Eindhoven, April 2025

Video Games as a Novel Habit Research Paradigm

by Paul Evers

Student Number: 1261215

in partial fulfilment of the requirements for the degree of

Master of Science in Human-Technology Interaction

Supervisors:
Dr. Chao Zhang
Dr. Max Birk

Abstract

This study introduces a video game paradigm as an innovative approach to experimentally induce and study habit in human participants. Habits play a fundamental role in human behavior, influencing daily decision-making and actions. However, experimental research on habit formation has faced challenges in reliably replicating its effects as well as its limited ecological validity. Due to the increased interest in cognitive games, we proposed to develop a video game based on the outcome-devaluation task, a common method to experimentally study habit, with the expectation to more reliably induce habit as well as offer greater ecological validity. The game was designed to encourage participants to develop a habitual response by rewarding consistent behavior across multiple training rounds. Subsequently, this behavior was devalued to test whether participants continued the habit despite the change in incentive. The study followed a within-subject design across 3 days and employed both behavioral as well as self-report measures to assess habitual behavior. Results demonstrated that extensive training significantly increased RT switch costs, suggesting a stronger habitual control over behavior compared to the moderate training condition. Despite this, action slips and self-reported habit strength did not differ between conditions. These findings align with other studies on habit, indicating that video games hold the potential as an effective and engaging experimental paradigm for studying habit formation in controlled environments. Future research may build upon this approach to further explore and study habit. However, some significant shortcomings need to be addressed and considered for future research.

Keywords: Habit induction, outcome-devaluation task, game-based experiment, cognitive game

Acknowledgements

I would like to express my gratitude for the help and support of my supervisor Chao Zhang who was always prepared to brainstorm ideas with me and guided me whenever I got stuck. He has been very helpful and has given me many insights into conducting this master thesis. In addition, I want to thank Max Birk for giving me his expertise on videogames in psychology research and providing me with valuable tips on how to design the game and experiment. Finally, I wish to thank my family, my parents, grandparents and brother, as well as my dear friends who always support me no matter what. I want to give special thanks to my mom, who even during her hardest times remains one of my biggest supporters and sources of inspiration.

Video Games as a Novel Habit Research Paradigm

Habits play a crucial role in human behavior, routines, and influencing many daily decision-making processes. It is estimated that about a third to half of our daily behaviors are habitual in nature (Wood et al., 2002). Habits, by their definition, are behaviors that are performed with a certain degree of automaticity after being exposed to an associated cue. They are formed through so-called context-dependent repetition (Lally et al., 2010). In other words, performing a certain behavior repeatedly in a specific context strengthens the cue-association which at a certain point, when exposed to the cue again, makes people prefer the habitual behavior over other alternative behaviors. Cues can take form in a wide variety of ways including objects (e.g. not resisting a chocolate muffin), locations (e.g. clocking in at work), times (e.g. getting ready for bed at night), psychological state s(e.g. grabbing a soda when thirsty), or activities (e.g. getting a coffee when starting work; Verplanken & Orbell, 2019). Even more abstract contexts such as socializing or being at work can serve as cues to elicit habitual behavior (Shiffman et al., 1997). In contrast, goal-directed behavior is instrumental in nature. This means a person expects a certain outcome from the action they perform, which is different from a habitual behavior in which the expectation of an outcome is based on the associated cue (de Wit & Dickinson, 2009). This signifies that goal-directed behaviors are more purposeful than habitual behaviors and require more conscious thought. However, it is assumed that most habitual behaviors actually start out as goal-directed behavior before transforming into habitual behavior after repeated execution in a stable context (Danner et al., 2008).

Habitual behavior in humans can have great implications. Habits can be beneficial and support positive health-related behaviors, such as the habitual brushing of one's teeth every night (Zhang et al., 2022) or support regular physical activity (Hawlader et al., 2023). Habits can be maladaptive in nature too. Think of mindlessly eating a bag of chips during a movie or scrolling on your phone for too long instead of going to sleep. Even different psychopathologies such as eating disorders and compulsive disorders are known to have a habitual component to them (Marsh et al., 2007; Gillan et al., 2011). Evidently, it becomes clear that the study of habit and understanding the underlying cognitive and neurological mechanisms can bring about interventions that could positively impact the lives of many people.

Unsurprisingly, research into habit has gained traction over the years, from research areas such as neuroscience to cognitive psychology and applied health psychology. Many of these include field studies which have the benefit of studying habit in real-life settings. Results from field studies have shown that habit-related interventions can positively impact behavior in areas such as physical activity and dental health (Fournier et al., 2017; Judah et al., 2013). The very nature of field studies provides them with a high ecological validity, making their findings ideal for real-life applications. However, due to their deployment in dynamic real-world environments, it is hard for researchers to control variables, making it harder to isolate the precise mechanisms that drive habits from field studies.

On the contrary, experimental studies on habit are better at controlling variables and manipulations, but often at the loss of ecological validity. This has brought into question whether findings from experimental paradigms hold up in the real world (Nebe et al., 2024). Nonetheless, experimental control strengthens the causal interpretation of findings, helping to better understand habit at a more fundamental level. A common way to experimentally study habit that has shown successful results in rodents is through the so-called outcome-devaluation tasks (Pierce-Messick & Corbit, 2024). In short, outcome-devaluation tasks rely on building a cue-outcome association through repeated exposure (often referred to as training). Afterwards, the value of the outcome is diminished or removed (devaluation). If the subject still responds to the cue with the habitual behavior instead of goal-directed behavior, even though the outcome is devalued, it can be confidently stated that habit has been successfully induced. Similar experiments have been conducted on humans as well, for example a study by Tricomi et al. (2009) also found that the amount of training influenced the strength of the habitual behavior overriding goal-directed behavior with more training leading to a higher habit strength. Despite this, many outcome-devaluation experiments on humans have proven to be elusive in identifying habitual patterns. Most notably, the study by de Wit et al. (2018) replicated 5 rodent studies on habit on humans. Whereas the original studies were able to successfully identify habitual behavior, the results failed to replicate in human participants. De Wit et al. reasoned that a possible explanation for this is that humans are particularly good in overriding habits and that Tricomi et al.'s success could be attributed to environmental distractors such as noise. Nevertheless, it appears that the study of habit in humans, and particularly through outcome-devaluation tasks, is no easy feat.

Thus, to summarize, the experimental study of habit currently has two problems: firstly, ecological validity is low, making it hard to confirm whether findings still hold and can be applied in more chaotic real-life situations. Secondly, current experimental paradigms report mixed results in inducing and observing habitual behavior. Therefore, we set out to explore a new paradigm that can facilitate ecological validity and successfully induce habit while still maintaining a high degree of experimental control. To our knowledge, this is the first attempt to translate the outcome-devaluation task into a video game format.

In recent years, video games have been increasingly recognized not only as a form of entertainment but also as a new paradigm for research. Gamification is the use of game design elements within non-game contexts (Deterding et al., 2011) and has been applied to a variety of domains such as rehabilitation (e.g. Adlakha et al., 2020), education (e.g. Landers & Landers, 2014), training (e.g. Markopoulos et al., 2015), and health (Jones et al., 2014). Because of the entertainment value that video games pose, applications that are often considered mundane have found increased interest, and engagement among participants in their gamified counterparts. Similarly, as Markovitch et al. (2024) note, cognitive tasks are often repetitive and long which disengages and demotivates participants, skewing both results and ecological validity. Unsurprisingly, an increased interest in the gamification of classical cognitive tasks such as the Stop-Signal and Dot-Probe Task (Friehs et al., 2020; Wiley et al., 2020) has taken place to test its effect on performance and experience of participants. Most results seem to point out that cognitive games do in fact positively impact player experiences but not necessarily performance compared to their original tasks. Despite this, even though outcome-devaluation tasks are typically lengthy and repetitive, they have not yet been adapted into more engaging, game-based formats. Introducing game elements could address some of the challenges inherent in these tasks. For instance, gamification might improve participant motivation and engagement throughout long experimental sessions, leading to more consistent performance and potentially more reliable data. Enhanced motivation could also reduce attrition rates and increase the likelihood that participants complete the task as intended. Moreover, habitual behaviors are already known to emerge organically in video game contexts. Players often develop routines, such as harvesting crops in a farming simulator or reflexively reloading a gun after a gunfight. These behaviors are often repeated in specific contexts and triggered by consistent cues, closely resembling the way habits form in real life. This suggests that video games may already support the type of context-dependent repetition necessary to develop strong cue-outcome associations. In addition, video games offer a unique opportunity to simulate complex, dynamic environments that better reflect real-world settings than traditional lab tasks. While typical outcome-devaluation paradigms prioritize experimental control by simplifying context and limiting variables at the cost of ecological validity. Video games, in contrast, can integrate complexity while preserving control over variables and manipulations, allowing for the precise measurement of behavior in scenarios that feel more lifelike to participants and possibly making findings more applicable to real-world contexts. Because of this we proposed our main research question: *Can a video game paradigm successfully induce habit in an experimental set-up?*

To answer this question, we developed a cognitive game based on the outcome-devaluation task aimed at inducing and observing habit in experimental settings while still maintaining a high degree of ecological validity. In the game, the player, over the course of several levels, needed to navigate towards the exit before their time ran out. During training rounds, coins (the cue) appeared throughout the maze that could be collected for extra points to form a cue-outcome association. Afterwards, during test rounds, the value of the coins was removed and no longer rewarded points. Data collected during these rounds would help us assess whether participants' reaction to the coin was affected by their habitual tendencies. We chose a maze game because of its intuitive gameplay and easy-to-understand objective. Moreover, the repetitive nature of maze navigation further aligns with the conditions necessary to induce habits through repeated behavior in a stable context.

Should this paradigm prove effective, it could significantly open up new opportunities to study habit by offering more engaging and ecologically valid experimental designs. This would not only advance our theoretical understanding of the cognitive mechanisms underlying habitual behavior but also offer valuable insights for designing interventions that effectively target maladaptive habits in domains such as mental and physical health.

Background

Defining Habit

Before we delve deeper into habit related theories, it is important to establish what a habit precisely is. Although definitions vary slightly depending on the research context, many features of habit are largely consistent across studies and researchers. In the domain of health psychology, Gardner (2015) defines habit as a process in which a stimulus generates an impulse to act as a result of a learned stimulus-response association. He explains that habits form through repetition of behavior and outcome in a specific context further reinforcing a context-behavior association. As habits develop through repetition, when exposed to the same environmental cues, habits have the potential to activate the related behavior again. These cues are stored in memory rather than triggered by a conscious deliberate choice and can manifest in a variety of ways such as time, location, objects, people, physiological states, or activities (Verplanken & Orbell, 2022). Shiffman et al. (1997) even found that more abstract concepts such as socializing or having a smoke with others could serve as cues to trigger habitual behavior. Similarly, from a cognitive psychological perspective, Verplanken & Orbell (2022) describe habits as memory-based propensities that automatically lead to a response when triggered by a cue that led to performance in the past. They define habit as not the act itself, but a propensity to act. Once a habitual cue-outcome association has been established, they are very resistant to change and take significantly more effort to override compared to non-habitual behaviors. Additionally, Gardner (2015) describes the execution of habits as without awareness, conscious control, cognitive effort, or deliberation; often described as automaticity. This aspect of habit makes the execution of habitual behavior require minimal cognitive effort—enabling a person to perform other tasks during the habitual behavior, i.e., multi-task. It can also lead to a certain "tunnel vision" in which the person has no attention or interest in the habit or its context of performing its associated behavior (Verplanken & Orbell, 2019). This can lead to a so-called action slip: the circumstance in which a habitual behavior takes precedence over an intended more appropriate behavior (Sjoerds et al., 2016). Wood et al. (2022) give a practical example of how habits can cause action-slips in real-life: a professor heading to the campus enters the doorway they have used for years, despite knowing that the door is closed for maintenance. Nonetheless, due to their habits, the professor mindlessly attempts to walk through the door at which point they realize their mistake and switches to a more deliberate strategy to find the new entrance. This demonstrates how action-slips caused due to habitual behavior can conflict with the instrumental goals of a person.

In contrast, goal-directed behaviors are actions performed with the intention of achieving a specific outcome. They do not rely on the activation of the action by a cue, but rather the instrumental goal itself. Because of this, they are flexible and better adapted to changing circumstances as the decision-making process for goal-directed behaviors is more reliant on evaluating different options and outcomes. However, this comes at the cost of a higher cognitive load due to a more conscious deliberation often leading to people relying on their habitual tendencies (Gera et al., 2024). Habits often start out as goal-directed intentions. For example, someone might start snacking at work to reduce hunger. After enough repetition, the association with food and the office is established. Now when at the office, the person automatically starts snacking, even when not hungry; a habit has been established. As such, habit formation has been described as the process by which behavioral control switches from goal dependence to context dependence (Mazar & Wood, 2018).

Many cognitive models on habit are based on the assumption of a dual system (Mazar & Wood, 2018) in which behavioral choices are either goal-directed or habitual behaviors. This system is often described as a conflict in which either the habit or the goal-directed behavior "wins" over the other. Daw et al. (2005) describe an internal arbitration system that chooses between the two behaviors based on the predictability of its outcomes. The system prefers habitual behavior when outcomes are more predictable and prefers a goal-directed response when outcomes are less predictable. Initially, goal-directed behavior takes precedence as the person explores the outcome of the executed behavior. As the person becomes more familiar with the outcome in a stable context, the habitual system becomes more dominant. Others have formulated similar arbitration models (e.g. Miller et al., 2019).

In contrast, alternative models have been proposed too, such as that of Tobias (2009). A distinction that Tobias makes is that he considers habit as a facilitator to remembering and enabling goal-directed behavior rather than a conflict that needs to be arbitrated. His model explains how and when people remember and repeat actions in their prospective memory. He argues that for a behavior to be executed it must be (1) possible, (2) the behavior option must be remembered, and (3) the behavior must have a higher preference than competing available behaviors. For a behavior to be selected is mainly dependent on two key factors: accessibility and threshold. Firstly, accessibility is how easily a behavior comes to mind when needed and is improved by, among other things, performing the behavior frequently. Secondly, the threshold determines the level of accessibility required for a behavior to be remembered and executed. The threshold is reduced when habit strength increases, making habitual behaviors easier to access even when cognitive resources are limited. Habit strength is, according to Tobias, increased by associating the behavioral performance with situational cues which is again similar to other dual-process models as described by others (e.g. Mazar & Wood, 2018). Additionally, when cognitive resources are limited or distractions are present, the threshold rises, making it harder to recall intended behaviors.

Existing Habit Research Paradigms

A significant amount of effort has been made in understanding and researching habit. Two of the most common paradigms are field studies and experimental studies through outcome-devaluation tasks. In the following sections we will discuss both. Firstly, we will discuss when field studies on habit have been applied, their effectiveness, what measurements are used, and finally their advantages and disadvantages. Secondly, we will examine outcome-devaluation tasks which is a common method for researchers to experimentally induce and study habit. We will discuss their (dis)advantages, how outcome-devaluation tasks have been applied, their struggle to replicate in humans, and potential new metrics to better detect habitual behavior in humans. By evaluating both the strengths and weaknesses of field and experimental studies we can more accurately assess what necessary improvements can be made for subsequent novel paradigms.

Field Studies

Longitudinal field studies are a common way to observe and study habit, especially in health-related contexts. Often, these studies are aimed at manipulating different aspects of habit formation (e.g. placing a cue in the participant's context) and its influence on the participant's behavior—potentially leading to effective healthcare interventions. Examples of such studies include, promoting physical activity (e.g. Fournier et al., 2017), promoting healthy diets (e.g. van der Weiden et al., 2020), or promoting dental flossing (e.g. Judah et al., 2013) to name a few. A meta-study by Singh et al. (2024) analyzed 20 health-related field studies on habit formation and found significant improvement from pre- to post-interventions when leveraging healthy habit formation. Most field studies on habit rely on self-report questionnaires to assess habit strength in participants. Historically, the Self-Report Index of Habit Strength (SRHI; Verplanken & Orbell, 2003) is commonly used. The SRHI is a 12-item instrument that is self-reported by participants on a 7- or 11-point Likert scale from 1 (agree) to 7 (disagree) on different statements. Statements are phrased as "Behavior X is something I do frequently" or "Behavior X is something I do without thinking". The scale is designed to measure different features of habit such as the history of repetition of behavior, automaticity, and an identity element. The original study found high internal and test-retest reliability as well as strong correlations with past behavioral frequency and the response frequency measure of habit. Subsequent studies that implemented the SRHI found similar results (Gardner et al., 2011) thus rendering the SRHI as the de-facto standard for assessing habit strength in field studies. However, more recently, a shortened subscale of the SRHI, the Self-Report Behavioural Automaticity Index (SRBAI; Gardner et al., 2012), is commonly applied as well. The authors of the SRBAI found that the repeated behavior and identity subscale were redundant in measuring habit, unnecessarily burdening participants who already criticized its length resulting in less reliable responses and attrition. They found that the automaticity subscale on its own proved adequate in measuring habit strength. This drastically reduced the total items on the SRBAI from 12 to 4, making it less burdensome for participants to complete. Items on the scale are similarly scored and phrased as the SRHI. The SRBAI too found high reliability in subsequent studies (e.g. Feil et al., 2021; Raison & Harris, 2024) and has arguably become the preferred method of measuring habit strength through self-report measures.

There are several advantages of researching habit through field studies: first, there is a higher number of ecological validity—research is conducted in natural environments and therefore findings are more reflective of real-life behaviors and therefore more applicable too. Second, mundane realism is greater and influence of the Hawthorne Effect is smaller—as participants partake in the study in their own environments, integrating habits and interventions in their own life, they are less conscious of being part of a study, thus rendering their behaviors as more genuine; and finally, habits can take a variety of time to develop but can take an average of roughly 1 to 4 months to develop (Lally et al., 2010)—longitudinal field studies can measure participants over a longer period of time with minimal amount of obtrusiveness.

However, field studies in habit research face several limitations too. First, natural environments are highly variable, and changes in context can confound results by interrupting or altering habit cues. In real-world settings, a behavior that is automatic in one context might not be triggered if the cues or environment differ. Such contextual variability means field studies have less control over the stable cues that normally sustain automatic habits, making it harder to observe consistent habit expression (Stojanovic et al., 2022). Because of this lack of control, it is harder to deduce causal relationships from the findings of field studies. Secondly, as previously discussed, field studies often rely on self-reported behavior frequencies or habit ratings, which introduces bias–participants may misremember or give socially desirable answers about their habitual behaviors, especially in health-related fields where field studies are often conducted (van de Mortel, 2008). Moreover, a core feature of habit is its automaticity, something that is hard to capture through field measures. Field studies typically lack direct measures of automaticity, forcing researchers to use less objective measures such as surveys that may not fully reflect the non-conscious aspects of habit behavior. These issues, the highly variable contexts, self-report biases, and difficulties measuring unconscious automatic processes, highlight the challenges of field studies in habit research.

Outcome-Devaluation Tasks

Experimental research on habit addresses many of the shortcomings that field research carries. Problems such as uncontrolled environments leading to the potential presence of confounding variables can more easily be controlled for in experimental set-ups. In turn, this makes it easier to more confidently claim causal relationships. However vice versa, advantages of field studies are mostly missing for experimental studies. They are less ecologically valid and often lack the intricacies of real-world contexts which makes the applicability of findings less reliable. That is not to say that the experimental study of habit carries great importance in understanding and modelling the underlying cognitive mechanisms of habit.

One way to study habit experimentally is by exploiting the habit-goal conflict to which the outcome-devaluation task is among the most popular ways to do so. The outcome-devaluation paradigm, first introduced by Adams & Dickinson (1981), is used to differentiate between goal-directed and habitual behaviors. In this procedure, an animal or human subject is trained to perform an action to receive a valued outcome (e.g. food) associated with a cue to establish a cue-outcome association, i.e., a habit. Following the initial training, the value of the outcome is reduced, for example by satiating the subject or poisoning the food. The subject is then presented with the cue and given the opportunity to perform the instrumental action again. If during this test the behavior is goal-directed, the subject should respond less, because the value of the outcome has been diminished or removed—leaving no need to execute the behavior. Otherwise, if the subject did in fact respond, it would have been so out of habit due to the cue-outcome association. The initial findings of the outcome-devaluation task on rodents supported the idea that after the cue-outcome association was strong enough, habitual behavior would take precedence over goal-directed behavior. Other animal studies too demonstrated that after extensive training (overtraining), rodents were unable to immediately and flexibly adjust their behavior when the outcome no longer constituted a goal, and thus relied more heavily on habitual behavior compared to rodents with no or minimal training (De Wit & Dickinson, 2009).

The outcome-devaluation paradigm has also been applied to study habit in humans. Tricomi et al. (2009) conducted a study on whether the outcome-devaluation task could be induced in humans following overtraining. Results showed that the 1-day training group reduced their response rates for the devalued reward, indicating goal-directed behavior, while the 3-day training group continued to respond for the devalued reward, indicating habitual behavior. Thus, Tricomi et al., seemed to confidently show the reproducibility of the outcome-devaluation task in humans. However, despite Tricomi et al.'s success, a study by de Wit et al. (2018) found a discrepancy between animal and human studies on habit formation. In their study, they replicated 5 studies on habit formation in rodents on human participants. De Wit et al. detailed five experiments designed to induce habitual behavior through extensive training, using variants of previously published outcome-devaluation paradigms structured as an outcome-devaluation task, including instrumental training, outcome devaluation, and a test phase in extinction to assess the impact of devaluation on previously learned behavior. The central measure was the degree to which participants persisted in responding to devalued outcomes, which would indicate the formation of stimulus-response habits. Across these experiments, all of them failed to find evidence for increased habit strength as a consequence of overtraining in humans. The authors suggested that this might be because current outcome-devaluation tasks tap predominantly into goal-directed control. They also allow for the possibility that overtrained habits play a moderate role in the outcome sensitivity of human action control or that the training durations were insufficient to instill sufficiently strong habits. The authors suggest that paradigms with a single response-outcome contingency might be more effective. They also suggest that, to prevent a conscious, goal-directed strategy from dominating performance, experimenters should embed such a simple task within a more complex task or be offered concurrently with another task, distractors, or stressors. They also use this as an explanation as to why Tricomi et al.'s study did successfully induce habit. Their participants were in a noisy fMRI machine, potentially serving as a distractor, thus limiting goal-directed action control. This is similar to Tobias' (2009) ideas on reduced cognitive resources limiting goal-directed functioning. Similarly, studies such as that of Schwabe & Wolf (2010) and Hartogsveld et al. (2020) found that adding stressors resulted in a preference for habitual over goal-directed behaviors. In conclusion, de Wit et al.'s findings highlight the difficulty of experimentally inducing habits in humans. The authors suggest that future research should focus on developing new paradigms and theories and more critical investigations.

Luque et al. (2020) proposed that experimental research on habit is not focused on the right outcome variables. Instead of relying on overt response selections in participants (i.e., action slips), Luque et al. proposes a novel more subtle metric: response time (RT) switch cost—the increase in response time when a participant has to switch from a previously optimal response during training to a different now-more-appropriate response during devaluation. The cost in response time is then attributed to a conflict between the goal-directed and habitual system. They confirmed their hypothesis in 3 separate experiments. Each experiment followed a within-subject design and was structured like an outcome-devaluation task over the course of 3 consecutive days. Results showed that RT switch cost increased with overtraining, especially under time pressure. This is, again, presumably due to inducing stress in participants and thus limiting cognitive function resulting in overly relying on habitual behavior. Moreover, explicit action slips did not increase with overtraining as expected from the previously failed replications. Later studies that used RT switch cost as a criterion for habit found similar results (e.g. Jiang et al., 2024) thus giving reasonable credit to RT switch cost as a valuable metric for habit strength. However, at the time of writing no comprehensive large-scale literature study has validated the effectiveness of RT switch cost.

Additionally, Hardwick et al. (2019) found that the time given to respond to a cue influenced whether goal-directed or habitual behavior would dominate. Results showed that habitual responses were automatically prepared at short latency but are replaced by goal-directed responses at a longer latency. This would explain why action slips are fairly rare in outcome-devaluation tasks in humans as people have enough time to switch to a goal-directed strategy. However, this does not mean that habit did not affect behavior selection. When participants were limited to respond in an incredibly short time span, participants more likely executed habitual responses instead. This further supports the idea that habit cannot be exclusively derived from the action outcome but should rather be observed through more subtle ways (such as RT switch costs).

Using Video Games for Cognitive Psychology Research

While never been applied to habit research, video games as research paradigms have increasingly garnered interest. The process of applying game design elements in other contexts than video games themselves is referred to as gamification (Deterding et al., 2011) and has already been used in a variety of fields. For example, domains such as rehabilitation have found increased motivation and adherence to exercise treatment when undergone gamification (Alfieri et al., 2022). Another example, education has also benefitted from applying gamification principles—showing that students are more engaged, motivated, and experience a greater sense of achievement (Zeybek & Saygı, 2024). These are a few practical examples of how adding game elements can greatly increase the experience of the player. Video games have the ability to tap into intrinsic motivation by giving players a sense of mastery, competence, enjoyment, immersion, and flow, and helps in converting otherwise mundane tasks into engaging and exciting experiences (Koivisto & Hamari, 2019).

Besides being applied as interventions, video games have also already been used in different fields of experimental psychological research. Cognitive psychologists have used video games as a replacement for pre-existing cognitive tasks. Cognitive tasks, such as the flanker or stop-signal task are often experienced as repetitive, boring, and effortful leading to underperformance in participants (DeRight & Jorgensen, 2015). By adding game elements such as scoring systems, graphical interfaces, and narratives to the task, participants are stimulated to be more motivated and engaged during cognitive experiments. Results have shown that participants are more motivated and more favorable towards gamified variants of tasks, also referred to as cognitive games. However, a substantial difference in performance seems absent and rather show similar results to their non-gamified counterparts (Lumsden et al., 2016; Friehs et al., 2020). This is not particularly a bad thing and has established cognitive games as a valid and reliable alternative to traditional tasks (Wiley et al., 2021).

There are many design decisions that go into the development of a video game. The effectiveness of game affordances in increasing a player's experience differs from element to element. Game features such as visuals, point systems, and narratives appear to have a limited effect on experience. Markovitch et al. (2024) argue that a player's sense of control is an essential factor in enhancing a player's experience. In their study they did this by combining multiple stimuli in a single task and giving participants the opportunity to correct their mistakes. Results showed that the cognitive game did indeed elicit a greater sense of control, resulting in experiential benefits for participants while still maintaining reliable measures as a cognitive task. Another study, by Birk et al. (2016), similarly found that by implementing character customization in a game, players were more likely to identify with the playable character. They found that greater identification with the character showed increased experiential benefits and elevated levels of sense of control (autonomy). Additionally, greater identification showed higher levels of intrinsic motivation—resulting in more time spent in-game by participants. Moreso, a subsequent study found that implementing character customization even provides the added benefit of decreased attrition rates (Birk & Mandryk, 2018). Evidently, it becomes clear that the gamification of cognitive tasks can greatly improve intrinsic motivation, adding benefits to both participants and the quality of the data.

Assessing the experience of players on (cognitive) games is often done through self-report surveys. A common method of measuring a player's experience of (cognitive) games is through the Player Experience Inventory (PXI; Abeele et al., 2020). The PXI measures the player's experience at two levels. Firstly, *Functional Consequences* are the "immediate, tangible consequences, experienced as a direct result of game design choices" and consist of 5 constructs: ease of control, goals and rules, challenge, progress feedback, and audiovisual appeal. Secondly, *Psychosocial Consequences* are second-order emotional experiences and also consist of 5 constructs: meaning, curiosity, mastery, immersion, and autonomy. Finally, a general enjoyment construct is measured as well. The inventory has 33 items, 3 for each construct, that are rated on a 7-point Likert scale from -3 (strongly disagree) to 3 (strongly agree). An independent subsequent study also found valid and reliable results for the PXI (Perrig et al., 2024) with the exception of internal validity for the immersion construct, which the authors note has been difficult to define and measure in player experience research in general.

A simplified version of the PXI, the miniPXI (Haider et al., 2022), was developed to assess enjoyment more quickly and efficiently in video games. Instead of 3 items per construct, the miniPXI uses 1 item per construct, reducing the total number of items to 11. The miniPXI shows satisfactory reliability and validity with the exception of some constructs such as immersion and mastery. The miniPXI has been applied in studies (e.g. Verkuyl et al., 2024) as a helpful tool in assessing player's experience. Some caution must be taken interpreting the aforementioned construct. However, for a more global overview of a game's experience, the authors of the miniPXI deem the miniPXI an adequate tool.

Besides the increased enjoyment that gamified tasks can bring, cognitive games provide other benefits too. One key advantage is that gamified experiments can maintain both high levels of control as well as ecological validity. Games can be designed in such a way that mimic real-life situations, making findings more applicable to real-world scenarios (McMahan et al., 2011). One practical example is that of Virtual Reality (VR) Exposure Treatment (VRET), in which a VR game is designed in a way that it resembles real life accurately enough to elicit and successfully treat different phobias (Freitas et al., 2021).

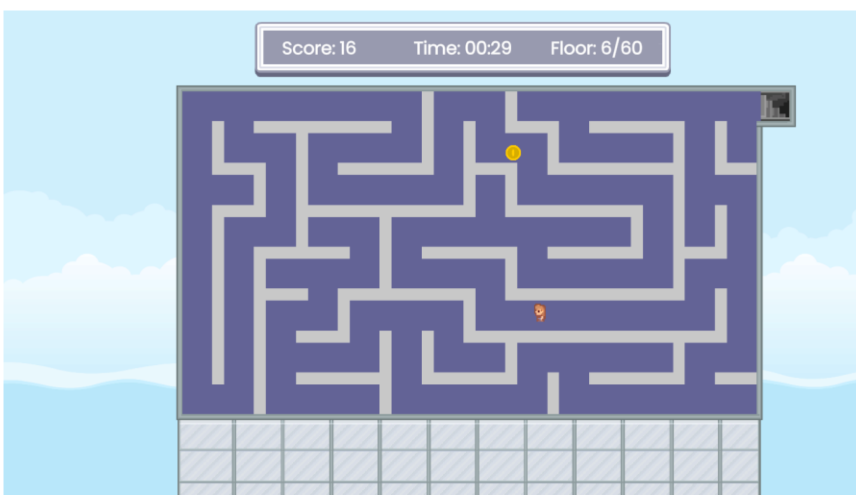
However, some limitations must be considered when using video games for experimental research too. Firstly, video games can introduce more confounding variables that cannot or hardly be controlled for (McMahan et al., 2011), for example by introducing certain gameplay mechanics or visual complexity through graphics that may not be directly related to the subject of research. Additionally, many video games provide a certain freedom in which players can make choices and also correct choices. While Markovitch et al. (2024) found that this improved players' experience, the data that was collected from corrected behaviors could not be easily compared with accurate responses. Even though this can open up possibilities for more insight into behavioral choices, the vast array of which participants can respond can introduce noise and difficulty in collecting and interpreting useful and reliable data. Simply put, introducing more complex and dynamic environments with the combination of added freedom to player's behavioral choices makes it harder to isolate outcome variables.

This Study

Taken together, current habit research paradigms struggle with either ecological validity (in experimental studies) or experimental robustness (in field studies). Additionally, traditional outcome-devaluation tasks often fail to induce habitual behavior in human participants, suggesting a need for novel paradigms that can bridge this gap. Our study aims to address this by introducing a gamified variant of the outcome-devaluation task that leverages experimental control with increased experiential benefits and ecological validity. A screenshot of the game we developed can be seen in Figure 1. In the game, participants were tasked with traversing a 2D maze towards the finish to earn points before their time ran out. Just like an outcome-devaluation task, the game facilitated the development of a cue-outcome association by placing a reward (a coin earning an extra point) in the maze during training rounds. After several rounds of picking up the coin and strengthening the cue-outcome association—the cue was devalued through instruction. In subsequent test rounds, participants were faced with two choices when the coin spawned: revert to the previously learned behavior of collecting the coin (i.e., a habitual action) or ignore the coin and proceed directly to the exit (i.e., a goal-directed action). During training rounds, the optimal strategy was to head back for the coin and get an additional point. For test rounds this would have been to ignore the coin as it does not reward any extra points. Depending on the amount of training rounds participants played (moderate or extensive)—we expected subtle differences in behavior when needing to switch to a goal-directed strategy during the test rounds. The experiment was conducted over a period of three days. On each day participants were trained to from cue-outcome associations with the coin. The devaluation on the first day served as the moderate training condition, and the third day as the extensive condition.

Figure 1

Screenshot of the game



We expected to detect these differences in habit strength through both behavioral data collected by the game, as well as self-reported measures through SRBAI surveys. First, we hypothesized that the number of action slips would be more common for the extensive training condition where habit strength would be stronger. An action slip, the execution of a habitual behavior when a goal-direct behavior was preferred, in our case meant that a player would return for a devalued coin during a test round. Based on this, we proposed our first hypothesis as follows:

H_1: The number of action slips is greater for the extensive training condition than for the moderate training condition.

Second, similar to the findings of Luque et al. (2020), we expected that more subtle manifestations of habit could be uncovered by observing differences in RT switch costs. We therefore expected that RT switch costs would be higher on day 3 (extensive training) compared to day 1 (moderate training) and proposed the second hypothesis:

H_2: RT switch costs are higher for the extensive training condition than for the moderate training condition.

Finally, besides objective differences in behavior between the extensive and moderate condition, we expected that participants would be aware of differences in their behavior between conditions too. Therefore, we expected that self-reported habit measure scores would be higher after the extensive condition than the moderate one and propose the third hypothesis:

H_3: Self-reported habit strength is greater for the extensive training condition than for the moderate training condition.

In addition to answering these hypotheses, we collected insights on the enjoyment of the game to inform us how well the experiment functions as a cognitive game and to inform us of potential design improvements for future studies.

Method

Experimental Design & Procedure

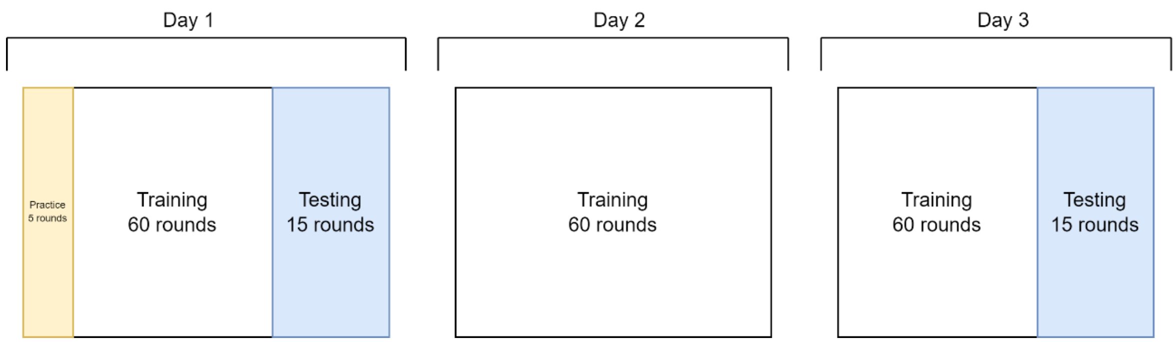
Our experimental design was modelled after the study by Luque et al. (2020) on RT switch costs. Similarly, the experiment followed a within-subject design that was spread out over 3 days. We expected that habits would grow stronger the more their associated behavior and outcome had been performed. Therefore, just like other outcome-devaluation tasks, we exploited this feature by having two conditions that were different in the number of cue-outcome associations that formed: a 1) moderate and an 2) extensive training group. Habit strength was then later assessed by devaluing outcome and observing participants' behavior. If participants were to behave in a habitual way corresponding to the cue-outcome association, we could conclude that the game successfully induced habit. Differences in habitual tendencies between both conditions would further establish that overtraining leads to an increase in habit strength—further positing the video game paradigm as a viable research paradigm for habit.

In the game, players were tasked with finding and reaching the end of a maze before their time ran out over the course of multiple rounds. Completing the maze in time would reward them with two points, not reaching the finish in time would reward them no points. Additionally, coins would spawn in the maze that could earn them one additional point as well making it possible to earn three points per round. These coins served as the cue to associate with the outcome of the additional point. After initial training rounds, the value of the coin was devalued through an instruction. This was then followed by test rounds in which players were again tasked with reaching the finish, only now coins would appear, but not reward any points when picked up. The behavior of players during test rounds was then used to assess habitual tendencies. Participants were instructed that the goal of the video game was to earn a high as possible number of points through playing the game by reaching the finish of each maze and picking up coins in the training phases.

Each day started with 60 training rounds intended to establish the cue-outcome association with the coin. On the first and third day, after the 60 training rounds, the devaluation of the coin was instructed to the player through a pop-up. This was then followed by 15 test rounds with the devalued coin. From the data collected during these rounds, the habitual tendencies of the participants were compared from the first to the third day with the expectation that habitual tendencies would be stronger on the third day. The reason for this being that the number of training rounds a participant underwent would be significantly larger on day 3 compared to day 1 (180 vs. 60 rounds), thus establishing a stronger cue-outcome associations. Due to budgetary limits and time constraints, the amount of training and testing rounds had been limited to 60 and 15 each day (with the exception of day 2 which had no test rounds), which is fewer than the number of training rounds in Luque et al.'s original experiment. Additionally, on the first day of the experiment, participants were given 5 rounds of practice in which they could freely try out and familiarize themselves with the game's controls. An overview of how the rounds were distributed over each day can be found in Figure 2.

Figure 2

Distribution of rounds



Note. At the test round of the first day, the participant would only have had a moderate amount of training of 60 rounds. By the testing phase of day 3, this would have been 180 rounds at which a stronger habit strength was expected.

The experiment was administered through an online web-application that was accessible to participants from their home. This choice was mostly based on saving time (there was no need to reserve lab spaces and have someone present) and to reduce attrition rates and increase participation, as coming to the lab 3 days in a row might prove too effortful. Especially considering the relatively moderate compensation for the effort of participation. Additionally, this gave the freedom to the participants to participate during a timeslot that worked for them, as well as the ability to independently choose their starting date. Notwithstanding that participants were explicitly instructed to partake in the experiment for 3 consecutive days at the same time each day to reduce the time effects on forgetting. A downside to this freedom is that it comes at a cost of control of the environment in which participants perform the experiment. However, a case can be made for the fact that habits also do not develop in sterile lab-like environments in real life either.

Despite this, there is no way for us to determine how noisy, busy, distracting, etc. the environment of each participant was.

Once a participant registered for the experiment through the online form, an email was manually sent by the researcher that included instructions on the experiment. The email contained 3 separate URLs that pointed to the 3 different days of the experiment. The email and the website both provided instructions to the participant. They were informed that they needed to be on a computer or laptop with keyboard input and a stable internet connection to participate. In addition, participants were asked to participate at approximately the same time each day, that their participation for the day was registered at the end of the experiment each day, that their participation payment would be made at the end of the experiment, and that they could contact the researcher in case of any further questions. Once ready, the participant clicked the *Start Experiment* button to begin. A screenshot of this page can be seen in Appendix A1. At the end of each day participants were thanked for their contribution and reminded to return for the next day. Once all participants finished all their sessions, scores were evaluated which revealed 6 players reaching the maximum score. Instead of only giving the top 5 participants the €5 bonus reward, the top 6 were given the bonus. Additionally, participants were paid according to their contributions, €6 for 1 day, €12 for 2 days and €20 for 3 days. At the time of writing, the experiment can still be accessed on https://htionline.tue.nl/1af8d72c/. Do note that data is no longer persisted, therefore some functionality, such as scoring and character selection, do not work as intended anymore.

Sample Size Justification

The sample size was chosen on the basis of practical constraints and other comparable studies. Due to limited time and budget constraints the sample size had been set at $N = 30$. Considering it has been hard to elicit habitual responses in humans (De Wit et al., 2018), suggesting a small effect size, this number may seem low. However, the study by Luque et al.

(2020) on RT switch cost were able to find significant results for a medium effect size with a sample size of 24 participants for their within-subject group. However, one caveat is that their number of training rounds were significantly larger than those used in our experiment. Participants partook in 9 blocks each day for 3 consecutive days where each block consisted of 48 training trials, leading to a total of 432 training trials each day compared to 60 per day in this experiment. Presumably, the higher amount of training trials makes it easier to detect an effect considering more training would increase habit strength.

An additional 30% had been added to the sample size because of expected attrition rates due to the experiment taking place over a period of 3 days which might render the experiment too effortful for some. Therefore, the goal for recruitment had been set at a total of 39 participants.

Participants

Participants were recruited using the PPDB participant database of the TU/e and through the personal network of the researcher. Participants were asked to sign up and give consent using an online form. In the form, participants were asked for their email, age, gender, and banking details for their participation reward. In addition to this, participants were explicitly asked if they had access to a computer or laptop with keyboard input and whether they were able to participate for 3 consecutive days. The recruitment form was framed in such a way that it did not reveal any demand characteristics. Instead of talking about habit, the instructions detailed the subject as motivation in video games. After registering, participants were sent an email that included instructions and three links to the experiment—one for each day.

Participants were paid €20 for their participation for all 3 days. In addition, to ensure that players did their best and were motivated to pick up the coin during training rounds, an additional €5 was promised to those who would manage to enter the top 5 with the highest scores. Therefore, a participant could potentially earn a maximum of €25. If a participant decided to drop out before finishing all 3 days their data would not be of any value, however, they would still be paid for their efforts. As such, for each day the participant participated they would earn €6.

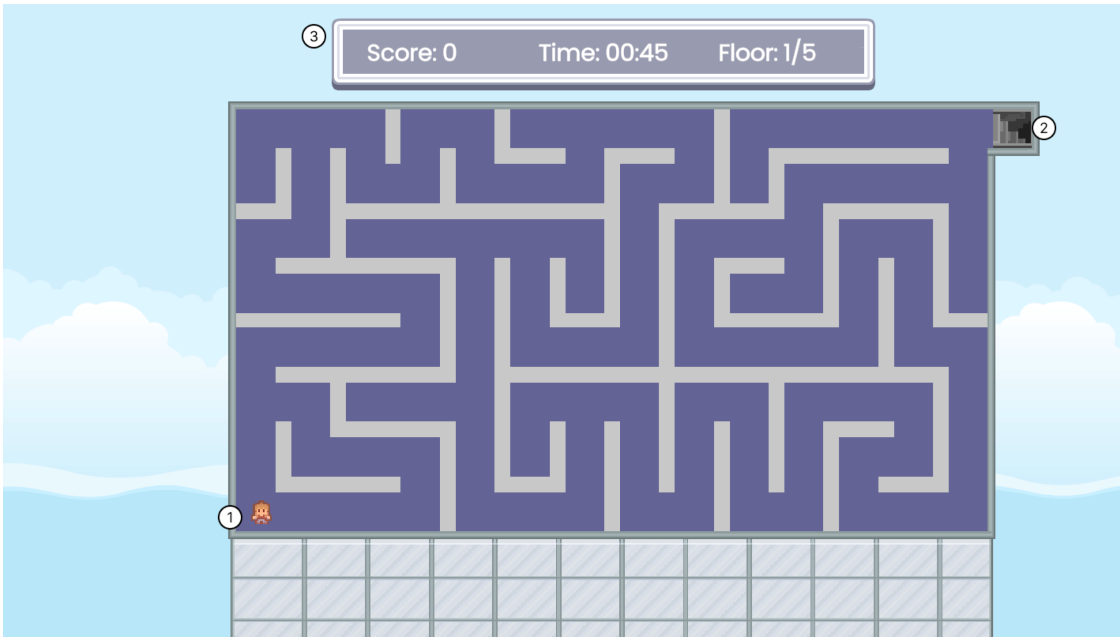
A total of 43 people registered for the experiment through the online form. However, 6 people did not respond to the email to partake in the experiment. Another 3 dropped out during the experiment of which 1 person dropped out due to their game crashing two times in a row, 1 dropped out because they thought the game was too hard and took them too much time, and the third person did not complete the third day for unknown reasons. Because these participants did not finish the experiment they were excluded from the sample. However, they have been paid for their efforts accordingly. This leaves the total sample size of participants who finished all 3 days to 34 people (N = 34). The mean age of the participants was 32.9 years (SD = 16.0), ranging from 20 to 78 years old of which 56% identified as female (n = 19) and 44% as male (n = 15).

Game Design

The game was developed using the Unity game engine 6000.0.26f1 (Unity Technologies, 2024) and presented participants with a 2D top-down perspective of a maze. A screenshot of the game during a practice round can be seen in Figure 3. Each round started with the player positioned at the bottom-left corner of the maze, with the objective of navigating their way to the exit in the top-right corner by using the arrow or WASD keys on their keyboard. The layout of each round was generated based on a Unity implementation of the depth-first search algorithm by Akthar (2024), which uses random values to generate different layouts. The game is supplied with seeds from 1 to 100 to generate specific layouts. For each player the order of these seeds, and thus layouts, was randomly determined to control for any order effects. At the top of the screen, a Graphical User Interface (GUI) displayed feedback about their progress on the game: current score, remaining time, which round they were on, and how many rounds were left. Graphics were provided by the royalty-free game asset creator Kenney (2024).

Figure 3

Annotated screenshot of the game during the practice phase

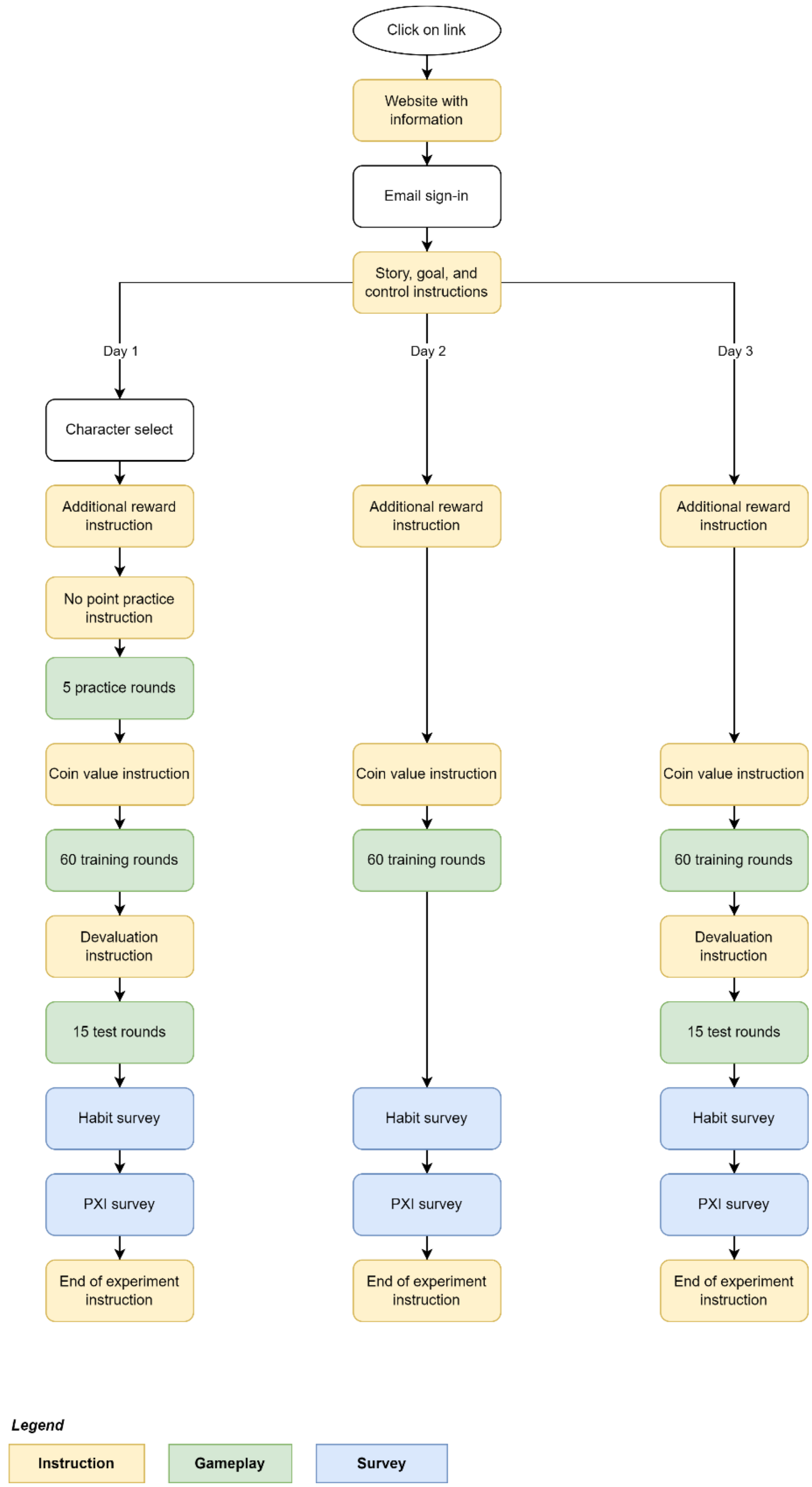


Note. 1) Starting position; 2) Maze exit; 3) GUI showing score, remaining time, and current round (floor).

Because each day differed slightly in structure and goals, the game presented slightly different instructions and phases per day. An overview of the game's flow and structure for all 3 days can be found in Figure 4. At the start of each session, players entered their email address, which was saved to a database to synchronize their data across days (Appendix A2). The game opened with a title screen that displayed the name of the game, *Office Escape,* (Appendix A3) and a short narrative introducing the story, controls, and goal of the game (Appendix A4). From this point on, instructions and phases varied slightly between days.

Figure 4

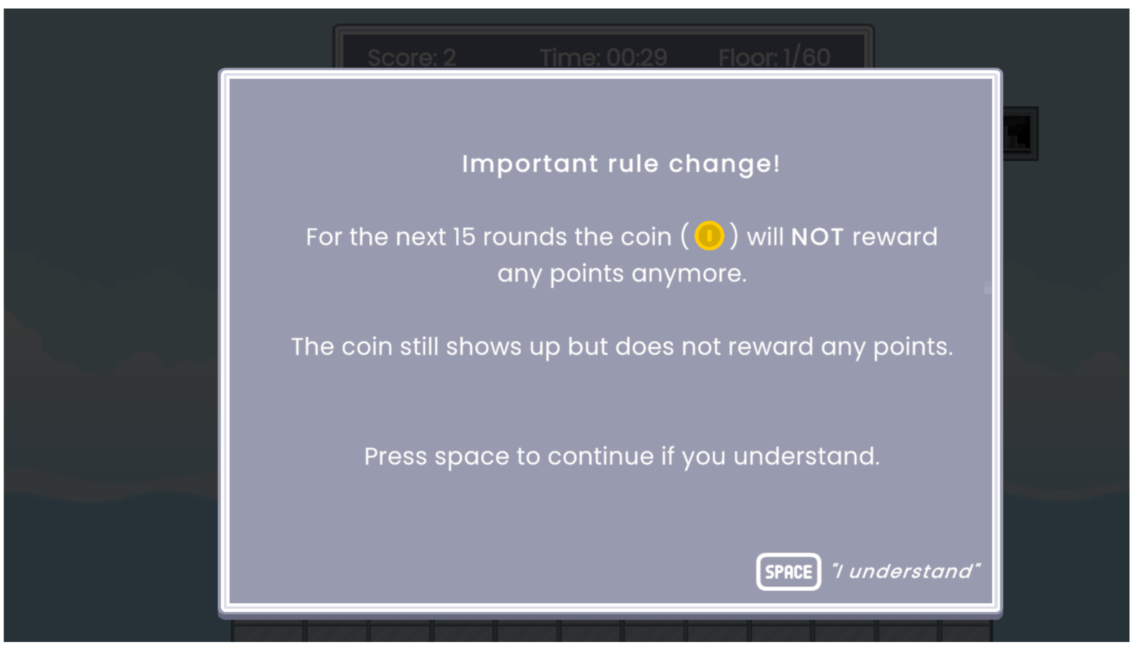
Flowchart of the different instructions and gameplay the participant was presented with each day



On the first day only, players selected a character which was stored and loaded on subsequent days (Appendix A5). Following the character selection, a reminder that the highest performing scores earn an additional €5 was presented to motivate players to do their best. This reminder was repeated at the start of all following days. Next, players participated in 5 practice rounds to familiarize themselves with the controls and objective of the game. During these rounds coins did not yet spawn, and scores were also not counted yet. Once completed, they were instructed the rules of the game changed and points were now being counted, as well as that coins would start spawning in the maze. This instruction was shown for at least 5 seconds before the player could continue to ensure they read it. Once read, the player participated in 60 training rounds. After the 60 training rounds, the devaluation instruction was presented (Figure 5), which informed the player that the coin still appeared but did not reward any points when picked up anymore. Again, this instruction was shown for at least 5 seconds.

Figure 5

Screenshot of devaluation instruction



Players then proceeded to play 15 test rounds, during which coins still spawned but did not reward any additional points when picked up. Following the test phase, the player was presented with surveys of the SRBAI and miniPXI (Appendix B). Once filled in and submitted, game data and survey data were uploaded to the database. The participant was thanked for their participation and reminded to return tomorrow for the next day of the experiment. Finally, they were instructed they could safely close the window.

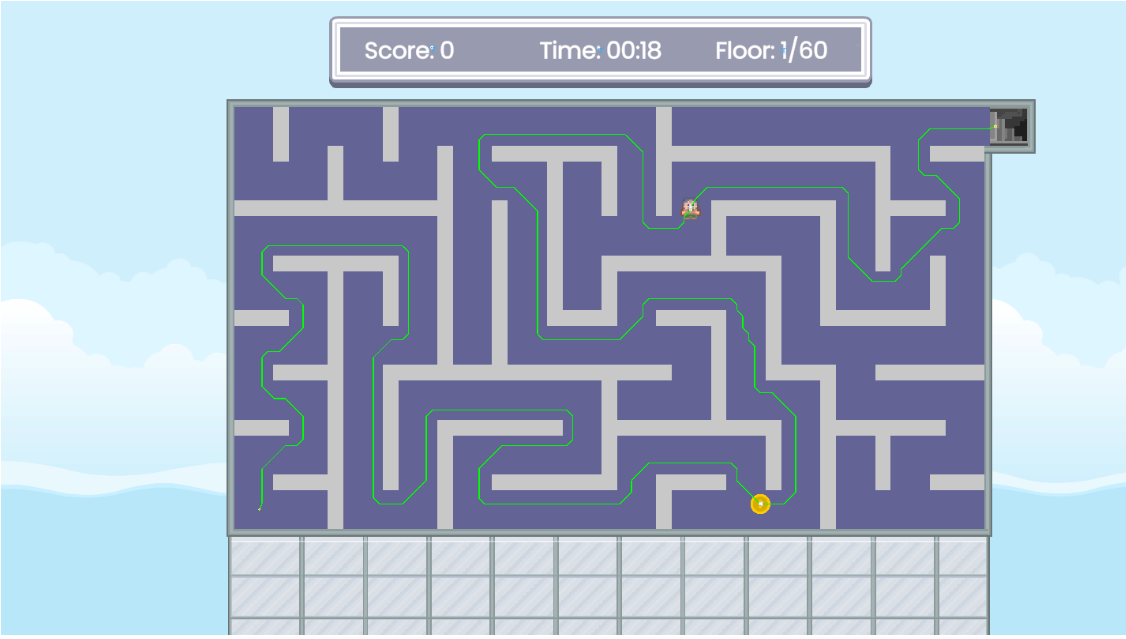
On Day 2, participants were informed that the coin once again held value and awarded one point. This day included 60 training rounds only, no test rounds, to induce habit for the extensive condition on day 3. After training, participants completed the same post-session surveys. Finally, on day 3, participants played another 60 training rounds. These were then followed by 15 test rounds with devalued coins used as the extensive training condition. Participants then completed another SRBAI and miniPXI survey and were finished with the experiment.

Each round had a time limit of 45 seconds in which the participant could reach the finish before moving on to the next round. This time limit was added to add additional pressure to the player. As Luque et al. (2020) found in their study, time pressure was important in detecting habitual behavior noting that participants prefer habitual behavior over goal-directed behavior when faced with a behavioral choice under time pressure. Careful thought was put into designing the mazes in such a way that it was possible to both pick up the coin and finish the maze in a timely manner with leaving a small margin of error. The spawn behavior of the coin was based on the position of the player in relation to the finish and starting position. A Unity implementation of the A* Search Algorithm by Granberg (2021) was used to calculate the distance of the player towards the start and finish of the maze as can be seen in Figure 6. The coin spawn distance was determined as follows: at the start of the round, the total distance from the start until the finish is calculated.

Simultaneously, a random percentage of the total distance between 65% and 85% was chosen. Once the player started traversing the maze and reached the pre-calculated spawn distance, the coin appeared at around 40% of the path towards the start of the round. This means that the coin always spawned in such a position that the participant needed to move away from the finish back to the start. This spawning behavior left enough time for the player to head back and pick up the coin and finish the rounds while still adding a certain amount of pressure.

Figure 6

Visualization of path from player to start of the maze to the finish



Note. The green line representing the path calculations was not visible to participants.

Other design decisions were largely based on constructs of the PXI. Players assumed the role of an office worker trying to escape a skyscraper after everyone else had left. The visual sequence opened with a bustling city scene before transitioning to the building interior, presenting the maze. An overview of different design decisions and their corresponding PXI constructs can be seen in Table 1.

Table 1

Overview of design decisions based on PXI constructs

Construct	Design Decision
Audiovisual Appeal	Pixel-art graphics of city and office scenes; no audio due to lack of control over output
Challenge	Tight time limit; coin spawn behavior tuned for retrieval and completion with minimal margin
Ease of Control	Intuitive maze navigation using arrow/WASD keys; brief practice rounds
Clarity of Goals	Clear objective: reach the exit, collect coins, earn points; explicit textual instructions
Progress Feedback	GUI displayed score, remaining time, and round count
Autonomy	Character selection on Day 1; consistent narrative and gameplay loop; however, freedom in choices is limited

Measurements

While participants partook in the experiment, the game collected various in-game data. Generic data about the participant such as their email address, character selection, and total scores were recorded. In addition, every round various data was collected from the participant too. Round data included on which day the round took place, during which phase (training or test), the total length of the maze from start to finish, the timestamp the coin appeared, whether they picked up the coin, whether they finished, and the time it took them to finish. Additionally, every 0.02 seconds, the game logged the timestamp and the distance of the player towards the finish using the A* Search Algorithm path. These consecutive logs helped track the movement of the player throughout the round. An example graph of the movement of a player during a training round can be seen in Figure 7. In the example, the participant can be seen moving towards the finish up until approximately 6 seconds where they return for the coin. At approximately 8 seconds, the participant continues their trajectory towards the finish again. A section of its corresponding data points, as logged by the game, can be seen in Table 2.

Figure 7

Example of player movement visualized in a line graph

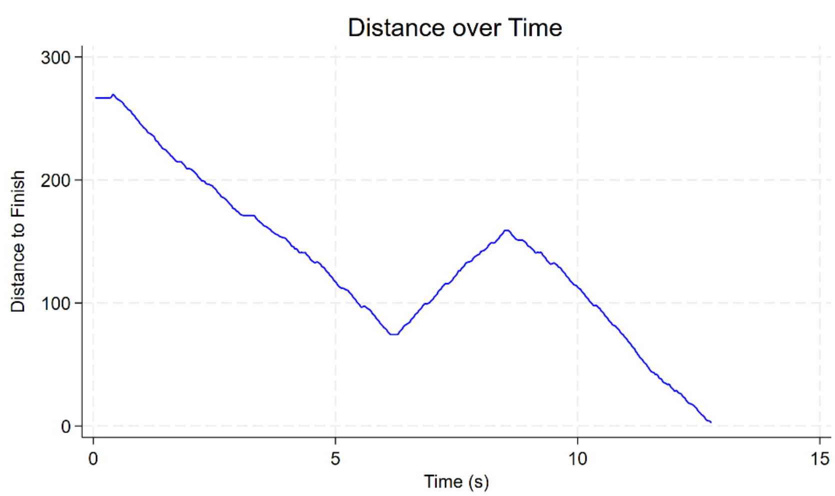


Table 2

Example of log data collected by the game

Time (s)	...	4.00	4.02	4.04	4.06	4.08	4.10	...
Distance to finish	...	151.04	150.24	149.45	148.65	147.06	146.18	...

Note. Some logs have been omitted (...) for brevity.

Additionally, at the end of each day, participants were administered both the SRBAI and miniPXI surveys. Items were selected using a Likert scale and submitted to finish the respective day of the experiment. The SRBAI and miniPXI were administered each day at the end of the experiment. The SRBAI is a 4 item that are answered on a 7-point Likert scale ranging from 1 (strongly disagree) to 7 (strongly agree). A screenshot of the survey as seen in-game can be seen in Appendix B1. The miniPXI was administered after the SRBAI survey and consists of 11 items that are rated on a 7-point Likert scale as well but coded from -3 (strongly disagree) to 3 (strongly agree). A screenshot of the miniPXI and its items can be found in Appendix B2.

Action Slips

The data collected during rounds was used to calculate various dependent variables. To start, we wanted to measure the frequency of action slips. To determine whether an action slip occurred we specified the following criteria; firstly, an action slip can only occur during test rounds and can only occur 1 time per round. So, per test round an action slip either did or did not occur. Secondly, an action slip can only occur after the coin has spawned—the choice to return for the coin could not have been made if the coin was not present. Finally, we looked at the movement logs of the round to determine whether participants did indeed move back for the coin after it spawned. The movement logs describe the distance of the player towards the finish at a given moment in consecutive order. In an ideal situation, this distance would consistently decrease during test rounds as participants continue to the finish uninterrupted, ignoring the cue. In the case of an action slip, it would thus mean that the distance towards the finish increased again, meaning the player moves away from the finish towards the coin. Therefore, we determined an action slip occurred if there was a timestamp after the coin spawn in which the distance towards the finish increased rather than decreased during a test round. However, we quickly found that this definition was too sensitive as it gave an abnormally large number of action slips over the rounds. This was likely due to noise being present in the movement of the player thus mistakenly registering as going back for the coin far too often. Because of this, we slightly altered the threshold of when to register the participant as returning for the coin. Instead of looking at the first increasing timestamp, we looked at the first increasing timestamp that kept consecutively increasing over at least 8 timestamps (0.16 seconds). An example of what this would look like in the log data can be seen in Table 3.

Table 3

Example of logging data when turning back

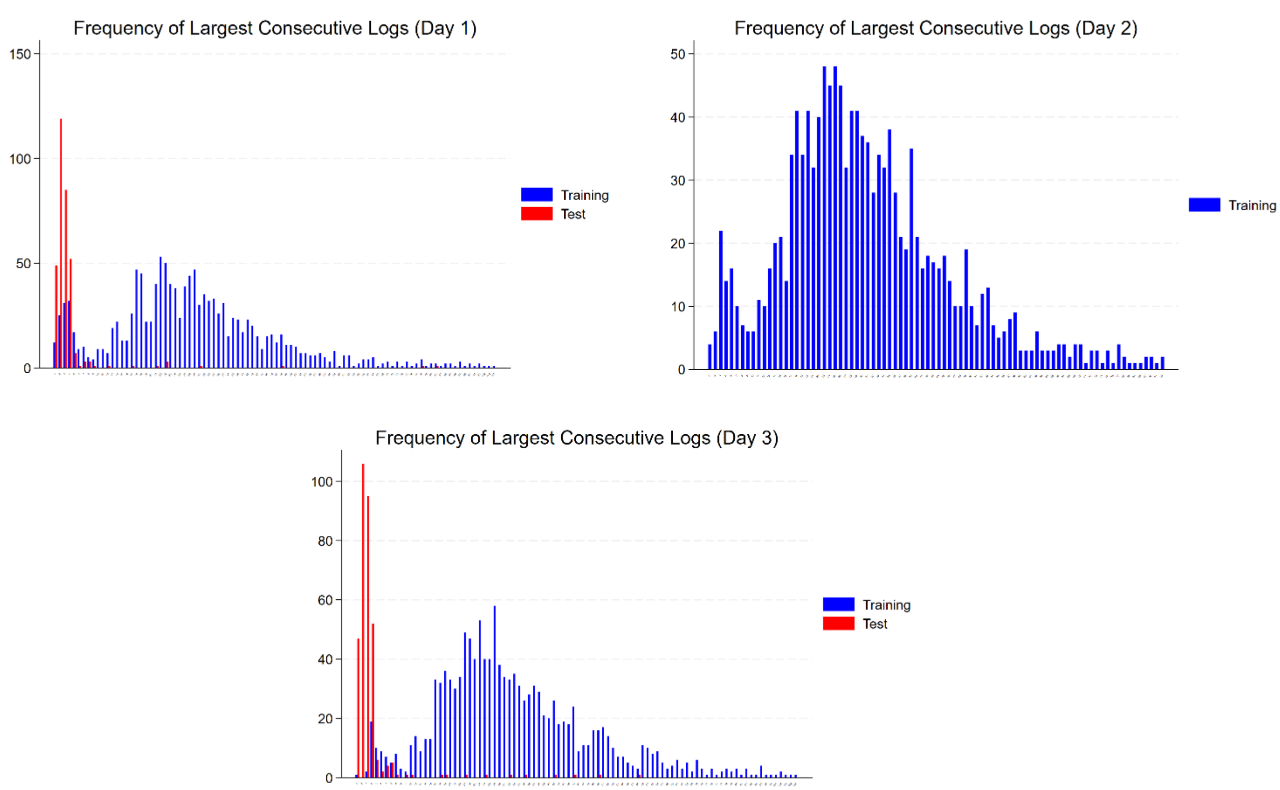
Time (s)	...	6.98	7.00	...	7.16	7.18	7.20	7.22	7.24	7.26	7.28	...
Distance to finish	...	36.0	34.4	...	24.0	23.2	24.0	25.6	27.2	28.0	29.6	...

Note. The coin appears at the green cell, 6.98 seconds into the round. The player starts moving toward the coin at the blue cell, 7.18 seconds into the round, as is exemplified by the increasing distance to the finish. They do this for at least 8 consecutive timestamps, thus registering as an action slip. Some logs have been omitted (...) for brevity.

We arrived at a threshold of 8 by looking at the frequency of the largest streak of consecutive logs of movement toward the coin across all rounds. Figure 8 shows these frequencies for each day with the X-axes representing the length of the streak of consecutive logs and the Y-axes the frequency at which they occur. The graph reveals a bimodal distribution for the training phases on each day. In contrast, the test phases only show a peak for shorter streaks of consecutive logs. From this, we derived that the shorter streaks were most likely due to noise. Therefore, we estimated that the ideal threshold was in between the two modes, giving us a threshold of approximately 8 timestamps, or 0.16 seconds, of consecutive increasing distance before being registered as an action slip.

Figure 8

Largest consecutive streak of data logs for each day

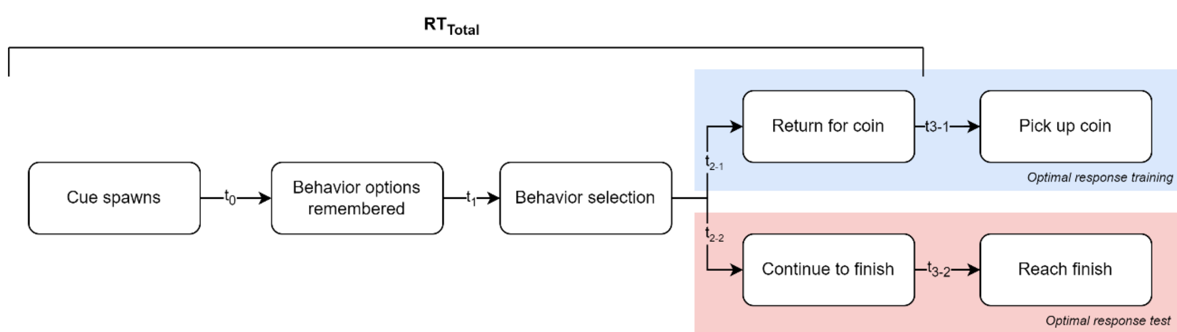


RT Switch Costs

Besides identifying action slips, behavioral data from the game was also used to calculate RT switch costs. Again, RT switch costs are the differences in reaction times when successfully switching from habitual to goal-directed behavior. To better illustrate the cognitive steps when making a behavioral decision we introduce the Behavioral-Choice Model (BCM; Figure 9). The model is based on previously discussed studies such as Tobias' model on behavior option generation and selection.

Figure 9

Behavioral-Choice Model



The model describes 5 discrete steps from identifying the cue up until selecting and executing the behavior. Firstly, the cue appears and is perceived by the participant. This is followed by two cognitive processes, the remembering of potential behavior options, and the selection of an option. This option is either habitual or goal-directed depending on habit strength and situational factors (e.g. time pressure). In the model, the time to remember the behavioral options is t_0. The time to select a behavior is t_1. Finally, executing the action is t_{2-1} and t_{2-2} for returning to the coin and continuing to the finish respectively. The time to finish their action is marked by t_{3-1} and t_{3-2}.

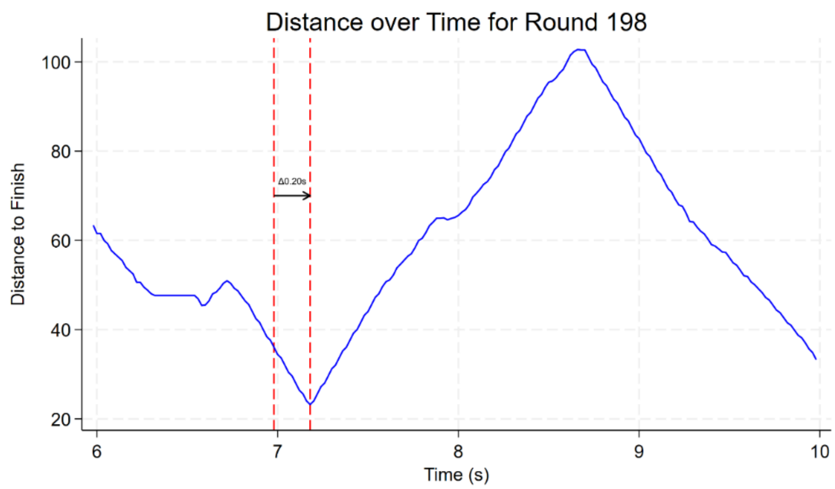
The total time from the cue spawn until the start of the action execution is RT_{total}. Optimal responses are different for each phase, but both prioritize a as high as possible score and as quick as possible time to finish a round. For training rounds this means returning for the coin when it spawns (blue). For test rounds this means ignoring the coin and moving towards the finish (red).

Once we established a cue-outcome association during training rounds, we expected participants to experience a conflict between the habitual and goal-directed system during test rounds. We suspected that this would manifest in participants having to more consciously remember their option not to return (i.e., continue) as well as making the goal-directed choice to continue. This would mean that both the remembering of behavior options (t_0) as well as the option selection of continuing (t_1) would be slower. However, ultimately there is no way of extracting these times from the collected data. Despite this, slow down in either or both of these times would still manifest in a higher RT_{total}, which is indeed measurable.

Only reaction times of optimal responses were used considering that participants' suboptimal response, e.g. returning for the coin in a devalued test round, did not successfully override their habitual inclinations. Determining the reaction time (RT_{total}) was done with the collected timestamps and distance data and was calculated slightly differently for each optimal response. Firstly, for training rounds, the reaction time was calculated as the time between the start of the coin spawning until the first movement of a streak of 8 consecutive timestamps (0.16 seconds) towards it. An example graph of such a situation can be seen in Figure 10. In this example we see that the coin spawned 6.98 seconds into the round. The player continues towards the finish as can be seen from the decreasing distance. At 7.18 the distance towards the finish increases, an indication that the player moved in the opposite direction and is now heading towards the coin. The time between the start of the 8-log streak and the spawn of the coin is the reaction time. For this example, that would be 7.18 - 6.98 = 0.20 seconds. The peak in the graph represents the moment the participant picked up the coin and resumed moving towards the finish.

Figure 10

Example of a training round in which the participant moves away from the finish to pick up the coin

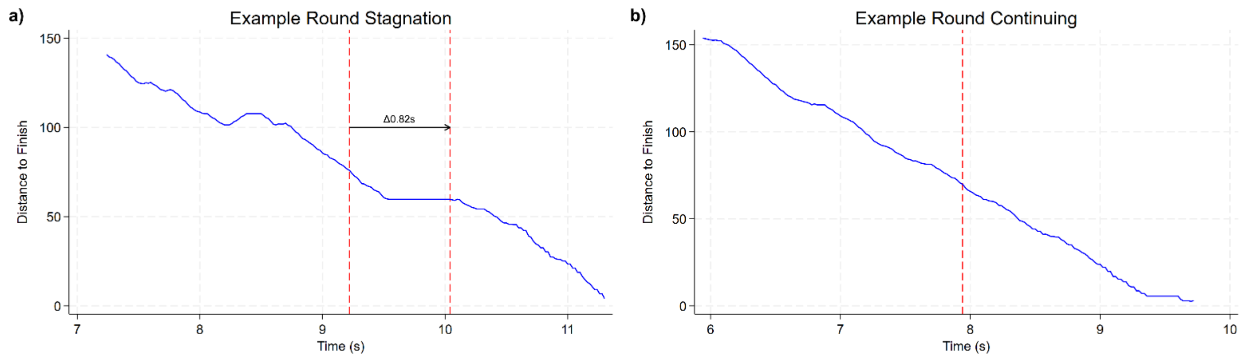


Note. The first dotted line in red is the time the coin spawned. The second dotted red line represents the start of when the participant starts heading towards the coin. The peak is the moment the participant picks up the coin and continues towards the finish.

Secondly, the optimal response during the test rounds means not returning for the coin but continuing to the finish. We found that there were two ways in which this behavior showed up in the data; first, the participants movement temporarily stops before continuing again. The reaction time is then the time from when the participant started moving again minus the spawn time of the coin (see Figure 11*a* for an example). Again, a threshold of 8 consecutive timestamps had been applied to avoid accidentally measuring noise. This meant that the participant needed to stay stagnant for at least 0.16 seconds to be registered.

Figure 11

Movement logs during test rounds



Note. a) An example of a test round where the participant stopped moving after the coin spawned before continuing to move again. The first red line represents the time the coin spawned. The second red line represents the end of when the participant started moving again. The reaction time of this particular example is 0.82 seconds. b) An example of a test round in which the participant continues towards the finish without stopping. The dotted line in red is the time the coin spawned.

A second way an optimal response could occur during test rounds was by a participant not stopping but making the behavioral decision while continuing to move (see Figure 11*b* for an example). The latter of these two behaviors cannot be used to determine a reaction time. It is possible the decision was made extremely fast or that the decision was anticipated and made before the cue appeared. Preferably, both reaction times for either behavioral choice were measured and calculated. However, it is not possible to conclude with certainty when the behavioral choice for continuing had been made as it cannot be derived from the data. For example, when the coin spawns and the player is moving towards the finish, they might continue moving forward and make the decision whilst still moving. In these cases, the behavior choice to continue was made and would show up in the data as a reaction time of 0 seconds. However, it cannot certainly be said that it was actually 0 seconds or that the decision was made later whilst still moving. Therefore, when comparing reaction times, only behavioral decisions with stagnation were considered, as it is clear from the data when a participant explicitly starts continuing towards the coin again.

Data Analysis

All data was subjected to statistical analyses using either Stata 18 (StataCorp, 2023) or R 4.3.2 (R Core Team, 2023).

Exclusion Criteria

Before any analyses took place, the data was pre-processed and checked for any anomalies. There were several exclusion criteria we applied to the participants. Firstly, participants who did not finish all 3 days in consecutive order were excluded. Secondly, participants who failed to participate in the experiment as intended were excluded too. This either meant a participant did not pick up enough coins during the training rounds, thus not inducing any habit, or the participant picked up too many coins during test rounds, thus not understanding the devalued outcome of the coin. As such, participants who missed more than 50 coins during training rounds were excluded, as well as those who picked up more than 5 coins during test rounds on either day.

Descriptive Statistics

First, we performed descriptive analyses to get a general overview of the participants' performance of the game. We looked at how many coins were picked up in both the training and test rounds as well as their ability to finish the rounds on time and how long it took them. Additionally, we used the total scores of participants as a general performance indicator. Besides their total score, we also calculated their average speed across rounds. Mazes differ in size, some have shorter routes to the finish, whereas others are longer. Therefore, the average speed was used by dividing the total distance the participant traversed by the time it took them to finish. A regression analysis was run to test whether speed, and thus performance, changed with gaining experience over the game.

Action Slips

To test for H_1, the expectation that the number of action slips is larger for the extensive training condition, we aggregated the number of action slips per participant and performed a repeated measures two-tailed t-test between the moderate and extensive training condition (day 1 vs day 3). However, when checking assumptions, a Shapiro-Wilk test revealed that the data violated the normality assumption. As such, a non-parametric Wilcoxon signed-rank test was performed instead.

RT Switch Costs

Besides action slips, we hypothesized that RT switch costs would be higher for the extensive training condition than the moderate (H_2). To test this, a linear mixed-effects model (LMM) of reaction time was fitted using a restricted maximum likelihood estimation (REML). The model included phase (training or test), day (1 or 3), and their interaction as fixed effects, with a random intercept for each participant to account for repeated measures. Advantages of using a mixed-effects model is that they are more robust towards unbalanced data compared to a repeated measures analysis of variance (ANOVA) tests or t-tests. Additionally, LMMs account for within-group correlation, making them ideal for longitudinal data and repeated measures (Gałecki & Burzykowski, 2013). A main effect of phase would mean that switching from training to test rounds significantly impacts reaction times—thus showing evidence for a RT switch cost. More importantly, a significant interaction effect between phase and day, where day represents the amount of training, would mean that the difference in differences between phases is significantly different for the moderate and extensive condition. An increase in these differences would mean that RT switch costs are thus higher for the extensive training condition compared to the moderate one.

SRBAI Scores

Finally, the SRBAI scores were aggregated to find a habit strength score for each day. A repeated measures ANOVA was run to test for individual differences and a main effect of day (amount of training). Furthermore, a Bonferroni post-hoc analysis was conducted to identify differences in SRBAI scores between specific days. In addition to this, we tested for a correlation between the SRBAI scores and the RT switch costs of participants per day. Results could show whether differences in RT switch costs were also correlated to higher habit strengths through higher SRBAI scores.

Results

Data Pre-Processing

Each player participated in a total of 215 rounds. That is, they each performed in 5 practice rounds, 180 training rounds, and 30 testing rounds over the course of 3 days. However, a few had more or less than the total 215 rounds. 2 participants only registered 214 rounds and were thus missing 1 round each. After further investigation, presumably due to a weak internet connection or another technical error, the two rounds were unable to upload correctly to the database. However, due to them both being in the training phase and it only being one round per participant, it can be safely assumed that their missing rounds had a minimal impact on the analysis and results of the data.

Four participants had more than the maximum 215 rounds recorded. One of them participated in an extra 4th day by doing the third day two times, presumably by accident. Their extra rounds and their respective logs had been excluded, as well as their total score had been corrected for. The other 3 participants seemed to each have restarted the experiment at some point during the experiment. Two of them restarted on the first day, one after the practice and first two training rounds, and the other after the training phase. The third participant restarted on the second day in the middle of a training round. For each of these participants, the rounds that were not completed until the end had been excluded. However, it must be noted that due to them restarting, and thus participating in more rounds, their training and thus their habit strength might be slightly different than those of other participants.

Besides the obvious exclusion of the participants who did not finish the experiment, after further investigation, it appeared some participants' data could heavily skew the results of the analysis. These outliers generated data that were inconsistent with the goal of the experiment. Some participants failed or refused to pick up a majority of coins during the training phase, thus most likely, not training them correctly to correlate the cue with the behavior to go back—failing to induce of habit. Additionally, there were 8 participants who did not pick up more than 50 coins during the training phases over the course of the 3 days. In addition to this, 3 participants picked up all (15) coins during one or both of the test phases. Another participant picked up 14 coins spread over the two test phases. This left us with potentially 12 participants that did not participate in the experiment as intended. Subsequent analyses were therefore only carried out using the subset of the full sample which excluded the aforementioned participants (N = 22). The mean age of this subset was 31.4 years old (SD = 13.7) with the youngest person being 20 and the oldest 68 years of age. 14 participants identified as female (53.85%), and 12 as male (46.15%). The exclusion of data of participants who behaved unexpectedly or beside the goal of the experiment should render more robust results.

Descriptive Statistics

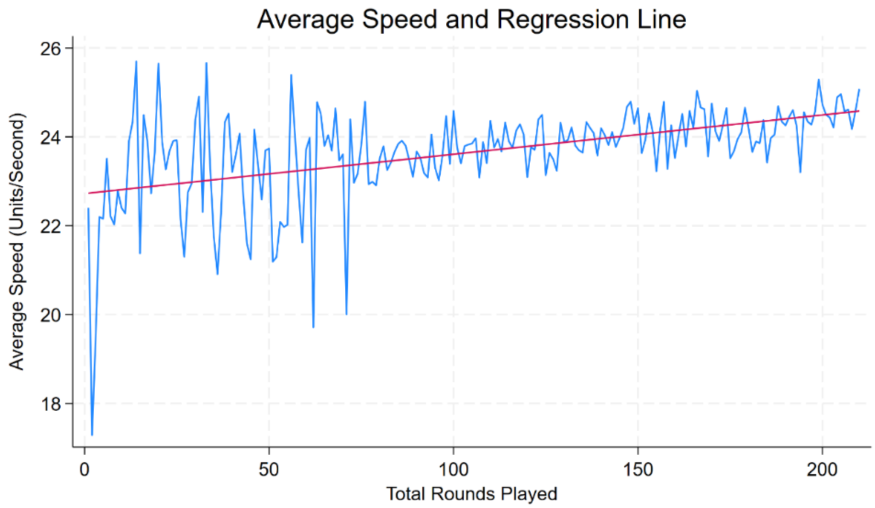
Before we discuss the main research hypotheses, we will go over some descriptive statistics to give a better overview of the experiment. 22 participants participated in a total of 4619 rounds. Over the course of all rounds, a total of 3812 coins were picked up. During the training rounds, on day 1, 89.9% of coins were picked up. For day 2 this was 98.3% and for day 3 this was 99.1% of all coins. Participants were able to reach the finish before the timer ran out most of the time—in only 0.3% of rounds participants did not manage to reach the finish before the time ran out. For the rounds that participants did finish, on average it took them 20.3 seconds (SD = 7.1). For training rounds this was on average 21.6 seconds (SD = 6.6), whereas for test rounds this was 12.5 seconds (SD = 4.6), which reveals that participants had ample extra time in finishing rounds, on average 45 - 20.3 = 24.7 seconds.

The mean total score was 591 (SD = 14.7) with the lowest score being 550 and the maximum score being the highest possible score of 600. To achieve the maximum score of 600 participants had to pick up all coins during the training rounds and finish all rounds of both the training and test rounds in time. This was achieved by a total of 6 participants. Do note that these scores are particularly high due to the prior exclusion of the poor-performing participants in the full sample.

Furthermore, another metric for performance was how quickly participants were able to finish rounds. The average speed of all participants over 3 days was 23.66 units/second (SD = 3.79) with the slowest round being 7.31 units/second and the fastest being 33.06 units/second. Performing a regression analysis revealed a main effect of total rounds (p = < .001) on the speed of participants. With a coefficient of 0.0087, players thus increased in speed approximately 0.0087 units/second per round played. An increase in speed is expected as players get more familiar with the controls and the goal of the game. In other words, over time participants' skill improved. Figure 12 shows the average speed of the participants over the total rounds they played with the regression line shown in red.

Figure 12

Average speed over total rounds played with regression line



Optimal responses for the training and test rounds differed in frequencies. Table 4 shows an overview of the number of optimal responses per day and phase. The number of optimal responses during the training phase on day 1 was 1184 (89.7%), day 2 this was 1247 (94.5%) and 1272 (96.4%) for day 3 which each had a total of 1320 training rounds. For the training phase that means that a total of 3703 rounds (93.5%) had an optimal response by the participants. Compared to 628 (95.2%) optimal responses during the test phase with 316 (95.8%) on day 1 and 312 (94.8%) on day 3 out of 330 test rounds per participant per day.

Table 4

Optimal responses for each day and phase

Day	1		2	3	
Phase	Training	Test	Training	Training	Test
Number of optimal responses	1184 (89.7%)	316 (95.8%)	1247 (94.5%)	1272 (96.4%)	312 (94.8%)

Hypothesis Testing

H_1: Action Slips

Our first hypothesis claims that the occurrence of action slips is more common in the extensive training condition compared to the moderate one. As mentioned before, action slips in the context of habit research is the failure to override habitual behavior in favor of a goal-directed behavior. In the context of this experiment this means returning towards the coin during test rounds even though it is devalued. A total of 32 action slips occurred, of which 43.8% (14) occurred on day 1, the moderate training condition, and 56.3% (18) on day 3, the extensive training condition. The mean number of action slips per participants for day 1 was 0.6 (SD = 1.1) and 0.8 (SD = 1.5) for day 3. The number of action slips across participants were, according to the Shapiro-Wilk test, neither normally distributed for day 1 ($W = 0.79$, $p < .001$) nor day 3 ($W = 0.70$, $p < .001$). Therefore, a non-parametric test was conducted in favor of a t-test. The Wilcoxon signed-rank test did not reveal significant differences between the number of action slips of participants between day 1 and day 3 ($W = 87.50$, $Z = -0.210$, $p = .83$), and a very small effect size, $r = -.04$. Therefore, the data does not support hypothesis H_1.

H_2: RT Switch Cost

Furthermore, we looked at RT switch costs as metrics for habit strength in participants. From our second hypothesis follows that RT switch costs would be greater with extensive training (day 3) compared to moderate training (day 1). An overview of the mean reaction times per day and phase can be seen in Figure 13 and Figure 14. The mean reaction time for optimal responses across both days and phases was 0.60 seconds (*SD* = 0.46). Per day, the mean was 0.57 seconds (*SD* = 0.40) for day 1 and 0.64 seconds (*SD* = 0.50) for day 3. The reaction time for optimal responses during training rounds had a mean of 0.48 seconds (*SD* = 0.32) and 0.50 seconds (*SD* = 0.31) for day 1 and 3 respectively. For test rounds the mean time for day 1 was 0.93 seconds (*SD* = 0.51) and 1.21 seconds (*SD* = 0.74) for day 3.

Figure 13

Bar chart of mean reaction time in seconds by day and phase

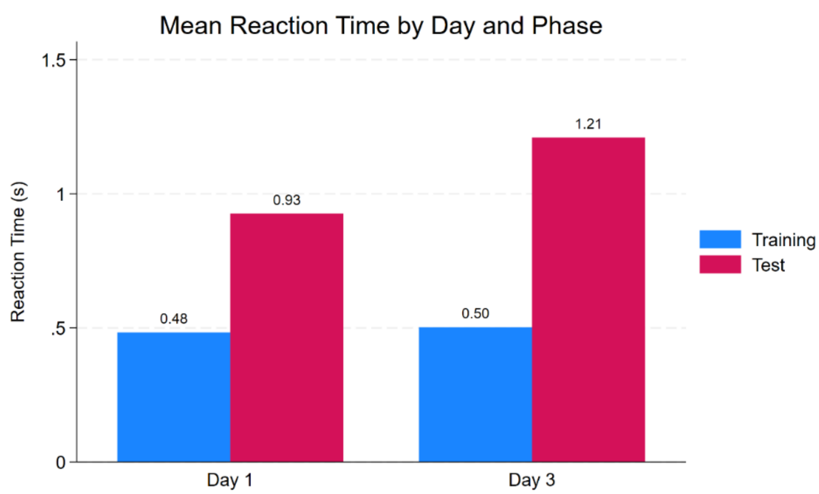
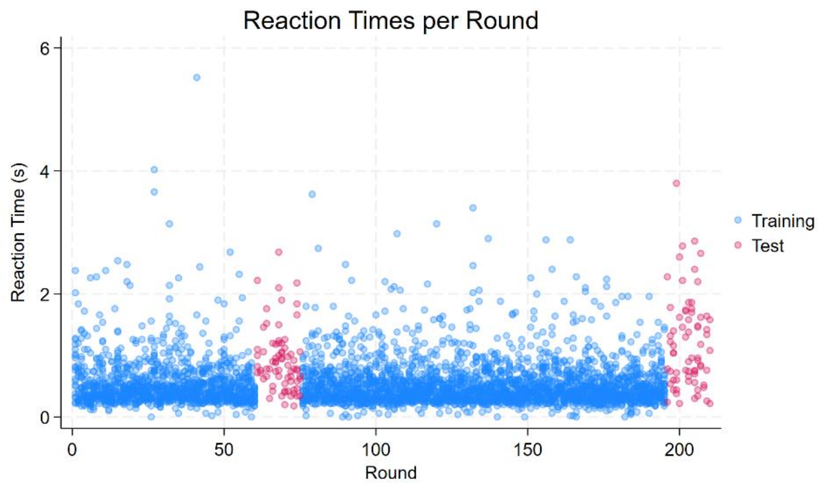


Figure 14

Scatterplot of reaction times per round



RT switch costs were calculated by subtracting the baseline RT, that of the training phase, from the RT of the test phase. Larger RT switch costs indicate greater difficulty in overriding habitual behavior in favor of goal-directed behavior.

A linear mixed-effects model using REML was conducted to examine the effects of phase (training vs. test), amount of training (day 1 vs. day 3), and their interaction on reaction time (RT) as fixed effects. Individual participants served as random effects. The model was specified as follows:

$$RT \sim phase * day + (1|participant)$$

First, for the random effect of individual differences among participants, variance was estimated at 0.002 (*SD* = 0.045). Residual variance was 0.153 (*SD* = 0.391) and the intercept (i.e., baseline RT) was 0.773 seconds. Secondly, we found a significant main effect of phase, b = -0.279, $p < .001$, indicating that reaction times were faster for training rounds than for test rounds by 0.279

seconds. There was also a significant main effect of day (i.e., amount of training), $p < .001$, revealing that reaction times were faster for day 1 than day 3 by 0.076 seconds. Finally, a significant interaction effect between phase and day was observed, $p < .001$. The interaction suggests that the difference in reaction times between training and test phases increased from day 1 to day 3, indicating a greater RT switch cost on day 3, $b = -0.067$. An overview of the results of the model can be found in Table 5.

Table 5

Results linear mixed-effects model

Effect	Estimate	*SE*	*df*	*t*	*p*
Fixed effects					
Intercept	.773	.021	39.5	36.95	<.001
Phase (Training vs. Test)	-0.279	.019	706.3	-15.06	<.001
Day (1 vs. 3)	-0.076	.018	751.8	-4.17	<.001
Phase × Day Interaction (RT Switch Cost)	0.067	.018	752.3	3.66	<.001

On day 1, reaction times were higher during the test phase (.909 seconds) than during the training phase (.484 seconds) with an average action switch cost of 0.909 - 0.484 = 0.425 seconds. For day 3, reaction times were also higher during the test phase (1.195 seconds) than during the training phase (.502 seconds). The average action switch cost for day 3 was 1.195 - 0.502 = 0.693 which was significantly higher than the action switch cost of day 1 ($p < .001$). From this we find that RT switch costs increased by 0.693 - 0.425 = 0.268 seconds from day 1 to day 3. These findings support hypothesis H_2, demonstrating that RT switch costs were significantly larger for the extensive training condition than the moderate one suggesting stronger habitual tendencies.

H_3: Habit Strength Survey Results

Finally, hypothesis H_3 states that self-reported habit strength is stronger for the extensive condition than the moderate condition. To test this, we compared self-reported scores of the SRBAI surveys administered each day. The SRBAI survey consisted of 4 items that are rated on a scale from 1 (strongly disagree) to 7 (strongly agree). Higher scores indicate a stronger habit strength. The overall mean score of the SRBAI across all days among participants (N = 22) was 4.56 (SD = 1.53). When broken down by day, the mean score for day 1 was 4.03 (SD = 1.42), for day 2 this was 4.84 (SD = 1.34), and for day 3 was 4.80 (SD = 1.73). The lowest mean score was 1, while the maximum mean score was 7. An overview of the SRBAI scores can be seen in Table 6. Figure 15 shows the individual SRBAI scores across participants.

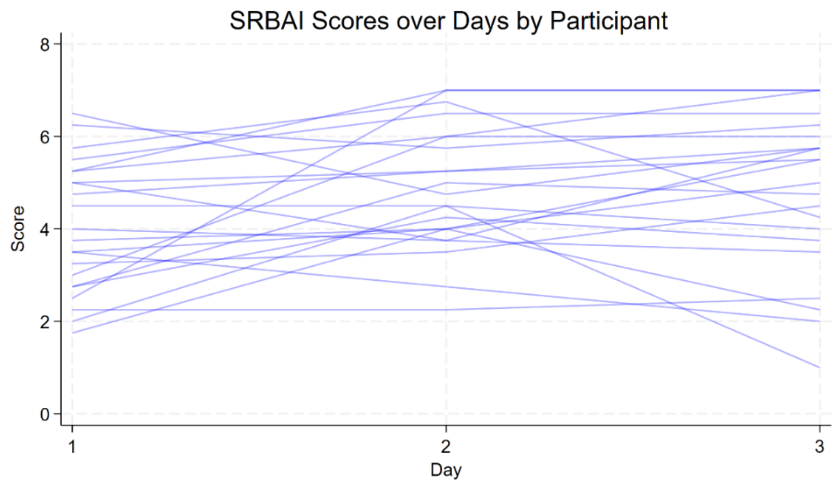
Table 6

SRBAI scores across days

Day	*M*	*SD*	Min	Max
1	4.03	1.42	1.75	6.5
2	4.84	1.34	2.25	7
3	4.80	1.73	1	7
All days	4.56	1.53	1	7

Figure 15

SRBAI scores per participant per day



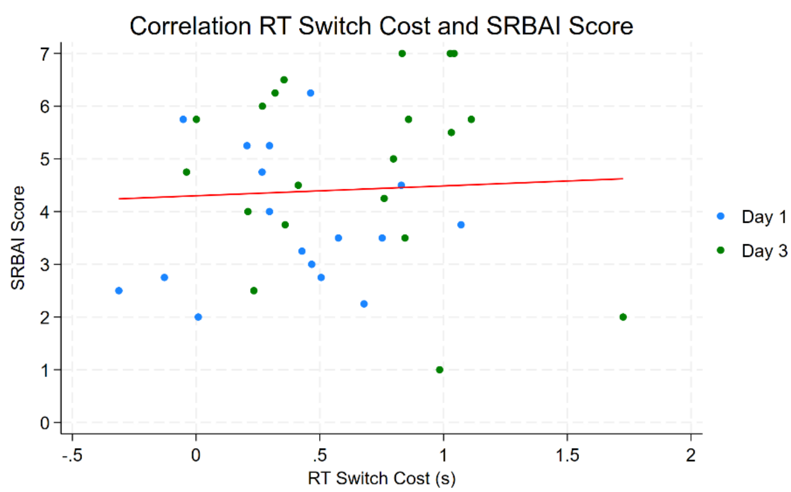
A repeated measures ANOVA was run to test whether the effect of day, i.e., the number of training rounds, influenced the SRBAI score. Assumptions for normality and homogeneity of variances were both confirmed using Shapiro-Wilk and Levene's tests. The analysis revealed a significant main effect of individual differences on the SRBAI score ($F(21, 42) = 3.28$, $p < .001$). In addition, there was also a significant main effect of day ($F(2 ,42) = 3.59$, $p = .0364$). This meant that both individual differences and the day the survey was taken had a significant effect on SRBAI score, and thus self-reported habit strength. The model explained 65.0% of the variance in the SRBAI score ($R^2 = 0.65$), with an adjusted $R^2 = 0.46$ accounting for 46% of the variance which indicates a moderate model fit.

Additionally, a post-hoc pairwise comparison with Bonferroni correction was applied to compare differences between days. The mean difference between day 1 and day 2 was 0.81 (SE = 0.34), $t(21) = 2.38$, $p = 0.065$, 95% CI [-0.04, 1.65]. Similarly, the mean difference between Day 3 and Day 1 was 0.76 (SE = 0.34), $t(21) = 2.25$, $p = 0.089$, 95% CI [-0.08, 1.61]. Finally, the difference between Day 3 and Day 2 was -0.05 (*SE* = 0.34), $t(21) = -0.13$, $p = 1.000$, 95% *CI* [-0.89, 0.80]. None of the comparisons reached statistical significance. However, a one-tailed t-test did reveal a statistically significant difference in SRBAI scores between day 1 and day 2 ($t(21) = -2.57$, $p = .0088$). Nonetheless, these findings do not support H_3, which tells us that self-reported habit strength did not increase after extended training.

Finally, SRBAI scores and RT switch costs neither correlate ($r = .05$) nor did they reveal a statistically significant relationship ($p = .77$). Figure 16 shows the data points and the fitted regression line. From this figure we can see two outliers from day 3 at approximately a SRBAI score of 1 and RT switch cost of 1 second as well as a SRBAI score of 2 and a RT switch cost of 1.75 seconds. Removing these outliers revealed a weak to moderate correlation ($r = .31$) but was again not statistically significant ($p = .068$).

Figure 16

Correlation between self-reported SRBAI scores and RT switch costs.



Player Experience Inventory Results

At the end of each experiment on each day, participants were asked to fill in the miniPXI. We made no predictions on the results of the miniPXI across days, but rather use the miniPXI results to gain insight into different aspects and design decisions of the game. The miniPXI consisted of 11 items that are rated on a 7-point Likert scale ranging from -3 (strongly disagree) to 3 (strongly agree). Each item represented a different construct. The total score is the aggregated score of all constructs of that day divided by 11 for ease of interpretation. Higher scores indicate a greater satisfaction with the corresponding construct.

An overview of the total scores and constructs can be found in Table 7. The mean score was 1.30 (*SD* = .79) across all days and constructs. On day 1 the mean was 1.45 (*SD* = .75), day 2 it was 1.18 (*SD* = .75), and on day 3 it was 1.26 (*SD* = .88). The functional subscale had a mean score of 1.70 (*SD* = .74) and the psychosocial subscale had a mean score of 0.95 (*SD* = .95).

Table 7

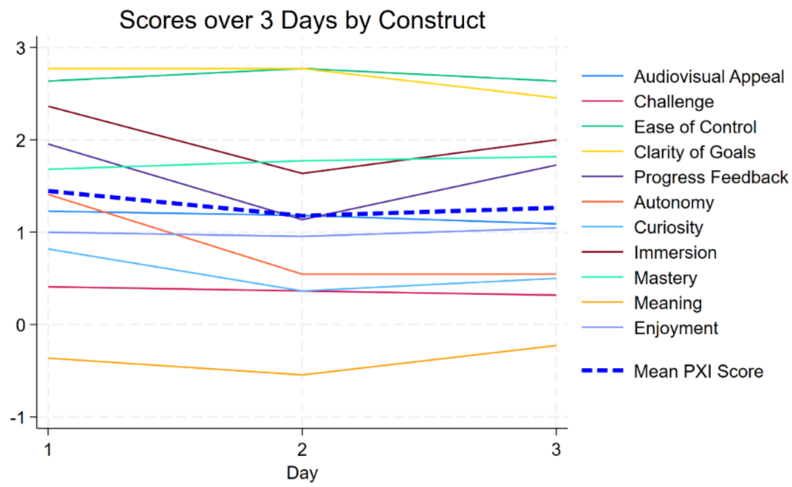
Scores of miniPXI survey across all 3 days

Construct	All	Day 1	Day 2	Day 3
Functional subscale				
Audiovisual Appeal (AA)	1.17	1.23	1.18	1.09
Challenge (CH)	0.36	0.41	0.36	0.32
Ease of Control (EC)	2.68	2.64	2.77	2.64
Clarity of Goals (GR)	2.67	2.77	2.77	2.46
Progress Feedback (PF)	1.61	1.96	1.14	1.72
Psychosocial subscale				
Autonomy (AUT)	0.83	1.42	0.55	0.55
Curiosity (CUR)	0.56	0.82	0.36	0.50
Immersion (IMM)	2.00	2.36	1.64	2.00
Mastery (MAS)	1.76	1.68	1.77	1.82
Meaning (MEA)	-0.38	-0.36	-0.55	-0.23
Enjoyment (ENJ)	1.00	1.00	0.96	1.05

A repeated measures ANOVA did not reveal a main effect of day on the aggregated miniPXI score $F = 3.12$, $p = .0545$. Normality assumptions for the score on each day were tested and confirmed. As well as homogeneity of variances. Additionally, the functional subscale and the psychosocial subscale scores were tested using a repeated measures ANOVA. The functional subscale did not find a main effect of day ($F = 1.28$, $p = 0.2876$). The psychosocial subscale did reveal a main effect of day ($F = 4.17$, $p = .0223$). A post-hoc Bonferroni-corrected pair-wise comparison finds an increase in the psychosocial subscale between day 1 and 2 of 0.42 ($t = -2.87$, $p = .019$). An overview of the mean miniPXI scores and each construct can be seen Figure 17.

Figure 17

Mean miniPXI scores and construct scores across days



Constructs such as ease of control, clarity of goals, and immersion scored relatively high (>= 2.00). Audiovisual appeal, progress feedback, and mastery showed a moderate score (1.17 <=> 1.76). Finally, challenge, autonomy, curiosity, and meaning showed lower scores (-0.38 <=> 0.83) with meaning even showing negative numbers.

Discussion

In this study we set out to investigate a new paradigm for habit induction in experimental settings. Researchers have struggled with consistently experimentally inducing habit in human subjects (de Wit et al., 2018). Others, such as Luque et al. (2020), argue that established metrics to study habit in rodents did not suffice in the study of habit in humans. In addition to this, habits in the real world form over a relatively long period of time in very dynamic environments, potentially making findings from experimental studies on habit poorly applicable to the real-life situations. Because of these problems, we proposed a video game paradigm to study habit. Video games can be experimentally applied and customized for specific use-cases while more closely mimicking real-life situations. Three hypotheses were tested: (H_1) action slips would be more frequent after extensive training than after moderate training, (H_2) that RT switch costs would be greater in the extensive training condition, and (H_3) that self-reported habit strength would be higher following extensive training.

During the experiment we collected data through self-reported questionnaires and implicit behavioral data gathered from the game. We found that participants were strongly engaged in the game following high completion rates (99.7%) of rounds. In addition, the near-perfect coin pickup rates during training rounds by day 3 show that participants became familiar and skilled with the mechanics and goals of the game. A regression analysis demonstrated the correlation between the number of rounds played and the average speed of the player which further supported that participants grew more skilled over time. This is not very surprising, and in line with literature that finds that gaming efficacy is largely dependent on prior gaming experience (e.g. Erfani et al., 2010).

Habitual Action Slips after Overtraining

Our first hypothesis stated that extensive training would yield more frequent action slips compared to moderate training during devalued test rounds. However, this hypothesis was not supported by the data. Participants with extensive training did not commit significantly more action slips during test rounds compared to those with moderate training. That is, when the coin was devalued, participants generally adjusted their behavior and avoided picking up the coin, regardless of the amount of prior training. This suggests that participants maintained sensitivity to outcome value across both conditions and that habitual control, as defined by action slips, did not dominate behavior. This result contrasts with earlier work such as that by Tricomi et al. (2009), who observed increased habitual behavior through action slips after overtraining in a food reward task on human participants. It also differs from the classic findings in animal literature, where overtraining frequently yields outcome-insensitive responding such as in the original outcome-devaluation task (Adams & Dickinson, 1981). One obvious explanation for the absence of action slips in our study may be that the game did not produce strong enough cue-response associations, or that participants retained sufficient cognitive resources to override automatic tendencies. However, the failure to replicate the expected effect on action slips is not totally unsurprising and aligns more closely with the findings of De Wit et al. (2018), who reported multiple failed replications of habit induction as measured by action slips through outcome-devaluation tasks in humans. De Wit et al. argued that humans are especially good at suppressing habitual responses when task demands or awareness are high. Our task, although gamified and engaging, may not have sufficiently limited goal-directed control to elicit habitual behavior. Additionally, the devaluation manipulation—removing the point value of the coin—may not have been psychologically salient enough to discourage habitual behavior.

RT Switch Costs as a Marker of Habit

The second hypothesis posited that the extensively trained participants would exhibit larger RT switch costs, reflecting greater difficulty in the remembering and selection of behavior options. Indeed, our linear-mixed effects model found that the difference in reaction times did not only significantly increase between training and test rounds, but that this difference was also larger after extensive training on day 3 compared to moderate training on day 1. This increase in switch cost suggests that the habitual response had become mostly or partly automatic, causing an interference when a switch to a goal-directed strategy was required. This result is consistent with the interpretation that extensive training strengthens habitual response tendencies, making it more effortful for participants to override those tendencies when needed.

RT switch costs were first introduced by Luque et al. (2020) as a better marker for habit than action slips in human subjects after several failed replications (de Wit et al., 2018). They demonstrated that even when humans do not overtly commit more action slips after overtraining, they may exhibit subtler signs of habit formation in their reaction time patterns, especially under time pressure, as was also demonstrated by Hardwick et al. (2019). Specifically, Luque et al. reported that the RT switch cost–the delay when participants had to switch to an alternative action after outcome devaluation–increased as a result of training. They argued that RT switch costs can reveal the influence of the habit system even in cases where overall choice accuracy remains high. Similarly to Luque et al.'s study, we designed our experiment with these findings in mind by adding time pressure to the game and following a similar within-subject design over the course of 3 days. Our findings were in line with those of Luque et al.: in the extensive condition, many participants still managed to choose the correct (goal-directed) response during test rounds, but they did so more slowly, implying an internal competition between the persisting habit and the conscious adjustment. In contrast, moderately trained participants, who would have weaker habitual tendencies, adjusted their responses more quickly. Thus, the findings from the RT switch costs strengthen the conclusion that extensive training engaged habitual control more strongly. It is worth noting that the magnitude of the RT switch cost in our study was not large, indicating that this measure captures a more subtle effect. Nonetheless, the presence of a significant switch cost supports H_2 and resonates with the idea that habits can be discovered using reaction times under certain conditions. Our use of a game paradigm contributes to this literature by replicating the RT switch cost phenomenon in an ecologically rich setting.

Self-Reported Habit Strength

Our third and final hypothesis predicted that self-reported habit strength would be higher after extensive training than after moderate training. However, SRBAI scores did not differ significantly between conditions, indicating that participants did not perceive their behavior as becoming more automatic and habitual after extensive training. This perception was further supported by the lack of correlation between SRBAI scores and RT switch costs. Therefore, we could not find support for this hypothesis. Even though participants did not experience their behavior as more automatic or habitual in the extensive training condition, they did show a greater RT switch cost. There are several possible interpretations for this finding. Firstly, it may indicate that participants remained aware and in control of their actions in both conditions, in such a way that neither condition experienced their behavior as an automatic habit. Habits in daily life are often characterized by a sense of automaticity, but the duration of our experiment (only three days of training) and duration of trials (1 hour each day) may have been too short for participants to internalize the habit to an extent to consciously notice. Similarly, Nebe et al. (2024) found that laboratory tasks often fail to correlate with self-reports of habit such as the SRBAI. Specifically, they found that neither action slips nor RT switch costs significantly correlated with SRBAI scores, calling into question the construct and ecological validity of automaticity as a habitual measure for habit.

Secondly, it is also possible that participants might have rated their behavior as goal-directed since they were aware they were in an experiment, resulting in no observable group difference. Overall, the failure to find any significant correlations or differences in self-reported habit strength across conditions highlights a gap between objective and subjective measures of habit—a theme also emphasized by Nebe et al.

Implications

The use of a video game paradigm to induce and study habit carries several important theoretical and practical implications. Theoretically, the differences in findings for explicit (action slips) and implicit (RT switch costs) behaviors further show the predisposition of humans in successfully applying goal-directed strategies, yet still being affected by conflicting systems as evidenced by their reaction times. The extensive training condition showed evidence for an internalized cue-outcome association influencing behavior through reduced reaction times when switching tasks, even though participants ultimately still behaved in a goal-directed manner. This is compatible with other dual-process studies such as that of Luque et al. (2020), Hardwick et al. (2019), and Zhang et al. (2024). This shift in balance in the behavioral conflict aligns with the idea that habitual control gradually develops rather than abruptly taking over decision-making (e.g. Zhang et al. 2024).

In addition, a video game environment may better mimic real-life scenarios in which habits form, providing better ecological validity which traditional lab tasks often lack. Our paradigm required participants to integrate stimuli, responses, and outcomes within a playful context. The enjoyable nature of the game, as evidenced by moderately positive scores of the miniPXI, indicates that the paradigm can potentially increase motivation and adherence to experiments. Practically, a game-based paradigm can leverage gamification to increase participant motivation and adherence in experiments, improving the ability to conduct habit research over extended periods of time.

In summary, the success of our game paradigm in inducing habitual behavior supports both the theoretical understanding that habits can be experimentally induced in humans, and a new practical method for researchers aiming to investigate or apply habit learning in an engaging and more ecological-valid way.

Limitations

Despite the advantages that a video game paradigm might bring there are several limitations to consider. Firstly, the sample used for statistical analysis was relatively small (n = 22) providing limited statistical power and generalizability. Even more so, the total sample (N = 32) was drastically reduced due to unusable data due to users incorrectly partaking in the experiment, implying significant shortcomings in the design of the game and the experiment. This exclusion not only reduced our sample size but may have introduced bias as well. In addition, participants who performed poorly were excluded too, making the sample only represent more experienced and proficient video game players, reducing the generalizability of the finding to less experienced players and the general population. For example, people who found the task too difficult might have responded differently. They could potentially rely more on goal-directed strategies as they need to more consciously put in effort in controlling the character—thus never switching to a habit-based strategy. However, this would not be something that we could capture in our study. In short, the study's modest size and selective exclusions limit the confidence in generalizing the results to larger or more diverse groups.

Secondly, the experiment was conducted in an unsupervised online setting, which raises concerns about experimental control. Participants played the game on their own computers, presumably in their home or other environment. Even though this might have improved ecological realism, it also means we had little control over distractions and variations in the setting. Some participants might have been interrupted, multitasking, or using different hardware set-ups, all of which can introduce variability. The fact that some participants incorrectly participated suggests that instructions or the at-home setup may have led to some misunderstanding by a subset of participants. Even though we attempted to standardize the experience via the game's design, the lack of a controlled lab environment remains a limitation potentially introducing noise to our measurements.

Thirdly, there were limitations in how we measured habitual behavior in the game, which could affect our findings. The threshold of detecting an action slip in our study was quite strict. We counted an action slip only if the player moved back toward the coin for at least 0.16 seconds. This threshold was chosen to filter out random noise in movement data, but it also meant that very brief or tentative moves towards the coin were not recorded as action slips. The possibility exists that some participants momentarily started to go for the coin (out of habit) and then quickly corrected themselves in under 0.16 seconds. Such instances would not meet our criteria for an action slip and would thus not have been detected. Effectively, our measure of action slips might underestimate the true frequency of action slips. A participant who hesitated or initiated a turn toward the coin but stopped immediately, or who oscillated in place, could have experienced a conflict between habit and goal-directed control that we failed to capture. Similarly, our reaction time measure, defined as the time to start an optimal response, might have suffered from the same problem. Particularly during test rounds, approximately only a quarter of optimal responses were captured compared to nearly hundred percent of training rounds. The reason for this, as explained in the methods section, is due to our detection algorithm only being able to capture instances of participating pausing before choosing to continue again. Participants that did not stop but continuously moved were therefore not captured—despite perhaps making the decision on the fly while still moving. This loss of nearly three quarters of optimal responses during test rounds means we missed different behaviors that may have contained valuable information regarding the habitual tendencies of participants. For instance, participants who were able to quickly and fluently make a goal-directed decision without stopping might still have experienced a momentary internal conflict between the habitual and goal-directed system—a conflict that was not observable due to the limitations of our detection method. As such, the current approach may have disproportionately favored the detection of slower, more deliberate decision-making while overlooking more subtle or automatic processes in others. Additionally, the SRBAI results did not seem to pick up on the more automatic processes of habit either. This could be due to the problems mentioned earlier, however another important consideration is the timing of the SRBAI within the experimental procedure. The survey was administered after the devaluation rounds—at a point when participants may have already begun to adjust their behavior in response to the devalued outcome. Since the SRBAI relies on retrospective self-assessment of automaticity, administering it after devaluation might have led to responses that reflected a more goal-directed mindset, rather than capturing the strength of habit during the training phase.

Finally, there are broader limits to the generalizability of this study to consider. Our study posits video games as a singular monolithic entity, however, video games are a very diverse field in which many genres and playstyles exist. In our experiment, we used a top-down 2D puzzle maze that was controlled using a keyboard. However, the generalizability to other types of games, for example platformers, shooters, RPGs, or other types of puzzlers, is unknown. Conversely, the medium, e.g. consoles or virtual reality sets, could also have varying implications on participant behavior. In addition, the habit we induced, repeatedly collecting a coin in a 2D maze game, is a relatively artificial and specific behavior. While it captures certain aspects of habit, such as repetition in a stable context leading to an automatic response, it may not encompass the full complexity of real-world habits. We did find that the game kept participants reasonably motivated over multiple days as indicated by the miniPXI results, and most participants returning each day, which is encouraging for its viability. However, an engaging game may still not trigger the same depth of habit learning as a behavior someone performs daily in their life. This is even more exacerbated by the lack of correlation between RT switch costs and SRBAI scores, bringing into question the ecological validity of the game and RT switch costs in general as argued by Nebe et al. (2024).

Recommendations for Game Design

We propose several adjustments to the game design for future research. To start, there were several reasons as to why many of the participants had to be excluded from the sample. Firstly, the video game should better account for participant's gaming experience in such a way that all participants can collect the coin and finish training rounds on time. One way to achieve this is through Dynamic Game Difficulty Balancing (DGDB; e.g. Perez et al., 2016; Tijs et al., 2008)—a technique in which game parameters, such as the time limit, dynamically change based on the performance of the player. Matching the difficulty of a game with the player's skill level can reduce feelings of frustration or boredom based on their performance. Applied to our game, this means that participants who were unable to finish rounds could now do so with more ease. For the goal of our experiment, it is imperative that participants form the cue-outcome association by picking up the coin during training rounds. If their game efficacy does not allow them to do that, the game needs to be adjusted to be more inclusive for less experienced players.

Additionally, some players were able to finish most rounds in time but refused to return to pick up any or most coins during training rounds. We speculate that the incentive to pick up the coin might have been too low. The coin only provided one extra point, even though returning to the coin took a significant amount of time and effort. Perhaps participants could not be bothered to invest extra energy and rather finished the experiment as quickly as possible. It does not help that the additional reward for getting the highest score was only €5—a relatively low number compared to the invested time necessary. Increasing the immediate point and potential monetary reward could make participants more motivated to pick up coins during training rounds in the future. In contrast, other players picked up coins in both the training and test rounds. Picking up coins during test rounds beats the purpose of the experiment making their data unusable. A reason as to why participants behaved this way could be due to them not having registered or understood the devaluation instruction. Therefore, it is important to more clearly communicate this devaluation. One such way could be by providing better instructions. A potentially more effective way could be to actively punish participants for picking up coins during test rounds by decreasing their score.

In addition, the results of the miniPXI found that the game was both easy to control and that the goals were clear, as well as a fairly high amount of immersion as experienced by the players. Future iterations should therefore follow similar design choices when it comes to these constructs.

Audiovisual appeal, progress feedback, and mastery scored lower than the aforementioned constructs but were still rated positively. However, there is still room for improvement in these constructs by improving graphics and providing clearer feedback on how the player is doing. One reason as to why the progress feedback construct might have scored lower is due to the fact that the floor counter in the GUI only represents the total floors of the current phase and not the total of the day. This means that participants might feel tricked that after reaching the 60/60th round when the counter resets to 0/15 for the test phase. Showing the total rounds of the day might be a more honest representation of their progress. Additionally, progress is only provided by their total score and floor counter going up. Adding additional markers of progress could potentially improve this construct. An example could be through changing the environments as players progress or having the ability to customize their character with points they have collected. Furthermore, challenge, autonomy, and curiosity were rated at the lower end of the spectrum, however still positively. The interpretation of the challenge construct is rather hard considering it is phrased in such a way that the game could either be too hard or too easy for people. Considering the response was fairly neutral, we assume the game strikes a good balance in difficulty for the sample (which excluded poor performing individuals).

Finally, future research could focus on applying the video game paradigm to other genres and modalities. Other genres, besides the 2D maze game used in the current study, might be better suited in ubiquitously forming cue-outcome associations for their participants—potentially further enhancing the gameplay experience. Additionally, deploying the paradigm on more accessible platforms, such as smartphones, could increase participant reach and allow for longer-term engagement. Since habits typically develop over extended periods, enabling participants to engage with the task in their everyday environments and at their own convenience could better capture the gradual nature of habit formation.

Recommendations for Experimental Design

Besides changes to the design of the game, we propose several recommendations for the experimental design, data collection, and analysis as well. Firstly, an obvious recommendation is to aim for a larger sample size. A bigger sample provides greater power to detect subtle effects and more reliable estimates of behavior variability. Habits can vary greatly between individuals (Singh et al., 2024), so a larger sample can ensure we capture that diversity. Essentially, a large sample supports scientific rigor by reducing the risk of false negatives and increasing confidence in the robustness of results. Adding to this, the current sample size was determined based on practical limitations. Future research should implement power analyses to more confidently justify the chosen sample size and claim that the study is adequately powered to detect the expected effects. This ensures that any null results can be more confidently interpreted, rather than dismissed as underpowered.

Secondly, we found that action slips did not significantly change from the moderate to extensive training condition. A reason as to why action slips might not have differed between conditions is because of how they are detected. One solution to this is by implementing a more accurate algorithm that is better at distinguishing real action slips from unintended movement data. Perhaps a simpler way is to redesign the game in such a way that complicated detection algorithms are not necessary at all—for example, instead of relying on movement data to detect action slips, using more unambiguous data points (e.g. a single button press) can help detecting action slips more easily. However, this might limit the users' sense of movement and control potentially diminishing experiential benefits that come with the video game paradigm (Markovitch et al., 2024).

Thirdly, as discussed earlier, SRBAI surveys were administered after devaluation rounds potentially causing participants to have unlearned habitual tendencies. Future designs may therefore benefit from incorporating the SRBAI immediately after training, to more accurately measure the automaticity of the behavior before the cue-outcome association is weakened or changed during test rounds. In addition to this, as Hardwick et al. (2019) noted, SRBAI scores and behavioral measures have trouble correlating limiting the ecological validity of behavioral measures. Future research could use a video game paradigm to try to consolidate this gap by developing longer studies that capture habit formation more organically, potentially leading to more similar SRBAI scores.

Finally, motivation is considered an important factor in the formation of habits, with evidence suggesting that highly motivated individuals form habits more quickly (Marien et al., 2018). Although motivation alone may not sustain long-term behavior, initial motivation is essential for executing goal-directed actions that can ultimately form into habits. As discussed, cognitive games have the potential to enhance participant engagement and motivation. This opens an interesting avenue for future research: investigating the interplay between gameplay experience, motivation, and habit formation. For instance, one could examine how manipulating elements of the PXI, such as autonomy, influences intrinsic motivation and, in turn, the induction of habits.

Conclusion

This study investigated the potential of a video game paradigm to successfully induce and study habit in an experimental setup. By embedding an outcome-devaluation task in a video game, we aimed to explore whether video games could serve as an alternative habit research paradigm with a higher ecological validity and experimental control. Our findings provide partial support for the efficacy of this novel paradigm. Participants demonstrated stronger habitual tendencies after extensive training as evidenced by increased RT switch costs. However, these tendencies were not consistently captured by other measures, such as action slip frequency or self-reported habit strength. Nonetheless, these findings were not surprising when compared to prior habit research and support the idea that habits often manifest in more tacit ways that may elude explicit measures like self-reports or action slips. Despite these promising findings, the study's limitations, such as the small sample size and problems in computationally identifying habitual patterns, warrant caution in interpreting the results. Future research should address these challenges and continue to refine the use of video games as experimental tools. With their capacity for immersion, adaptability, and long-term engagement, video games offer a promising opportunity for capturing the nuanced dynamics of habit formation. Leveraging this potential may ultimately contribute to a better understanding of habit.

References

Abeele, V. Vanden, Spiel, K., Nacke, L., Johnson, D., & Gerling, K. (2020). Development and validation of the player experience inventory: A scale to measure player experiences at the level of functional and psychosocial consequences. *International Journal of Human-Computer Studies*, *135*, 102370. https://doi.org/10.1016/J.IJHCS.2019.102370

Adams, C. D., & Dickinson, A. (1981). Instrumental Responding following Reinforcer Devaluation. *The Quarterly Journal of Experimental Psychology Section B*, *33*(2), 109–121. https://doi.org/10.1080/14640748108400816

Adlakha, S., Chhabra, D., & Shukla, P. (2020). Effectiveness of gamification for the rehabilitation of neurodegenerative disorders. *Chaos, Solitons & Fractals*, *140*, 110192. https://doi.org/10.1016/J.CHAOS.2020.110192

Akthar, S. (2024). *Unity Game Development Tutorials*. Github. https://github.com/shamim-akhtar/gamdev-unity/tree/main/MazeGen

Alfieri, F. M., da Silva Dias, C., de Oliveira, N. C., & Battistella, L. R. (2022). Gamification in Musculoskeletal Rehabilitation. *Current Reviews in Musculoskeletal Medicine*, *15*(6), 629–636. https://doi.org/10.1007/S12178-022-09797-W

Birk, M. V., Atkins, C., Bowey, J. T., & Mandryk, R. L. (2016). Fostering intrinsic motivation through avatar identification in digital games. *Conference on Human Factors in Computing Systems - Proceedings*, 2982–2995. https://doi.org/10.1145/2858036.2858062

Birk, M. V., & Mandryk, R. L. (2018). Combating attrition in digital self-improvement programs using avatar customization. *Conference on Human Factors in Computing Systems - Proceedings*, *2018-April*. https://doi.org/10.1145/3173574.3174234

Danner, U. N., Aarts, H., & De Vries, N. K. (2008). Habit vs. intention in the prediction of future behaviour: The role of frequency, context stability and mental accessibility of past behaviour.

British Journal of Social Psychology, *47*(2), 245–265. https://doi.org/10.1348/014466607X230876

Daw, N. D., Niv, Y., & Dayan, P. (2005). Uncertainty-based competition between prefrontal and dorsolateral striatal systems for behavioral control. *Nature Neuroscience 2005 8:12*, *8*(12), 1704–1711. https://doi.org/10.1038/nn1560

De Wit, S., & Dickinson, A. (2009). Associative theories of goal-directed behaviour: A case for animal-human translational models. *Psychological Research*, *73*(4), 463–476. https://doi.org/10.1007/S00426-009-0230-6

de Wit, S., Kindt, M., Knot, S. L., Verhoeven, A. A. C., Robbins, T. W., Gasull-Camos, J., Evans, M., Mirza, H., & Gillan, C. M. (2018). Shifting the balance between goals and habits: Five failures in experimental habit induction. *Journal of Experimental Psychology: General*, *147*(7), 1043–1065. https://doi.org/10.1037/XGE0000402

DeRight, J., & Jorgensen, R. S. (2015). I Just Want My Research Credit: Frequency of Suboptimal Effort in a Non-Clinical Healthy Undergraduate Sample. *The Clinical Neuropsychologist*, *29*(1), 101–117. https://doi.org/10.1080/13854046.2014.989267

Deterding, S., Dixon, D., Khaled, R., & Nacke, L. (2011). From game design elements to gamefulness: Defining “gamification.” *Proceedings of the 15th International Academic MindTrek Conference: Envisioning Future Media Environments, MindTrek 2011*, 9–15. https://doi.org/10.1145/2181037.2181040

Erfani, M., Seif El-Nasr, M., Milam, D., Aghabeigi, B., Lameman, B. A., Riecke, B. E., Maygoli, H., & Mah, S. (2010). The effect of age, gender, and previous gaming experience on game play performance. *IFIP Advances in Information and Communication Technology*, *332*, 293–296. https://doi.org/10.1007/978-3-642-15231-3_33

Feil, K., Allion, S., Weyland, S., & Jekauc, D. (2021). A Systematic Review Examining the Relationship Between Habit and Physical Activity Behavior in Longitudinal Studies. *Frontiers in Psychology*, *12*, 626750. https://doi.org/10.3389/FPSYG.2021.626750

Fournier, M., d'Arripe-Longueville, F., & Radel, R. (2017). Testing the effect of text messaging cues to promote physical activity habits: a worksite-based exploratory intervention. *Scandinavian Journal of Medicine & Science in Sports*, *27*(10), 1157–1165. https://doi.org/10.1111/SMS.12730

Freitas, J. R. S., Velosa, V. H. S., Abreu, L. T. N., Jardim, R. L., Santos, J. A. V., Peres, B., & Campos, P. F. (2021). Virtual Reality Exposure Treatment in Phobias: a Systematic Review. *Psychiatric Quarterly*, *92*(4), 1685–1710. https://doi.org/10.1007/S11126-021-09935-6

Friehs, M. A., Dechant, M., Vedress, S., Frings, C., & Mandryk, R. L. (2020). Effective Gamification of the Stop-Signal Task: Two Controlled Laboratory Experiments. *JMIR Serious Games*, *8*(3), e17810. https://doi.org/10.2196/17810

Gałecki, A., & Burzykowski, T. (2013). *Linear Mixed-Effects Models Using R*. https://doi.org/10.1007/978-1-4614-3900-4

Gardner, B. (2015). A review and analysis of the use of "habit" in understanding, predicting and influencing health-related behaviour. *Health Psychology Review*, *9*(3), 277–295. https://doi.org/10.1080/17437199.2013.876238

Gardner, B., Abraham, C., Lally, P., & de Bruijn, G. J. (2012). Towards parsimony in habit measurement: Testing the convergent and predictive validity of an automaticity subscale of the Self-Report Habit Index. *International Journal of Behavioral Nutrition and Physical Activity*, *9*(1), 1–12. https://doi.org/10.1186/1479-5868-9-102

Gardner, B., De Bruijn, G.-J., Lally, P., Gardner, B., Lally, P., & De Bruijn, G.-J. (2011). A Systematic Review and Meta-analysis of Applications of the Self-Report Habit Index to Nutrition and

Physical Activity Behaviours. *Annals of Behavioral Medicine*, *42*(2), 174–187. https://doi.org/10.1007/S12160-011-9282-0

Gera, R., Barak, S., & Schonberg, T. (2024). A novel free-operant framework enables experimental habit induction in humans. *Behavior Research Methods*, *56*(4), 3937–3958. https://doi.org/10.3758/S13428-023-02263-6

Gillan, C. M., Papmeyer, M., Morein-Zamir, S., Sahakian, B. J., Fineberg, N. A., Robbins, T. W., & De Wit, S. (2011). Disruption in the balance between goal-directed behavior and habit learning in obsessive-compulsive disorder. *American Journal of Psychiatry*, *168*(7), 718–726. https://doi.org/10.1176/APPI.AJP.2011.10071062

Granberg, A. (2021). *A* Pathfinding Project*. https://arongranberg.com/astar/

Haider, A., Harteveld, C., Johnson, D., Birk, M. V., Mandryk, R. L., Seif El-Nasr, M., Nacke, L. E., Gerling, K., & Vanden Abeele, V. (2022). miniPXI: Development and Validation of an Eleven-Item Measure of the Player Experience Inventory. *Proceedings of the ACM on Human-Computer Interaction*, *6*. https://doi.org/10.1145/3549507

Hardwick, R. M., Forrence, A. D., Krakauer, J. W., & Haith, A. M. (2019). Time-dependent competition between goal-directed and habitual response preparation. *Nature Human Behaviour 2019 3:12*, *3*(12), 1252–1262. https://doi.org/10.1038/s41562-019-0725-0

Hartogsveld, B., Van Ruitenbeek, P., Quaedflieg, C. W. E. M., & Smeets, T. (2020). Balancing Between Goal-Directed and Habitual Responding Following Acute Stress. *Experimental Psychology*, *67*(2), 99–111. https://doi.org/10.1027/1618-3169/A000485

Hawlader, M. D. H., Mozid, N. E., Sharmin, S., Monju, I. H., Ahmed, S. B., Sarker, W., Amin, M. A., Jhumur, S. S., & Dalal, K. (2023). The art of forming habits: applying habit theory in changing physical activity behaviour. *Journal of Public Health (Germany)*, *31*(12), 2045–2057. https://doi.org/10.1007/S10389-022-01766-4

Jiang, Y., Han, J., Xiao, M., Liu, Y., & Chen, H. (2024). Time pressure promotes habitual control over goal-directed control among individuals with overweight and obesity. *Current Psychology*, *43*(5), 4431–4442. https://doi.org/10.1007/S12144-023-04315-8

Jones, B. A., Madden, G. J., & Wengreen, H. J. (2014). The FIT Game: preliminary evaluation of a gamification approach to increasing fruit and vegetable consumption in school. *Preventive Medicine*, *68*, 76–79. https://doi.org/10.1016/J.YPMED.2014.04.015

Judah, G., Gardner, B., & Aunger, R. (2013). Forming a flossing habit: An exploratory study of the psychological determinants of habit formation. *British Journal of Health Psychology*, *18*(2), 338–353. https://doi.org/10.1111/J.2044-8287.2012.02086.X

Kenney. (2024). *Assets · Kenney*. https://kenney.nl/assets

Koivisto, J., & Hamari, J. (2019). The rise of motivational information systems: A review of gamification research. *International Journal of Information Management*, *45*, 191–210. https://doi.org/10.1016/J.IJINFOMGT.2018.10.013

Lally, P., Van Jaarsveld, C. H. M., Potts, H. W. W., & Wardle, J. (2010). How are habits formed: Modelling habit formation in the real world. *European Journal of Social Psychology*, *40*(6), 998–1009. https://doi.org/10.1002/EJSP.674

Landers, R. N., & Landers, A. K. (2014). An Empirical Test of the Theory of Gamified Learning: The Effect of Leaderboards on Time-on-Task and Academic Performance. *Simulation and Gaming*, *45*(6), 769–785. https://doi.org/10.1177/1046878114563662

Lumsden, J., Edwards, E. A., Lawrence, N. S., Coyle, D., & Munafò, M. R. (2016). Gamification of cognitive assessment and cognitive training: A systematic review of applications and efficacy. *JMIR Serious Games*, *4*(2), e5888. https://doi.org/10.2196/games.5888

Luque, D., Molinero, S., Watson, P., López, F. J., & Le Pelley, M. E. (2020). Measuring habit formation through goal-directed response switching. *Journal of Experimental Psychology: General*, *149*(8), 1449–1459. https://doi.org/10.1037/XGE0000722

Markopoulos, A. P., Fragkou, A., Kasidiaris, P. D., & Davim, J. P. (2015). Gamification in engineering education and professional training. *International Journal of Mechanical Engineering Education*, *43*(2), 118–131. https://doi.org/10.1177/0306419015591324

Markovitch, B., Markopoulos, P., & Birk, M. V. (2024). Tunnel Runner: a Proof-of-principle for the Feasibility and Benefits of Facilitating Players' Sense of Control in Cognitive Assessment Games. *Conference on Human Factors in Computing Systems - Proceedings*. https://doi.org/10.1145/3613904.3642418

Marsh, R., Steinglass, J. E., Graziano, K., Peterson, B. S., & Walsh, B. T. (2007). Self-Regulatory Control and Habit Learning in the Development of Eating Disorders. *Current Psychiatry Reviews*, *3*(1), 73–83. https://doi.org/10.2174/157340007779815691

Mazar, A., & Wood, W. (2018). Defining habit in psychology. *The Psychology of Habit: Theory, Mechanisms, Change, and Contexts*, 13–29. https://doi.org/10.1007/978-3-319-97529-0_2

McMahan, R. P., Ragan, E. D., Leal, A., Beaton, R. J., & Bowman, D. A. (2011). Considerations for the use of commercial video games in controlled experiments. *Entertainment Computing*, *2*(1), 3–9. https://doi.org/10.1016/J.ENTCOM.2011.03.002

Miller, K. J., Shenhav, A., & Ludvig, E. A. (2019). Habits without values. *Psychological Review*, *126*(2), 292–311. https://doi.org/10.1037/REV0000120

Nebe, S., Kretzschmar, A., Brandt, M. C., & Tobler, P. N. (2024). Characterizing Human Habits in the Lab. *Collabra. Psychology*, *10*(1), 92949. https://doi.org/10.1525/COLLABRA.92949

Perez, L. J. F., Calla, L. A. R., Valente, L., Montenegro, A. A., & Clua, E. W. G. (2016). Dynamic game difficulty balancing in real time using evolutionary fuzzy cognitive maps. *Brazilian Symposium* *on Games and Digital Entertainment, SBGAMES*, 24–32. https://doi.org/10.1109/SBGAMES.2015.17

Pierce-Messick, Z., & Corbit, L. H. (2024). Outcome Devaluation as a Means of Distinguishing Habits from Goal-Directed Actions. *Habits*, 23–43. https://doi.org/10.1007/978-3-031-55889-4_2

R Core Team. (2023). *R: A Language and Environment for Statistical Computing* (4.3.2). https://www.R-project.org/

Raison, H., & Harris, R. V. (2024). Does the Self-Reported Behavioural Automaticity Index provide a valid measure of toothbrushing behaviour in adults? *Community Dentistry and Oral Epidemiology*, *52*(4), 381–388. https://doi.org/10.1111/CDOE.12922

Schwabe, L., & Wolf, O. T. (2010). Socially evaluated cold pressor stress after instrumental learning favors habits over goal-directed action. *Psychoneuroendocrinology*, *35*(7), 977–986. https://doi.org/10.1016/J.PSYNEUEN.2009.12.010

Shiffman, S., Engberg, J. B., Paty, J. A., Perz, W. G., Gnys, M., Kassel, J. D., & Hickcox, M. (1997). A day at a time: Predicting smoking lapse from daily urge. *Journal of Abnormal Psychology*, *106*(1), 104–116. https://doi.org/10.1037/0021-843X.106.1.104

Singh, B., Murphy, A., & Smith, A. E. (2024). *Time to Form a Habit: A Systematic Review and Meta-Analysis of Health Behaviour Habit Formation and Its Determinants*. https://doi.org/10.3390/healthcare12232488

Sjoerds, Z., Dietrich, A., Deserno, L., de Wit, S., Villringer, A., Heinze, H. J., Schlagenhauf, F., & Horstmann, A. (2016). Slips of Action and Sequential Decisions: A Cross-Validation Study of Tasks Assessing Habitual and Goal-Directed Action Control. *Frontiers in Behavioral Neuroscience*, *10*(DEC), 234. https://doi.org/10.3389/FNBEH.2016.00234

StataCorp. (2023). *Stata Statistical Software: Release 18*. StataCorp LLC.

Stojanovic, M., Grund, A., & Fries, S. (2022). Context Stability in Habit Building Increases Automaticity and Goal Attainment. *Frontiers in Psychology*, *13*, 883795. https://doi.org/10.3389/FPSYG.2022.883795

Tijs, T. J. W., Brokken, D., & Ijsselsteijn, W. A. (2008). Dynamic Game Balancing by Recognizing Affect. *Lecture Notes in Computer Science (Including Subseries Lecture Notes in Artificial Intelligence and Lecture Notes in Bioinformatics)*, *5294 LNCS*, 88–93. https://doi.org/10.1007/978-3-540-88322-7_9

Tobias, R. (2009). Changing Behavior by Memory Aids: A Social Psychological Model of Prospective Memory and Habit Development Tested With Dynamic Field Data. *Psychological Review*, *116*(2), 408–438. https://doi.org/10.1037/A0015512

Tricomi, E., Balleine, B. W., & O'Doherty, J. P. (2009). A specific role for posterior dorsolateral striatum in human habit learning. *European Journal of Neuroscience*, *29*(11), 2225–2232. https://doi.org/10.1111/J.1460-9568.2009.06796.X

Unity Technologies. (2024). *Unity* (6000.0.26f1). https://unity.com/

van de Mortel, T. F. (2008). Faking It: Social Desirability Response Bias in Self-report Research. *The Australian Journal of Advanced Nursing*, *25*(4), 40–48. https://search.informit.org/doi/abs/10.3316/INFORMIT.210155003844269

van der Weiden, A., Benjamins, J., Gillebaart, M., Ybema, J. F., & de Ridder, D. (2020). How to Form Good Habits? A Longitudinal Field Study on the Role of Self-Control in Habit Formation. *Frontiers in Psychology*, *11*, 494700. https://doi.org/10.3389/FPSYG.2020.00560

Verkuyl, M., Violato, E., Harder, N., Southam, T., Lavoie-Tremblay, M., Goldsworthy, S., Ellis, W., Campbell, S. H., & Atack, L. (2024). Virtual simulation in healthcare education: a multi-professional, pan-Canadian evaluation. *Advances in Simulation*, *9*(1). https://doi.org/10.1186/S41077-023-00276-X

Verplanken, B., & Orbell, S. (2003). Reflections on Past Behavior: A Self-Report Index of Habit Strength1. *Journal of Applied Social Psychology*, *33*(6), 1313–1330. https://doi.org/10.1111/J.1559-1816.2003.TB01951.X

Verplanken, B., & Orbell, S. (2019). Habit and Behavior Change. *Social Psychology in Action: Evidence-Based Interventions from Theory to Practice*, 65–78. https://doi.org/10.1007/978-3-030-13788-5_5

Verplanken, B., & Orbell, S. (2022). Attitudes, Habits, and Behavior Change. *Annual Review of Psychology*, *73*(Volume 73, 2022), 327–352. https://doi.org/10.1146/ANNUREV-PSYCH-020821-011744

Wiley, K., Robinson, R., & Mandryk, R. L. (2021). The Making and Evaluation of Digital Games Used for the Assessment of Attention: Systematic Review. *JMIR Serious Games*, *9*(3), e26449. https://doi.org/10.2196/26449

Wiley, K., Vedress, S., & Mandryk, R. L. (2020). How Points and Theme Affect Performance and Experience in a Gamified Cognitive Task. *Conference on Human Factors in Computing Systems - Proceedings*. https://doi.org/10.1145/3313831.3376697

Wood, W., Mazar, A., & Neal, D. T. (2022). Habits and Goals in Human Behavior: Separate but Interacting Systems. *Perspectives on Psychological Science*, *17*(2), 590–605. https://doi.org/10.1177/1745691621994226

Wood, W., Quinn, J. M., & Kashy, D. A. (2002). Habits in everyday life: thought, emotion, and action. *Journal of Personality and Social Psychology*, *83*(6), 1281–1297. https://doi.org/10.1037/0022-3514.83.6.1281

Zeybek, N., & Saygı, E. (2024). Gamification in Education: Why, Where, When, and How?—A Systematic Review. *Games and Culture*, *19*(2), 237–264. https://doi.org/10.1177/15554120231158625

Zhang, C., Spelt, H., van Wissen, A., Lakens, D., & IJsselsteijn, W. A. (2022). Habit and Goal-Related Constructs in Determining Toothbrushing Behavior: Two Sensor-Based Longitudinal Studies. *Health Psychology*, *41*(7), 463–473. https://doi.org/10.1037/HEA0001199

Zhang, C., van Wissen, A., Dotsch, R., Lakens, D., & IJsselsteijn, W. A. (2024). A Sequential Sampling Approach to the Integration of Habits and Goals. *Computational Brain and Behavior*, *7*(3), 480–501. https://doi.org/10.1007/S42113-024-00199-4

Appendix A

Screenshots of Game

Figure A1

Screenshot of the introduction page for the experiment on day 1

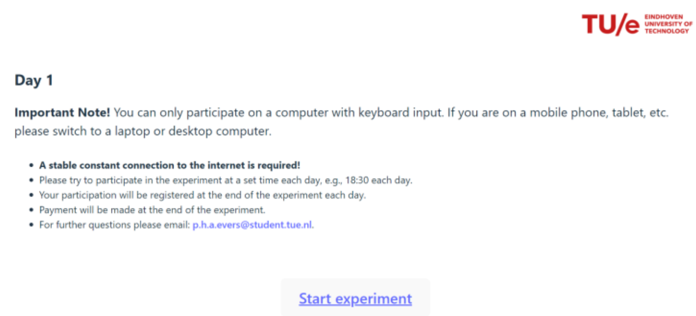


Figure A2

Screenshot of the first screen of the game where the participant is asked to enter their email to identify themselves

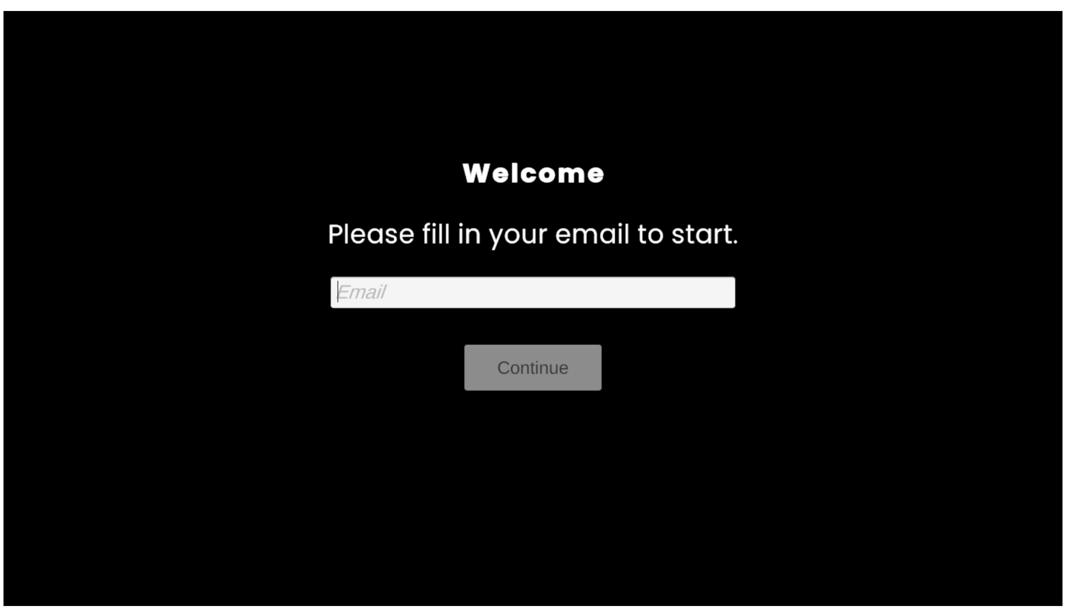


Figure A3

Screenshot of the title screen on day 1

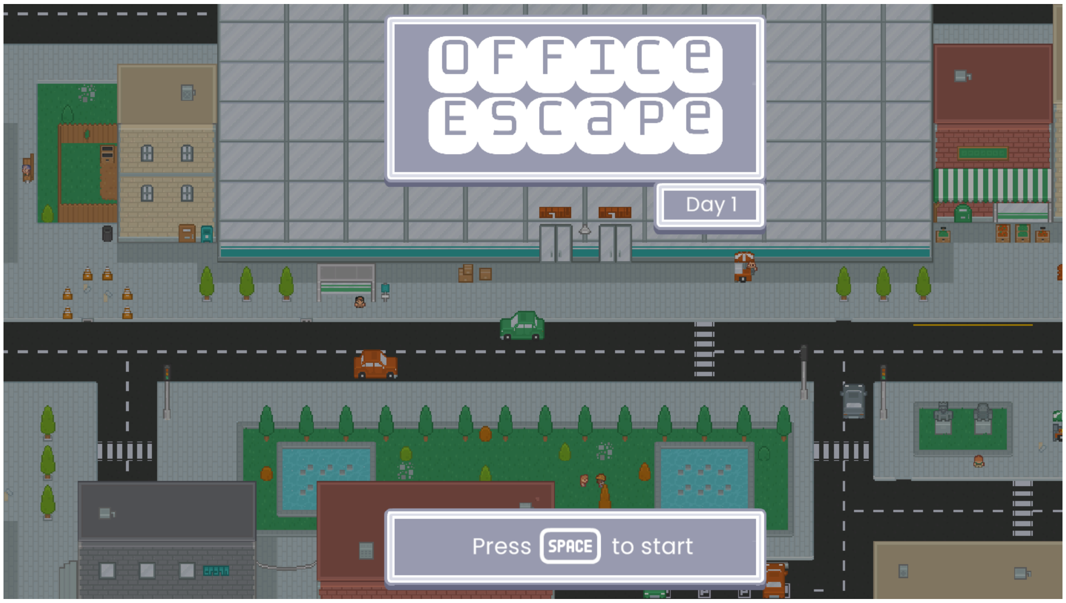


Figure A4

Screenshot of the narrative, instructions, and controls of the game

Figure A5

Screenshot of the character selection, with the 3rd option chosen

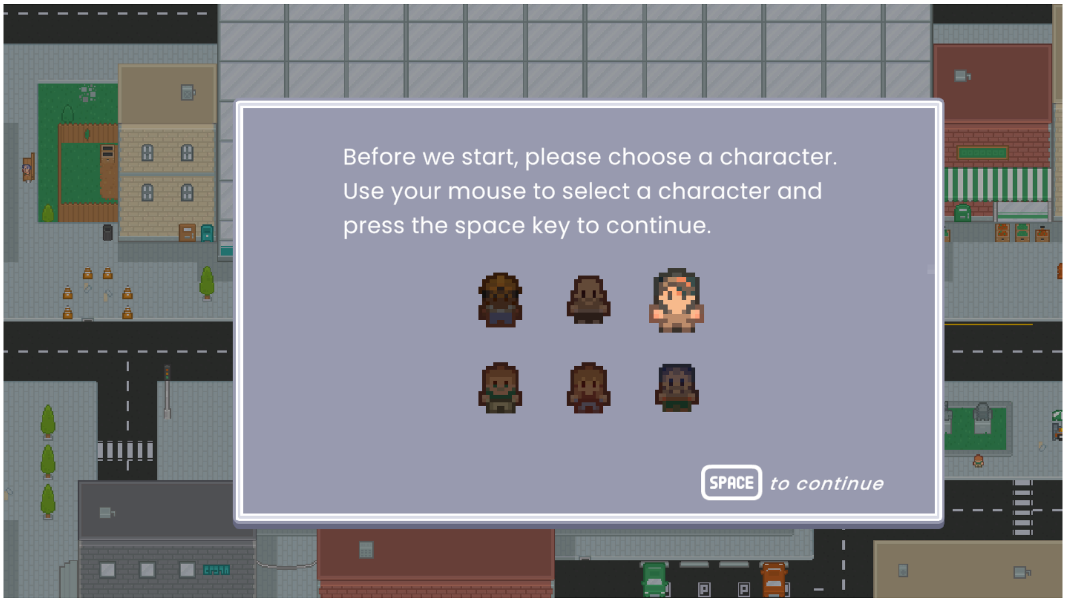


Figure A6

Final screen reminding players to return for the next day and that they can safely exit the page

Appendix B

Survey Screenshots

Figure B1

Screenshot of the SRBAI survey as seen in-game

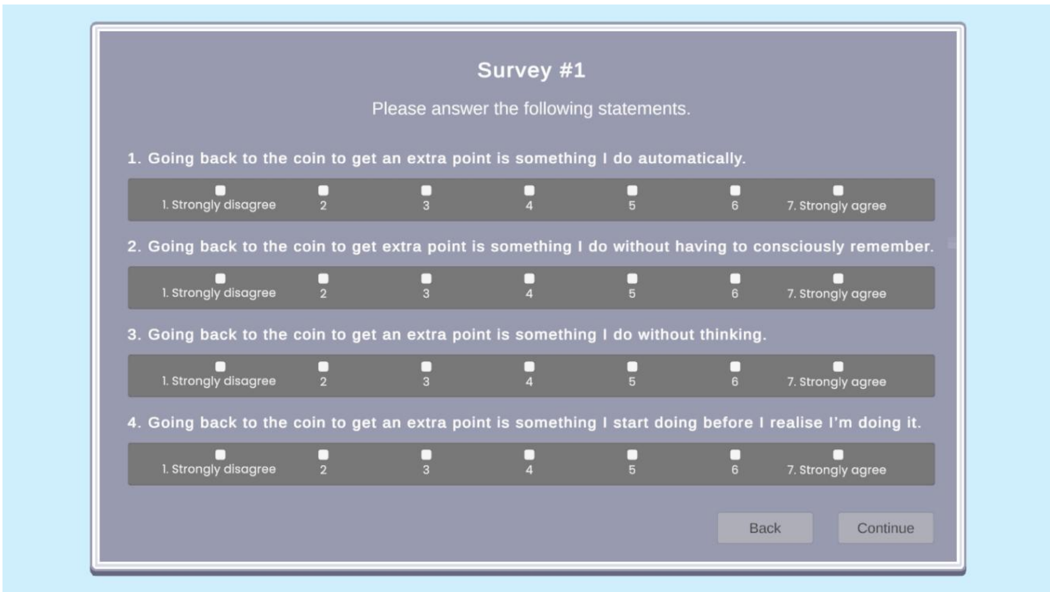
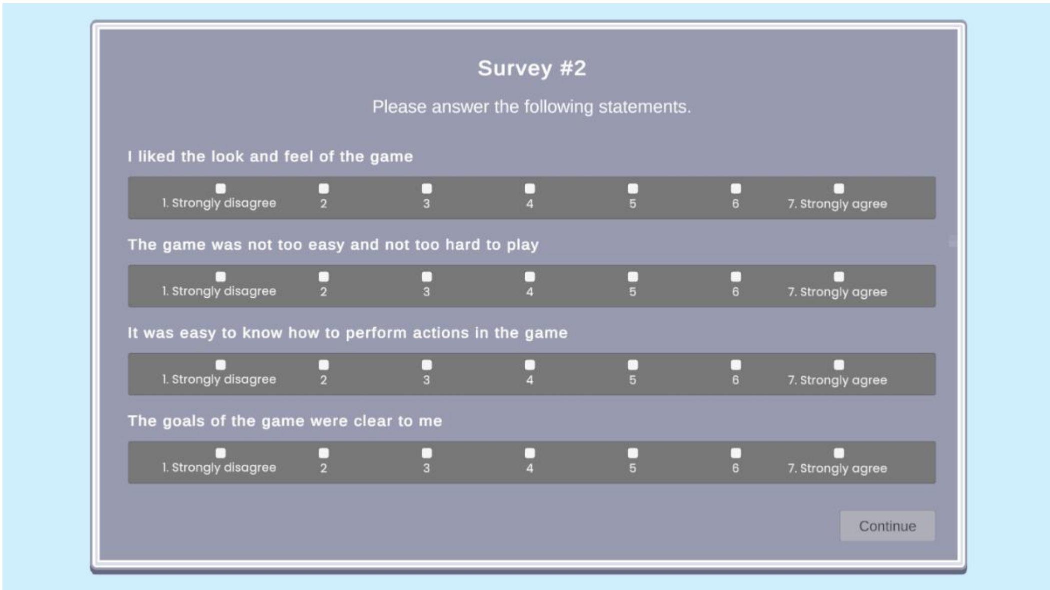
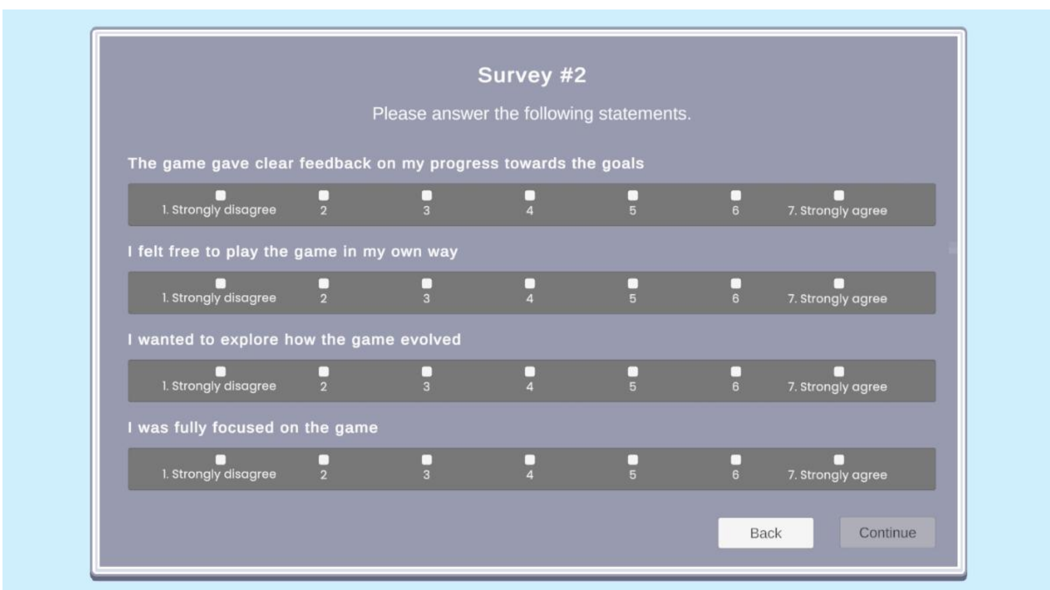
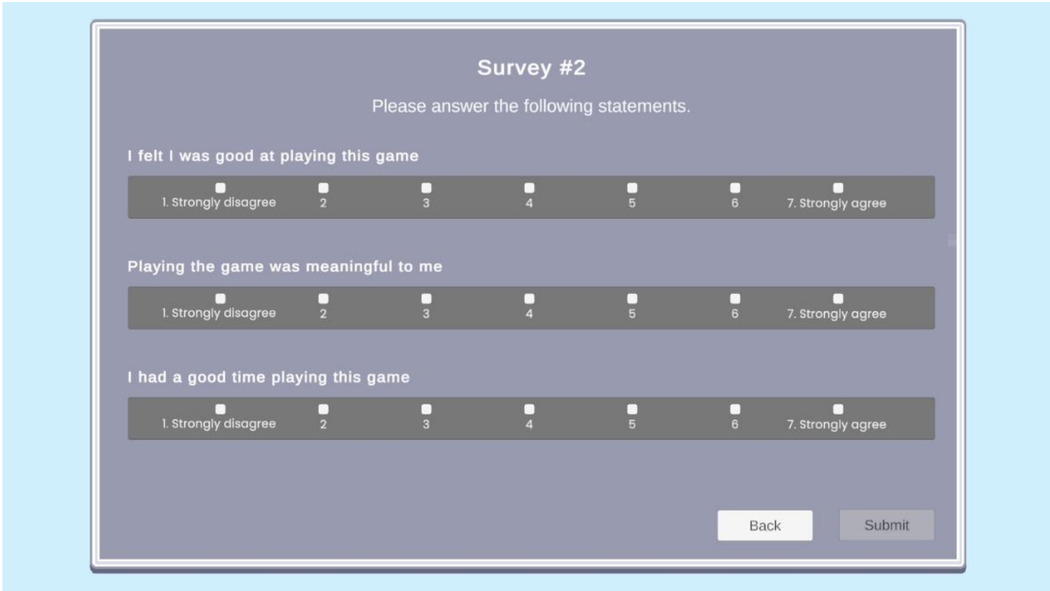


Figure B2

Screenshots of the miniPXI survey as seen in-game in chronological order






Survey #2
Please answer the following statements.
I felt I was good at playing this game
1. Strongly disagree
2
3
4
5
6
7. Strongly agree
Playing the game was meaningful to me
1. Strongly disagree
2
3
4
5
6
7. Strongly agree
I had a good time playing this game
1. Strongly disagree
2
3
4
5
6
7. Strongly agree
Back
Submit