggesting that participants who were typically more affected by effort costs tended also to show higher Pavlovian biases. However, analysis of the model fits suggested there were possibly some issues, leading to questions over the extent to which this result can be seen as reliable. Moreover the result was not replicated in the model-agnostic analysis, so ought to be interpreted with caution. In secondary analyses, I found that there was a significant correlation between effort sensitivity and symptoms of trait anxiety and depression, which corresponds with earlier work linking effort with both of these conditions. On the other hand, I found there was no significant correlation between Pavlovian bias and anxiety or depression symptoms – this qualifies earlier results which have shown significant associations both in clinical samples and in healthy participants undergoing a state anxiety manipulation (Mkrtchian, Aylward, et al., 2017; Mkrtchian, Roiser et al., 2017).

7.1.5 Chapter 6

Chapter 6 was a standalone theory chapter in which I aimed to address what I see as the biggest problem with cognitive effort research at the moment, namely that we still cannot explain why effort should be costly. My ambition was not necessarily to solve this problem outright, but instead to introduce some new ideas from outside of neuroscience, which I hope may stimulate future debate and provide momentum for research on this issue. Specifically, in this chapter I first showed that what appears to be evidence of cognitive costs—effort discounting—can in fact be accounted for by optimal decision-making in the absence of any intrinsic costs, provided outcomes are (or participants believe them to be) non-ergodic. The classic example of this is when rewards are multiplicative: what you gain or lose depends on what you already have. The mathematical framework on which this observation is based is called Ergodicity Economics (Peters et al., 2019) and from it I have demonstrated a number of other specific results. For example, given a multiplicative dynamic, effort costs should depend quadratically on outcome variance (in the domain of motor control, this now finally explains why physical effort costs are associated with movement endpoint variance; Harris & Wolpert, 1998). In the domain of working memory, we can recreate the characteristic von Mises-like distribution of response errors. Finally it also explains why motor and cognitive vigour are costly and why the speed-accuracy trade-off can be "broken" by reward (Manohar et al., 2015).

The one cost that does not seem to be accounted for in the Ergodicity Economics framework is that of attenuating noise – from an economic standpoint, outcome noise is costly and should always be minimised as much as possible. In the second part of this chapter I therefore addressed this gap directly, reviewing a second idea, Landauer's Principle (Landauer, 1961), which explains that there are unavoidable energetic costs of attenuating noise. Together these two ideas, Ergodicity Economics and Landauer's Principle, seem to provide a more or less comprehensive account of effort costs. To what extent this account is found to be correct will of course depend on future experimental research.

7.2 Implications

Together the four empirical chapters of this thesis contribute to an improved understanding of the role of effort in control over Pavlovian biases, a link which had not been made at all before. In particular these results suggest that we should consider Pavlovian biases within the framework of effort-based decision-making. In the introduction to this thesis I posed several questions, including: why do Pavlovian biases exist, and why do we not rely on the supposedly optimal instrumental systems alone? I would now hazard at least a partial answer that, when Pavlovian biases affect behaviour, it is because we have made an economic decision not to exert control; this decision in turn depends on external quantities like the incentive offered for accurate responding, and internal quantities like the estimated efficacy of exerting control, and the subjective effort costs of doing so. By understanding Pavlovian biases in terms of effort, we are then able to describe the calculations and mechanisms that are thought to be involved (see Shenhav et al., 2013, 2017) and therefore also identify a set of targets that we might try to manipulate if we were to seek to shift these biases (see Section 7.4 below).

The particular significance of Chapters 2 and 3 was to provide direct evidence that Pavlovian biases are flexible and able to be changed. This is an important, implicit assumption of the hypothesis that these biases are subject to cognitive control, and one that had not been tested previously. Earlier papers had speculated on the existence of a cognitive control mechanism that regulates the Pavlovian system, but the actual evidence mainly comprised neuroimaging studies which correlate frontal brain activity with performance and claim that this reflects cognitive control (Cavanagh et al., 2013; Guitart-Masip et al., 2011, 2014). Thus Chapters 2 and 3 allow us to be more confident in the assertion that Pavlovian biases are in principle able to be controlled. Building on this, in Chapters 4 and 5, I then showed that the strength of Pavlovian biases is associated with a participant's willingness to exert cognitive effort. If this result is validated and confirmed (note discussion of the limitations of this study in Section 7.3 and future research needed in 7.4) it would be consistent with the idea that the influence of Pavlovian biases on behaviour is affected by control, and that this in turn requires effort. This would also help to inform our understanding of the results of Chapter 3, because it is then possible that the training worked by altering participants' willingness to exert control over their biases. This would potentially have wider, more significant implications for enhancing effort in other domains, but of course further research is needed to answer this directly (see Section 7.4).

These results are illustrated in Figure 7.1, which reproduces the proposed scheme for the relationship between effort, control and Pavlovian biases shown previously in the introduction. Here, though, I have now also highlighted the relevance of Chapters 3 and 5 in particular, the sources of the main results of this thesis, highlighting how these address the links in the chain between effort and cognitive control on the one hand, and subsequently between control and Pavlovian biases on the other.

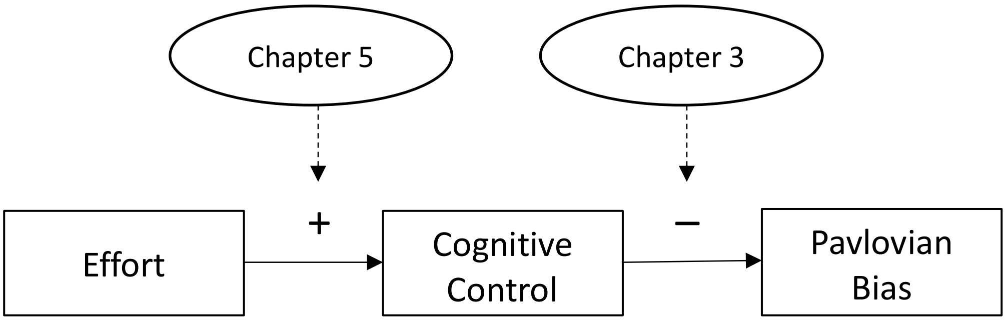


Figure 7.1. The assessed relationship between effort, cognitive control and Pavlovian biases. This figure was previously presented in the introduction as the *proposed* relationship between these quantities; now, informed in particular by the results of Chapters 3 and 5, it seems that this scheme broadly holds true.

7.2.1 New treatments for anxiety and depression symptoms?

One of the secondary, but still important, implications of the study in Chapter 3 is that it suggests that behavioural training might prove a useful approach for trying to treat enhanced Pavlovian biases in, for example, anxiety or depression. Naturally this training is still a long way off from even pre-clinical research in its current state, but it is nevertheless an intervention that may have some promise.

The use of this training as a potential treatment for conditions like anxiety and depression of course ultimately depends on it shifting not just Pavlovian biases themselves but also, more importantly, symptoms. The failure to see a significant active training effect on symptoms in both Chapters 2 and 3 was therefore not ideal; in reality, however, the training used here was relatively short and low intensity, so this negative result should by no means be regarded as conclusive. Indeed SSRIs, which are believed to have a similar effect on symptoms through bottom up changes in biases, also take several weeks to impact on mood (Harmer & Cowen, 2013), so this result is not entirely unexpected. Likewise, the lack of transfer of enhanced cognitive control to the other two tasks (the Affective Bias

Task and the Risk Taking Task) is not fatal – transfer effects are notoriously difficult to achieve and, as we will discuss in Section 7.4, if they are a priority in future studies then there is scope to alter the training to try to facilitate them.

7.2.2 Improved measurement of cognitive effort

The new cognitive effort task (Chapter 4) clearly makes a practical contribution to cognitive effort research in that it fulfils a need for a task which is designed explicitly for individual differences research, particularly research with clinical populations whose cognitive capacity may be decreased relative to healthy controls. One of the methodological difficulties in studying cognitive effort in these populations is that it is difficult to disentangle differences in effort sensitivity from differences in the chance of successfully completing the task; in other words, if we observe greater effort discounting in patients, say, this may be because they experience greater effort costs or because the expected value of the task is lower. The new cognitive effort task resolves this issue by allowing the task difficulty to be standardised across participants; as such it could potentially be very useful in future individual differences and mental health studies.

7.2.3 Theoretical advances

Finally, the significance of Chapter 6 is naturally somewhat different from that of the four empirical chapters. The key motivation was to address a major problem at the core of cognitive effort research: although effort costs are an important topic of research, with a lot of studies (including in this thesis) based on quantifying them and relating them to other aspects of cognition, we do not know why they exist; indeed, we do not have a theory which says they necessarily do exist at all. This chapter is therefore important first of all in highlighting that this is a problem which urgently needs addressing – there is little point in building up a research programme that looks at, say, the neural correlates of effort costs or their association with other aspects of behaviour and cognition if the foundations ultimately turn out to be shaky. Secondly, the ideas presented in Chapter 6 give a relatively comprehensive account of possible sources of effort costs which, though it may be superseded in the future, will hopefully have provided some new impetus to this topic.

7.3 Limitations of studies

There were, as I acknowledged in Chapter 2 itself, some major problems with the first training study. The most significant of these was of course that we did not have usable data for the full Orthogonal Go/No-Go Task. This meant that we had to rely on the data from the training sessions themselves to infer that there was no improvement in the active training group and, while this was fairly convincing, it nevertheless does leave some room for doubt. Besides this, it was also naturally rather unsatisfying to be missing data for one of our primary measures. In addition to this, the study did not include any explicit comprehension checks or performance-based exclusions, so it is possible that the data quality is not as good as it could have been. As we saw later in Chapters 3 and 5, when these checks were then put in place, some exclusions were necessary; therefore it is feasible that in the earlier study some participants may have been included who misunderstood the instructions or failed to pay sufficient attention. Finally, the sample size was definitely a limiting factor in this study, making it hard to interpret the null result with much confidence. The sample size had been determined based on detecting a difference in the training effect between the active and sham groups of $d = 0.6$, which I had determined based on results from an earlier pilot study; this was too large, however, to allow me to claim that the null training result that we then observed was evidence of there being no training effect at all, as opposed to it merely being small.

Subsequently these limitations were all addressed in a replicated and improved training study, reported in Chapter 3 – the sample size, and therefore statistical power, was substantially increased, the issue with the Go/No-Go Task was fixed, and comprehension checks and preregistered, performance-based exclusion criteria were added. Having then found a significant training effect in this version of the study, perhaps the biggest remaining limitation is that we still did not see any transfer either to the secondary tasks (the Affective Bias Task and the Risk Taking Task) or to the anxiety and depression symptom measures. Thus while we think the best interpretation of these results is that participants were able to exert greater cognitive control over their Pavlovian biases following the active training, it is interesting that this did not seem to translate to an enhancement of cognitive control in general. It may have been that participants learned to predict when control would be required based on the appearance of specific stimuli (so-called 'proactive control'; Braver, 2012) rather than by learning to better detect response conflicts and deploy cognitive control effectively in response ('reactive control'). If so this may limit the usefulness of this training intervention as a tool for enhancing cognitive control, since it is generalised control and not task-specific control that we would most want to improve. That said we did not optimise the intervention for transfer to other contexts, so this is an aspect of the study that future research may be able to improve on.

In Chapters 2, 3 and 5 there was consistently no significant correlation between Pavlovian bias and anxiety or depression scores, which was somewhat surprising given previous research. However this result should not be over-interpreted: the previous studies focussed on comparing patients with healthy controls, rather than examining correlations with continuous symptoms in the healthy population, and moreover the difference between patients and controls depended on a state anxiety manipulation (threat of shock). Therefore these earlier studies are not exactly comparable with the studies in this thesis, and it may be that, as I suggested in Chapter 5, the association between symptoms and Pavlovian bias depends on symptoms being sufficiently severe that individuals meet clinical thresholds for diagnosis, and/or the presence of additional stress (Mkrtchian, Aylward et al., 2017).

The development and validation of the Number Switching Task (Chapter 4) was largely successful, but two limitations do need to be borne in mind. Firstly, we do not have estimates of test-retest reliability currently, meaning we cannot be totally confident yet that effort sensitivity, as measured by this task, is a stable cognitive trait. Secondly, the task has so far only been tested with healthy participants, despite it being explicitly designed to be suitable for use with clinical populations and in particular for making comparison between patients with mental health or neurological conditions and healthy controls. There is therefore further work to be done before the NST can be considered fully validated. This also relates to one of the other apparent limitations with this study, that the effort discounting effect was relatively modest – indeed there were some participants who accepted every offer regardless of effort level; I would expect, however, that in a clinical sample known to have difficulties with cognitive effort, the discounting slope would be much steeper. There is therefore clearly an overall balance to be had: the task needs to have sufficient dynamic range to be able to work with both populations. Exploring this balance and optimising the task will, of course, require further studies to be done.

The final experimental study, reported in Chapter 5, had some more substantial limitations affecting our confidence in the modelling results. In the Implications section above I took at face value the apparent correlation between model-based effort sensitivity and Pavlovian bias which, if true, would tally with our hypothesis that Pavlovian biases are determined by effort-based decision-making and cognitive control. This correlation was, however, derived from a model of the Go/No-Go Task which seemed to have some issues with the quality of the fit, as revealed by small but systematic errors in the posterior predictions and an unexpected combination of posterior parameter estimates. Informal checks as part of the modelling process suggested that this was not a result of the specific model chosen; instead there seems to have been a deeper problem with fitting the pattern of data seen in Chapter 5. More fundamental work, perhaps considering a different set of models, may therefore be required.

7.4 Directions for future research

7.4.1 Understanding the effects of training on effort

The studies reported in this thesis present a number of fruitful avenues for future research. Regarding the two training studies (Chapter 3 in particular) I have suggested that the significant training effect reflects enhanced cognitive control over Pavlovian biases, and the final study (Chapter 5) further indicates that Pavlovian biases are related to willingness to exert effort. However, these studies do not prove the link conclusively, so there is a need to conduct further follow-up studies to try to understand the mechanism of the training effects. For example it is possible that the training may have shifted participants' effort sensitivity (though this is perhaps unlikely given the training effect did not generalise to the secondary tasks); alternatively participants may have learned that specific stimuli predicted the need to exert greater control, and that this control was effective in leading to better outcomes. It would therefore be worthwhile to conduct a study in which effort sensitivity is assessed before and after training, in order allow us to compare these two hypotheses directly. In addition one could also look at varying the outcome controllability during the training phase – since participants show greater reliance on Pavlovian bias when outcome controllability is lower (Dorfman & Gershman, 2019), presumably they would likewise experience less of a training effect when practicing on lower controllability trials. If so this would match the prediction of the Expected Value of Control theory, that participants should only exert control where this is expected to lead to improved outcomes (Shenhav et al., 2013). Finally, it would also be worthwhile examining changes in some of the neural correlates of Pavlovian biases identified in earlier studies, e.g. activity in the IFG (in fMRI) or mid-frontal theta power (in EEG; Cavanagh et al., 2013). Although, as I have noted, these neural correlates are not proof in themselves of the involvement of cognitive control, they nevertheless help to provide converging evidence which would be particularly convincing if it could be shown that the successful training is associated with changes in the activity in these regions.

Of course we also need to address the fact that, in Chapter 5, the apparently significant correlation between effort sensitivity and Pavlovian bias is derived from a model that is clearly struggling to fit the observed data; this result therefore requires further verification before we can give it full credence. Part of the issue is that in this thesis I have prioritised continuity of the modelling across previous chapters, but it may be that the set of models so far considered are simply not able to fit the data well; future work could consider starting from scratch, considering a different range of models, but of course there is no guarantee that this will work either. Ultimately what may be helpful is a replication study, in order to ascertain whether this result is reliable, and indeed whether the model fitting problem persists.

7.4.2 Optimising the Pavlovian bias training for transfer effects

Staying on the topic of the training studies, another extension that was already suggested in Section 7.3 above would be to try to optimise the training with the goal of achieving transfer to other domains and tasks. As noted above, the training programme used in Chapters 2 and 3 was primarily designed simply to prove the principle that Pavlovian biases can be trained, and as such participants were trained on just one stimulus per condition and tested on the same stimuli. With this in mind it is maybe understandable that we did not see transfer effects. The obvious next development would therefore be to include multiple stimuli per condition and different training and test sets. This would be a much more difficult training regime, probably requiring both more sessions and more trials per session in order to see a substantial training effect, but is likely to be what is required in order to achieve transfer to other tasks. This further research would be especially necessary if the training is ultimately to be considered seriously as a potential treatment for Pavlovian avoidance biases in anxiety and depression.

Regarding anxiety and depression symptoms specifically, I have already noted that one reason we may not have seen significant training effects is because the association between these symptoms and Pavlovian bias may depend on the presence of an additional stressor. To test this explanation it may therefore be worthwhile including in future studies of the Pavlovian Bias training a modified version of the Go/No-Go task in which a state anxiety manipulation, like threat of shock (as in Mkrtchian, Aylward et al., 2017) is included. It would be interesting to investigate whether there is any interaction between symptoms and response to the training – we might predict, for example, that threat of shock will 'undo' some of the training effect, pulling participants back towards their original Pavlovian bias values, particularly for those higher in trait anxiety.

7.4.3 Further validating the cognitive effort task

As noted above, the Number Switching Task (Chapter 4) was originally designed with the intention of its being used with clinical populations, but we so far have not validated it with these groups. This will therefore need to be a priority for the future; as well as showing that the task continues to avoid confounding by probability discounting, these studies will also need to examine the precise configuration of the task parameters (such as the calculation of the allowed time for each trial). We must ensure that the cognitive effort demand is sufficiently high for all participants, especially when testing patients and healthy controls in the same experiment.

In addition, it would also be worthwhile measuring the test-retest reliability of the metrics derived from the NST – this could potentially be done in one of the studies suggested in Section 7.4.1, where the NST is administered at several timepoints alongside the Go/No-Go Training. This would allow us to be more confident in the task and the idea that we are measuring a stable trait in cognitive effort sensitivity.

7.4.4 Testing the theoretical proposals

Finally, the theoretical ideas discussed in Chapter 6 obviously present a number of promising suggestions for future empirical work. Regarding the Ergodicity Economics part of the chapter, it would be interesting to examine whether effort costs are affected by changes in the reward dynamic – if my hypothesis is correct, and effort costs typically do reflect a cost of outcome noise, then effort sensitivity should increase if the dynamic is shifted from additive towards multiplicative (i.e. the required ergodicity transformation becomes increasingly concave). In addition to this the theory makes relatively specific predictions about the optimal vigour of a response and the shape of the error function of working memory. Again these could both be tested with relative ease.

The second part of Chapter 6, which considered the implications of Landauer's Principle for cognitive effort, is less easy to test explicitly – probably the best chance of testing this idea is with cellular-level experiments such as that by Padamsey et al., 2022. In humans, one could feasibly design an experiment in which different incentives were offered for different levels of working memory precision, say, in which case my strong prediction is that higher levels of precision would consume more energy. However, the key difficulty with a study like this is selecting a method to measure energy consumption. Global measures such as blood sugar levels are unlikely to be successful (consider the demise of ego-depletion theory; Gailliot & Baumeister, 2007; Gailliot et al., 2007; see Kurzban et al., 2013 for a review of the evidence against it); perhaps the most promising technique could be PET imaging with radiolabelled glucose. Of course, an experiment like this would need careful feasibility studies first. More fundamentally, I think there is also still work to be done to develop the mathematical foundations of this particular hypothesis. Therefore attempts to test this idea experimentally may be easier to carry out further in the future, once this theoretical work has been done and we are able to make more precise predictions.

7.5 Conclusion

In this thesis I have investigated cognitive effort and its involvement in exerting control and overcoming Pavlovian biases. I have shown that Pavlovian biases are indeed not fixed, they can be shifted through a relatively straightforward programme of behavioural training, and their influence on behaviour may be associated with participants' sensitivity to cognitive effort costs. Together these results suggest we can think about Pavlovian biases in terms of effort-based decision-making and control: people are able to control their Pavlovian biases but to do so they must be willing to exert effort; this in turn depends on economic factors, including not just the incentives on offer for accurate performance but also the efficacy of control and the intrinsic cost of effort.

I had hoped that these results would also relate to symptoms of anxiety and depression, in which both enhanced Pavlovian biases and higher effort sensitivity have previously been reported. Here, however the results were more mixed. I found no associations between the strength of Pavlovian biases and either anxiety or depression symptoms in any of the studies we conducted; there were, however, significant correlations between effort sensitivity and both trait anxiety and depression. This latter result was encouraging and matches our expectations based on previous research. On the other hand, future research on the relationship between Pavlovian biases and anxiety and depression should take our results into account, particularly if, as I hope, the Pavlovian bias training programme is to be considered as a potential treatment for some of the symptoms of these conditions.

Overall these results are suggestive, but more work needs to be done to conclusively demonstrate the link between Pavlovian biases and effort-based decision-making. In particular this might include more training studies looking at other factors such as outcome controllability or transfer to control in other domains, as well as neuroimaging to test whether the training effect is associated with changes in brain activity in regions previously associated with Pavlovian bias (like IFG).

Finally, I also included a theory chapter aimed at providing a normative account of the existence of effort costs. My express intention with this chapter was not necessarily to provide a comprehensive and unassailable explanation. Rather, I sought first to highlight some of the problems involved, which have otherwise been largely overlooked in the cognitive effort literature, and then to suggest some promising new ideas which, it is hoped, may provide a stepping stone to a complete solution in the future.

In summary, effort is a significant component in cognition, which interacts with fundamental processes like the expression of Pavlovian responses and contributes to symptoms of several mental health conditions. By studying cognitive effort we can begin to understand, and even modify, these other aspects of cognition. There remain, however, significant and underappreciated gaps in our theoretical knowledge which I have highlighted and sought to address in this thesis.

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