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Towards a Mechanistic Understanding of Repetitive Behaviours in Psychiatry

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Table of Contents

<u>DECLARATION, ONLINE ACCESS AND THE GENERAL DATA PROTECTION REGULATION</u>	4
<u>SUMMARY</u>	5
<u>LIST OF PUBLICATIONS AND PRESENTATIONS</u>	8
<u>ACKNOWLEDGEMENTS</u>	11
<u>CHAPTER 1: INTRODUCTION</u>	14
1.1 What Are Habits & Why Are They Important?	14
1.2 A Cognitive Theoretical Account of Habits: The Dual-systems Framework	15
1.3 Traditional Experimental Measures of Habits	18
1.4 The Role of Habits in Psychiatry	24
1.5 Current Challenges & New Approaches To Studying Habits	36
1.6 Experiments and Hypotheses	45
<u>CHAPTER 2: USING SMARTPHONES TO OPTIMISE AND SCALE-UP THE ASSESSMENT OF MODEL-BASED PLANNING</u>	47
2.1 Introduction	47
2.2 Method	50
2.3 Results	60
2.4 Discussion	72
<u>CHAPTER 3: MODEL-BASED PLANNING IS UNAFFECTED BY KETAMINE, ANTIDEPRESSANT AND INTERNET DELIVERED COGNITIVE BEHAVIOURAL THERAPY TREATMENTS</u>	79
3.1 Introduction	79
3.2 Method	81
3.3 Results	86
3.4 Discussion	91
<u>CHAPTER 4: COMPULSIVITY IS ASSOCIATED WITH ACCELERATED STIMULUS-RESPONSE HABIT-LEARNING</u>	95
4.1 Introduction	95

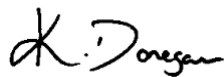
4.2 Method	97
4.3 Results	102
4.4 Discussion	112
CHAPTER 5: DISCUSSION	116
5.1 Summary	116
5.2 Implications	123
5.3 Limitations and Future Directions	133
5.4 Conclusion	137
CHAPTER 6: REFERENCES	139
CHAPTER 7: SUPPLEMENTARY MATERIALS	180
7.1 Supplementary Materials for Chapter 2	180
7.2 Supplementary Materials for Chapter 3	218
7.3 Supplementary Materials for Chapter 4	248

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28th February 2025

Summary

Repetitive behaviours are a hallmark characteristic of several psychiatric conditions including obsessive-compulsive disorder (OCD), disordered eating, addiction, tics, bodily-focused repetitive behaviours, and autism. However, the cognitive mechanisms underlying these repetitive behaviours remain unclear. This is partly explained by the limitations of traditional research methodologies relying on small, underpowered, and cross-sectional study designs. By leveraging online and smartphone-based methods, this thesis aimed to explore the mechanisms underpinning repetitive behaviours at-scale and to identify individuals most susceptible to disorders of habit.

Chapter 2 aimed to develop and validate a gamified task, Cannon Blast, to scale-up the assessment of model-based planning, a cognitive capacity that protects against the over-reliance on habits. In a series of within-subject and between-subject manipulations, we also explored ways to optimise the task's psychometric properties, enhancing its sensitivity in tracking individual differences in compulsivity. In a smaller validation sample ($N = 57$) who completed both the traditional and gamified task, we found a moderate positive cross-task correlation ($r = .40$) and comparable rates of internal consistency. Leveraging data from a large citizen science cohort ($N = 5005$), we replicated established correlates of age, gender, education, and specific associations with compulsivity ($n = 1432$). We found altering transition ratios and reward drift rates improved the sensitivity between model-based planning and compulsivity. Additionally, we found this association can be detected in as few as 25 trials, with additional trials yielding minimal further benefit. This chapter highlighted how model-based planning can be measured at-home, at scale, and can be optimised to better capture individual differences in model-based planning.

Chapter 3 aimed to explore state- versus trait-dependencies of model-based planning. While previous research has consistently shown that individuals with higher compulsivity exhibit reduced model-based planning in cross-sectional studies, it remains unclear whether these differences reflect transient state-like

distress or a stable trait-like vulnerability. Further, while prior studies have found no association between model-based and depression, these studies have largely relied on general population samples. To address these gaps, we followed treatment-seeking patients with anxiety and depression over the initial four-weeks of either internet-based cognitive behavioural therapy ($N = 611$) or antidepressant medication ($N = 83$), alongside a control group with no current psychiatric illness ($N = 83$). Model-based planning and clinical symptoms were assessed at two-time points: pre- and post-treatment. Both treatment groups reported reductions in anxious-depression and compulsivity following treatment demonstrating the transdiagnostic therapeutic benefits of these interventions. However, these clinical improvements did not translate into changes in model-based planning. Furthermore, within-person changes in anxious-depression and compulsivity were not correlated with changes in model-based planning. This chapter suggested that model-based planning is a relatively stable cognitive trait, not necessarily compromised in depression, and may hold specific relevance for compulsive disorders.

Chapter 4 aimed to develop a gamified task to measure individual and clinical differences in habit-learning. Current theories of compulsivity suggest that impairments in goal-directed control (or its computational formalisation model-based planning) shift towards an over-reliance on habits. However, methodological confounds have limited the direct investigation of habit formation in compulsivity. In a pre-registered study, we trained $N = 1000$ crowd-sourced participants on visuo-spatial habits over a week, along with collecting self-report clinical data concerning aspects of psychiatry involving repetitive behaviours such as compulsivity, Tourette's, bodily-focused repetitive behaviours, and autism. We replicated the over-training and time-dependency effects on habit expression, confirming the task's validity. In terms of individual differences, we found habit-learning was reduced in older adults and in females (compared to males) but was accelerated in those with higher education. Crucially, we found compulsivity, rather than other forms of repetitive psychiatric behaviours, was specifically associated with exaggerated habit-learning. This chapter suggested that compulsive behaviours may not only stem from differences in goal-directed control but also an enhanced capacity for habit-formation, refining the habit-hypothesis model of compulsivity.

In sum, this thesis demonstrated the utility of large-scale, online, and smartphone-based research methods in advancing our understanding of the cognitive mechanisms underpinning repetitive behaviours in psychiatry. By employing robust sample sizes and longitudinal designs, this work suggests that higher-order compulsions observed in OCD, disordered eating, and addiction, may be explained by both reductions in goal-directed control, which are trait-like, and an increased propensity to form habits. Furthermore, these compulsive behaviours share some overlap but also appear to be distinct from lower-order repetitive behaviours, such as tics and bodily focused repetitive behaviours, that share a strong motor component. Collectively, this thesis has significant clinical and research implications, supporting a shift from traditional laboratory-based studies to scalable real-world assessments.

List of Publications and Presentations

This thesis incorporates material published and/or in review in the following manuscripts:

Donegan, K. R., Suddell, S., Buabang, E. K., Gallagher, E., Hanlon, A. K., & Gillan, C. M. (2024). Psychiatric correlates of stimulus-response habit learning. Pre-registration: <https://osf.io/quwb3>

Donegan, K. R., Hosseini, S., Penny B. M., Brown V. M., ... Gillan, C. M., & Price, R. B. (2024). Model-based planning is unaffected by ketamine, antidepressant, and internet delivered cognitive behavioural therapy treatments. Pre-print: <https://osf.io/preprints/psyarxiv/hbt6j>

Buabang, E. K., **Donegan, K. R.**, Rafei, P., & Gillan, C. M. (2024). Leveraging cognitive neuroscience for making and breaking real-world habits. *Trends in Cognitive Sciences*.

Donegan, K. R., Brown, V. M., Price, R. B., Gallagher, E., ... & Gillan, C. M. (2023). Using smartphones to optimise and scale-up the assessment of model-based planning. *Communications Psychology*, 1(1), 31.

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The following are presentations arising from this work:

Donegan, K. R., Suddell, S., De Forest, S., Sun, J., ... & Gillan, C. M. Compulsivity is associated with increased stimulus-response habit learning. Oral & Poster Presentation at the Reinforcement Learning and Decision-Making Conference. Trinity College Dublin, June 2025.

Donegan, K. R. Leveraging gamification for longitudinal within-person assessment. Oral Presentation at the Reinforcement Learning and Decision-Making Conference. Trinity College Dublin, June 2025.

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Invited Oral Presentation at the Bodily-focused Repetitive Behaviours Conference, University of Oxford, September 2024.

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Donegan, K. R., Zech, H., Qiu, Z., & Brown, V. M. Parsing different sources of variability in model-based planning. Symposium Talk at the Computational Psychiatry Conference, University of Minnesota, July 2024.

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Donegan, K. R., Brown, V. M., Price, R. B., Gallagher, E., ... & Gillan, C. M. Using smartphones to optimise and scale-up the assessment of model-based planning. Poster Presentation at the Society of Biological Psychiatry Conference, San Diego, April 2023.

Donegan, K. R., Brown, V. M., Price, R. B., Gallagher, E., ... & Gillan, C. M. Using smartphones to optimise and scale-up the assessment of model-based planning. Poster Presentation at the School of Psychology's Annual Research Day, Trinity College Dublin, March 2023.

Donegan, K. R., Brown, V. M., Price, R. B., Gallagher, E., ... & Gillan, C. M. Using smartphones to optimise and scale-up the assessment of model-based planning. Poster Presentation at the Neuroscience Ireland Early Careers Researchers Symposium, Trinity College Dublin, October 2022.

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Chapter 1: Introduction

1.1 What Are Habits & Why Are They Important?

Habits are fundamental to human behaviour, serving as blueprints for actions we frequently repeat (James, 1890). They manifest in countless ways; from simple daily tasks like brushing our teeth to complex routines such as commuting to school or work. With each repetition, these actions become automated, requiring little to no conscious effort to perform (Dickinson, 1985; Poldrack et al., 2005). This efficiency is crucial to human functioning, as it frees up mental resources that can be reallocated to more sophisticated behaviours such as decision-making, problem solving and multi-tasking (Dickinson, 1985; Graybiel, 2008). Without habits, each day would begin anew, requiring us to relearn even the basic functions, ultimately hindering our ability to reach our full potential.

The downside to this efficiency, however, is their inherent rigidity in the face of change (Neal et al., 2006). Due to their imprudent nature, they can lead us to perform ‘slips of actions’, also referred to as habitual errors (Norman, 1981). These errors occur when a behaviour, that has been repeated often in the past, is performed despite no longer being optimal (Mylopoulos, 2022; Norman, 1981). For example, inadvertently entering an old passcode when attempting to unlock your device, or taking a more familiar but unintended turn while driving. Although such habitual errors are minor and inconsequential, in the extreme, researchers have suggested that the same cognitive mechanisms underlying habits may contribute to clinical conditions such as compulsions in obsessive-compulsive disorder (OCD), drug-taking in addictions and bingeing behaviours in disorder eating (Ersche et al., 2021; Everitt & Robbins, 2016; Foerde et al., 2021; Gillan et al., 2011; Hartogsveld et al., 2022; Vandaele & Janak, 2018). Thus, the same automated and effortless nature that make habits efficient can simultaneously be their demise (Mendelsohn, 2019).

Given their profound implications from a society and public health perspective, it is unsurprising that habits have long been a focus of basic, applied and clinic research (Dickinson, 2012, 2016; Gardner, 2015; Gardner et al., 2021; Gillan, Robbins, et al.,

2016; James, 1890; Mendelsohn, 2019; Orbell & Verplanken, 2020; Verhoeven & de Wit, 2018; Voon et al., 2015; Wood & Runger, 2016). This thesis adopts a cognitive perspective on habits and aims to advance the mechanistic understanding of these behaviours and their relation to psychiatric conditions. In the context of this thesis and in the summarisation of the literature, the term 'habit' is reserved for the observable behaviour and defined as an action triggered by contextual stimuli, which develops through frequent repetition, and persists regardless of current beliefs or motivations (Buabang et al., 2024; Gardner & Lally, 2023; Robbins & Costa, 2017).

1.2 A Cognitive Theoretical Account of Habits: The Dual-systems

Framework

The leading cognitive theoretical account of habits is based on a dual-systems framework. This framework theorises that habits arise from the interplay between two systems: the goal-directed and stimulus-response system (Corbit, 2018; de Wit & Dickinson, 2009; Dickinson & Balleine, 1994; Dolan & Dayan, 2013; Wood et al., 2022) (Figure 1.1A). These systems rely on distinct cognitive representations that compete for dominance in controlling behaviour (Corbit, 2018; Wood et al., 2022).

The goal-directed system governs deliberate and intentional actions that are designed to achieve optimal outcomes based on current beliefs and goals (Dickinson & Balleine, 1994; Dolan & Dayan, 2013). It operates by encoding contingencies between actions and their consequential outcome, along with the value of that outcome ($A-O^v$) (Adams, 1982; de Wit & Dickinson, 2009; Dickinson & Balleine, 1994; Dolan & Dayan, 2013) (Figure 1.1A). This allows individuals to flexibly select actions based on the outcomes that best align with their current motivations (Dickinson, 1985). However, this flexibility comes at the cost of being computationally demanding, involving multiple higher-order cognitive processes such as simulation, valuation and prospective planning (Buschman & Miller, 2014; Dickinson & Balleine, 1994).

On the other side, the stimulus-response system governs behaviours that have been established through frequent repetition and that are divorced from the current value of the associated outcome (Dickinson, 1985; Dickinson & Balleine, 1994). This

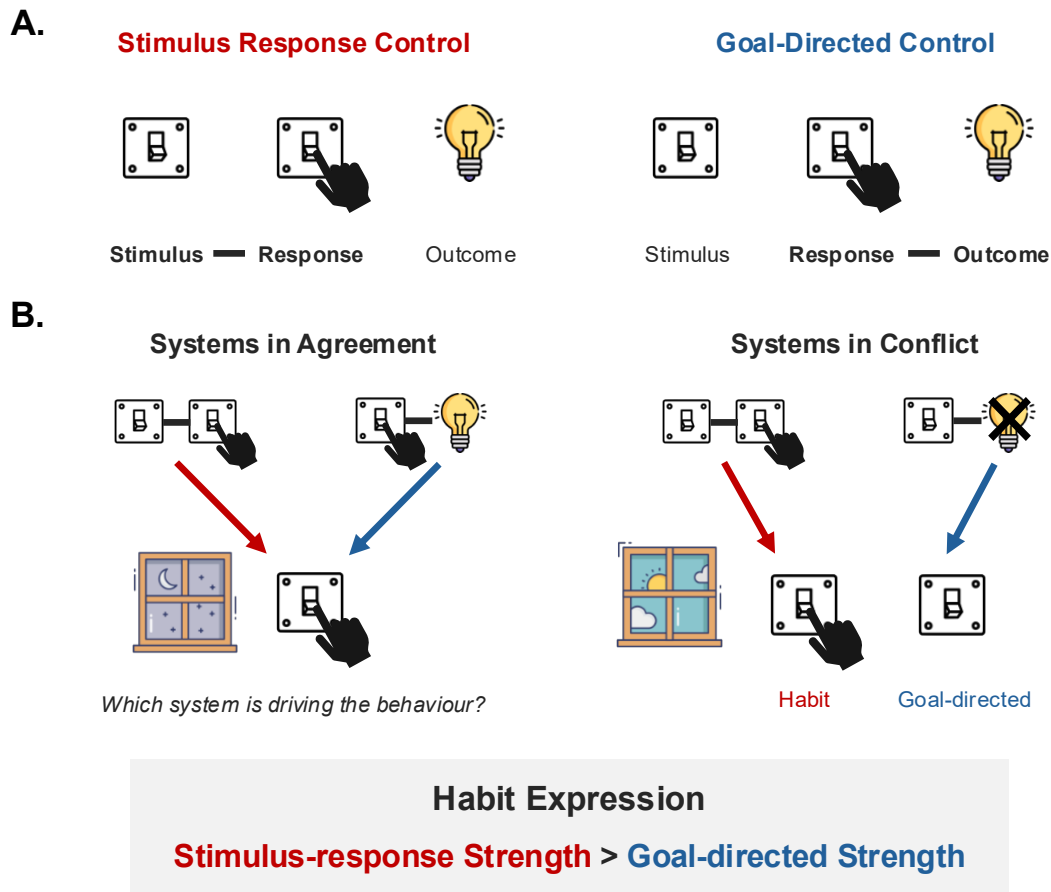


Figure 1.1. The dual-systems of control. **A.** Stimulus-response control occurs when a stimulus (e.g., light switch) triggers an associated response (e.g., flipping the switch) through repeated reinforcement, independent of its outcome. In contrast, goal-directed control involves performing a response (e.g., flipping the switch) to obtain a desired outcome (e.g., light turning on). **B.** When both systems align, the resulting behaviour is identical, making it difficult to determine which system is driving the response. For example, at night-time, flipping the switch could be stimulus-driven, cued by the sight of the light switch, or goal-directed to illuminate the space. However, when the systems are in conflict, habits become evident as they persist despite changes in goal and motivations. For example, during the day when there is sufficient light, habits are triggered by the antecedent stimuli (e.g., light switch), whereas goal-directed control inhibits this response as the outcome (e.g., light turning on) is no longer valuable.

system encodes memory traces between a behavioural response and the antecedent stimulus context (S-R), that strengthen with each reoccurrence (Dickinson, 1985; Dickinson, 2012; Dolan & Dayan, 2013; Wood et al., 2022) (Figure 1.1A). While stimulus-bound responses are efficient in stable environments, they are ill-suited when there is a degree of changeability (Wood & Runger, 2016). As stimulus-driven control produces behaviour that is automatically triggered by familiar cues and lacks foresight of its succeeding outcome, this system is often

referred to in the literature as the 'habit system' (Gardner & Lally, 2023). However, it is important to distinguish the distinction between the 'habit' stimulus-response system (hereafter the stimulus-response system), which is a necessary cognitive representation, and the behavioural expression 'habit'. The former alone is not sufficient to explain habit expression (De Houwer, 2019; Gardner & Lally, 2023).

The cognitive distinction between the two systems is complimented by neuroimaging research supporting distinct yet parallel sub-circuitry within the cortico-striatal-thalamo-cortical loop (Balleine & O'doherty, 2010; Dolan & Dayan, 2013; Knowlton & Patterson, 2018). This has been explored through lesion studies in rodents (Corbit & Janak, 2010; Yin & Knowlton, 2006; Yin et al., 2004; Yin et al., 2006) and in functional and structural imaging in humans (de Wit et al., 2009; de Wit et al., 2012; Gläscher et al., 2009; Tricomi et al., 2009; Valentin et al., 2007). Such research has identified regions involved in encoding action-outcome contingencies and outcome valuation that support goal-directed control, including the caudate (or dorsomedial striatum in rodents), medial orbitofrontal cortex, ventromedial cortex and dorsolateral cortex (de Wit et al., 2012; Gläscher et al., 2009; Valentin et al., 2007). Conversely, the putamen (dorsolateral striatum in rodents) and sensorimotor cortices have been identified to support the gradual build-up of stimulus-response associative traces (Balleine & O'doherty, 2010; de Wit et al., 2012; McNamee et al., 2015; Tricomi et al., 2009), though some of these findings have been inconsistent (Gera et al., 2023; van Timmeren et al., 2024; Watson et al., 2018).

According to the dual systems framework, habits occur when the strength of the stimulus-response system is greater than the engagement of the goal-directed system (Buabang et al., 2024). This can result from either an overexpression of the stimulus-response system or a weakened goal-directed system, which may be influenced directly or indirectly by various external factors such as stable contexts (Bouton, 2021), over-training (Adams, 1982; Dickinson, 1985), stress (Schwabe & Wolf, 2009, 2010; Smeets et al., 2023; Wirz et al., 2018), and working memory (Otto, Gershman, et al., 2013; Otto, Raio, et al., 2013). Understanding the relative contributions of these systems on habit expression has posed significant challenges

for cognitive researchers and has ultimately shaped the way we study habits in the lab.

1.3 Traditional Experimental Measures of Habits

When both goal-directed and stimulus-response systems are in agreement, they can produce the same behaviour, making it difficult to delineate which system is driving the action (Buabang et al., 2024) (Figure 1.1B). Thus to experimentally measure habits, researchers have focused on situations where these systems are in conflict (Figure 1.1B). To differentiate goal-directed from habitual behaviour, researchers have typically assessed sensitivity to changes in outcomes and contingencies. If a behaviour is performed in the absence of self-proposed goals, the behaviour is considered not goal-directed and therefore a habit (Vandaele, 2024).

1.3.1 Outcome devaluation and degradation

Much of our mechanistic understanding of habits has been derived from outcome devaluation and outcome degradation paradigms (Dickinson, 1985). Initially developed in animal research, these paradigms involved training rodents on stimulus-response associations, such as a lever-press (stimulus = lever, response = pressing the lever), which was accompanied by a reinforcer (e.g., sucrose pellets) (Adams, 1982; Dickinson, 1985). After a period of training, the value of the outcome was removed either through satiation or taste aversion, altering the desirability of performing the response. In the test for habits, rodents were re-exposed to the lever. The rodents who reduced their responding were suggested to have updated their behaviour appropriately in light of this change (i.e., sensitive to outcome devaluation), were considered to be under goal-directed control. By contrast, rodents who continued to press the lever, though no longer desirable, were considered to be acting habitual. Contingency degradation followed the same premise, but instead altered the relationship between the action and the outcome (i.e., making the outcome occur independently of the action). Habits were tested whether behaviour adjusted to this change. These tasks have since been adapted for use in human research, where participants learn stimulus-response associations through on-screen stimuli and keyboard responses, in exchange for food, points or

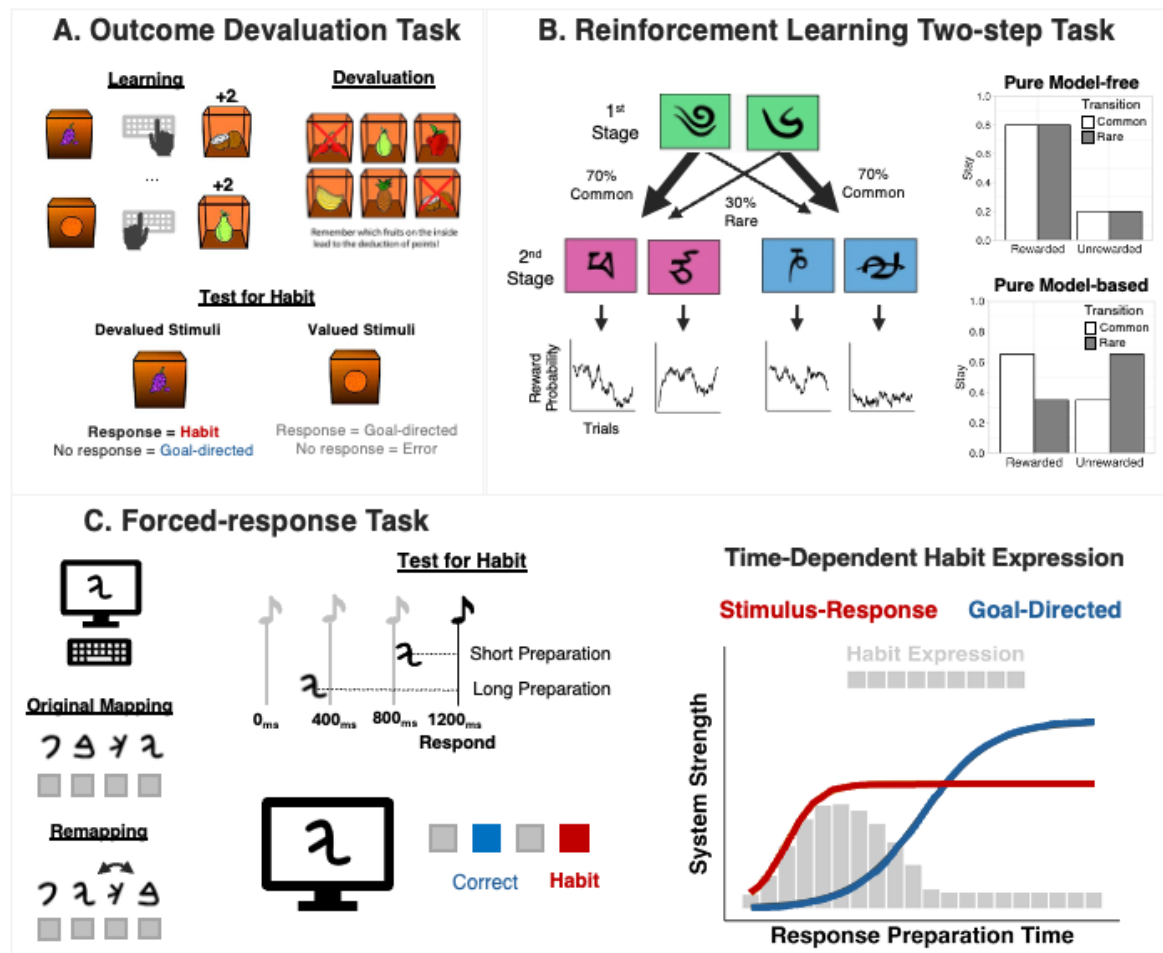


Figure 1.2. Experimental paradigms to measure habits and goal-directed control. A. Outcome Devaluation Paradigm (Slips-of-action Task): Participants first learn associations between stimuli (e.g., fruits) and responses (e.g., keyboard responses) and outcomes (e.g., another fruit that earns points). Next, they are informed which outcomes no longer yield points (i.e., outcome devalued) and that they must withhold responses to the associated stimuli. In the test, adjusting the behaviour by withholding responses to devalued stimuli reflected goal-directed control, whereas persisting in responding to devalued stimuli reflected habits. **B.** Two-Step Task: Participants make sequential choices to maximise rewards. Each trial begins with a choice between two green boxes, each probabilistically leading to one of two second-stage states (pink or blue). One box transitions to the pink state 70% of the time (common transition) and to the blue state 30% of the time (rare transition), with reverse probabilities for the other box. In the second-stage, participants chose between two boxes and receive either a reward (e.g., point or money) or no reward. The probability of being rewarded drifts slowly and independently over the course of the task for each second-stage option. Pure model-free learners repeat their first-stage choice after a reward, regardless of transition. In contrast, pure model-based learners account for the task's transition structure, repeating first-stage choices after a reward that followed a common transition but switching the first-stage choice after a reward that followed a rare transition. **C.** Forced-Response Paradigm: Participants first learn associations between four arbitrary stimuli and distinct keyboard responses. After training, half of these associations are reversed, and participants must relearn until a desired level of accuracy is reached. In the test, participants are forced to synchronise their response with the fourth of four evenly

spaced tones. The onset of the stimulus is varied relative to this desired response imposing differing preparation times, in a random uniformed distribution. Habits are identified as responding with the previously trained response to remapped stimuli. Habits are more likely to be expressed at shorter preparation times when behaviour is governed by stimulus-response control (red line). In contrast, goal-directed control (blue line) which requires more time to engage effectively inhibits habit expression at longer response preparation times.

monetary rewards before being devalued (de Wit et al., 2007; de Wit et al., 2012; Tricomi et al., 2009) (Figure 1.2A).

This paradigm has offered many insights into factors that influence habit expression in animal research. For example, one of the earliest demonstrations of overtraining effects was conducted by Adams (1982) who compared sensitivity to devaluation between rodents who trained on stimulus-response associations minimally (100 lever presses) or extensively (500 lever presses). Insensitivity to the devaluation was only observed in the extensive training group, where rodents continued to press the lever even though it was no longer desirable to do so. This was in contrast to the group that received minimal training, who were able to adjust their behaviour accordingly. This is a well-established finding in animal research (Adams & Dickinson, 1981; Dickinson, 1985), but has proven difficult to translate in humans. Using the human equivalent paradigm, only one study by Tricomi et al. (2009) found that extended training increased habit expression ($N = 32$). They showed while participants who trained for one day were sensitive to outcome devaluation, demonstrated by a reduction in responding to devalued stimuli, participants who trained for three days were insensitive to devaluation and responded equally to both valued and devalued stimuli. However, large-scale multi-site replications, including two preregistered studies, failed to reproduce this effect (de Wit et al., 2018; Gera et al., 2023; Pool et al., 2021). It has been suggested that these inconsistencies may be due to the heinous activation of goal-directed processes that constrain habit expression, especially in experimental settings where participants have ample time to deliberate and select an appropriate response.

One limitation of this paradigm is that it relies on a zero-sum hypothesis (Balleine & Dezfouli, 2019; Robbins & Costa, 2017). That is, if the behaviour is not goal-directed, it must be a habit. However, such null effects could arise from other factors,

such as unestablished learning of the stimulus-response association or exploration (Balleine & Dezfouli, 2019; Watson, O'Callaghan, et al., 2022). Additionally, the binary classification of habits at a group level can mask individual differences (Pool et al., 2021). For instance, Pool et al. (2021) found a group-level significant difference in responding to the devalued relative to the valued stimuli, suggesting participants relied on goal-directed behaviour. Despite the group-level finding, further inspection revealed approximately one-third of participants showed sensitivity to outcome devaluation whereas the other two-third responded equally across valued and devalued stimuli, suggestive of habitual behaviour. A final and crucial criticism of outcome devaluation and contingency degradation paradigms is that these approaches do not indicate whether habit expression results from strong stimulus-response learning during training or equivocally weakened fluctuations in exerting goal-directed control at the time of testing.

1.3.2 Reinforcement Learning Accounts of Goal-Directed and Habitual Control

In an effort to separate the influence of goal-directed and stimulus-response associative learning on behaviour, Daw et al. (2011; 2005) leveraged trial-by-trial based analysis and computational models to assess action-outcome associations without the interference of associative learning. Within this account, actions and habits are conceptualised as two complementary learning mechanisms embedded within a reinforcement learning framework; model-based and model-free learning respectively (Daw et al., 2011; Daw et al., 2005). These computations differ in the valuation of actions. As the name implies, model-based learning relies on an internal model of the environment that maps the contingency between actions and their associated valued outcomes (Daw et al., 2005; Doll et al., 2012; Tolman, 1948). Within this decision tree, actions and their associated outcomes are simulated and then evaluated to determine the most optimal outcome. In contrast, model-free learning is based solely on reinforcement history, whereby actions that have previously led to rewards accrue value and are more likely to be repeated (Thorndike, 2017). While relying on cached values from experience is computationally efficient, it neglects the actions current value (Daw et al., 2005; Dolan & Dayan, 2013; Doll et al., 2012).

To test the influence of model-based and model-free control in decision-making, Daw and colleagues (2011) developed the two-step Markov decision-making task (Figure 1.2B). In this task, agents (either human participants or simulated agents) sequentially navigate through probabilistic states in order to maximise rewards. In the first stage, participants choose between one of two actions, each associated with a transition that is likely ('common' transition) and unlikely ('rare' transition) to a second stage state. At the second stage, participants choose again between two actions for the chance of obtaining a reward. Model-based and model-free learners exhibit distinct behavioural patterns in this task (Figure 1.2B). Consider a scenario in which a first-stage action results in an unlikely rare transition to a second-stage state where the chosen action is rewarded. A purely model-free learner would repeat the same first-stage action that previously led to the reward, irrespective of the transition likelihood. In contrast, a purely model-based learner would switch their first-stage choice, understanding that the alternative first-stage action is more likely to transition to the rewarding second-stage state. This results in distinct stay probabilities across trials where pure model-free learners display high stay probabilities following rewarding trials, independent of transition type, while model-based learners exhibit high stay probability following rewarding trials but only when a reward follows a common transition and a low stay probability when a reward follows a rare transition. Across all trials and participants, behaviour in this task is best explained by a hybrid model incorporating both model-free and model-based influences. This is statistically reflected in a mixed effects model, in which reward, transition, and their interaction are regressed on the subsequent trials stay probability. Model-free behaviour is reflected by the main effect of reward, whereas model-based behaviour is indicated by a significant reward and transition interaction. Computationally, reinforcement learning models can be used to estimate the relative contribution of model-based and model-free learning through a weighting free parameter ω , where values closer to 0 indicate a greater reliance on model-free learning and values closer to 1 reflect stronger model-based learning.

Conceptually, model-based and model-free learning have been mapped as computational proxies for goal-directed and stimulus-response control, respectively. Empirical studies have shown that individual differences in model-based learning correlate negatively with habit expression, supporting the view that this

sophisticated, computationally demanding type of prospective planning is a sensitive formalisation of goal-directed control (Friedel et al., 2014; Gillan, Otto, et al., 2015; Sjoerds et al., 2016). In contrast, despite model-free learning resembling the way stimulus-response associations are developed (e.g., based solely on past rewarding experiences), several studies have found that individual differences in model-free learning are not predictive of habit expression. Exploring the cross-task correlation between insensitivity in outcome devaluation and model-based/model-free learning, Friedel and colleagues (2014) found a positive association between relative insensitivity to devaluation and model-based, but not model-free control. This has since been replicated in two separate experiments, both observing no association between habitual expression in the outcome devaluation task and model-free control (Gillan, Otto, et al., 2015; Sjoerds et al., 2016). One possible explanation for this lack of association is that the trial-by-trial structure of the task, designed to remove associative strength, limits its sensitivity as a measure of stimulus-response control. Furthermore, model-free computations rely on model updating based on values associated with the outcome, which differs from the definition of stimulus-response control that is disassociated from the value of the outcome (Melnikoff et al., 2021; Miller et al., 2019; Pauli et al., 2018). As such, it is now agreed that these traditional measures of habit expression are more sensitive to individual differences in goal-directed control rather than a measure of stimulus-response associative strength.

1.3.3 Time-dependent Nature of Habit Expression

One recent line of research has capitalised on a key distinction between the stimulus-response and goal-directed systems: their timing (Hardwick et al., 2019; Keramati et al., 2011; Luque et al., 2020). This is based on the theoretical assumption that more computationally expansive goal-directed actions take longer to process than lower-order associative stimulus-response links. Drawing on the findings from the cognitive-motor domain where it has been recognised that preparing a response is independent to initiating the response (Haith et al., 2016), researchers questioned whether a similar principle applies to traditional habit paradigms. That is, that stimulus-response habits are prepared, but not expressed, being overridden by more accurate goal-directed behaviour in free-operant test conditions (Courter & Ahmed, 2019). To test this, Hardwick et al. (2019) employed

a forced-response paradigm (Haith et al., 2016) (Figure 1.2C). In this study, participants learned four distinct visuospatial associations between arbitrary stimuli and keyboard responses. Using a within-subject counterbalanced design, participants trained on these associations under two conditions: minimally over one day and extensively over four days, with different sets of symbols and response mappings for each condition. Following training, half of these associations were swapped, and participants trained on these new mappings until they reached a desired level of accuracy, ensuring that performance differences in the test phase were not due to insufficient learning. Critically, in the habit test, participants were forced to respond at the same time on each trial. The stimuli appeared either shortly before or a long before the desired response, varying randomly on a continuous scale. This manipulation controlled the time available between a response being prepared and it being performed. Habitual errors were defined as performing the previously trained response on trials that incurred a remapping. Hardwick et al. (2019) found habitual errors peaked during short preparation trials, between 300-600_{ms} of preparation time, and declined as preparation time increased. This was also moderated by training, with more habitual errors made following extensive compared to minimal training. In a follow-on experiment, these effects were even more pronounced indexed by greater peaks in short preparation trials following a 20 day training condition. These findings suggested that stimulus-response habits can be revealed, but only when the more computationally expansive goal-directed system is limited. Notably, these findings were the first since Tricomi et al. (2009) to demonstrate an overtraining effect on habit expression in human participants. These findings have since been replicated in several smaller lab-based studies (Du et al., 2022; Du & Haith, 2025). This paradigm presents a promising opportunity to test for individual differences in the propensity to form stimulus-response associations and add to our mechanistic understanding habits in psychiatry.

1.4 The Role of Habits in Psychiatry

Habits are recognised as a common characteristic of several psychiatric conditions (Gillan, Robbins, et al., 2016; Verhoeven & de Wit, 2018). In particular, research suggests that habits play a significant role in the development and maintenance of compulsive behaviours in OCD (Gillan & Robbins, 2014; Graybiel & Rauch, 2000), eating disorders (Godier & Park, 2014) and addictions (Everitt & Robbins, 2016). In

these conditions, behaviours are persistently performed despite awareness of their detrimental effects on health, occupational, and social functioning (Robbins et al., 2012). These behaviours have been theorised to arise from imbalances in the dual-systems framework, either due to impairments in goal-directed control or excessive stimulus-response learning, leading to an over-expression of habitual behaviour that manifest into psychopathology. This section will first summarise the role of habits in various neuropsychiatric disorders, followed by discussing how this framework may be useful in aiding our understanding of other psychiatric conditions characterised by maladaptive repetitive behaviours. Finally, I will explore an emerging field taking a dimensional approach and argue for an extension to the transdiagnostic dimensions influenced by habits.

1.4.1 Obsessive-Compulsive Disorder

The largest body of work investigating the role of habits has focused on OCD (Graybiel & Rauch, 2000). Compulsive behaviours are a hallmark characteristic of OCD, manifesting as rigid ritualistic actions (Figeo et al., 2016). These compulsions are often intended to alleviate distress and anxiety associated with intrusive, unwanted thoughts or obsessions (Salkovskis, 1985; Starcevic et al., 2011). For example, individuals may engage in excessive handwashing to alleviate contamination fears or repeatedly check locks to confirm doubts about safety. These behaviours persist despite explicit knowledge of their harmful consequences, such as skin blisters from excessive hand washing and irrationality as failing to perform the behaviour will be catastrophic and could lead to death.

The habit-hypothesis model is a leading theoretical account for compulsions in OCD. This model proposes that compulsions arise from a shift in control from goal-directed action to stimulus-response habits (Gillan & Robbins, 2014). One of the first studies to demonstrate this shift found that OCD patients were insensitive to outcome devaluation, continuing to respond to devalued stimuli compared to controls (Gillan et al., 2011). This finding was replicated in a similar avoidance paradigm where OCD patients were trained to press pedals to avoid mild electrical shocks (Gillan, Morein-Zamir, Urcelay, et al., 2014). After training, researchers overtly disconnected the OCD patients from the shock administering device, rendering performing these avoidant response as redundant. Again OCD patients

failed to update their behaviour in light of the change in value compared to controls and continued to press the pedal. Notably, these behavioural deficits occurred despite patients explicitly reporting that the response was no longer necessary. However, these early studies failed to determine whether habit expression was due to failures in goal-directed control or an excessive build-up of stimulus-response associations.

To address this, studies employed the two-step reinforcement learning task to isolate the influence of goal-directed from stimulus-response strength on habit expression. Voon et al. (2015) found that OCD patients displayed reduced model-based learning, a computational proxy for goal-directed control, compared to controls. This finding has been observed in other clinical samples (Gillan et al., 2020; Gillan, Morein-Zamir, Kaser, et al., 2014) and in general population samples self-reporting OCD severity (Gillan, Kosinski, et al., 2016; Seow et al., 2021). Furthermore, neuroimaging studies have investigated the neural correlates of habit biases in OCD. Insensitivity to outcome devaluation in OCD was found to be associated with hyper-activity in the caudate and medial orbitofrontal cortex, but not the putamen, a region that supports goal-directed action (Gillan, Apergis-Schoute, et al., 2015). Moreover, increased caudate activity was correlated with self-reported urge to perform habitual behaviours in OCD patients. These findings have since been supported by other studies reporting similar alterations in caudate and medial orbitofrontal cortex activity and habit expression in OCD (Banca et al., 2015; Chase et al., 2020). These studies suggest that habit biases in OCD are primarily driven by failures in goal-directed control rather than an excessive build-up of stimulus-response associations. However, as these findings are based on cross-sectional designs, it remains unclear whether impairments are a consequence of OCD symptoms or represent a pre-existing trait vulnerability.

1.4.2 Behavioural Addictions

The habit model has also been implicated in behavioural addictions, including drug (Everitt & Robbins, 2005; Everitt & Robbins, 2016; Halbout et al., 2016; Hogarth, 2018; Hogarth et al., 2013) and alcohol addiction (Chen et al., 2021; Sebold et al., 2014; Sjoerds et al., 2013; Voon et al., 2015). It is proposed that initially these behaviours are goal-directed and purposeful, but with sustained reinforcement, they

transition to stimulus-driven control, becoming difficult to abstain from despite severe fatal consequences (Everitt & Robbins, 2016).

One of the first studies to examine this in humans was conducted by Sjoerds et al. (2013) using an outcome devaluation paradigm. They found that abstinent alcohol-dependent participants were insensitive to outcome devaluation, compared to age and gender-matched controls. These effects were attributed to reduced goal-directed action-outcome knowledge, rather than excessive stimulus-response learning, as no differences were observed during the learning phase. Furthermore, this deficit correlated with reduced activation in the ventromedial prefrontal cortex, a key region involved in goal-directed control. A similar finding was observed in a group of cocaine-dependent participants compared to controls (Ersche et al., 2016), who also exhibited failures in action-outcome knowledge. However, cocaine-dependent participants and controls showed comparable outcome devaluation performance in the aversive-shock procedure similar to Gillan et al. (2014). Both groups equally reduced foot pedal responses corresponding to a now-disconnected shock administrator, suggesting intact goal-directed control. A series of studies conducted by Hogarth et al. (2012; 2011) found both tobacco users and controls remained sensitive to outcome devaluation, and that sensitivity to devaluation was not correlated with tobacco dependence. This suggests that not all addictive behaviours may be characterised by an overreliance on habits.

Evidence from the two-step reinforcement learning task further supports the link between reduced goal-directed control and behavioural addiction in humans. Patients with stimulant dependency displayed reductions in model-based planning compared to controls (Voon et al., 2015). Similarly, detoxified alcohol-dependent patients showed reduced model-based planning compared to controls (Sebold et al. 2013), although this effect became non-significant when controlling for cognitive speed. In a general population sample, greater self-reported alcohol-use dependency was associated with reductions in goal-directed control (Gillan, Kosinski, et al., 2016). However, another study found no association between model-based planning and alcohol consumption in 18-year-old social drinkers (Nebe et al., 2018). These data suggest imbalances in goal-directed control may underlie compulsive drinking and drug-taking, which could be dependent on factors

such as clinical severity and the addictive substance. In an attempt to explore whether model-based deficits predisposed to risky alcohol use in the future, Chen et al. (2021) assessed model-based and model-free learning in 146 18-year-old men and tracked their alcohol use and binge-drinking trajectories over three years. They found lower model-based planning at baseline was negatively associated with future binge-drinking, even after controlling for executive functioning and impulsivity. These findings highlight the protective trait-like nature of model-based planning over habits (Gillan, Otto, et al., 2015) and that variation in goal-directed control may represent a predisposing factor for compulsive drinking.

1.4.3 Eating Disorders

Eating disorders, including binge eating, bulimia and anorexia nervosa, are characterised by severe disturbances in eating patterns, often leading to periods of excessive food consumption, self-induced purging, or extreme food restriction (Rothenberg, 1986). These disorders are also associated with rigid thought patterns and intense preoccupations with weight and food (Klein & Walsh, 2004). For example, individuals with binge-eating disorder feel compelled to engage in periods of uncontrollable eating despite feeling satiated or being aware of the associated health risks. Similarly, obesity can be conceptualised as a failure of self-regulation over eating behaviour, persisting despite negative consequences such as long-term health complications and impaired social and occupational functioning (Horstmann, 2017).

Consistent with findings in OCD and addiction, eating disorders have been linked to reductions in goal-directed control, resulting in imbalances in goal-directed/habitual control. Voon et al. (2015) found that individuals with binge-eating disorder demonstrated deficits in model-based learning compared to controls. However, no differences in model-based or model-free learning were observed between obese participants without binge-eating disorder and controls. However, the average body mass index for the obesity group was only marginally above the clinical threshold, which may have limited the ability to detect between-group effects. Supporting this, Janssen et al. (2020) found that obese participants relied less on model-based planning compared to overweight or normal-weighted participants. A similar pattern is observed in anorexia nervosa. Foerde et al. (2021) found individuals with

anorexia nervosa exhibited reduced model-based learning, which did not remit at weight restoration. This suggested that these deficits may serve as a trait marker for compulsive eating patterns (Foerde et al., 2021). Given the heterogeneity within anorexia nervosa, Brown et al. (2024) investigated whether sub-classifications of this disorder were associated with dissociable alterations in model-based and model-free learning. The study examined individuals with restrictive eating behaviours and individuals with binge/purging behaviours in both reward-based (e.g., gaining \$1 or nothing as in the traditional task (Daw et al., 2011)) or punishment-based (e.g., losing \$1 or nothing). Individuals with restrictive-eating anorexia showed greater reliance on model-free learning against controls for rewards but displayed reduced model-based learning when avoiding punishment. Moreover, greater model-based learning was associated with activity in the orbitofrontal cortex, a brain region implicated in goal-directed control, and was negatively associated with eating disorder severity. No differences in model-based learning were observed between the binge/purging group and controls, which was inconsistent with prior reports of binge-eating pathology associated with impairments in goal-directed control (Foerde et al., 2021; Voon et al., 2015). In a general population sample, Gillan et al. (2016) found that impairments in model-based planning were most strongly associated with self-report levels of eating pathology, surpassing the associations observed with OCD, alcohol addiction and impulsivity. Collectively, findings from clinical and community samples suggest that maladaptive eating behaviours, across various subtypes of eating disorders, are associated with attenuated model-based learning, which may lead to an overreliance on habits in these disorders.

1.4.4 Towards a Dimensional Approach to Habits in Psychiatry

The dual-systems framework has been instrumental in understanding compulsive behaviours in OCD, addiction, and eating disorders. However, similarities in the neurocognitive profile across disorders raise questions about the specificity of these findings to any one diagnostic condition. Additionally, comorbidity rates are high, with greater heterogeneity observed within psychiatric disorders than between (Gillan & Seow, 2020). Transdiagnostic approaches offer a more nuanced framework for conceptualizing mental health, moving beyond arbitrary categorical

boundaries to map latent cognitive mechanisms onto behavioural phenotypes with greater reliability and sensitivity.

1.4.4.1 The compulsive dimension

One of the first studies to adopt this approach aimed to address concerns regarding the lack of specificity of neurocognitive models of habit. Deficits in goal-directed control were observed in a host of psychiatric conditions including OCD, addiction, and eating disorders, but also in social anxiety and schizophrenia (Alvares et al., 2014; Culbreth et al., 2016; Gillan, Robbins, et al., 2016; Voon et al., 2015), prompting researches to question the explanatory value of these findings. Gillan et al. (2016) investigated whether a dimensional factor could better explain variations in model-based planning than individual clinical measures. In a large online general population sample (N>1400), participants completed the two-step task to assess model-based planning while also providing self-report data on a battery of clinical questionnaires related to alcohol dependency, apathy, depression, eating disorder, impulsivity, OCD, schizotypy, social anxiety and trait anxiety. Consistent with prior work in the respective clinical silos, Gillan et al. (2016) revealed non-specific effects. Reductions in model-based planning were associated with greater self-report severity of alcohol dependency, eating disorder, impulsivity and OCD. Next, they conducted an exploratory factor analysis on all 209 questionnaire items. This identified three orthogonal dimensions: anxiety-depression, characterised by high loadings from anxiety, apathy, and depression; compulsivity and intrusive thought, consisting of items from OCD, eating pathology, alcohol dependency, impulsivity and some schizotypy items; and social withdrawal, with items related to social anxiety and isolation. Notably, only the compulsivity and intrusive thought dimension explained individual variation of model-based planning, whereas anxious-depression and social withdrawal did not. Moreover, these effects were greater in comparison to any individual questionnaire measure. This has been replicated in multiple studies across various cohorts including students (Seow et al., 2021), online crowd-sourced samples (Patzelt et al., 2019), and general smartphone-based samples (Sookud et al., 2024). Nonetheless, these studies relied on non-clinical samples. To determine whether effects extended to clinical populations, Gillan et al. (2020) compared the relationship between model-based planning and both OCD diagnosis and a self-report transdiagnostic dimension of compulsivity and

intrusive thought. They found a negative association between model-based planning and compulsivity and intrusive thought, but not with OCD diagnosis, suggest that transdiagnostic constructs may better capture underlying neurocognitive mechanisms.

Interestingly, while general population samples typically do not reveal associations between individual differences in anxious-depression and model-based planning (Gillan, Kosinski, et al., 2016; Seow et al., 2021), one study found that individuals high in depression were more sensitive to the effects of stress on model-based planning (Heller et al., 2018). A separate case-control study found no differences between patients with depression and controls, though warranted replication in a larger sample (N=23) (Knolle et al., 2023). Although evidence linking model-based planning and depression remains inconclusive, transdiagnostic methods provide a compelling picture that the dimension of compulsivity and intrusive thought appears to be a more proximal and sensitive neurocognitive marker than categorical diagnoses.

1.4.4.2 A novel motoric dimension of mental health

A critique of the compulsivity and intrusive thought dimension is its reliance on a limited set of clinical questionnaires. It remains unclear whether this dimension could be enhanced through the inclusion of other psychiatric disorders characterised by repetitive behaviours such as Tourette's, bodily focused repetitive behaviours and certain aspects of autism. While direct behavioural evidence for dual-system imbalances in these disorders is lacking, insights from cognitive neuroscience and treatment studies provide indirect support for the potential conceptualisation of these repetitive behaviours as habits.

Tourette Syndrome and tic disorders are characterised by sudden, repetitive motor or vocal tics, often preceded by an urge or premonitory sensation (Conelea et al., 2010). Tics typically emerge in childhood, peak in adolescence, and may remit in adulthood, though 10–20% of cases persist (Leckman & Cohen, 1998; Peterson et al., 2022). Though direct tests of goal-directed versus habitual control in Tourette's Syndrome are scarce, one study found that unmedicated patients with Gilles de la Tourette Syndrome continued to respond to devalued stimuli compared to controls,

indicating insensitivity to outcome devaluation (Delorme et al., 2016). No differences were observed between the medicated and unmedicated patients or between medicated patients and controls. Further, insensitivity to outcome devaluation was positively correlated with tic severity in the unmedicated group. In a subset of medicated and unmedicated patients, they found greater responses towards devalued outcomes were also correlated with hyper-connectivity between the putamen and motor cortex, regions involved in the gradual build-up of stimulus-response associations. This aligns with prior functional MRI studies demonstrating that conscious tic suppression involved deactivations in the putamen and increased activation in the prefrontal cortex and caudate, regions related to goal-directed control (Albin & Mink, 2006; Leckman, 2002). Other studies have revealed abnormal activity in the putamen and premotor areas at resting state (Worbe et al., 2012) and during tic generation (Bohlhalter et al., 2006). These preliminary findings suggest excessive stimulus-response associations may contribute to tics, though larger samples and study designs that isolate influences of goal-directed strength would be needed to confirm this hypothesis.

Bodily-focused repetitive behaviours (BFRBs) are characterised by self-directed repetitive behaviours such as hair-pulling (trichotillomania), skin-picking (excoriation disorder), nail biting, and cheek chewing, that result in physical harm and significant distress (Conelea et al., 2017; Flessner & Woods, 2006). Until 1987 for trichotillomania (hereafter hair-pulling), and 2013 for excoriation (hereafter skin-picking), these were not recognised as mental health conditions and at present fall under the diagnostic category of OCD-related diagnoses (Hallion et al., 2017; Stein et al., 2010). This lack of formal recognition has likely limited what we know mechanistically about these behaviours. There is some support for impaired inhibition (Chamberlain et al., 2006; Grant et al., 2011; Odlaug et al., 2010), cognitive flexibility (Grant & Chamberlain, 2023; Odlaug et al., 2010) and executive functioning (Barber & Lee, 2024); however, evidence across and within the domains of cognition is largely inconsistent (Blum et al., 2018; Bohne et al., 2008; Chamberlain et al., 2007; Dougherty et al., 2022; Snorrason et al., 2011). This is likely due to methodological differences in tasks used to measure various aspects of cognition and reliance on small and convenient-based (e.g., university students) samples.

Though no study has yet directly tested the imbalances between goal-directed and stimulus-response control, some indirect evidence for a potential mechanistic role of habits can be drawn from neuroimaging studies. Structural MRI studies in hair-pulling have reported altered putamen volumes, both reduced (Chamberlain et al., 2018; O'Sullivan et al., 1997), and increased (Chamberlain et al., 2008); however, sample sizes ranged between 10-18. In a larger sample ($N = 68$), Isobe et al. (2018) found reduced putamen volumes, suggesting unlike its archetype OCD, hair-pulling may align better with motor behaviours in tic disorders than with compulsions (Houghton et al., 2017; Lamothe et al., 2019; Woods & Miltenberger, 2001). Findings in skin-picking are more limited and less conclusive. Preliminary research found reductions in the orbitofrontal cortex and insula (Schienle & Wabnegger, 2024), with connectivity in these regions related to motor symptom severity (Blum et al., 2018), which again are constrained with small samples ($N = 10-35$). These regions are involved in encoding outcome-guided behaviour and inhibitory control respectively and are similarly found to be disrupted in OCD. Overall, while some studies suggest a pathophysiological overlap between OCD and BFRB's, particularly between skin-picking, there are also notable differences suggesting while BFRBs fall under Obsessive Compulsive Related Diagnoses, the mechanisms underpinning these clinically similar behaviours may be distinct (Schienle, 2025).

Autism Spectrum Disorder is characterized by restricted interests and repetitive behaviours, including motor stereotypies, rigid routines, and compulsive rituals (Turner, 1999). Some of these repetitive forms of behaviour may be habitual, yet no direct tests of goal-directed versus habitual control in autism spectrum disorder currently exist. Highly functioning patients with autism spectrum disorder have however demonstrated deficits on a reversal learning task, which were correlated with symptom severity (D'Cruz et al., 2013). The reversal learning task measures flexible choice behaviour that involves learning stimulus-response associations before reversing the response to an alternative option. Participants have to inhibit the prepotent response to the previously associated stimuli that are no longer correct, bearing similarities to the recent response-reversal paradigms used to prob habit formation (Hardwick et al., 2019). Other domains such as task-shifting (Miller et al., 2015), and motor inhibition (Mosconi et al., 2009) have also been identified

as altered in autism. With deficits reported in multiple domains, some have suggested repetitive behaviours arise from wide-spread executive functioning deficits (Leekam et al., 2011). Neuroimaging studies have provided evidence for a disassociation between mechanisms underlying lower-order repetitive acts (e.g., finger tapping) and higher-order repetitive behaviours (e.g., rituals (Langen et al., 2014; Langen et al., 2009)). Lower-order repetitive acts have been linked to alterations in the putamen, a region involved in stimulus-response learning (Hollander et al., 2005). In contrast, higher-order behaviours have been associated with caudate abnormalities involved in goal-directed control (Hollander et al., 2005; Langen et al., 2009; Wolff et al., 2013). These findings suggest that the dual-systems habit model may help disentangle different subtypes of repetitive behaviours in autism spectrum disorder.

While a dimension of compulsivity and intrusive thought has been established as a transdiagnostic dimension linking goal-directed deficits to compulsive behaviours, it is unclear whether repetitive behaviours with a strong motor component, such as tics, BFRBs, and aspects of autism, align with this dimension or constitute a separate construct of mental health. Comorbidity between tics and OCD can be as high as 20% in some cohorts (Kloft et al., 2018). Additionally, BFRBs are diagnostically classified under OC-related disorders, suggesting that similar cognitive mechanisms may underlie these behaviours. However, there are also some notable differences. Unlike compulsions in OCD, which are typically driven by distressing internal experiences (e.g., intrusive thoughts), tics, BFRBs and some autistic-like behaviours (e.g., tapping) are often preceded by sensory urges or a need for gratification rather than an explicit attempt to neutralise anxiety (Conelea et al., 2010). This distinction suggests that motor-driven repetitive behaviours may arise from different cognitive mechanisms than compulsive behaviours. More emerging evidence for this possible distinct dimension comes from recent transdiagnostic approaches. Snorrason et al. (2021) examined the higher-order structure of OCD-related disorders in a large transdiagnostic psychiatric sample. The data supported a two-factor model where OCD, body dysmorphia, and hoarding clustered under a general 'obsessions-compulsions' factor, while hair-pulling and skin-picking formed a distinct 'body-focused repetitive behaviour' factor. A follow-up study in a separate patient sample further reiterated this distinction (Snorrason et

al., 2022). Here they identified three distinct factors: distress, compulsivity which loadings from OCD, body dysmorphia and hoarding, and grooming behaviours that included hair-pulling and skin-picking. Additional indirect evidence comes from treatment research. Habit reversal therapy, a first-line behavioural intervention for tics has been adapted for BFRBs (Azrin & Nunn, 1973; Conelea et al., 2017; Conelea et al., 2010). This therapy is designed to increase the patients ability to detect tics and associated premonitory urges and replace them with an alternative response. It is suggested that this alternative response is strengthened through training so that it competes with the tic or BFRB for control, supporting the idea that tics and BFRB may be represented as stimulus-response habits that have developed through excessive stimulus-response control rather than failures in goal-directed control associated with compulsive-like behaviours. To add further complexity, within disorders there has been a consistent sub-classification between motor tics and compulsions in Tourette's, automatic and focused picking/pulling in BFRBs, and lower-order motor acts vs higher-order rituals in autism. Whether these subtypes correspond to distinct neurocognitive mechanisms remains an open question.

Together, these findings raise a number of important questions: do repetitive behaviours associated with compulsivity and those with a stronger motor component represent dissociable traits with distinct cognitive underpinnings? Recent methodologies such as time-based paradigms which are sensitive to measuring stimulus-response response provide a promising avenue to examine this. Specifically, are compulsive behaviours associated with both alterations in goal-directed and stimulus-response learning? Or does variation in stimulus-response learning correspond to a separate behavioural phenotype, one that reflects lower-order motor habits? These questions will require support from large population-based methods capable of detecting what we know now to be small brain-behaviour associations, which traditional habit paradigms have struggled to recruit.

1.5 Current Challenges & New Approaches To Studying Habits

1.5.1 Challenges of Traditional Approaches

Investigating habits has been stifled by methodological flaws associated with traditional approaches, most notably small sample sizes and cross-sectional designs that hinder our ability to derive reliable clinical insights. However, the rise of smartphone and online platforms present an unprecedented opportunity to overcome these barriers, offering scalable, longitudinal data collection that can transform the way we study habits.

1.5.1.1 Low-powered small sample studies

Nearly 20 years after Ioannidis (2005) influential article *Why most published research findings are false* 20 years ago, cognitive neuroscience remains dominated by small, underpowered studies (Bishop, 2019; Button et al., 2013b). In the article, Ioannidis (2005) argued that most published research findings are likely to be false, a claim they compellingly supported through a series of simulations demonstrating that the probability of true findings decreases when sample sizes are low and effect sizes are small. Despite longstanding awareness of these issues, cognitive neuroscience remains heavily reliant on small sample sizes, often ranging from tens to fewer than 100 participants (Marszalek et al., 2011; Tibon et al., 2022). In neuroimaging research for example, the median sample size per group across three decades of studies was a mere 12 (Szucs & Ioannidis, 2020). This increased modestly to 24 in studies conducted in 2018 (Szucs & Ioannidis, 2020). These persistently small sample sizes inherently lead to low statistical power, which is the ability to detect an effect of a given magnitude (Baker et al., 2021). Underpowered studies contribute to several major issues (Button et al., 2013a, 2013b; Ioannidis, 2005; Tibon et al., 2022). First, they have a reduced ability to detect true effects, increasing the likelihood of Type II errors where genuine effects go undetected. Second, they lower the positive predictive value, meaning that statistically significant findings are less likely to be true, thereby also increasing Type I errors or false positives. Third, when underpowered studies do detect an effect, it is often exaggerated. This inflation occurs because small samples require a large effect to reach statistical significance. However, this does not necessarily mean that the true effect is large or even that it exists (Button et al., 2013a). Consequently, replications tend to yield smaller effect estimates, closer to their true magnitude.

Moreover, publication biases exacerbate this issue as statistically significant and novel findings are more likely to be published than null or replications ‘of what we already know’ (Bishop, 2019; Button et al., 2013b; Fanelli, 2012; Francis, 2012; Ioannidis, 2005). The cumulative impact of small sample sizes and publication bias has contributed to the ongoing replication crisis. This is where many originally reported efforts fail to replicate (Barch & Yarkoni, 2013; Ioannidis, 2005; Pashler & Harris, 2012; Tibon et al., 2022), with lack of statistical power due to insufficient samples identified as a leading cause (Button et al., 2013b; Turner et al., 2018). An investigation by the Open Science Collaboration (2015) attempted to replicate 100 experimental and correlational studies using high-powered designs. They found the mean effect size of the replication was only half of that reported in the original articles. Further, while 97% of the original studies reported significant results, only 36% of the replications did, with 47% of the original effect sizes falling within the 95% confidence interval of the replication effect size.

In cognitive neuroscience, brain-behaviour associations are now widely accepted to have small effect sizes. A clear related example of this trend comes from genome-wide association studies, where data from tens of thousands of participants have revealed that the magnitude of genetic effects on behaviour is much smaller than previously once thought (Ioannidis et al., 2003). Many genes initially identified in small N studies failed to replicate in these well-powered samples, ultimately refining the research space. Small effects are nonetheless powerful when tackling from a public health perspective (Edenberg, 2021; Thase, 2002) and so remain important to detect. The improvements called for by Ioannidis (2005) over two decades therefore still apply; a shift towards large well-powered samples to aid the reliable detection of what we now accept to be small brain-behaviour based effects.

1.5.1.2 Cross-sectional designs

Most studies to date have relied on cross-sectional design, where variations in task performance are compared across groups (e.g., clinical vs control), or linked to individual differences in a group (e.g., age, clinical severity). However, cross-sectional studies capture associations at a single moment in time, limiting their ability to determine whether the observed effect reflects a trait-like characteristic or temporary state-dependent fluctuation (Karvelis et al., 2023). For instance,

performance on cognitive tasks, or indeed self-report measures, can be influenced by state-dependent factors such as mood, sleep, caffeine intake, or attentional state, which cannot be disentangled from stable trait-like differences.

Cross-sectional studies fail to capture the temporal dynamics in cognition, mood and motivational processes, limiting their ability to disentangle within person fluctuations from between-group differences. For example, one study found that risk-taking behaviour was linked to the time of day, with more risk-taking behaviour observed at later times. However, as the study was cross-sectional, it could not determine whether this pattern reflected true within-person effects, where individuals take more risks later in the day, or between-group differences in the individuals who completed the task during the day versus night. These effects were only confirmed by moving beyond testing at one time point and repeatedly testing the same individual over time (Bedder et al., 2023).

Cross-sectional studies fail to account for causal explanations of the links between cognition and mental health. That is, if alterations in cognitive function contribute to, or result from, psychiatric conditions. A large body of work has identified cross-sectionally, that deficits in model-based planning are associated with greater compulsive tendencies. But this fails to indicate whether these deficits are a temporary impairment tied to their current state confirming a state-distress hypothesis, or whether they reflect stable trait-like vulnerabilities that predispose to future illness. Longitudinal designs can help clarify this distinction. If model-based impairments are state-like, they should be remediated alongside symptom improvement such as after treatment. However, preliminary evidence suggests that this may not be the case. Wheaton et al. (2019) examined model-based planning in a small group of OCD patients ($N = 18$) before and after exposure-response prevention therapy. While the OCD patients exhibited significant clinical improvement, this was not accompanied by changes in model-based which remained unchanged pre- to post-treatment. This suggests that these deficits may be more trait-like and confer long-term vulnerability. However, the lack of control comparison and small sample size necessitate replication to confirm this hypothesis.

Additionally, cross-sectional studies fail to assess the stability of cognitive effects over time (Karvelis et al., 2023). Karvelis et al. (2023) highlighted an important distinction between the reliability and stability of a measurement, emphasising their different time scales. Reliability refers to a task's ability to produce consistent results over two or more administration and is measured on shorter time scales, while stability examines its persistence over longer periods, e.g., six months or more. Although reliability is important, it does not necessarily imply stability. The goal of assessing stability is not to identify the most stable traits, but instead to determine which trait-like variations track mental health status or serve as risk markers for future disorders (Karvelis et al., 2023). While there are some, though few, available studies showing evidence for the reliability of model-based estimates (Brown et al., 2020; Shahar et al., 2019), these assessments have typically been measured weeks to months apart and have not directly examined the relationship with compulsivity. As a result, the long-term stability of this effect over time remains unknown.

And so, if cognitive tasks are to have clinical utility, they must demonstrate longitudinal validity (Yip et al., 2023). This requires moving beyond cross-sectional designs to studying within-person effects, tracking how cognitive measures and psychiatric symptoms fluctuate and interact over time, whether naturally or in response to interventions such as treatment. Without such data, the potential for cognitive tasks to serve as reliable diagnostic tools or predictors of clinical outcomes remains uncertain, thus limiting their translational value in psychiatric research.

1.5.2 Online & Smartphone Science Solutions

In the last decade, research has undergone a digital transformation, with novel methodologies shifting studies from traditional lab based settings into participants homes through online platforms and smartphone technology (Brown et al., 2014; Gillan & Daw, 2016; Gillan & Rutledge, 2021; Harari et al., 2016; Torous et al., 2021). This shift has led to the digitalisation of traditional paper-based surveys and cognitive tasks, enabling them to be delivered entirely remotely through web-browsers, online crowd-sourcing platforms and more recently through smartphone applications. These approaches aim to address the limitations of traditional methods by increasing sample size tenfold and enabling longitudinal designs with ease, while

also improving participant engagement and in some cases the psychometric properties of cognitive tasks.

1.5.2.1 Crowd-sourced platforms

Crowd-sourcing was one of the earliest methods enabling large-scale data collection, paving the way for an online revolution to cognitive and mental health research. It is anticipated that almost all future studies will incorporate crowd-sourced participants (Gillan & Daw, 2016; Stewart et al., 2017). Crowd-sourcing involves the mass distribution of research tasks to ‘workers’, individuals who complete tasks in exchange for monetary compensation (Stewart et al., 2017). Platforms such as Amazon Mechanical Turk (MTurk), CloudResarch, and Prolific act as a host for the direct connection between researchers and an extensive and diverse pool of participants (Johnson et al., 2022; Stewart et al., 2017). This approach addresses challenges associated with traditional in-lab studies by enabling rapid data collection from large, well-powered samples, thereby improving statistical power and facilitating testing for reproducibility and external validation. For example, Bonstrup et al. (2020) utilised crowd-sourcing to replicate in-lab investigations of the effects of consolidation on motor learning, dramatically increasing the sample size from 27 to 389, thereby strengthening the confidence in these effects. Additionally, tasks can be distributed either generally or to specific demographics groups such as transgender populations, improving generalisability but also improving research on under-studied populations who are typically marginalised in conventional research. In particular, this mode of research has had a significant impact on psychiatric research, with crowd-sourced populations demonstrating a range of psychiatric symptoms and severities (Shapiro et al., 2013). Gillan et al. (2016) used a crowd-sourced sample from MTurk in their investigation of deficits in goal-directed control and various psychiatric symptoms. Collecting data from over 1400 participants, they replicated in-house findings that linked deficits in model-based planning with individual clinical measures of eating disorder, OCD, and alcohol dependency. Moreover, the large-scale collection of clinical data allowed them to generate new insights, revealing that individual questionnaire items could be reduced to three dimensional constructs that demonstrated greater specificity in the relationship with model-based planning. Since then, numerous studies have leveraged crowdsourcing for this ‘replication-

and-exploration' based research, contributing to significant strides forward in our understanding of mental health conditions (Patzelt et al., 2019; Rouault et al., 2018; Seow et al., 2021; Sookud et al., 2024; Wise & Dolan, 2020). These platforms also support longitudinal research, offering the ability to re-survey or re-test the same participants over multiple time points (Johnson et al., 2022). For instance, Sookud et al. (2024) re-assessed model-based planning in a third of their sample ($n = 344/600$) 12 months after their baseline assessment, demonstrating the power of crowdsourcing approaches for addressing questions related to the stability of effects with robust within-person design.

However, concerns about data quality have surmounted in recent years (Chmielewski & Kucker, 2019; Hauser et al., 2015; Kennedy et al., 2020). Given that there is a financial incentive on such platforms, participants can often aim to finish the study as quickly as possible so they can move onto the next paid-assessment. This can be accomplished through not complying with instruction or hastily completing surveys, compromising the data quality and integrity. Inattentive and careless responding can significantly impact correlations between task performance and mental health symptomatology, leading to inflated or even spurious associations (Zorowitz et al., 2023). Other studies have reported intentional misrepresentation, wherein participants falsely endorse certain characteristics to gain entry into a study (Chandler & Paolacci, 2017; Hydock, 2018). For example, Agle et al. (2021) found significantly higher rates of self-reported alcohol, anxiety and depression symptoms in a study arm with no quality control measures applied. When stricter quality checks were implemented, mean estimates of clinical measures substantially reduced. Further concerns include the possibility that some responses might not even be human. Studies have reported instances of automated scripts and bots mimicking human responses (Buchanan & Scofield, 2018; Stokel-Walker, 2018). Notably, these concerns have predominantly been associated with MTurk, with no equivalent reports for other crowd-sourced platforms such as Prolific. Comparing data quality across crowd-sourcing platforms, Eyal et al. (2021) found Prolific outperformed MTurk and CloudResearch across all quality metrics, including comprehension of instructions, honesty, appropriate response times, and attention checks.

1.5.2.2 Citizen science & gamification

Over four billion people own a smartphone, accounting for more than half of the world's population (PrioriData, 2025). Additionally, it is also estimated that approximately 3 billion people play games (PrioriData, 2025). Appealing to all ages and cross-culturally, researchers have begun to harness the power of games as a tool for data collection, to study brain processes and solve complex mental health problems (Allen et al., 2024; Gillan & Rutledge, 2021). Gamification refers to the process of applying game-design elements such as scoring systems, narration, and graphics to standardised cognitive tests (Allen et al., 2024; Kawachi, 2017). One of the earliest examples, 'The Great Brain Experiment' (www.thegreatbrainexperiment.com), allowed participants to play four standardised cognitive tasks measuring perception, action inhibition, decision-making and short-term memory in the form of brief games (Brown et al., 2014). Within its first month online, The Great Brain Experiment collected data from over 20,000 users, and to date, has amassed data from over 50,000 individuals worldwide, which increase daily. Another notable example, Sea Hero Quest, is a smartphone navigational game in which players steer a boat to locations they must recall from a map. This app has collected data from over half-a-million users, increasing sample sizes astronomically. Since, many more cognitive tasks have been gamified and deployed through smartphones including the Neureka app (<https://www.neureka.ie>), the BrainExplorer app (<https://brainexplorer.net>), and the TrackYourHappiness app (<https://go.trackyourhappiness.org>). Collectively, these tools aim to enhance our understanding of mental health conditions through large well-powered designs. To do this, smartphones capitalise on 'citizen science' initiatives, in which members of the general public voluntarily engage in scientific research to advance a motivating cause. Given their genuine interest and intrinsic motivation, citizen scientist cohorts can provide more reliable data via research apps and online platforms. This reduces the risk of disingenuous participation associated with financial incentives, thereby enhancing the overall quality and authenticity of the data collected.

Smartphone apps can integrate the collection of gamified cognitive tasks and clinical data through the digitalisation of self-report clinical scales and the development of ecological momentary assessments. Self-report clinical questionnaires are being favoured over standard clinical interviews, such as the

Structured Clinical Interview for DSM Disorders, particularly when diagnostic category is not of pertinent interest. Online clinical data collection in this way offers several advantages. First, self-report measures can be administered faster and more efficiently than clinical interviews which can take up to an hour per participant. Second, they demonstrate high internal consistency (Fox, McDonogh, et al., 2024) and test-retest reliability. For example, Shapiro et al. (2013) found depression severity obtained through the Becks Depression Inventory, measured one week apart, was highly correlated ($r = .97$) in a fully remote online sample, addressing concerns related to reliability. Moreover, self-report assessment mitigate issues related to poor inter-rater reliability of clinical interviews. Finally, self-report measures may track differences in cognition better than clinical diagnosis. As mentioned in a previous section, Gillan et al. (2020) found that a self-report measure of compulsivity and intrusive thought better explained variation in model-based planning than a formal OCD diagnosis.

Given that at-home testing environments are inherently less controlled than laboratory settings, concerns related to reduced sensitivity and reliability from increased noise have been raised. However, it has been suggested that increased noise may be mitigated through these well-powered samples. Using data from 16,000 smartphone users of The Great Brain Experiment, Brown et al. (2014) replicated canonical effects across four domains of cognition with comparable effect sizes to those observed in traditional laboratory settings. Similarly, Sea Hero Quest replicated well-established age and gender differences in spatial navigation (Coutrot et al., 2018). Other smartphone applications have demonstrated similar successes, replication in-house findings with similar magnitudes of effect, reinforcing the validity of remote smartphone assessment (Fox, McDonogh, et al., 2024; Rosická et al., 2024; Rutledge et al., 2014). Smartphone-based assessments also have demonstrated clinical sensitivity. Using data from gamified perceptual decision-making task to measure metacognitive biases in a large citizen science sample ($N = 3410$), Fox et al. (2024) replicated dissociable effects of under- and overconfidence with mental health dimensions of anxious-depression and compulsivity and intrusive thought respectively. Similarly, Skvortsova and Hauser (2022b) used a gamified task and applied computational modelling to identify

distinct computational markers of compulsivity and impulsivity, further demonstrating the potential of smartphone-based measures in psychiatric research.

A critical advantage of smartphone-based assessments is their ability to facilitate repeated within-person research. Unlike conventional designs, which typically involve single-session experiments, smartphone apps rely on short, brief games that participants reengage with. This increases the amount of data collected within-person and, importantly, over-time, offsetting concerns raised with reduced trial numbers (Allen et al., 2024). The accessibility of smartphone-based testing mitigates logistical barriers associated with longitudinal design in traditional research spaces which are resource-intensive and time-consuming for both the researcher and the participant. Smartphones reduce participation burden, allowing participants to engage at a time and location that suits them. This extension of within-person sampling over time opens the door to addressing many research gaps that cannot be answered through single-shot experiments. As mentioned in the limitations of traditional design, longitudinal designs can help disentangle state-versus-trait effects, separating transient fluctuations in cognition from stable trait-like differences. Repeated testing also allows for the study of practice effects. Practice effects are suggested to systematically bias estimates due to increased familiarity with the task of re-engagement. The extent to which practice effects influence brain-behaviour associations is largely unknown and requires investigation if we want to move beyond cross-sectional research accurately. Finally, frequent sampling of cognition combined with self-report or EMA measures allows for the exploration of the temporal dynamics between mood and cognition (Riley & Holden, 2012). For example, Hewitt et al. (2024) found both subjective-state and trait-like apathy effects on a gamified decision-making task delivered through the Brain Explorer app (www.brainexplorer.net).

Beyond scalability and longitudinal testing, online methods provide a platform for optimising the psychometric properties of our cognitive tasks (Gillan & Rutledge, 2021). In comparison to static laboratory-based tasks, app-based tasks offer design flexibility that can be used for A|B testing. This is an approach where variations of a task are compared to identify the most sensitive features (Daniel-Watanabe et al., 2022). Such features could be used to improve psychometric properties like

reliability or to improve signal detection in individual differences research. Online and smartphone approaches are poised to revolutionise research, enabling large-scale investigations and replications, facilitating longitudinal research, and improving psychometric rigor, and ultimately transforming how we do research.

1.6 Experiments and Hypotheses

Chapter 2: Using smartphones to optimise and scale-up the assessment of model-based planning

The first experimental chapter aimed to evaluate the validity and psychometric properties of a gamified smartphone task to measure model-based planning. We first assessed validity in a cross-task counterbalanced study by establishing canonical patterns of model-based planning, correlating model-based estimates between the traditional computer-based and gamified task, and evaluating split-half reliability. Next, we evaluated validity patterns of model-based planning and the split-half reliability, in an uncontrolled citizen science sample. We then examined test-retest reliability in a subset of participants with repeated plays. To assess external validity, we tested whether model-based estimates correlated with previously established correlates of age, gender, education and specific associations with compulsivity and intrusive thought. Finally, we assessed if various task manipulations and increasing trial numbers influenced the external validity and split-half reliability estimates.

Chapter 3: Model-based planning is unaffected by ketamine, antidepressant and internet-delivered cognitive behavioural therapy treatments

The second experimental chapter aimed to determine whether model-based planning is associated with various levels of depression severity and whether model-based planning changes following a range of treatments for depression including ketamine infusion therapy, antidepressant medication and internet-based cognitive behavioural therapy (iCBT). Across two parallel studies, we measured model-based planning at two time points: pre- and post-treatment. In all groups, we tested for clinical changes and model-based improvement following treatment. Then, we examined if changes in clinical improvement were related to changes in model-based planning. In the larger iCBT group, we investigated individual

differences in model-based planning. Finally, in the antidepressant and iCBT groups, we explored if changes in a transdiagnostic dimension of compulsivity and intrusive thought were related to changes in model-based planning over treatment.

Chapter 4: Compulsivity is associated with accelerate stimulus-response habit-learning

The third experimental chapter, a pre-registered study, aimed to develop and validate a smartphone task to measure stimulus-response habit learning and use it to examine individual differences in stimulus-response habit formation. We assessed validity against our pre-registered criteria that reaction times and errors would decrease with training and that more habitual errors would be made following extended compared to minimal and that this difference would be greatest during short preparation trials. We predicted that an index of habit-learning would be associated with greater levels of tics, hair-pulling, skin-picking, and components of autistic-like traits. Using data from a separate task measuring model-based planning, we hypothesised a negative association between model-based planning and compulsivity and intrusive thought. Finally, we hypothesised that enhanced habit-learning would have a stronger association with a transdiagnostic ‘motoric’ factor of mental health than with the original questionnaire’s total scores.

Chapter 2: Using smartphones to optimise and scale-up the assessment of model-based planning

2.1 Introduction

Model-based or goal-directed planning is a cognitive capacity that involves building a mental map of potential action-outcome links and using that to make considered, flexible and optimal decisions (Daw et al., 2011; Doll et al., 2015). A consistent finding in the literature suggests that compulsive behaviours, as seen in obsessive-compulsive disorder (OCD), addiction and aspects of eating disorders, are associated with impairments in model-based planning. This has been shown in online general population samples where individuals vary on a spectrum of compulsivity (Gillan, Kosinski, et al., 2016), in clinical cohorts (Gillan et al., 2020; Voon et al., 2015), and is suggested to have a developmental origin (Vaghi et al., 2020). Mechanistically, theories suggest these deficits arise due to a failure to create accurate internal models of the world (Seow et al., 2021; Sharp et al., 2023), which leaves patients vulnerable to getting stuck performing habits (Gillan, Otto, et al., 2015). Although the finding is consistent, like in many studies assessing the relationship between cognition and mental health symptoms, the effect size is small. To progress our understanding of if and how model-based planning causally relates to compulsivity and develop real-world clinical or public health applications, we need to rethink how we measure it, in whom, and in what setting. One option is to consider population approaches - studying small behavioural effects such as this in larger samples, in real world settings, and where possible, repeatedly through time. Smartphone-science is a promising way to achieve this, though there are concerns that a departure from the experimental control of a lab environment, coupled with changes to core design features of cognitive tasks may come at the cost of validity, reliability, and data quality. Indeed the latter has been a source of considerable debate in the cognitive neuroscience literature.

In recent years, several studies have raised issues with how alterations to key parameters of a task commonly used to assess model-based planning, the two-step task, can affect its measurement. One of the earliest studies in this area illustrated that model-based planning is reduced when there are concurrent working memory

demands, and that this reduction depends on individual differences in working memory capacity (Otto, Gershman, et al., 2013). Kool and colleagues (2016) gathered data on two versions of a two-step task; the original version developed by Daw et al., (2011) and a modified version, which their simulations suggested would increase the incentive value of engaging in model-based planning. They found that the modified version (which included several changes to key task parameters) indeed elicited greater model-based planning compared to the original. Others have shown through simulation that changes to reward probabilities may undermine the validity of standard analyses of the task (Feher da Silva & Hare, 2018). For example, in cases where reward probabilities are unequal (i.e., one second stage state is more rewarding than another), simulated model-free agents can produce behaviour that appears model-based. In another study, they found model-based estimates were significantly greater when participants received in-depth instruction and more practice trials compared to the original task (Feher da Silva & Hare, 2020). More recently, researchers assessed the emergence of model-based planning in a task that was initially absent of any instruction whatsoever (Castro-Rodrigues et al., 2022). They found that only a minority of participants adopted a model-based approach to solving a two-step task without instruction, and once instructions were provided, model-based planning estimates rose rapidly. Across all of these studies, an important facet remains untested – do these task variations shift model-based planning scores equivalently across individuals, or do alterations to task design fundamentally change the meaning of the quantity under study, i.e., its external validity. Recent work suggests mixed evidence. Assessing differences in task motivations, Patzelt et al., (2019) found that offering larger reward amounts increased mean-level model-based planning levels, but this did not affect its association with compulsivity. Castro-Rodrigues (2022), on the other hand, found evidence to suggest that differences between OCD patients and controls may be smaller when detailed instructions are provided, though the sample ($N = 46$ OCD patients) was perhaps too small to test this definitively.

A second and important issue is how task modifications affect reliability, which sets a ceiling for the size of the association one can observe with compulsivity. Test-retest estimates of model-based planning from the traditional task have been mixed, ranging from poor to good ($r = .14-.40$) (Shahar et al., 2019), or non-existent to

excellent ($r = -.10-.91$, median = .45) (Brown et al., 2020), depending on analytic choices. This finding is common to many tasks used for individual difference research (Enkavi et al., 2019) and is thought to in part be the result of the reliability paradox, where tasks designed to examine within-subject effects (such as Flanker, Stroop) have low between-subject variability (Hedge et al., 2018). One simple way to increase reliability is to increase the amount of data (i.e., trial number) gathered per-participant (Zorowitz & Niv, 2023). While this helps, reliabilities eventually plateau, often below an acceptable level. For example, Stroop reaction time can become more reliable with additional trials to a point but intraclass correlation coefficient (ICC) values plateau around 0.4 (Hedge et al., 2018). Similarly, Price et al., (2015a) found relatively consistent ICC values for an attentional bias metric measured from just 48 trials compared to an estimation from 320 trials suggesting that the benefit of adding trials may be tenuous. Further, it is unclear if and how reinforcement learning tasks like the two-step task benefit from additional trials and importantly if improvements in reliability translate into improvements in external validity. For example, early trials might measure something qualitatively different from later trials, particularly in high-order cognitive tests where rules are learned and then deployed, allowing more automatic forms of behaviour to take over.

The present study aimed to address these issues by gamifying and then optimising a commonly-used task that tracks individual differences in compulsivity, testing if key features of task design and trial number could boost its reliability and external validity. This requires large samples, and so we developed a diamond-shooting game called Cannon Blast that could be played by members of the public, aka citizen scientists (Roskams & Popović, 2016), from anywhere in the world in an at-home environment. Cannon Blast was designed to be fun and repeatable, but critically it contained key features of the classic two-step task allowing us to assess model-based planning. We aimed to validate the game in two ways: first by establishing that it elicits model-based behaviour similar to the traditional task and then by demonstrating that model-based estimates correlate across tasks. Next, we released the game to the general public through our lab's non-profit Neureka app (<http://www.neureka.ie>), and by leveraging large-scale data collection, aimed to test if the estimates of model-based planning derived from the gamified task would show the same associations with demographic individual difference measures such as

older age (Eppinger et al., 2013; Nassar et al., 2016), female gender (Gillan, Kosinski, et al., 2016) and lower IQ and processing speed (Gillan, Kosinski, et al., 2016; Schad et al., 2014) but also specific negative associations with compulsivity (Gillan et al., 2020; Gillan, Kosinski, et al., 2016; Seow & Gillan, 2020). Finally, we wanted to utilize these associations as ‘ground truth’ to assess if the external validity of model-based planning estimates are affected by modifications to the task set-up. We compared transition probabilities that were more or less deterministic (80:20 vs 70:30), used different sets of drifting reward probabilities, varied concurrent task demands (i.e. the difficulty of the diamond shooting task itself), compared earlier vs later trials of the game and tested the impact of increasing trial numbers.

2.2 Method

2.2.1 Ethical Considerations and Data Protection

This research was granted ethical approval by the Research Ethics Committee of the School of Psychology at Trinity College Dublin (Approval number: SPREC072019-01). The Neureka app is a non-profit smartphone application developed and maintained by the Gillan Lab, Trinity College Dublin. For Experiment 1, prospective participants received an information sheet and gave informed consent through the online survey platform Qualtrics. Participant across both experiments were required to also read the information sheet and consent to participation embedded to the registration process for Neureka. This described the wider scientific aims of the Neureka Project, what participation involves, terms of data use, data protection procedures, health risks, withdrawal of data procedures and points of contact. For more detail on the exact contents of this information sheet provided to participants, see Supplementary Note 7.1.4.1. Data collected through Neureka is stored and processed in accordance to EU General Data Protection Regulations.

2.2.2 Experiment 1

Participants

We recruited participants to complete the traditional two-step task in a web-browser and Cannon Blast in the smartphone app Neureka. We targeted a minimum sample size of $N = 50$, which provides 80% power to detect a medium effect with a significance level set at $p < .05$. To allow for data-loss and exclusions, data were

collected from $N = 68$ participants who were 18 years or older and have access to both smartphone and computer devices with an internet connection. Participants were compensated €10 upon completion of both tasks. Post exclusion criteria, $N = 57$ remained for analysis (43 women (66%) and 14 men (34%) aged between 18–46 ($M = 22.95$, $SD = 5.6$)). Gender identification was collected in-app by asking ‘What gender do you most identify with’ and a list of seven options: male (hereafter ‘man’), female (hereafter ‘woman’), transgender male (hereafter ‘non-cisgender’), transgender female (hereafter ‘non-cisgender’), non-binary (hereafter ‘non-cisgender’), not-listed (hereafter ‘non-cisgender’), or prefer not to say.

Procedure

Participants were recruited and tested online. During the sign-up process, they provided electronic consent, along with self-reporting basic demographic (age, gender, education) and eligibility information. They completed the traditional two-step task in a web-browser on a laptop or desktop computer and Cannon Blast on an iOS or android smartphone. The order of tasks was counterbalanced across subjects and the entire study took less than 60 minutes.

Cannon Blast. The goal of Cannon Blast is to hit as many diamonds as possible in 100 shots (Figure 2.1a). On each trial, participants first aimed their cannon and then selected between two containers containing purple and pink balls. The left cannon always contained more purple balls (80%) and the right more pink balls (80%). In contrast to the traditional task, this transition structure did not have to be learned or remembered; it was visibly displayed on-screen i.e., each container possessed eight balls of the corresponding colour and two balls of the alternate colour. After the container was selected, a ball was randomly pulled from this container, and was consistent with the most prominent colour (80%, a common transition) or produced the minority colour (20%, a rare transition) (Figure 2.1b).

On what we define rewarding trials, participants received a good ball that exited from the cannon and could be used to hit the diamond (Figure 2.1c). There was no guarantee that a good ball would actually hit a diamond, this depended on the participant’s aim and timing. Alternatively, participants were unrewarded if the ball disintegrated upon firing, thus reducing the chance of hitting a diamond to zero.

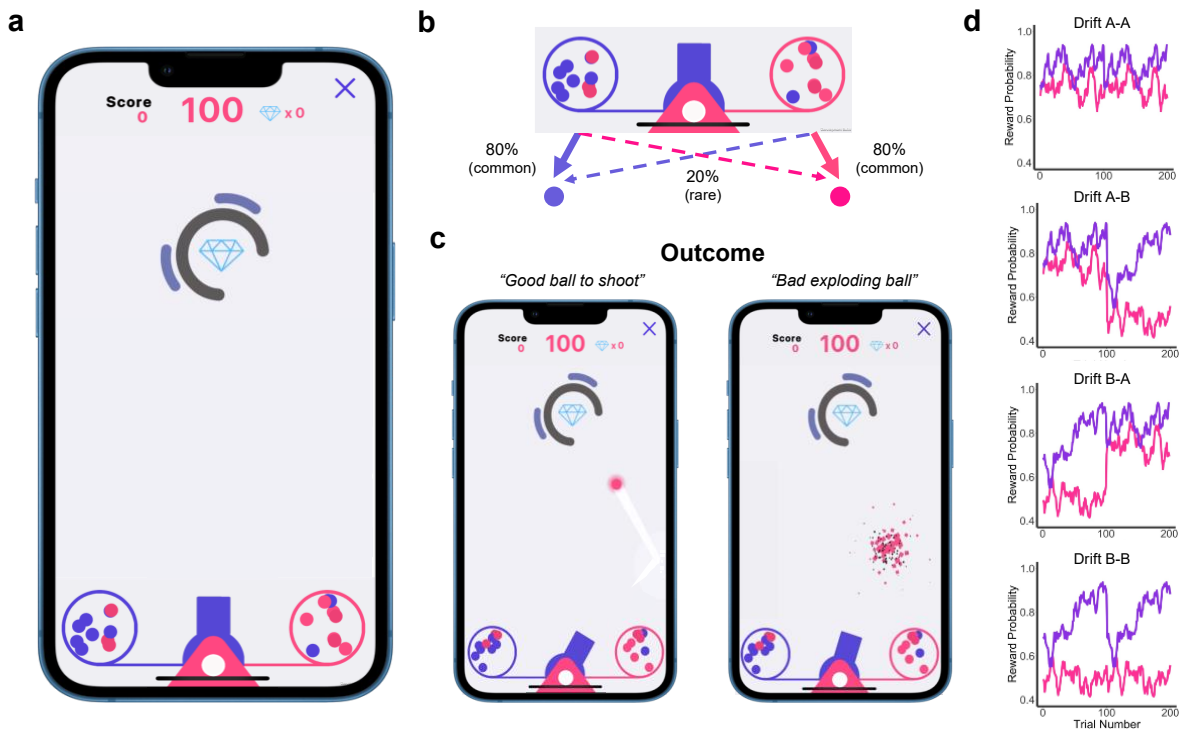


Figure 2.1. Task structure of Cannon Blast, a smartphone game to assess model-based planning. **a.** In this game, participants' goal is to shoot as many diamonds as possible before their total number of shots (100 per block) runs out. To do so, they must aim a central cannon and then select which circular container to draw from. **b.** Purple and pink balls dynamically bounce around each of the flanked containers which depict the probability of a pink or purple ball being released. For example, the left container displays 8 purple balls and releases a purple ball 80% of the time (a common transition) and displays 2 pink balls, giving a pink ball on 20% of trials (a rare transition). **c.** The purple and pink balls have different values that dynamically change throughout the game. The value of the ball is defined as the probability of it being a 'good ball', i.e. one that remains intact after firing (rewarding trial), or a dud ball (non-rewarding trial) that explodes shortly after being fired, and therefore cannot reach the diamond. **d.** We included 2 drifting reward probabilities (A, B) that quantitatively differed on various metrics (see Supplementary Table 7.1.2.6). Participants were randomly assigned a reward drift set at each block leading to four distinct drift set combinations (A-A, A-B, B-A, B-B).

The probability of being rewarded (in other words receiving a good ball) drifted independently over the course of the task, much like the second stage outcomes in the traditional task. However, the traditional task typically utilises a single determined drifting reward probability structure for the 200 trials of the task. To allow us to assess the potential impact of drift dynamics on parameter estimation (in Experiment 2), Cannon Blast instead used two possible drift structures for each block of trials. Participants were randomly assigned drift A or drift B for each of their

100 trial blocks, leading to a total of four reward probability drift sets combinations for Cannon Blast participants (A-A, A-B, B-A, B-B: Figure 2.1d).

The similarities and differences between original two step task and our gamified Cannon Blast are presented in Supplementary Table 7.1.2.1. Like the original, Cannon Blast consisted of 200 trials which was divided into two blocks of 100 trials. The first block was set at an easy difficulty, and the second at a medium difficulty. Level had no direct bearing on the core parameters-of-interest (which container participants select, rewards, drifts etc), and instead reflected how challenging the aim and shoot trajectory was. Though as we explore in Experiment 2, level difficulty can be conceived of as a distraction manipulation. Easy levels included trials where the diamond did not move, had static obstructions that limited the angle at which it could be hit, or where diamonds moved slowly around the screen. Medium difficulty levels included more challenging trials with both moving diamonds and moving obstructions (Supplementary Table 7.1.2.2). While on average medium trials are more difficult than easy (average hit rate medium = 45%, easy = 52%), there was variation within both easy levels (hit rates 83%, 53%, 44%, 29%) and medium (hit rates 75%, 20%, 39%, 45%) (Supplementary Table 7.1.2.2). Reward probabilities in Cannon Blast were set higher on average (mean reward probability [range]: A-A = .80 [.64–.94], A-B = .72 [.41–.94], B-A = .72 [.41–.94], B-B = .64 [.41–.94]) than the original task (mean reward probability [range] = .52 [.25–.75]) to promote enjoyment and limit frustration. In contrast to the traditional two-step task, which includes 40 practice trials, Cannon Blast starts with a short, passive walk-through demonstration of the task (Supplementary Figure 7.1.3.1). While the traditional task design has both first- (choice of rocket) and second-stage (choice of alien) actions, Cannon Blast has first-stage (choice of container) actions only. The decision to remove second-stage actions was in part done for gameplay reasons but also has been shown to increase the importance of model-based contributions in the first stage choice (Kool et al., 2016). A final major distinction between the tasks was the stated goal; in the traditional task, participants are directly told to earn rewards (space treasure). In Cannon Blast, participants are told to shoot as many diamonds as possible, and that this can be facilitated by ensuring they maximise rewards (good balls).

Traditional two-step reinforcement learning task. Participants completed an adapted version of the two-step reinforcement learning task (Daw et al., 2011), developed by Decker et al. (2016). The contents of this task have been described in detail in Decker et al. (2016) and are summarised Supplementary Figure 7.1.3.2 and Supplementary Table 7.1.2.1.

Data Analysis

Exclusion Criteria. Participants were excluded from the traditional task if they: (a) missed more than 20% of trials ($n = 2$) (Seow et al., 2021), (b) responded with the same key press at the first stage of the task on more than 95% of trials ($v = 5$) (Gillan, Kosinski, et al., 2016). Exclusion criteria for Cannon Blast were harmonised with these as much as possible. We excluded participants if they had (a) missed more than 20% of trials ($n = 1$) or (b) selected the same container more than 95% of the time ($n = 4$). However, it is important to note that for criterion (a) as there was no time limit to make this response (unlike the traditional task), participants could not miss trials due to being too slow or disengaged (unless they quit the experiment entirely). Notwithstanding, we noted that some trials were missing for 2 users from our app database (presumably due to a technical glitch) and for one of these, this exceeded the 20% threshold and were therefore excluded. Combining all exclusion criteria for both tasks, $n = 11$ (16%) participants were excluded with $N = 57$ remaining for analysis (37 women, aged between 18–32 ($M = 22.01$, $SD = 4.12$)).

Quantifying Model-Based Planning. All analysis were performed through RStudio version 1.4.1106 (<http://cran.us.r-project.org>). Across both task versions, data distribution was assumed to be normal (Figure 2.2a-b) but this was not formally tested. Hierarchical logistic regression models, which are mixed effects models for a binary outcome variable, were conducted using mixed effects models implemented with the lme4 package in R. The model tested if participants' choice behaviour in the first stage state (coded as switch: 0 and stay: 1, relative to their previous choice) was influenced by reward (coded as unrewarded: -1 and rewarded: 1), transition (coded as rare: -1 and common: 1), and their interaction, on the trial preceding. Within participants factors (main effect of reward, transition and their interaction) were modelled as random effects. Model-based index (MBI) is quantified as the interaction between reward (traditional task: space treasure vs.

dust; Cannon Blast: good vs. dud ball) and transition (traditional task: common vs rare transition to a planet from the chosen rocket; Cannon Blast: common vs rare ball colour appearing from the chosen container). In line with prior work on the traditional task, we also quantified model-free index (MFI: the main effect of reward) and choice repetition (the intercept of the model). Individual estimates for each parameter (MBI, MFI, choice repetition (hereafter 'stay') and transition) were extracted for each task and compared across tasks using Pearson correlation. We assessed the internal consistency of each task using split-half correlation (odds-even split method) using the guidelines from Cicchetti (1994): <.4 poor, .4-.7 fair, .7-.9 good and >.9 excellent.

2.2.3 Experiment 2

Participants

Between June 2020 and October 2022, we collected data from 7466 unpaid Citizen Scientist users of the Neureka app. After applying exclusions detailed below, $N = 5005$ remained for analysis with 3225 (64%) women, 1683 (34%) men, 82 (2%) who did not identify as cisgender (including transgender, non-binary or not listed) and 15 who preferred not to disclose (0.3%) (Supplementary Table 7.1.2.3). The sample included a wide age range of 18–84 ($M = 45.38$, $SD = 14.54$) and 64% had attained post-secondary degree or higher ($n = 3220$). The sample was well-powered for individual difference analysis; a priori power analysis based on a prior paper (Gillan, Kosinski, et al., 2016) indicated that a sample size of $N = 541$ participants was required to achieve 80% power (two-tailed test, $p < .05$) in detecting the association between self-reported compulsivity symptoms and model-based planning measured from the traditional task using an online paid sample, controlling for covariates of age, gender and IQ ($r = .12$).

Procedure

Cannon Blast. Data was collected from citizen scientists who downloaded the Neureka app from the Play Store (Android users) or Apple's App Store (iPhone users), provided informed consent and completed tasks designed to help scientists learn about the brain. Participants completed Cannon Blast within the Neureka app in two ways: as part of a science challenge called Risk Factors designed to measure aspects of cognition and individual-level risk factors, or as a stand-alone challenge

within the Free Play section of the app. Risk Factors included two other games along with a battery of self-report sociodemographic and lifestyle questionnaires which were used to collect information on participants age, gender and education. The order in which games and questionnaires were presented where pseudo-randomised such that a game was always delivered first, followed by alternating blocks of questionnaires and games. The majority of users completed the version of Cannon Blast described in Experiment 1 ($n = 2884$), but $n = 2138$ (gathered from July 2021 onwards) completed a version where the transition structure was set to 70:30, allowing us to examine the impact of transition probability on model-based planning. In Free Play, participants could reengage with Cannon Blast as a stand-alone game. Here, participants selected a difficulty level of their choice and completed 100 trials in that setting. This allowed us to examine the test-retest reliability of model-based planning, assess how collecting more trials per participant affects the reliability and validity of estimates, and disentangle the impact of block difficulty (easy, medium) from order effects (1st block, 2nd block). An additional Hard difficulty level was available in the Free Play section only, with even more challenging diamond movements and obstacles to navigate.

Self-report psychiatric questionnaires and transdiagnostic factors. The Neureka app contains another science challenge called My Mental Health, where participants can complete validated questionnaires assessing nine aspects of mental health (209 items). Of the $N = 5005$ participants for whom we had Cannon Blast data, $n = 1451$ additionally completed this section. The nine questionnaires were used to measure alcohol dependency (Alcohol Use Disorder Identification Test, AUDIT (Saunders et al., 1993)), apathy (Apathy Evaluation Scale, AES (Marin et al., 1991)), depression (Self-rated Depression Scale, SDS (Zung, 1965)), eating disorders (Eating Attitudes Test, EAT-26 (Garner et al., 1982)), impulsivity (Barratt Impulsivity Scale, BIS-11 (Patton et al., 1995)), obsessive compulsive disorder (Obsessive Compulsive Inventory Revised, OCI-R (Foa et al., 2002)), schizotypy (Short Scale for Measuring Schizotypy, SCZ (Mason et al., 2005)), social anxiety (Liebowitz Social Anxiety Scale, LSAS (Liebowitz & Pharmacopsychiatry, 1987)) and trait anxiety (Trait portion of the State-Trait Anxiety Inventory, STAI (Spielberger, 1983)). All questionnaires were presented to participants in a randomized order. Mean and standard deviations of the mental health questionnaire total scores along

with their associations with age, gender and education are presented in Supplementary Table 7.1.2.4. Internal consistency of all questionnaires ranged from good to excellent, Cronbach's $\alpha = .87-.95$. Prior work has shown that these item level responses can be summarised as three transdiagnostic factors Anxious-Depression (AD), Compulsivity and Intrusive Thought (CIT), and Social Withdrawal (SW) (Gillan, Kosinski, et al., 2016). We applied the weights from Gillan et al. (2016) to the Neureka data for all analyses reported here, but additionally repeated the factor analysis to ensure that there were no major differences across the samples. Correlations between the weights derived from Gillan, Kosinski, et al. (2016) and the present study were very high for each dimension, $r = .95-.97$. Of note, there was a typographical error in the response options. Sensitivity analysis presented in Supplementary Analysis 7.1.1.1 suggests this has little bearing on the results, and indeed the correlation across derived factors from the two datasets suggests a high degree of consistency. Other science challenges within the app collected data to derive transdiagnostic scores of compulsivity and anxious-depression using a reduced set of items from those scales. Previous work has validated this reduced set against the original set of items . Using these items, we had task and compulsivity data from $n = 2369$ participants. We used data from $n = 1451$ who had compulsivity score from the full set of items to test for independent clinical associations with MBI along with testing a covariate model with anxious-depression, compulsivity and social withdrawal. We then used data of $n = 2369$ with a compulsivity score from reduced items in the analyses related to associating with game play metrics and also for testing the impact of task-optimizations on external validation.

Data Analysis

Exclusion Criteria. As in Experiment 1, participants were excluded for (a) missing more than 20% trials on their first session ($n = 2394$, most of whom started, but did not complete the game), and (b) selecting the same container on more than 95% of the trials ($n = 797$). A further $n = 48$ were excluded for having incomplete demographic data required for external validation leaving $N = 5005$ remaining for analysis (33% data loss).

Quantifying model-based behaviour. We used the same basic hierarchical logistic regression as in Experiment 1 for all analyses. Data distribution was assumed to be normal (Figure 2.3a) but this was not formally tested. Additional analyses comparing the reliability and external validity of alternative approaches of deriving model-based planning scores as point estimates and using hierarchical Bayesian modelling can be found in the supplementary material (Supplementary Analysis 7.1.1.2). Overall the results were highly similar, but there was an advantage for the hierarchical logistic regression and so this estimation method was brought forward for analyses (Supplementary Table 7.1.2.18). Participants' individual regression co-efficient for the interaction between reward and transition (the model-based index, MBI) were extracted from the hierarchical logistic regression and brought forward for analyses (e.g. to assess association with clinical and individual differences, test the impact of task-modifications and to assess split and test retest reliability).

Examining how task parameters affect model-based estimates. We carried out a series of analyses designed to test if alterations to task parameters affect (i) mean MBI levels, (ii) it's external validity: defined as the associations between MBI and individual difference measures of compulsivity, age, gender and education and (iii) it's internal consistency using split-half correlation and/or test-retest reliability using intraclass coefficients. The structure of these analyses varied across parameter manipulations due to between vs within-subject manipulations and data availability. First, we examined transition structure, which was manipulated between-subject. As fewer participants experienced the 70:30 ($n = 2138$) transition ratio compared to the 80:20 ($n = 2884$), we down-sampled the 80:20 group and propensity-score matched them on age, gender and education using the MatchIt R package (for descriptive information on this matching see Supplementary Table 7.1.2.3). Second, we manipulated task difficulty and order within-subject. We tested for differences in model-based planning estimates during the easy (1st block) vs. medium (2nd block) trials, and complemented this with analysis of Free Play data. As each block consisted of just 100 trials, we used the Spearman-Brown prophecy formula to assess reliability which allows for corrections when trial number is reduced (Shahar et al., 2019; Snijder, 2023): $\text{corrected reliability} = [2 * \text{reliability}] / [1 + \text{reliability}]$. A

subset of participants ($n = 785$) had repeated plays of Cannon Blast, accessed in the Free Play section of the app. These data allow us to disentangle practice/order effects from difficulty. Descriptive information relating to these Free Play sessions are presented in Supplementary Table 7.1.2.5. Third, the classic version of this task includes a single drift sequence that all subjects experience and little is known about the implications of using different sequences. To address this, we randomised participants (i.e. between-subject) to drifting reward probability conditions that differed in several potentially important dimensions, including their distinguishability, average reward rate and the changeability of reward probabilities (Supplementary Table 7.1.2.6). To avoid a wash-out of effects we tested the impact of drifts experienced in the first block of their first play. Also, to keep data as homogenous as possible, we decided to implement in a participant's first play only two out of the possible ten reward drifts used in app. In a between-subjects design, participants were either assigned drift A ($n = 2139$) or drift B ($n = 2345$). In repeated plays, participants were randomly assigned to one of the ten possible drifts (drift A–J, Supplementary Figure 7.1.3.3). Unfortunately at the time of publishing this experimental chapter we did not have the sufficient power to present work using the other eight possible drifts. Finally, we tested the impact of trial number on model-based estimates. To do this, we examined within-subject data from those who played at least 300 trials of Cannon Blast ($n = 716$). Here, we estimated MBI with varying trials collected per participant starting at 25 trials and increasing in bins of 25 trials until 300 trials.

2.2.4 Data Availability

The data sourced from previously publicly available data sets can be found at <https://osf.io/usdgt/> (Gillan, 2016; Gillan, Kosinski, et al., 2016) and at <https://osf.io/mx9kf/> (Seow et al., 2021). The processed data that supports the findings of this study are publicly available at <https://osf.io/arhng/> without restrictions. Due to the sensitive nature of the unprocessed data generated in this study (i.e., responses to mental health questionnaire items) and to comply with data protection regulation, the unprocessed data are not shared publicly. These data can be made available from the authors upon request through the corresponding author.

2.2.5 Code Availability

Code to reproduce the findings and figures are available at <https://osf.io/arhng/>.

2.3 Results

2.3.1 Construct Validity and Reliability of Cannon Blast

Cannon Blast embeds the classic two-step task structure (i.e., drifting rewards, a probabilistic transition structure) within a diamond shooting game. In this game, users aim a cannon at a diamond presented on screen, which might be static, moving around the screen or partially obstructed, depending on the difficulty level. Next, they select which of two containers they want to draw a ball from. The containers each have a mix of purple and pink balls; one has 80% pink balls and the other 80% purple, corresponding directly to the probability that a ball of that colour will be released. Not all balls work; some explode upon being released from the cannon. This is partially predictable from the colour of the ball, whereby the chances that a pink/purple ball will explode drifts slowly and independently over the course of the task. Unlike the traditional task, where rewards are an end unto themselves, in Cannon Blast, rewards (i.e., a good ball to shoot with) have value insofar as they allow the user to shoot the diamond. This means there are two potential forms of reward in this task – getting a good ball and hitting the diamond. For clarity, we define reward in Cannon Blast as the former, but unpack the impact of the latter on choice in our later analyses. To validate the task, 57 paid participants played Cannon Blast and the Traditional version of the task. Both tasks demonstrated a significant main effect of reward (model-free index, hereafter ‘MFI’) and a reward x transition interaction (model-based index, hereafter ‘MBI’) (Table 2.1, Figure 2.2a). In both tasks, participants tended to repeat choices across trials (i.e., a positive intercept), but the tasks differed in the main effect of transition, which was positive and negative in the traditional and gamified tasks respectively. To directly compare behaviour across the two tasks, an analysis of the entire dataset was conducted with Task Type (Cannon Blast, Traditional) as a fixed effect. This revealed a number of differences across tasks. Participants playing Cannon Blast tended to repeat their choices more often, they were more model-based, and less likely to stay following a common transition (Table 2.1). There was a moderate positive association between MBI derived from Cannon Blast and the Traditional

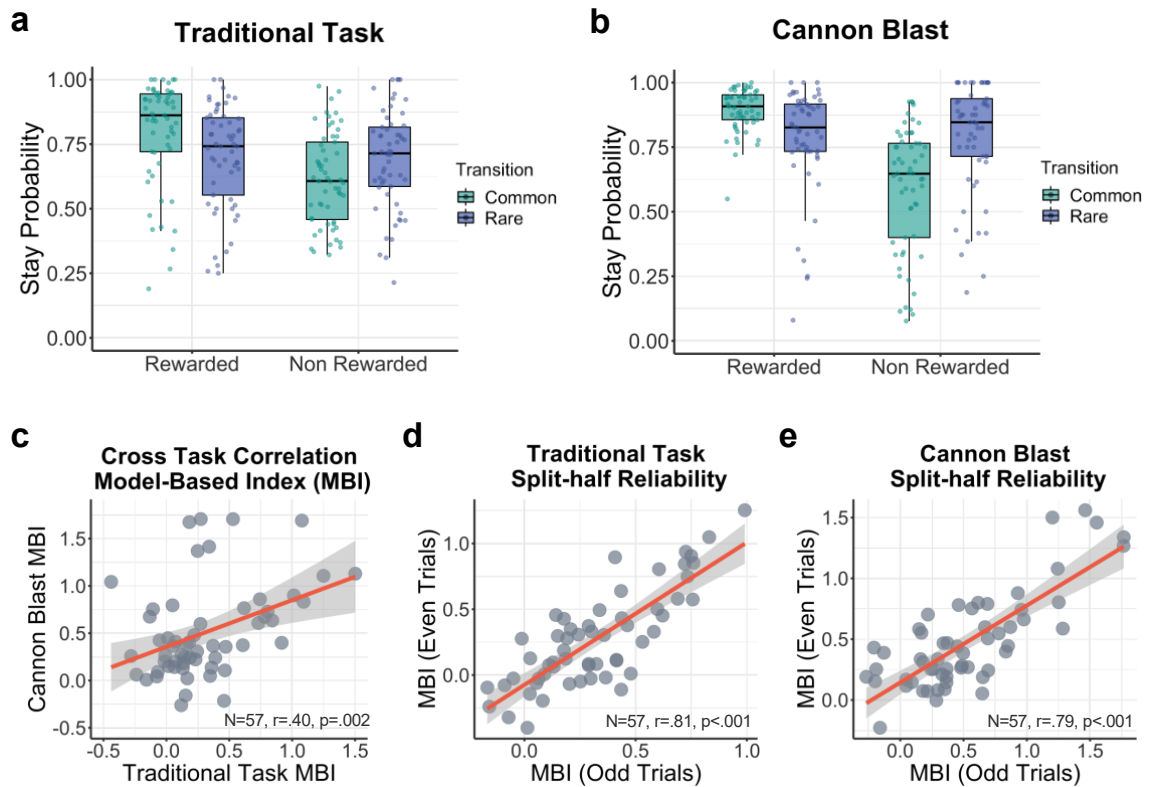


Figure 2.2. Validation of Cannon Blast against the traditional two-step task (n = 57). **a–b.** Box-plot of stay probabilities across all trials and all participants from (a) the traditional task and (b) Cannon Blast. Each dot represents a participant. **c.** Model-based indices (MBI) from the traditional two-step task positively correlated with model-based indices derived from Cannon Blast. **d–e.** Internal consistency of model-based indices in traditional task (d) and Cannon Blast (e) using the split-half odds-even reliability approach. For c–e, each dot represents a participant, the red line indicates line of best fit while grey area represents 95% confidence interval.

task ($r = .40, p = .002$, Figure 2.2b). Split-half reliability for MBI were similar for both the traditional ($r = .81, p < .001$) and Cannon Blast ($r = .78, p < .001$, Figure 2.2c).

Next, we replicated the findings reported above in a larger cohort of unpaid citizen scientists ($N = 5005$) who played Cannon Blast after downloading the smartphone app Neureka. There was evidence of model-free behaviour (main effect of reward: $\beta = .47, SE = .01, p < .001$), model-based behaviour (reward x transition: $\beta = .27, SE = .01, p < .001$), an overall tendency to repeat choices (Intercept: $\beta = 1.33, SE = .01, p < .001$), and participants were more likely to stay following a rare transition (Transition: $\beta = -.08, SE = .00, p < .001$) (Figure 2.3a; Supplementary Table 7.1.2.7). Internal consistency of the MBI was lower than in the smaller paid cohort ($r = .65, p < .001$, Figure 2.3b). An intra-class correlation (ICC) was used to assess the test-

Table 2.1*Mixed effects logistic regression analysis of the Traditional Task and Cannon Blast*

	Est.	Std. Error	z	p
Traditional Task				
Intercept	1.11	.13	8.84	<.001
Reward	.35	.05	7.08	<.001
Transition	.11	.04	2.58	.010
Reward: Transition	.32	.07	4.82	<.001
Cannon Blast				
Intercept	1.51	.11	13.31	<.001
Reward	.48	.07	6.61	<.001
Transition	-.11	.05	-2.19	.030
Reward: Transition	.52	.08	6.13	<.001
Full Comparison Model				
Intercept	1.32	.09	15.31	<.001
Reward	.41	.05	8.86	<.001
Transition	-.01	.04	-.16	.873
Task	-.2	.09	-2.46	.014
Reward: Transition	.42	.06	6.64	<.001
Reward: Task	-.06	.04	-1.48	.142
Transition: Task	.11	.03	3.98	<.001
Reward: Transition: Task	-.10	.04	-2.43	.015

N = 57Model: $\text{stay} \sim \text{reward}_{t-1} * \text{transition}_{t-1} + (\text{reward}_{t-1} * \text{transition}_{t-1} + 1 | \text{id})$

Stay coded as 1 = stay, 0 = switch, Reward coded as 1 = reward, -1 = non-rewarded, Transition coded as 1 = common, -1 = rare, Task coded as 1 = traditional, -1 = Cannon Blast

Est = Beta estimate, Std. Error = Standard error, z = test statistic, p = significance

retest reliability of the MBI from those who had played at least 4 blocks of Cannon Blast (i.e., 400 trials, $n = 423$) within a 30-day timeframe. Specifically, we compared MBI from their first session of Cannon Blast (200 trials, which were completed alongside questionnaires and other games) with their next 200 trials (completed in a section of the app where participants could repeatably play Cannon Blast exclusively). We found test-retest reliability estimates were moderately associated ($\text{ICC}_1: r = .63, p < .001$, Figure 2.3c). Mean time elapsed between first and last session was 6.12 (± 6.93) days, with a median interval of 4 days.

2.3.2 External Validation of Cannon Blast

Using data collected from citizen scientists ($N = 5005$) playing Cannon Blast remotely, we replicated previous lab-based associations between MBI and

individual differences (Figure 2.3d, Supplementary Table 7.1.2.8). Older adults ($\beta = -.02$, $SE = .00$, $p < .001$), women ($\beta = -.02$, $SE = .00$, $p = .001$), and those with less education ($\beta = -.04$, $SE = .00$, $p < .001$) all showed reductions in MBI. $n = 1451$ completed an additional section of the app that contained a comprehensive battery of self-report mental health questionnaires. We observed associations between MBI and eating disorder symptoms ($\beta = -.02$, $SE = .01$, $p = .031$) and impulsivity ($\beta = -.02$, $SE = .01$, $p = .026$), controlling for age, gender, and education (Figure 2.3e, Supplementary Table 7.1.2.8). We refactored the 209 raw questionnaire items into three transdiagnostic dimensions and defined them as; Anxious-Depression (AD), Compulsivity and Intrusive Thought (CIT) and Social Withdrawal (SW) based off previous work (Gillan, Kosinski, et al., 2016). These factors were entered together as IVs in a model predicting MBI with age, gender and education controlled for. Consistent with prior work using the traditional task, we found a specific pattern of a statistically significant association between MBI and CIT ($\beta = -.03$, $SE = .01$, $p = .004$) but no statistically significant evidence for the association between MBI and AD ($\beta = -.00$, $SE = .01$, $p = .733$) or SW ($\beta = .02$, $SE = .01$, $p = .106$) (Figure 2.3e, Supplementary Table 7.1.2.8). For completeness we also assessed the association of these individual difference measures and model-free and stay behaviour. There was no statistically significant evidence for the associations between individual differences or clinical associations with the tendency to repeat choices (Supplementary Table 7.1.2.9). However, individual differences in model-free learning behaved similarly to model-based planning in our task. The MFI was statistically associated with age, gender, education, and compulsivity, in the same direction as MBI (Supplementary Table 7.1.2.9; Supplementary Analysis 7.1.1.4).

2.3.3 Broader patterns of gameplay by citizen scientists

On average participants received rewarding good balls on 145/200 trials (~75%). Consistent with the set-up of the task, the more participants utilised a model-based approach, the more good balls they received ($\beta = 1.76$, $SE = .28$, $p < .001$); there was no statistically significant evidence for individual differences in model-free learning ($\beta = .27$, $SE = .19$, $p = .149$). Those with less education were less likely to receive good balls ($\beta = -.26$, $SE = .09$, $p = .003$) and this trended in the same direction for women ($\beta = -.36$, $SE = .19$, $p = .057$). There was no statistically significant evidence for the association between receiving good balls

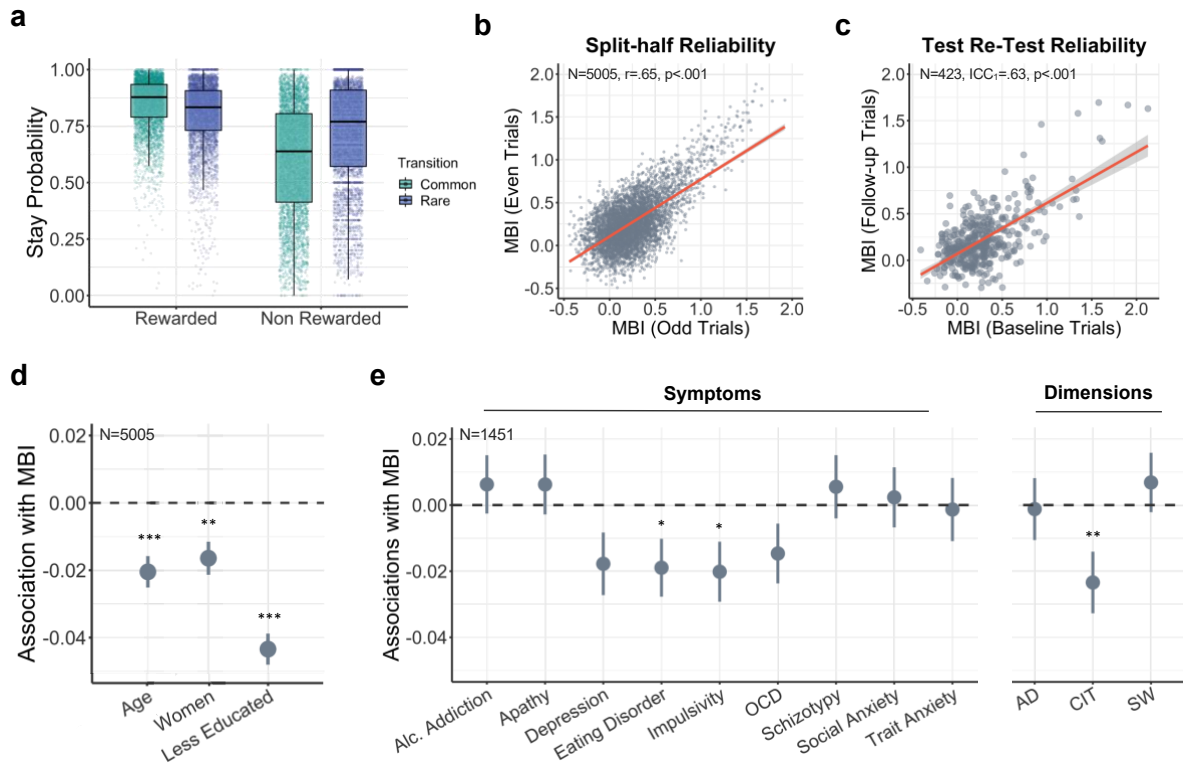


Figure 2.3. Large-scale external validation of Cannon Blast in Citizen Scientists (N=5005). **a.** Box-plot of stay probabilities across all trials and all participants for Cannon Blast (N=5005). Each dot represents a participant. **b.** Internal consistency of model-based index (MBI) using split-half (odds vs evens) method. **c.** Test-retest reliability of MBI from $n = 423$ who had 400 trials of Cannon Blast using intraclass correlation coefficient (ICC). MBI estimates from participants first 200 trials (Baseline Trials) plotted against MBI estimates from participants next 200 trials (Follow-up Trials). For **b–c**, each dot represents a participant, the red line indicates line of best fit while the grey area represents 95% confidence interval. **d.** Model-based associations with individual differences (age, gender, and education) in N=5005 citizen scientists. Reductions in MBI associated with older adults, women and those less educated. **e.** Model-based associations with individual clinical questionnaires and transdiagnostic dimensions in a sub-sample ($n = 1451$) who had completed a battery of self-report clinical questionnaires. Greater levels of eating disorder and impulsivity symptom severity were associated with deficits in MBI. A transdiagnostic dimension of Compulsivity and Intrusive Thought (CIT) showed a specific association relative to Anxious-Depression (AD) and Social Withdrawal (SW). For **d–e**, error bars reflect the standard errors of mean. Both analyses controlled for age, gender, and education. * $p < .05$; ** $p < .01$; $p < .001$ ***

and age ($\beta = -.13$, $SE = .09$, $p = .134$). In $n = 2369$ participants who completed an abbreviated compulsivity scale (20 items versus 209, see Methods (Wise & Dolan, 2020)), we found no statistically significant evidence for an association between number of good balls received and CIT ($\beta = .16$, $SE = .19$, $p = .416$) or AD ($\beta = -.06$, $SE = .20$, $p = .762$). In terms of hitting the diamond, older adults ($\beta = -3.39$, $SE =$

.17, $p < .001$), women ($\beta = -9.48$, $SE = .35$, $p < .001$), and those less educated ($\beta = -.47$, $SE = .16$, $p = .003$) were less likely to hit the diamond (considering only trials with good balls). This was also the case for those with greater self-report CIT ($\beta = -1.29$, $SE = .30$, $p < .001$), while AD was associated with being more likely to hit the diamond ($\beta = .89$, $SE = .29$, $p = .002$), controlling for age, gender, and education. Both model-based ($\beta = 6.57$, $SE = .50$, $p < .001$) and model-free ($\beta = 4.38$, $SE = .33$, $p < .001$) behaviours were positively associated with hitting the diamond, suggestive of a general attention/engagement effect.

The diamond-hitting task was fairly challenging in that on trials where participants received good balls (and therefore a hit was possible), they hit the target on just 36.5% of shots. This would also mean that on ~27% of all trials, users received not only a good ball, but also received the additional reward of hitting a diamond, which increased their score. This additional reward could plausibly impact model-based/free behaviour by amplifying the reward signal on those trials. Another possibility however is that because the diamond moves location after it is successfully hit, this could in fact interrupt learning and so have the opposite effect. We tested this by entering diamond hit into the model with reward and transition on stay behaviour. We found no statistical significance evidence for a main effect of lagged diamond hit on stay behaviour ($\beta = -.05$, $SE = .03$, $p = .053$). Similarly, on trials following a diamond hit, users had reduced model-free behaviour (reward*diamond hit interaction, $\beta = -.14$, $SE = .03$, $p < .001$), and reduced model-based behaviour (reward*transition*diamond hit interaction, $\beta = -.09$, $SE = .03$, $p < .001$) (Supplementary Table 7.1.2.10). Together these findings suggest that the receipt of a diamond functioned to impair learning, not potentiate it.

2.3.4 Effect of task modifications on the external validity and reliability of model-based planning

Transition structure

The initial set of participants completed a version of the task with an 80:20 transition ratio (i.e., probability of purple/pink balls from the respective containers). To test if this adjustment to transition ratio impacted estimates of model-based planning, we adjusted this in-app to 70:30 (as per the classic two-step task) and continued to gather data. We subsampled our total dataset to achieve two age, gender, and

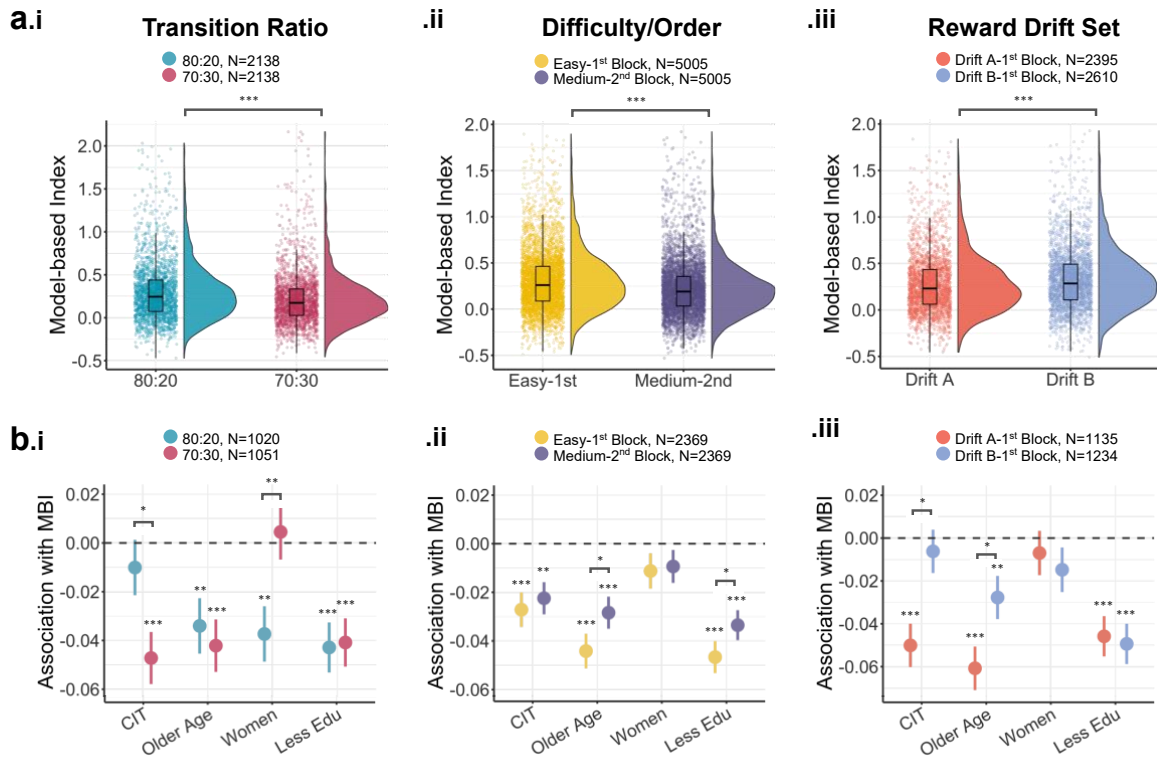


Figure 2.4. Impact of task modifications on model-based estimates and its association with compulsivity and individual differences. **a.** Distribution and mean value of model-based scores across (i) transition ratio, (ii) difficulty/order, and (iii) reward drift set. Mean model-based scores were larger when estimated from an 80:20 transition ratio structure compared to 70:30, in easy/1st play compared to Medium/2nd play and in those who experienced Drift set B compared to Drift set A. **b.** Model-based associations with compulsivity (CIT) and individual differences (age, gender and education ('Less Edu')) across (i) transition ratio, (ii) difficulty/order, and (iii) reward drift set. The association between greater self-report compulsivity and MBI was greater when estimated from 70:30 transition ratio structure and using drift set A. Error bars reflect standard errors of the mean. * $p < .05$; ** $p < .01$; *** $p < .001$

education-matched samples of $n = 2138$ that completed the 80:20 and 70:30 versions, respectively. Participants in both groups had a main effect of reward, transition, and a reward x transition interaction (Supplementary Table 7.1.2.7). The 80:20 transition ratio yielded significantly greater MBI ($M = .30$, $SD = .34$) compared to those who experienced the 70:30 version ($M = .22$, $SD = .31$), $t = 8.40$, $p < .001$ (Table 2.2, Figure 2.4a.i). However, transition ratio structure did not affect its reliability with both versions showing similar split-half reliability (80:20; $r = .67$, $p < .001$, 70:30: $r = .63$, $p < .001$, Table 2.2). From this sample, $n = 2071$ participants (80:20: $n = 1020$, 70:30: $n = 1051$) had compulsivity scores and demographic information available. A significant negative association between MBI and CIT was observed in the 70:30 group ($\beta = -.05$, $SE = .01$, $p < .001$) but there was no

Table 2.2

The impact of different task modifications on mean-level model-based estimates and its reliability

	<i>N</i>	MBI Score <i>M</i> (<i>SD</i>)	Range	Split-half [95%CI] ^a	t/F
Overall Task	5005	.26 (.33)	-.49 - 2.26	.65 [.63 - .67]	n/a
Transition Ratio (Between-subject)^b					8.40***
80:20	2138	.30 (.34)	-.47 - 2.03	.67 [.64 - .69]	
70:30	2138	.22 (.31)	-.45 - 2.16	.63 [.61 - .66]	
Difficulty/Order (Within-subject)					20.16***
Easy/1 st Block	5005	.30 (.32)	-.49 - 1.90	.71 [.71 - .76]	
Medium/2 nd Block	5005	.22 (.29)	-.53 - 1.92	.68 [.66 - .70]	
Reward Drift Set (Between-subject)					-5.33***
Drift A/1 st Block	2395	.28 (.32)	-.46 - 1.84	.74 [.71 - .76]	
Drift B/1 st Block	2610	.33 (.33)	-.51 - 1.93	.69 [.66 - .71]	

^a Data was split using odds-evens method, reporting Pearson R

^b Subsample of total dataset to achieve two age, gender and education matched samples

MBI = Model-based Index, M = Mean, SD = Standard Deviation, CI = Confidence Interval, t/F = test statistic

*** $p < .001$

statistically significant evidence for the same relationship in the 80:20 group ($\beta = -.01$, $SE = .01$, $p = .374$) (Figure 2.4b.i; Supplementary Table 7.1.2.11). This difference was confirmed by entering transition ratio (80:20, 70:30) into the model, where we found a significant CIT x transition ratio interaction ($\beta = -.02$, $SE = .01$, $p = .022$, Supplementary Table 7.1.2.12, Figure 2.4b.ii). In terms of the other individual differences, we found a significant Gender x Transition interaction in the full comparison model ($\beta = .02$, $SE = .01$, $p = .009$, Supplementary Table 7.1.2.12). This was driven by a negative association between MBI and Gender in 80:20 ratio ($\beta = -.04$, $SE = .01$, $p = .001$, Supplementary Table 7.1.2.11) but this was not statistically significant in the 70:30 group ($\beta = .00$, $SE = .01$, $p = .689$, Supplementary Table 7.1.2.11). We found no statistical significant evidence that transition ratio effects the relationship between MBI and age ($\beta = -.00$, $SE = .01$, $p = .658$) or education ($\beta = .00$, $SE = .01$, $p = .874$) (Supplementary Table 7.1.2.12).

Difficulty and Order Effects

Difficulty in Cannon Blast referred to how challenging the diamond shooting task was on a given trial, with harder levels requiring more timing and spatial reasoning

to hit the target than easy trials (where the diamond was unobstructed or static). We hypothesised that this would put a strain on the cognitive resources required for maintaining an accurate model of the task environment and therefore impede model-based planning. As part of their first play of Cannon Blast during the Risk Factors section, all participants completed two blocks of trials; a block of 100 easy difficulty trials (Easy-1st Block), followed by a block of 100 medium difficulty trials (Medium-2nd Block). In each of the two blocks (Easy-1st Block, Medium-2nd Block), we observed main effects of reward, transition, and their interaction (Supplementary Table 7.1.2.7). However, we found MBI estimates were significantly larger in the Easy-1st ($M = .30$, $SD = .32$) than Medium-2nd ($M = .22$, $SD = .29$) blocks, $t = 20.16$, $p < .001$ (Table 2.2, Figure 2.4a.ii). Because difficulty and order are confounded with one another, we cannot confirm if this reduction was driven by increased task difficulty, order, or both. To isolate the effect of difficulty from order, we turned to data from the Free Play section of the app, where a subset of participants re-engaged with Cannon Blast at a chosen difficulty level for short games of 100 trials. We compared their initial play (first play) at each difficulty level (easy, medium) with their next play (second play) of that difficulty. In $n = 689$ participants who had completed two sessions of Cannon Blast at an easy difficulty, we found MBI was greater at their first play ($M = .32$, $SD = .33$) compared to their second ($M = .21$, $SD = .23$), $t = 8.91$, $p < .001$. Importantly, we did not find this difference when we repeated this analysis in $n = 556$ who had two sessions of play at medium difficulty (first play: $M = .24$, $SD = .33$; second play: $M = .25$, $SD = .33$, $t = -.59$, $p = .552$). This suggests that our difficulty manipulation did not affect model-based planning, but that there was an effect of order – MBI estimates are greatest during the first play of Cannon Blast, which reduces and stabilises across subsequent sessions. To confirm this, we compared model-based estimates in $n = 335$ who had sessions at both difficulties (easy, medium) and at two time points (1st play, 2nd play). We found MBI at easy-1st play ($M = .33$, $SD = .35$) was significantly larger than all other times, including medium-1st play ($M = .25$, $SD = .34$, $p = .002$), easy-2nd play ($M = .22$, $SD = .23$, $p < .001$), and medium-2nd play ($M = .25$, $SD = .32$, $p = .002$). More evidence for the uniqueness of the first block in which people play comes from an analysis of test re-test reliability of task sections. MBI estimated from the easy-1st Block of the game was weakly associated with their second sessions at easy ($n = 689$, $ICC_1 = .35$). The same analysis of medium plays yield higher reliability ($n = 556$,

ICC₁ = .51) (Supplementary Table 7.1.2.13). In terms of reliability within each block, we found comparable split-half reliability estimates (easy-1st Block: $r = .72$, $p < .001$, 95%CI = .70–.74; medium-2nd Block $r = .68$, $p < .001$, Table 2.2).

Having established mean-level differences across the blocks of the task, we next tested if the observed relationships with compulsivity, age, gender, and education varied across these blocks (easy-1st Block, medium-2nd Block). In each block, we found deficits in MBI were associated with greater compulsivity, older adults, and those with less education (Figure 2.4b.ii; Supplementary Table 7.1.2.11). In the full comparison model where difficulty/order (easy-1st Block, medium-2nd Block) was entered into the model, we found no statistical significant evidence that difficulty/order impacts the association between MBI and compulsivity ($\beta = .00$, $SE = .00$, $p = .476$). There was a significant interaction between age x difficulty/order ($\beta = .01$, $SE = .00$, $p = .017$) and between education x difficulty/order ($\beta = .01$, $SE = .00$, $p = .032$), such that the association between MBI and age and MBI and education were greater when estimated from the easy-1st Block (Supplementary Table 7.1.2.12).

Reward Drifts

At each block, participants were randomly assigned to one of two drift sets (drift A, drift B). It is important to note that these drifts differed in more than one dimension (Figure 2.1d); but descriptively, drift A included two relatively stable reward probabilities (SD : purple = .053, pink = .049) and relatively high reward rates (purple = .845, pink = .748), with purple balls outperforming pink, though at times there was very little evidence to distinguish the most rewarding ball colour. Drift B, in contrast, had lower overall reward rates (purple = .774, pink = .501). Purple and pink balls started out with a similar level of reinforcement and throughout the 100 trials the value of the purple steadily increased, while pink remained low (close to 0.5). Participants were randomly assigned their drifts independently across blocks, creating four possible reward drift combinations: A-A, A-B, B-A and B-B for their first 200 trials of the game. To avoid washing out effects in the mixed conditions (A-B, B-A), we compared A and B in the 1st Block only (100 trials). Participants were randomly assigned drift A ($n = 2139$) and drift B ($n = 2345$). In both drift sets, we observed main effects of reward, transition, and their interaction (Supplementary

Table 7.1.2.7), but MBI estimates were significantly larger in those who received drift B ($M = .33$, $SD = .33$) compared to drift A ($M = .23$, $SD = .31$), $t = -5.33$, $p < .001$ (Table 2.2, Figure 2.4a.iii).

In a subset of participants who had compulsivity scores and demographic information available (drift A: $n = 1135$, drift B $n = 1234$), we found a negative association between MBI and compulsivity when estimated in those who experienced drift A ($\beta = -.05$, $SE = .01$, $p < .001$, Supplementary Table 7.1.2.11), but no statistically significant evidence for this association when estimated from drift B ($\beta = -.01$, $SE = .01$, $p = .540$). In a full comparison model where drift (drift A, drift B) was entered into the model, this difference was confirmed by a significant CIT x drift interaction ($\beta = .02$, $SE = .01$, $p = .002$, Supplementary Table 7.1.2.12, Figure 2.4b.iii). We also observed a significant age x drift set interaction ($\beta = .02$, $SE = .01$, $p = .024$, Supplementary Table 7.1.2.12), with estimates between age and MBI greater when estimated from those in drift A.

Trial Number

To test the impact of increasing the total number of trials used to estimate MBI per participant, we generated each participant's MBI several times, starting with a participant's first 25 trials and increasing by 25 trials in each iteration, until 300 trials per participant was reached. In all cumulative trial number sets, we observed main effects of reward, transition, and their interaction (Supplementary Table 7.1.2.14). This was done in the sub-sample of participants who had 300 trials of Cannon Blast ($n = 716$). We found there was overall a reduction in MBI as trial number increased ($\beta = -.02$, $SE = .01$, $p = .004$, Figure 2.5a.i, Supplementary Table 7.1.2.15). As expected, increasing the amount of data collected per participant improved internal consistency of MBI estimates; reliability at 25 trials was $r = .41$, $p < .001$ and increased to $r = .71$, $p < .001$ at 300 trials (Figure 2.5b.i; Supplementary Table 7.1.2.16). However, in terms of external validity, there was no statistical evidence that the association between MBI and compulsivity was impacted by additional trials (CIT x trial number: $\beta = -.00$, $SE = .00$, $p = .819$, Figure 2.5c.i, Supplementary Table 7.1.2.17), which was significant when estimated with as few as 25 trials ($\beta = -.04$, $SE = .01$, $p = .002$, Supplementary Table 7.1.2.15). There were significant interactions between age x trial number ($\beta = -.01$, $SE = .00$, $p = .044$), gender x trial

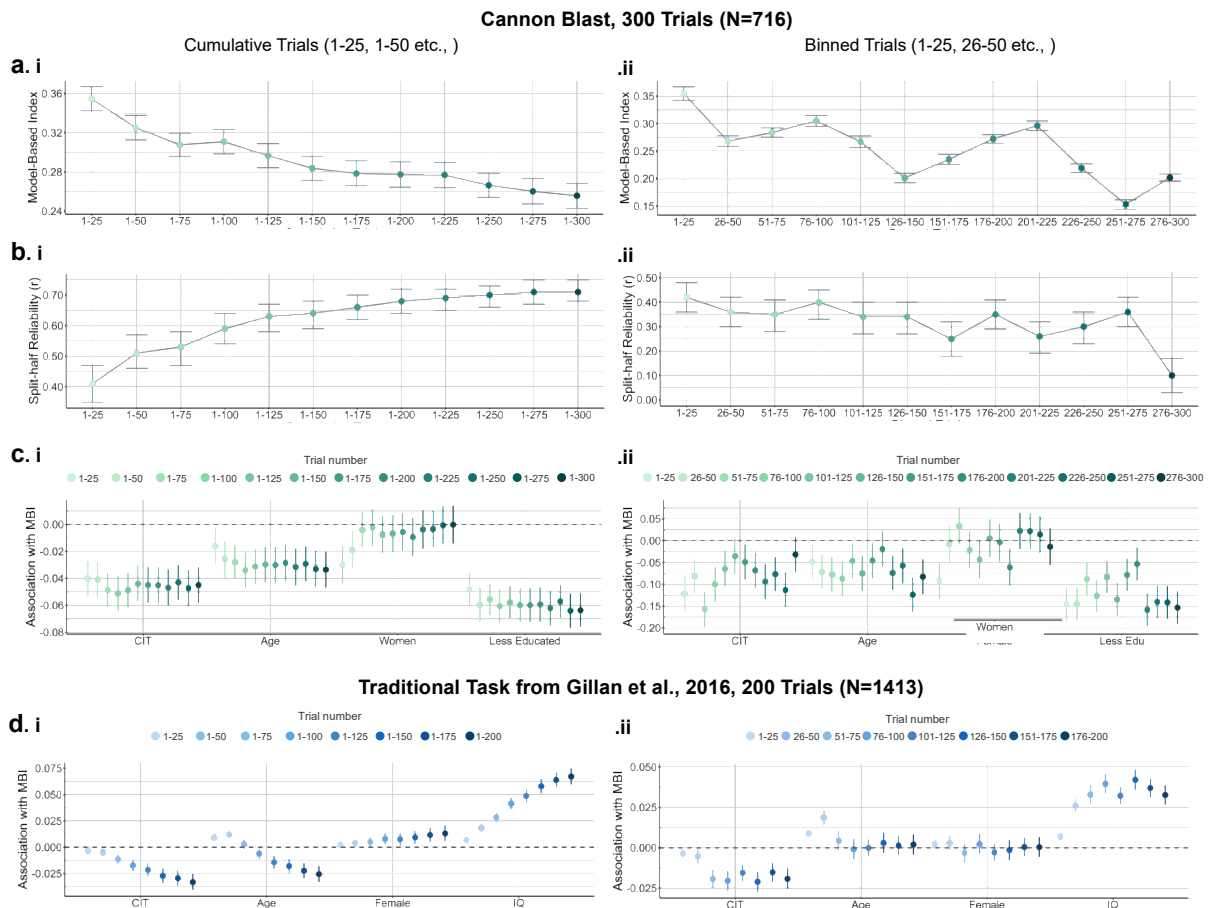


Figure 2.5. The impact of increasing number of trials on mean-level model-based estimates, its reliability and their association with individual differences ($n = 716$). **a.** Mean MBI estimated with (i) cumulative trials i.e., increasing trial numbers sequentially by 25 at a time, until 300 trials per participant was reached, and (ii) binned trials i.e., bins of 25 trials sampled sequentially through the task, in chronological order. Mean-level MBI decreased with more trials. Error bars reflect standard errors of the mean. **b.** Split-half reliability co-efficient as a function of increasing trial number using (i) cumulative trials and (ii) binned trials. Using the cumulative trials, reliability of the MB estimate increased with additional trial collected per participant. Error bars reflect 95% confidence intervals **c.** Model-based associations with compulsivity, age, gender and education using (i) cumulative trials and (ii) binned trials. Increasing the number of trials per participant did not significantly increase the association between model-based planning and individual differences. No statistical significant difference was found between model-based associations with compulsivity when estimated at participants' first 25 vs 300 trials. Model-based associations with age and education became stronger with the addition of trials while the association between model-based and gender reduced. Data from the binned analysis demonstrated that this effect is in part driven by stronger signal in earlier versus later trials. **d.** Repeated analysis using a publicly available dataset of $N = 1413$ individuals who completed the traditional two-step task (200 trials). Here we found (i) the association between model-based planning and individual differences increased as trials collected per participant increased and (ii) this effect is driven by later trials compared to earlier trials. For **c-d**, error bars reflect standard errors of the mean.

number ($\beta = .00$, $SE = .01$, $p < .001$) and education x Trial number ($\beta = .00$, $SE = .01$, $p = .042$) (Supplementary Table 7.1.2.17). The association between both age and education and MBI became stronger with the addition of more trials while the association between gender and MBI reduced.

Next, we wanted to test if any specific collection of trials were driving these effects. To do this, we repeated the above analyses using bins of 25 trials (0-25, 26-50, etc). Similar to cumulative trials, we found an overall reduction in MBI for later trial bins (Figure 2.5a.ii). Reliability estimates were relatively stable across the bins of trials (Figure 2.5b.ii). But associations between MB and CIT reduce as the task progresses ($\beta = .002$, $SE = .00$, $p = .010$, Figure 2.5c.ii). To test if this pattern is specific to our task, we carried out the same analysis on data from the traditional task in a previously published study ($N = 1413$ who completed the traditional two-step task remotely online) (Gillan, Kosinski, et al., 2016). In the traditional task, participants must learn the transition probabilities through trial and error (unlike our task where they are shown on-screen throughout), so it follows that the early trials should not be informative for assessing model-based planning. Indeed this was the case; in the traditional task, associations with CIT increase with more trials when examined cumulatively (Figure 2.5d.i) and associations with CIT were absent from the first 2 bins of trials, and only become significant later (Figure 2.5d.ii).

2.4 Discussion

We developed a smartphone-based diamond-shooting game capable of assessing model-based planning in out-of-the-lab unconstrained settings. We demonstrated that estimates derived from the game are valid by replicating previously established correlates of model-based planning: age (Eppinger et al., 2013), gender (Gillan, Kosinski, et al., 2016), education (similar to previous work on IQ (Gillan, Kosinski, et al., 2016) and processing speed (Schad et al., 2014)) and compulsivity (Gillan et al., 2020; Gillan, Kosinski, et al., 2016; Patzelt et al., 2019; Seow et al., 2021; Voon et al., 2015) in a large sample of citizen scientists and by demonstrating comparable psychometrics to the traditional. We then used this task to tackle a question posed by recent studies (Castro-Rodrigues et al., 2022; Feher da Silva & Hare, 2018, 2020; Kool et al., 2016; Otto, Raio, et al., 2013; Patzelt et al., 2019): are associations between compulsivity and model-based planning dependent on key aspects of the

task's design? In a series of within and between-participant experiments, we found that some task parameters (i.e. transition and reward drifts) are associated with better capture of individual differences. The association between model-based planning and compulsivity was greater when using a less deterministic transition ratio structure (70:30 compared to 80:20) and that this association also depends on the specific nature of the drifting reward probabilities used. We additionally showed that previously established effects could be observed in as few as 25 trials, and that increasing trial numbers had little impact. We did not find the association between model-based planning and compulsivity significantly differed between manipulations made to task difficulty. However, this could be a limitation of our task design, which used a relatively weak dual task manipulation. Specifically, difficulty in this context referred to the extra demands placed on participants to shoot the diamond on harder levels, which required taking into account the geometry of the screen and timing the shot. Perhaps crucially, this demand occurs after the model-based update had occurred and participants have presumably already decided which container to fire from. A more potent dual-task demand would impact the update itself (Otto, Gershman, et al., 2013).

Previous findings suggest that model-based planning problems in compulsivity arise from issues in building accurate mental models of action-outcome contingencies (Seow et al., 2021), which may arise from problems with learning probabilistic action-state transitions (Sharp et al., 2021), and assigning credit to actions (Shahar et al., 2021). Recent work extended this idea, finding that differences between OCD patients and controls are largest when instructions are absent and individual must learn the structure of the task from scratch (Castro-Rodrigues et al., 2022). In our task, the relationship between an action (choosing left or right) and the resulting state (ball colour) was visible on-screen throughout the game as the proportion of balls displayed in each container. Despite this, we found the characteristic reduction in model-based planning in compulsive individuals. This suggests that the mechanisms underlying this relationship reflect more than a failure to learn about the statistical properties of action-state transitions through experience. One possibility is that in compulsivity, broader issues in executive function might cause individuals to simplify tasks to avoid overload of working memory or other finite cognitive resources (Snyder, Kaiser, et al., 2015). This notion is supported by the

finding from the present study that the association between model-based planning and compulsivity is greater when the action-state transitions are more uncertain, i.e. less deterministic (i.e. 70:30), and model-based planning is more effortful to employ (i.e. requires more win-switch and lose-stay actions).

In our task, the drift that had the stronger association between model-based planning and compulsivity had higher overall reward rates, a fairly stable time course and relatively low distinguishability between the two states compared to a drift rate with lower over reward rates and higher distinguishability between states. On a similar note to the above point, it could be that the cost-benefit of engaging in model-based behaviours is not worthwhile in environments where there isn't a noticeable difference in reward magnitudes of outcomes. However, because the drifts used differed on more than one dimension, future research is needed to understand and further optimise the selection of drifts. This finding is nonetheless important, as studies often vary in the drifts employed with no established best-practice. One suggestion is to revisit these analyses using all ten possible drift sets available in the app, once we have collected a sufficient amount of data per drift set. Any candidate drift properties (e.g., boundaries, variance, drift rate) that affect the relationship between model-based planning and compulsivity can later be more systematically confirmed in new drift sequences optimised to maximally compare relevant dimensions.

In many areas of psychology, collecting more data per participant can improve the reliability and therefore the strength of associations with individual difference measures (Elliott et al., 2019). However, it is important to note that the relationship between trial number and reliability is non-linear; reliability improves steeply until it reaches asymptote (Cooper et al., 2017a; Price et al., 2015a). In line with this, we observed the split-half reliability of model-based estimates increased with the addition of trials however began to taper off at around 125 trials. A second issue when considering trial number is that additional trials can change the nature of the measurement, not just its reliability. Depending on the design, behaviour on tasks that involve some element of learning can signal different processes at different stages of the task. That is, the initial trials where a participant explores and becomes familiar with the task can reflect a different cognitive process than later trials when

a participant is updating and executing what they have learned. This is consistent with what we observed in the present study; model-based planning was greatest during participants first block of trials and decreased steadily over time, and test-retest estimates were lower for the 1st play than the 2nd. The learning that takes place in the first trials of the task may mean that those trials carry maximal variation in the process of interest, and so counter-intuitively it could mean that additional trials in fact reduce the magnitude of group differences. This finding is important for the future research studies considering to use tasks with elements of reinforcement learning measured over multiple time points for example pre- and post-treatment. To make these sessions more homogenous, longer intervals between sessions may be required to restore the initial block effect. In the present study, we found that additional trials did not affect the strength of the observed relationship with compulsivity, which was apparent with just 25 trials of data. More trials did however modestly improve the effect sizes for the link between MBI and age and education, but this was mostly driven by gains from 25 to 100 trials. This early signal in the task stands in contrast to the traditional version, where we show that early trials do not carry much individual difference signal. We speculate that this difference is because in the traditional task, participants must learn the transition probabilities through trial and error (and so model-based planning cannot be executed from the 1st trial, whereas in Cannon Blast, the probabilities are displayed on-screen. This simple change in design may have major implications for future studies aiming to reduce the trials required to estimate model-based planning.

We found that model-free estimates were associated with the same individual and clinical associations as model-based planning. This is in line with a prior study that also observed reduced model-free learning in compulsivity (Gillan et al., 2020), but it is important to note that the majority of studies have not observed this (Gillan, Kosinski, et al., 2016; Vaghi et al., 2020; Voon et al., 2015). In fact, conceptually, one might expect model-free behaviour to be enhanced in compulsivity, given the popular framework that describes their trade-off (Daw et al., 2011). This apparent inconsistency follows a growing literature that has raised doubts about what the model-free estimates truly represent. Prior work has shown that model-free learning is not related to the gold-standard index of habit, performance at a devaluation test (Gillan, Otto, et al., 2015) and as we report here, studies consistently show a

positive correlation between model-based and model-free estimates (Gillan, Kosinski, et al., 2016; Seow et al., 2020). Recent work suggests model-free behaviour on this task may be better understood as a different form of model-based choice, that we fail to model accurately (Feher da Silva & Hare, 2020). In line with this, Konovalov and Krajbich (2020) found that model-free participants made more fixations prior to choice, indicating choice deliberation rather than habitual selection. In the present study, we embedded the classic two-step task within a game, that is, we made model-free and model-based behaviour operate in the service of a reward (gaining good balls), which was itself only valuable insofar as it served the higher-order goal of shooting diamonds. This meant that model-free behaviour was no longer the lowest level of engagement with the task and it is therefore possible that those with the lowest cognitive capacity may have focused exclusively on the primary task of shooting the diamonds, adding new model-based signal to the model-free estimate. Indeed, the cross-task correlations in the present study support this idea as model-free measures from Cannon blast were positively associated to both model-free and model-based estimates measured from the traditional task.

There is increasing evidence that true correlations between cognitive test performance and individual differences in mental health are small. This is increasingly recognised to be the case for various levels of analysis in psychiatry, such as neuroimaging (Marek et al., 2022), studies of environmental risk (Baldwin et al., 2021), and genetics (Dunn et al., 2015). Small effects can nonetheless have big impact (Götz et al., 2022), if tackled at a population level, from a public health perspective. But in order to estimate these effects and interpret them accurately, we need larger samples in our research studies. One way to achieve this is by taking our assessments out of the lab and into daily life through gamification and smartphones (Gillan & Rutledge, 2021; Long et al., 2023). Our results add to the growing evidence that smartphones can deliver valid cognitive tests data with clinical implications (Coughlan et al., 2019; Rutledge et al., 2014; Skvortsova & Hauser, 2022a). In this paper, we emphasise another advantage of smartphone science in the cognitive space: it facilitates A/B testing so we can systematically begin to improve the psychometric properties of our tasks and optimise them for specific clinical populations (Enkavi et al., 2019). Another key advantage of this

method is that it leverages citizen scientists, rather than exclusively relying on university students. This results in a more diverse samples and makes research more accessible to those living farther from research centres or unable to attend during working hours. Other forms of remote assessment, such as Amazon's Mechanical Turk (MTurk), connect users to studies for payment. However, recently these platforms have been at the mercy of increasing issues with data quality (Chmielewski & Kucker, 2019; Kennedy et al., 2020). In citizen science, incentives of researcher and participant may be better aligned and this may have a positive impact on data quality (Donegan & Gillan, 2022).

2.4.1 Limitations

This research is not without limitations to consider. Taking cognitive and clinical assessments out of controlled laboratory settings and into the noisy real world can introduce concerns related to data quality. For example, task performance and self-report scores may be affected by lapses in attention, distractions or careless responding and there is a risk that this creates spurious or inflated associations between cognitive performance and mental health symptomatology (Zorowitz et al., 2021). To mitigate this, participants in our study are not tied to specific time constraints and have the freedom to engage with assessments suited to stop the game if interrupted, and are not financially induced to participate. A second limitation concerns sampling bias; smartphone science in some respects helps us to tackle issues with small and unrepresentative samples in psychology research, but it too comes with its own biases. Participants in this study self-selected to engage with this research and did so without remuneration. They may carry certain characteristics that are not representative of the broader population, particularly compared to those with low digital literacy or who don't have internet access.

2.4.2 Conclusions

In a brief smartphone game, where participants shot at diamonds, we replicated robust associations with socio-demographics and compulsivity at-scale, and demonstrate canonical effects of model-based behaviour with psychometric properties similar to its traditional version. Overall, we present evidence that smartphone science opens the door to data-driven task-optimization, increasing

their potential be translated into clinical decision tools in the future, bringing research into practice.

Chapter 3: Model-based planning is unaffected by ketamine, antidepressant and internet delivered cognitive behavioural therapy treatments

This chapter was jointly authored with Dr. Shabnam Hossein. My contributions included the conceptualisation of the design and data analysis for Experiment 2, while Dr. Shabam Hossein contributed to the conceptualisation and data analysis for Experiment 1. Both joint first authors shared equal contributions in drafting and writing of the manuscript.

3.1 Introduction

Depression is a prevalent, debilitating and potentially life-threatening condition and one of the world's leading causes of disability (Friedrich, 2017). A range of effective treatments exist, but none are universally effective. Response to first-line treatments such as antidepressant medication or cognitive behavioural therapy (CBT) is estimated at around 50% (Cuijpers et al., 2023; Trivedi et al., 2006), and up to 30% of patients do not respond to any of the available forms of treatment (McIntyre et al., 2023). Newer treatments such as intravenous (IV) ketamine have shown great promise for patients who are characterised as 'treatment resistant' to first-line therapies, but response rates are still not adequate (Price, Kissel, et al., 2022). One potential explanation for the variability seen in treatment response is the heterogeneity that exists in clinical presentation and impairments. One aspect of this heterogeneity is cognitive impairments which are present in 20%-50% of patients with depression (Hack et al., 2023; Liu et al., 2023; McIntyre et al., 2013). Research has shown that cognitive problems persist into remission and are associated with poorer treatment outcomes overall (Douglas & Porter, 2009; LeMoult & Gotlib, 2019). Meta-analyses of cognitive dysfunctions in depressed patients suggest pronounced deficits in inhibition (overriding dominant or prepotent responses), shifting (switching between mental sets or tasks), and updating (adding or discarding working memory contents) (Snyder, Miyake, & Hankin, 2015). Cognitive problems also include deficits in memory, executive functioning, cognitive flexibility (ability to adapt to changing circumstances or update beliefs based on new

information) (Conradi et al., 2011; Levens & Gotlib, 2015; McIntyre et al., 2013), as well as subjective difficulties in concentration and decision-making listed as DSM-5 diagnostic criteria (American Psychiatric Association, 2013). These attenuated cognitive processes are critical for goal-directed behaviour, crucial for navigating and adapting to complex environments.

Goal-directed behaviour requires planning, simulation, and reflection to achieve a desired outcome (Dickinson, 1985; Dolan & Dayan, 2013). Inspired by computational theories of reinforcement learning, the two-step decision-making task (Daw et al., 2011) formalizes goal-directed control as a construct called ‘model-based planning’. Model-based planning is quantified as the extent to which decision-makers use a mental model of action-state relationships to guide choices. Prior studies examining goal-directed planning in depression are surprisingly few. Most studies to date have used general population samples, rather than patients with depression, and have not revealed any cross-sectional associations between individual differences in depression and model-based planning (Donegan et al., 2023; Gillan, Kosinski, et al., 2016; Seow et al., 2021). However, one study showed that individuals with high levels of depression were more sensitive to the deleterious effects of stress on model-based planning (Heller et al., 2018). Another study found a similar negative association between anxious-depression and model-based planning, but only when the task had low stakes (Patzelt et al., 2019). More recently, researchers compared patients diagnosed with depression to healthy controls and found no differences in model-based planning (Knolle et al., 2023), but sample size was likely too small (N=23) to make definitive conclusions. No study has yet tested whether model-based planning improves following treatments for depression, or indeed how heterogeneity in this cognitive capacity within individuals with depression might differentially relate to treatment response. Understanding how different therapeutic interventions impact goal-directed planning in depression could reveal (or refute) plausible treatment mechanisms and help tailor treatments to address specific cognitive impairments.

Our understanding of the effect of treatment on cognitive deficits in depression is growing, but it remains incomplete. For example, some studies showed beneficial effects of antidepressant medications such as improvement in verbal memory

(Keefe et al., 2014), delayed recall (Rosenblat et al., 2016) and working memory (Baune & Renger, 2014; Dam et al., 2021); others have shown no changes in executive functioning and cognitive control (Rosenblat et al., 2016). In a large randomised longitudinal clinical trial (iSPOT-D) of N>1000 unmedicated patients with depression, Shilyansky and colleagues (2016) found no effect of three types of antidepressant medication on a range of cognitive domains including cognitive flexibility. Studies examining cognitive changes following CBT have similarly yielded inconclusive results. A systematic review of 19 studies found preliminary evidence of improved executive functioning following evidence-based psychotherapy for depression (Groves et al., 2021). Studies examining the effects of IV ketamine on cognitive dysfunction are still in their infancy. A recent systematic review (Grasso et al., 2024) on post IV ketamine treatment found that from the 14 studies examined, more than half of them reported positive cognitive outcomes, including improvements in working memory, verbal and visual memory, executive function, attention, and auditory verbal episodic memory. In contrast, the rest of the studies showed no improvement and in one case reported negative effects on verbal memory.

Building on this work, we administered the two-step task across two parallel studies using three different types of interventions: IV ketamine infusion, antidepressant medication, and internet-based CBT (iCBT). Experiment 1 was a randomized controlled trial of ketamine in patients with treatment-resistant depression. Experiment 2 was a longitudinal observational study following patients receiving antidepressant medication or iCBT for 4 weeks, along with a control group receiving no treatment to control for practise effects. In all cases, we tested if task performance improved following treatment and if the extent of clinical improvement was associated with the degree of cognitive improvement.

3.2 Method

3.2.1 Experiment 1

Data were collected as part of a double-blind placebo-controlled randomized clinical trial of IV ketamine for treatment resistant depression (Figure 3.1A). A detailed description of the methods used can be found in Price et al. (2022). The protocol was ethically approved by the University of Pittsburgh Institutional Review Board.

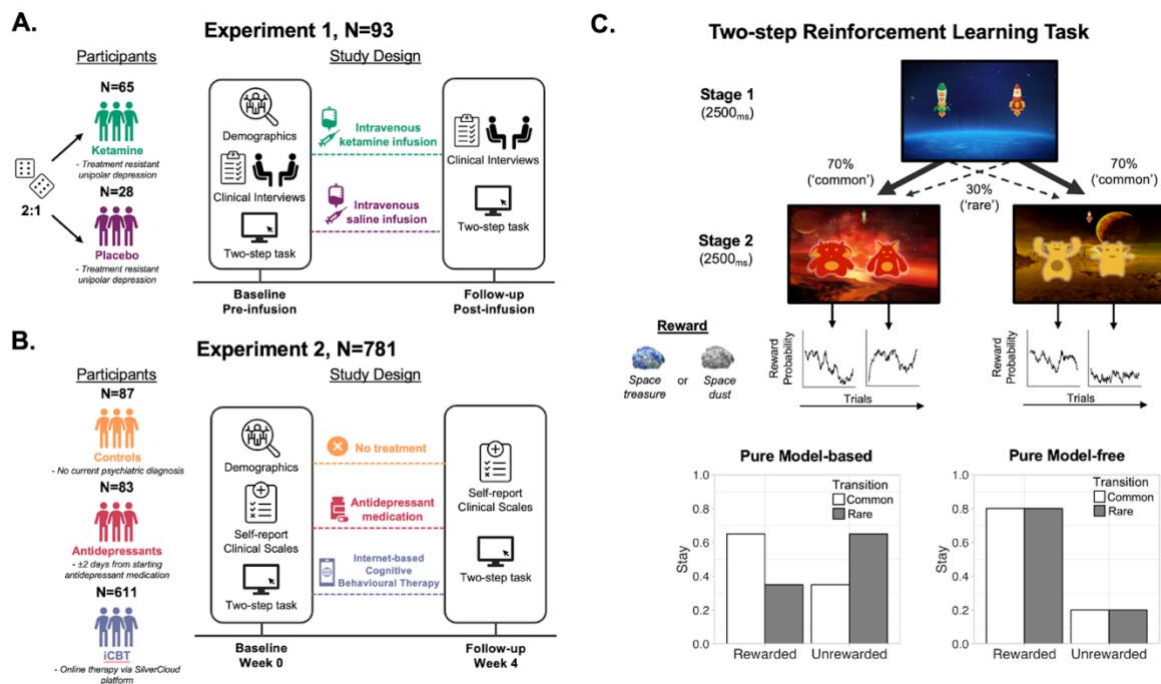


Figure 3.1 Overview of study procedures. **A.** Experiment 1 collected data from a double-blind placebo-controlled randomized clinical trial of ketamine-infusion therapy for treatment resistant depression. Participants were randomly assigned to either the ketamine or saline (placebo) group in a ratio of 2:1. Pre-infusion, or at baseline, participants self-reported on sociodemographics, completed a clinician-administered scale for assessing depression severity, and the two-step task. Participants then received either a single dose of intravenous ketamine-infusion or saline-infusion. 24 hours post-infusion, participants were reassessed on the clinician-administered scale and completed the two-step task. **B.** Experiment 2 consisted of an observational study design where participants were followed during their first four weeks of antidepressant or internet-based cognitive behavioural therapy treatment. At baseline, participants reported on sociodemographics and a battery of mental health questionnaires used to derive transdiagnostic scores of anxious-depression and compulsivity and intrusive thought. They also completed the two-step task. Following four weeks, the participants re-engaged with the mental health questionnaires and completed the two-step task. An additional group of individuals who reported no current psychiatric illness completed the same procedure in the same time frame to serve as a control group. **C.** Schematic of the two-step reinforcement learning task participants completed. In the first stage, participants first choose between two stimuli, and are probabilistically transitioned to one of two second stage states (70% preferred planet, 30% alternative planet). Participants choose again between two stimuli, being rewarded with space treasure or unrewarded with space dust. The probability of being rewarded drifts slowly and independently for each stimulus. In this task, model-based learners will use an internal model of the structure to maximise the amount of space treasure collected while model-free learners will base their decision on previous reinforcement.

Participants

Individuals between 18-60 years of age with treatment resistant unipolar depression were recruited from the community and outpatient clinical settings through website

advertisements, clinical referrals, or a local research registry. Treatment-resistant depression was defined as at least one unsuccessful adequate trial of an antidepressant medication approved by the U.S. Food and Drug Administration in the current depressive episode. All participants met DSM-5 criteria for major depressive disorder and reported moderate to severe levels of depression (a score ≥ 25 on the Montgomery-Åsberg Depression Rating Scale (MADRS); (Montgomery & Åsberg, 1979)). Further details and full inclusion and exclusion criteria are available at ClinicalTrials.gov: NCT03237286. Baseline demographics are reported in Supplementary Table 7.2.2.1. There were no significant age ($p = .580$), sex assigned at birth ($p = .650$) or education attainment ($p = .688$) differences observed between the ketamine and saline-control arms.

Study Procedures

Participants were randomized to a single dose of IV ketamine (0.5 mg/kg) or IV saline (50 mL 0.9% NaCl) administered over a 40-minute infusion, followed by 4 hours of clinical monitoring. All infusions were administered by blinded, licensed nurses in a medical hospital setting. Pre-infusion participants provided psychological, clinical, and cognitive data in the larger parent study. A subset of those measures were analysed as secondary data analysis. At both pre-infusion and 24-hour post-infusion, participants' depressive symptoms were assessed using MADRS, and a subset of participants, enrolled during later stages of the parent trial, completed the two-step reinforcement learning task. After exclusion criteria, there were $N = 65$ participants in the ketamine arm and $N = 28$ participants in the saline arm that had both task and clinical data at both time points. A 2:1 ketamine: saline condition was used due to the additional comparator groups used to test primary aims of the parent study (see Price et al., (2022) for primary clinical trial findings).

Two-step Reinforcement Learning Task

Participants completed 100 trials (Decker et al., 2016) of an adapted version of the two-step reinforcement learning task (Daw et al., 2011) (Figure 3.1C). In the first step, participants had to choose between two rockets which probabilistically travelled to one of two planets. Each rocket travelled to a preferred planet 70% of the time ('common' transition) or to the other planet 30% of the time ('rare' transition). Next, participants had to choose between two aliens, each with a unique

probability of being rewarded. The probability of being rewarded drifted slowly and independently for each alien over the course of the task. In both stages, participants had up to 2500ms to respond. In this task, model-based and model-free learning have distinctive behavioural signatures. A pure model-based learner takes into account estimates of how rewarding each alien is and how likely each rocket is to travel to the planet where that alien resided. Behaviourally, this is expressed by an interaction between reward and transition. By contrast, a pure model-free learner only considers previous reinforcement and is behaviourally characterised by a main effect of reward.

Clinical Outcome Measure

The primary clinical outcome measure was the Montgomery-Åsberg Depression Rating Scale (MADRS). The MADRS is a 10-item clinician-administered scale, designed to be particularly sensitive to antidepressant treatment effects in patients with major depression. The MADRS used in present analyses was collected at two time points: a pre-infusion baseline visit (completed approximately 1 hour prior to infusion) and at a 24-hour post-infusion visit, and was administered by a single, blinded, Master's-level study rater.

3.2.2 Experiment 2

Data were collected as part of the Precision in Psychiatry Study (PIPS), an observational study that followed individuals through their first four weeks of treatment with iCBT or antidepressant medication (Figure 3.1B). The protocol obtained ethical approval from the Research Ethics Committee of School of Psychology, Trinity College Dublin and the Northwest-Greater Manchester West Research Ethics Committee of the National Health Service, Health Research Authority and Health and Care Research Wales.

Participants

Individuals commencing iCBT on the SilverCloud platform within 2 days of beginning their treatment were recruited for this study. They were invited through an NHS mental health service after being prescribed iCBT or if they elected to avail of iCBT provided by an Irish-based mental health charity, Aware. An antidepressant arm was also recruited ± 2 days from beginning their treatment, they were recruited through

physical e.g., pharmacies, and online advertisements. An age and gender-matched control group were included if they reported to not be undergoing current psychiatric treatment and did not have a current psychiatric diagnosis. Participants in all arms were excluded if they were not between 18-70 years of age, were not fluent in English or did not have computer access. The active treatments arm had to score over 10 on the Work and Social Adjustment Scale, while the control arm had to score below 10. The control arm was younger ($M = 28.5$, $SD = 11.9$) than the iCBT arm ($M = 31.5$, $SD = 10.8$; Tukey HSD: $p = .040$; Supplementary Table 7.2.2.1), but there were no significant gender ($p = .474$) or education attainment ($p = .121$) differences.

Study Procedures

A detailed description of the study design has been published in Lee et al. (2021). Participants completed a baseline assessment, three brief weekly check-in assessments and a final assessment at the 4-week follow-up. At baseline, participants completed the two-step task along with a battery of clinical questionnaires which were repeated at the 4-week follow-up. Participants completed the entirety of the procedure online, which included additional assessments not pertinent to the present study.

Two-step task

The same paradigm was used as described in Experiment 1, except that participants in this experiment completed 150 trials.

Self-report questionnaires

We focus on a subset of self-report data gathered from the PIP study including age, gender and educational attainment and a transdiagnostic measure of ‘anxious-depression’ and ‘compulsivity and intrusive thought’. These transdiagnostic dimensions are derived from a factor analysis and capture relevant variance in psychopathology across a set of previously validated items (Gillan, Kosinski, et al., 2016). Here, ‘anxious-depression’ reflects items related to anxiety and depression, and not an anxious subtype of depression, while ‘compulsivity and intrusive thought’ reflects items related to OCD, disordered eating, impulsivity and schizotypy.

Additional information about the derivation of these scores is provided in the online supplement (Supplementary Methods and Analyses 7.2.1.2).

Data Analysis

Quantifying model-based planning. Data were analysed using a hierarchical logistic regression model implemented using the lme4 R package (Bates et al., 2015). The model explains stay or switch (coded 1,0) behaviour, from the type of transition on the last trial (coded common = 1, rare = -1), whether that trial was rewarded (coded rewarded = 1, unrewarded = 0), and their interaction. Reward, transition and their interaction were included as random effects. The outcome of interest, model-based planning, is quantified by the interaction between reward and transition. The model also quantifies other indices of task performance such as choice preservation (the model's intercept) and a model-free index (the main effect of reward). To examine individual differences in model-based planning, we use subject-level reward*transition coefficients (random slopes) as regressors or in correlation analyses. Analyses were performed using R (version 4.2.3 and version 4.0.4 for studies 1 and 2).

3.3 Results

3.3.1 Experiment 1

Does ketamine treatment improve model-based planning?

A hierarchical mixed effects model ($\text{stay}_t \sim \text{reward}_{t-1} * \text{transition}_{t-1} * \text{infusion group} * \text{session}$) was used to test for within-group differences from pre-infusion to 24 hours post-infusion, between-group differences in the ketamine vs saline arm, and their interaction on model-based planning (Supplementary Table 7.2.2.3, Figure 3.2A). As expected, there was an overall tendency to repeat choices trial-to-trial ($\beta = 1.40$, $SE = .23$, $p < .001$), and a main effect of reward ($\beta = .45$, $SE = .12$, $p < .001$), indicative of model-free learning, and an interaction between reward and transition ($\beta = .16$, $SE = .07$, $p = .030$), reflecting model-based learning. Crucially, we failed to find differences in model-based planning between the ketamine and saline group (reward*transition*group interaction: $\beta = .03$, $SE = .09$, $p = .708$), or between pre-infusion and 24-hours post-infusion (reward*transition*session interaction: $\beta = -.12$, $SE = .08$, $p = .10$). We also observed no significant interaction between group and

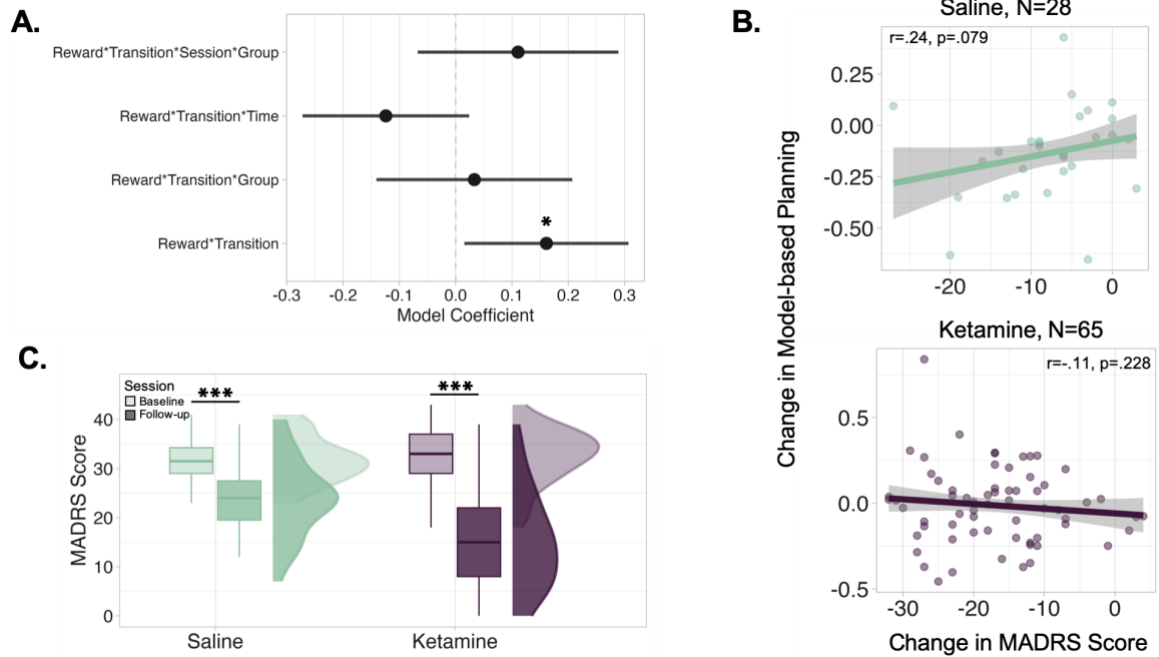


Figure 3.2. Results from Experiment 1: A Randomized Clinical Trial of Ketamine for Treatment Resistant Depression. **A.** Regression beta coefficients of the hierarchical model on stay behaviour explained by reward, transition, group, session and their interactions. There was an overall effect of model-based planning (reward*transition interaction). No other significant interactions were observed suggesting model-based did not change from pre- to post-infusion in either arm. Error bars reflect 95% confidence interval. **B.** Correlation between changes in depression severity MADRS Change and change in model-based planning per arm (saline, ketamine). Each dot represents a participant, the line represents the line of best fit while the shaded areas represent 95% confidence interval. **C.** Distribution and boxplot of values of MADRS scores across (ketamine vs saline control) and within (pre- vs post-infusion) arms. A significant group*session interaction was observed with both groups demonstrating reductions of depressive symptoms from pre- to post-infusion however this effect was steeper in the ketamine group. Central line reflects median values. For all panels: * $p < .05$, ** $p < .01$, *** $p < .001$.

session on model-based planning (reward*transition*infusion group*session: $\beta = .11$, $SE = .09$, $p = .222$), suggesting change in model-based planning from pre- to post-infusion did not differ across the groups.

We then tested whether changes in depressive symptoms were associated with changes in model-based behaviour. In other words, we examined whether those who experienced the biggest therapeutic benefit (i.e., greater MADRS change score), also have the biggest change in model-based learning. A linear mixed-effects model was used to explain model-based planning from interactions between session*MADRS change scores from pre-infusion to 24-hours post-infusion,

controlling for MADRS score at baseline. There were no significant associations between model-based planning and change in depression scores in any of the infusion groups (Session*MADRS change score: Saline group: $\beta = .01$, $SE = .01$, $p = .226$, Ketamine group: $\beta = -.00$, $SE = .00$, $p = .399$, Supplementary Table 7.2.2.4). This was supported by non-significant simple correlations between changes in model-based scores and changes in depression severity as measured by the MADRS in each group (Saline arm: $r = .24$, $p = .079$, Ketamine arm: $r = -.11$, $p = .228$, Figure 3.2B).

Are individual differences in model-based planning associated with response to ketamine treatment?

A linear mixed-effects model was used with the MADRS scores as the dependent variables and session, infusion group, baseline model-based scores, and their interaction as regressors. We observed a main effect of session ($\beta = -7.89$, $SE = 1.57$, $p < .001$, suggesting depression severity reduced in both groups from pre to post 24-hour infusion, and a significant interaction effect between session and infusion group ($\beta = -9.14$, $SE = 1.87$, $p < .001$, Supplementary Table 7.2.2.5). This was confirmed by simple effects within each group indicating those in the ketamine arm exhibited a greater decrease in the MADRS score ($t = -15.65$, $p < .001$) compared to the saline arm ($t = -5.86$, $p < .001$, Supplementary Table 7.2.2.6, Figure 3.2C). To test if clinical improvement was moderated by baseline model-based planning, we examined the interaction between baseline model-based scores, session and infusion group which was non-significant ($p = .183$, Supplementary Table 7.2.2.5).

3.3.2 Experiment 2

Does model-based planning improve following antidepressant medication or iCBT?

A hierarchical mixed-effects model ($\text{stay}_t \sim \text{reward}_{t-1} * \text{transition}_{t-1} * \text{group (iCBT, antidepressant, control as the reference)} * \text{session}$) was used to test for group differences in model-based planning, improvement pre-post treatment, and their interactions. Full model results are presented in Supplementary Table 7.2.2.7; here, we focus only on the key results in text. First, looking at basic task behaviour in the reference group at baseline, we identified the model-free effect of reward on stay

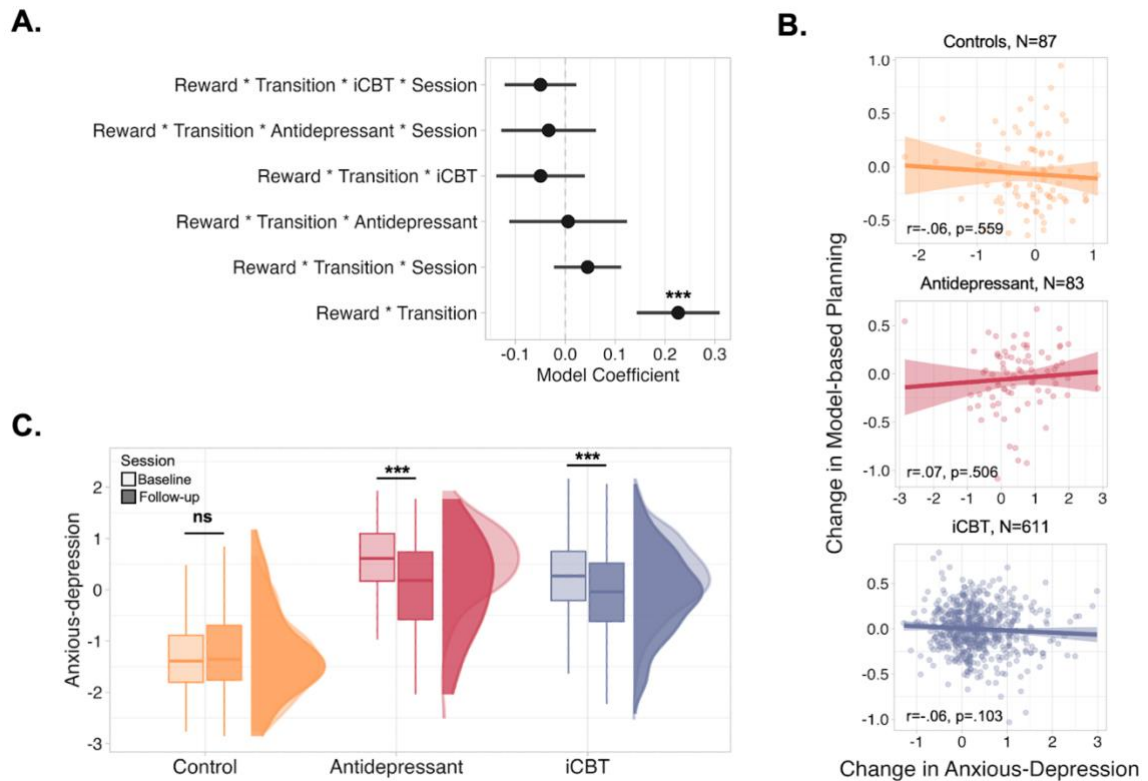


Figure 3.3 Results from Experiment 2: Precision in Psychiatry Study. **A.** Regression beta coefficients of the hierarchical model on stay behaviour explained by reward, transition, group (Control, Antidepressant and iCBT), session (baseline, follow-up) and their interactions. For illustrative purposes, only model-based results are presented. There was an overall effect of model-based planning (reward*transition interaction). No other significant interactions were observed suggesting model-based did not change from baseline to follow-up infusion and in any group (control as reference group). Error bars reflect 95% confidence interval. **B.** Correlation between changes in anxious-depression (AD) and change in model-based (MB) planning per group. Each dot represents a participant, the line represents the line of best fit while the shaded areas represent 95% confidence interval. **C.** Distribution and boxplot of values of anxious-depression scores across control and treatment groups (Antidepressant, iCBT) and within (baseline, 4-week follow up) groups. Anxious depression scores reduced from baseline to follow-up in the treatment groups only. Central line reflects median values. For all panels: ns $p > .05$, * $p < .05$, ** $p < .01$, *** $p < .001$.

behaviour ($\beta = .30$, $SE = .05$, $p < .001$), a model-based reward*transition interaction ($\beta = .23$, $SE = .04$, $p < .001$), and a tendency to stay with the same choice on subsequent trials ($\beta = 1.26$, $SE = .10$, $p < .001$). Crucially, the effect of model-based planning at baseline did not differ by group (reward*transition*antidepressant: $\beta = .01$, $SE = .06$, $p = .925$; reward*transition*iCBT: $\beta = -.05$, $SE = .05$, $p = .274$, Figure 3.3A). There was no change from baseline to follow-up in model-based planning in controls (reward*transition*session: $\beta = .05$, $SE = .03$, $p = .193$, Figure 3.3A) and

there were no differences between the control group and the antidepressant or iCBT groups in this regard (all $p > .176$). This was confirmed by the results of simple effects within each group. Model-based planning did not significantly improve from baseline to 4-week follow-up in the control group ($\beta = .05$, $SE = .03$, $p = .174$, Supplementary Table 7.2.2.8), antidepressant treatment group ($\beta = .01$, $SE = .03$, $p = .722$, Supplementary Table 7.2.2.8), or in the group of individuals who received 4-weeks of iCBT treatment ($\beta = -.01$, $SE = .01$, $p = .687$, Supplementary Table 7.2.2.8). To test if intra-individuality in treatment response might explain this, we examined if changes in model-based planning were evident in those who had the largest improvement in symptoms. This was tested using a repeated measures model with model-based scores as the dependent variable and independent variables of session, change in anxious-depression and group. The critical interaction between change in anxious-depression scores, group and time was not significant (all $p > .284$, Supplementary Table 7.2.2.9). For completion, we also ran this separately in each of the groups (session*AD change – all $p > .133$, Supplementary Table 7.2.2.10). Finally, this was confirmed again by non-significant simple correlations between changes in model-based scores and changes in anxious-depression in each group (Control: $r = -.06$, $p = .559$, Antidepressant: $r = .07$, $p = .506$, iCBT: $r = -.06$, $p = .133$, Figure 3.3C).

Are individual differences in model-based planning at baseline associated with response to antidepressants or iCBT treatment?

A linear mixed-effects model was used with anxious-depression scores serving as the dependent variable and main effects of session, group (iCBT, antidepressant, control as the reference), model-based scores at baseline, and their interaction entered as regressors (Supplementary Table 7.2.2.11). There was a main effect of antidepressant group, indicating that anxious-depression scores were higher in the antidepressant group, compared to the control group ($\beta = 1.84$, $SE = .15$, $p < .001$), and similarly they were higher in the iCBT compared to the control group ($\beta = 1.52$, $SE = .15$, $p < .001$). There was no main effect of session in the control group ($\beta = .10$, $SE = .08$, $p = .217$). There was an interaction between session and the antidepressant ($\beta = -.62$, $SE = .13$, $p < .001$) and the iCBT group ($\beta = -.45$, $SE = .09$, $p < .001$), such that both groups showed larger reductions in symptoms at follow-up. Simple effects within each group confirmed that both groups had a

reduction in anxious-depression from baseline to follow-up (Antidepressants: $\beta = -.59$, $SE = .11$, $p < .001$; iCBT: $\beta = -.40$, $SE = .03$, $p < .001$), while the control group did not ($\beta = .12$, $SE = .08$, $p = .103$, Supplementary Table 7.2.2.12). There was no main effect of model-based scores at baseline on anxious-depression in the control group ($\beta = -.22$, $SE = .23$, $p = .337$, Supplementary Table 7.2.2.11) and this did not differ across the treatment groups ($p > .595$). Finally, there was no interaction between baseline model-based scores and treatment response, reflected by the non-significant interactions between session, group and model-based planning at baseline (all $p > .433$).

Compulsivity and model-based planning

Prior research has consistently shown that individual differences in compulsivity (CIT) are related to model-based planning. As a validation step, we sought to replicate these associations here. At baseline, we identified a significant association in all three groups (Antidepressant: $\beta = -.08$, $SE = .04$, $p = .053$, Controls: $\beta = -.12$, $SE = .06$, $p = .031$, iCBT: $\beta = -.05$, $SE = .02$, $p = .002$, Supplementary Table 7.2.2.13). There were no significant changes in this association from baseline to follow-up in any group, reward*transition*CIT*session, all $p < .23$ (see Supplementary Table 7.2.2.14 for full results).

Test-retest reliability of the two-step task estimates

Using the control group, which underwent no treatment, we estimated the 4-week test-retest reliability of model-based estimates to understand if issues with task reliability may have reduced our ability to detect changes. An intraclass correlation coefficient of $ICC_1 = .59$, $p < .001$ was observed suggesting model-based estimates were moderately reliable over 4 weeks.

3.4 Discussion

The present study found that model-based planning did not improve following three evidence-based treatments for depression: ketamine-infusion, antidepressant medication, or internet-based cognitive behavioural therapy. Experiment 1 was one of the largest randomised controlled ketamine trials in depression to date. In line with the previous literature, we found rapid reductions in mood symptoms in the ketamine group compared to saline-control (Singh et al., 2016; Zarate Jr et al.,

2012). Importantly, these improvements were not accompanied by increases in model-based planning. While considerable attention has been paid to ketamine's effects on mood, its impact on cognitive functions is less understood. Some have suggested that ketamine reverses neuroplasticity deficits in depression, as measured by increase of BDNF release and synapse number as well as the stimulation of mTOR signalling in the PFC (Kopelman et al., 2023) and that this might produce cognitive enhancements. In line with this, some preclinical and clinical studies have found improvements in cognitive flexibility following ketamine administration, as indexed by performance in neurocognitive tests such as the set-shifting task or Stroop interference test following the administration of ketamine (Gass et al., 2019; Permoda-Osip et al., 2015; Shiroma et al., 2014). However, many studies show no improvements, and in general, studies have been limited by small sample sizes and a lack of rigorous control arms (Gill et al., 2021; Grasso et al., 2024). Our null findings suggest that the enhanced neuroplasticity induced by a single dose of ketamine might not lead to an effect on model-based planning.

We observed a consistent null result in Experiment 2 in patients following 4-weeks of antidepressant and iCBT treatments. As with ketamine, these treatments were effective in reducing anxious-depression from baseline to follow-up, but this did not translate to changes in model-based planning. These findings add to a growing body of literature examining cognitive changes following these treatments, which largely operate on the premise that iCBT and antidepressant medications could improve cognition by alleviating depressive symptoms that potentially interfere with cognitive functions. Our null findings with respect to antidepressant medications do not support this but are in line with those of the iSPOT-D study, which failed to identify cognitive improvements in >1000 individuals randomised to a variety of antidepressant medications (Shilyansky et al., 2016). Prior studies of psychotherapy have suggested that there is some preliminary evidence for executive function changes following treatment, but evidence is overall mixed, depending on sample size and the existence of pre-treatment deficits (Groves et al., 2021). The present study employed a large sample size making it an important addition to the literature.

Leveraging the large sample size in the iCBT arm, we also tested for cross-sectional associations with depression symptomatology. We found that anxious-depression

was not associated with individual differences in model-based planning while a dimension of “compulsivity and intrusive thought” was associated with reduced model-based planning, consistent with prior research (Donegan et al., 2023; Gillan, Kosinski, et al., 2016; Seow et al., 2021). The lack of association with depression is consistent with what has been previously observed in general population studies (Donegan et al., 2023; Gillan, Kosinski, et al., 2016; Seow et al., 2021), and more recently in a small patient sample (Knolle et al., 2023). Our results extend these findings to a large treatment-seeking sample. Although compulsivity symptoms also reduced following iCBT and medication treatments in Experiment 2, similar to depression, there was no link between improvements in compulsivity and changes in model-based planning. This mirrors the findings of a prior study examining model-based planning after exposure and response prevention for OCD, where model-based planning did not improve despite substantial reductions in OCD scores following treatment (Wheaton et al., 2019). Another study found that a single session of theta burst stimulation did not alter model-based planning, in spite of significant clinical improvements among patients with clinical compulsive behaviours (Brown et al., 2022). More recently, a study followed a general population sample over 3 and 12 months and also found that model-based planning remained relatively stable and did not fluctuate with changes in compulsivity within-person over time (Sookud et al., 2024). These findings together suggest that model-based planning has a trait-like quality. Moreover, both the cross-sectional and longitudinal findings of the present study provide convergent evidence that current (i.e. ‘state’) levels of distress, anxiety or generalized impairment are an unlikely explanation for the reductions in model-based planning consistently reported in compulsivity.

3.4.1 Limitations

In Experiment 1, patients received a single dose of IV ketamine. It is possible that repeated ketamine administrations (e.g., 6 infusions over 2-3 weeks, as is the typical induction phase in treatment-resistant depression treatment) might have an effect on model-based planning. Experiment 2 was an observational and rather than randomised controlled design and so causal inferences about different treatments cannot be made. However, this limitation is mitigated given our principal analysis depends on within-person changes. The iCBT treatment was self-paced and

modules varied across participants (see Lee et al., (2021) for further details), this means that participants received different effective doses of the intervention. We therefore cannot exclude the possibility that a more potent and regimented intervention may incur larger clinical improvements and potentially translate to changes in model-based planning. Finally, it is possible that four weeks of treatment is insufficient to achieve the sort of sustained clinical gains required to see knock-on improvements in cognition. However, the consistency of our findings to the longer duration iSPOT-d study (Shilyansky et al., 2016) suggest this is likely not the case.

3.4.2 Conclusion

This study provides convergent evidence that model-based planning is not linked to within-person changes in depressive symptoms following a range of treatments, or with individual differences in depression in a large treatment-seeking sample. These data support the notion that model-based planning is a relatively stable aspect of cognitive functioning with specific relevance for compulsive and not mood disorders.

Chapter 4: Compulsivity is associated with accelerated stimulus-response habit-learning

4.1 Introduction

Compulsive behaviours are characterised by intense urges to perform repetitive acts that led to negative consequences (Figeet al., 2016). These behaviours persist despite being recognised as harmful (e.g., drug-taking), irrational (e.g., believing that failure to act will cause harm to a loved one), or misaligned with broader goals (e.g., continuing to wash hands after they are cleaned). Compulsive behaviours manifest transdiagnostically; implicated in psychiatric conditions such as obsessive-compulsive disorder (OCD), binge eating, and substance-use disorders (Anagnostou et al., 2011; Fineberg et al., 2014; Hollander et al., 2016). Considerable research has identified impairments in goal-directed control in clinical (Ersche et al., 2021; Gillan et al., 2020; Sjoerds et al., 2013; Voon et al., 2015) and general population samples (Donegan et al., 2023; Gillan, Kosinski, et al., 2016; Patzelt et al., 2019; Seow et al., 2021; Sharp et al., 2023; Sookud et al., 2024), with those reporting higher levels of compulsivity exhibiting more pronounced reductions in goal-directed learning and/or control. Current theories propose that such differences disrupt the balance between habitual/goal-directed processes, causing an over-expression of stimulus-response habits (Gillan, Robbins, et al., 2016). For example, in traditional habit paradigms where participants are extensively trained on stimulus-response associations, OCD patients continue to execute the previously learned responses (i.e. the habit), even after the outcome of that action has been rendered undesirable (i.e., devalued) (Gillan, Morein-Zamir, Urcelay, et al., 2014; Gillan et al., 2011). However, a key limitation of these methods is that they confound goal-directed and stimulus-response associative strength. That is, greater habit expression may stem from reductions in the former or an excess of the latter. Moreover, growing evidence suggests that these tasks may not be sensitive to stimulus-response learning at all, as they fail to show habit expression even after manipulations designed to induce habits such as over-training (de Wit et al., 2018; Gera et al., 2023; Pool et al., 2021). Thus, while reductions in goal-directed control may be sufficient to explain increased habit expression in compulsive individuals, it

is also possible that the ability to form stimulus-response associations is exaggerated in compulsivity.

To address the limitations of existing paradigms, Hardwick et al. (2019) designed a task to parse the contributions of stimulus-response and goal-directed strength in habit expression. Their approach leveraged a key distinction between the systems: the stimulus-response system operates faster than the goal-directed system. By constraining the amount of time individuals had to prepare a response, they demonstrated the impact of overtraining on habit expression, a finding that has since been replicated (Du & Haith, 2023, 2025; Martínez-López et al., 2025). This provides a novel opportunity to test whether compulsivity is associated not only with reductions in goal-directed control, but also with an accelerated propensity to learn stimulus-response associations. To date, studies have focused on the speed of motor sequence learning (Banca et al., 2024) or performance on implicit learning tasks (Deckersbach et al., 2002; Soreff et al., 2021), and report no differences, or even deficits, between OCD patients and healthy controls. This may be because such paradigms make it difficult to control for the influence of goal-directed processes during initial learning, a challenge better addressed by the time-dependent habit paradigm.

Beyond compulsivity, it remains unclear to what extent habit processes contribute to other forms of repetitive behaviour in psychiatry. These include behaviours with a strong motor component, such as tics and bodily-focused repetitive behaviours (e.g., hair-pulling and skin-picking). Unmedicated patients with Tourette's Syndrome ($N = 17$) have shown increased habit expression compared to healthy controls in the classic devaluation paradigm (Delorme et al., 2016). These individuals also demonstrated intact response-outcome learning, suggesting that enhanced stimulus-response learning may be driving this effect. Furthermore, although bodily-focused repetitive behaviours are diagnostically classified under obsessive-compulsive related disorders, the degree to which these conditions are related remains a point of contention. Conceptual differences and factor analytic studies increasingly support the classification of grooming behaviours as distinct from compulsions (Maraz et al., 2017; Snorrason et al., 2021; Snorrason et al., 2022).

To address these questions, we developed a gamified habit-learning smartphone task based on Hardwick et al. (2019) to test for individual differences in habit learning. We trained $N=1000$ participants on this game for 7 days and collected transdiagnostic self-report clinical data to assess the specificity and generalisability of associations between habit learning and psychiatric traits. In a set of pre-registered hypotheses and analyses, our aims were twofold: (1) to assess the validity of the gamified task in measuring stimulus-response habits by replicating overtraining and time-dependence effects on habit expression, and (2) to determine whether individual differences in habit learning were associated with compulsivity and/or lower-order motor behaviours.

4.2 Method

4.2.1 Participants

Participants ($N = 1000$) were recruited through Prolific (www.prolific.com), an online crowdsourcing platform. Participants were included if they were over 18, had access to a smartphone device and a stable internet connection, and were fluent in English. A power analysis, based on prior associations between self-reported levels of compulsivity and intrusive thought and model-based planning (Donegan et al., 2023; Gillan, Kosinski, et al., 2016), determined that $N = 690$ would be required to detect a similar small effect with 80% power at a .05 level of significance. We planned to over-recruit to $N = 1000$ to allow for data loss due to incomplete material and data quality. Following exclusions (described in the Statistical Analysis section), $N = 847$ were included for behavioural analysis of the habit-learning game (mean age = 38.12 ± 12.29 ; 67% women; 64% attained a third-level degree). Of these, $N = 822$ had complete demographic and mental health data for individual differences testing (mean age = 37.93 ± 12.23 ; 68% women; 66% attained a third-level degree). A full breakdown of demographic variables is reported in Supplementary Table 7.3.2.3.

4.2.2 Measures and Material

CogQuest: A Gamified Habit Learning Task

We developed CogQuest as a gamified adaptation of the task used in Hardwick et al. (2019) that could be administered through the smartphone application Neureka (www.neureka.ie). This task aimed to develop habits through overtraining of simple

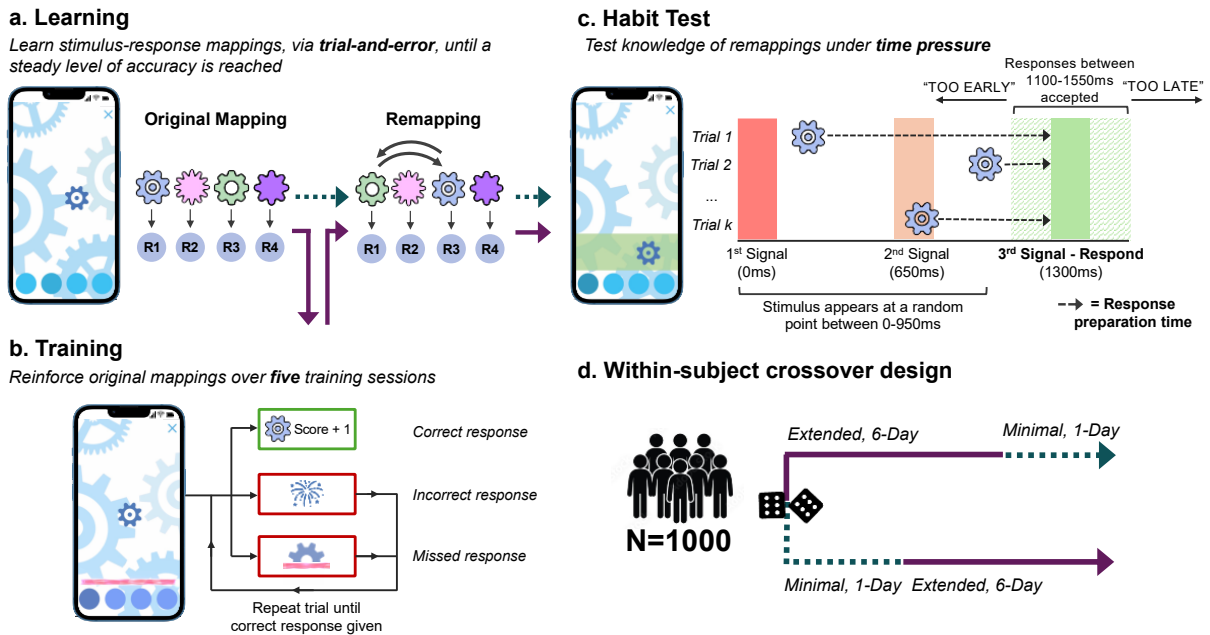


Figure 4.1. CogQuest, a gamified tool to measure habit-learning. **a.** In the learning phase participants learn four stimulus-response mappings, via trial-and-error, until they reach five consecutive correct responses for each stimulus. Immediately after (minimal training: dotted arrow) or following extensive training (extended training: solid arrow), two out of the four mappings are swapped which participants must relearn until criterion. **b.** In the extended training condition, participant reinforce the original mappings over five daily training sessions. Incorrect or missed i.e., no response made before the cog reaches the laser, are repeated until a correct response is given. **c.** In the habit test, participants are required to respond in sync with a signal. At the start of each trial the response zone flashes red (0_{ms}), then amber (650_{ms}) and finally green (1300_{ms}) indicating to respond. Response within 1100-1550_{ms} are accepted as on-time. Response outside of this are flagged as too early/late. The stimulus is presented at a random point before this desired response to manipulate response preparation time. **d.** In a within-subject cross-over design, $N = 1000$ crowd-sourced participants were randomly assigned to either the minimal-first or extended-first condition, and completed both training conditions over the course of the study.

stimulus-response pairings. We employed a within-person crossover design in which all participants completed both minimal (1-day) and extensive (6-day) training, in a randomised order. In both conditions, participants first learned, through trial and error, the correct response to four distinct stimuli ('Original Mapping') (Figure 4.1a). Stimuli were cogs falling from varying screen heights, and responses involved selecting one of four buttons located at the bottom of the screen. Participants continued this until they reached five consecutive correct responses for each stimulus. In the extended training condition only, participants then practiced these pairings over five daily training sessions, responding to the falling cogs before they reached the bottom of the screen (Figure 4.1b). Each of these training sessions

consisted of five blocks with a minimum of 80 trials per block (at least 400 trials daily), as incorrect trials were repeated until a correct response was made. Following this, two of the four mappings were switched, and participants learned these new mappings ('Remapping') until they reached the same five-trial criterion as they did at the outset of training (Figure 4.1a). Participants were then tested under varying levels of time pressure on these revised mappings in a forced-response procedure to assess habitual responding (Figure 4.1c). The test comprised five blocks of 80 trials (400 trials total). Participants were instructed to only respond when the cog passed through the centre of a coloured bar positioned at the bottom of the screen. To make timing the response easier, each trial began with the response bar flashing red (0ms), then amber (650ms), and finally a green (1300ms) when a response was permitted. Preparation time was then manipulated by varying the time at which the cog appeared on the screen which corresponded directly to its starting position. Cogs falling from higher positions allowed more preparation time than those appearing later and closer to the bar. Stimulus onsets were sampled from a uniform distribution between 0-950ms, resulting in a continuous variable of response preparation time ranging from 350-1300ms. This is the task's key manipulation; prior work has shown that shorter preparation times favour habitual responses – i.e., performing the original, but now incorrect, response for remapped stimuli (Du & Haith, 2023, 2025; Hardwick et al., 2019; Martínez-López et al., 2025). After completing the first test, participants started their second training regime of the crossover design (extended or minimal) using new stimuli and response pairings to prevent any transfer of learning (Figure 4.1d). Further details about the task are provided in Supplementary Methods and Analyses 7.3.1.2.

Self-report Mental Health Measures

Before each training session, participants completed one block of self-reported questionnaires related to an aspect of mental health. We assessed Autism Spectrum Disorder (Autism Spectrum Quotient - Short, (Hoekstra et al., 2011)), Tourette's Syndrome (Motor tic, Obsessions and compulsions, Vocal tic Evaluation Survey, (Gaffney et al., 1994)), Skin-picking, (Skin-Picking Scale, (Keuthen et al., 2001)), Trichotillomania hereafter 'hair-pulling' (Massachusetts General Hospital Hairpulling Scale, (Keuthen et al., 1995)), and a reduced set of items used to derive transdiagnostic scores for 'Anxious-depression' and 'Compulsivity and Intrusive

Thought’ previously validated by Wise & Dolan (Wise & Dolan, 2020) (see Supplementary Table 7.3.2.4 for a list of clinical measures used). Anxious-depression was also assessed to serve a control measure to assess the specificity of any significant associations to repetitive behaviours in psychiatry. Questionnaires were presented in a randomised order across the five days of training to minimise demands on any single day. Means and standard deviations for mental health total scores and dimensional factor scores are presented in Supplementary Table 7.3.2.5 and Supplementary Figure 7.3.3.3.

Cannon Blast: A Gamified Goal-Directed Learning Task

Though not the primary focus, we measured model-based planning using a separate gamified task (‘Cannon Blast’) to compare findings from the habit-learning task with prior observations of reduced model-based planning in compulsivity. Cannon Blast is a previously validated, gamified adaptation of the two-step task commonly used to assess goal-directed or ‘model-based’ decision-making (Daw et al., 2011; Donegan et al., 2023). A detailed description of the task is available in the online supplement (Supplementary Methods and Analyses 7.3.1.3; Supplementary Figure 7.3.3.1), but in brief, the task measures the extent to which participants’ choices to stay or switch are influenced solely by reward history (‘model-free’) and/or by a combination of reward history and knowledge of the task structure (‘model-based’).

4.2.3 Study Design

The study received ethical approval from the School of Psychology Ethics Committee, Trinity College Dublin (Ethical Approval Code: SPREC072019-01). All participants provided electronic informed consent prior to participation. The study was conducted over a minimum of seven days via Neureka in the following order: self-reported demographics, the habit-learning task (CogQuest) and the goal-directed learning task (Cannon Blast). Participants could not complete more than one CogQuest assessment per day. If a participant missed an assessment (i.e., no activity in app for 24-hours), the missed session became available upon their next login, extending their study schedule. Participants were informed that inactivity for three consecutive days would result in their withdrawal from the study. Upon

completing all study parts, participants received an electronic debrief and were reimbursed.

4.2.4 Statistical Analysis

Data Exclusions

Behavioural task data. In accordance with the pre-registered analysis plan, participants were excluded from the habit-learning task if they failed to: (1) reach criterion in 200 trials in any of the criterion mappings ($n = 62$), (2) have at least 90% training trials ($n = 0$), (3) have at least 90% test trials in either test ($n = 14$), (4) have at least 50% valid test trials (i.e., on-time), in either test ($n = 22$) or (5) if they repeated the same response consecutively for more than 16 trials (i.e., 20%) per block in either test ($n = 26$). A breakdown of exclusions per training condition (minimal; extended) is provided in Supplementary Table 7.3.2.2. We applied the same exclusion criteria to the goal-directed learning task to that from Donegan et al. (Donegan et al., 2023). Participants were excluded if they: (1) had an incomplete session ($n = 65$) or (2) selected the same container more than 95% of the time ($n = 83$).

Self-report demographic and clinical questionnaire data. Participants were excluded from analyses related to individual differences if they did not report basic demographic information (age, gender and education) ($n = 1$) or if they did not complete all mental health questionnaires ($n = 49$) (Supplementary Table 7.3.2.2).

Careless/Inattentive responders. We identified careless/inattentive responders in two ways. First, we embedded an attentional catch question within the Barratt Impulsivity Scale. Participants were asked to respond to the statement “I completed at the 1917 Sumer Olympic Games” with “Rarely/Never”, “Occasionally”, “Often” and “Almost always/Always” as response options. Those who responded “Rarely/Never” were classified as attentive, while all other responses were classified as careless/inattentive responders ($n = 7$ (<1%)). Second, although not pre-registered, we compared age reported on Prolific with the age reported at registration in the Neureka app. Consistency across platforms was very high ($r = .98$, $p < .001$), but a small number of participants ($n = 26$ (2.6%)) had discrepancies in age that were implausible, i.e. greater than two years (Supplementary Figure

7.3.3.2). We examined the impact of careless/inattentive responders on behavioural and questionnaire data in Supplementary Methods and Analyses 7.3.1.4 and decided to exclude these participants on this basis.

Analysis

All of the main hypotheses and statistical analyses were defined prior to data collection and pre-registered at <https://osf.io/quwb3>. A full list of pre-registered hypotheses and their results are supplied in Supplementary Table 7.3.2.1. All analyses described were carried out as planned with the exception of a small number of deviations in the online supplement (Supplementary Methods and Analyses 7.3.1.1). All analyses were performed using R (2023.12.1+402).

Exploratory Factor Analysis. An exploratory factor analysis was carried out using the `fa()` function in the *psych* R package on all single-level item responses (110 items) with an oblique rotation in a subset of the sample ($n = 922$) with complete clinical data. Although our pre-registered plan specified the use of the Cattell-Nelson Gorsuch (CNG) scree test to determine the optimal factor solution, no clear inflection (or ‘elbow’) was observed. We therefore considered alternative factor selection methods, including the CNG and Very Simple Structure tests, and evaluated candidate models using multiple model fit indices including the Chi-square statistic and root mean square error of approximation (lower values indicate better fit), as well as the Tucker-Lewis Index and Comparative Fit Index (higher values indicate better fit). The final solution was selected based on both statistical fit and theoretical interpretability. For transparency, we report the pre-registered exploratory factor analysis and its results without this deviation in Supplementary Methods and Analyses 7.3.1.6, which did not change the main result.

4.3 Results

4.3.1 Replicating the overtraining effect on habit-expression

We first tested whether performance in training, indexed by response times and error rates, improved with repeated practice in a set of pre-registered analyses. Paired t-tests ($n = 847$) showed a significant reduction in response times, defined as the time between stimulus onset and the first response, from the initial ($M \pm SD = 773 \pm 130\text{ms}$) to the final ($M \pm SD = 734 \pm 118\text{ms}$) training session of the extended

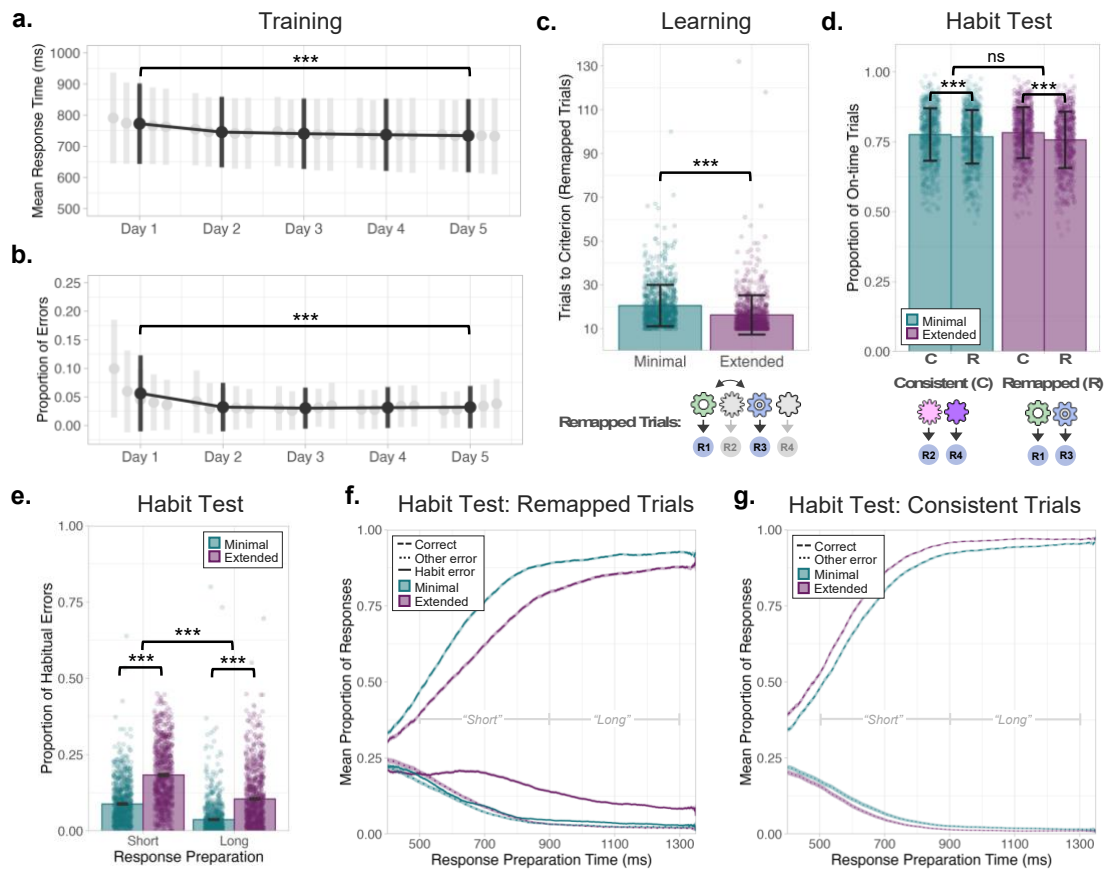


Figure 4.2. Replicating the over-training effect on habit expression. **a.** Mean response times defined as the time (in milliseconds) elapsed between the stimulus presenting on screen and participants first response as a function of training day. Black dot represents the mean response time per day. Grey dots represent the mean response times per block (five in total per day). Error bars reflect standard deviation. Response times significantly decreased from day 1 to day 5. **b.** Mean proportion of errors made as a function of training day. Black dot represents the mean error rate per day. Grey dots represent the mean error rate per block (five in total per day). Error bars reflect standard deviation. The proportion of errors significantly decreased from day 1 to day 5. **c.** Number of trials it took to reach the five-trial criterion for remapped trials following minimal and extended training. Error bars reflect standard deviation. Each dot represents a participant. It took less trials to reach criterion following extended compared to minimal training. **d.** Proportion of on-time trials per test (minimal; extended) and trial type (consistent (C); remapped (R)). Error bars reflect standard deviation. Each dot represents a participant. No difference in the proportion of on-time trials following minimal and extended training. Within each test, less on-time trials during trials where a remapping occurred. **e.** Proportion of habitual errors per response preparation (short; long) and training duration (minimal; extended). Error bars reflect standard deviation. Each dot represents a participant. Participants made more habitual errors following extended (purple) compared to minimal (teal), and this was moderated by response preparation with habitual errors greatest in the short (left) compared to long (right) preparation trials. **f.** Mean proportion of responses (dashed=correct, dotted=other error, solid=habitual error) for remapped trials across both tests (minimal=teal, extended=purple). Shaded area reflects standard error. **g.** Mean proportion of responses (dashed=correct, dotted=error) for consistent mapped trials in both forced responses tests (minimal=teal, extended=purple). Shaded area reflects standard error. * $p < .05$, ** $p < .01$, *** $p < .001$

condition, $t(846) = 23.26$, $p < .001$ (Figure 4.2a). This was supported by an exploratory linear mixed effects model on all response times across all training sessions, with a significant negative main effect of training day ($\beta = -.01$, $SE = .00$, $p < .001$). Similarly, the proportion of errors made also declined from the initial ($M \pm SD = .06 \pm 0.07$) to final ($M \pm SD = .03 \pm 0.04$) training session, $t(846) = 14.90$, $p < .001$ (Figure 4.2b). We then explored whether repeated practice led to differences in learning the stimulus-response associations after they were swapped ('remapped' trials) (Figure 4.2c). We found that participants required less trials to meet the five-trial criterion following extended training ($M \pm SD = 16.3 \pm 8.97$) compared to following minimal practice ($M \pm SD = 20.5 \pm 9.44$), $\beta = -4.26$, $SE = .39$, $p < .001$. This is non-surprising as participants were instructed to focus on learning the new mappings and were not under time constraints in this section.

In a pre-registered analysis, we then tested if habitual errors increased with extended training, and if this effect was more pronounced under time pressure. To manipulate this, participants were required to respond at the same time (1300ms) on each trial, while the stimulus onset in relation to this fixed response was varied. Responses made between 1100-1550ms were categorised as 'on-time', while responses outside of this window were excluded from analyses. The proportion of on-time trials did not significantly differ between tests conducted after minimal vs. extended trainings ($p = .304$, Figure 4.2d, Supplementary Table 7.3.2.6). As expected, within each test, participants were less likely to respond on-time during trials where a remapping had occurred (Figure 4.2d, Supplementary Table 7.3.2.6). For context, this reflected small mean differences in the number of on-time trials per trial mapping in each test (Minimal; Consistent, $M \pm SD = 156 \pm 21$: Remapped, $M \pm SD = 154 \pm 21$: Extended; Consistent, $M \pm SD = 157 \pm 20$: Remapped, $M \pm SD = 152 \pm 22$). Based on pilot data described in our pre-registration, we binned response preparation time into three windows: a pre-processing window where responding was mostly random (300-500ms), a short preparation (500-900ms) and a long preparation (900-1300ms) window. Trials during pre-processing were removed from the analyses. A mixed effects model tested the effects of training (minimal; extended), preparation time (short; long), and their interaction on the proportion of habitual errors made (Table 4.1, Figure 4.2e). Supporting our pre-registered hypothesis, we found a main effect of training ($\beta = .07$, $SE = .00$, $p < .001$), with a

Table 4.1

Linear regression of the influence of training, response preparation and their interaction on the number of habitual errors made

	Est	Std Error	t	p
Intercept	.03	.00	13.13	<.001
Training	.07	.00	20.45	<.001
Preparation	-.05	.00	-15.53	<.001
Training * Preparation	-.02	.00	-4.62	<.001

n = 847

Model: Proportion habitual errors ~ training * response preparation + (1 | id)

Training coded as factor with 2 levels: minimal, extended

Response preparation codes as factor with 2 levels: short, long

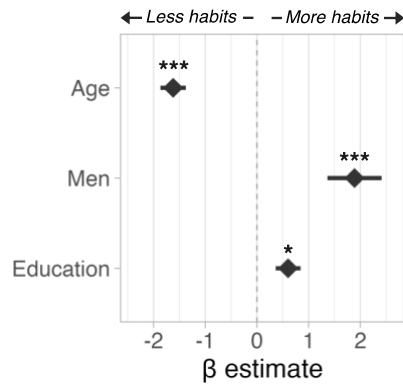
Est = Estimate, Std Error = Standard Error, t = test statistic, p = significance value

higher proportion of habitual errors made following extended compared to minimal training, a main effect of preparation type ($\beta = -.05$, $SE = .00$, $p < .001$), suggesting fewer habitual errors made during long preparation compared to short preparation trials. Crucially, there was a significant interaction ($\beta = -.02$, $SE = .00$, $p < .001$), with the greatest proportion of habitual errors made during short preparation trials following extensive training. The full time-series of responses (correct; habitual errors; other errors) are displayed in Figure 4.2f for remapped and Figure 4.2g for consistent mapped trials for both habit tests (minimal; extended). It shows how the difference between habitual errors made following extended training (purple solid line) peaks during short preparation times and steadily declines with time. In comparison, following minimal training (teal solid line) habitual errors decline rapidly at the same rate as other errors (teal dotted line). It also shows greater accuracy on remapped trials following minimal compared with extended training ($\beta = -.08$, $SE = .00$, $p < .001$), while the reverse was true for consistent trials, where a greater proportion of correct trials were observed following extended training ($\beta = .04$, $SE = .00$, $p < .001$) (Supplementary Table 7.3.2.7). We found good internal consistency for the proportion of habits made in the minimal ($r = .81$, $p < .001$) and in the extended test ($r = .83$, $p < .001$).

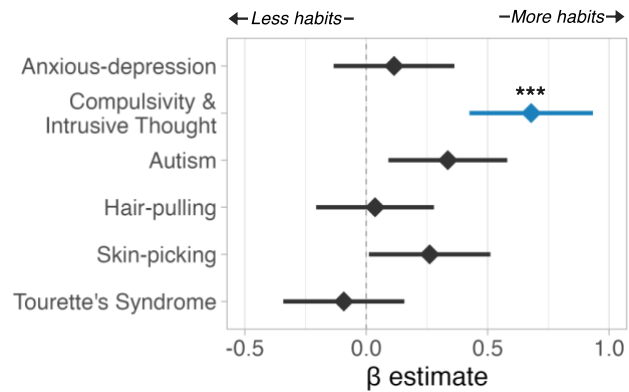
4.3.2 Individual differences in stimulus-response learning

We first characterised the demographic correlates of stimulus-response learning, which were pre-registered as covariates in subsequent clinical analyses. We predefined stimulus-response strength, or 'habit-learning', as number of habitual errors made during short preparation trials following extensive training, controlling

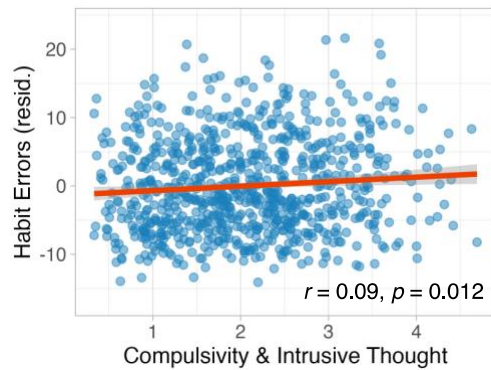
a. Demographics



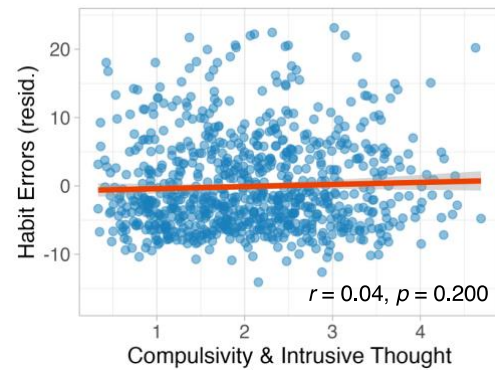
b. Clinical Associations



c. Short Preparation Trials



d. Long Preparation Trials



e. Cross-task Correlations

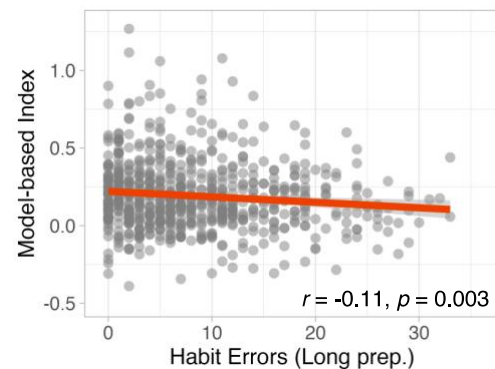
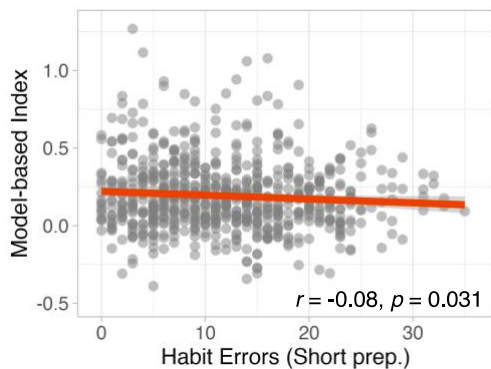


Figure 4.3. Individual and clinical differences in habit-learning. **a.** Decreased habit-learning was associated with older age, while increased habit-learning observed in men and those with more education. Dots reflect beta regression coefficient $\pm$ standard error. **b.** Increased habit-learning was associated with a transdiagnostic dimension of 'Compulsivity and Intrusive Thought' and not 'Anxious-depression', nor with individual clinical scores measuring autistic-like traits, hair-pulling, skin-picking, or Tourette's Syndrome. Dots reflect beta regression coefficient $\pm$ standard error from independent models controlling for age, gender, and education. **c.** Residual values of habitual errors made during short preparation time positively correlated with 'Compulsivity and Intrusive Thought' scores. **d.** Residual values of habitual errors made during long preparation trials had no significant relationship with 'Compulsivity and Intrusive Thought'. For **c-d**, residual values controlled for age, gender, education, and other errors. **e.** Cross-task correlations between participants' model-based indices and habit errors made at short- (left graph) and long-preparation trials (right

graph). For **c-e**, dots reflect participants and line reflects line of best fit. * $p < .05$, ** $p < .01$, *** $p < .001$

for other errors made in the same time frame. Among $n = 822$ with demographic and clinical data, we found habit-learning was reduced in older adults ($\beta = -1.62$, $SE = .25$, $p < .001$), but greater in men compared to women (cis-gender; $\beta = 1.89$, $SE = .52$, $p = .001$) and those with a higher education ($\beta = .60$, $SE = .24$, $p = .013$) (Figure 4.3a; Table 4.2).

We next tested for clinical associations using the same habit index as a dependent variable and each clinical total score as an independent regressor controlling for the covariates of age, gender, and education (Figure 4.3b; Table 4.2). We hypothesised that stimulus-response strength would be related to self-report severity of motor acts including tics, hair-pulling, skin-picking and autistic-like traits related to repetitive motor behaviours, rather than compulsivity and intrusive thought. Contrary to our pre-registered hypothesis, we found a positive association between habit-learning and compulsivity and intrusive thought ($\beta = .68$, $SE = .25$, $p = .008$), such that individuals high in this dimension had stronger stimulus-response strength (Figure 4.3c). Habit-learning was not associated with other forms of repetitive behaviours including skin-picking ($p = .296$), hair-pulling ($p = .880$), Tourette's Syndrome ($p = .711$), or autistic-like traits ($p = .170$) (see Supplementary Table 7.3.2.8 for associations with subscales). We found this effect was also specific to 'Compulsivity and Intrusive Thought' and not with a dimension of 'Anxious-Depression' ($p = .645$).

To further contextualise this finding, we carried out exploratory analyses to understand whether other aspects of performance also differed in those high in 'Compulsivity and Intrusive Thought'. First, we assessed whether the association between 'Compulsivity and Intrusive Thought' and habit learning was specific to short preparation trials. We found no significant association at long-preparation trials ($\beta = .35$, $SE = .26$, $p = .176$; Figure 4.3d), suggesting that our findings pertain to stimulus-response rather than goal-directed processes. For results with the other clinical measures see Supplementary Table 7.3.2.9. We then explored performance during training, prior to the remapping. Higher 'Compulsivity and Intrusive Thought' scores were associated with an overall greater proportion of errors during training ($\beta = .01$, $SE = .00$, $p < .001$), but also a greater reduction in errors over training

Table 4.2

Regression coefficients of the association between habit-learning and individual differences

	Est	Std Error	t	p
Demographics				
Intercept	9.57	.46	20.99	<.001
Age	-1.62	.25	-6.60	<.001
Gender ^a	1.89	.52	3.60	<.001
Education	.60	.24	2.50	.013
Transdiagnostic Dimensions^b				
Anxious-depression	.11	.25	.46	.645
Compulsivity and Intrusive Thought	.68	.25	2.67	.008
Independent Clinical Scores				
Autism	.34	.24	1.37	.170
Hairpulling	.04	.24	.15	.880
Skin-picking	.26	.25	1.04	.296
Tourette's Syndrome	-.09	.25	-.37	.711

n = 822

Demographics model: stimulus-response index ~ scale(age) + factor(gender) + scale(education) + number of other errors made

Independent clinical models: stimulus-response index ~ scale(i) + scale(age) + factor(gender) + scale(education) where each clinical measure is substituted for i

Stimulus-response index=number of habitual errors made during short preparation trials following extended training controlling for other errors made

^aGender coded as male, female & non-cisgender, with cisgender male as reference.

^bTransdiagnostic factor scores derived from an abbreviated battery of clinical questionnaires validated by Wise & Dolan, 2020

Shaded grey areas=significant results

Est = Estimate, Std Error = Standard Error, t = test statistic, p = significance value

($\beta = -.005$, $SE = .00$, $p = .015$). We found no association between 'Compulsivity and Intrusive Thought' and overall response times ($\beta = .000$, $SE = .00$, $p = .852$), though higher 'Compulsivity and Intrusive Thought' scores showed smaller response time improvements over training ($\beta = .002$, $SE = .00$, $p < .001$). Finally, we examined other errors (i.e. excluding habit errors) at short preparation times, which were controlled for in our pre-registered analysis. 'Compulsivity and Intrusive Thought' was associated with more non-habitual errors during short preparation trials ($\beta = .37$, $SE = .14$, $p = .008$), but not long preparation trials ($\beta = .09$, $SE = .09$, $p = .304$). However, the association between 'Compulsivity and Intrusive Thought' and other errors (on short preparation trials) was approximately half the size of its association with raw habitual errors measured in the same time window (Habit errors: $\beta = .68$ vs. Other errors: $\beta = .37$). In consistently mapped trials, we again found 'Compulsivity and Intrusive Thought' was associated with more errors at short

preparation ($\beta = .28$, $SE = .10$, $p = .003$) and not long preparation times ($\beta = .04$, $SE = .04$, $p = .373$). Results from these exploratory analyses as they pertain to demographic variables (i.e., age, gender, and education) are reported in Supplementary Methods and Analyses 7.3.1.5.

4.3.3 Individual Differences in Model-Based Planning

In addition to the main task, we collected data ($n = 790$) from a previously validated gamified task that assessed goal-directed learning, or ‘model-based’ planning, to understand how habit-learning and goal-directed control may differ in their demographic or clinical correlates. The gamified task demonstrated the typical behavioural patterns of model-based (Reward x Transition: $\beta = .19$, $SE = .02$, $p < .001$) and model-free behaviours (Reward: $\beta = .39$, $SE = .02$, $p < .001$) (Supplementary Table 7.3.2.10). We replicated the association between lowered model-based planning in older adults ($\beta = -.02$, $SE = .01$, $p = .029$), but we were surprisingly unable to replicate the typically observed associations (Donegan et al., 2023; Gillan, Kosinski, et al., 2016; Patzelt et al., 2019; Seow et al., 2021; Sharp et al., 2023; Sookud et al., 2024) between model-based planning and gender ($p = .973$), education ($p = .866$) or ‘Compulsivity and Intrusive Thought’ ($p = .702$) (Supplementary Table 7.3.2.11). Some differences in this implementation of the task compared to prior work may have affected its validity and so these null effects should be interpreted cautiously (see Discussion). That said, the task demonstrated convergent validity when measured against the primary outcome of the main habit-learning task. In $n = 705$ with data from both tasks, we found a negative correlation between model-based planning and habitual errors made at short preparation ($r = -.08$, $p = .031$) and long preparation trials ($r = -.11$, $p = .003$) (Figure 4.3e). However, a mixed effects model revealed a non-significant interaction between model-based planning and preparation type on habitual errors ($p = .379$), suggesting the relationship between model-based planning and habitual errors did not differ between preparation conditions.

4.3.4 Is habit-learning better explained by a transdiagnostic dimension with compulsivity dimension with additional habit behaviours?

Following our pre-registered hypothesis, we tested whether lower-order repetitive behaviours such as tics and bodily-focused repetitive behaviours would load

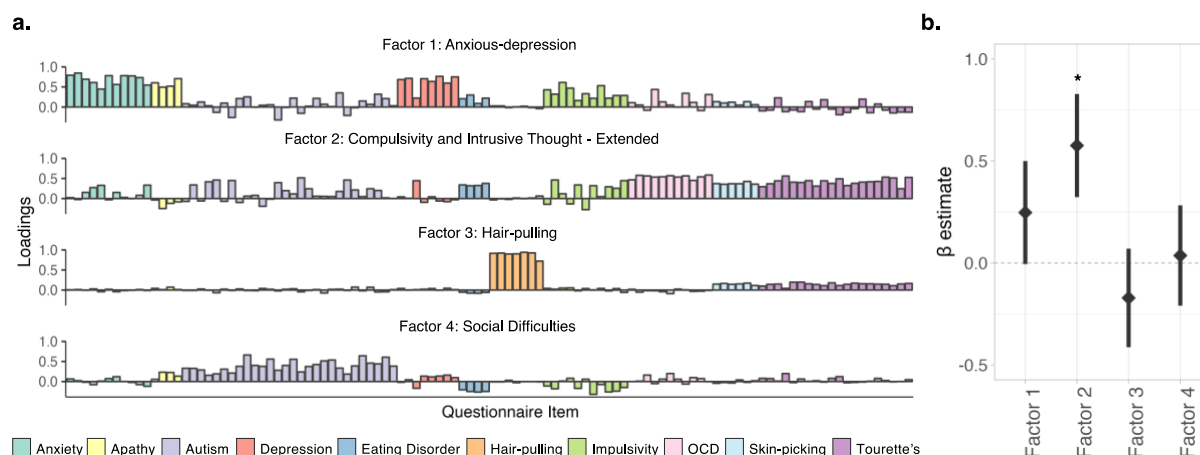


Figure 4.4. Exploratory factor analysis. **a.** Exploratory factor analysis on 110 questionnaire items suggested a 4-factor best explained the covariance of item-level responses. Factors were defined as: anxious-depression, compulsivity and intrusive thought extended, autism, and motoric acts. Colour-codes reflect the clinical measure. Individual item-level loadings for each factor are presented in Supplementary Table 7.3.2.13. **b.** Factor 2 that extended the ‘compulsivity and intrusive thought’ factor with the inclusion of items from Tourette’s, autism and skin-picking scales was positively associated with stimulus-response strength. Dots reflect beta regression estimates $\pm$ standard error from independent models controlling for age, gender and education. $*p < .05$.

together onto a dimension that is distinct from compulsions. The pre-registered method of factor selection, the CNG test, suggested a 3-factor structure, but the very simple structures test suggested a 4-factor structure. When we compared both candidate factor structures, all model-fit indices favoured the 4-factor solution (Supplementary Table 7.3.2.12), which upon inspection also offered greater clinical interpretability with respect to the separation of anxious-depression from autism (Figure 4.4a). We thus decided to forward the 4-factor solution but report the results of the 3-factor model in Supplementary Methods and Analyses 7.3.1.6 for comparison. We noted that 19 of the questionnaire items showed extreme skew (>1.5), posing a threat to the validity of the factor analysis (Ellis, 2017; Gorsuch, 1997). These items originated from the hair-pulling, skin-picking & Tourette’s Syndrome scales. We retained them because removing them would have precluded us from addressing the pre-registered hypothesis, but for this reason, the following results should be interpreted cautiously. Factor 2, which extended the ‘Compulsivity and Intrusive Thought’ dimension was positively associated with habit learning ($\beta = .58$, $SE = .25$, $p = .023$, Figure 4.4b). This factor had high loadings from OCD (mean loading = .54) and eating disorder (mean loading = .34), but also included loadings

Table 4.3

Regression coefficients of the association between stimulus-response learning and transdiagnostic factors from the exploratory factor analysis

	Est	Std Error	t	p
Independent models				
Factor 1 ('anxious-depression')	.25	.25	.98	.328
Factor 2 ('compulsivity and intrusive thought-extended')	.58	.25	2.28	.023
Factor 3 ('hair-pulling')	-.18	.24	-.71	.478
Factor 4 ('social difficulties')	.04	.25	.15	.882
Covariate models				
Factor 1 ('anxious-depression')	.03	.29	.10	.918
Factor 2 ('compulsivity and intrusive thought-extended')	.57	.28	2.05	.041
Factor 3 ('hair-pulling')	-.17	.24	-.70	.483
Factor 4 ('social difficulties')	-.04	.26	-.15	.879

n = 822

Independent model: number of habits ~ scale (i) + scale(age) + gender + scale(education) + other errors, where i is substituted for each factor

Covariate model: number of habits ~ scale (F1) + scale (F2) + scale (F3) + scale(F4) + scale(age) + gender + scale(education) + other errors

Shaded grey areas=significant results

Est = Estimate, Std Error = Standard Error, t = test statistic, p = significance value

from the Tourette's Syndrome (mean loading = .42) and skin-picking scales (mean loading = .38), and items from the numbers/patterns autism subscale (mean loading = .46, e.g., "I prefer to do things the same way over and over again"). Notably, this effect was weaker than the association with the original 'Compulsivity and Intrusive Thought' ($\beta = .68$, $SE = .25$, $p = .008$). This is in line with the null findings for habit-learning differences with respect to the Tourette's, skin-picking, and autism scales reported earlier. There was no association between habit learning and Factor 1 ('Anxious-Depression': $\beta = .25$, $SE = .25$, $p = .328$), comprising anxiety symptoms (mean loading = .69), depression (mean loading = .64), and apathy (mean loading = .58). There was also no association to Factor 3, which was dominated by (rarely endorsed) items from the hair pulling scale ($\beta = -.18$, $SE = .24$, $p = .478$), or Factor 4 ($\beta = .04$, $SE = .25$, $p = .882$), consisting primarily of loadings from the autism scale, with highest loadings from the social skills subscale (mean loading = .54), e.g., "I find social situations easy" and "I enjoy meeting new people". When all four factors were entered into a covariate model (controlling for age, gender and education), the relationship between Factor 2 and habit-learning remained significant ($\beta = .57$, SE

= .28, $p = .041$). The item loadings from the exploratory factor analysis are presented in Supplementary Table 7.3.2.13.

4.4 Discussion

In this study, we developed a gamified smartphone task, based on Hardwick et al. (2019), and leveraged a large crowd-sourced sample to examine how individual differences in psychiatric dimensions relate to the propensity to form habits. We first validated the task by replicating key effects of training duration and preparation time on habit expression. In a within-subject design, we found that, as expected, individuals committed more habitual errors following extensive training than compared to minimal training. Crucially, this effect was most pronounced when response preparation time was short, which theoretically limits the impact of slower goal-directed processes on choice and reveals more fully the strength of stimulus-response associations (Du & Haith, 2025; Hardwick et al., 2019).

In this same window of short preparation time, we found that individuals with self-reported greater compulsive tendencies expressed more habitual errors, suggesting they had developed stronger stimulus-response associations. This finding substantially builds on current theoretical accounts of compulsivity which until now have proposed that deficits in goal-directed control cause patients to behave habitually (Gillan, Robbins, et al., 2016). Such theories have remained silent about whether stimulus-response learning is enhanced in disorders of compulsivity. This, in part, has been due to challenges in isolating the influence of stimulus-response habit associations on behaviour in experimental paradigms and the apparent sufficiency of the goal-directed account to explain habitual slips in mainstay tasks (Friedel et al., 2014; Gillan, Otto, et al., 2015; Sjoerds et al., 2016). Our results suggest that, in addition to reductions in goal-directed control, compulsive individuals may also form stronger stimulus-response associations, and the combination of these two factors may conspire to prioritise habitual behaviours across various contexts.

Most of the prior cognitive literature on compulsivity has focused on the reductions in model-based planning (Donegan et al., 2023; Gillan et al., 2020; Gillan, Kosinski, et al., 2016; Seow et al., 2021; Sharp et al., 2023; Sookud et al., 2024) and broader

deficits in executive functioning (Snyder, Kaiser, et al., 2015). An increased tendency to form habits, in contrast, could be construed as an adaptive function. Habits allow us to reliably repeat well-practised actions in familiar contexts, freeing up cognitive resources for more complex and sophisticated tasks such as multi-tasking or strategic decision-making (Buabang et al., 2024). In this light, the finding of enhanced habit formation in compulsivity could align with evolutionary perspectives on psychiatric disorders (Durisko et al., 2016). Specifically, it is unclear why maladaptive traits associated with psychiatric conditions are preserved, even though they are associated with poorer functioning and distress. One possibility is that certain aspects of cognition associated with these conditions confers benefits that perhaps only become maladaptive in the extreme or when experienced in tandem with other traits (Durisko et al., 2016; Krupp et al., 2013; Leedom & Almas, 2012). Stimulus-response learning was not only enhanced in those high in ‘Compulsivity and Intrusive Thought’, but also in those with higher educational attainment, men, and younger adults, all of which are often associated with enhanced executive functioning ((Fisk & Sharp, 2004; Gaillard et al., 2021; Huizinga et al., 2018), dependent on the aspect of executive functioning for gender (Grissom & Reyes, 2019)). Another, not mutually exclusive, possibility is that enhanced stimulus-response habit learning may be a form of compensation for difficulties in goal-directed learning. Indeed, problems in goal-directed learning might manifest in a higher overall error rate during learning, which we observed in compulsivity. Outside of the present study, this might reconcile the findings of prior work on procedural and implicit learning that show comparable or deficient performance in individuals with compulsive conditions (Banca et al., 2024; Deckersbach et al., 2002; Soreff et al., 2021).

Contrary to our expectations, lower-order repetitive behaviours such as tics, skin-picking and hair-pulling were not significantly linked to habit-learning. This suggests that these repetitive behaviours, which share a superficial similarity to compulsions, may arise through distinct underlying cognitive mechanisms. That said, our exploratory factor analysis revealed a tendency for compulsions (e.g., OCD, eating disorder) to load with tics, skin-picking, and motor features in autism, extending a previously validated dimension of ‘Compulsivity and Intrusive Thought’ (Gillan, Kosinski, et al., 2016; Wise & Dolan, 2020). However, the addition of these items to

the ‘Compulsivity and Intrusive Thought’ factor nominally reduced the association between habit learning and the original construct. This findings illustrate a limitation of the computational factor modelling approach, which has historically shown improved correspondence between cognitive markers and transdiagnostic markers derived from factor analysis compared to traditional questionnaires (Fox, Teckentrup, et al., 2024). Here, we see a case where symptoms converge on the same factor, but may not arise through the same mechanism. One explanation for this might be a reduction in factor analysis validity due to the skewed distributions in the self-reported questionnaires, particularly those assessing tics, skin-picking and hair-pulling (Ellis, 2017; Gorsuch, 1997). The low endorsement rates, coupled with high internal consistency and extreme skew, may have inflated covariances in the factor analysis (Delucchi & Bostrom, 2004; Reise & Waller, 2009; Reise et al., 2000). Moreover, this restriction in the variation of scores may have limited our ability to identify associations with the clinical measures (Goodwin & Leech, 2006). Nevertheless, our findings are consistent with prior research that identified ‘grooming behaviours’ including hair-pulling and skin-picking distinct from ‘compulsions’ (Snorrason et al., 2021; Snorrason et al., 2022). Future work that includes a greater spread of symptom endorsement will be necessary to confirm and refine these findings. As with all studies focused on unselected samples, an extension to patient populations would be needed to confirm if these results have clinical relevance.

In our sample, rates of observed careless/inattentive responding were very low. This might be due to the involved nature of our study, which required the installation of an app on a personal device and seven days of participation. But, at least in the limited way that we could assess it, we found a small tendency for careless/inattentive responders to amplify the observed clinical-cognitive associations. Future work must pay careful attention to this issue and, in addition to quality control, seek convergence of findings across different samples, paid and unpaid, patient and general population. Our study failed to replicate previous associations between model-based planning and compulsivity, education, and gender which may be due to differences in the implementation of this task. In the present study, the instructions of the task were presented in a separate section of the app, and we cannot confirm whether this was accessed by the participant.

Additionally, this version of the task employed ten different reward probability structures, in contrast to the two (Drift 1; Drift 2, Supplementary Figure 7.3.3.1) used in the original validation which was demonstrated to affect the measurement of model-based planning and its correlates (Donegan et al., 2023). These changes may have adversely affected the tasks external validity.

4.4.1 Conclusions

In this pre-registered study using a well-validated task, we found novel evidence that individuals with high levels of compulsivity exhibited enhanced stimulus-response habit learning. The ability to form habits is well-understood to be a double-edged sword; habits allow us to perform tasks on auto-pilot, facilitating multi-tasking and the development of complex repertoires of behaviour. But with this benefit comes fundamental inflexibility and as our data suggests, is related to the development of real-world compulsive behaviours. Future work should explore the interplay between the goal-directed and habit-learning system, with the goal of leveraging this intersection to promote the flexible control of habits.

Chapter 5: Discussion

5.1 Summary

The dual-systems framework suggests that habits can emerge either from excessive stimulus-response learning or failures in goal-directed control (Dolan & Dayan, 2013; Gillan, Robbins, et al., 2016). To date, research has consistently shown that compulsive behaviours observed in OCD, disordered eating, and addiction manifest from reductions in goal-directed control that shift towards an overreliance on the stimulus-response system (Gillan, Robbins, et al., 2016). This well-established finding has been observed in both general population samples (Gillan, Kosinski, et al., 2016; Patzelt et al., 2019; Seow et al., 2021; Sookud et al., 2024) and clinical populations (Ersche et al., 2021; Foerde et al., 2021; Gillan, Apergis-Schoute, et al., 2015; Gillan et al., 2020; Gillan, Morein-Zamir, Urcelay, et al., 2014; Sjoerds et al., 2013; Voon et al., 2015), and is specifically associated with individual differences in the dimension of compulsivity and intrusive thought (Gillan, Kosinski, et al., 2016; Patzelt et al., 2019; Seow et al., 2021). Nonetheless, if we are to derive meaningful clinical insights then we must further develop the theory between model-based planning and compulsivity by moving beyond conventional cross-sectional designs. At present it is unclear whether these differences in goal-directed control are state-dependent, and resolve with treatment, or are they better understood as trait-like risk markers for compulsivity. Furthermore, owing to methodological shortcomings, the other side of this framework remains untested. Are compulsive behaviours associated with an enhanced ability to form habits? Are compulsions similar in this regard to lower-order repetitive behaviours with a strong motor element, such as tics and bodily focused repetitive behaviours? To answer these outstanding questions, we require methods that support scalable and longitudinal within-person testing, such as smartphone- and online-based methods (Brown et al., 2014; Gillan & Daw, 2016; Gillan & Rutledge, 2021). Across three independent experimental chapters, this thesis aimed to develop these methodologies for the specific purpose of advancing our understanding of the cognitive mechanisms underlying compulsivity.

The first experimental chapter (Chapter 2) aimed to develop and validate a gamified task to scale-up and provide a repeatable measure of model-based planning. We found a moderate positive correlation between model-based planning derived from the traditional and gamified task, suggesting both tasks measured the same construct, albeit partially. This outcome is perhaps expected, given the substantial modifications made to the task design. These include the removal of the second stage choice, the introduction of new reward rate drift sequences, having a visible transition structure on-screen, varying levels of difficulty, and most notably, the task becoming secondary to the performance of a diamond shooting game as a by-product of gamification. Despite these changes, the task demonstrated good psychometric properties, with reliable estimates within and across sessions, as illustrated by the moderate-to-good split-half and test re-test reliability estimates comparable to the traditional task (Brown et al., 2020; Shahar et al., 2019). Using data from this task, we replicated previously established correlates of model-based planning including age (Eppinger et al., 2013; Nassar et al., 2016), gender (Gillan, Kosinski, et al., 2016), and education — which has shown similar relationships IQ and processing speed in prior work (Gillan, Kosinski, et al., 2016; Schad et al., 2014). Importantly, we replicated the association between model-based planning and compulsivity and intrusive thought (Gillan, Kosinski, et al., 2016; Patzelt et al., 2019; Seow et al., 2021; Sharp et al., 2023). Together, these findings demonstrate the feasibility and utility of hiding cognitive tasks within smartphone games to capture individual differences in model-based planning outside of traditional laboratory settings.

We then used these associations, focussing specifically on the relationship with compulsivity and intrusive thought, as ‘ground truth’ to assess if we could improve the sensitivity of our task through modifications to the task structure. Previous work has shown that alterations to the tasks structure, such as dual-task demands (Otto, Gershman, et al., 2013), reward probabilities (Feher da Silva & Hare, 2018), incentives (Kool et al., 2016; Patzelt et al., 2019) or instruction (P. Castro-Rodrigues et al., 2022; Feher da Silva & Hare, 2020) affects the measurement model-based planning. However, only one study tested the impact on its external validity with compulsivity and intrusive thought, finding no affect (Patzelt et al., 2019). In a series of within-subject (difficulty) and between-subject (transition ratio and reward drifts)

manipulations, we found that structures characterised by greater uncertainty (i.e., less deterministic transitions and low distinguishability between rewarding outcomes), yielded the strongest signal between model-based planning and compulsivity and intrusive thought. These findings align with prior work (Fradkin et al., 2020; Sookud et al., 2024). In subsequent work, Sookud et al. (2024) modified our gamified task used in Chapter 2 to isolate state-action learning by masking the coloured balls inside the container, requiring participants to learn these probabilities and instead displayed the reward probabilities. Using a computational model on their modified task, they found that compulsive individuals learned these state-action transitions more slowly. Additionally, they also found the association between compulsivity and intrusive thought and model-based planning was mediated by uncertainty about the task structure. These data suggest that compulsive individuals may be particularly sensitive to environmental uncertainty, reducing the perceived utility of engaging in computationally costly model-based control when the cost-benefit trade-off is ambiguous. Importantly, these results also highlighted specific conditions under which the expected association between model-based planning and compulsivity failed to emerge. This variability raises concerns regarding the gamified tasks external validity to detect well-established correlates of model-based planning. These concerns were reinforced in Chapter 4, where the sensitivity of the gamified task to capture individual differences was again called into question.

A long-standing question in the field concerns whether these impairments reflect alterations in learning the structure of the environment or in executing goal-directed strategies. In support of the former, compulsivity has been linked to difficulties in learning the task structure (P. Castro-Rodrigues et al., 2022; Seow et al., 2021; Sharp et al., 2023; Sookud et al., 2024). For example, Sharp et al. (2023) found that compulsivity was associated with altered learning rates for state-action transitions, both when transitions were stable and when they changed over time. Seow et al. (2021) found weaker transition structure knowledge in those high in compulsivity and intrusive thought. This was behaviourally indexed by smaller reaction time differences between rare and common transitions, a finding they replicated in a larger independent dataset (Gillan, Kosinski, et al., 2016). This behavioural effect was complimented by EEG recordings showing reduced alpha responses to unlikely transitions. Furthermore, Castro-Rodrigues et al. (2022) found the largest group

differences in model-based planning between OCD patients and controls when instructions were removed and the task had to be learned directly from experience. We extended this theory by providing evidence of alternative; that compulsive individuals also display reductions in executing the model. We simplified the task by displaying the transition ratio structure on-screen throughout, eliminating the need to learn it unlike the traditional task. Still, we found compulsive individuals were less likely to employ model-based strategies. Our findings suggest differences in model-based planning in compulsive individuals may arise from deficits in executing, rather than just learning, goal-directed strategies.

It has been suggested that increasing the amount of data collected per participant, through improving the reliability of the measure, can improve the strength of the association with individual differences. Our findings suggest otherwise. While collecting more data per participant improved split-half reliability rates, in line with prior work this increase was non-linear and plateaued (at approximately 125 trials), after which further gains were negligible (Cooper et al., 2017b; Price et al., 2015b). Importantly, this increased reliability did not translate into improvements in external validity. We found the association between compulsivity and intrusive thought and model-based planning could be detected within as few as the first 25 trials, and the strength of this association did not significantly improve with the addition of 275 more trials per participant. However, this result must be interpreted in consideration with the reliability results, where striking a balance between task length and psychometric properties is crucial to ensure that one does not ultimately compromise the other (Rouder & Haaf, 2019; Zech et al., 2023).

In the second experimental chapter (Chapter 3), we employed a fully online design that followed a large group of treatment-seeking individuals in the first four weeks of either internet-based cognitive behavioural therapy (iCBT) or antidepressant medication, alongside a control group. Research has shown pronounced impairments in cognitive processes such as inhibition, shifting, and updating, mechanisms critical for goal-directed control in individuals with depression (Snyder, Miyake, & Hankin, 2015). Despite this, many studies have reported no association between self-reported depression symptoms and model-based planning, a finding reinforced in Chapter 2 (Donegan et al., 2023; Gillan, Kosinski, et al., 2016; Seow

et al., 2021). However, these studies have primarily relied on general population samples. We examined if differences in model-based planning were related to depression, within large treatment-seeking samples with varying levels of depression severity. Further, to our knowledge, this was the first study to investigate if model-based planning improved following standard mental health treatments. Importantly, both treatment modalities were effective; self-report anxious-depression factor scores declined from baseline to the four-week follow-up. However, this reduction did not translate to changes in model-based planning. Additionally, within-person changes in anxious-depression symptoms were neither associated with baseline estimates nor within-person changes in model-based planning. These findings challenge recent hypotheses suggesting that antidepressants and iCBT could improve cognition by alleviating depressive symptoms that interfere with cognitive function. Instead, our data align with a large-scale randomised controlled trial that found no cognitive improvements following antidepressant medication, extending these null effects to a large iCBT treatment-seeking sample. Moreover, in the larger iCBT sample powered to detect individual differences, we found no association between model-based planning and anxious-depression. This is consistent with studies in general population samples and in one study with a small patient sample ($N = 23$) (Knolle et al., 2023). Collectively, these data provide convergent evidence that model-based planning is neither influenced by a range of standard clinical treatments nor with individual differences in clinical depression.

We also collected self-report data on compulsivity and intrusive thought to explore if within-subject changes in this dimension, previously identified to track differences in model-based planning cross-sectionally, were associated with improvements in model-based planning, supporting a 'state-distress' model (Rachman, 1998). Alternatively, if impairments persisted despite symptom alleviation, this would suggest a trait-like marker for compulsivity. Both treatment groups demonstrated reductions in compulsivity and intrusive thought scores following treatment, reinforcing the transdiagnostic widespread benefit of standard treatments (Lee et al., 2021). Again, we found these clinical reductions were not accompanied by improvements in model-based planning, nor were within-person changes in compulsivity and intrusive thought correlated with changes in model-based planning

across all groups. This aligns with prior work showing that model-based planning did not improve in OCD patients following exposure response prevention therapy, despite significant symptom improvement (Wheaton et al., 2019). However, this study lacked a control group for comparison, and its sample size ($N=18$) limited the strength of its conclusion. A similar pattern was observed in a double-blinded trial of OCD patients who received theta burst stimulation; while clinical symptoms improved, model-based planning remained unchanged (Brown et al., 2022). Our findings provide evidence that current state-levels of distress are not a likely explanation for the reductions in model-based planning consistently reported in compulsivity. Instead, these differences appear stable and trait-like.

Current theoretical accounts of compulsive behaviours proposing these behaviours arise due to reductions in goal-directed control which cause an over-expression of stimulus-response habits. However, research is yet to address the other side of this theory. That is, the ability to form these stimulus-response habits, is this also altered in compulsivity? The third experimental chapter (Chapter 4) aimed to address this by developing and validating a gamified habit-learning task and delivered it to a crowd-sourced sample to explore individual and clinical differences in the propensity to form habits.

Our gamified task, based on Hardwick et al. (2019), demonstrated validity to measure stimulus-response habits passing pre-registered criteria. In a within-person counterbalanced design, we replicated the over-training effects on stimulus-response habits, showing that participants made more habits following extended compared to minimal training. We also replicated the time-dependent nature of these effects with more stimulus-response habits made during trials with limited time to prepare compared with longer preparation trials. Our findings add to the growing number of replication attempts using this paradigm as a sensitive marker of stimulus-response associative strength (Du & Haith, 2025; Hardwick et al., 2019), which could be delivered in a scalable way through smartphones.

We then used this index of habit-learning to test for individual differences across demographic variables of age, gender and education, along with aspects of mental health that involved repetitive behaviours including autism, hair-pulling, skin-picking,

Tourette's Syndrome, and transdiagnostic dimensions of compulsivity and intrusive thought and anxious-depression. We found reductions in stimulus-response habit-learning was associated with older age and in females compared to males. Greater stimulus-response learning was found in those who attained higher levels of education. Interestingly, these associations are reported, in the same direction, to differences in goal-directed control. That is, older adults and females exhibit reduced goal-directed control, while those with higher education demonstrate stronger goal-directed control (Donegan et al., 2023; Eppinger et al., 2013; Gillan, Kosinski, et al., 2016). Crucially, we found that individuals who reported greater levels of compulsivity and intrusive thought expressed more habitual errors in the short preparation trials, suggesting they had developed stronger stimulus-response associations. Against our expectation, stimulus-response habit learning was not associated with other aspects of repetitive behaviours including autism, hair-pulling, skin-picking or Tourette's Syndrome. Our data adds an important new dimension to current theoretical models of compulsivity in that these behaviours may arise from not only failures in goal-directed control but also through an increased propensity to form stimulus-response associations (Gillan, Robbins, et al., 2016).

It is unknown if analogous forms of repetitive behaviours such as tics and bodily-focused repetitive behaviours can also be modelled under the same habit-hypothesis framework. There is some preliminary support that areas involved in the gradual build-up of stimulus-response links (e.g., putamen and sensorimotor cortices) are activated during tic generation (Bohlhalter et al., 2006), while areas subserving goal-directed control (e.g., caudate and prefrontal regions) are activated during tic suppression (Leckman, 2002). Behavioural and neural evidence for the role of habit in bodily-focused repetitive behaviours is scarce. These behaviours fall under the category of obsessive-compulsive related disorders (OCDs) but recent efforts have questioned their labelling as OCDs and suggest these may align better with repetitive behaviours with a strong motor element (e.g., tics) (Houghton et al., 2017; Lamothe et al., 2019). We addressed these questions by exploring whether these items extended the compulsivity and intrusive thought dimension or if they mapped onto a novel motoric dimension of mental health. Our data suggested both. The compulsivity and intrusive thought dimension was extended to incorporate elements of obsessive-compulsive tic-related behaviours, skin-picking and autistic-

like behaviours related to numbers and patterns to OCD and eating disorder items. However, we found that this did not improve the strength of the association between the factor of compulsivity and intrusive thought without these items and habit-learning. This runs contrary to recent work suggesting transdiagnostic dimension through factor analysis tend to show stronger correlates than constituent elements (Fox, Teckentrup, et al., 2024). Additionally, we observed a motoric-dimension of mental health made up of items of hair-pulling, skin-picking and tic behaviours with strong motor elements e.g., grunting and jerking. This aligned with prior factor analytic work reported in Snorrason et al. (2021, 2022) where bodily focused repetitive behaviours such as hair-pulling and skin-picking loaded separately into a factor constituting 'grooming behaviour' that were distinct from its archetypical OCD behaviours. Contrary to our expectations, this was not associated with habit learning. This result was non-surprising given the null results across independent clinical measures and could in part be explained by the extreme low endorsement of these behaviours in our general population sample which would need confirming in a more varied sample.

Finally, in a subset of participants with data from both the habit-learning task CogQuest and the goal-directed learning task Cannon Blast (used in Chapter 2), we observed evidence of model-based planning at the group-level and a negative association between model-based planning and habit errors supporting the tasks' convergent validity. However, we only observed individual differences between model-based planning and age and failed to replicate individual differences with gender, education, or compulsivity. While changes to the task design implemented in Chapter 2, some of which we identified to impact the signal strength (i.e., reward drifts), may have reduced the sensitivity of the gamified task, these findings again raise concerns about its external validity, which are considered further in Chapter 5.3.

5.2 Implications

5.2.1 Treatment of Compulsive Disorders

Understanding the mechanisms underlying compulsive behaviours may lead to the discovery of novel therapeutic targets and enhance our understanding of which treatments are most effective and for whom (Fineberg et al., 2018). Currently,

approximately 40% of patients with OCD fail to respond to standard therapies, and treatment resistance is common (Castle et al., 2015; Foa et al., 1983). Established treatments include selective serotonin reuptake inhibitors (SSRIs) and cognitive behavioural therapy (CBT) involving exposure response prevention therapy. However, it remains unclear whether and how pharmacological interventions and CBT address cognitive impairments associated with compulsivity. Our findings suggest that these treatments do not exert their effects by targeting model-based planning, a cognitive capacity that protects against the formation of habits (Gillan, Otto, et al., 2015). Despite improvements in compulsive symptoms, model-based planning remained unchanged following the initial four weeks of internet-based CBT (without exposure response prevention therapy) and SSRI treatment. Notably, our study primarily including patients diagnosed with depression and anxiety rather than OCD, and these interventions were not specifically tailored to treat compulsive behaviours (see section 5.3 Limitations). Nevertheless, these findings align with emerging evidence suggesting that model-based planning is a stable, trait-like characteristic that confers vulnerability to compulsivity. This suggests that standard treatments may not directly target this cognitive mechanism underlying the development and/or maintenance of compulsive behaviours, and potentially explaining suboptimal treatment response rates and relapse.

Exposure response prevention (ERP) therapy is one of the most effective behavioural treatments for OCD (Abramowitz et al., 2009). It has been shown to significantly reduce OCD severity compared to CBT without ERP and to other forms of cognitive therapy (Olatunji, Rosenfield, et al., 2013). Additionally, ERP has demonstrated large pre- to post treatment effect sizes (Abramowitz et al., 2003; Eddy et al., 2004; Olatunji, Davis, et al., 2013). Despite these successes, only half who complete ERP achieve remission, and residual symptoms often persist (Fineberg & Robbins, 2021). These success rates may be inflated, as they typically exclude those who did not complete the full course of treatment. Many patients find ERP intolerable, with 20-30% discontinuing treatment prematurely (Abramowitz, 2006; Franklin et al., 2003). ERP involves patients confronting situations that trigger their compulsions while preventing the corresponding behavioural response (Fineberg & Robbins, 2021). Through repeated exposure and refrainment, the distress associated with the trigger declines, reducing the urge to neutralise the

distress through performing the compulsion (Foa et al., 2012). Theoretically, ERP works by building a new association between the trigger and the absence of the feared outcome, gradually strengthening these safe associations while weakening the distress-related associations (Apergis-Schoute et al., 2017; Fineberg & Robbins, 2021; Sulkowski et al., 2013). Perhaps the reason this method is effective in some patient can be attributed to the exaggerated habit-forming biases in compulsivity that we observed in Chapter 4. However, ERP also requires patients to inhibit prepotent responses, a process that relies on sufficient goal-directed control. Theoretical models of compulsivity propose reductions in such control, a notion corroborated by our findings in Chapter 2 and 3. Given these impairments, patients may struggle to inhibit previous associations in favour of newly formed associations, leaving them susceptible to relapse (Button et al., 2012). New methods are needed to extinguish these maladaptive stimulus-response habits in a manner that is less dependent on goal-directed control mechanisms that may be compromised in compulsive disorders.

Our findings suggesting that compulsive individuals exhibit not only differences in goal-directed control but also an accelerated propensity to form stimulus-response associations opens new avenues to disrupt these links between triggering stimuli and compulsive responses. These associations can be broken down through various routes such as weakening the stimulus-response link, avoiding triggering stimuli, or forming competing stimulus-response associations (Buabang et al., 2024). Avoiding contexts that contain triggering stimuli has shown some success in reducing smoking behaviours. For example, large-scale public health initiatives that banned smoking in public spaces reduced the number of smoking-related cues, leading to decrease smoking behaviours (Feliu et al., 2019). However, these avoidance strategies are not always feasible, particularly when the triggering stimuli are bound to the home environment (e.g., compulsions related to oven checking or light switches in OCD). Like ERP, cue discontinuation methods are susceptible to relapse, as the strength of the maladaptive habit remains.

A potentially more effective strategy involves habit substitution, wherein a new competing stimulus-response association is formed (Gardner et al., 2021). Based on this principle, habit reversal therapy is an effective behavioural treatment for tics

and later adapted for bodily focused repetitive behaviours (Azrin & Nunn, 1973; Conelea et al., 2010; Sulkowski et al., 2013). Habit reversal therapy involves training patients to recognise premonitory urges and physical sensations prior to the behavioural response. Unlike ERP, which requires patients to inhibit their response, habit reversal therapy trains an alternative competing response (Sulkowski et al., 2013). For example, a patient with a shoulder jerk tic may be trained to hold their arm straight by their side instead. This treatment has been widely successful in the treatment of tics (Bate et al., 2011; Conelea et al., 2010), hair-pulling (Bate et al., 2011; Moritz et al., 2024; Rahman et al., 2017; Slikboer et al., 2017) and more recently extending to skin-picking (Fineberg et al., 2024; Lochner et al., 2017; Moritz et al., 2024). Given our findings that compulsive individuals exhibit exaggerated stimulus-response learning, it is reasonable to speculate that habit reversal therapy may be effective for disorders of compulsion. One study investigated the effects of habit reversal therapy and OCD in patients with trichotillomania (Moritz & Rufer, 2011). They found a variant of habit reversal therapy correlated with improved of OCD symptoms, hinting of a possible therapeutic role.

Beyond its increased tolerability compared to ERP, habit reversal therapy may offer a more mechanistically robust approach to treatment. A key distinction between these two similar approaches lies in their respective learning mechanisms; while ERP establishes a new association between a stimulus and the absence of a response, habit reversal therapy fosters a competing stimulus-response link. Research suggests that stimulus-response associations are more robustly formed when the stimulus is linked to an active behavioural response rather than mere response inhibition (Du & Haith, 2025). Empirical studies have demonstrated inconsistencies in revealing habits using different paradigms that target these distinct learning mechanisms. For example, studies employing the remapping approach, leveraged in Chapter 4, where stimulus-response associations undergo a reversal, consistently reveal habits, that increase as a function of training (Du et al., 2022; Hardwick et al., 2019). By contrast, withholding paradigms, where participants are instructed to suppress responses have demonstrated mixed findings and don't become stronger with extensive practice (de Wit et al., 2018; Pool et al., 2021; Watson, Gladwin, et al., 2022). These differences may be explained by methodological differences where participants have to actively learn remapping's

via trial and error compared to instructions in withhold conditions, trial length where remapping tends to involve more training trials, or time pressure in the test phase where participants in the remapping have limited time to respond compared with withholding condition that impose no time limits. Alternatively, these differences may reflect distinct cognitive processes where remapping's rely on stimulus-response control, whereas withholding depends on goal-directed control. Controlling for methodological differences, Du & Haith (2025) compared these approaches within the same study design. Consistent with prior work, in the remapping condition stimulus-response habits were revealed when the time to respond was limited and declined as time increased. In contrast, in the withhold condition, stimulus-response habits declined from baseline levels as response preparation time increased, suggesting participants in this condition could flexibly adapt to withholding their response quickly, mimicking goal-directed control. Importantly, this was not due to differences in task complexity (i.e., more difficulty to relearn associations than withhold), as results did not change when a simplified remapping was used. These differences could explain the limitations of ERP, as response inhibition relies on cognitive control demands, which are exacerbated in clinical conditions with limited or reduced capacity such as compulsivity.

Breaking compulsive habits remains a significant clinical challenge due to their inherent rigidity, strengthened through many repetitions by the time individual get to treatment. Current treatments may not directly target the underlying mechanisms driving compulsive behaviours (e.g., model-based planning), be too aversive (e.g., ERP) or impractical (e.g., home-based cues). Future work needs to explore whether exaggerated habit learning is a premorbid vulnerability factor or a consequence of diminished goal-directed control, and how these systems interact over the course of compulsive disorders. Longitudinal studies are necessary to identify at-risk individuals at the earliest possible opportunity, allowing for the implementation of interventions aimed at strengthening goal-directed control or providing psychoeducation on habit-breaking techniques to equip individuals with skills before compulsions become entrenched.

5.2.2 Smartphones & Online Methods Facilitate the Collection of Repeated Within-person Assessments

Most research studies on individual-differences have adopted a 'nomothetic' approach, focusing on variations in cognitive task performance between individuals at one moment in time (Beck & Jackson, 2021). This approach makes general predictions about the population by examining inter-individual variation between or across people (Beltz et al., 2016; Molenaar, 2004). An alternative approach to studying individual differences is through an idiographic approach has received much less attention (Beck & Jackson, 2021). This examines intra-individual variation over time to make specific predictions about an individual (Beltz et al., 2016; Molenaar, 2004). While nomothetic studies are easier to conduct, examining a population at a single time point, idiographic studies demand repeated assessment, making them more resource-intensive and burdensome for the researchers and participants. A limitation of the nomothetic approach is that assumes homogeneity across the population and time. However, most cognitive processes are nonergodic, and therefore interindividual variation may not reflect intraindividual variation (Hamaker, 2012). Additionally, most clinical conditions are heterogenous, with patients from the same disorder differing from each other and the symptom profile of one patient changing over time. If we are to achieve the clinical goal of 'bench to bedside' (Kazdin & Rabbitt, 2013), making accurate predictions at the level of the individual, we must begin to adopt idiographic approaches into our research.

Smartphones and online-methods have revolutionised data collection, providing a platform to collect large-scale, repeated assessments at ease for both sides (i.e., the researcher and participant). These approaches have dramatically increased our sample size and facilitated replication-and-exploration style research studies that have complimented (or refuted) observations from small-scale laboratory-based studies as a result of improved statistical power (Gillan & Rutledge, 2021). Most importantly, they now allow researchers to track within-person variability across different time scales, from testing within the same day to months, or even years apart. This shift beyond cross-sectional studies is not only crucial for establishing the temporal stability of brain-behaviour associations over time but also in developing casual models of how cognitive processes and symptoms interact over

the natural course of clinical development or through treatment. However, within-person repeated assessments are not without their challenges. Developing and maintaining smartphone applications come at a significant financial cost (Tibon et al., 2022). Additionally, participant engagement is a key factor. To improve adherence, researchers must consider participants at the forefront of study design, transforming long-winded monotonous cognitive tasks into brief and engaging games. Given the financial and logistical burdens of large-scale studies, it is imperative that we carefully consider which cognitive tasks we take forward for repeated testing.

5.2.2.1 Rethinking Reliability & Individual Differences for Clinical Insights

Traditionally, cognitive research employed a between-group approach, manipulating a cognitive function in one group, and comparing it to a control (Hedge et al., 2018). A task was considered reliable if it consistently produced between-group differences (Bruton et al., 2000; Hedge et al., 2018). Tasks that demonstrated large between-group effects were assumed to qualify as starting points for individual difference testing, aimed at identifying characteristics associated with cognitive processes. However, this requires large variation between participants to reveal statistical differences. The issue here is that tasks that demonstrate a between-group effect require low variation between individuals, and therefore are not suitable for individual difference testing (Cooper et al., 2017b; Hedge et al., 2018; Karvelis et al., 2023). Given this, it is unsurprising that many tasks used previously to demonstrate reliable between-group effects have demonstrated poor reliability in individual difference testing (Elliott et al., 2019; Enkavi et al., 2019; Karvelis et al., 2023). Reliability is crucial in individual difference testing aimed at identifying stable characteristics. This has launched efforts to improve the psychometrics of our tasks to ensure stability of inter-individual differences in the construct they measure, but does this translate clinically?

One key challenge in clinical research is determining whether these associations between cognitive measures and individual differences are merely descriptive markers or whether they have predictive utility (Yip et al., 2023). For instance, model-based planning was initially studied using between-group comparisons (e.g., clinical vs control group), has since been linked to individual differences in

demographics (e.g., age, gender, IQ and education), and specifically to a transdiagnostic dimension of compulsivity, where reductions in model-based planning are associated with greater reporting of compulsive tendencies. While this association has been well-replicated cross-sectionally, a critical question remains; do differences in model-based planning predict changes in compulsivity over time (i.e., predictive value), or are they simply correlated with compulsive traits (i.e., descriptive utility)? With this in mind, we developed a gamified task to measure model-based planning repeatedly within the same individual. We found no difference in the strength of the association between model-based planning and compulsivity across game play sessions. Additionally, we found model-based planning remained unchanged despite improvements in compulsivity over treatment, suggest model-based planning has a more trait-like quality than predictive marker of within-person change in compulsivity. Although both were measured within a relatively short timescale, the within-subject association between model-based planning and compulsivity has been shown to remain stable up to a year (Sookud et al., 2024). If within-person variation is so low and remains stable over-time it calls into question if we can derive any predictive utility from this measure.

While within-person variability is often treated as noise that reduces reliability, it might be more clinically informative than achieving stability (Beck & Jackson, 2021; Hoffman, 2007; Salthouse, 2007). For example, MacDonald et al. (2003) found that increased within-person variability in response times among older adults predicted future cognitive decline. Similarly, Konova et al. (2020) revealed in patients with substance-use disorders that within-subject fluctuations in ambiguity intolerance over weekly and monthly assessments predicted opioid use in one to four weeks, despite no significant differences between patients and controls at a group level. Additionally, in this study the patient group demonstrated lower reliability, which likely reflected the clinically meaningful change. These findings highlight an important trade-off where measures that are highly stable are less sensitive to clinically meaningful within-person variability, whereas state-like effects have more clinical utility at the cost of lower reliability estimates. This is not to claim stable traits aren't important. If premorbid to a condition, they could help identify individuals for early intervention or be used to understand the development trajectory of a disorder.

For example, future studies could benefit from investigating if model-based deficits are apparent prior to disorders of compulsivity. Moving forward, we must begin to identify both state-like and trait-like measures so that we can measure these constructs on appropriate timescales to avoid participants repeatedly engaging in tasks with little variation and redirect resources to tasks that track meaningful clinical change (Piccirillo & Rodebaugh, 2019).

Testing for individual differences requires large, well-powered samples to capture sufficient variability in the characteristics of interest. However, larger samples also introduce greater heterogeneity, which can obscure individual-level effects (Fisher et al., 2018). A key concern is the issue of nonergodicity, the lack of generalisability from group-level statistics to the individual, which poses a threat to the inferences drawn from these large N studies. In other words, to what extent do large group data map onto individual-level data. Using multiple large datasets, Fisher and colleagues (2018) found substantial numerical disagreement between individual- and group-level estimates where aggregated group-level estimates did not consistently align with individual-level patterns (Fisher et al., 2018). This raised concerns that population-level effects may misrepresent the true nature of psychological variables and their relationships at the individual level. Moreover, there is a growing recognition that the associations between cognitive task performance and mental health are small — an observation that extends across multiple domains in psychiatry including neuroimaging (Marek et al., 2022), genetics (Dunn et al., 2015), and environmental risk factors (Baldwin et al., 2021). While such effects may lack theoretical or clinical utility at an individual-level, they can still be meaningful from a public health perspective, especially when scaled to the population (Gotz et al., 2022). Although large-N studies have addressed longstanding statistical power issues in lab-based research and yielded replicable brain-behaviour associations cross-sectionally, they still remain limited in detecting within-person dynamics or contextual influences that require testing beyond a single session. Without complimentary idiographic approaches, there is a risk that large scale studies may reinforce population-level generalisations that fail to translate to individual-level prediction or intervention.

5.2.2.2 Casual Models & Mechanistic Insights

Within-person longitudinal data can also aid casual inferences (Rohrer & Murayama, 2023). A long-standing debate about compulsive behaviours concerns the directionality between compulsions and obsessions. Traditional theoretical models propose that obsessions drive compulsions, where compulsive behaviours serve to alleviate anxieties and distress associated with the intrusive thoughts. However, an alternative hypothesis suggests compulsions are the main drivers of obsessions, with individuals developing strong beliefs after engaging in compulsive behaviours as a way of rationalising them (Gillan & Sahakian, 2014). In a post-hoc analysis, OCD patients were asked to explain why they responded to avoid a mild electrical shock even though the outcome of this response was devalued by overtly disconnecting them from the shock administrator machine (Gillan, Morein-Zamir, Urcelay, et al., 2014). Contrary to the knowledge, patients reported they thought they still might get shocked. This suggests irrational beliefs might emerge consequently rather than being the initial driver of compulsive behaviour which is reinforced on repetition. Repeated within-person assessments can help resolve this debate by tracking how the temporal dynamics between obsessions and compulsions unfold through time (Rohrer & Murayama, 2023; Voelkle et al., 2018). Similarly, these methods can help delineate the temporal processes of how treatment occurs (Zilcha-Mano, 2020). It remains unclear whether treatments work by improving cognition functioning that precede symptoms improvement, or whether symptom improvement precedes changes in cognition (Greenberg & Safran, 1984). As demonstrated throughout the thesis, smartphone and online methods can facilitate the collection of high frequency cognitive and clinical assessments, providing new avenues for tackling causal mechanisms of compulsivity and understanding how treatment works at a mechanistic level.

The next frontier in psychiatry requires a shift from small, cross-sectional and observational studies towards large-scale, longitudinal within-person assessments to close the gap between description and prediction. These will be logistically challenging and expensive, but they are critical if we are to translate to clinical utility. To achieve this, research must embrace remote testing and novel techniques such as gamification to maintain long-term participation. However, this first requires the careful selection of tasks that maximise predictive and translational utility.

5.3 Limitations & Future Directions

The thesis relied entirely on remote data collection, which presents several limitations when sampling from this population. First, participant verification is a significant challenge. In laboratory settings, researchers can confirm participant identity through in-person contact (Uittenhove et al., 2023) . In contrast, remote testing makes it difficult to verify who is performing the experiment. The task could be completed by multiple individuals or not even by humans and instead through automated bots (Kennedy et al., 2020; Moss & Litman, 2018; Moss et al., 2021; Storozuk et al., 2020). Additionally, there is a risk of duplicate participation, where individuals create multiple accounts to complete the study more than once (Newman et al., 2021). This violates the statistical assumption of independent observations. While we adhered to recommended guidelines by accepting only one response per account and device, research has indicated that participants circumvent these restrictions by using virtual private servers to engage in the same task multiple times (Dennis et al., 2020; MacInnis et al., 2020). In Chapter 4, our cross-validation between reported data on Prolific and the smartphone app Neureka revealed two Prolific accounts linked to a single Neureka account, suggesting duplicate participation. These cases were removed from the analysis but were only identified through cross-validation which many studies don't implement. Thus, the current scale of this issue is currently unknown. Second, as online studies become more prevalent, there is an increasing likelihood that participants have previously encountered the same or similar tasks (Chandler et al., 2013; Newman et al., 2021; Stewart et al., 2017). This raises concerns regarding task naivety. It is suggested that most participation from crowd-sourced platforms comes from a small number of 'super-workers' that complete a disproportionate number of tasks (Deetlefs et al., 2015; Stewart et al., 2017). For example, Chandler et al. (2014) found at the time of publishing, 10% of MTurk workers completed 40% of all tasks on the platform. This increased familiarity can influence the sensitivity of the task and reproducibility (Zwaan et al., 2018). In a pre-registered study, Chandler et al. (2015) found effect sizes reduced among participant who had previously participated in the same study. This becomes less problematic in longitudinal within-person designs where repeated task exposure is intentional but highlights the influence of practice effects. Finally, self-selection biases limit the generalisability of our findings (Donegan & Gillan, 2022). In Chapter 2, participants voluntarily engaged with our gamified task

and a subset of the sample re-engaged with the task out of their own free will; in Chapter 3, treatment-seeking individuals self-selected to participate in research in parallel to their treatment; and in Chapter 4, crowd-sourced participants self-selected this task to complete for monetary compensation. Further, two of these studies required repeated assessment while the other offered re-engagement as optional. As a result, these samples are likely to reflect highly motivated and engaged individuals, which may not be representative of the broader population. However, they are also the individuals whom the research objectives resonates most with (Donegan & Gillan, 2022).

Despite a dramatic increase in the number of participants who played our gamified task in Chapter 2, we found that our task became less sensitive with repeated play. This challenges the assumption that collecting more trials per participant increases the reliability of the estimate and thereby improves its sensitivity (Kadlec et al., 2023; Zorowitz & Niv, 2023). This is concerning if we want to leverage these games in repeated within-person testing through time. A possible explanation is that the relatively static task format led to reduced engagement over time. Unlike naturalistic games, which increase in complexity the more time spent playing or follow the development of a story, our task remained unchanged from one session to the next (with the exception of random drift rates). Research suggest that individuals re-engage with games based on the perceived challenge and learning progress (Gottlieb et al., 2013). Engagement often follows an inverted U-shape function, with highest engagement occurring at intermediate difficulty levels where learning is maximal (Brändle et al., 2024; Gottlieb et al., 2013). If a task is too easy, learning is minimal as participants already possess the required skill to complete; if too difficult, participants disengage due to frustration. This may explain the decline in task sensitivity, as increased familiarity led to reduced cognitive effort and the adoption of different strategies. One suggestion is to implement adaptive difficulty through stair-casing, where task difficulty is titrated to the individual's own performance (Zorowitz & Niv, 2023). For example, Tran et al. (2024) applied a stair-casing approach to the stop-signal task, adjusting the stop-signal target based on participants' previous success at inhibiting the response. This provided a more accurate estimation of individual differences in stop-signal reaction times. They also found this approach retained more valid trials and could detect effects in fewer trials.

Similarly, Fox et al. (2024) titrated difficulty in a gamified perceptual decision-making task by making the difference between the number of dots contained in two stimuli more or less easy to distinguish based on the previous trials' accuracy. They too found external validity could be detected in as few as 40 trials and effects were sensitive with repeated play. Applying a staircasing approach therefore may strike a balance between challenging and learning. Future research should embrace interdisciplinary teams (e.g., gaming developers) in designing gamified tasks to get the most from using games to study cognition (Allen et al., 2024; Long et al., 2023).

In light of these task design considerations, it is important to think critically and revisit the games central aim of capturing individual differences in model-based planning, particularly its relationship with compulsivity. This robust theoretical relationship was inconsistently observed across chapters using the gamified task, raising concerns about the validity and robustness of the task. As mentioned, theoretical and empirical models have consistently linked reductions in model-based control to individual variation in compulsivity. This finding has replicated across laboratory and online settings, in both general and clinical populations (Gillan, Kosinski, et al., 2016; Patzelt et al., 2019; Seow et al., 2021; Sharp et al., 2023; Sookud et al., 2024; Voon et al., 2015; Voon et al., 2017). Consistent with this, in Chapter 2 we found evidence supporting a negative association between model-based planning and compulsivity using a gamified version of the traditional two-step task, but notably only under specific task conditions – namely, when using a less deterministic transition ratio and specific reward drift rate. In other words, under certain task environments, we did not observe the robust relationship between model-based planning and compulsivity using the gamified task. Further, this association failed to replicate in Chapter 4 despite implementing the less deterministic transition ration that previously enhanced sensitivity. While group-level model-based behaviour and convergent validity with habitual errors were observed, individual differences to compulsivity, as well as gender and education, were absent. These inconsistencies raise concerns regarding the external validity of model-based planning measured in this gamified task.

Several factors may contribute to this variability. First, and as mentioned prior, the multiple modifications to the task design may have inadvertently reduced the

sensitivity of the gamified task to detect individual differences. Though the strongest transition ratio was implemented, other task modifications such as multiple reward drift rates (ten possible drift rates implemented in Chapter 4 as opposed to two drift rates in Chapter 2) and suboptimal instruction delivery (passive as opposed to active instruction which were accessed via a separate app section in Chapter 4) may have reduced the sensitivity to detect these subtle individual differences. Second, the expression of model-based control may depend on contextual task demands and cognitive load, which can vary across tasks versions. More ambiguous or challenging conditions appeared to amplify individual differences related to compulsivity, whereas clearer, more deterministic conditions may have diminished these differences. As some of the manipulations were between-subject or with different populations, we can't determine in this thesis if there are environments where compulsive individual can engage in model-based planning adequately. Subsequently, while model-based control demonstrated trait-like stability over repeated game sessions (Chapter 2) and through treatment (Chapter 3), we cannot rule out state-dependent fluctuations influenced by momentary factors such as motivation, fatigue, or engagement. For example, model-based planning was measure at a single time-point following seven days of consecutive assessment in Chapter 4. Together, these inconsistencies highlight the fragile and context-dependent nature of the relationship between model-based planning and compulsivity, particular when measured through in a gamified, variable format. This variability underscores the need for adopting multi-method approaches, including multiple task variants and longitudinal designs, to accurately capture the complexity of compulsivity and its cognitive underpinnings. Future research should carefully consider contextual factors, including practice effects and instruction delivery, and how these influence the external validation of our tasks.

Finally, our ability to interpret clinical results in Chapter 4 was limited by the highly skewed distribution of hair-pulling, skin-picking and tic-related behaviours. Contrary to our expectations, exaggerated habit learning was not associated with increased severity of these behaviours. These behaviours, triggered by premonitory urges, resemble the cognitive representation of stimulus-response habits triggered from antecedent stimuli. Furthermore, habit reversal therapy, designed to break the link between the premonitory urge and behavioural response (akin to breaking stimulus-

response habits) has proven effective in treating these conditions (Bate et al., 2011; Dutta & Cavanna, 2013; Lee et al., 2019; Rahman et al., 2023). One suggestion is that the lack of variability in symptoms constrained our ability to detect meaningful associations. The strength of statistical relationships depend on sufficient variability in both measures, and restricted ranges can attenuate effect sizes (Goodwin & Leech, 2006). Skewed distributions also impact factor analysis. When certain items exhibit extreme skewness, they tend to cluster together, potentially creating artificial factors (Ellis, 2017; Gorsuch, 1997). A common recommendation is to exclude items with low variance that strongly deviate from normality (Ellis, 2017). In our case, this would have required removing hair-pulling and some skin-picking and tic items entirely. Additionally, factor analysis assume that item responses are normally distributed (Ellis, 2017; Gorsuch, 1997). However, this is difficult to achieve with a small number of discrete response options (e.g., 1-2-3-4), instead recommending response options to be between 5-7. However, the scales selected were based on validated instruments, and modifying them would have compromised construct validity. Future research should consider developing improved scales, particularly in measuring bodily-focused repetitive behaviours with few available, with a greater number of items and greater number of response options to enhance variability and better capture the symptoms associated with these understudied conditions. Additionally, targeted recruitment of participants with higher symptom severity could increase variation in these measures, allowing for a more robust factor analysis.

5.4 Conclusion

This thesis leveraged smartphone and online methodologies to advance our understanding of the cognitive mechanisms underlying compulsive disorders. Through three experimental chapters, we developed and validated novel gamified smartphone tasks to measure model-based planning and stimulus-response habit formation, demonstrating their feasibility for large-scale, repeated assessments beyond traditional laboratory settings. Our findings reinforced current theoretical models proposing that compulsive behaviours arise from imbalances within the dual-system of control causing an overreliance on habits. Importantly, we extended this theory suggesting that not just differences in goal-directed control influence the expression of habits, but also a greater propensity to form habits. This insight suggests that interventions targeting stimulus-response associations may be more

effective than existing treatments that rely on sufficient goal-directed control. If we are to improve the description, explanation and prediction of clinical research we must embrace the digital shift transitioning the research space from underpowered, cross-sectional, laboratory-bound studies to large-scale, longitudinal testing in the wild. This movement poses its own challenges but ultimately critical if we want to translate clinical research into clinical action.

Chapter 6: References

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Chapter 7: Supplementary Materials

7.1 Supplementary Materials for Chapter 2

7.1.1 Supplementary Methods and Analyses

7.1.1.1 Assessing the impact of an error in STAI-T administration

A typographical error of the STAI questionnaire occurred affecting the users included in our analysis of clinical associations. The response options coded in the app reflected the STAI-State portion of the questionnaire rather than STAI-trait portion (State responses: ‘Not at all’, ‘Somewhat’, ‘Moderately so’, ‘Very much so’ as opposed to Trait responses: ‘Almost never’, ‘Sometimes’, ‘Often’, ‘Almost always’ respectively).

To check if this changed the meaning of the responses unduly, we compared the depression and anxiety scores in our sample of $n = 1451$ and compared it to previously found associations using the correct version. Prior research found the associations between depression anxiety scores measured from the Self-rated Depression Scale (SDS) and State-Trait Anxiety Inventory (STAI) respectively was $r = .84$ in $n = 495$ individuals (Rouault et al., 2018). In line with this, we found the association between SDS depression scores and STAI scores was $r = .88$. Additionally, as reported in the main text, factors derived from our dataset including participants with the incorrect version were all correlated with factors drawn from a dataset using the correct version in excess of .9, providing comfort that this error had no substantial impact of findings.

7.1.1.2 Comparing different estimation approaches

Method: Internal consistency, test-rest, external validity

There are various ways to analyse data on this task. Hierarchical logistic regression (HLR) models are a popular choice, but there are alternatives range from extremely computationally cheap point estimates (PE) to expensive generative models using hierarchical Bayesian modelling (HB). We tried these alternatives and selected the

best approach in a data driven manner, guided by the reliability and external validity of the measures.

Point estimates were calculated by the sum of the probability of staying on [Common, Rewarded trials + Rare, Unrewarded] minus the sum of the probability of staying on [Common, Unrewarded + Rare, Rewarded trials]. Using PE could cut down computational demand and time to calculate estimates, and so may be ideally suited to app-based implementation. However, research suggests they suffer from poor reliability as they fail to consider trial-by-trial individual variability (Haines, 2020). Both HLR and HB methods account for uncertainty at the level of individual subjects and in the context of the twostep task, have been shown to perform similarly well (Brown et al., 2020). For the HB model, we fitted candidate models and selected the best to bring forward for comparison to these other approaches (as described in Supplementary Analysis 7.1.1.3, Supplementary Table 7.1.2.19).

We operationalised the (i) internal consistency of the different measures as Cronbach's alpha comparing model-based planning estimates derived from participants' 1st 100 trials to their 2nd 100. We tested this on $n = 1451$ individuals that had complete demographic and mental health data. Note that we did not use odds/evens here, because HB relies on information accumulated slowly over many trials occurring in series. We also tested for (ii) test re-test reliability from a subset of $n = 423$ who completed two sets of 200 trials of Cannon Blast. Finally, we assessed (iii) the external validity of these measures by testing the relationship between estimates of model-based planning and individual differences in age, gender, education and compulsivity in $n = 1451$.

As outlined above, we compared three methods of calculating model-based estimates: a point estimate approach (PE), hierarchical logistic regression (HLR) and hierarchical Bayesian modelling (HB). The three analytic approaches produced highly correlated estimates (HLR vs PE: $r = .94$, HLR vs HB: $r = .82$, PE vs HB: $r = .74$).

Using the split-half method where model-based scores from first 100 trials were compared with model-based estimates from the second 100 trials, we found the

HLR demonstrated the greatest reliability with fair reliability ($r = .56$, Table 7.1.2.18). Point estimates and computational modelling also demonstrated fair split-half reliability of model-based estimates (PE: $r = .48$, HB: $r = .44$). Again, we found the HLR demonstrated the highest test-retest reliability with fair reliability ($ICC_1 = .54$). Point estimates also demonstrated fair test retest reliability ($ICC_1 = .63$), while Hierarchical Bayesian model showed poor reliability ($ICC_1 = .29$). In terms of external validity, the HLR demonstrated the best signal between model-based planning and individual differences (Supplementary Table 7.1.2.18).

Result: HLR outperforms PE and HB across all metrics

7.1.1.3 Computational Modelling

The basic reinforcement-learning (RL) models used in this study were based on those developed and refined across a range of studies modelling behaviour in the classic version of this task. We considered the 3 models tested previously considered in the context of validity of model-based assessments (Brown et al., 2020). These were adapted to the fact that our task does not include a second stage decision as per Gillan et al. (2015). For all models, Q values refer to the expected value of a given state. The first states are the two containers that can be selected, $a_{i,t}$, where i refers to the “mostly pink” or “mostly purple” containers and t refers to trial. The second states correspond to the colour of the ball fired by the cannon, pink or purple, denoted $s_{j,t}$, where j refers to pink or purple. Reward on a given trial is defined as the ‘ball quality’, i.e. whether or not it explodes (‘good’ or ‘dud’) prior to hitting its target, denoted r_t .

Model A. This model is based on the original paper describing this task (Daw et al., 2011) and consists of 5 free parameters, ω (the relative contribution of model-based and model-free Q values on choice), α_1 (the stage 1 learning rate), α_2 (the stage 2 learning rate), ρ (the perseveration or ‘stickiness’ parameter) and β (the inverse temperature).

The model-free algorithm updates the Q values, $Q_{MF1}(a_{i,t})$, for two choices of containers ($a_{i,t}$) at stage 1 as follows: $Q_{MF1}(a_{i,t}) = Q_{MF1}(a_{i,t}) + \alpha_1 * (\delta_{1,t-1} +$

$\delta_{2,t-1}$), where $\delta_{1,t}$ is the reward prediction error (RPE) associated with the chosen container ('first stage' in classic task): $Q_{MF2}(s_{j,t}) - Q_{MF1}(a_{i,t})$ and $\delta_{2,t}$ is the RPE associated with the ball (pink/purple) that was released ('second stage' in the classic task): $r_t - Q_{MF2}(s_{j,t})$. These Q values for the two ball colours are updated according to $Q_{MF}(s_{j,t}) = Q_{MF}(s_{j,t}) + \alpha \delta_{2,t}$. The model-based algorithm applies a transition function, using known probabilities for how likely it is that a given ball is produced following a given container choice to ensure that terminal rewards are correctly assigned to initial choices. We assumed full knowledge of transitions in this task (80:20), as the proportion of pink/purple balls for each side was explicitly stated to subjects during the instructions and was visible throughout the experiment as the balls jumped around the two s. The model-based Q values for each container, $Q_{MB1}(a_{i,t})$, were thus computed as $Q_{MB}(a_{i,t}) = Q_{MF2}(s_{j,t}) * P(s_{j,t}|a_{i,t})$. To connect the values to choice of container, we use a Softmax choice rule, which assigns a probability to each action according to the weighted combination of the MB and MF estimates $Q_{net}(a_{i,t}) = \omega * Q_{MB}(a_{i,t}) + (1 - \omega) * Q_{MF1}(a_{i,t})$. The probability of choosing each of the two containers is calculated, accordingly, as:

$$P(a_{i,t}) = \frac{\exp [\beta * Q_{net}(a_{i,t}) + \rho * rep(a_{i,t})]}{\sum_{a'} (\exp [\beta * (Q_{net}(a'_t) + \rho * (rep(a'_t))])}$$

Model B. This model is based on that described in Otto et al. (2013) and consists of 4 free parameters, α (a single learning rate that is applied to both stages), ρ (the perseveration or 'stickiness' parameter) and β_{MF} (the model-free inverse temperature) and β_{MB} (the model-based inverse temperature). The model has two additional features compared to Model A: (i) rescaling of rewards (based on α), which is designed to reduce parameter collinearity and (ii) a decay or 'forgetting' of unselected parameters (based on $1 - \alpha$). The former manifests in the RPE terms where $\delta_{1,t} = \frac{Q_{MF2}(s_{j,t})}{\alpha} - Q_{MF1}(a_{i,t})$ and $\delta_{2,t} = \frac{r_t}{\alpha} - Q_{MF2}(s_{j,t})$. The latter is implemented by multiplying the unchosen model-free Q values by $1 - \alpha$, as follows: $Q_{MF1}(a_{unchosen,t}) * 1 - \alpha$ and $Q_{MF2}(s_{unchosen,t}) * 1 - \alpha$. The choice rule is implemented as follows:

$$P(a_{i,t}) = \frac{\exp [\beta MF * Q_{MF1}(a_{i,t}) + \beta MB * Q_{MB}(a_{i,t}) + \rho * rep(a_{i,t})]}{\sum_{a'} (\exp [\beta MF * (Q_{MF1}(a'_t)) + \beta MB * (Q_{MB}(a'_t)) + \rho * (rep(a'_t))])}$$

Model C. This model is based on Decker et al. (2016) and is identical to Model B, except that it omits the decay of unchosen values and the rescaling of rewards.

Model D. This is identical to model B, without the reward rescaling feature (analogously it is the same as Model C but it includes a decay of unchosen values).

Model E. this is identical to model D with an additional free parameter wherein the decay or ‘forgetting’ of the value of unselected options is governed by ‘ αD ’, rather than $1 - \alpha$.

Model F. this is identical to model D with rescaling only on reward values (not qt1)

Model G. this is identical to model D with separate learning rates for rewarding and non-rewarding trials.

Group-Level Modelling

For each of the models above, hierarchical Bayesian estimation was carried out using we Markov Chain Monte Carlo (MCMC) techniques (specifically the No-U-Turn variant of Hamiltonian Monte Carlo) as implemented in the Stan modelling language. All parameters were specified as normally distributed; parameters with (0,1) ranges ($\alpha 1$, $\alpha 2$, αD , and ω) were then inverse logit transformed. Means of inverse temperature parameters ($\beta 1$, βMF , and βMB) were constrained to be greater than zero. Weakly informative priors were used: for means, normal (0,2.5) for $\alpha 1$, $\alpha 2$, αD , ω , and ρ or normal (0,5) for $\beta 1$, βMF , and βMB ; for scales, half-Cauchy (0,1), constrained to be greater than 0, for $\alpha 1$, $\alpha 2$, αD , ω , ρ , and βMF or half-Cauchy (0,2), constrained to be greater than 0, for $\beta 1$, and $\beta 1 MB$. For model-selection, we ran four chains of 4000 samples each, discarding the first 2000 samples of each chain for burn-in. When modelling the larger dataset, we reduced this to 2000 samples total for speed.

Model Comparison

To determine the best model, we ran each model on a subset of $N = 100$ of the total sample. We compared models using a range of indicators of validity, with a summary presented in the table below.

MCMC diagnostics

We first examined the chains visually for convergence and also computed Gelman and Rubin's (1992) potential scale reduction factors. For this, large values indicate convergence problems, whereas values near 1 are consistent with convergence. We ensured that these diagnostics were less than 1.05 for all variables. Model A had 78 divergent transitions and Model B had 371. Models C and D had no divergent transitions. Model E had 1 divergent transition and model F had 2.

LOOIC

We compared the fit of each model using the LOOIC (loo package in R). The lowest LOOIC was found for Model D, followed closely by model E, F then B.

Posterior Predictive Checks (PPC)

We examined the trial-by-trial predictions for subjects' choices ('y_pred') derived from the model and tested how closely they mirrored actual choices per subject ('y_actual'). For each subject we calculated the proportion of trials where the predicted choice was the same as the actual one and summarised that consistency metric across the entire sample (mean, min, max, and proportion of subjects with consistency >80%). Model G performed the best considering all PPC metrics combined, but there were relatively modest differences across models.

Parameter Recovery

We tested parameter recovery by simulating new choices from agents operating under the true estimated median parameters of our $N = 100$ subjects. We then fitted our models to these choices to test if we could recover the true data generating parameters. We computed correlation coefficients as a measure of parameter recovery. Model B and F showed the strongest recovery of model-based betas at .74, but rates were similar at .73 for model G, .72 for model E, .71 for Model D and .70 for Model C.

Based on the combination of validation checks, we progressed with Model D for comparison analysis with hierarchical logical regression (HLR) and the point-estimate (PE) approach. Most notably, model D was one of the few models with no divergent transitions, had the lowest LOOIC, and strong performance on all other metrics, if not the best.

7.1.1.4 Model-Free Estimates, Reliability and Relationship to Model-Based Planning

MFI was less consistent across tasks, with estimates from the traditional task and Cannon Blast correlating at just $r = .15$. In the same sample, MFI from Cannon Blast correlated with both MFI and MBI estimates from the traditional task, at comparable strengths (MFI-CB and MFI-Trad $r = .15$ & MFI-CB and MBI-Trad $r = .19$). This could suggest that the MFI estimate from Cannon Blast partially captures model-based processes. However, we urge caution in over-interpreting this result. The sample size for Experiment 1 was small ($n = 57$) and we in fact observed larger correlations between model-based and model-free planning in the traditional task ($r = .6$) than in the gamified one ($r = .06$) in Experiment 1. In the larger citizen scientist dataset, the latter correlation rose to .33. Comparing these results to publicly available datasets, there is a relatively large range of values. For example correlation between MBI and MFI was .18 in Gillan et al., (2016) and .36 in Seow et al., (2020) (Supplementary Figure 7.1.3.4).

7.1.2 Supplementary Tables

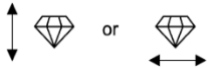
7.1.2.1 Task parameters used in the traditional two-step task and Cannon Blast

	Traditional Task	Cannon Blast
Trial Number	200	200
Choice Time	1500 _{ms}	Unlimited
Reward Probability (Mean)	.52	.72 ^a
Rewards Received (Mean, Range)	104.32 (83-127)	145.05 (124-161) ^b
Unique Drifts	1	4
Transition Probabilities	70:30	80:20
Pre-task tutorial	40 active training trials	5-step passive instruction
Stated Objective	Collect Space Treasure	Collect diamonds

^a This is the average reward probability across the two ball colours across all four drift sets in Cannon Blast.

^b This is the average rewards actually received by participants across all four drift sets in Cannon Blast.

7.1.2.2 Descriptive information of easy and medium levels of difficulty in Cannon Blast

Difficulty	Level	Description	Diamond Behaviour	Hit Rates
Easy	1	Static diamond		83%
Easy	2	Moving diamond horizontally or vertically		53%
Easy	3	Moving diamond in direction of an arc		44%
Easy	4	Static diamond facing down in 3 directions		29%
Medium	5	Static diamond with shell facing down		75%
Medium	6	Moving diamond diagonally with shell facing down		20%
Medium	7	Static diamonds with rotating shells		38%
Medium	8	Static diamonds with gates		45%

7.1.2.3 Demographic characteristics in experiment 2 in the whole sample and the two age, gender and education matched samples.

Characteristics	Whole Sample	80:20 Matched	70:30 Matched
N	5005	2138	2138
Mean (SD)			
Age	45.69 (14.54)	47.23 (13.62)	48.12 (14.42)
N (% of N)			
Gender			
Cisgender female	3225 (64%)	1397 (65%)	1429 (67%)
Cisgender male	1683 (34%)	701 (33%)	661 (31%)
Non-cisgender ^a	82 (1.9%)	37 (1.9%)	44 (1.9%)
Preferred not to say	15 (>.01%)	3 (>.01%)	4 (>.01%)
Education Attainment			
No formal education	71 (1%)	21 (1%)	25 (1%)
Lower Second Level	399 (8%)	161 (8%)	167 (8%)
Upper Second Level	1314 (26%)	579 (27%)	594 (27%)
University/College	2043 (42%)	846 (39%)	876 (41%)
Master's	958 (19%)	434 (20%)	377 (18%)
PhD (or equivalent)	219 (4%)	97 (5%)	99 (5%)

^a 'Non-cisgender' includes those who identify as Transgender Male, Transgender Female, Non-Binary, or not-listed

SD = Standard deviation

7.1.2.4. Mental health summary in experiment 2 (n = 1451): mean symptoms and their association with age, gender, and education.

Construct (Measure)	α	Mean (SD)	Age		Gender		Education	
			r	p	diff ^a	p	F ^b	p
Apathy (AES)	.90	31.54 (8.55)	-.23	<.001	1.08	.119	39.51	<.001
Alcohol Dependency (AUDIT)	.83	4.16 (4.28)	-.14	<.001	1.23	.000	6.55	.001
Impulsivity (BIS)	.87	57.98 (11.56)	-.25	<.001	-.52	.859	36.94	<.001
Eating Disorders (EAT-26)	.87	29.27 (15.94)	-.16	<.001	-5.21	<.001	1.82	.162
Social Anxiety (LSAS)	.95	28.65 (15.31)	-.27	<.001	-3.49	<.001	27.97	<.001
OCD (OCI-R)	.91	13.94 (11.34)	-.30	<.001	-.47	.890	11.79	<.001
Schizotypy (SCZ)	.89	12.68 (8.00)	-.40	<.001	-1.08	.083	31.05	<.001
Depression (SDS)	.90	38.91 (1.74)	-.35	<.001	-3.69	<.001	42.12	<.001
Trait Anxiety (STAI)	.95	42.40 (13.62)	-.41	<.001	-3.27	<.001	23.95	<.001
Transdiagnostic Factor		Mean (SD)	r	p	diff^a	p	F^b	p
Anxious-Depression	-	2.92 (.89)	-.34	<.001	-.11	.128	42.28	<.001
Compulsivity and Intrusive Thought	-	1.45 (.94)	-.38	<.001	-.23	<.001	7.11	<.001
Social Withdrawal	-	1.50 (.94)	-.25	<.001	-.23	<.001	19.14	<.001

α =Cronbach's alpha, SD = Standard deviation, r = Pearson's coefficient, p = significance

^a Mean group difference between cis-gender males and cis-gender females. Positive values indicate higher scores among males, while negative t-values indicate higher scores in females.

^b Positive F values indicate higher scores among those who did not attain third level education compared with those who obtained third level education and those who attained greater than third level education.

AES: Apathy Evaluation Scale, AUDIT: Alcohol Use Disorders Identification Test, BIS: Barratt's Impulsivity Scale, EAT: Eating Attitudes Test, LSAS: Liebowitz Social Anxiety Scale, OCD: Obsessive Compulsive Disorder, OCIR: Obsessive Compulsive Inventory Revised, SCZ: Short Scale Measures for Schizotypy, SDS: Self-rated Depression Scale, STAI: Spielberger's Trait Anxiety Inventory.

7.1.2.5 Descriptive information of those with repeated plays of Cannon Blast

Number of participants who played Free Play section	785
Number of Free Play sessions completed	2929
Level of difficulty chosen	
Easy	1045
Medium	1161
Hard	723
Mean number of free play sessions (SD)	3.73 (9.11)
Median number of free play sessions	2
Range of free play sessions (Min.-Max.)	1 – 138
Mean time (in days) between 1 st play and follow-up (SD)	4.46 (5.86)
Median of time (in days) elapsed	2
Range of time (in days) elapsed	0-30

7.1.2.6 Descriptive information of reward drift sets assigned at each block

Reward Drift Set	Mean Reward Probability		Mean Reward Probability	Mean Reward Difference	SD Reward Probability		Mean SD Reward	Mean SD Difference
	Purple	Pink			Purple	Pink		
A	.845	.748	.797	.096	.053	.049	.052	.005
B	.774	.501	.638	.273	.103	.041	.072	.062
C	.649	.829	.739	.180	.113	.088	.101	.025
D	.756	.809	.783	.053	.109	.075	.092	.034
E	.542	.611	.576	.069	.110	.059	.084	.051
F	.649	.647	.648	.002	.043	.096	.070	.053
G	.909	.575	.742	.335	.066	.081	.074	.015
H	.510	.564	.537	.054	.043	.096	.069	.053
I	.849	.698	.774	.151	.062	.092	.077	.030
J	.532	.906	.719	.374	.069	.043	.056	.026

Only Drift Set A & Drift Set B used in participants first play ('Risk Factors' section of the app). All ten drifts (A-J) used in repeated plays of Cannon Blast ('Free Play section of the app)

Mean reward/SD difference=Absolute value between the highest mean reward/SD probability minus the lowest mean reward/SD probability

7.1.2.7 Logistic regression analysis across various task modifications

	Est.	Std. Error	z	p
Base Model N=5005				
Intercept	1.38	.01	111.52	<.001
Reward	.47	.01	53.24	<.001
Transition	-.08	.00	-18.37	<.001
Reward: Transition	.27	.01	39.84	<.001
80:20 Transition n = 2138^a				
Intercept	1.31	.02	72.78	<.001
Reward	.47	.01	34.67	<.001
Transition	-.09	.01	-12.79	<.001
Reward: Transition	.31	.01	28.69	<.001
70:30 Transition, n = 2138^a				
Intercept	1.33	.02	72.77	<.001
Reward	.44	.02	33.66	<.001
Transition	-.07	.01	-11.36	<.001
Reward: Transition	.22	.01	23.14	<.001
Easy-1st Block, N=5005				
Intercept	1.24	.01	97.67	<.001
Reward	.44	.01	46.59	<.001
Transition	-.09	.01	-15.94	<.001
Reward: Transition	.31	.01	39.72	<.001
Medium-2nd Block, N=5005				
Intercept	1.55	.02	93.98	<.001
Reward	.48	.01	42.79	<.001
Transition	-.07	.01	-11.38	<.001
Reward: Transition	.23	.01	27.27	<.001
Drift A-1st Block, n = 2395				
Intercept	1.29	.02	67.08	<.001
Reward	.45	.02	28.54	<.001
Transition	-.09	.01	-9.95	<.001
Reward: Transition	.28	.01	23.28	<.001
Drift B-1st Block, n = 2610				
Intercept	1.19	.02	7.43	<.001
Reward	.44	.01	37.28	<.001
Transition	-.08	.01	-11.48	<.001
Reward: Transition	.34	.01	31.99	<.001

Model: $\text{stay} \sim \text{reward}_{t-1} * \text{transition}_{t-1} + (\text{reward}_{t-1} * \text{transition}_{t-1} + 1|\text{id})$

Stay coded as 1=switch, 0=stay, Reward coded as 1=reward, -1=non-rewarded, Transition coded as 1=common, -1=rare

^a Subsample of total dataset to achieve two age, gender and education matched samples

Est = Beta estimate, Std Error = standard error, z = test statistic, p = significance

7.1.2.8 Associations between model-based planning and individual differences (N=5005) and clinical associations (n = 1451)

	Est.	Std. Error	z	p
Individual Differences, N=5005				
Age	-.02	.00	-4.39	<.001
Gender	-.02	.00	-3.34	.001
Education	-.04	.00	-9.41	<.001
Questionnaire Total Scores^a, n = 1451				
Alcohol Dependency	.01	.01	.71	.478
Apathy	.01	.01	.69	.492
Depression	-.02	.01	-1.88	.061
Eating Disorder	-.02	.01	-2.17	.031
Impulsivity	-.02	.01	-2.22	.026
OCD	-.01	.01	-1.62	.105
Schizotypy	.01	.01	.58	.564
Social Anxiety	.00	.01	.26	.797
Trait Anxiety	-.00	.01	-.14	.888
Transdiagnostic Factors^b, n = 1451				
Anxious-Depression	-.00	.01	-.34	.733
Compulsivity	-.03	.01	-2.87	.004
Social Withdrawal	.02	.01	1.62	.106

^a Independent models controlling for age gender and education

^b Covariate model controlling for age, gender and education

Est = Beta estimate, SE = standard error, z = test statistic, p = significance, OCD=Obsessive Compulsive Disorder

7.1.2.9 Associations with stay, model-free, model-based, and transition estimates from Cannon Blast with individual differences and clinical associations

	Est.	Std. Error	z	p
Individual differences, N=5005				
<i>Stay</i>				
Age	-.01	.01	-.90	.368
Gender	.02	.02	1.57	.117
Education	-.00	.01	.00	.997
<i>Model-Free</i>				
Age	-.08	.01	-5.61	<.001
Gender	-.09	.01	-6.28	<.001
Education	-.10	.01	-6.85	<.001
<i>Transition</i>				
Age	.03	.01	-2.44	<.001
Gender	-.00	.02	-.19	.852
Education	.09	.01	6.17	<.001
<i>Model-based</i>				
Age	-.06	.01	-4.39	<.001
Gender	-.05	.01	-3.34	.001
Education	-.13	.01	-9.41	<.001
Clinical differences, n = 1451				
<i>Stay</i>				
Age	.01	.03	.40	.686
Gender	.03	.03	1.13	.260
Education	.02	.03	.57	.566
<i>Model-Free</i>				
Age	-.01	.03	-.34	.736
Gender	-.10	.03	-3.28	.001
Education	.02	.03	.55	.582
<i>Transition</i>				
Age	.02	.03	.71	.478
Gender	.06	.03	2.16	.031
Education	-.05	.03	-1.57	.118
<i>Model-based</i>				
Age	-.01	.03	-4.15	.733
Gender	-.08	.06	-2.88	.004
Education	.05	.03	1.62	.106

Gender: Comparison of cisgender female with reference to cisgender male

Education: negative beta values reflect those with less education associated with greater MBI deficits

Clinical differences: covariate models controlling for age, gender and education

Est = Beta estimate, SE = standard error, z = test statistic, p = significance

7.1.2.10 Linear Regressions Predicting Stay Behaviour by Reward, Transition and Diamond Hit on the Previous Trial.

	Est.	Std. Error	z	p
Intercept	1.38	.03	49.33	<.001
Reward	.43	.03	16.22	<.001
Transition	.01	.03	.42	.676
Diamond Hit	-.05	.03	-1.94	.053
Reward: Transition	.19	.03	7.22	<.001
Reward: Diamond Hit	-.14	.03	-5.39	<.001
Transition: Diamond Hit	.08	.03	3.10	.002
Reward: Transition: Diamond Hit	-.09	.03	-3.35	<.001

Model: $stay \sim reward_{t-1} * transition_{t-1} * diamondhit_{t-1} + (reward_{t-1} * transition_{t-1} * diamondhit_{t-1} + 1 | id)$

Stay coded as 1=switch, 0=stay, Reward coded as 1=reward, -1=non-rewarded, Transition coded as 1=common, -1=rare, Diamond Hit coded as 1=hit, -1=miss

Est = Beta estimate, SE = standard error, z = test statistic, p = significance

7.1.2.11 Linear regressions assessing the external validity across various task modifications

	Est.	Std. Error	z	p
80:20 Transition, n = 1020				
CIT	-.01	.01	-.89	.374
Age	-.03	.01	-2.99	.003
Gender	-.04	.01	-23.29	.001
Education	-.04	.01	-4.18	<.001
70:30 Transition, n = 1051				
CIT	-.05	.01	-4.43	<.001
Age	-.05	.01	-3.92	<.001
Gender	.00	.01	.40	.689
Education	-.04	.01	-4.12	<.001
Easy-1st Block, n = 2369				
CIT	-.03	.01	-3.82	<.001
Age	-.04	.01	-6.24	<.001
Gender	-.01	.01	-1.55	.122
Education	-.05	.01	-7.08	<.001
Medium-2nd Block, n = 2369				
CIT	-.02	.01	-3.39	.001
Age	-.03	.01	-4.31	<.001
Gender	-.01	.01	-1.38	.167
Education	-.03	.01	-5.46	<.001
Drift A-1st Block, n = 1135				
CIT	-.05	.01	-4.94	<.001
Age	-.06	.01	-5.99	<.001
Gender	-.01	.01	-.68	.500
Education	-.05	.01	-4.88	<.001
Drift B-1st Block, n = 1234				
CIT	-.01	.01	-.61	.540
Age	-.03	.01	-2.75	.006
Gender	-.01	.01	-1.42	.155
Education	-.05	.01	-5.24	<.001

Models: model-based index ~ scale(CIT) + scale(age) + factor(gender) + scale(education)

Gender: Comparison of cisgender female with reference to cisgender male

Education: negative beta values reflect those with less education associated with greater MBI deficits

Est = Beta estimate, SE = standard error, z = test statistic, p = significance, CIT = Compulsivity

7.1.2.12 Full comparison models across task modifications

	Est.	Std. Error	z	p
Transition Ratio, n = 2071				
CIT	-.03	.01	-3.62	<.001
Age	-.04	.01	-4.86	<.001
Gender	-.02	.01	-2.05	.041
Education	-.04	.01	-5.87	<.001
Transition ratio	-.01	.01	-.32	.748
CIT*Transition ratio	-.02	.01	-2.29	.022
Age*Transition ratio	-.00	.01	-.45	.658
Gender*Transition ratio	.02	.01	2.61	.009
Education*Transition ratio	.00	.01	.16	.874
Difficulty/Order, n = 2369				
CIT	-.02	.01	-4.12	<.001
Age	-.04	.01	-6.05	<.001
Gender	-.01	.01	-1.67	.095
Education	-.04	.01	-7.18	<.001
Difficulty/Order	-.12	.04	-3.13	.002
CIT*Difficulty/Order	.00	.00	.71	.476
Age* Difficulty/Order	.01	.00	2.40	.017
Gender* Difficulty/Order	.00	.00	.28	.782
Education* Difficulty/Order	.01	.00	2.15	.032
Reward Drift Set, n = 2369				
CIT	-.03	.01	-3.94	<.001
Age	-.04	.01	-6.16	<.001
Gender	-.01	.01	-1.48	.138
Education	-.05	.01	-7.14	.002
Drift set	.05	.08	.65	.518
CIT*Drift set	.02	.01	-3.09	.002
Age*Drift set	.02	.01	2.25	.024
Gender*Drift set	-.00	.01	-.53	.595
Education*Drift set	-.00	.01	-.19	.853

Full comparison models: model-based index ~ (CIT + age + gender + education)*[task parameter]

Task parameters coded in model as: Transition ratio:0=80:20, 1=70:30); Difficulty/Order: 0=Easy-1st Block, 1=Medium-2nd Block; Drift set: 0=Drift A, 1=Drift B

Gender: Comparison of cisgender female with reference to cisgender male

Education: negative beta values reflect those with less education associated with greater MBI deficits

Est = Beta estimate, SE = standard error, z = test statistic, p = significance, CIT = Compulsivity

7.1.2.13 Intraclass correlation co-efficient (ICC) testing the test-retest reliability of model-based indices across task difficulty/order

	N	ICC ₁	CI Lower	CI Upper
1 st play Easy vs 2 nd play Easy	689	.35	.30	.41
1 st play Easy vs 1 st play Medium	556	.49	.44	.54
1 st play Medium vs 2 nd play Easy	689	.45	.40	.50
1 st play Medium vs 2 nd play Medium	556	.51	.46	.56

ICC=Intraclass Correlation Co-efficient, CI=Confidence Interval

Supplementary Table 7.1.2.14 Logistic Regression Analysis with Increasing Trial Number (n = 716)

	Est.	Std. Error	z	p
25 Trials				
Intercept	.96	.04	25.51	<.001
Reward	.41	.03	13.65	<.001
Transition	-.11	.03	-4.13	<.001
Reward: Transition	.37	.03	12.05	<.001
50 Trials				
Intercept	1.06	.03	31.12	<.001
Reward	.42	.03	16.25	<.001
Transition	-.10	.02	-5.34	<.001
Reward: Transition	.33	.02	13.52	<.001
75 Trials				
Intercept	1.11	.03	34.78	<.001
Reward	.44	.02	18.01	<.001
Transition	-.07	.02	-4.51	<.001
Reward: Transition	.32	.02	14.72	<.001
100 Trials				
Intercept	1.16	.03	36.62	<.001
Reward	.49	.02	19.11	<.001
Transition	-.07	.01	-4.72	<.001
Reward: Transition	.32	.02	15.62	<.001
125 Trials				
Intercept	1.16	.03	38.11	<.001
Reward	.46	.02	19.95	<.001
Transition	-.06	.01	-4.95	<.001
Reward: Transition	.30	.02	15.98	<.001
150 Trials				
Intercept	1.19	.03	39.79	<.001
Reward	.47	.02	19.95	<.001
Transition	-.06	.01	-5.41	<.001
Reward: Transition	.29	.02	15.81	<.001
175 Trials				
Intercept	1.22	.03	4.74	<.001
Reward	.47	.02	2.87	<.001
Transition	-.08	.01	-6.65	<.001
Reward: Transition	.28	.02	15.73	<.001
200 trials				
Intercept	1.25	.03	41.51	<.001
Reward	.48	.02	21.45	<.001
Transition	-.09	.01	-7.12	<.001
Reward: Transition	.28	.02	15.91	<.001
225 trials				
Intercept	1.23	.03	43.52	<.001

Reward	.48	.02	22.64	<.001
Transition	-.07	.01	-6.94	<.001
Reward: Transition	.28	.02	16.37	<.001

250 trials

Intercept	1.23	.03	45.33	<.001
Reward	.49	.02	24.21	<.001
Transition	-.06	.01	-8.00	<.001
Reward: Transition	.27	.02	16.74	<.001

275 trials

Intercept	1.24	.03	46.34	<.001
Reward	.48	.02	24.07	<.001
Transition	-.07	.01	-7.37	<.001
Reward: Transition	.26	.02	16.12	<.001

300 trials

Intercept	1.24	.03	46.95	<.001
Reward	.48	.02	24.53	<.001
Transition	-.07	.01	-7.38	<.001
Reward: Transition	.26	.02	16.24	<.001

N = 761

Model: $\text{stay} \sim \text{reward}_{t-1} * \text{transition}_{t-1} + (\text{reward}_{t-1} * \text{transition}_{t-1} + 1 | \text{id})$

Stay coded as 1=switch, 0=stay, Reward coded as 1=reward, -1=non-rewarded, Transition coded as 1=common, -1=rare

Est = Beta estimate, SE = standard error, z = test statistic, p = significance

Supplementary Table 7.1.2.15 Linear Regressions assessing the External Validity with Increasing Trial Number (n = 716)

	Est.	Std. Error	z	p
25 Trials				
CIT	-.04	.01	-3.14	.002
Age	-.02	.01	-1.28	.229
Gender	-.03	.01	-2.14	.033
Education	-.05	.01	-3.97	<.001
50 Trials				
CIT	-.04	.01	-3.15	.002
Age	-.03	.01	-1.93	.052
Gender	-.02	.01	-1.33	.183
Education	-.06	.01	-4.87	<.001
75 Trials				
CIT	-.05	.01	-3.93	<.001
Age	-.03	.01	-2.23	.026
Gender	-.00	.01	-.30	.763
Education	-.06	.01	-4.82	<.001
100 Trials				
CIT	-.05	.01	-3.96	<.001
Age	-.03	.01	-2.62	.026
Gender	-.00	.01	-.16	.763
Education	-.06	.01	-5.03	<.001
125 Trials				
CIT	-.05	.01	-3.79	<.001
Age	-.03	.01	-2.42	.016
Gender	-.01	.01	-.54	.588
Education	-.06	.01	-4.80	<.001
150 Trials				
CIT	-.04	.01	-3.38	<.001
Age	-.03	.01	-2.29	.022
Gender	-.01	.01	-.49	.625
Education	-.06	.01	-4.93	<.001
175 Trials				
CIT	-.04	.01	-3.36	.001
Age	-.03	.01	-2.24	.025
Gender	-.01	.01	-.40	.689
Education	-.06	.01	-4.79	<.001
200 trials				
CIT	-.04	.01	-3.32	.001
Age	-.03	.01	-2.10	.036
Gender	-.01	.01	-.66	.507
Education	-.06	.01	-4.71	<.001
225 trials				
CIT	-.05	.01	-3.44	.001

Age	-.03	.01	-2.32	.020
Gender	-.00	.01	-.26	.793
Education	-.06	.01	-4.90	<.001

250 trials

CIT	-.04	.01	-3.33	.001
Age	-.03	.01	-2.26	.024
Gender	-.00	.01	-.24	.809
Education	-.06	.01	-4.74	<.001

275 trials

CIT	-.05	.01	-3.49	.001
Age	-.03	.01	-2.44	.020
Gender	-.00	.01	-.04	.793
Education	-.06	.01	-5.09	<.001

300 trials

CIT	-.04	.01	-3.37	.001
Age	-.03	.01	-2.52	.012
Gender	-.00	.01	-.01	.994
Education	-.06	.01	-5.13	<.001

N = 716

Models: model-based index ~ scale(CIT) + scale(age) + factor(gender) + scale(education)

Gender: Comparison of cisgender female with reference to cisgender male

Education: negative beta values reflect those with less education associated with greater MBI deficits

Est = Beta estimate, SE = standard error, z = test statistic, p = significance, CIT = Compulsivity

Shaded rows indicate the maximum association observed for each individual difference measure

Supplementary Table 7.1.2.16 Assessing the Impact of Increasing Trial Number per Participant on Mean-level Model-based Indices and their Reliability (n = 716)

Trials	M	SD	Range	Split-half [95% CI]
25	.35	.33	[-.63-1.20]	.41 [.35-.47]
50	.33	.33	[-.52-1.55]	.51 [.46-.57]
75	.31	.32	[-.58-1.61]	.53 [.47-.58]
100	.31	.33	[-.50-1.79]	.59 [.54-.64]
125	.30	.33	[-.46-1.83]	.63 [.58-.67]
150	.28	.33	[-.35-1.97]	.64 [.59-.68]
175	.28	.34	[-.38-2.14]	.66 [.62-.70]
200	.28	.34	[-.45-2.25]	.68 [.64-.72]
225	.27	.35	[-.37-2.28]	.69 [.65-.72]
250	.27	.33	[-.25-2.25]	.70 [.66-.73]
275	.26	.34	[-.30-2.31]	.71 [.67-.75]
300	.26	.34	[-.27-2.31]	.71 [.68-.75]

N = 716

Split-half reliability co-efficient assessed using odd-evens approach

M = Mean, SD = Standard Deviation, CI=Confidence Interval

Supplementary 7.1.2.17 Full Comparison Models with Increasing Trial Number (n = 716)

	Est.	Std. Error	z	p
CIT	-.04	.01	-3.60	<.001
Age	-.02	.01	-1.88	.061
Gender	-.02	.01	-1.39	.165
Education	-.05	.01	-4.61	<.001
Trial Number	-.02	.01	-2.86	.004
CIT: Trial Number	-.00	.00	-.23	.819
Age: Trial Number	-.01	.00	-2.00	.044
Gender: Trial Number	.00	.00	3.55	<.001
Education: Trial Number	.00	.00	-2.03	.042

n = 716

Models: model-based index ~ (scale(CIT) + scale(age) + fac(gender(+ scale(education)))*Trial Number

Gender: Comparison of cisgender female with reference to cisgender male

Education: negative beta values reflect those with less education associated with greater MBI deficits

Est = Beta estimate, SE = standard error, z = test statistic, p = significance, CIT = Compulsivity

Supplementary Table 7.1.2.18 Comparing the Association between Model-based Index and Individual Differences using Different Analytic Approaches to Calculate Model-based Index

	n = 1451				n = 423	
	Est.	Std. E	z	p	Split-Half (95% CI)	Test Retest (95% CI)
<i>Point Estimates</i>					.48 (.44-.52)	.58 (.52-.63)
CIT	-.07	.03	-2.33	.020		
Age	-.10	.03	-3.43	.001		
Gender	-.06	.03	-2.03	.042		
Education	-.11	.03	-4.36	<.001		
<i>Hierarchical Linear Regression</i>					.56 (.51-.58)	.63 (.57-.67)
CIT	-.07	.03	-2.51	.012		
Age	-.12	.03	-4.40	<.001		
Gender	-.07	.03	-2.41	.016		
Education	-.13	.03	-5.23	<.001		
<i>Hierarchical Bayesian Model</i>					.44 (.40-.48)	.29 (.21-.36)
CIT	-.04	.03	-1.36	.173		
Age	-.07	.02	-2.77	.006		
Gender	-.03	.02	-.89	.372		
Education	-.16	.02	-6.04	<.001		

Models: model-based index ~ CIT + age + gender + education

Gender: Comparison of cisgender female with reference to cisgender male

Education: negative beta values reflect those with less education associated with greater MBI deficits

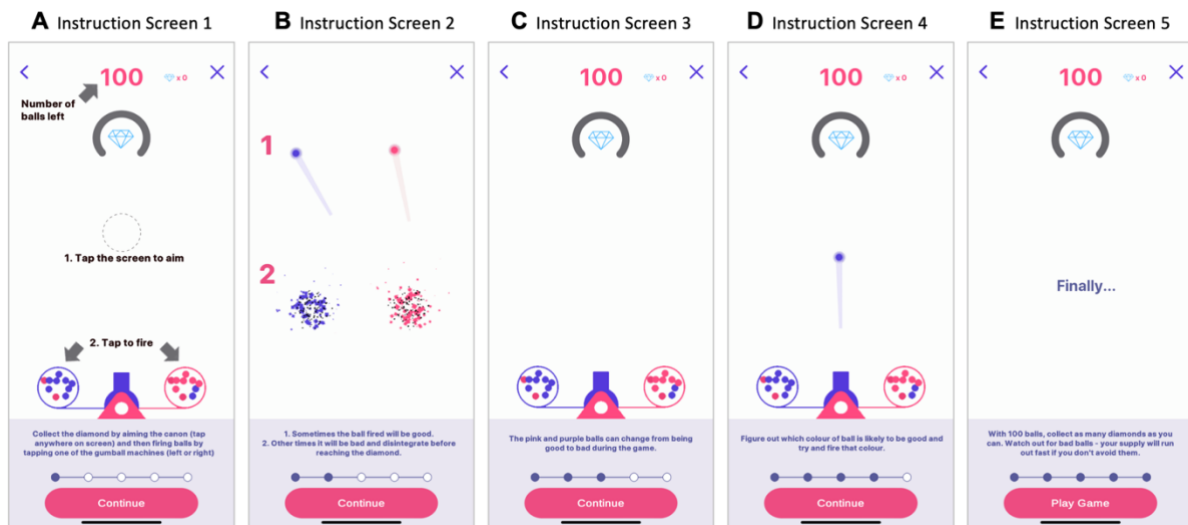
Est = Beta estimate, SE = standard error, z = test statistic, p = significance, CI=confidence interval, CIT = Compulsivity

Supplementary Table 7.1.2.19 Candidate Model Comparison and Evaluation for Hierarchical Bayesian Models

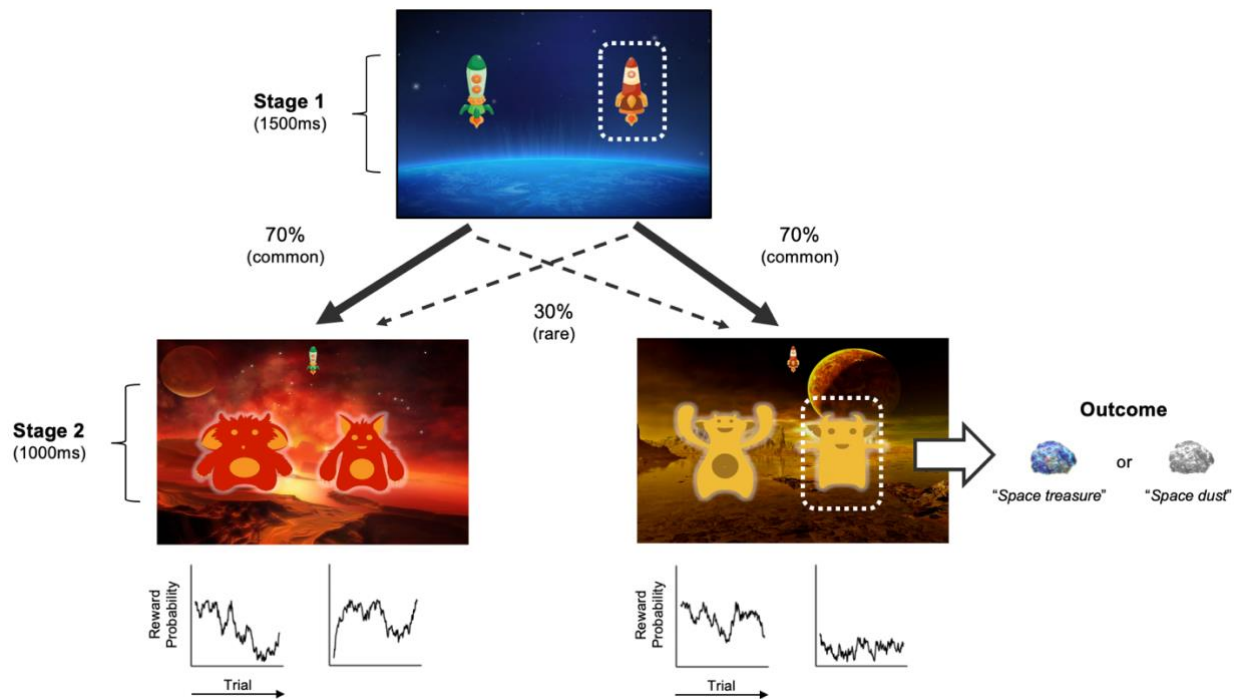
Model Name		A	B	C	D	E	F	G
MCMC diagnostics								
Divergence		78	371	0	0	1	2	0
All rhats <1.05		NO	YES	YES	YES	YES	YES	YES
LOOIC		17283	16637	17310	16486	16495	16565	16533
PPC								
Mean % agree		.81	.82	.81	.82	.82	.82	.82
Median % agree		.82	.84	.82	.84	.84	.84	.84
Prop. over 80% agree		.59	.66	.59	.63	.64	.64	.60
Min		.59	.53	.60	.60	.58	.59	.64
Max		.96	.96	.96	.96	.96	.96	.97
Parameter Recovery								
ρ	Perseveration	.96	.96	.96	.97	.97	.97	.97
α_1	1st stage LR	.73	.82	.79	.87	.86	.79	-
α_2	2 nd stage LR	.64	-	-	-	-	-	-
β_1	Inverse temp.	.74	-	-	-	-	-	-
ω	MB/MF weight	.63	-	-	-	-	-	-
β_{MB}	Model-based β	-	.74	.70	.71	.72	.74	.73
β_{MF}	Model-free β	-	.78	.65	.75	.78	.82	.81
α_D	Decay rate	-	-	-	-	.35	-	-
α_+	1 st stage LR+	-	-	-	-	-	-	.75
α_-	1 st stage LR-	-	-	-	-	-	-	.79

All models were run on $n = 100$ subjects (*simulated agents* for parameter recovery), with models estimated from 4K iterations. For each row, the best performing model (or joint-best) is highlighted in grey.

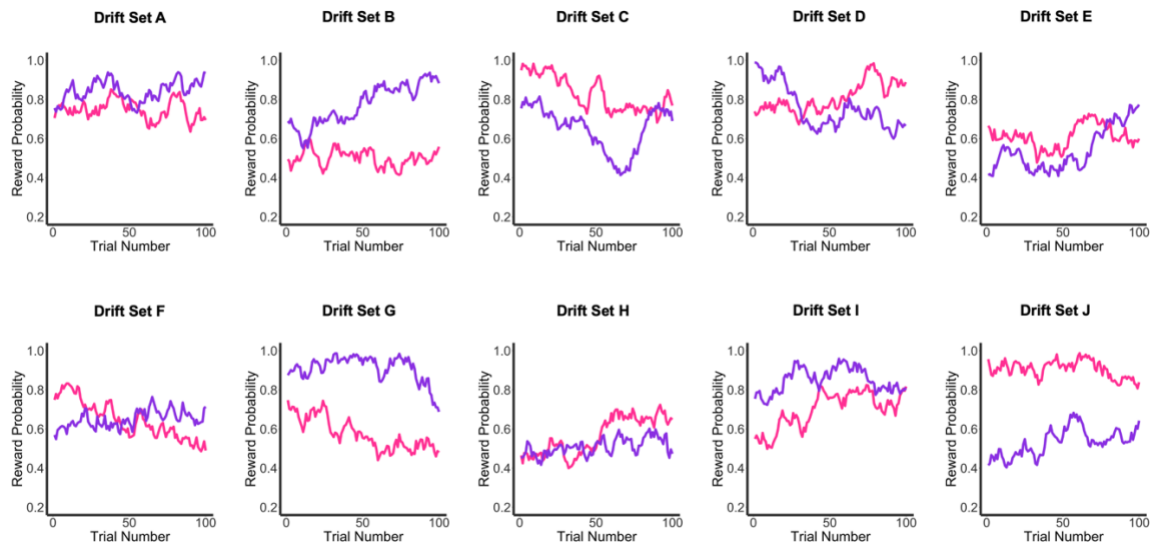
7.1.3 Supplementary Figures



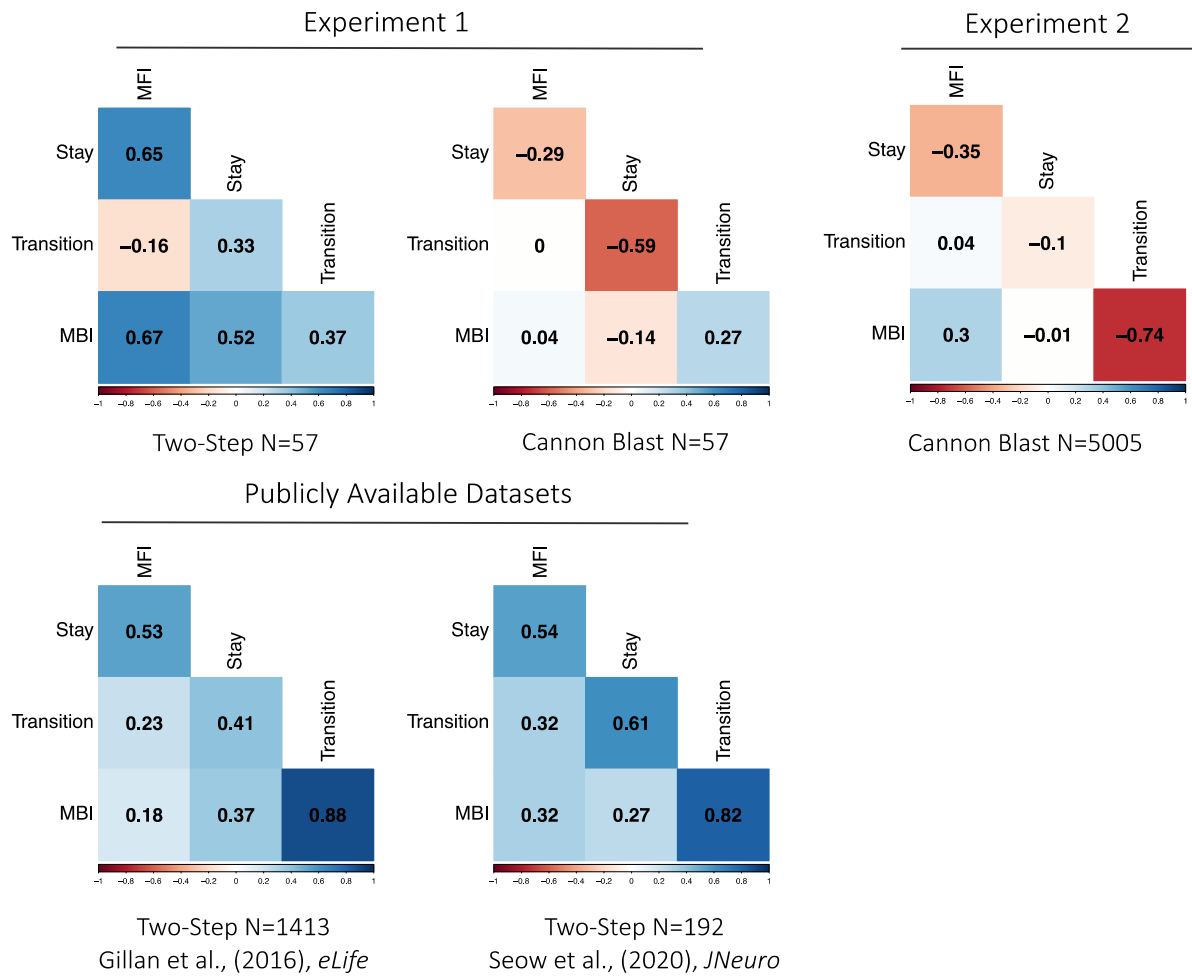
Supplementary Figure 7.1.3.1 Passive instructions from Cannon Blast shown to participants. **A.** Instructs participants of the aim of Cannon Blast (i.e., to collect diamonds). Demonstrates visually the steps a participant must take: first tapping the screen to aim the cannon and second to tap a coloured container to load a ball. **B.** Participants are made aware that they will either be rewarded with a good ball or unrewarded with a bad ball that disintegrates upon firing. **C.** Participants are told of the drifting nature of the ball quality over the course of the game. **D.** Participants are instructed to figure out which ball colour is likely to be rewarding and to select this colour. **E.** Participants are explicitly told of the finite source of balls (100) and to collect as many diamonds as possible by trying to avoid bad balls.



Supplementary Figure 7.1.3.2 Two-step reinforcement learning task for assessing model-based learning. This paradigm (adapted by (Decker et al., 2016)) consisted of two stages where in the first stage subjects chose between two rockets which probabilistically transitioned to one of two second stage planets. Each rocket travelled to a preferred planet 70% of the time ('common' transition) or to the alternative planet 30% of the time ('rare' transition). In the second stage, subjects had to choose between two aliens, both with a unique probability of being rewarding ('space treasure') or unrewarding ('space dust') that drifted slowly and independently over the course of the experiment in pre-determined trajectories. Participants had to simultaneously track the distinct reward probabilities of each alien and incorporate knowledge of the transition structure in order to maximise their chance of reward.



Supplementary Figure 7.1.3.3. Reward trajectories for each of the ten drifting reward probabilities sets. On participants first play ('Risk Factors') participants were randomly assigned to receive either Drift Set A or Drift Set B at each block of trials. On participants repeated plays ('Free Play'), participants were randomly assigned to receive one out of the possible ten drifts (A – J).



Supplementary Figure 7.1.3.4. Within-task correlations in our dataset and other publicly available datasets. Within-task correlations of task outcomes including choice preservation ('Stay'), transition, model-based (MBI) and model-free (MFI) in our data sets (top graphs) and in publicly available datasets (bottom graphs). There are differences between the within-task correlations from our dataset to other data sets using the classic two-step task i.e., the correlation between MBI and MFI. Within task correlations in our dataset from Cannon Blast are replicated in the larger citizen science sample.

7.1.4 Supplementary Notes

Supplementary Note 7.1.4.1 Information Sheet for The Neureka project In-App

The Neureka app comes from a group of scientists at the Global Brain Health Institute who are trying to uncover new ways to detect and prevent disorders of brain health. The brain is a big mystery and we need the help of people like you to solve it.

At Neureka, we think that everyone should be able to participate in science, that's we are taking experiments out of the lab, and into your smartphone. Our team have been hard at work for the past year making games that are not only fun to play, but help us learn about the brain at the same time.

We aim to use your anonymous data, combined with data from other players around the world, to conduct one of the biggest neuroscience studies of all time!

What will happen if I agree to take part?

You will be asked to provide an email-address and a password in order to register an account on the app. We need your email address so that you can sign into the app across different devices and so that we can contact you in future about your contribution to science and any future studies you may want to participate in. You can turn email communication off at any time by changing your settings.

After providing your email you will be asked to complete some basic demographic information and to complete a 'science challenge', where you will play games that tell us things about how your brain works and provide us with some personal information about your physical and mental health, lifestyle and family history. Once complete, you will be free to play the games whenever you like and to complete additional science challenges, at your leisure.

How long will it take to participate?

To complete science challenge 1, it will take approximately 1 hour and it doesn't need to be completed in one go. After that, you can play each game as much or as little as you like. Likewise, you can provide us with as much information about yourself as you like in our 'about me' section. Other science challenges may take longer, with some of them asking you to play the games according to a schedule for days, weeks or even months. These are entirely optional.

What data will I be sharing?

Your email address, the type of device on which you used the app (e.g. iPhone 6), your performance on the games that you choose to play, the demographic information you will be asked to provide during sign-up and any questionnaires that you complete within the app.

What will happen to my data?

We will use the data you provide us from within the app for academic research only. We will share fully anonymized data with other university researchers to help with the collective research effort of improving early detection of dementia and mental health disorders. We will never share personally identifiable information, such as your email address, with anyone. We will publish the findings that your contribution has helped generate in scientific journals and make them available for you to read and share online at www.neureka.ie.

Can I access my data?

You are also free to download all data that you have submitted through the app by tapping "settings"-"GDPR"-"download my data". A password will be required to do this to ensure your privacy and security and data will be sent to your personal email address provided at the time of sign up. Under the Freedom of Information Act 2014, you will have access to any data you provide through the app for up to 10 years after you have provided it, up until the point at which it has been fully anonymised and we can no longer recover your unique contribution.

Is it confidential?

Yes. We will not disclose any personally identifiable information (e.g. your unencrypted email address) to any third party. However, there are legal limits that apply to confidentiality in the Republic of Ireland, specifically if you provide unsolicited information to the research team pertaining to risk of harm to yourself or another person, this additional information is not protected by confidentiality and may be disclosed to the relevant authorities. Secondly, any information that you provide (solicited or unsolicited) may be disclosed as part of a legal process or police investigation in the Republic of Ireland, without your permission being sought.

Is it secure?

All data you provide through the app is encrypted before it is submitted over the internet. The data you send us through this app is stored and processed in accordance with EU General Data Protection Regulation (GDPR) with our procedures for storing and processing data having undergone and passed review by the data protection officer at Trinity College Dublin.

Gaining credit through SciStarter

We recently partnered with SciStarter, a website that hosts really cool citizen science projects like neureka. If you hold a SciStarter account under the same email as you have registered with neureka, you will automatically receive credit for that on SciStarter. This "linking" of information for registered SciStarter users is done in a fully encrypted way.

Connecting to other studies

Neureka is a powerful tool for engaging people in research, allowing study participants to take part at home and from all over the world. We want to share it with other researchers to help facilitate other important studies, allowing them to compare the same tasks and tools in different populations. If you are part of a study that is using Neureka to gather data, you will be asked by the researchers to enter a Study ID in the settings section of the app. They will provide you with this ID. By providing us with a valid ID, you agree for us to share your data with that research team. The data that we will share with approved studies are specific. That means that only relevant data are shared as outlined in the specific consent you have completed for that study, e.g. scores on a questionnaire, or data from one of our games and never contain any identifiable information (e.g. email address are never shared).

Do I have to take part in the research to use the app?

If you decide that you don't want to take part in the research then you should go to the "settings"->"GDPR" screen (accessed through the settings button in the top right of the app) and deselect "participate in research". This will stop any new data from your device being submitted to our server. Your use of the app won't be affected by this. If you change your mind you can reselect "participate in research" at any time.

How do I withdraw (i.e., get out) of taking part in the research?

Taking part in this research is completely voluntary. You can stop taking part in the research at any time using the process described above to stop sending any new data from your device to our server. If you want to delete data that you have previously submitted to us from the app then you can email the research team by clicking on the "settings "->"GDPR" and selecting "erase data". This will send us an email to delete your data and your data will be deleted from the server and our off-line data store held on a password protected computer in Trinity College Dublin. You will be notified by email once we have completed the process of deleting your data. The only time we won't be able to delete your data is if we have already performed analyses on it and a scientific report/journal article using this data has been submitted and/or if data has already been fully anonymised.

How much Mobile Data (i.e. smartphone access to the internet without wifi) does the app use?

Not very much. The average user will use less than 3 megabytes in total during their time using the app. This doesn't include the data used to download or update the app.

Are there any risks involved in participating?

The risks associated with participating in this research are minimal. However, it is possible that you might find the sensitive nature of some of the questions in questionnaires that you might choose to complete upsetting. If you experience upset or distress we encourage you to contact your General Practitioner/Doctor. We also provide information on support services in a dedicated tab within the app.

Are there any benefits to participating?

This study will not benefit you in any direct way. The data that we collect from you will hopefully allow us to develop a tool to better detect early cognitive changes that are markers of future risk of developing dementia and mental health disorders and in this way your contribution might benefit others.

Will the research team take responsibility for diagnosing app users with dementia or mental health disorders?

No. The games within the app are not diagnostic in isolation and your basic performance level is not indicative of risk for disorders of brain health. Users will not be provided with any processed scores, or individualised predictions on risk of dementia or mental health disorders based on the data that they provide through the app. While we hope that one day, some combination of these measures might help us to detect illness early – the science is simply not there yet.

Who is running this study?

The study is being conducted by a research team based at the Global Brain Health Institute at Trinity College Dublin in the Republic of Ireland. The study has received ethical approval from the School of Psychology Research Ethics Committee at Trinity College Dublin.

In taking part and providing consent, you are agreeing to participate in a study that handles your data in line with the Republic of Ireland Data Protection Act 2018 which is in line with the EU General Data Protection Regulation (GDPR) 2016/679. For information on Data Protection law in Ireland see <https://dataprotection.ie/docs/A-guide-to-your-rights-Plain-English-Version/r/858.htm>

7.1.5 Supplementary References

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7.2 Supplementary Materials for Chapter 3

7.2.1 Supplementary Methods and Analyses

7.2.1.1 Exclusion criteria for both studies

In line with previous studies, in both experiments we excluded participants who (a) missed more than 20% of trials and (b) pressed the same key on >95% of trials (Supplementary Table 7.2.2.2). Across both experiments, only those who had task and clinical questionnaire data at baseline and follow-up were included for analyses. Post exclusion criteria, $N = 93$ participants ($N = 65$ ketamine: $N = 28$ saline control) remained in Experiment 1, and $N = 781$ remained for analysis in Experiment 2 ($N = 87$ control: $N = 83$ antidepressants arm: $N = 611$ iCBT).

7.2.1.2 Experiment 2: Precision in Psychiatry Study

Self-report questionnaires

In order to test the transdiagnostic benefits of common therapies for depression in the parent observational treatment study (Lee et al., 2021), clinical changes were assessed through a range of self-report questionnaires completed at baseline and 4-week follow-up. These measures assessed apathy (Apathy Evaluation Scale, (Marin et al., 1991)), alcohol dependency (Alcohol Use Disorder Identification Test, (Saunders et al., 1993)), depression (Self-rated Depression Scale, (Zung, 1965)), eating disorder (Eating Attitudes Test, (Garner et al., 1982)), impulsivity (Barratt Impulsivity Scale, (Patton et al., 1995)), OCD (Obsessive Compulsive Inventory Revised, (Foa et al., 2002)), schizotypy (Short Scale for Measuring Schizotypy, (Mason et al., 2005)), social anxiety (Liebowitz Social Anxiety Scale, (Liebowitz, 1987)), and trait anxiety (State-Trait Anxiety Inventory Trait Subscale, (Spielberger, 1983)). These instruments allow for the estimation of three transdiagnostic dimensions (anxious-depression, compulsivity and intrusive thought, and social withdrawal) where in the individual item-level responses are weighted based on factor loadings identified in a prior factor analysis of these data (Gillan et al., 2016). The anxious-depression and compulsivity and intrusive thought dimensions have been shown to map onto aspects of cognition, such as model-based planning, better than individual questionnaire totals and so were used as the primary outcome measures for this study. For a detailed description of item loadings please see Gillan

et al., (2016), but in brief the anxious-depression factor is comprised of items from both anxiety and depression scales rather than reflecting an anxious subtype of depression. The factor defined compulsivity and intrusive thought had loadings from items from the OCD, eating disorder, impulsivity and schizotypy scales.

7.2.1.3 Individual differences in model-based planning

The full model output is reported in Supplementary Table 7.2.2.13, here we will only report results of interest. In terms of demographics, a negative association between model-based planning and age was observed in both treatment groups at baseline (Antidepressant: $\beta = -.07$, $SE = .04$, $p = .062$, iCBT: $\beta = -.06$, $SE = .02$, $p = .001$, Controls: $p = .495$), which remained significant at 4-week follow-up in the iCBT group only (iCBT: $\beta = -.05$, $SE = .02$, $p = .004$, Antidepressant: $p = .835$, Controls: $p = .772$). There was a significant effect of gender on model-based planning in the antidepressant group at baseline ($\beta = .14$, $SE = .04$, $p = .001$). No other significant gender differences were found. A positive association between model-based planning and education was found at baseline in the control and iCBT group (Controls: $\beta = .10$, $SE = .04$, $p = .004$; iCBT: $\beta = .03$, $SE = .01$, $p = .002$), which trended significance at 4-week follow-up in the iCBT group (iCBT: $\beta = .05$, $SE = .02$, $p = .056$, Controls: $p = .373$). There were no significant associations between model-based planning and education in the antidepressant group (Baseline: $p = .431$, Follow-up: $p = .520$). In terms of clinical differences, we failed to find an association at baseline between model-based planning and anxious-depression in all three groups (Controls: $\beta = .04$, $SE = .06$, $p = .520$, Antidepressant: $\beta = .04$, $SE = .04$, $p = .285$, iCBT: $\beta = .00$, $SE = .02$, $p = .942$, Supplementary Table 7.2.2.13). Associations with compulsivity are reported in the main manuscript.

7.2.1.4 Are individual differences in model-based planning at baseline related to changes in compulsivity following treatment in the PIP study?

Prior work has shown these common forms of treatment for anxiety and depression can have a transdiagnostic therapeutic effect, including a reduction in compulsivity (Lee et al., 2021). We therefore repeated the analyses from the main paper, but focused on compulsivity scores instead of anxious-depression. However, these results should be taken with a degree of caution as the therapeutic dose of antidepressant medications for OCD is larger than for depression and iCBT does

not include gold-standard treatments for compulsivity such as exposure and response prevention. It follows that improvements in compulsivity in the PIP study were considerably smaller than improvements in anxious depression.

To test if model-based planning scores at baseline moderated the effect of treatment on compulsivity, we used a repeated measures regression with compulsivity scores as the dependent variable, and session, group and baseline MBI as independent variables (Supplementary Table 7.2.2.15). Taking the control group as the reference, the antidepressant ($\beta = .99$, $SE = .17$, $p < .001$) and iCBT group ($\beta = .61$, $SE = .12$, $p < .001$) had higher levels of compulsivity compared to controls. There was a main effect of session ($\beta = -.17$, $SE = .06$, $p = .007$), reflecting a reduction in compulsivity scores from baseline to follow-up in the control group (Supplementary Figure 7.2.3.1A). This trended to be greater in the antidepressant group compared to controls ($\beta = -.19$, $SE = .10$, $p = .051$) but not iCBT vs controls ($\beta = -.10$, $SE = .07$, $p = .143$). Tests of simple effects confirmed that compulsivity scores significantly reduced in those who received 4-weeks of antidepressant medication ($\beta = -.34$, $SE = .06$, $p < .00$, Supplementary Table 7.2.2.16, Supplementary Figure 7.2.3.1A) as well as those who received iCBT treatment ($\beta = -.28$, $SE = .02$, $p < .001$, Supplementary Table 7.2.2.16, Supplementary Figure 7.2.3.1A). Model-based planning scores at baseline did not moderate the effect of session, session*MBI ($\beta = .27$, $SE = .15$, $p = .067$) or in interactions with antidepressant group ($\beta = -.06$, $SE = .27$, $p = .824$) or iCBT ($\beta = -.09$, $SE = .17$, $p = .599$).

7.1.2.5 Do changes in model-based planning relate to changes in compulsivity over 4 weeks?

We conducted a repeated measures ANOVA with model-based planning as the dependent variable and session, group and compulsivity changes, and their interaction, as the IVs. Consistent with the results from the main paper, there was no main effect of session ($p = .092$) on model-based planning scores or no interaction between group and session (all $p > .104$). Looking at each group individually, there was no interaction between session and compulsivity changes in the antidepressant ($\beta = -.05$, $SE = .06$, $p = .459$) or iCBT groups ($\beta = .02$, $SE = .02$, $p = .298$). There was an interaction between session and compulsivity changes in the control group ($\beta = .16$, $SE = .07$, $p = .020$) such that bigger improvements in

compulsivity were linked to bigger improvements in model-based planning. These findings were recapitulated in simple correlations between change in model-based planning and changes in compulsivity, with a negative correlation observed in the control group ($r = -.25, p = .020$), but not in the antidepressant ($r = .08, p = .459$), nor iCBT ($r = -.04, p = .298$) group (Supplementary Figure 7.2.3.1B). $n = 3$ outliers were identified in the control group and when analyses were run excluding these outliers, the interaction between session and compulsivity changes in the control group was no longer significant ($\beta = .05, SE = .03, p = .097$). The correlation between changes in compulsivity and changes in model-based planning was also no longer significant ($r = -.21, p = .051$).

7.2.2 Supplementary Tables

Supplementary Table 7.2.2.1 Demographic and clinical characteristics at baseline

<u>Experiment 1</u>	Ketamine	Saline	F/X ²	p	
N	65	28			
Age, M(SD)	33.75(1.57)	32.46 (9.87)	-.55	.580	
Sex, No. (%)			.20	.650	
Male	24 (36.9%)	9 (32.1%)			
Female	41 (63.1%)	19 (67.9%)			
Highest level of education			.75	.688	
Below undergraduate	2 (3.1%)	1 (3.6%)			
Undergraduate	46 (7.8%)	22 (78.6%)			
Above undergraduate	17 (26.2%)	5 (17.9%)			
<u>Experiment 2</u>	Control	Antidepressant	iCBT	F/X ²	p
N	87	83	611		
Age, M(SD)	28.47 (11.86)	3.79 (1.93)	31.53 (1.83)	2.99	.051
Gender, No. (%)				1.49	.474
Male	21(24%)	22 (27%)	129 (21%)		
Female	66 (76%)	61 (73%)	481 (79%)		
Highest level of education				7.29	.121
Below undergraduate	14 (16%)	13 (15%)	140 (23%)		
Undergraduate	58 (67%)	49 (59%)	326 (53%)		
Above undergraduate	15 (17%)	21 (25%)	145 (24%)		

Note: Highest level of education was scored numerically from 0 - No formal education to 10 - Doctoral degree. Education is treated as a numerical continuous variable in the models. F=test statistic, X²=Chi Square, p = significance, M = mean, SD = standard deviation, iCBT = internet-based cognitive behavioural therapy

Supplementary Table 7.2.2.2 Task exclusion criteria

	<u>Experiment 1</u>						<u>Experiment 2</u>			
	Ketamine		Saline		Antidepressant		Control		iCBT	
	BL	FU	BL	FU	BL	FU	BL	FU	BL	FU
Missed more than 20% trials	0	1	0	0	4	1	1	3	14	23
Choose the same side on more than 95% trials	4	3	2	5	2	5	1	2	10	32

BL=baseline, FU=follow-up, iCBT = internet-based cognitive behavioural therapy

Supplementary Table 7.2.2.3 Hierarchical regression model predicting stay from reward, transition, session, group (Ketamine vs Placebo) and their interactions.

	Est.	Std. Error	z	p
Intercept	1.40	.23	6.22	<.001
Reward	.44	.12	6.22	<.001
Transition	-.13	.67	-2.01	.044
Session	-.10	.08	-1.38	.167
Group	.06	.27	.21	.830
Reward * Transition	.16	.07	2.17	.030
Reward * Session	.14	.08	1.89	.058
Reward * Group	-.08	.15	-.57	.570
Transition * Session	.04	.08	.54	.586
Transition * Group	.17	.08	2.11	.034
Session * Group	.05	.09	.51	.612
Reward * Transition * Session	-.12	.08	-1.64	.100
Reward * Transition * Group	.03	.09	.38	.708
Reward * Session * Group	-.08	.09	-.84	.398
Transition * Session * Group	.01	.09	.09	.926
Reward * Transition * Session * Group	.11	.09	1.22	.222

N = 93

Model: $\text{lm}(\text{stay} \sim \text{reward}_{t-1} * \text{transition}_{t-1} * \text{session} * \text{group} + (\text{reward}_{t-1} * \text{transition}_{t-1} + 1 \mid \text{id}))$

Session coded as a factor: baseline, follow-up

Group coded as a factor (saline control, ketamine) with saline control as reference

Est = Beta estimate, SE = standard error, z = test statistic, p = significance

Supplementary Table 7.2.2.4 Linear mixed effect model to predict model-based planning from interactions between session and change in depression severity, controlling for depression severity at baseline within group

	Est.	Std. Error	t	p
Saline, N = 28				
Intercept	-.12	.17	-.71	.485
Session	-.08	.06	-1.18	.248
Session*MADRS change	.00	.01	1.24	.226
MADRS baseline	-.00	.00	-.55	.583
Ketamine, N = 65				
Intercept	.30	.15	2.02	.047
Session	-.06	.06	-.96	.340
Session*MADRS change	-.00	.00	-.85	.339
MADRS baseline	.00	.00	-.33	.742

Model: model-based score ~ session * MADRS change + MADRS baseline

Session coded as factor: baseline, follow-up

MADRS= Montgomery-Åsberg Depression Rating Scale

Est = Beta estimate, SE = standard error, z = test statistic, p = significance

Supplementary Table 7.2.2.5 Repeated measures model testing if changes in depression severity are influenced by session, group or baseline model-based estimates.

	Est.	Std. Error	t	p
Intercept	31.94	1.42	22.37	<.001
Session	-7.89	1.57	-5.03	<.001
Group	.49	1.70	.28	.776
Baseline model-based score	1.11	1.30	.85	.397
Session*Group	-9.14	1.87	-4.89	<.001
Session* Baseline model-based score	-.71	1.43	-.49	.620
Group* Baseline model-based score	-1.04	1.63	-.64	.525
Session*Group* Baseline model-based score	2.40	1.79	1.34	.183

N = 93

Full Model: MADRS ~ session * group * baseline model-based index + (1 | id)

Session coded as a factor with levels: baseline, follow-up

Group coded as factor with saline control as reference

Est = Beta estimate, SE = standard error, z = test statistic, p = significance

Supplementary Table 7.2.2.6 Statistical changes in mean estimates of depression severity scores and model-based planning from pre infusion to 24 hours post infusion per group

		<u>Pre infusion</u>	<u>Post infusion</u>			
	N	M(SD)	M(SD)	Est. (SE)	t	p
Saline	28	31.79 (.90)	24.00 (1.51)	-7.79 (1.75)	-4.44	<.001
Ketamine	65	32.43 (.69)	15.51 (1.19)	-16.92 (1.38)	-12.28	<.001

Model: MADRS ~ session + (1 | id)

Session coded as: pre infusion=0, 24 hours post infusion=1

Est = Beta estimate, SE = standard error, t = test statistic, p = significance, M = mean, SE = standard error, MADRS= Montgomery-Åsberg Depression Rating Scale

Supplementary Table 7.2.2.7 Hierarchical mixed effects model testing if model-based planning is influenced by group, session or their interactions

	Est.	Std. Error	z	p
Intercept	1.26	.10	12.59	<.001
Reward	.30	.05	6.16	<.001
Transition	.03	.03	.93	.354
Antidepressant	-.10	.14	-.73	.465
iCBT	-.17	.11	-1.61	.107
Session	.17	.03	4.88	<.001
Reward*Transition	.23	.07	5.34	<.001
Reward*Antidepressant	-.08	.05	-1.11	.266
Reward*iCBT	-.06	.04	-1.30	.193
Transition*Antidepressant	.00	.03	.12	.905
Transition*iCBT	-.00	.03	-.02	.985
Reward*Session	.14	.03	3.99	<.001
Transition*Session	.06	.05	1.85	.065
Antidepressant*Session	-.05	.04	-.99	.324
iCBT*Session	-.04	.06	1.19	.234
Reward*Transition*Antidepressant	.01	.05	.10	.925
Reward*Transition*iCBT	-.05	.03	-1.09	.274
Reward*Transition*Session	.04	.05	1.30	.193
Reward*Antidepressant*Session	-.05	.04	-1.01	.309
Reward*iCBT*Session	-.07	.05	-1.90	.057
Transition*Antidepressant*Session	.00	.04	.09	.929
Transition*iCBT*Session	-.06	.04	-1.63	.103
Reward*Transition*Antidepressant*Session	-.03	.05	-.69	.492
Reward*Transition*iCBT*Session	-.05	.04	-1.36	.176

N = 781
Model: $\text{lm}(\text{stay} \sim \text{reward}_{t-1} * \text{transition}_{t-1} * \text{session} * \text{group} + (\text{reward}_{t-1} * \text{transition}_{t-1} + 1 \mid \text{id}))$
Session coded as a factor with levels: baseline, follow-up
Group coded as factor with 'Control' as reference
Est = Beta estimate, Std. error = standard error, z = test statistic, p = significance, iCBT = internet based cognitive behavioural therapy

Supplementary Table 7.2.2.8 Independent regression models testing if model-based planning is influenced by session within each group

	Est.	Std. Error	z	p
<u>Controls, n = 87</u>				
Intercept	1.28	.11	11.65	<.001
Reward	.32	.05	5.78	<.001
Transition	.04	.04	.91	.361
Session	.17	.03	4.86	<.001
Reward*Transition	.24	.05	4.59	<.001
Reward*Session	.14	.03	4.01	<.001
Transition*Session	.07	.03	1.91	.057
Reward*Transition*Session	.05	.03	1.36	.174
<u>Antidepressant, n = 83</u>				
Intercept	1.15	.09	12.09	<.001
Reward	.22	.05	4.58	<.001
Transition	.05	.03	1.67	.094
Session	.12	.03	3.57	<.001
Reward*Transition	.25	.05	5.39	<.001
Reward*Session	.09	.03	2.61	.009
Transition*Session	.07	.03	2.00	.045
Reward*Transition*Session	.01	.03	.36	.721
<u>iCBT, n = 611</u>				
Intercept	1.08	.04	28.04	<.001
Reward	.23	.02	12.61	<.001
Transition	.02	.01	2.18	.029
Session	.12	.01	9.91	<.001
Reward*Transition	.17	.02	11.24	<.001
Reward*Session	.07	.01	5.41	<.001
Transition*Session	.00	.01	.30	.766
Reward*Transition*Session	-.01	.01	-.40	.687
Model: $\text{stay} \sim \text{reward}_{t-1} * \text{transition}_{t-1} * \text{session} + (\text{reward}_{t-1} * \text{transition}_{t-1} \pm 1 \text{id})$ Session coded as a factor with levels: baseline, follow-up Est = Beta estimate, Std. error = standard error, z = test statistic, p = significance, iCBT = internet based cognitive behavioural therapy				

Supplementary Table 7.2.2.9 Repeated measures model testing if model-based planning is influenced by session, group or changes in anxious depression.

	Est.	Std. Error	t	p
Intercept	.23	.03	7.57	<.001
Session	.07	.03	2.42	.016
AD Change	.00	.05	.05	.963
Antidepressant	-.00	.05	-.08	.942
iCBT	-.06	.03	-1.66	.097
Session*AD Change	.04	.05	.74	.463
Session*Antidepressant	-.01	.04	-.22	.830
Session*iCBT	-.07	.03	-2.38	.018
AD Change*Antidepressant	-.01	.06	-.11	.915
AD Change*iCBT	-.03	.06	-.49	.621
Session*AD Change*Antidepressant	-.06	.06	-1.07	.284
Session*AD Change*iCBT	-.01	.05	-.25	.806

N = 781
Model: model-based index ~ session * AD change * group (+1 | id)
Session coded as a factor with levels: baseline, follow-up
Group coded as factor with 'Control' as reference
Est = Beta estimate, Std. error = standard error, t = test statistic, p = significance, iCBT = internet based cognitive behavioural therapy, AD Change = anxious-depression change

Supplementary Table 7.2.2.10 Independent repeated measures model testing if model-based planning is influenced by session or changes in anxious depression within group

	Est.	Std. Error	t	p
Controls, <i>n</i> = 87				
Intercept	.23	.04	5.72	<.001
Session	.07	.04	1.93	.057
AD Change	.00	.04	.04	.972
Session*AD Change	.04	.06	.59	.559
Antidepressant, <i>n</i> = 83				
Intercept	.23	.04	5.54	<.001
Session	.06	.04	1.44	.153
AD Change	-.00	.04	-.11	.916
Session*AD Change	.03	.04	.67	.506
iCBT, <i>n</i> = 611				
Intercept	.18	.01	15.04	<.001
Session	-.00	.01	-.41	.682
AD Change	-.03	.02	-1.51	.132
Session*AD Change	.02	.02	1.50	.133
Independent models: model-based score ~ session * AD change (+1 id) Session coded as a factor with levels: baseline, follow-up Est = Beta estimate, Std. error = standard error, t = test statistic, p = significance, iCBT = internet based cognitive behavioural therapy, AD Change = anxious-depression change				

Supplementary Table 7.2.2.11 Repeated measures model testing if changes in anxious depression are influenced by session, group or baseline model-based estimates

	Est.	Std. Error	t	p
Intercept	-1.25	.10	-12.48	<.001
Session	.10	.08	1.24	.217
Antidepressant	1.84	.15	11.89	<.001
iCBT	1.52	.11	14.11	<.001
Baseline model-based score	-.22	.23	-.96	.337
Session*Antidepressant	-.62	.13	-4.83	<.001
Session*iCBT	-.45	.09	-5.03	<.001
Session*Baseline model-based score	-.01	.19	-.03	.975
Antidepressant*Baseline model-based score	.16	.42	.39	.700
iCBT*Baseline model-based score	.14	.27	.53	.595
Session*Antidepressant*Baseline model-based score	.06	.35	.17	.867
Session*iCBT*Baseline model-based score	.17	.22	.78	.433

n = 781

Full Model: Anxious-depression ~ session * group * baseline model-based score + (1 | id)

Session coded as a factor with levels: baseline, follow-up

Group coded as factor with 'Control' as reference

Est = Beta estimate, Std. error = standard error, t = test statistic, p = significance, iCBT = internet based cognitive behavioural therapy

Supplementary Table 7.2.2.12 Mean estimates of anxious-depression scores and statistical changes from baseline to follow-up within group

		<u>Baseline</u>	<u>Follow-up</u>			
	N	M (SD)	M (SD)	Est. (SE)	t	p
Control	87	-1.30 (.77)	-1.20 (.87)	.12 (.08)	1.65	.103
Antidepressant	83	.57 (.70)	.06 (.93)	-.59 (.11)	-5.33	<.001
iCBT	611	.25 (.72)	-.06 (.83)	-.40 (.03)	-12.42	<.001

Model: $\text{lm}(\text{AD} \sim \text{session} + (1 | \text{id}))$

Session coded as a factor with levels: baseline, follow-up

M = mean, SD = standard deviation, Est = Beta estimate, SE = standard error, t = test statistic, p = significance, iCBT = internet based cognitive behavioural therapy

Supplementary Table 7.2.2.13 Independent models predicting stay by reward, transition, anxious-depression and compulsivity, while controlling for covariates of age, gender and education within each group and at each time point

	Est.	Std. Error	t	p
Independent models at baseline				
<u>Controls, n = 87</u>				
Intercept	.18	.52	.34	.732
Reward	-.44	.25	-1.75	.080
Transition	-.25	.18	-1.37	.171
AD	.13	.12	1.09	.274
CIT	-.15	.12	-1.18	.237
Age	.15	.12	1.30	.192
Gender	-.19	.13	-1.48	.138
Education	.16	.08	2.03	.042
Reward * Transition	-.41	.24	-1.72	.085
Reward * AD	-.03	.06	-.47	.635
Reward * CIT	-.08	.06	-1.39	.166
Transition * AD	-.04	.04	-.90	.370
Transition * CIT	-.05	.04	-1.07	.286
Reward * Age	.08	.06	1.42	.155
Transition * Age	-.05	.04	-1.21	.226
Reward * Gender	-.12	.06	-2.07	.038
Transition * Gender	-.01	.04	-.16	.876
Reward * Education	.11	.04	2.87	.004
Transition * Education	.05	.03	1.64	.102
Reward * Transition * AD	.04	.06	.64	.520
Reward * Transition * CIT	-.12	.06	-2.15	.031
Reward * Transition * Age	-.04	.05	-.68	.495
Reward * Transition * Gender	.04	.06	.71	.478
Reward * Transition * Education	.10	.04	2.87	.004
<u>Antidepressants, n = 83</u>				
Intercept	1.16	.37	3.11	.002
Reward	.32	.22	1.49	.137
Transition	.01	.11	.08	.935
AD	-.18	.10	-1.93	.054
CIT	-.18	.10	-1.84	.066
Age	.18	.10	1.85	.065

Gender	.20	.11	1.88	.060
Education	.02	.05	.32	.750
Reward * Transition	.21	.15	1.40	.162
Reward * AD	-.09	.06	-1.52	.127
Reward * CIT	-.06	.06	-1.02	.310
Transition * AD	.04	.03	1.45	.148
Transition * CIT	.02	.03	.58	.562
Reward * Age	-.02	.06	-.38	.705
Transition * Age	-.02	.03	-.57	.568
Reward * Gender	.06	.06	.91	.361
Transition * Gender	.02	.03	.60	.547
Reward * Education	-.01	.03	-.25	.804
Transition * Education	.01	.02	.39	.697
Reward * Transition * AD	.04	.04	1.07	.285
Reward * Transition * CIT	-.08	.04	-1.94	.053
Reward * Transition * Age	-.07	.04	-1.86	.062
Reward * Transition * Gender	.14	.04	3.20	.001
Reward * Transition * Education	.01	.02	.66	.512
<i>iCBT, n = 611</i>				
Intercept	.35	.16	2.11	.035
Reward	.13	.08	1.65	.098
Transition	.02	.04	.43	.670
AD	.02	.04	.50	.616
CIT	-.15	.04	-3.58	<.001
Age	.20	.04	4.73	<.001
Gender	-.06	.05	-1.12	.262
Education	.11	.02	4.65	<.001
Reward * Transition	-.02	.06	-.26	.799
Reward * AD	.01	.02	.45	.654
Reward * CIT	-.06	.02	-3.12	.002
Transition * AD	.01	.01	.49	.623
Transition * CIT	-.02	.01	-1.81	.070
Reward * Age	.01	.02	.45	.651
Transition * Age	.00	.01	-.39	.695
Reward * Gender	-.02	.02	-.76	.447
Transition * Gender	.01	.01	.39	.697
Reward * Education	.02	.01	1.28	.200
Transition * Education	.00	.01	.22	.827
Reward * Transition * AD	.00	.02	.07	.942

Reward * Transition * CIT	-.05	.02	-3.05	.002
Reward * Transition * Age	-.05	.02	-3.22	.001
Reward * Transition * Gender	-.01	.02	-.48	.634
Reward * Transition * Education	.03	.01	3.10	.002

Independent models at follow-up

Controls, n = 87

Intercept	.28	.62	.45	.652
Reward	.02	.28	.07	.943
Transition	.03	.23	.11	.912
AD	-.10	.15	-.69	.491
CIT	.07	.15	.47	.639
Age	.11	.14	.82	.413
Gender	.04	.15	.29	.769
Education	.20	.09	2.11	.035
Reward * Transition	.04	.27	.14	.889
Reward * AD	-.07	.07	-1.10	.271
Reward * CIT	.03	.06	.45	.650
Transition * AD	-.09	.05	-1.74	.081
Transition * CIT	-.04	.05	-.82	.413
Reward * Age	.05	.06	.86	.389
Transition * Age	.01	.05	.18	.856
Reward * Gender	-.07	.07	-.99	.320
Transition * Gender	.02	.05	.40	.687
Reward * Education	.07	.04	1.67	.094
Transition * Education	.02	.04	.51	.607
Reward * Transition * AD	-.03	.06	-.45	.653
Reward * Transition * CIT	-.13	.06	-2.12	.034
Reward * Transition * Age	.02	.06	.29	.772
Reward * Transition * Gender	-.08	.06	-1.23	.218
Reward * Transition * Education	.04	.04	.89	.372

Antidepressants, n = 83

Intercept	1.76	.42	4.16	<.001
Reward	.38	.21	1.83	.068
Transition	.28	.13	2.10	.036
AD	.08	.11	.78	.438
CIT	-.21	.11	-1.87	.062
Age	.16	.11	1.48	.138
Gender	.21	.12	1.70	.089
Education	-.05	.06	-.79	.428

Reward * Transition	.42	.22	1.88	.060
Reward * AD	-.01	.05	-.21	.832
Reward * CIT	-.02	.05	-.37	.708
Transition * AD	.01	.03	.43	.670
Transition * CIT	-.04	.03	-1.23	.220
Reward * Age	.05	.06	.91	.363
Transition * Age	-.06	.04	-1.72	.085
Reward * Gender	.10	.06	1.65	.098
Transition * Gender	.07	.04	1.77	.077
Reward * Education	.00	.03	.11	.915
Transition * Education	-.02	.02	-.88	.380
Reward * Transition * AD	.06	.06	1.09	.275
Reward * Transition * CIT	-.08	.06	-1.41	.158
Reward * Transition * Age	.00	.06	-.02	.985
Reward * Transition * Gender	.09	.06	1.35	.178
Reward * Transition * Education	-.01	.03	-.45	.655
<i>iCBT, N = 611</i>				
Intercept	.82	.18	4.64	<.001
Reward	.08	.08	.93	.352
Transition	.00	.05	-.05	.962
AD	-.02	.05	-.37	.709
CIT	-.10	.05	-2.25	.024
Age	.16	.04	3.61	<.001
Gender	-.04	.05	-.68	.495
Education	.07	.03	2.50	.012
Reward * Transition	.06	.07	.93	.353
Reward * AD	.00	.02	.22	.822
Reward * CIT	-.04	.02	-1.95	.051
Transition * AD	.01	.01	1.13	.257
Transition * CIT	.00	.01	.35	.727
Reward * Age	.04	.02	1.73	.084
Transition * Age	-.02	.01	-1.30	.192
Reward * Gender	.00	.02	-.14	.886
Transition * Gender	.00	.01	-.33	.743
Reward * Education	.04	.01	3.12	.002
Transition * Education	.00	.01	.55	.584
Reward * Transition * AD	.01	.02	.47	.638
Reward * Transition * CIT	-.02	.02	-1.33	.184
Reward * Transition * Age	-.05	.02	-2.87	.004

Reward * Transition * Gender	.03	.02	1.21	.225
Reward * Transition * Education	.02	.01	1.91	.057

Model: $\text{lm}(\text{stay} \sim \text{reward}_{t-1} * \text{reward}_{t-1} * (\text{scale}(\text{AD}) + \text{scale}(\text{CIT})) + \text{reward} * \text{transition} * (\text{scale}(\text{age})) + \text{reward} * \text{transition} * \text{gender} + \text{reward} * \text{transition} * \text{education} + (\text{reward} * \text{transition} + 1 | \text{id}))$

Est = Beta estimate, Std. error = standard error, t = test statistic, p = significance, iCBT = internet based cognitive behavioural therapy, AD = Anxious-depression, CIT = Compulsivity and Intrusive Thought

Supplementary Table 7.2.2.14 Hierarchical models predicting stay by reward, transition, anxious-depression, compulsivity, session and their interactions while controlling for covariates of age, gender and education within each group

	Est.	Std. Error	t	p
<u>Controls, N = 87</u>				
Intercept	.15	.50	.31	.758
Reward	-.34	.23	-1.50	.132
Transition	-.18	.16	-1.09	.277
Session	.13	.04	3.64	<.001
AD	.16	.06	2.76	.006
CIT	-.12	.05	-2.13	.033
Age	.16	.11	1.49	.136
Gender	-.10	.12	-.82	.410
Education	.17	.08	2.27	.023
Reward * Transition	-.25	.23	-1.11	.267
Reward * Session	.12	.04	3.40	.001
Transition * Session	.06	.04	1.65	.098
Reward * AD	-.02	.04	-.40	.688
Reward * CIT	-.08	.04	-2.03	.043
Transition * AD	-.02	.04	-.59	.553
Transition * CIT	-.03	.04	-.76	.447
Session * AD	-.07	.04	-1.71	.087
Session * CIT	.14	.04	3.51	<.001
Reward * Age	.07	.05	1.32	.186
Transition * Age	-.02	.04	-.46	.649
Reward * Gender	-.11	.05	-1.95	.051
Transition * Gender	.00	.04	.04	.967
Reward * Education	.09	.03	2.76	.006
Transition * Education	.03	.02	1.35	.177
Reward * Transition * Session	.01	.04	.26	.798
Reward * Transition * AD	.08	.04	1.80	.072
Reward * Transition * CIT	-.14	.04	-3.44	.001
Reward * Session * AD	.05	.04	1.14	.254
Reward * Session * CIT	.03	.04	.76	.449
Transition * Session * AD	-.01	.04	-.32	.747
Transition * Session * CIT	-.05	.04	-1.39	.164
Reward * Transition * Age	.00	.05	-.06	.956
Reward * Transition * Gender	-.03	.05	-.47	.638
Reward * Transition * Education	.08	.03	2.24	.025

Reward * Transition * Session * AD	-.02	.04	-.57	.570
Reward * Transition * Session * CIT	-.04	.04	-.93	.354
<u>Antidepressant, N = 83</u>				
Intercept	1.42	.34	4.15	<.001
Reward	.29	.18	1.61	.107
Transition	.08	.09	.91	.361
Session	.03	.04	.72	.472
AD	-.19	.05	-4.03	<.001
CIT	-.13	.06	-2.33	.020
Age	.19	.09	2.18	.029
Gender	.20	.10	2.09	.037
Education	-.01	.05	-.29	.771
Reward * Transition	.29	.16	1.81	.070
Reward * Session	.07	.04	1.69	.091
Transition * Session	.09	.04	2.49	.013
Reward * AD	-.08	.04	-2.02	.043
Reward * CIT	.00	.04	-.04	.971
Transition * AD	.06	.03	1.81	.071
Transition * CIT	.01	.03	.19	.851
Session * AD	.22	.04	5.10	.000
Session * CIT	.01	.04	.18	.857
Reward * Age	.02	.05	.42	.676
Transition * Age	-.04	.02	-1.62	.104
Reward * Gender	.07	.05	1.42	.156
Transition * Gender	.05	.02	1.84	.066
Reward * Education	.00	.03	-.05	.960
Transition * Education	.00	.01	-.40	.690
Reward * Transition * Session	.03	.04	.89	.376
Reward * Transition * AD	.05	.04	1.36	.173
Reward * Transition * CIT	-.04	.04	-1.12	.262
Reward * Session * AD	.10	.04	2.28	.023
Reward * Session * CIT	.01	.04	.19	.849
Transition * Session * AD	-.04	.04	-1.13	.259
Transition * Session * CIT	-.03	.03	-.96	.336
Reward * Transition * Age	-.03	.04	-.65	.513
Reward * Transition * Gender	.12	.05	2.58	.010
Reward * Transition * Education	.00	.02	.02	.982
Reward * Transition * Session * AD	.01	.04	.26	.797
Reward * Transition * Session * CIT	-.02	.04	-.60	.548
iCBT, N = 611				
Intercept	.52	.14	3.62	<.001

Reward	.08	.07	1.20	.229
Transition	.02	.03	.54	.587
Session	.08	.01	5.61	<.001
AD	-.03	.02	-1.77	.076
CIT	-.12	.02	-6.37	<.001
Age	.17	.04	4.74	<.001
Gender	-.04	.04	-.97	.332
Education	.09	.02	4.07	<.001
Reward * Transition	.04	.06	.74	.459
Reward * Session	.05	.01	3.78	<.001
Transition * Session	.00	.01	.18	.859
Reward * AD	-.01	.01	-.44	.663
Reward * CIT	-.04	.01	-2.90	.004
Transition * AD	.00	.01	.32	.747
Transition * CIT	-.02	.01	-1.52	.128
Session * AD	-.01	.01	-.89	.374
Session * CIT	.07	.01	5.14	<.001
Reward * Age	.02	.02	1.24	.215
Transition * Age	-.01	.01	-1.20	.231
Reward * Gender	-.01	.02	-.38	.705
Transition * Gender	.00	.01	.32	.750
Reward * Education	.02	.01	2.34	.020
Transition * Education	.00	.01	.26	.796
Reward * Transition * Session	-.02	.01	-1.33	.183
Reward * Transition * AD	-.01	.01	-.88	.377
Reward * Transition * CIT	-.03	.01	-1.95	.051
Reward * Session * AD	.00	.01	-.31	.759
Reward * Session * CIT	.00	.01	.37	.710
Transition * Session * AD	.00	.01	.08	.933
Transition * Session * CIT	.02	.01	1.83	.067
Reward * Transition * Age	-.05	.01	-3.20	.001
Reward * Transition * Gender	.01	.02	.69	.490
Reward * Transition * Education	.02	.01	2.73	.006
Reward * Transition * Session * AD	.00	.01	-.14	.891
Reward * Transition * Session * CIT	.01	.01	1.12	.263

Model: stay ~ reward(t-1) * transition(t-1) * session * (scale(AD) + scale(CIT)) + reward* transition * (scale(age)) + reward * transition * (gender) + reward * transition * (education) + (reward * transition + 1 | id)

Session coded as a factor: baseline, follow-up

Est = Beta estimate, Std. error = standard error, t = test statistic, p = significance, iCBT = internet based cognitive behavioural therapy, AD = Anxious-depression, CIT = Compulsivity and Intrusive Thought

Supplementary Table 7.2.2.15 Repeated measures model testing if changes in compulsivity are influenced by session, group or baseline model-based estimates

	Est.	Std. Error	t	p
Intercept	-.39	.11	-3.66	<.001
Session	-.17	.06	-2.70	.007
Antidepressant	.99	.17	5.99	<.001
iCBT	.61	.12	5.27	<.001
Baseline model-based score	-.47	.25	-1.90	.058
Session*Antidepressant	-.19	.10	-1.95	.051
Session*iCBT	-.10	.07	-1.47	.143
Session*Baseline model-based score	.27	.15	1.83	.067
Antidepressant*Baseline model-based score	-.28	.45	-.63	.527
iCBT*Baseline model-based score	.01	.29	.05	.959
Session*Antidepressant*Baseline model-based score	-.06	.27	-.22	.824
Session*iCBT*Baseline model-based score	-.09	.17	-.53	.599

N = 781

Full Model: Compulsivity ~ session * group * baseline model-based score + (1 | id)

Session coded as a factor with levels: baseline, follow-up

Group coded as factor with 'Control' as reference

Est = Beta estimate, Std. error = standard error, t = test statistic, p = significance, iCBT = internet based cognitive behavioural therapy

Supplementary Table 7.2.2.16 Mean estimates of compulsivity and intrusive thought scores and statistical changes from baseline to follow-up within group

		<u>Baseline</u>	<u>Follow-up</u>			
	N	M (SD)	M (SD)	Est. (SE)	t	p
Control	87	-.50 (.75)	-.61 (.79)	-.14 (.07)	1.96	.052
Antidepressant	83	.43 (.93)	.11 (.99)	-.34 (.06)	-5.11	<.001
iCBT	611	.14 (.84)	-.11 (.85)	-.28 (.02)	-12.12	<.001

Model: lm (CIT ~ session + (1 | id))
Session coded as a factor with levels: baseline, follow-up
M = mean, SD = standard deviation, Est.=Beta estimate, SE = standard error, t = test statistic, p = significance, iCBT = internet-based cognitive behavioural therapy

7.2.3 Supplementary Figures

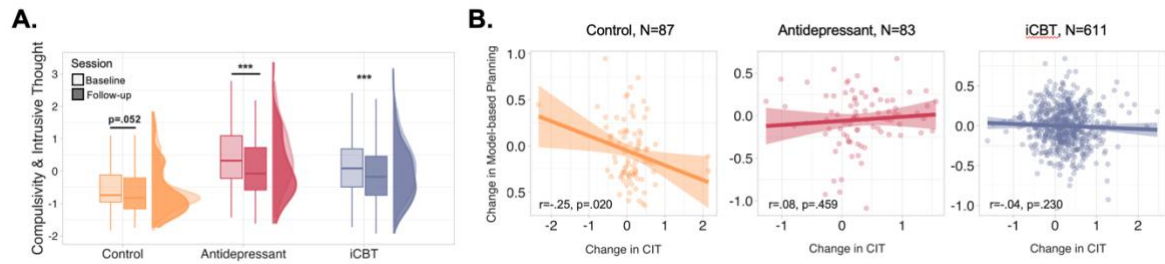


Figure 7.2.3.1 Changes in compulsivity in the PIP study and its association with changes in model-based planning. A. Distribution and boxplot of values of compulsivity and intrusive thought scores across control and treatment groups (Antidepressant, iCBT) and within (baseline, 4-week follow up) groups. Compulsivity and intrusive thought scores reduced from baseline to follow-up in the treatment groups only. Central line reflects median values. **B.** Correlation between changes in compulsivity and intrusive thought (CIT) and change in model-based (MB) planning per group. Each dot represents a participant, the line represents the line of best fit while the shaded areas represent 95% confidence interval. For all panels: * $p < .05$, ** $p < .01$, *** $p < .001$.

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7.3 Supplementary Materials for Chapter 4

7.3.1 Supplementary Methods and Analyses

7.3.1.1 Deviations from pre-registration

A list of hypotheses and statistical analyses were defined prior to data collection at <https://osf.io/quwb3>. There were a couple of small deviations from the original plan

Deviation 1: To reduce the potential impact of inattention on our findings, we excluded participants whose self-reported age differed by more than two years between Prolific and the Neureka app. The pre-registration only listed excluding participants who failed the attention check question, but the number of failures was very few. On observing these discrepancies in reported age, we felt it prudent to also exclude these participants and this allowed us to additionally estimate any potential impact of (undetected) inattention on our findings (Supplementary Methods and Analyses 7.3.1.4). **Deviation 2:** The pre-registration mistakenly stated in Hypothesis 3A that all “single level items (136 items) will be entered as variables in the factor analysis” (p.13). During development of the task, we added two measurements: The Massachusetts General Hospital Hairpulling Scale (MGH) and the Skin Picking Scale (SPS). These were intended to replace the Milwaukee Inventory for Subtypes of Trichotillomania – Adult Version (MISTA) and the Milwaukee Inventory for the Dimensions of Adult Skin-picking (MIDAS). This decision was made because in piloting we realised that the MISTA and MIDAS rely on a baseline level of hair-pulling/skin-picking and were therefore unsuitable for use in a general population sample. We retained these scales in the experiment so we could carry out exploratory analyses within a subset of individuals who reported at least some hair-pulling/skin-picking (Supplementary Methods and Analyses 7.3.1.7). But this change to what would go in the factor analysis were mistakenly not updated in the pre-registration. Another vestige of this error is that while the inclusion of the MGH and SPS was reflected in the list of clinical measures in Table 1 of the pre-registration (though with a typographical error in the number of MGH items, which read 6 items instead of 7), these scales were omitted from the Hypothesis 2 Analysis Plan entirely. **Deviation 3:** We opted for a 4-factor solution, rather than following the suggestion of the CNG test (3-factors). This was because the inflection on the scree plot was unclear and landed between 3 and 4 factors.

Using an alternative method for factor selection, the very simple structure test, outputted a 4-factor solution. When we compared both candidate models, the 4-factor solution performed better on all model-performance metrics and was more clinically interpretable. The results of the 3-factor structure are reported in Supplementary Methods and Analyses 7.3.1.6 for full transparency.

7.3.1.2 Detailed description of the habit-learning task

Tutorial. Participants completed two practice blocks of trials to familiarise them with how to respond in certain phases of the task. First, the participants practiced how to respond during training (20 trials). Participants were instructed to figure out which cog i.e., the stimulus, was correctly paired with one of the button presses i.e., the response. Each trial consisted of a cog falling, from various heights, towards a laser beam. Participants were instructed to respond before the cog reaches this laser beam. Once a button was pressed, participants received feedback on whether their response was correct, cog turns gold and score increased by one, or incorrect by the cog exploding. If the participant failed to respond before the cog reached the laser beam, it melted behind the laser and the trial was deemed missed. To simplify the task during the practice, the same cog was presented sequentially five times, followed by the next cog and so on.

Participants then practice how to respond during the forced-response test (60 trials). Participants were instructed to respond in sync with a signal that fixed the timing of the response initiation on every trial. To manipulate the amount of time a participant had to prepare their response before the desired response, the stimulus was presented in a uniform distribution shortly before or with ample time before the signal. For continuity, the same stimuli and pairings used in the practice of training trials were used. At the end of practice, participants received the number of on-time trials and number of correct responses. If participants failed to respond on-time for more than 20% of their last 20 trials i.e., less than 16 on-time responses in their last 20 trials, they were given the opportunity to repeat this part of the tutorial. A separate set of stimuli were used in the tutorial section to that of the main task, so no learning of specific SR associations transferred.

Criterion Mapping. During this section, participants were instructed to learn, through trial and error, four stimulus-response associations between stimuli, represented as falling cogs, and a response, reflected by pressing one of the buttons located along the bottom of the screen. To ensure a basic knowledge of each mapping, participants were instructed to complete five consecutive correct response for each stimulus to progress. This section consisted of at least 20 trials, and we implemented a maximum cut-off of 200 trials at which participants were presented with the instructions and reengaged with the section until the criterion was reached. Participants completed two criterion sections (original; remapping) during each training duration (minimal; extended). The first criterion in each training condition defined the original mappings. After minimal or extended training, participants completed the second criterion section defined as remapping. Here, two out of the four mappings were switched, and participants relearned these new mappings until criterion.

Training. During training participants reinforced mappings through five daily training sessions. Training sessions consisted of five blocks of 80 trials (minimum of 400 trials per day). Each trial began with a cog appearing on screen and falling towards a laser beam. To maintain consistency between training and test trials, each trial began with the stimulus appearing at a random location on screen, vertically and horizontally in a uniform distribution. Participants had to respond by pressing the associated button. If correct, the cog turned gold, and their score increased by one point. If incorrect or if the participant failed to respond before the cog reached the laser beam, the cog exploded or melted respectively and the same cog repeated until a correct response was given. Each stimulus was presented at least 20 times per block. Participants were instructed to complete the blocks as quickly and as accurately as possible. At the end of each block, participants were presented with their completion time along with the number of errors made, and number of missed-timed trials as a form of motivation to improve on. At the end of the training session, participants were presented with a personal leader board consisting of their overall three fastest block completion times.

Habit Test. Participants completed two forced response tests: one following minimal and the other following extensive training. The test consisted of 5 blocks of

80 forced response trials (400 trials per test). Participants were instructed to respond in sync with a signal i.e., the response bar turning green. A trial started with the response bar flashing red (0ms), followed by an amber flash (650ms), and finally a green at 1300ms, indicating to the participant to respond. Responses between 1100-1550ms were accepted as these reflected times where the cog appeared to pass through the response bar while green. Responses outside of this window were flagged to the respective “Too early” or “Too late” text appearing on screen irrespective of if the response was correct or incorrect. The stimulus appeared at a random point in a uniform distribution between 0-950ms, which created a range of times in which participants had to respond from 350-1300ms.

7.3.1.3 Detailed description of the goal-directed learning task Cannon Blast

In this task, participants were instructed to shoot as many diamonds as possible. To do so, participants first aimed a central cannon and then selected one of two flanked containers. Each contained an unequal distribution of coloured balls (70% dominant; 30% rare) that directly translated to the probability of drawing that ball colour (i.e., transition variable). Some balls exploded upon firing, negating the reward (i.e., reward variable). The likelihood of drawing these ‘duds’ varied for each ball colour over the course of the task, requiring participants to track which colour was currently safer and infer which container was most likely to yield that ball colour. In this study, participants completed an abbreviated version consisting of 100 trials at an easy difficulty level and were randomly assigned to one of ten drifting reward probability sets.

7.3.1.4 Inattentive/careless responding

As reported in the main text, we identified careless/inattentive responding in two ways: those who failed an attention check question embedded within a questionnaire, and those whose self-reported ages differed by more than two years between Prolific and the Neureka app (Supplementary Figure 7.3.3.2a). Recent studies have shown that careless/inattentive responders can systematically bias data such that they tend to appear more symptomatic, which when combined with poorer performance on behavioural tasks, may lead to overestimated or even spurious associations (Sarna et al., 2025; Zorowitz et al., 2023). Inattention was overall very low using these two metrics. This is possibly because this study

involved installing an app on a phone, took a week to complete, and so may have been enriched for particularly motivated participants. Nonetheless, we explored the impact of inattention on our findings. In terms of behavioural data ($n = 871$), we compared the proportion of habit errors made, between careless/inattentive ($n = 24$, hereafter ‘inattentive’) and attentive ($n = 847$) participants (Supplementary Figure 7.3.3.2b). Although group sizes differed too greatly for statistical comparison, inattentive participants made a nominally greater proportion of habit errors during both short preparation (Inattentive: $M \pm SD = .21 \pm .13$; Attentive: $M \pm SD = .18 \pm .10$) and long preparation times (Inattentive: $M \pm SD = .15 \pm .14$; Attentive: $M \pm SD = .11 \pm .10$). We then compared dimensional factor scores and individual questionnaire total scores (Supplementary Figure 7.3.3.2c). Across most measures, inattentive responders showed a positive bias in total scores. Finally, to explore the impact on the association between clinical and task measures, we repeated the analysis testing the link between habit-learning and ‘Compulsivity and Intrusive Thought’ controlling for age, gender and education and other errors, using the full sample including inattentive responders ($n = 846$). We found a slightly stronger effect between habit-learning and ‘Compulsivity and Intrusive Thought’ ($\beta = .77$, $SE = .25$, $p = .003$) than reported in the main text without careless/inattentive responders ($\beta = .68$, $SE = .25$, $p = .008$) (see Supplementary Figure 7.3.3.2d for simple correlations in the attentive vs. inattentive group). Although the estimated impact was small, we thus deviated from our pre-registration to be conservative and removed these participants from the main analyses.

7.3.1.5 Demographic differences in response times, errors made during training and errors made during the habit test

In the main text, we reported that older adults demonstrated fewer habits, while males and those with greater education had made more habits in the critical short preparation trials. To contextualise this result, we examined a range of other performance metrics associated with these demographics. Older adults ($\beta = .03$, $SE = .00$, $p < .001$) and males (compared to females: $\beta = .02$, $SE = .00$, $p < .001$) responded more slowly during training, while those with greater education responded more quickly ($\beta = -.004$, $SE = .00$, $p = .023$). However, older adults ($\beta = -.002$, $SE = .00$, $p < .001$) and males ($\beta = -.02$, $SE = .00$, $p < .001$) became faster over time, while those with higher education improved less over time ($\beta = .004$, SE

= .00, $p < .001$). Older adults made a greater proportion of errors during training ($\beta = .004$, $SE = .00$, $p = .023$) but also showed more improvement over time ($\beta = -.01$, $SE = .00$, $p < .001$). Males (compared to females) made more errors during training ($\beta = .01$, $SE = .00$, $p = .043$), but improved as much as females during training ($\beta = -.003$, $SE = .00$, $p = .422$). We found no association between education and the proportion of errors made ($\beta = -.001$, $SE = .00$, $p = .420$) or improvement of errors over training ($\beta = .003$, $SE = .00$, $p = .092$).

Next, we examined if age, gender and education were associated with other performance metrics in the habit test. Examining long preparation trials, older adults were less likely to perform habitual errors ($\beta = -.74$, $SE = .25$, $p = .003$), but this effect was about half the size of the effect at short preparation (Short: $\beta = -1.62$ vs. Long: $\beta = -.74$). We found no association between number of habits made during long preparation trials and male gender ($\beta = .74$, $SE = .53$, $p = .160$) or education ($\beta = .21$, $SE = .24$, $p = .398$). Next, we examined other (non-habitual) errors made which we controlled for in the analyses above. Older adults were less like to make other errors but only during short preparation ($\beta = -.52$, $SE = .14$, $p < .001$) and not long preparation trials ($\beta = .12$, $SE = .09$, $p = .175$). Males (compared to females) showed a trend in making more other errors during short preparation ($\beta = .52$, $SE = .29$, $p = .081$) but not at long preparation trials ($\beta = .27$, $SE = .18$, $p = .136$). We found no association between other errors and education at either short ($\beta = -.12$, $SE = .13$, $p = .328$) or long preparation trials ($\beta = -.12$, $SE = .08$, $p = .158$). A similar pattern was observed in consistent mapped trials. Older adults were less likely to make errors during short preparation trials ($\beta = -.33$, $SE = .10$, $p = .001$) and showed a trend to make less errors during long preparation trials ($\beta = -.08$, $SE = .04$, $p = .062$). Males (compared to females) made more errors at short ($\beta = .66$, $SE = .20$, $p = .001$) and long preparation trials ($\beta = .22$, $SE = .09$, $p = .020$). Again, no associations between errors and education were observed at either short ($\beta = .11$, $SE = .09$, $p = .238$) or long preparation times ($\beta = -.002$, $SE = .04$, $p = .961$).

7.3.1.6 Exploratory factor analysis using a 3-factor structure

Despite model performance metrics such as the Root Mean Square Error of Approximation, Tucker-Lewis Index of reliability, and Comparative Fit Index supporting a 4-factor structure, our pre-registered method of factor section, the CNG

test, supported a 3-factor structure which we ran for completeness. The first factor resembled Factor 1 reported in the main manuscript, consisting of high loadings from anxiety (mean loading = .66), depression (mean loading = .64), apathy (mean loading = .65), impulsivity (mean loading = .27) and autistic-like trait related to social skills (mean loading = .39) and switching (mean loading = .35). The second factor was similar to Factor 2 reported in main text, with loadings from OCD (mean loading = .56), Tourette's Syndrome (mean loading = .43), eating disorder (mean loading = .34), and skin-picking (mean loading = .29). Factor 3 consisted of high loadings from the hair-pulling scale (mean loadings = .89), skin-picking (mean loading = .14) and Tourette's Syndrome items (mean loading = .13). A full list of the item loadings is presented in Supplementary Table 7.3.2.14 and displayed in Supplementary Figure 7.3.3.4a.

We then tested whether participant-weighted scores on these factors explained variance in habit learning (Supplementary Table 7.3.2.15, Supplementary Figure 7.3.3.4b). We found an association between habit learning and Factor 2, which was composed of similar items to that of the 'Compulsivity and Intrusive Thought' dimension reported in the main manuscript ($\beta = .56$, $SE = .25$, $p = .026$). No association was found between habit-learning and Factor 1 (similar to 'anxious-depression' in main text: $\beta = .21$, $SE = .25$, $p = .395$) or Factor 3 (similar to 'motoric acts' in main text: $\beta = -.16$, $SE = .24$, $p = .510$), which had a negative beta similar to that reported in the main manuscript. When all factors were entered into the model alongside covariates of age, gender and education, the association between habit learning and Factor 2 remained significant ($\beta = .59$, $SE = .28$, $p = .038$). Thus, the decision to bring forward the 4-factor model instead of the 3-factor model supported by the pre-registered method of factor selection (i.e., CNG test) did not substantially alter the results.

7.3.1.7 Do automatic vs. focused subtypes of bodily-focused repetitive behaviours differ in the association with habit-learning?

Prior research has suggested that bodily-focused repetitive behaviours can be categorised into automatic (i.e., without conscious attention) and focused (i.e., purposeful picking/pulling) subtypes, which may reflect distinct underlying cognitive mechanisms. We explored this hypothesis in subsets of the sample who reported

some level of engagement in skin-picking or hair-pulling behaviours, examining associations with habit learning. From $n = 822$ who had both CogQuest and questionnaire data, $n = 133$ (16%) scored equal to or greater than 7 on the Skin-Picking Scale (Keuthen et al., 2001) indicating suspicious skin-picking behaviour. Automatic versus focused skin-picking was measured through the Milwaukee Inventory for Dimensions of Adult Skin-picking (Walther et al., 2009), a 12-item questionnaire containing 6-items related to automatic- and 6-items related to focused skin-picking. We found no association between habit learning and automatic ($\beta = .41$, $SE = .68$, $p = .545$) or focused ($\beta = .19$, $SE = .69$, $p = .787$) skin-picking controlling for age, gender and education. Regarding problematic hair-pulling, $n = 105$ (13%) reported a score equal to or greater than 6 on the Massachusetts General Hair-pulling Scale (Keuthen et al., 1995). Automatic and focused hair-pulling was measured through the Milwaukee Inventory for Sub-types of Trichotillomania Adults (Flessner et al., 2007), a 15-item questionnaire containing 5-items related to automatic hair-pulling and 10-items related to focused hair-pulling. Again, we found no significant association between habit learning and automatic ($\beta = -.48$, $SE = .80$, $p = .546$) or focused ($\beta = .54$, $SE = .79$, $p = .495$) hairpulling controlling for covariates. Given the small and underpowered subgroups, we recommend these findings are interpreted with caution. When we specifically examined the distress and interference items necessary for a bodily-focused repetitive behaviour diagnosis (SPS Item 4; MGH Item 7), group sizes reduced to $n = 28$ (3%) who reported a score greater than 1 for skin-picking (i.e., had at least some interference due to skin-picking) and $n = 26$ (3%) who reported a score greater than 1 for hair-pulling (i.e., had experienced some distress due to hair-pulling). While these rates are similar to prevalence rates of bodily-focused repetitive behaviours in a general population sample, it highlights the need for future work to target clinical-specific groups to test this sufficiently.

7.3.2 Supplementary Tables

7.3.2.1 Description of preregistered hypotheses, predictions and study outcomes

	Prediction	Outcome	Supported
Hypothesis 1. Replicate the overtraining effect on habit expression			
1A	During training, reaction times will decrease from day 1 to day 5	Reaction times significantly decreased from day 1 to day 5	Yes
1B	During training, proportion of errors will decrease from day 1 to day 5	Proportion of errors significantly decreased from day 1 to day 5	Yes
1C	In forced response test, proportion of habitual errors will be greater following extended compared to minimal training (main effect of training), greater in short compared to long preparation trials (main effect of preparation) and that the effect of training on habitual errors will be greatest at short preparation trials (interaction effect).	Significant main effects of training and preparation and a significant interaction effect	Yes
Hypothesis 2. Identify distinct psychiatric profiles that correlate with increased stimulus response habit learning			
2A	An elevated stimulus-response system will be associated with greater self-report levels of tics, automatic hair-pulling, automatic skin-picking and components of Autistic-like traits.	Elevated stimulus response system associated with greater levels of automatic skin-picking and a transdiagnostic factor of compulsivity	Partial
2B	Deficits in goal-directed control will be associated with greater self-report levels of a transdiagnostic measure of compulsivity.	No association between model-based planning and compulsivity	No
Hypothesis 3. Are individual differences in stimulus response habit learning explained better by a transdiagnostic factor compared with individual clinical measures?			
3A	An elevated stimulus-response will have greater association with a transdiagnostic motoric factor developed from a factor analysis than original questionnaire total scores.	Addition of tic and skin-picking items to compulsivity did not improve the signal with habit-learning	No

Note: Shaded areas reflect (yes=darker grey: partial=lighter grey) support for preregistered hypotheses.

7.3.2.2 Exclusion criteria for task and self-report data

	N Excluded	
Task Data		
CogQuest	Minimal	Extended
Incomplete sections of the task	9	14
Fail to reach criterion in 200 trials in any learning phase	28	34
Fail to have at least 90% training trials	n/a	0
Fail to have at least 90% habit test trials	3	10
Fail to have at least 50% valid on-time habit test trials	13	16
Repeated the same response consecutively for 16 trials (20%) in any habit test	16	15
<i>Total N excluded per condition</i>	56	63
<i>Total N (%) excluded</i>	106 (11%)	
Cannon Blast		
Fail to have complete session	65	
Repetitively pressed the same side for >95% trials	83	
<i>Total N excluded</i>	148 (15%)	
Questionnaire Data		
Demographics		
Fail to have complete demographic data	1 (.01%)	
Misreported age greater than two years	26 (3%)	
Mental Health		
Fail to have complete all clinical questionnaires	49	
Failed the attention check question	7	
<i>Total N excluded</i>	55 (5.5%)	
N remaining for behavioural analysis	847 (85%)	
N remaining for testing individual differences	822 (82%)	

7.3.2.3 Sample demographics of those remaining for behavioural analysis and those remaining for analyses pertaining to individual differences

	Behavioural	Individual Differences
N	846 ^a	822
Age, M±SD [Range]	38.12 ± 12.29 [18-72]	38.02 ± 12.25 [18-72]
Gender, N(%)		
Female	571 (67%)	556 (68%)
Male	262 (31%)	253 (31%)
Non-binary	11 (1%)	11 (1%)
Transgender Female	0 (0%)	0 (0%)
Transgender Male	2 (<1%)	2 (<1%)
Not-listed	0 (0%)	0 (0%)
Prefer not to say	0 (0%)	0 (0%)
Education attainment, N(%)		
No formal education	0 (0%)	0 (0%)
Lower second level	36 (4%)	35 (4%)
Upper second level	268 (32%)	262 (32%)
Third level degree	370 (44%)	361 (44%)
Masters degree	144 (17%)	137 (17%)
PhD/Doctoral or equivalent	28 (3%)	27 (3%)
Operating device, N(%)		
iOS	459 (54%)	457 (56%)
Android	387 (46%)	365 (44%)

^a $n = 1$ in the subset for behavioural analysis did not have complete demographic data, therefore demographics presented on the $n = 846$ with complete data

7.3.2.4 List of clinical measures

Scale (Abbreviation)	Description	N Items	Citation
Autism Spectrum Quotient – Short (AQ-Short)	This measures self-report severity of autistic traits. It consists of five subscales (social skills, routine, switching, imagination, and numbers/patterns). Higher total scores indicate higher levels of severity.	28 items (7 items for social skills, 4 items for routine, 4 items for switching, 8 items for imagination and 5 items for patterns/numbers)	(Hoekstra et al., 2011)
Milwaukee Inventory for the Dimension of Adult Skin-Picking (MIDAS)	This measure is designed to assess the degree to which an individual engages in skin-picking (excoriation) in an 'automatic' and 'focused'. Higher score reflects greater severity of symptoms.	12 items (6 items in each subscale of 'automatic' and 'focused').	(Walther et al., 2009)
Skin Picking Scale (SPS)	This measure assesses the frequency, intensity and interference of skin-picking symptomatology.	6 items	(Snorrason et al., 2012)
The Massachusetts General Hospital Hair-pulling Scale (MGH)	This measure assesses the frequency, intensity and interference of hair-pulling symptomatology.	7 items	(Keuthen et al., 1995)
Milwaukee Inventory for Subtypes of Trichotillomania – Adult (MIST-A)	This measure is designed to assess 'automatic' and 'focused' subtypes of hair-pulling (trichotillomania). Higher score reflects greater severity of symptoms.	15 items (10 items in the automatic subscale and 5 in the focused subscale).	(Flessner et al., 2007)
Motor tic, Obsessions and compulsions, Vocal tic Evaluation Survey (MOVES)	This measures the severity of Tourette Syndrome symptoms. The scale consists of five subscales (motor tics, vocal tics, obsessions, compulsions, and complex symptoms). Items are combined to generate a tic severity and compulsion severity score with higher scores reflecting greater severity.	20 items (8 items in Simple Tics, 12 items in Simple & Complex Tics, 8 items in Obsession and Compulsions).	(Gaffney et al., 1994)
'My Mental Health' battery: Transdiagnostic battery of items from Apathy Evaluation Scale (AES), Barrett's Impulsivity Scale (BIS), Eating Attitudes Test (EAT), Obsessive Compulsive Inventory-Revised (OCIR), Self-rating Depression Scale (SDS), State-Trait Anxiety Inventory (STAI)	AES is used to measure anhedonia. BIS is used to measure impulsivity. EAT is used to measure eating disorder symptoms. OCIR is used to measure OCD. SDS is used to measure depression. STAI is used to measure trait anxiety.	49 items (4 items from AES, 11 items from BIS, 4 items from EAT, 11 items from OCIR, 8 items from SDS, 11 items from STAI).	(Foa et al., 2002; Garner et al., 1982; Marin et al., 1991; Patton et al., 1995; Spielberger, 1983; Zung, 1965)

7.3.2.5 Mean, standard deviations and ranges of clinical questionnaire total scores in those with CogQuest, demographic and mental health data (n = 840)

Clinical construct (Abbreviated Scale)	Mean $\pm$ Std. Dev.	Median	Cronbach's α
Independent Clinical Measures			
Autistic-like traits (ASQ)	65.4 $\pm$ 13.10	65	.88
Hair-pulling (MGH)	1.51 $\pm$ 4.05	0	.95
Skin-picking (SPS)	3.05 $\pm$ 3.91	2	.91
Tourette's Syndrome(MOVES)	4.91 $\pm$ 6.60	3	.92
Transdiagnostic Dimensions			
Anxious-depression	2.49 $\pm$.75	2	-
Compulsivity and Intrusive Thought	2.06 $\pm$.91	2	-

Subset of $n = 822$ with complete CogQuest, demographic and mental health data for individual difference testing

AQS=Autism Quotient Short (AQS), MGH= Massachusetts General Hospital Hair-pulling Scale, MIDAS=Milwaukee Inventory for Dimensional Skin-picking, MISTA= Milwaukee Inventory for Subtypes of Trichotillomania – Adult, MOVES= Motor and vocal tic and Obsessions and compulsions Evaluation Scale, SPS=Skin-picking Scale, Std. Dev. = Standard deviation

7.3.2.6 Means and standard deviations of the number and proportion of on-time trials between test conditions (minimal; extended) and within test conditions (consistent; remapped)

	Minimal				Extended		β (SE)	p
M(SD)	309 (37.0)				309 (37.3)		-.41(.84)	.625
M _{prop} (SD)	.77 (.09)				.77 (.09)		-.00(.00)	.304
	Consistent	Remapped	β (SE)	p	Consistent	Remapped	β (SE)	p
M (SD)	156 (20.8)	154 (20.5)	-2.00(.63)	.002	157 (20.5)	152 (21.9)	-5.04(.69)	<.001
M _{prop} (SD)	.77 (.09)	.76 (.10)	-.01(.00)	<.001	.78 (.09)	.76 (.10)	-.02(.00)	<.001

n = 847

M = Mean, M_{prop}= Mean Proportion, SD = Standard deviation, β = beta estimate, SE = Standard Error

7.3.2.7 Means and standard deviations of the number and proportion of correct trials between trial mappings (consistent; remapped) and within trial mappings (minimal; extended)

	Consistent				Remapped			
	Minimal	Extended	β (SE)	p	Minimal	Extended	β (SE)	p
M (SD)	131 (22.2)	138 (20.2)	7.10 (.71)	<.001	125 (24.3)	110 (23.8)	-14.5(.80)	<.001
M _{prop} (SD)	.84 (.10)	.88 (.08)	.04 (.00)	<.001	.81 (.12)	.73 (.14)	-.08(.00)	<.001

$n = 847$

M = Mean, M_{prop}= Mean Proportion, SD = Standard deviation, β = beta estimate, SE = Standard Error

7.3.2.8 Regression coefficients of the association between habit-learning and clinical total scores and subscales

	Est	Std Error	t	p
Autism	.34	.24	1.37	.170
Social Skills	.13	.24	.52	.606
Routine	.33	.24	1.35	.177
Switching	.25	.25	1.03	.305
Imagination	.25	.24	1.04	.299
Numbers and Patterns	.21	.25	.84	.399
Hairpulling	.04	.24	.15	.881
Skin-picking	.26	.25	1.04	.297
Tourette's Syndrome	-.10	.25	-.37	.711
Tics	-.06	.25	-.22	.822
Obsessions and compulsions	-.11	.25	-.45	.652

n = 822

Independent clinical models: stimulus-response index ~ scale(i) + scale(age) + factor(gender) + scale(education) where each clinical measure is substituted for i

Stimulus-response index=number of habitual errors made during short preparation trials following extended training controlling for other errors made

Est = Estimate, Std Error = Standard Error, t = test statistic, p = significance value

7.3.2.9 Regression coefficients of the association between habits made at long preparation trials and clinical scores

	Est	Std Error	t	p
Demographics				
Age	-.74	.25	-3.03	.003
Gender	.74	.53	1.41	.160
Education	.21	.24	.85	.398
Transdiagnostic Dimensions				
Anxious-depression	-.18	.25	-.71	.475
Compulsivity	.35	.26	1.36	.176
Independent Clinical Measures				
Autism	-.06	.25	-.26	.792
Hairpulling	.38	.24	1.54	.123
Skin-picking	.40	.25	1.59	.111
Tourette's Syndrome	-.22	.25	-.88	.378
<i>n</i> = 822				
Independent clinical models: habits at long preparation trials ~ scale(i) + scale(age) + factor(gender) + scale(education) + other errors made where each clinical measure is substituted for i				
Est = Estimate, Std Error = Standard Error, t = test statistic, p = significance value				

7.3.2.10 Influence of model-free ('reward') and model-based planning ('reward x transition') on stay behaviour in Cannon Blast

	Est	Std Error	z	p
Intercept	1.32	.03	44.85	<.001
Reward	.39	.02	18.93	<.001
Transition	-.03	.01	-2.71	.007
Reward * Transition	.19	.02	12.45	<.001

$n = 790$
 Model: $\text{stay} \sim \text{reward}_{t-1} * \text{transition}_{t-1} + (\text{reward}_{t-1} * \text{transition}_{t-1} + 1 \mid \text{id})$
 Est = Estimate, Std Error = Standard Error, z = test statistic, p = significance value

7.3.2.11 Regression coefficients of the association between goal-directed-learning and individual and clinical differences

	Est	Std Error	t	p
Demographics				
Intercept	.19	.01	19.69	<.001
Age	-.02	.01	-2.18	.029
Gender	-.00	.02	-.04	.973
Education	.00	.01	.17	.866
Transdiagnostic Dimensions				
Anxious-depression	.02	.01	2.50	.013
Compulsivity	.00	.01	.38	.702
Independent Clinical Measures				
Autism	.03	.01	4.21	<.001
Hairpulling	.01	.01	.64	.522
Skin-picking	.01	.01	1.79	.074
Tourette's Syndrome	.01	.01	.67	.501
<i>n</i> = 790				
Demographic model: MB index ~ scale(age) + factor(gender) + scale education)				
Independent clinical models: MB index ~ scale(i) + scale(age) + factor(gender) + scale(education)				
where each clinical measure is substituted for i				
Model-based index=subject-level reward x transition coefficient				
Gender coded as factor with males as reference				
Est = Estimate, Std Error = Standard Error, t = test statistic, p = significance value				

7.3.2.12 Model-fit indices comparing 4-factor vs. 3-factor structure

	χ^2	RMSEA	TLI	CFI
3-factor	30212	.069	.569	.59
4-factor	26019	.063	.634	.66

χ^2 = Chi Square statistic, RMSEA=Root mean squares of approximation, TLI=Tucker Lewis Index of Reliability, CFI=Comparative Fit Index.

For the chi-square statistic and RMSEA lower values indicate better model-fit, while for the TLI and CFI higher values indicate better fit.

Shaded areas=winning model

7.3.2.13 Item loadings from the exploratory 4-factor analysis

Factor 1	Factor 2	Factor 3	Factor 4	Item	Item description
.79	.02	.01	.06	STAI_1	I feel pleasant (R)
.85	-.02	.01	.02	STAI_3	I feel satisfied with myself (R)
.69	.15	.00	-.02	STAI_5	I feel like a failure
.61	.27	.03	-.08	STAI_8	I feel that difficulties are piling up so that I cannot overcome them
.45	.33	-.04	.01	STAI_9	I worry too much over something that really doesn't matter
.78	-.03	.02	.07	STAI_10	I am happy (R)
.56	.15	-.04	.12	STAI_12	I lack self-confidence
.78	.03	-.01	.00	STAI_13	I feel secure (R)
.78	.00	-.01	-.02	STAI_16	I am content (R)
.73	.08	.01	-.08	STAI_19	I am a steady person (R)
				STAI_20	I get in a state of tension or turmoil as I think over my recent concerns and interests
.55	.33	-.01	-.12		
.60	-.04	.04	.06	AES_2	I get things done during the day (R)
.50	-.25	.01	.23	AES_7	I approach life with intensity (R)
.52	-.12	.07	.23	AES_17	I have initiative (R)
.71	-.08	.01	.13	AES_18	I have motivation (R)
.08	-.07	-.01	.33	AQShort_1	I prefer to do things with others rather than on my own
.04	.34	-.01	.33	AQShort_2	I prefer to do things the same way over and over again
.13	-.06	.03	.29	AQShort_3	Trying to imagine something, I find it easy to create a picture in my mind (R)
.03	.42	.00	.16	AQShort_4	I frequently get strongly absorbed in one thing
-.13	.46	-.07	.19	AQShort_5	I usually notice car number plates or similar strings of information
.10	-.07	.02	.31	AQShort_6	Reading a story, I can easily imagine what the characters might look like (R)
-.26	.45	-.01	.21	AQShort_7	I am fascinated by dates
.21	-.10	.03	.38	AQShort_8	I can easily keep track of several different people's conversations (R)
.25	.06	-.01	.66	AQShort_9	I find social situations easy (R)
.00	.08	-.02	.40	AQShort_10	I would rather go to a library than to a party
.04	-.19	.04	.38	AQShort_11	I find making up stories easy (R)
.06	-.01	.02	.56	AQShort_12	I find myself drawn more strongly to people than to things (R)
-.32	.40	-.02	.28	AQShort_13	I am fascinated by numbers
-.01	.20	.02	.40	AQShort_14	Reading a story, I find it difficult to work out the character's intentions
.22	.12	.03	.55	AQShort_15	I find it hard to make new friends
-.16	.52	-.04	.24	AQShort_16	I notice patterns in things all the time
.02	.24	-.01	.37	AQShort_17	It does not upset me if my daily routine is disturbed (R)
.22	.06	.01	.42	AQShort_18	I find it easy to do more than one thing at once (R)
.07	.05	-.07	.51	AQShort_19	I enjoy doing things spontaneously (R)
.01	.05	.02	.54	AQShort_20	I find it easy to work out what someone is thinking or feeling (R)
.35	.18	.02	.33	AQShort_21	If there is an interruption, I can switch back very quickly (R)

-.21	.46	-.01	.19	AQShort_22	I like to collect information about categories of things
-.02	.21	.07	.39	AQShort_23	I find it difficult to imagine what it would be like to be someone else
.15	.05	-.02	.64	AQShort_24	I enjoy social occasions (R)
.07	.24	.07	.46	AQShort_25	I find it difficult to work out people's intentions
.32	.20	-.05	.44	AQShort_26	New situations make me anxious
.21	.02	-.04	.61	AQShort_27	I enjoy meeting new people
.03	.00	.01	.38	AQShort_28	I find it easy to play games with children that involve pretending (R)
.68	.04	.04	-.02	SDS_11	My mind is as clear as it used to be (R)
.71	.01	.00	.04	SDS_12	I find it easy to do the things I used to (R)
.22	.45	.01	-.17	SDS_13	I am restless and can't keep still
.71	-.09	.02	.13	SDS_14	I feel hopeful about the future (R)
.64	.04	.04	.12	SDS_16	I find it easy to make decisions
.76	-.06	.00	.14	SDS_17	I feel that I am useful and needed(R)
.60	-.08	.04	.16	SDS_18	My life is pretty full (R)
.75	-.03	.01	.10	SDS_20	I still enjoy the things I used to do
.21	.34	-.05	-.21	EAT_1	I am terrified about being overweight
.30	.32	-.08	-.25	EAT_11	I am preoccupied with a desire to be thinner
.09	.33	-.08	-.26	EAT_12	I think about burning calories when I exercise
.22	.38	-.06	-.25	EAT_14	I am preoccupied with the thought of having fat on my body
.03	-.03	.92	-.01	MGH_1	On an average day, how often did you feel the urge to pull your hair?
.03	-.01	.92	-.01	MGH_2	On an average day, how intense or "strong" were the urges to pull your hair?
-.01	-.02	.90	.03	MGH_3	On an average day, how much control do you have over the urges to pull your hair?
.01	.01	.90	-.03	MGH_4	On an average day, how often did you actually pull your hair?
.02	.00	.94	.00	MGH_5	On an average day, how often did you make an attempt to stop yourself from actually pulling your hair?
-.01	.01	.93	.00	MGH_6	On an average day, how often were you successful at actually stopping yourself from pulling your hair?
-.04	.06	.72	-.01	MGH_7	Hairpulling can make some people feel moody, "on edge," or sad. During the past week, how uncomfortable did your hairpulling make you feel?
.43	-.14	.04	-.11	BIS_1	I plan tasks carefully (R)
.32	.46	.02	-.18	BIS_6	I have "racing" thoughts
.61	.12	.05	.01	BIS_9	I concentrate easily (R)
.47	-.13	.06	.08	BIS_13	I plan for job security (R)
.16	.34	-.02	-.17	BIS_14	I say things without thinking
.33	-.28	.04	.06	BIS_15	I like to think about complex problems (R)
.22	.32	.00	-.32	BIS_17	I act "on impulse"
.53	.05	.04	-.07	BIS_20	I am a steady thinker (R)
.22	.29	-.02	-.26	BIS_22	I buy things on impulse
.29	.26	.04	-.24	BIS_25	I spend or charge more than I earn

.29	.45	-.06	-.15	BIS_26	I often have extraneous thoughts when thinking
.11	.47	.02	.00	OCIR_1	I have saved up so many things that they get in the way
.05	.58	-.05	.01	OCIR_2	I check things more often than necessary
-.08	.56	-.02	.16	OCIR_4	I feel compelled to count while I am doing things
.44	.54	-.02	-.05	OCIR_6	I find it difficult to control my own thoughts
.13	.54	-.04	.06	OCIR_7	I collect things I don't need
.05	.57	-.09	.20	OCIR_9	I get upset if others change the way I have arranged things
.00	.53	.02	.06	OCIR_11	I sometimes have to wash or clean myself simply because I feel contaminated
.34	.56	.00	-.06	OCIR_12	I am upset by unpleasant thoughts that come into my mind against my will
.11	.45	-.03	.10	OCIR_13	I avoid throwing things away because I am afraid I might need them later
-.09	.54	-.03	.07	OCIR_16	I feel that there are good and bad numbers
.31	.59	-.01	.00	OCIR_18	I frequently get nasty thoughts and have difficulty in getting rid of them
.13	.37	.15	-.04	SPS_1	How often do you feel the urge to pick your skin?
.10	.36	.16	-.02	SPS_2	How intense or "strong" are the urges to pick your skin?
.12	.37	.14	-.04	SPS_3	How much time do you spend picking your skin? How frequently does it occur? How much longer than most people does it take you to complete routine activities because of your picking?
.05	.37	.15	.04	SPS_4	How much does your skin picking interfere with your social or work (or role) functioning? (If currently not working determine how much your performance would be affected if you were employed)
.13	.43	.17	-.02	SPS_5	How much distress do you experience as a result of your skin picking? How would you feel if prevented from picking your skin? How anxious would you become?
.06	.36	.11	.02	SPS_6	Have you been avoiding doing anything, going any place, or being with anyone because of your skin picking? If yes, then how much do you avoid?
-.10	.30	.10	.08	MOVES_1	I make noises (like grunts) that I can't stop
-.08	.37	.14	.07	MOVES_2	Parts of my body jerk again and again, that I can't control
.23	.44	.15	.01	MOVES_3	I have bad ideas over and over, that I can't stop
-.11	.56	.04	.20	MOVES_4	I have to do things in certain order or certain ways (like touching things)
-.07	.41	.20	-.03	MOVES_5	Words come out that I can't stop or control
-.11	.40	.20	.06	MOVES_6	At times I have the same jerk or twitch over and over
.14	.45	.16	.01	MOVES_7	Certain bad words or thoughts keep going through my mind
-.05	.31	.14	.02	MOVES_8	I have to do exactly the opposite of what I'm told

.18	.44	.11	-.01	MOVES_9	The same unpleasant or silly thought or picture goes through my mind
-.05	.38	.18	.08	MOVES_10	I can't control all my movements
-.19	.49	.11	.12	MOVES_11	I have to do several movements over and over again, in the same order
-.13	.32	.14	-.03	MOVES_12	Bad or swear words come out that I don't mean to say
-.04	.38	.17	-.02	MOVES_13	I feel pressure to talk, shout, or scream
.20	.41	.16	-.01	MOVES_14	I have ideas that bother me (like germs or like cutting myself)
-.14	.42	.16	.01	MOVES_15	I do certain things (like jumping or clapping) over and over
.08	.43	.15	.08	MOVES_16	I have habits or movements that come out more when I'm nervous
-.08	.53	.14	.03	MOVES_17	I have to repeat things that I hear other people say
-.15	.51	.11	-.01	MOVES_18	I have to do things I see other people do
-.13	.24	.15	.00	MOVES_19	I have to make bad gestures (like the finger)
-.13	.53	.16	.05	MOVES_20	I have to repeat words or phrases over and over

(R) = Reverse coded item

7.3.2.14 Item loadings from the exploratory factor analysis with a 3-factor structure

Factor 1	Factor 2	Factor 3	Item	Item description
.80	.03	.01	STAI_1	I feel pleasant (R)
.83	-.02	.02	STAI_3	I feel satisfied with myself (R)
.65	.16	.00	STAI_5	I feel like a failure
.53	.28	.03	STAI_8	I feel that difficulties are piling up so that I cannot overcome them
.41	.34	-.05	STAI_9	I worry too much over something that really doesn't matter
.79	-.02	.02	STAI_10	I am happy (R)
.59	.17	-.05	STAI_12	I lack self-confidence
.75	.04	-.01	STAI_13	I feel secure (R)
.74	.01	.00	STAI_16	I am content (R)
.66	.09	.02	STAI_19	I am a steady person (R)
.44	.34	-.01	STAI_20	I get in a state of tension or turmoil as I think over my recent concerns and interests
.61	-.03	.04	AES_2	I get things done during the day (R)
.61	-.24	.00	AES_7	I approach life with intensity (R)
.62	-.11	.06	AES_17	I have initiative (R)
.76	-.07	.01	AES_18	I have motivation (R)
.25	-.06	-.03	AQShort_1	I prefer to do things with others rather than on my own
.18	.36	-.04	AQShort_2	I prefer to do things the same way over and over again
.27	-.05	.01	AQShort_3	Trying to imagine something, I find it easy to create a picture in my mind (R)
.08	.43	-.03	AQShort_4	I frequently get strongly absorbed in one thing
-.06	.48	-.09	AQShort_5	I usually notice car number plates or similar strings of information
.25	-.06	.00	AQShort_6	Reading a story, I can easily imagine what the characters might look like (R)
-.18	.46	-.04	AQShort_7	I am fascinated by dates
.40	-.08	.00	AQShort_8	I can easily keep track of several different people's conversations (R)
.56	.09	-.05	AQShort_9	I find social situations easy (R)
.19	.09	-.04	AQShort_10	I would rather go to a library than to a party
.24	-.18	.02	AQShort_11	I find making up stories easy (R)
.33	.01	-.02	AQShort_12	I find myself drawn more strongly to people than to things (R)
-.20	.41	-.05	AQShort_13	I am fascinated by numbers
.18	.21	-.01	AQShort_14	Reading a story, I find it difficult to work out the character's intentions
.48	.14	-.01	AQShort_15	I find it hard to make new friends
-.07	.54	-.07	AQShort_16	I notice patterns in things all the time
.19	.26	-.04	AQShort_17	It does not upset me if my daily routine is disturbed (R)
.41	.08	-.02	AQShort_18	I find it easy to do more than one thing at once (R)
.32	.07	-.11	AQShort_19	I enjoy doing things spontaneously (R)
.27	.07	-.02	AQShort_20	I find it easy to work out what someone is thinking or feeling (R)
.50	.20	.00	AQShort_21	If there is an interruption, I can switch back very quickly (R)
-.14	.48	-.04	AQShort_22	I like to collect information about categories of things

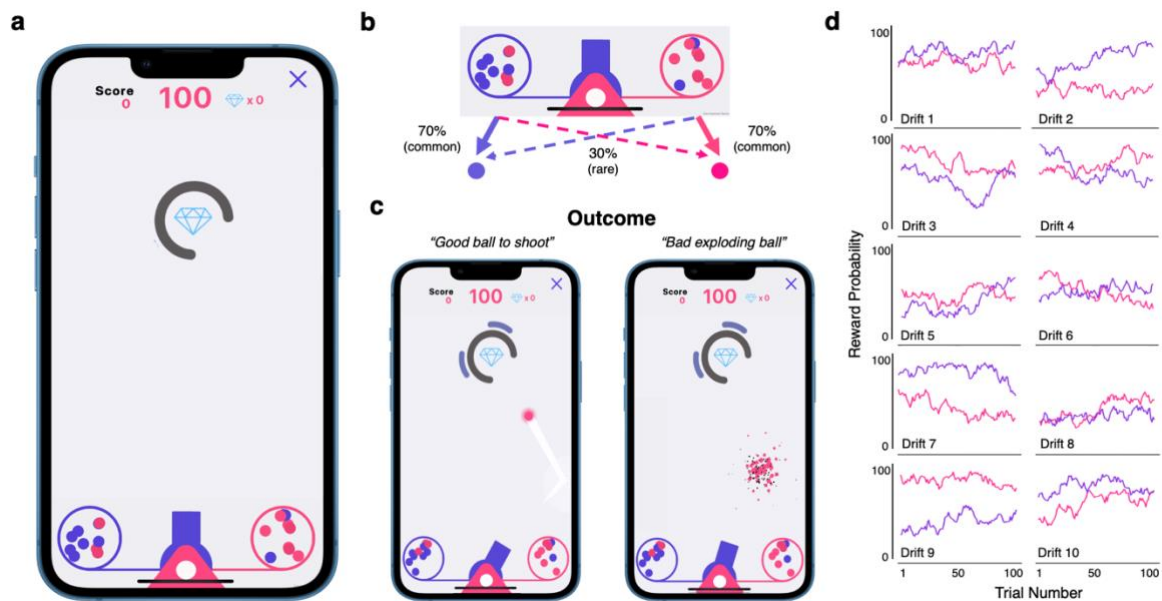
.16	.23	.04	AQShort_23	I find it difficult to imagine what it would be like to be someone else
.45	.07	-.06	AQShort_24	I enjoy social occasions (R)
.28	.27	.03	AQShort_25	I find it difficult to work out people's intentions
.51	.22	-.08	AQShort_26	New situations make me anxious
.50	.04	-.08	AQShort_27	I enjoy meeting new people
.22	.02	-.02	AQShort_28	I find it easy to play games with children that involve pretending (R)
.65	.05	.04	SDS_11	My mind is as clear as it used to be (R)
.71	.02	.00	SDS_12	I find it easy to do the things I used to (R)
.10	.45	.01	SDS_13	I am restless and can't keep still
.76	-.09	.02	SDS_14	I feel hopeful about the future (R)
.67	.05	.03	SDS_16	I find it easy to make decisions
.81	-.05	-.01	SDS_17	I feel that I am useful and needed(R)
.66	-.07	.04	SDS_18	My life is pretty full (R)
.78	-.02	.01	SDS_20	I still enjoy the things I used to do
.07	.34	-.04	EAT_1	I am terrified about being overweight
.14	.32	-.06	EAT_11	I am preoccupied with a desire to be thinner
-.07	.32	-.07	EAT_12	I think about burning calories when I exercise
.06	.38	-.05	EAT_14	I am preoccupied with the thought of having fat on my body
.03	-.03	.92	MGH_1	On an average day, how often did you feel the urge to pull your hair?
.03	.00	.92	MGH_2	On an average day, how intense or "strong" were the urges to pull your hair?
.01	-.02	.89	MGH_3	On an average day, how much control do you have over the urges to pull your hair?
-.01	.02	.90	MGH_4	On an average day, how often did you actually pull your hair?
.02	.00	.94	MGH_5	On an average day, how often did you make an attempt to stop yourself from actually pulling your hair?
-.01	.02	.92	MGH_6	On an average day, how often were you successful at actually stopping yourself from pulling your hair?
-.04	.07	.72	MGH_7	Hairpulling can make some people feel moody, "on edge," or sad. During the past week, how uncomfortable did your hairpulling make you feel?
.37	-.15	.05	BIS_1	I plan tasks carefully (R)
.19	.47	.03	BIS_6	I have "racing" thoughts
.59	.13	.05	BIS_9	I concentrate easily (R)
.50	-.13	.06	BIS_13	I plan for job security (R)
.05	.34	-.01	BIS_14	I say things without thinking
.37	-.28	.04	BIS_15	I like to think about complex problems (R)
.03	.32	.02	BIS_17	I act "on impulse"
.47	.05	.05	BIS_20	I am a steady thinker (R)
.06	.29	-.01	BIS_22	I buy things on impulse
.14	.26	.05	BIS_25	I spend or charge more than I earn
.17	.45	-.06	BIS_26	I often have extraneous thoughts when thinking
.07	.48	.01	OCIR_1	I have saved up so many things that they get in the way
.01	.60	-.07	OCIR_2	I check things more often than necessary
-.04	.58	-.04	OCIR_4	I feel compelled to count while I am doing things
.36	.55	-.02	OCIR_6	I find it difficult to control my own thoughts
.12	.56	-.06	OCIR_7	I collect things I don't need

.11	.59	-.12	OCIR_9	I get upset if others change the way I have arranged things
-.01	.55	.00	OCIR_11	I sometimes have to wash or clean myself simply because I feel contaminated
.26	.58	.00	OCIR_12	I am upset by unpleasant thoughts that come into my mind against my will
.13	.47	-.05	OCIR_13	I avoid throwing things away because I am afraid I might need them later
-.09	.56	-.05	OCIR_16	I feel that there are good and bad numbers
.26	.61	-.02	OCIR_18	I frequently get nasty thoughts and have difficulty in getting rid of them
.08	.38	.14	SPS_1	How often do you feel the urge to pick your skin?
.06	.37	.16	SPS_2	How intense or "strong" are the urges to pick your skin?
.07	.38	.14	SPS_3	How much time do you spend picking your skin? How frequently does it occur? How much longer than most people does it take you to complete routine activities because of your picking?
.04	.38	.14	SPS_4	How much does your skin picking interfere with your social or work (or role) functioning? (If currently not working determine how much your performance would be affected if you were employed)
.09	.44	.16	SPS_5	How much distress do you experience as a result of your skin picking? How would you feel if prevented from picking your skin? How anxious would you become?
.04	.37	.10	SPS_6	Have you been avoiding doing anything, going any place, or being with anyone because of your skin picking? If yes, then how much do you avoid?
-.07	.31	.08	MOVES_1	I make noises (like grunts) that I can't stop
-.06	.38	.12	MOVES_2	Parts of my body jerk again and again, that I can't control
.20	.46	.14	MOVES_3	I have bad ideas over and over, that I can't stop
-.05	.58	.01	MOVES_4	I have to do things in certain order or certain ways (like touching things)
-.11	.42	.18	MOVES_5	Words come out that I can't stop or control
-.11	.42	.18	MOVES_6	At times I have the same jerk or twitch over and over
.10	.46	.14	MOVES_7	Certain bad words or thoughts keep going through my mind
-.06	.32	.13	MOVES_8	I have to do exactly the opposite of what I'm told
.14	.46	.10	MOVES_9	The same unpleasant or silly thought or picture goes through my mind
-.04	.39	.16	MOVES_10	I can't control all my movements
-.16	.51	.09	MOVES_11	I have to do several movements over and over again, in the same order
-.17	.34	.13	MOVES_12	Bad or swear words come out that I don't mean to say
-.08	.39	.16	MOVES_13	I feel pressure to talk, shout, or scream
.16	.42	.15	MOVES_14	I have ideas that bother me (like germs or like cutting myself)
-.16	.43	.14	MOVES_15	I do certain things (like jumping or clapping) over and over
.08	.45	.13	MOVES_16	I have habits or movements that come out more when I'm nervous
-.10	.55	.13	MOVES_17	I have to repeat things that I hear other people say
-.18	.53	.09	MOVES_18	I have to do things I see other people do
-.14	.25	.14	MOVES_19	I have to make bad gestures (like the finger)
-.14	.55	.14	MOVES_20	I have to repeat words or phrases over and over
(R) = Reverse coded item				

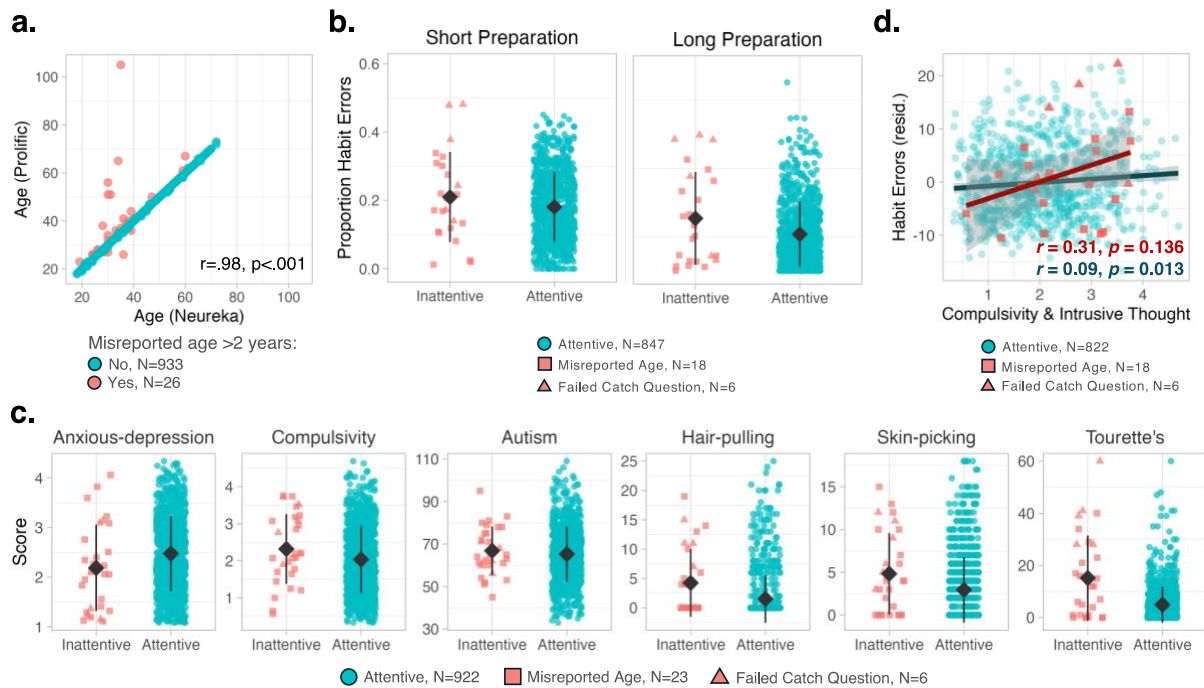
7.3.2.15 Regression coefficients of the association between stimulus-response learning and factors from the supplementary exploratory factor analysis

	Est	Std Error	t	p
Independent models				
Factor 1 ('anxious-depression')	.21	.25	.85	.395
Factor 2 ('repetitive behaviours')	.56	.25	2.23	.026
Factor 3 ('autism')	-.16	.24	-.66	.510
Covariate models				
Factor 1 ('anxious-depression')	-.04	.28	-.16	.875
Factor 2 ('repetitive behaviours')	.59	.28	2.07	.038
Factor 3 ('autism')	-.17	.24	-.71	.480
Independent model: number of habits ~ scale (i) + scale(age) + gender + scale(education) + other errors, where I is substituted for each factor				
Covariate model: number of habits ~ scale (F1) + scale (F2) + scale (F3) + scale(age) + gender + scale(education) + other errors				
Shaded grey areas=significant results				
Est = Estimate, Std Error = Standard Error, t = test statistic, p = significance value				

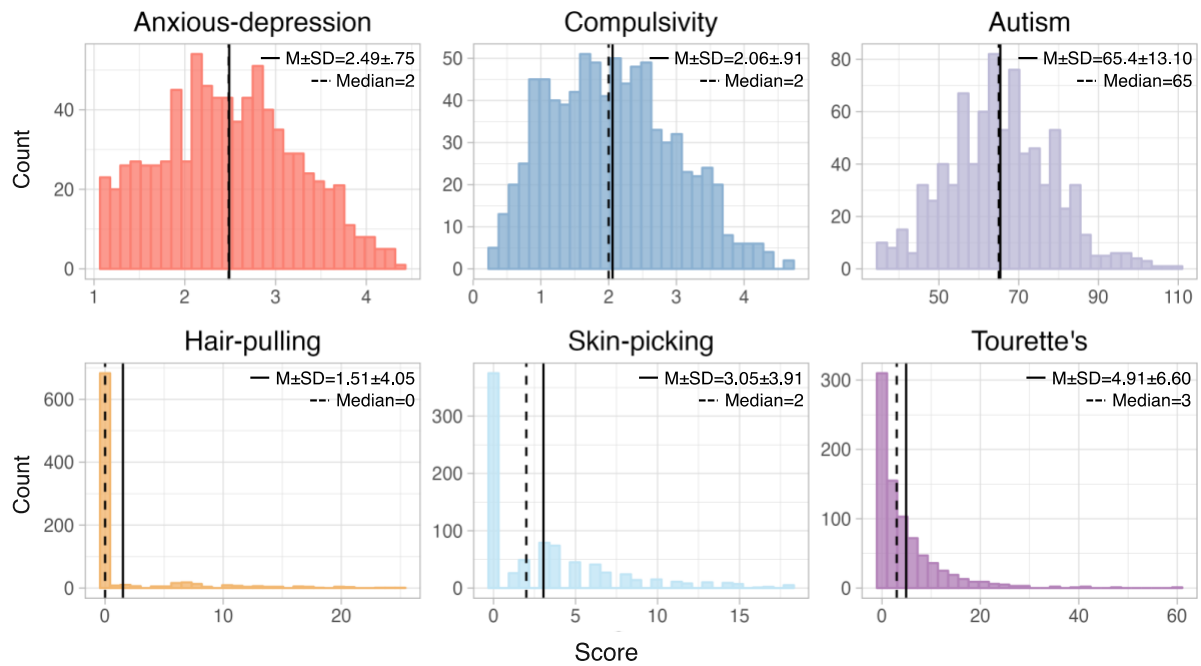
7.3.3 Supplementary Figures



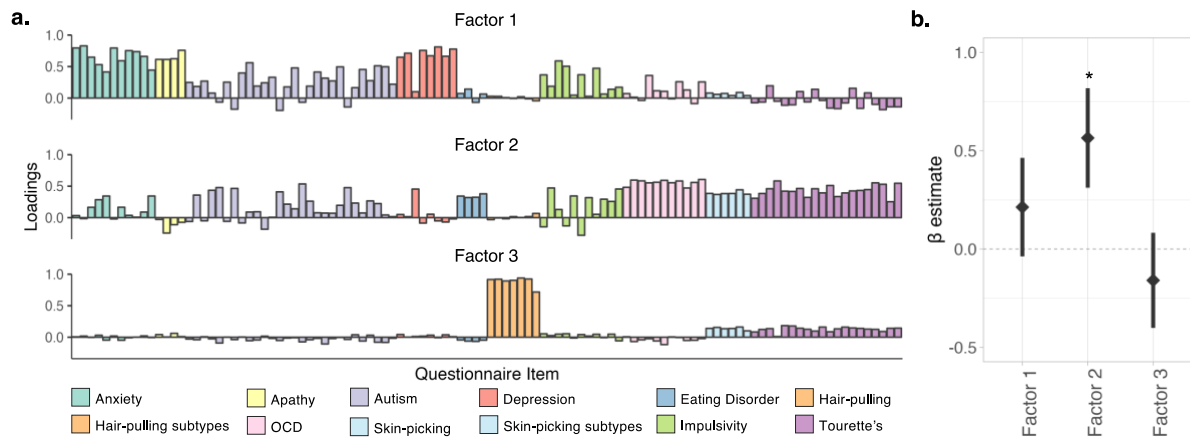
Supplementary Figure 7.3.3.1. Task structure of Cannon Blast, a smartphone game to assess model-based planning from Donegan et al., (2023). **a.** In this game, participants' goal is to shoot as many diamonds as possible in a finite amount of shots (100 shots). To do so, they must aim a central cannon and then select which container to draw from. **b.** Purple and pink balls dynamically bounce around each of the flanked containers which depict the probability of that colour ball being released. For example, the left container displays 7 purple balls and releases a purple ball 70% of the time ('common' transition) and displays 3 pink balls, giving a pink ball on 30% of trials ('rare' transition). **c.** The value of the ball is defined as the probability of it being a 'good ball', i.e. one that remains intact after firing (rewarding trial), or a 'dud ball' (non-rewarding trial) that explodes shortly after being fired, and therefore cannot reach the diamond. **d.** The probability of being rewarded with a good ball to shoot with drifts for each colour over the course of the task. Participants were randomly assigned to one of ten possible drift sets.



Supplementary Figure 7.3.3.2. Impact of careless/inattentive responder on task and questionnaire data. **a.** Consistency of age reported on Prolific and Neureka was highly correlated. Individuals who misreported their age greater than two years are coloured in red. Each dot reflects a participant. **b.** Proportion of habitual errors made across those identified as inattentive and inattentive at short and long preparation times. Dot reflects mean $\pm$ standard deviation. **c.** Dimensional factor scores and individual clinical questionnaire total scores between inattentive and attentive responders. Dot reflects mean $\pm$ standard deviation. **d.** Correlation between residual habit errors (controlling for age, gender, education and other errors) and 'Compulsivity and Intrusive Thought' in the inattentive group was stronger than the same effect reported in the attentive group. For **a-d**, each dot reflects a participant. Attentive responders coloured in blue, while inattentive responders coloured in red (misreported age=square, failed attention check=triangle).



Supplementary Figure 7.3.3.3. Distributions of transdiagnostic dimension scores and independent clinical total scores in a subset of participants for individual difference testing ($n = 822$). Solid line reflects the mean, while dashed line reflects the median value. M=Mean, SD=Standard deviation.



Supplementary Figure 7.3.3.4. Supplementary exploratory factor analysis with 3-factor structure. **a.** Individual item-level loadings onto each of the three identified factors. Colour-codes reflect the clinical measure. **b.** Regression coefficients from model with participant factor weighted scores. Dots reflect beta regression estimates $\pm$ standard error from independent models controlling for age, gender, and education.

7.3.4 Supplementary References

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