Examining the Preparatory Function of Counterfactual Thinking: Evidence on Content, Benefits, and Evaluation of Forgone Outcomes

Doctoral programme in Cognitive and Brain Sciences (XXXV Cycle)

Doctoral candidate: Alessandro Bogani

Tutor: Stefania Pighin, PhD

November 2023

Abstract

Functional Theory, the prevailing perspective on the function of counterfactual thinking, posits that the primary purpose of this form of mental simulation is to prepare individuals for the future. However, recent findings have presented challenges to this dominant view. The debate on this topic has recently centered around the possibility that these contradictory results may have arisen from the use of tasks that are inadequate to observe the preparatory function of counterfactuals. Moreover, it has been stressed the importance of considering also more spontaneous (or, at least, more intrinsically motivated) instances of counterfactual thinking when reflecting over its function. In this thesis, Experiments 1 to 3 investigated questions related to the content of counterfactual modifications and their beneficial effects on future performance, utilizing a novel task designed specifically to address previous limitations advanced by the proponents of the Functional Theory. Nonetheless, most of our results did not align with what would have been expected if counterfactual thoughts were produced mainly in a preparatory fashion. Experiments 4 to 6, instead, explored individuals' inclination to look for non-instrumental counterfactual information, which pertains to information about the outcome of forgone options that does not contribute to improving future outcomes. This type of information seeking, involving the comparison between an actual and a forgone outcome, can represent a proxy of an underlying, genuine process of counterfactual analysis, and thus provides valuable insights for the debate on the function of counterfactual thinking. Indeed, results indicated that individuals readily look for counterfactual information even when it cannot serve any preparatory goal, challenging the notion that the consideration of alternatives to past events is strictly tied to the presence of such goals. These findings, along with prior research, raise questions about the extent to which counterfactual thoughts are produced to prepare for the future, prompting a reevaluation of its underlying functions.

Table of Contents

Chapter 1 – Counterfactual Thinking: Theoretical Background

When thinking about the past, we are not bound to recollect it in the exact same way as it unfolded, and, in fact, it is a common experience to find ourselves wondering about what could have been, that is to engage in counterfactual thinking. Mastering this form of mental simulation is not an easy task: Its emergence in children has been linked to the acquisition of a host of skills, such as the ability to represent and compare multiple possibilities, that appears to depend on profound changes in executive functions (Beck & Riggs, 2014). In part because of the complexity of this form of thinking, there is still no consensus on the age range in which children start showing signs of producing counterfactual simulations (Beck, 2020), and differences with adults can still be noticed in children aged 8-13, for example in terms of the content of such simulations (Stragà et al., 2022). Once adulthood is considered, however, there is clear evidence that counterfactual thoughts constitute a non-negligible portion of people's mental landscape (Branch & Zickar, 2021; Summerville & Roese, 2008) and that they represent a rather unique form of mental time travel. Certainly, counterfactuals present some similarities with other types of simulations, such as future thinking, that allows us to explore, in general, possible future scenarios, or prefactual thinking, in which we specifically simulate how an event that we were told about or that we experienced personally could be different in the future. For example, future and counterfactual thoughts recruit largely the same brain regions when activated (Schacter et al., 2015), they can both induce memory distortions (e.g., Gerlach et al., 2014; Petrocelli et al., 2016; Schacter, 2012), and, compared to recalled events, they are perceived as less vivid and are less likely to include temporal details when remembered (De Brigard et al., 2016, 2020). However, several results have highlighted the many ways in which counterfactual simulations are different from future-oriented ones. Indeed, in terms of development, counterfactual thoughts appear to be more difficult to master compared to future ones (Beck et al., 2011), they are perceived as having lower emotional intensity (e.g., Özbek et al., 2017), and they are appraised as less plausible after repetition compared to future thoughts, whose plausibility instead increases when repeatedly simulated (De Brigard et al., 2013; Szpunar & Schacter, 2013).

Part of the reason why these counterfactual simulations occur so frequently amidst our thoughts is that they are intertwined with numerous facets of psychology (Byrne, 2016). For example, counterfactual simulations are known to shape causal (e.g., Gerstenberg et al., 2021; Quillien & Lucas, 2023; Wells & Gavanski, 1989) and blame attributions (e.g., Goldinger et al., 2003; Parkinson & Byrne, 2017), to act as the foundations for the emotion of regret, which deeply influences decision-making (e.g., Connolly & Zeelenberg, 2002; Coricelli & Rustichini, 2010; Zeelenberg & van Dijk, 2004), and in general to affect the kind of inferences that people make (e.g., Egan & Byrne, 2012; Ferguson, 2012; Frosch & Byrne, 2012). In line with their relevance for a variety of psychological processes, the investigation of counterfactual thoughts offers fruitful insights in a number of applied contexts, such as human-technology interaction (e.g., Miller, 2019; Warren et al., 2023), mental health research (e.g., Broomhall et al., 2017; Parikh et al., 2020) and the evaluation of political events (e.g., Effron, 2018; Tetlock, 1998).

As a consequence of the widespread interest that it elicited, the literature on counterfactual thinking has received contributions by researchers coming from disparate psychological subfields and, albeit employing a fairly consistent terminology, their conceptualizations of what should be considered a counterfactual might present some differences. Before discussing the main topic of this work, that is the function of counterfactual thinking, it is then useful to have an overview of this form of mental simulation, providing a clear definition and a summary of its core features.

Definition and Core Features of Counterfactual Thinking

Despite the many ways in which counterfactual thinking may be expressed (e.g., Son et al., 2017), most psychological works on this topic have investigated counterfactuals in the form of “if only…” conditional statements in the subjunctive mood (Byrne, 2005), where both the antecedent and the consequent are factually false (e.g., the sentence “If only we had left earlier, we would not have missed the show" conveys the message that the speaker and her friend did not leave early and missed the show). These kinds of conditionals thus depict an outcome that was (more or less) likely to happen in the past but that did not materialize, and are therefore distinct from semifactual conditionals where we envision that, "even if…" a different antecedent condition had held, the outcome would not have changed (e.g., "Even if had left earlier, we would have still missed the show"; McCloy & Byrne, 2002).

When considering the core features of counterfactual thoughts, there are three aspects that are typically discussed: What factors elicit them, along what dimensions they can be categorized, and the content they typically present (e.g., Byrne, 2016; Roese, 1997; Roese & Epstude, 2017).

Regarding the determinants of counterfactual thoughts, research has shown that there are two main factors that promote the production of such simulations. The first one is the experience of *negative affect*, typical of situations in which, for example, we fail to reach a goal or our expectations are frustrated. The greater recruitment of counterfactual thinking following negative affect has been reported by works conducted in both laboratory and real-life settings and that employed a variety of ways to measure the activation of this form of mental simulation, such as self-reported frequency of counterfactual production (e.g., Davis et al., 1995), reaction times to probes formulated in counterfactual terms (e.g., Roese & Olson, 1997, Experiment 5), and information seeking about the outcome of forgone choices (i.e., counterfactual outcomes; e.g., Summerville, 2011). The second factor is *outcome closeness*, that is the perception of how likely it is that an outcome different from the one experienced would have happened. Closeness can refer to both a broadly defined "distance" dimension (which include physical, temporal, or even purely numerical distance) or to a probability dimension (Doan et al., 2023) and, as in the case of negative affect, the relation between outcome closeness and the recruitment of counterfactual thinking has been found using a diverse array of methodologies. Among these, it possible to mention the spontaneous production of counterfactuals in free-response thought-listing tasks (e.g., Meyers-Levy & Maheswaran, 1992), the analysis of verbalizations made by broadcasters of baseball matches (e.g., Sanna et al., 2003), and memory distortions following the presentation of counterfactual-inducing materials (e.g., Smallman et al., 2022, Study 1b). The importance of these two factors in eliciting counterfactual thinking can be understood considering that negative affect typically signals that a problem is present (Schwartz, 2011), highlighting the desired state of things that we failed to achieve, and that, similarly, outcome closeness makes it fairly easy to conclude that an event was very likely to end in a different way (Kahneman & Tversky, 1982b). Counterfactual thoughts would then arise spontaneously from the process of comparing the current situation with the highly salient alternatives active in our minds.

The second aspect that is often considered in works on counterfactual thinking is what might be called the taxonomy of counterfactuals, that is the main dimensions according to which these simulations can be sorted. Typically, four of such dimensions are listed, which are entirely orthogonal.

Direction is a dimension characterizing the consequent part of a counterfactual conditional, and it refers to whether the simulation depicts a better alternative to reality (upward counterfactual; e.g., "If only we had left earlier, we would have found better seats") or a worse one (downward counterfactual; e.g., "If we had left later, we would have missed the show"). It has been found that, on average, people are more prone to produce upward rather than downward counterfactuals (e.g., Özbek et al., 2018; Roese & Hur, 1997) and that these two types of modifications appear to have different effects: while upward counterfactuals, by showing that better outcomes could have been achieved, appear to motivate people to improve the current situation, downward counterfactuals influence mostly the affective state of individuals, improving it by showing that things could have even been worse (Roese, 1994). However, during the years it has emerged that the relation between the direction of counterfactuals and their effects is more complex than what was initially thought, as it depends on factors such as the mode characterizing the comparison between the actual and counterfactual alternatives (Markman et al., 2008).

Two other dimensions, this time pertaining the antecedent part of a counterfactual conditional, are *structure* and *social focus*. Structure refers to whether the counterfactual scenario is simulated by including an element that was not present in the past event (additive counterfactual; e.g., “If only we had been part of the loyalty program, we would have spent less on tickets”) or by removing an element that was present (subtractive counterfactual; e.g., “If we hadn’t got caught in the traffic jam, we would not have missed the show”). As in the case of direction, people seem inclined to focus on one side of the structure dimension when producing counterfactuals, namely the additive side (e.g., Catellani & Covelli, 2013; Petrocelli et al., 2012), even if this tendency appears to depend on the valence of the outcome of the past event (Roese & Olson, 1993b, 1993a). Various works investigated the relation between counterfactual structure on one side and processing styles and regulatory focus on the other (e.g., McCulloch & Smallman, 2014; Roese et al., 1999), and how such relation affects, for example, creativity and problem solving (Markman et al., 2007). However, recent findings have questioned the existence of an influence of counterfactual structure on regulatory focus (Winter & Epstude, 2023), and therefore previous results on this topic should be considered with caution. In addition to this, it should be noted that the distinction between additive and subtractive modification is straightforward when categorical variables are considered, while it is less clear with continuous ones (in general, there seems to be a lack of results about possible differences between categorical versus continuous counterfactual antecedents; for two exceptions, see Warren et al., 2023; Woltin & Epstude, 2023).

Social focus refers to the fact that the element considered in the counterfactual antecedent can either be traced back to the self (self-focused counterfactual; e.g., “If only I had insisted on leaving earlier, we would not have missed the show”) or to some other agents that were present in the event considered (other-focused counterfactual; e.g., “If only you had started getting ready earlier, we would not have missed the show). In Roese and Epstude (2017), this dimension has been suggested to largely overlap with the distinction between internal versus external locus of causation proposed by Weiner (1985). Thus, while the dimension is typically framed as a juxtaposition between *one*self and the other (Epstude & Roese, 2008; Roese & Epstude, 2017), it may be possible to consider “self-focused counterfactuals” all those modifications pertaining to the protagonist of an event, be it us or someone else. Also in this case, some results suggest that people engage more frequently in self-focused rather than other-focused counterfactual thinking (e.g., Davis et al., 1995; Nasco & Marsh, 1999), but this tendency appears to be influenced by various factors, for example one's contextual propensity to protect her self-esteem (e.g., McCrea, 2007). It is not a case, then, that social focus has often been considered in studies investigating how counterfactual thinking is related to self-serving purposes and justification strategies (e.g., Catellani & Bertolotti, 2014; Goerke et al., 2004).

Finally, the *episodic* dimension of a counterfactual does not regard neither its antecedent nor its consequent, but more in general whether the content of the thought refers to an autobiographical element to which one can assign defined spatiotemporal coordinates (episodic counterfactual; e.g., "If only I had booked earlier, I would have found cheaper tickets for the show") or if it refers to general notions about the world (semantic counterfactual; e.g., "If ticket scalpers were prosecuted more efficiently, people would not struggle to find tickets for the shows"). De Brigard and Parikh (2019) proposed that these two labels should not be considered as indicating mutually excluding categories but, rather, the ends of a continuum, as, when building a counterfactual, we may draw from both episodic and semantic elements to varying degrees. This conceptualization offers a useful way to navigate the number of methodologies through which counterfactuals have been studied and, partly, the discrepancies that emerge once these methodologies are compared with one another. A clear example of this is the difference, in terms of content of the simulations, that is observed when comparing counterfactuals produced about recently experienced events with those referring to hypothetical scenarios, which fall in between fully autobiographical counterfactuals and semantic ones (Girotto et al., 2007; Pighin et al., 2011, 2021).

The third aspect that has attracted the attention of many psychologists is the content of counterfactual thoughts, that is what elements people mutate when simulating how a past event could have been different. This topic is particularly interesting because, potentially, people could create an infinite number of counterfactuals for every event, and yet various results have shown that their imagination appears to be rather constrained (Byrne, 2005; Kahneman & Tversky, 1982b). The literature has organized the findings on this topic in what have been called the four "fault lines" of reality (Byrne, 2016), denoting that certain features of past events tend to be mutated more readily than others.

The first of these mutability tendencies is the *exceptionality effect*. This refers to the fact that people are more likely to simulate a different outcome for a past event by mutating an out-of-the-ordinary element into its normal counterpart, rather than vice versa. A classical scenario used to first investigate this effect (Kahneman & Tversky, 1982b) is that of Mr. Jones, who, while driving back home, is killed in an accident. In one version of this scenario, Mr. Jones leaves the office at the usual time but takes a different route rather than the one he typically drives along; in another version, the route is the usual one, but the time of departure is not. When asked to imagine how things could have been different for Mr. Jones, the majority of participants reading the former version mutated the route taken, while those reading the latter mutated the time at which Mr. Jones left. This effect, that might be driven by a lower cognitive effort to come up with counterexamples to exceptional but not to normal events (Kahneman & Miller, 1986; Kahneman & Tversky, 1982b), has been considered in a multitude of works since its first appearance in the literature (Fillon et al., 2021). However, the exceptionality effect proved to be less straightforward than what was originally thought, as some works failed to find evidence for the effect (McEleney & Byrne, 2006) or showed that certain factors, like outcome optimality, can reverse it when manipulated (Dixon & Byrne, 2011). Some authors even argued that the original findings by Kahneman and Tversky cannot be considered as evidence in favor of the effect. Trabasso and Bartolone (2003) suggested that the tendency to modify the exceptional elements in the scenarios by Kahneman and Tversky was not driven by the abnormality of the elements, but rather by differences, across the "exceptional route" and "exceptional time" scenarios, in whether the story provided explanations for the presence of the normal and abnormal elements, which in turn might have increased the cognitive accessibility of the explained element.

The second mutability tendency is called the *controllability effect*. People prefer to build counterfactual modifications by changing an element of the past event that was under rather than outside the control of the protagonist of the event.[1] The effect was initially found by studies that used vignettes (Girotto et al., 1991), like the one in which Mr. Bianchi is delayed on his way home by the maneuvers of a truck, a tree trunk that fell on the road, and his own decision to stop at a bar for a drink. Once he arrives home, he finds his wife on the floor and realizes that she is having a heart attack, but unfortunately it is too late to save her. Participants asked to list, in order of importance, some ways in which the outcome of the story could have been different tended to indicate the decision to stop at the bar (i.e., the only controllable element of the story) at the top of the list. Girotto and colleagues, building upon an idea proposed in Wells et al. (1987), suggested that the higher mutability of controllable versus uncontrollable features might be related to people perceiving the former to be less constrained by external factors, which would make it easier to imagine them being different (for other results suggesting a privileged role of controllable elements in counterfactual thinking, see also Frosch et al., 2015; Mandel & Lehman, 1996). Also in the case of the controllability effect, later results expanded our comprehension of the phenomenon, for example by exploring possible moderators, such as the perceived appropriateness of a controllable element (McCloy & Byrne, 2000), and by showing that the effect did not apply to all types of counterfactuals. As mentioned above, a series of studies (Girotto et al., 2007; Pighin et al., 2011, 2021) asked participants to either read a story about someone taking part in a task (reader condition) or to take part in the task themselves (actor condition). After reading the story or completing the task (that in both conditions typically ended with a failure), participants were asked to imagine how things could have been better either for the protagonist of the story or for themselves. What emerged was a pattern that has been termed the "actor-reader effect": in line with the controllability effect, participants in the reader condition produced a majority of counterfactuals focused on controllable elements, while those in the actor condition tended to focus on uncontrollable ones. The authors explained these results in terms of possible differences in the richness of participants' representations, since actors, compared to readers,

[1] It should be noted that the controllability of a counterfactual (i.e., controllable vs uncontrollable) does not necessarily overlap with its locus of causation (i.e., internal vs external), as these two dimensions are orthogonal in certain conceptualizations (Roese et al., 2017).

would likely possess more information regarding the structural (uncontrollable) features of the task they tackled, which would then be available to be modified in their counterfactuals.

The third mutability tendency is the *action effect*. This effect describes people as being more inclined to produce counterfactuals about actions rather than inactions (sometimes also referred to as commissions and omissions; e.g., Kahneman, 1995). The works that initially found support for the effect (e.g., Gleicher et al., 1990; Kahneman & Tversky, 1982a) asked participants to consider stories like the one in which George, who owns shares in company A, decides to switch to shares in company B, while Paul, who owns shares in company B, considered switching to shares in company A, but in the end did not follow through his intention. At the end of the year, both of them find out that, had they owned shares in company A, they would have been better off by $1,200. What is typically observed is that people consider Paul, the one who acted, to feel more regret, which is taken as a sign that action-oriented counterfactuals were evaluated. The action effect, that has been suggested to account for results found also in other psychological literatures (e.g., Henne et al., 2019), has been explained by referring to differences in the alternatives that actions and inactions appear to elicit: while the former are likely represented by keeping in mind both the pre- and post-action state of the world, the latter can only include the past state (which is equivalent to the present one), a condition that might favor the perceived mutability of actions compared to inactions (Byrne & McEleney, 2000). As a final remark, a result suggested that the action effect was reversed when considering events from a long-term perspective (Gilovich & Medvec, 1994), but later findings showed that the effect is quite robust, except when mentally undoing inactions could lead to potentially better outcomes in the long run (Byrne & McEleney, 2000).

The last mutability tendency is made up of two distinct but related effects, the *temporal order* and the *causal order effects*. When people produce counterfactuals about sequences of events, they tend to modify the most recent one when the events are independent from one another (or, better, they refrain from modifying the first one; Segura et al., 2002); instead, they mentally undo the first one when the events are connected in a casual chain. Support for the temporal order effect was initially found with scenarios like the one in which Jones and Cooper are playing a coin-toss game where they can each win $1,000 if they both toss a head or a tail. Jones goes first, tossing a head, but then Cooper tosses a tail, so the two do not win anything. In these cases, while mutating the outcome of one of both tosses would result in a win, the majority of participants report that things would have been better if Cooper had tossed a head, and that he will likely feel more guilty for the outcome compared to Jones (Miller & Gunasegaram, 1990). One of the first scenarios showing evidence of the causal order effect, instead, told the story of William's attempt to get to a store located in the opposite part of the town to take advantage of a stereo systems sale. On his way there, William is fined with a speeding ticket, and to make up for the time lost he takes a shortcut, where, unfortunately, a glass on the road punctures one of his tires. Due to the delay, he finds himself stuck in a rush hour traffic jam, and as a result of these misadventures he fails to arrive at the shop in time. It was found that, when considering scenarios like this one, participants preferentially focused on the first element of the sequence when asked to list possible ways in which the event could have been different (Wells et al., 1987). Various explanations have been proposed that referred to how much each event of the sequence changes the probability of the outcome (e.g., Spellman, 1997) or to the kind of possibilities that people consider when reflecting over the sequence of events (e.g., Byrne et al., 2000; Walsh & Byrne, 2004). One hypothesis that seems able to account for both effects, is that, by default, the first event of a sequence is presupposed immutable, but when the sequence is a causal chain such presupposition is discarded, since causes might be understood by simultaneously keeping in mind mental models in which the causes are depicted as present and models in which they are depicted as absent (Segura et al., 2002). Having lost its status of immutability, the first cause would then be selected more frequently as it would be perceived as the least constrained (and thus the most easily mutable) one among those in the causal chain, where the status of a downstream cause is determined by the ones that preceded it (e.g., Miller & Gunasegaram, 1990; Wells et al., 1987). Still, an explanation would be needed for why the first event in a non-causal sequence is treated as immutable by default, since this aspect was not elaborated in Segura et al. (2002).

As it can be seen from this brief overview, the works that during the years investigated the four fault lines showed that these mutability tendencies are moderated by a large number of factors, which, from the theoretical point of view, returns the impression of a certain fragmentation in the literature. Given this, it has emerged the need to propose a more encompassing explanation, able to flexibly account for the results already present and, possibly, to produce novel predictions. To this end, Petrocelli et al. (2011) proposed a probabilistic model that should be able to capture the perceived plausibility a person assigns to a given counterfactual conditional, plausibility that might be modulated by the list of factors that have been uncovered as affecting the mutability tendencies. Interestingly, psychological works investigating the perceived plausibility of counterfactuals have been sparse (e.g., Over et al., 2007, Experiment 3; Tasso & Cherubini, 2007), but in recent years more attention is being devoted to the topic (e.g., De Brigard et al., 2021; López-Astorga et al., 2022; Lucas & Kemp, 2015; Stanley et al., 2017).

As suggested by the list of results reported in this section, a lot of effort has been put into expanding our knowledge on the core features of counterfactual thinking. Another area of research that has been traditionally very active is the one focusing on the function of counterfactual thinking, whose aim is to understand why people engage in this form of mental simulation.

The Function(s) of Counterfactual Thinking

Counterfactual statements can be considered as the tip of the iceberg of a goal-oriented scrutiny of past events which investigates what elements played (or could have played) a crucial role in determining a certain outcome (Byrne, 2016; Roese & Epstude, 2017). The emphasis on *goal-oriented* signals that there is a multitude of reasons for why people may want to mentally undo the past. For example, it has been proposed that counterfactual thinking is employed to ameliorate social relations (e.g., Summerville & Buchanan, 2014), to regulate one's mood (e.g., Branch & Zickar, 2021), or to preserve one's self-esteem (Tyser et al., 2012). Yet, according to the Functional Theory (FT; Roese, 1994; Roese & Epstude, 2017), the dominant theory on the topic, one function is far more central than the others in driving the production of counterfactual thoughts, and that is the *preparatory* function.

FT proposes that, especially after negative events such as a poor performance,[2] people take advantage of counterfactual thinking as a sort of virtual testing ground to explore what actions or strategies would increase their chances of experiencing a better outcome in the future. The proponents of FT distinguish two fundamental, non-mutually excluding ways in which counterfactual thinking can fulfill such preparatory function (Epstude & Roese, 2008, 2011; Roese & Epstude, 2017), the *content-neutral* and the *content-specific* pathways. When counterfactual thinking acts through the content-neutral pathway, it is proposed to exert its influence on future performances by either activating counterfactual mindsets (Galinsky & Moskowitz, 2000), that is particular modalities of information processing whose peculiarities will depend, for example, on the structure of the counterfactuals considered (e.g., Wong et al., 2008); or, more in general, by means of increased motivation, which may follow the realization that things could have been better, consequently resulting in a greater investment of time and resources to obtain that better possible outcome in the future. Independently from how this pathway operates, the central idea is that the effect of a counterfactual acting through it does not depend on its content (i.e., the element on which its antecedent is focused; hence “content-neutral”) but rather on its influence on how we interact with our psychological environment. As a consequence of this, the counterfactual would affect our behavior not just in contexts similar to the ones that prompted it, but also in unrelated domains, and it would not result necessarily in the implementation of the behavior considered in the counterfactual, but more in general of actions that may prove useful for our aims. For example, if after arriving late at a show we thought “If only we had left earlier, we would have arrived on time”, this might lead us, first of all, to consider leaving earlier than usual not only when we have to go to a show, but also when we have a job interview or when we have to catch a plane; additionally, in all those situations,

[2] While the core tenets of FT can potentially apply to both performance-based and decision-based outcomes, the focus of this thesis will be mainly on the former.

that counterfactual might prompt us to consider behaviors that are different from the "leave earlier" one, such as checking on the internet for the least congested route.

The opposite is true when counterfactual thinking operates through the content-specific pathway. In this case, the mental simulation acts at the causal inference level, subjectively assessing to what extent the element considered in the counterfactual antecedent would have been able to bring about the counterfactual outcome. The result of such assessment would then be used to inform intentions that should favor the implementation of the specific behavior considered (hence "content-specific"), and, if the causal inference at the core of the counterfactual was accurate, this would lead to an overall improvement of future performances. Due to its theorized relation with implementation intentions, that rely on the presence of specific contextual cues to operate (Gollwitzer, 1999), the effects of a counterfactual acting through this pathway should be observed in situations that resemble the one in which the thought was produced, and the behavior that it facilitates should be exactly the one on which it focused. Considering the previous example, the next time we will have to leave for a show (and not for a job interview or for catching a plane) we will be more likely to leave earlier (rather than, for example, to check the traffic on the internet).

Complementing the proposal of these two pathways, FT also specifies that the preparatory goal of counterfactual thinking will be most evident in conditions characterized by high levels of opportunity, which generally refers to the perception that one is able to produce changes in a situation of interest and that she will have the chance and the ability to operate such changes. Opportunity is considered a factor moderating the function of counterfactual thinking, since, when opportunity is low, people may engage in this form of mental simulation not to prepare for the future, but rather to improve their mood (i.e., counterfactual thinking would carry out a consolatory function). So, for example, if someone performs poorly in a task that she knows she will face again in the future and in which she is confident she can improve, then her counterfactuals about that poor performance will most probably revolve around possible ways to achieve said improvement. On the other hand, if it is highly unlikely that she will come across that task again or if she is under the impression that nothing can be done to obtain a better outcome in the future, counterfactual thinking aimed at securing an improvement will have little value, and so it might be more fruitfully employed to regulate her mood.

Starting from these elements, three core predictions can be derived from FT. As it will be discussed in the next chapters, while at first these predictions appeared to be backed by empirical results, several findings then emerged that were not in line with them.

Chapter 2 – The Content of Counterfactual Thoughts

According to the "form fits function" principle put forward by FT (Roese & Epstude, 2017), counterfactuals possessing certain characteristics are especially suited to serve a preparatory function. Thus, if the primary function of counterfactual thinking is indeed to prepare for the future, people should be more likely to produce counterfactuals exhibiting those characteristics. For example, counterfactuals that are upward, additive, and self-focused (versus downward, subtractive, and other-focused) are considered especially useful in preparatory terms because they depict something that one has failed to do and that could have led to experience a better outcome. As reported in the first chapter, these three types of counterfactuals are also the ones that, indeed, have been found to be produced more frequently in various studies, a result that, according to the principle just enunciated, lends support to FT. When discussing about the characteristics that counterfactuals should preferentially show, the supporters of FT often stress that modifications focused on controllable elements (i.e., elements that can be personally acted upon) are more useful to pursue preparatory goals compared to counterfactuals focused on uncontrollable elements (Smallman & Summerville, 2018), and thus they predict that, especially after negative events, the former should be observed more frequently than the latter (e.g., Roese et al., 2017; Roese & Epstude, 2017).

Evidence in Favor and Against the Counterfactual Content Prediction

The initial results on the mutability tendency called "controllability effect" appeared to be in line with what was predicted by FT. Indeed, it was observed that, when reflecting on how a hypothetical scenario could have ended differently, people mostly think about things that were under the control of the protagonist. FT's prediction appeared to be supported also by studies investigating counterfactual thinking through the recollection of events personally experienced in the past. For example, Davis et al. (1995) interviewed parents who, in previous weeks or months, had lost a child to Sudden Infant Death Syndrome (SIDS), a condition described as having no known cause and characterized by the sudden death of the infant. Given the peculiarities of SIDS, it would be unreasonable to think about possible actions that the parents could have performed to avoid the child's death, and yet it was found that the majority of parents who, after 18 months, still had thoughts about how things could have been different, focused their attention on their own behavior.

However, the literature lacked studies investigating counterfactual thinking about another class of events, namely those personally experienced in the immediate past, that are clearly relevant to the proposed preparatory function theorized by FT. When two lines of research started to address this knowledge gap, their results were in contrast with findings mentioned previously, and thus with the prediction of FT. First, as reported in the works on the "actor-reader effect", when producing counterfactuals about recently experienced events, people appear to shift their focus on elements outside their control, a tendency that could not be simply explained as an attempt to avoid self-blame (Girotto et al., 2007; Pighin et al., 2011, 2021). The second line of findings that runs counter the prediction made by FT has directly compared the content of prefactual and counterfactual simulations of recently experienced events. Prefactuals, in which we imagine how an event we went through might unfold differently in the future, have been repeatedly shown to refer to controllable elements to a much greater extent compared to counterfactual thoughts (e.g., Branch & Anderson, 2020; Ferrante et al., 2013; Mercier et al., 2017; Stragà & Ferrante, 2014). The focus of prefactuals on controllable elements makes intuitive sense since future-oriented thoughts are considered an important part of goal-oriented cognition (Schacter, 2012), but then it is difficult to reconcile the discrepancy that these two types of mental simulations shows in terms of content with the idea that, for both of them, the main function is to guide future behaviors, as it has been suggested by the proponents of FT (Epstude et al., 2016).

The findings just presented were opposite to what would have been expected based on FT, prompting its supporters to try and reconcile the inconsistencies between the data and the theory. Attempts to account for these findings analyzed the characteristics of the tasks employed in the works that proved problematic for the theory. A first proposal was that the prevalence of uncontrollable counterfactuals observed in those studies could be traced back to a lack of *self-initiation*, which refers to the fact that participants reflected on activities that did not originate entirely from their personal inclinations, but that rather were set in motion by the experimenters, thereby reducing the controllable modifications one could have possibly considered (Roese et al., 2017). However, the available data do not support this hypothesis. In the study that was run to corroborate this idea, Roese and colleagues asked participants to reflect counterfactually on a recollected negative event, a feature that makes their findings not directly comparable with those that they aimed to explain. Indeed, the tendency to focus counterfactual modifications on controllable versus uncontrollable features appears to change when the counterfactuals are produced right after the event in question (as in the case of the "actor-reader effect" and "prefactual-counterfactual asymmetry" studies) versus after a certain amount of time (D. Ferrante, personal communication, February 2, 2023). In line with this argument, Fernandez et al. (2022) showed that manipulating self-initiation levels did not affect the focus of counterfactuals regarding a just concluded anagram task.

A second proposal put forward to account for the prevalence of uncontrollable counterfactuals observed in some studies was that the tasks they employed (e.g., solving complex arithmetical operations or syllogisms) were characterized by a *simple causal domain*, where controllable features able to make a difference to the outcome might have been relatively scarce (Roese & Epstude, 2017). In those cases, when participants were asked to produce a counterfactual, they might have had few controllable targets to consider in their mental simulations, and because of this the number of uncontrollable modifications increased. They thus argue that, probably, one would observe a majority of controllable modifications if the production of counterfactuals is induced in a context characterized by a *complex causal domain*, where there are multiple actions and strategies that participants can implement (and imagine having implemented) to improve their performance. The possibility that the complexity of the causal domain of a task may influence the content of counterfactuals produced by individuals is certainly intriguing and deserves further investigation. Indeed, no study appears to have tested if past results showing a predominance of uncontrollable modifications regarding a recent, personal performance would extend to situations in which tasks characterized by a more complex causal domain are considered.

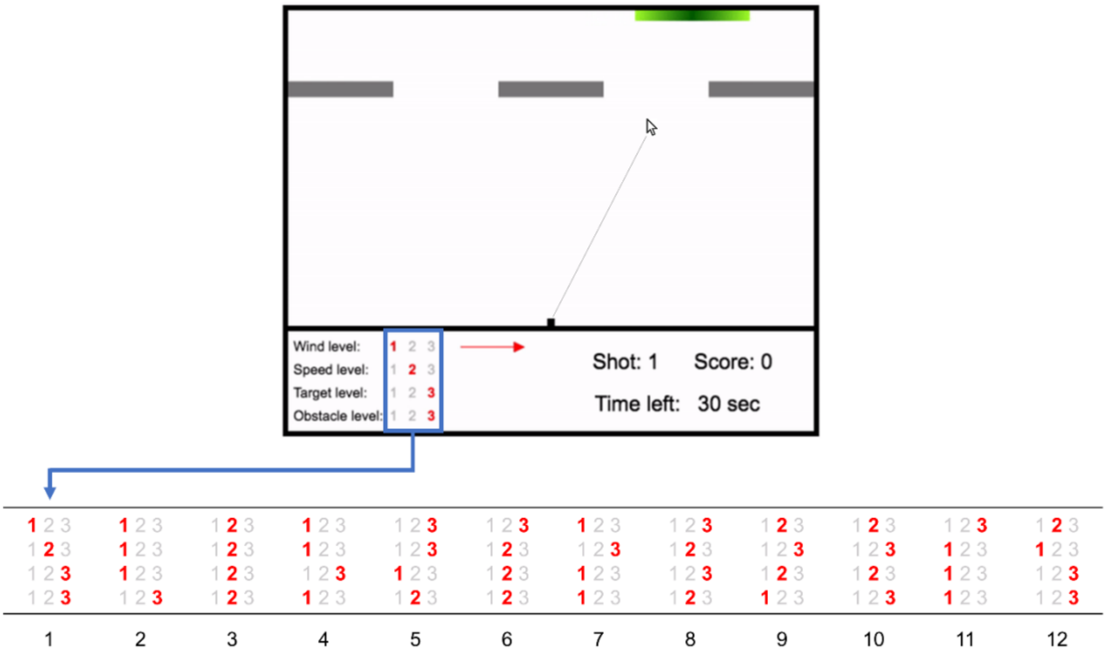
However, besides the complexity of the causal domain of the tasks utilized, it is interesting to note two aspects that, in previous studies, seem to covary with the differences in the proportion of controllable to uncontrollable modifications produced: the difficulty of the task and the presence of negative feedback about one's performance. Indeed, it appears that uncontrollable counterfactuals were produced to a greater extent after participants tackled a very challenging task (such as mentally solving mathematical or reasoning problems under time pressure; e.g., Girotto et al., 2007; Mercier et al., 2017; Pighin et al., 2011) compared to when they dealt with a task that was potentially more accessible or, at least, for which they likely had prepared (such as playing a videogame, taking a test or competing in a marathon; e.g., Hammell & Chan, 2016; Nasco & Marsh, 1999; Stragà & Ferrante, 2014). Similarly, studies making evident to participants that their performance in a task was unsatisfactory (e.g., Ferrante et al., 2013; Girotto et al., 2007) tended to report more uncontrollable modifications compared to studies where feedback, if provided, did not definitively state failure (e.g., Fernandez et al., 2022; Hammell & Chan, 2016; Nasco & Marsh, 1999; Stragà & Ferrante, 2014). Both factors (i.e., the difficulty of the task and the presence of negative feedback) might have hampered participants' abilities to identify strategies to improve their performances and their confidence in the effectiveness of such strategies. Indeed, experiencing a poor performance after a challenging task or receiving negative feedback can adversely affect how individuals evaluate their own performances, for example, by decreasing their perceived self-efficacy (e.g., Dahling & Ruppel, 2016; Escarti & Guzman, 1999). This, in turn, might lead to questioning whether any action that, in principle, could be implemented would have actually made any difference in the outcome, impacting the probability of considering controllable and uncontrollable modifications. However, before discussing possible mechanisms that could mediate the effect of task difficulty and negative feedback on the type of counterfactuals produced, we need to ascertain if these factors do indeed affect participants' modifications in a complex causal domain. We thus run two experiments in which we manipulated task difficulty (Experiment 1) and the presence of negative feedback (Experiment 2) to investigate if, and in what direction, they affect the production of controllable versus uncontrollable counterfactual thoughts. In particular, based on the results present in the literature and in contrast with what is predicted by FT, we expect an increased incidence of uncontrollable counterfactuals when the difficulty of the task increases as well as after receiving negative feedback on performance.

The Experimental Task

As mentioned above, one of the possible issues with the previous studies in the literature is that they used tasks in which there were few controllable features that participants could consider in their counterfactuals (i.e., they offered a simple rather than a complex causal domain), and possibly because of this the modifications tended to be uncontrollable rather than controllable (Roese & Epstude, 2017). Thus, we designed a new task, a target shooting video game, that could offer participants several controllable and uncontrollable elements on which to focus their counterfactuals (Figure 2.1). In this game, participants go through 12 shots, each of which must be completed in maximum 30 seconds. Their goal is to gain as many points as possible by hitting a moving target with a bullet that is aimed and fired using their mouse and keyboard. Scores are computed based on how accurate their shots are, ranging from 0 points, if they completely miss the target, to 10 points, if they hit the bullseye. The total score was the sum of the points obtained in all shots (and it, then, ranged from 0 to a maximum of 120 points). Crucially, to make the causal domain more complex, we included four variables that affect the difficulty of each shot: the speed of the target, the size of the target, the size of the obstacles (blocks that are interposed between the target and the starting position of the bullet) and the strength of the wind (a factor that influences the trajectory of the bullet, to the right or left). Each variable has three levels, 1-3, from easiest to hardest (e.g., the target at speed level 3 was the fastest, the target at size level 3 was the smallest, the obstacles at size level 3 were the biggest, the wind at level 3 produced the biggest deviation of the trajectory of the bullet), yielding 81 (i.e., $=3^4$) possible combinations. The values of all variables were displayed in each trial, allowing participants to play strategically and to better understand the complexity of the causal domain of the game, thus providing a number of potential controllable counterfactual modifications of their performance. A set of 12 shots was selected from the pool of combinations so as to balance the overall difficulty of variables (i.e., by ensuring that each level of difficulty appeared precisely four times for each variable) and to avoid shots with extreme conditions (i.e., by excluding shots with all variables at levels 1 or 3). The order of the selected shots was then randomized and fixed for all participants.

Figure 2.1

Game screen and combinations of variables levels for each of the 12 shots used



Note. The trajectory line connecting the bullet to the mouse pointer was not visible to the participants, but it is presented here for the purpose of illustrating the shot's path. The direction of the wind, which was randomly set to either the right or left at the beginning of each shot, was indicated by the red arrow next to the wind level.

We then generated one easy, one intermediate, and one difficult version of this sequence of shots by adjusting the values of the three levels of each variable. For example, obstacle size at level 1 was 45 pixels in the easy version, 70 pixels in the intermediate version, and 95 pixels in the difficult version. This allowed us to manipulate the difficulty of the game without making it transparent to participants (i.e., although the overall level of difficulty differed across the three versions, the same pattern of variable combinations was displayed in all versions).[3] See Table 2.1 for details about the values of the variables in the three versions of the game. A playable version of the game, together with the datasets and analyses scripts of Experiments 1, 2, and 3 (that will be introduced in the following chapter) can be found at https://osf.io/2q8je/.[4] See Appendix A for the transcript of the instructions and questions presented to participants and for details about the analyses performed.

For each experiment presented in this thesis, we report how the sample size was determined, along with all data exclusions. The statistical analyses were conducted using R (R Core Team, 2019).

Table 2.1
Values of the game variable levels in the three versions of the game

Level	Wind Strength	Target Speed	Target Size (px)	Obstacle Size (px)
Easy version				
1	0.5	1.5	218	45
2	1.25	3	184	60
3	2	4.5	150	75
Intermediate version				
1	2	3.5	177	70
2	3	5	143	85
3	4	6.5	109	100
Difficult version				
1	2.5	7.5	118	95
2	4	9	86	110
3	6.5	10.5	50	125

Experiment 1 – Effect of Task Difficulty on the Type of Counterfactual Produced

Experiment 1 investigated whether the difficulty of the task affects the rate of controllable versus uncontrollable counterfactuals produced about one's performance in the complex causal domain task described above. While FT does not make explicit predictions regarding the effects of task difficulty,

[3] It should be noted that our task bears similarities to the one used in Experiment 1 by Hammell & Chan (2016). However, in their case, the video game was a commercially available product for a gaming console, limiting the authors' ability to closely control various task features that could influence participants' counterfactual modifications.

[4] Note that, in the OSF repository, the numeration of studies is different compared to one used in this manuscript: Experiment 1 is indicated as Study 1a, Experiment 2 as Study 1b, and Experiment 3 as Study 2.

according to it negative outcomes should prompt a process of causal search aimed at identifying controllable elements that might improve future performances. Assuming that negative outcomes increase as the task becomes more difficult, higher rates of controllable counterfactuals should then be expected when difficulty increases. However, previous findings suggest that uncontrollable, rather than controllable, counterfactuals might be more frequent when participants reflect on their performance after having tackled a challenging task. Based on this, we expected an increase of uncontrollable modifications as the task difficulty increased.

Methods

Participants

The minimum sample size for Experiment 1 was estimated through a simulation approach (Brysbaert, 2019) implemented in R. The *a priori* power analysis suggested that a sample of 280 participants should provide 83% power for detecting a medium effect size (log odds ratio = 0.91, corresponding to Cohen's d = 0.5 according to Sánchez-Meca et al., 2003) for the effect of task difficulty. Experiment 1 (as well as all the other Experiments presented in this thesis) was conducted online, by implementing it on SoSci Survey (a platform for developing online experiments accessible at https://www.soscisurvey.de/, Leiner, 2019) and recruiting participants on Prolific (https://prolific.co/, Palan & Schitter, 2018). To account for possible data exclusions, 300 native-English-speaker participants were recruited, ages 18-40, with a Prolific approval rate greater than 90%. Participants (63% females, M_{age} = 27.2, SD_{age} = 6.30) received monetary compensation of £1.20, in line with the payment suggested by Prolific. This experiment was approved by the university's Ethical Review Board.

Materials and Design

In Experiment 1, we used all three versions of the game: easy, intermediate, and difficult. Task difficulty was manipulated between-subjects.

Procedure

Once enrolled, all participants were provided with the same instructions on how to play the game, without mentioning the existence of the different difficulty conditions. Participants were informed that, at the end of the data collection, five participants would be drawn at random, and the player with the highest score would receive a prize of £10. In fact, three lotteries were actually played and honored, one for each condition. After reading the instructions, participants were asked to rate their experience with video games on a 100-point slider, ranging from “very limited” to “very extensive”. They were also asked to rate their pleasure in playing video games on a 100-point slider, ranging from “not at all” to “very much”. Then, participants were randomly assigned to one of the three versions of the task, completed a series of tutorials (in which participants were instructed on how to aim and how the wind affected the shots), and had four practice shots, which were of the same difficulty level of the version of the game they had been assigned to. After this, participants estimated what their total score would be in the actual game by reporting a number between 0 and 120. They also rated their confidence in the accuracy of this estimate on a 100-point slider, ranging from “not confident at all” to “completely confident”. Following this, participants played the game. When the game was over, they rated their performance on a 100-point slider, ranging from “extremely bad” to “extremely good” and produced a counterfactual about how things could have been better. The counterfactual thought was elicited as follows:

> Complete the following sentence with the first thought that comes to your mind:
> “My score would have been **higher than ****, if only…” [** = participant’s score.]

The formulation of the sentence stem was selected to avoid any elements that could bias the answers towards controllable or uncontrollable features of the performance. Finally, participants’ expectations about their improvement in a possible second game were elicited by asking them to predict what their score would be in a hypothetical repetition of the same game. Participants’ demographics were provided by Prolific.

Results

Manipulation Check

Total scores were used as a manipulation check for the difficulty of the task. A Welch's one-way ANOVA was run, and it showed that the total score significantly differed in the three difficulty conditions, $F(2, 181.97) = 301.71$, $p < .001$, $\eta_p^2 = .598$. More specifically, a Games-Howell post-hoc test showed that the three conditions were all significantly different from one another (all *p*s < .001): participants who played the easy version scored higher (*M* = 64.2, *SD* = 18.4) than those who played the intermediate one (*M* = 38.7, *SD* = 20.6), who in turn scored higher than participants who played the difficult version (*M* = 12.5, *SD* = 10.7).

Coding of Counterfactuals

Two independent judges coded the counterfactual thoughts using criteria that were previously employed and documented in the literature (e.g., Ferrante et al., 2013; Hammell & Chan, 2016; Mercier et al., 2017; see also Roese et al., 2017). Agreement between judges was higher than 96%, and the few cases of disagreement were addressed by discussing with two additional judges. Counterfactual thoughts were categorized as *controllable* if they focused on an element over which the participants could have acted during the game or that could have been modified in an immediate repetition of it. Counterfactual thoughts were coded as *uncontrollable* if they referred to aspects of participants' performance over which they could not act or that could be implemented only by extensive practice. A counterfactual modification was coded as *ambiguous* if it concerned an element for which it was not clear that the participant could have acted upon it in an immediate repetition of the game (e.g., managing emotional states) or if the discussion with the two additional judges could not solve a case of disagreement. Finally, participants' answers were coded as *other* if they were not counterfactual thoughts. In line with previous literature, when more than one counterfactual thought was produced, the first one alone was considered and coded accordingly (over the three studies, the percentage of participants who produced more than one counterfactual thought was 4.6%). Table 2.2

reports the full list of counterfactuals produced, together with specific categories examples and frequency, while Table 2.3 reports the frequencies (and corresponding percentages) of counterfactuals according to the controllability criteria presented above (the tables also provide details on Experiment 3, which is presented in Chapter 3).

Table 2.2
Coding, categories, examples, and percentages of the counterfactual thoughts produced (total n = 1,054) in Experiments 1, 2, and 3

Coding	Category	Example	Percentage
Controllable	Time management	I had taken more time before shooting	23.7%
	Attention management	I had paid more attention to the wind direction	8.2%
	Effort management	I had put more effort	4.5%
	More readily understanding of game features	I got a hang of the wind effects quicker	2.0%
	Strategy implementation	I'd waited for the target bar to move in the same direction as the wind	0.8%
Uncontrollable	General ability	I had better hand-eye coordination	22.0%
	Amount/type of practice	I'd had more practice	14.1%
	Game features	There were no obstacles	10.8%
	Restatements	I had hit more targets	7.6%
	Personal skills	I was any good at calculating trajectories	1.3%
Ambiguous	Confusion	I would have been clicking the correct button	1.1%
	External temporary obstacles	I had an external mouse	0.8%
	Emotional states	I'd felt less nervous about playing the game	0.7%
Other	Not counterfactuals	Well, it's better than I predicted	2.3%
	Generic	I had been more tactical	0.1%

Note. The table does not report the data for 23 participants who scored more than 90 points in Experiment 2 nor of one participant who experienced technical problems in Experiment 3 (see main text for details).

Table 2.3
Frequencies (and percentages) of counterfactual thoughts (Cft) produced in each experimental condition of the Experiments 1, 2 and 3, according to controllability/uncontrollability coding

Experiment	Experimental condition	Participants' responses			
		Controllable Cft	Uncontrollable Cft	Ambiguous Cft	Other
1	Easy (*n* = 96)	49 (51%)	38 (40%)	4 (4%)	5 (5%)
	Intermediate (*n* = 105)	41 (39%)	59 (56%)	3 (3%)	2 (2%)
	Difficult (*n* = 99)	17 (17%)	78 (79%)	0 (0%)	4 (4%)
	Overall (*n* = 300)	107 (36%)	175 (58%)	7 (2%)	11 (4%)
2	Feedback-absent (*n* = 190)	82 (43%)	93 (49%)	10 (5%)	5 (3%)
	Feedback-present (*n* = 233)	90 (39%)	129 (55%)	9 (4%)	5 (2%)
	Overall (*n* = 423)	172 (41%)	222 (53%)	19 (4%)	10 (2%)
3	Counterfactual (*n* = 331)	133 (40%)	192 (58%)	3 (1%)	3 (1%)
	No-counterfactual (*n* = 168)	–	–	–	–

Note. The table does not report the data for 23 participants who scored more than 90 points in Experiment 2 nor of one participant who experienced technical problems in Experiment 3 (see main text for details).

The desire to avoid self-blame is sometimes suggested as a possible explanation for the production of uncontrollable counterfactuals (e.g., Elster, 1999; Mercier et al., 2017), especially if characterized by an external locus of control (Weiner, 1985). Indeed, stating that the outcome of an event could have been better if something that was under our control had been different would imply that we have some degree of responsibility for the poor outcome experienced. Thus, focusing on uncontrollable elements may be a valid strategy to avoid self-blame. To explore this possibility, for Experiments 1 and 2, an additional coding of counterfactuals was produced based on their "internal" versus "external" locus. Counterfactual thoughts were coded as *internal* if they referred to an element of the performance that could be traced to the participants (e.g., "if only I had been faster at deciding when to act") and as *external* if they referred to some aspect of the game (e.g., "If only there were no obstacles"). Note that this conceptualization departs from Weiner's theoretical framework, where controllability and locus of control were orthogonal dimensions, since in our case, by definition, all counterfactuals categorized as controllable had an internal locus of control, while uncontrollable counterfactuals could be either internal (e.g., “If only I had better hand-eye coordination”) or external (e.g., “If only the instructions were less confusing”). Finally, counterfactuals whose coding as internal or external was unclear were counted as *indeterminate* and excluded from the analyses of the locus of control. Table 2.4 reports the frequencies (and corresponding percentages) of uncontrollable counterfactuals according to the locus of control criteria.

Table 2.4
Frequencies (and percentages) of uncontrollable counterfactual thoughts produced in each experimental condition of Experiments 1 and 2, according to locus-of-control coding

Experiment	Experimental condition	Locus of control		
		Internal	External	Indeterminate
1	Easy (*n* = 38)	22 (58%)	13 (34%)	3 (8%)
	Intermediate (*n* = 59)	25 (42%)	33 (56%)	1 (2%)
	Difficult (*n* = 78)	35 (45%)	41 (53%)	2 (3%)
	Overall (*n* = 175)	82 (47%)	87 (50%)	6 (3%)
2	Feedback-absent (*n* = 93)	39 (42%)	40 (43%)	14 (15%)
	Feedback-present (*n* = 129)	72 (56%)	50 (39%)	7 (5%)
	Overall (*n* = 222)	111 (50%)	90 (41%)	21 (9%)

Note. The table does not report data for 11 participants who scored more than 90 points in Experiment 2 (see main article for details).

Effect of Task Difficulty on the Type of Counterfactual Produced

Participants’ counterfactuals about how their performance could have been better tended to focus, overall, on uncontrollable rather than controllable elements (58% versus 36%, respectively; $p < .001$, according to a binomial test). Eighteen participants produced either ambiguous counterfactual thoughts or none and were thus excluded from the following analyses. The final sample comprised 282 participants (62% female, M_{age} = 27.1, SD_{age} = 6.29).

As a first test of the effect of task difficulty, a Cochran-Armitage trend test was performed. The results showed that the proportion of uncontrollable counterfactuals significantly increased from the Easy (44%), to the Intermediate (59%), and to the Difficult condition (82%), $Z(3) = -5.36$, $p < .001$. Then, a logistic regression was run to determine the effect of task difficulty on the type of counterfactual produced, while controlling for a number of covariates. These were: experience in playing video games, pleasure in playing video games, mismatch with expected performance (i.e., difference between total score and pre-game estimate), confidence in the pre-game estimate, performance rating, expected improvement (i.e., difference between predicted total score in a possible second game and total score), age, and sex. All continuous variables were standardized before the analysis.

The logistic regression model including task difficulty turned out to be significantly better than the baseline model (which included only the covariates), $\chi^2(2) = 16.68$, $p < .001$, Nagelkerke R^2 = 0.20 (see Table A1 in Appendix A for further details). A comparison among the three experimental conditions (with Bonferroni correction) showed no significant difference in the type of counterfactual produced between easy and intermediate conditions (*OR* = 1.82, *p* = .224, 95% *CI* 0.95 to 3.55), though the difference was in the expected direction: a larger proportion of uncontrollable counterfactuals in the intermediate condition. Uncontrollable counterfactuals were significantly more frequent in the difficult condition compared to both the easy (*OR* = 5.27, *p* < .001, 95% *CI* 2.32 to 12.46) and the intermediate (*OR* = 2.89, *p* = .012, 95% *CI* 1.42 to 6.08) conditions. In addition, confidence in the pre-game estimate of total score (*OR* = 0.70, *p* = .026, 95% *CI* 0.51 to 0.96) and expected improvement (*OR* = 0.73, *p* = .034, 95% *CI* 0.54 to 0.97) were significantly associated with the type of counterfactual produced: the more participants were confident in their estimate and the more they expected to improve in a possible second game, the less likely they were to produce uncontrollable modifications.[5] The results regarding the experimental condition did not change also when considering a model that included only the experimental condition as a predictor, without any covariate.

With regards to the locus of control coding, six participants produced uncontrollable counterfactuals that were indeterminate with respect to the locus of control, and they were thus excluded from the following analysis, resulting in a sample of 276 participants (see Table 2.4 for the frequencies of internally and externally focused uncontrollable counterfactuals). A binomial test indicated that, among uncontrollable counterfactuals, internal and external ones were equally frequent, p = .758, a result that received moderate support from a Bayesian version of the analysis (BF_{10} = 0.20). Then, to examine whether the locus of control varied depending on the difficulty of the task, a chi-square test was run to compare the distribution of internally and externally focused uncontrollable counterfactuals among the three experimental conditions. The test showed no significant difference, $\chi^2(2) = 3.75$, p = .154, Cramer's V = .15, and a Bayes Factor analysis indicated that there was weak to moderate support for the null hypothesis (BF_{10} = 0.32).

Discussion

The results of Experiment 1 show an overall prevalence of uncontrollable counterfactuals over controllable ones in the context of a task in which both controllable and uncontrollable modifications were available, as indicated by the balanced proportion of these two types of modifications observed in the Easy condition. Importantly, task difficulty proved to be a significant factor in determining the type of counterfactual produced in that increased difficulty elicits more uncontrollable modifications. Thus, differences in task difficulty might have influenced the prevalence of uncontrollable counterfactuals reported in some of the previous studies. The results also showed that, in all experimental conditions, uncontrollable counterfactuals were equally distributed between internally

[5] However, the results concerning these two covariates should be interpreted with caution, as in their case the linearity assumption for the logistic regression appeared to be violated.

and externally focused thoughts. If the attempt to avoid self-blame were the primary reason behind the production of uncontrollable counterfactuals, we would have expected to observe a majority of externally focused modifications, as those are likely the most suitable to protect self-esteem. Therefore, our data suggest that the production of uncontrollable counterfactuals is not motivated solely or even mainly by the avoidance of self-blame.

Overall, the current findings seem to be at odds with what would be reasonable to expect according to FT. From that perspective, more challenging tasks should result in poorer performance, making the motivation to improve more salient and thus leading to greater consideration of controllable counterfactuals. Conversely, we observed an increased generation of uncontrollable modifications in these situations. This finding cannot be solely justified as the result of an attempt to avoid self-blame and thus necessitates alternative explanations. One possibility, which will be discussed in more detail in Chapter 5, is that highly challenging tasks may diminish individuals' perception of their capability to improve their performance, reducing the plausibility of counterfactuals centered on actions or strategies they could have employed to achieve a better outcome.

Experiment 2 – Effect of Negative Feedback on the Type of Counterfactual Produced

Experiment 2 explored whether negative feedback regarding performance affects the production of controllable versus uncontrollable counterfactual modifications in a task characterized by a complex causal domain. As with task difficulty, FT does not make explicit predictions about the role of negative feedback. However, negative feedback is likely to elicit negative affect, which, according to FT, generally triggers a process of causal search directed towards exploring ways to prepare for the future. From this perspective, it would be reasonable to anticipate that receiving negative feedback about one's performance should lead to a higher frequency of controllable modifications. However, previous findings (e.g., Ferrante et al., 2013; Girotto et al., 2007) suggest that, when negative feedback is provided, uncontrollable counterfactuals are more frequently produced. Considering this, in the absence of negative feedback, we expected to observe the same direct relation between the performance and type of counterfactual found in Experiment 1, with a heightened production of uncontrollable modifications being related to lower scores. However, the presence of negative feedback could modulate the aforementioned relationship. More specifically, participants who scored poorly might not be significantly affected by negative feedback, as their low score may lead them to interpret the feedback as a confirmation of their negative performance. Thus, in such cases, we expected a prevalence of uncontrollable modifications. On the other hand, negative feedback could negatively influence the self-evaluation of participants who obtained a good (or relatively good) score in the game, as this feedback would likely conflict with their own performance evaluation. This might prompt them to question whether any action they could have taken would have made a difference in the outcome, leading to an overall increased production of uncontrollable modifications compared to a situation in which they did not receive such feedback.

Methods

Participants

The required sample size was estimated through the same simulation approach used for Experiment 1. For this study, given that the effect of interest was an interaction and that detecting interactions might require larger samples compared to main effects (e.g., da Silva Frost & Ledgerwood, 2020), the sample size needed to detect a small-to-medium effect (i.e., log odds ratio = 0.64, corresponding to Cohen's $d = 0.35$) was computed. The *a priori* power analysis suggested that a sample of 380 participants should provide 83% power for detecting the interaction effect. A sample of 446 participants (54% female;[6] $M_{\text{age}} = 28.8$, $SD_{\text{age}} = 6.27$) was collected to account for possible data exclusions. Inclusion criteria and participants' compensation were the same as in Experiment 1. This experiment was approved by the university's Ethical Review Board.

[6] Two participants did not disclose information about sex and were excluded from the analyses using sex as a covariate.

Materials and Design

In this experiment, we used only the easy version of the game. Indeed, since we were interested in the moderating effect of negative feedback on the relation between performance and type of counterfactual, we needed an appreciable number of relatively good and relatively bad performances. In Experiment 1, the easy version yielded the most balanced distribution in this regard, with 64% of participants scoring more than 60 points, the midpoint of the range of possible scores. The presentation of negative feedback was manipulated between-subjects by randomly assigning participants to either the feedback-present or the feedback-absent conditions.

Procedure

Experiment 2 followed a similar procedure to that of Experiment 1, but with one key difference. Upon completing the game, participants were randomly assigned to either the feedback-absent or feedback-present conditions. The procedure for the feedback-absent condition was identical to that of Experiment 1. In contrast, the feedback-present condition involved showing participants, after the game, a message that read as follows:

Your overall score is ****** points. [** = participant's score.]
This score is **lower than the average score** obtained by the players that have already completed the game.
72% of the other players **scored better than you**.

(for similar bogus negative feedback, see Roese, 1994, Experiment 3). In both conditions, a waiting screen after the end of the game was added to increase the plausibility that – for participants in the feedback-present condition – their score was being compared to that of the other players. All participants who obtained a score greater than 90 (the value precisely below the top-scoring 5% of easy-version participants in Experiment 1) were excluded from the analyses because those in the feedback-present condition might have questioned the credibility of the feedback. In total, 10 participants were excluded from the feedback-absent condition, and 13 from the feedback-present condition. By excluding these top players in both groups, we kept the two groups comparable in terms of performance. At the conclusion of the experiment, participants who received negative feedback were fully informed about the deception and offered the opportunity to withdraw their data from the study without any negative consequences to their compensation. Only one participant requested to have their data removed.

Results

Manipulation Check

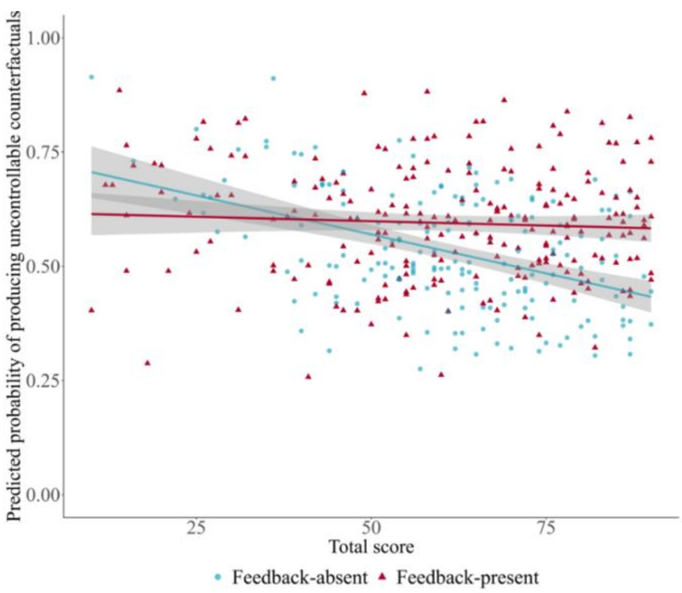
Post-game performance ratings were used to check the efficacy of the feedback manipulation. A multiple regression indicated that the experimental condition was a significant predictor of performance ratings (β = -0.67, *SE* = 0.08, $p < .001$), even when controlling for the total score, $F(2, 420) = 109.3$, $p < .001$, adjusted $R^2 = 0.34$. This showed that, independently from their score, participants who received negative feedback tended to evaluate their performance more negatively (*M* = 29.4, *SD* = 21.4) compared to those that did not (*M* = 43.9, *SD* = 20.8).

Effect of Negative Feedback on the Type of Counterfactual Produced

Overall, participants produced more uncontrollable rather than controllable counterfactuals (53% versus 41%, respectively; $p = .013$ according to a binomial test; see Table 2.3). In addition to the aforementioned exclusions, 29 participants who produced ambiguous or no counterfactuals were excluded from the analyses that follow. The final sample thus consisted of 394 participants (57% females, $M_{\text{age}} = 28.9$, $SD_{\text{age}} = 6.34$).

A logistic regression on the type of counterfactual produced (controllable vs. uncontrollable) was then run to assess the role of feedback as a possible determinant of the content of counterfactual. The model included as predictors experimental condition (feedback-absent vs. feedback-present), total score, and their interaction, along with the same set of covariates used in Experiment 1 (see Figure 2.2 and Table A2). The model including the interaction term was not significantly better than the one without it, $\chi^2(1) = 0.82$, $p = .365$, Nagelkerke $R^2 = 0.10$, as also shown by the interaction term failing to reach significance (*OR* = 1.23, $p = .366$, 95% *CI* 0.78 to 1.95). Additionally, computing the Bayes Factor by means of the BIC approximation method (Raftery, 1995; Wagenmakers, 2007) confirmed this result, showing positive evidence (BF_{10} = 0.08) for the model without the interaction term, compared to the one including it. Similarly, the experimental condition was not a significant predictor (*OR* = 1.41, *p* = .144, 95% *CI* 0.89 to 2.26). Nevertheless, the total score predicted the type of counterfactual in the feedback-absent condition, with lower scores corresponding to a higher probability of producing uncontrollable counterfactuals (*OR* = 0.64, *p* = .037, 95% *CI* 0.42 to 0.97), whereas in the feedback-present condition, the effect of the score failed to reach significance (*OR* = 0.79, *p* = .163, 95% *CI* 0.57 to 1.09).[7] Concerning the covariates, as in Experiment 1, confidence in the pre-game estimate of the total score (*OR* = 0.77, *p* = .039, 95% *CI* 0.60 to 0.99) and the expected improvement in a possible second game (*OR* = 0.73, *p* = .008, 95% *CI* 0.57 to 0.92) were significantly associated with the type of counterfactual produced. In this study, sex was also a significant covariate (*OR* = 2.15, *p* = .003, 95% *CI* 1.30 to 3.62), with males being more likely to produce uncontrollable modifications than females.

Figure 2.2
Predicted probability of producing uncontrollable counterfactuals based on feedback condition and total score



Note. Predicted probability of producing an uncontrollable counterfactual in the feedback-absent and feedback-present conditions, depending on the total score. Grey areas surrounding the regression lines represent 95% confidence intervals.

[7] The same analysis was repeated excluding participants who scored > 85 points (the value precisely below the top-scoring 10% of easy-version participants in Experiment 1) and the results did not change (see Table A3).

Considering the locus of control, 21 counterfactuals were coded as indeterminate and therefore excluded from the following analysis, resulting in a sample of 373 participants (see Table 2.4 for the frequencies of internally and externally focused uncontrollable counterfactuals). As in Experiment 1, a binomial test was run on the proportion of internally versus externally focused uncontrollable counterfactuals, which was not significant, $p = .158$, with a Bayesian analysis showing that there was only weak evidence in favor of the null hypothesis ($BF_{10} = 0.50$). This result was further explored by running a chi-square test on the distribution of internally and externally focused uncontrollable counterfactuals between the two experimental conditions to check whether the presence or absence of feedback had any effect on the locus of control. Once again, the test was not significant, $\chi^2(1) = 1.81$, $p = .179$, Cramer's $V = .09$, and a Bayesian analysis showed weak support for the null hypothesis ($BF_{10} = 0.44$).

Discussion

As it was observed in Experiment 1, participants again produced more uncontrollable than controllable modifications in a task featuring a complex causal domain. Additionally, in line with the effect of difficulty found in Experiment 1, in Experiment 2 participants who scored poorly (and therefore who presumably found the task difficult) and who did not receive negative feedback tended to produce more uncontrollable modifications. Contrary to what was expected, however, the receipt of negative feedback exerted only a very limited influence on the type of modifications produced, partially attenuating the relationship between total score and the production of uncontrollable counterfactuals. This might be due to the specific type of feedback used, which was a comparison of the player's performance to that of other players, or to the fact that all participants, even those in the feedback-absent condition, knew their total scores, which therefore might have acted as general feedback on performance.

Regarding the covariates, the tendency to observe more controllable modifications as the expected improvement increases appears in line with the idea of perceived opportunity proposed by FT: the more one thinks she can improve her performance in the future, the more the preparatory function of counterfactuals will be salient, resulting in a heightened production of controllable modifications. However, other interpretations may equally account for the data, as it will be proposed in the last chapter of the thesis. The observed significant effect of confidence in the pre-game estimate is, on the other hand, less easily explained based on existing literature on counterfactual thinking. One possibility is that this confidence might be associated with individual characteristics, such as personality traits, which also influence the type of counterfactuals produced. Nevertheless, interpreting this finding within such terms is challenging, given the limited research on counterfactual thinking and individual differences (for two exceptions, see Bacon et al., 2013, 2020). Finally, sex emerged as having a significant effect on the content of counterfactual thoughts, with uncontrollable modifications being produced more likely by males as compared to females. While, as in the case of individual characteristics, also the literature on sex differences in counterfactual thinking is extremely scarce, there is at least one work that showed how the counterfactuals produced by males and females about specific topics present some differences in terms of content (Roese et al., 2006). Future studies might try to explore more systematically sex differences in terms of counterfactual content. Additionally, as in Experiment 1, the results do not show any prevalence of externally focused uncontrollable modifications, further supporting the idea that the desire to avoid self-blame might not be the main reason behind the production of uncontrollable counterfactuals.

Overall, the results of Experiment 2 also do not appear to conform to the predictions that could be derived from FT. According to the theory, the presence of negative affect should make improvement goals more prominent, consequently increasing the consideration of controllable counterfactuals. However, our findings challenge this notion. First, in the absence of negative feedback, a poorer performance was linked to an increased generation of uncontrollable modifications, even when controllable ones were available. Second, the presence of negative feedback, despite significantly and adversely impacting participants' performance ratings, did not result in a higher incidence of controllable counterfactuals. In fact, participants were more inclined to produce uncontrollable rather than controllable modifications, a tendency that again cannot be solely attributed to self-blame avoidance.

General Discussion

Experiment 1 showed that greater difficulty of the task favors the production of uncontrollable modifications, while the results of Experiment 2 did not allow definitive conclusions regarding the effect of feedback, even if they did suggest that the receipt of negative feedback may somehow favor the production of uncontrollable modifications by partially moderating the effect of the quality of one's performance.

Still, the results of both experiments suggest that, even when considering situations characterized by complex causal domains and by the experience of negative affect, counterfactual thoughts about events that were just experienced tend to focus on uncontrollable rather than controllable elements, contrary to what would be expected based on FT. Importantly, these findings cannot be explained away as mere artifacts due to a lack of controllable elements to be considered in our task. Especially in Experiment 1, indeed, the proportions of controllable to uncontrollable counterfactuals varied within the same experimental paradigm, providing convincing evidence that the task was characterized by a complex causal domain (i.e., it offered a good number of controllable aspects people could focus their attention on when producing a counterfactual).

As our experiments suggest, part of the cross-task heterogeneity in the proportions of controllable to uncontrollable modifications observed in previous works may be traced back to differences in the difficulty of the task tackled. Based on this finding, future studies may try to uncover the mechanisms driving this effect. One possibility is that dealing with a difficult task is likely to result in a decrease of perceived self-efficacy (e.g., Bandura, 1977; Dahling & Ruppel, 2016; Escarti & Guzman, 1999; Stevens et al., 2012). This, in turn, might lead participants to question whether any action that, in principle, they could have implemented would have actually made any difference, determining a reduction of the plausibility of controllable counterfactual modifications, and, consequently, of their production. This idea certainly resonates with the concept of perceived opportunity proposed by FT, and one might then argue that our results simply reiterate the point made by the theory: Counterfactuals will be preparatory (i.e., focused on controllable elements) only to the extent that people perceive to have a chance to improve their performance. Yet, there is a crucial difference that distinguishes our view from that of FT. The theory proposes that the main function of counterfactual thinking is the preparatory one, and thus asking people to produce a counterfactual in contexts characterized by low perceived opportunity (e.g., when self-efficacy is low) would be like asking them a question that has no real answer (i.e., "What could I have done to improve my performance?"), to which they then reply with the production of uncontrollable modifications because they lack better alternatives. However, as it will be argued in the last chapter of this thesis, our hypothesis is that the question motivating the production of counterfactuals is not about what we could have done to achieve a better outcome, but rather about what factor had the most impact in producing a certain result rather than a different one. Under this view, in situations characterized by low perceived opportunity, certain modifications (i.e., uncontrollable ones) would be considered by participants not because the experimenters' request is odd, but because they appear as more plausible than others.

Chapter 3 – Effects of Counterfactuals in Contexts of Repeated Performances

Another crucial area of research for FT regards the effects that engaging in counterfactual thinking should have over future performances. As mentioned in Chapter 1, FT postulates two main mechanisms through which counterfactuals prepare people for the future, the content-neutral and the content-specific pathway. Broadly speaking, thanks to motivational effects and changes at the mindset level that the content-neutral pathway is theorized to induce, it should be observed that people reflecting counterfactually on a past performance will tend to perform better in the future compared to people who did not engage in counterfactual thinking. When a counterfactual acts through the content-specific pathway, instead, one is expected to observe an increased uptake of the action considered in the thought, following the idea that this kind of simulation serves as an input for intentions to perform said action, which may then lead to performance improvement.

Evidence in Favor and Against the Beneficial Effect of Counterfactuals on Performances

As seen for the content of counterfactual thoughts, FT's predictions regarding their impact on future performances have also received mixed support. For example, in line with the idea of a content-neutral pathway, engaging in counterfactual thinking has been found to improve performances in a variety of tasks, such as the solution of anagrams (Maloney & Egan, 2017), analytical reasoning quizzes (Fernandez et al., 2022), and games requiring coordination skills (Hammell & Chan, 2016). However, these beneficial effects have not always been observed (e.g., Branch & Anderson, 2020; Summerville et al., 2019). In some cases, the active consideration of counterfactual simulations has even been detrimental, a result that, depending on the task considered, authors explained in terms of counterfactuals inducing memory distortions (Petrocelli et al., 2013; Petrocelli & Crysel, 2009), misplaced perceptions of competence (McCrea, 2008; Petrocelli et al., 2012), and devaluation of information provided by others (Ditrich et al., 2019). Evidence is mixed even when considering the relative advantage that controllable modifications, better suited to fulfill a preparatory goal, should offer compared to uncontrollable ones in terms of general performance benefits. Indeed, in Experiment 2 by Maloney & Egan (2017), participants first had to take part in an anagram-solving task, and then half of them were asked to produce counterfactuals about their performance while the others had to complete a filler task. The authors reported not only that participants in the counterfactual condition improved more than those in the control condition in a subsequent (unanticipated) repetition of the task, but also that participants whose first produced counterfactual was controllable improved more than those who produced an uncontrollable thought as their first one. However, this result was not observed in Experiment 1 by Fernandez et al. (2022). Also in that case, participants had to complete an anagram task and then, before moving to a foretold second round of the task, it was manipulated whether they had to produce counterfactuals about their performance. The authors found no difference, in terms of performance improvement, between participants who produced counterfactuals about internally controllable elements and participants who focused on non-internally controllable aspects of their first performance (however, it is important to note that the coding of counterfactual thoughts, and thus the results, were not perfectly comparable between the two studies, as in Fernandez et al., 2022, uncontrollable and externally controllable modifications were grouped together). Overall, the picture surrounding the content-neutral pathway has become more intricate, and there are now indications that a re-examination and a clearer definition of said pathway may be required (Winter & Epstude, 2023). Turning to the content-specific pathway, a number of studies provided evidence that considering an action when reflecting counterfactually on a past event leads to stronger intentions to purse that action in the future (e.g., Smallman & Roese, 2009; Stanley et al., 2021; Walker et al., 2016). However, works that investigated whether counterfactual thoughts increase the actual uptake of the behaviors imagined are vanishingly few, and, moreover, they present conflicting results. For example, in a study by Roese (1994), participants who generated counterfactuals regarding using more cues to solve an anagram task subsequently increased their use of these cues. However, in Myers et al. (2014), counterfactuals about dedicating more time to a word-completion task were not associated with a subsequent increase in time spent on the task.

Also in this case, the proponents of FT suggested that the aforementioned inconsistencies may be due, at least in part, to some peculiar characteristics of the tasks employed in certain studies, rather than to issues in the theoretical framework of FT (Roese & Epstude, 2017). First, along the same lines of what has been claimed about the production of uncontrollable counterfactuals, some of those tasks might have presented very few actions or strategies that participants could have implemented to improve their performance (i.e., they were characterized by a simple causal domain). This scarcity would have made it less likely to spot such controllable elements and put them to use through counterfactual thinking, or, paired with the request to produce a counterfactual about how they could have obtained a better result, it might have confused participants. Indeed, it is possible that some of them had already correctly identified and used one of such few strategies when carrying out the task, but then, after being asked what they could have done better, they might have erroneously dismissed that strategy as ineffective and failed to find a substitute one. This would have led to the misguided impression that counterfactual thinking worsened participants' performance. Related to this point, it is worth mentioning that some tasks utilized in previous studies could be likened to pass-fail evaluations, lacking the capability to facilitate incremental performance enhancements (e.g., Petrocelli et al., 2013). As a result of these features, these tasks may not have been sensitive enough to detect more nuanced or gradual changes in performance resulting from counterfactual thinking. Overall, these criticisms are certainly justified for studies that employed tasks with limited implementable actions for optimizing performance (e.g., Petrocelli et al., 2016; Petrocelli & Harris, 2011). However, it cannot account for other results obtained in studies that investigated contexts, such as academic performance, where participants had a wide range of controllable elements to consider (e.g., Petrocelli et al., 2012; Summerville et al., 2019). A second point noted by the proponents of FT that may offer an insight on the observed inconsistencies is that perceiving a high opportunity to improve in a task, both in terms of capability and possibility to tackle the task again, should favor the use of counterfactuals in a preparatory fashion. Even if some results support this idea (for a summary, see Roese & Epstude, 2017), no study appears to have specifically explored whether the beneficial effects of counterfactuals, for example in terms of performance improvement, are observed predominantly among people who perceive high opportunity. Thus, the validity of perceived opportunity as a moderator of the preparatory value of counterfactuals has yet to be properly tested.

It is evident that further work is required to better comprehend if and to what extent counterfactual thinking impacts future performances, both in term of content-neutral and content-specific effects. To this end, Experiment 3 was run. We employed the same task used in Experiments 1 and 2, which addresses the criticisms raised about previous empirical tasks. Indeed, the task can be used in tightly controlled experimental settings and it features a complex causal domain, also enabling the observation of at least a small performance improvement even in an immediate repetition of it, a crucial component of an experimental paradigm that unfolds within a short timeframe. Additionally, the task allows for the objective measurement and comparison of participants' expectations of performance improvement with their actual performances in subsequent iterations of the task, which is a useful feature to test the moderating role of perceived opportunity. We believe that none of the tasks previously used in the literature on counterfactual thinking presented all these features at once. Hence, in Experiment 3 the potential beneficial effects of counterfactual thinking on future performances are investigated using a new, compelling, empirical paradigm.

Experiment 3 – Effects of Counterfactuals on Repeated Performances

In Experiment 3, the effect of counterfactuals via the content-neutral pathway was explored by examining whether the explicit production of a counterfactual on performance led to score improvement in a subsequent round of the same task. It was also investigated whether controllable modifications fostered performance improvement more than uncontrollable ones. The effect of counterfactuals via the content-specific pathway was explored by testing whether controllable counterfactuals increased the probability of enacting those specific modifications and, if so, whether this was associated with score improvement. To this end, we focused on counterfactuals about not rushing one's shots. These modifications were the only ones whose possible implementation we could objectively measure within our task, differently from, for instance, those about putting more effort or paying more attention to specific aspects of the game. However, coincidentally, counterfactuals about the shooting time were also by far the most frequent (62%) among the controllable counterfactuals produced in Experiments 1 and 2. Thus, the content-specific pathway was tested by investigating whether participants who produced such counterfactuals increased their shooting time in a second round of the game and whether this favored performance improvement.

Method

Participants

The minimum sample size for Experiment 3 was computed to detect an effect of the experimental condition (i.e., counterfactual vs. no counterfactual) over performance improvement. Given the mixed results in the literature for the benefits of counterfactuals on repeated performances, the sample size needed to detect a small effect ($f^2 = 0.02$) with 80% power was computed. The suggested sample was 485 participants. A sample of 500 participants was collected (47% female;[8] $M_{age} = 28.6$, $SD_{age} = 5.83$) to account for possible data exclusions. Inclusion criteria were identical to those of Experiments 1 and 2, and the monetary compensation for participation was £1.25. This experiment was approved by the university's Ethical Review Board.

Materials and Design

For the aims of Experiment 3, it was crucial to ensure that performance could be improved in a second execution of the task. Since a pilot study showed that participants consistently improved in an immediate second iteration of the game when playing the intermediate version, we employed this version in Experiment 3 (see Appendix A for further details about the pilot study). The production of counterfactual thoughts was manipulated, between-subjects, by assigning participants either to the counterfactual condition, in which they were asked to generate a counterfactual about their

[8] One participant did not disclose information about sex and was excluded from the analyses using sex as a covariate.

performance with the same procedure used in Experiments 1 and 2, or to the no-counterfactual condition, in which they went on without being asked to reflect counterfactually about their performances (for a similar control condition, see Roese, 1994, Experiment 3). Importantly, because about half (i.e., 56%) of participants who played the intermediate version of the game in Experiment 1 produced uncontrollable counterfactuals, in Experiment 3 we randomly assigned participants to the no-counterfactual and counterfactual conditions in a ratio of 1:2, in an attempt to obtain three roughly balanced groups of participants producing controllable counterfactuals, uncontrollable counterfactuals, and no counterfactuals.

Procedure

The procedure was similar to that of Experiments 1 and 2, but with some important differences. First, participants were informed from the beginning that they would play the exact same target-shooting video game twice. To keep them motivated in both games, two separate lotteries were drawn, one for each round of the game. Second, as previous studies suggested that negative mood might be a variable modulating the beneficial effects of counterfactuals on performance improvement (e.g., Myers et al., 2014), a mood assessment was included in between the two rounds of the game. More specifically, after the production of the counterfactual or after the performance rating in the no-counterfactual condition, participants were asked to produce five evaluations (order randomized) about how they felt in light of their performance: happy (on a 100-point slider, ranging from "unhappy" to "happy"), regretful, ashamed, disappointed, and angry (each on a 100-point slider, ranging from "not at all" to "very much").[9] Finally, to test the prediction of FT regarding the perceived opportunity for improvement, before the second round, participants were asked to estimate their total

---

[9] A reviewer rightfully pointed out that asking participants to rate regret, shame, and disappointment, that is emotions closely related to counterfactual thinking, might have favored the activation of this type of mental simulation. This would represent a problem especially in the control condition, in which individuals were not supposed to engage in counterfactual thoughts, and it is an aspect that should be explored in future works. However, it is worth noticing that, in Myers et al. (2014), the presence of similar ratings did not hinder the observation of results in line with the content-neutral pathway (namely, a greater accuracy in an anagram task by individuals who were, versus were not, explicitly asked to produce a counterfactual about a previous performance in the task). This suggests that, while it is important to consider the impact that such ratings might have had on our results, their presence does not necessarily preclude the possibility to observe the content-neutral effect of counterfactual thinking.

score in the round about to begin (rather than for a hypothetical future round as in Experiments 1 and 2), and their confidence in this estimate (on a 100-point slider ranging from "not confident at all" to "completely confident"). Those who declared that they expected to do better in the second round than in the first were presented with the following additional question:

> For the second game, you estimated a score that exceeds what you obtained in the first game by ** points.
> [** = difference between the estimated score for the second game and the score of the first game.]
> Do you think that this improvement, if achieved, would be due to your ability?
> Yes, at least in part. No, not at all.

Finally, to investigate the predictions made by the content-specific pathway, we measured the time taken by the participants to fire each shot in the two rounds of the game.

Results

Frequencies of Counterfactuals and Data Exclusions

Table 2.3 reports the frequencies of the different types of counterfactuals produced by the participants in the counterfactual condition. As it can be seen, the percentages of controllable and uncontrollable counterfactuals (40% and 58%, respectively) were in line with those observed in the intermediate condition of Experiment 1 (39% and 56%, respectively). In the counterfactual condition, one participant who experienced technical problems and six participants who produced ambiguous or no counterfactuals were excluded. The final sample thus consisted of 493 participants (47% female; M_{age} = 28.7, SD_{age} = 5.84).

Performance Improvement Via the Content-Neutral Pathway

Table 3.1 reports the mean performance scores in each condition. To test the effect of counterfactuals via the content-neutral pathway, a multiple linear regression model was fit, in which total score in the second round was predicted by the experimental condition (no-counterfactual vs. counterfactual). Results were controlled for the same set of covariates used in the previous studies, with the addition of the total score in the first round, a negative mood measure,[10] and confidence rating for the estimated score in the second round. The model including the experimental condition was not significantly better than the baseline model (including only the covariates), $F(1, 479) = 0.83$, $p = 0.364$, adjusted $R^2 = 0.45$ (see Table A4), and the use of a Bayesian approach to compare the full and baseline models further confirmed that there was moderate evidence ($BF_{10} = 0.17$) in favor of the latter. To further explore this result, we tested whether there was any difference in terms of performance improvement depending on the type of counterfactual produced. We ran a multiple regression similar to that described above, but, in this case, the counterfactual condition was further divided according to the type of counterfactual produced (resulting in a comparison of three groups: no vs. controllable vs. uncontrollable counterfactuals; see Table A5). Again, including the experimental condition did not significantly improve the model compared to the baseline one, $F(2, 478) = 2.37$, $p = .095$, adjusted $R^2 = 0.45$, with moderate evidence ($BF_{10} = 0.31$) in favor of the latter. Regarding the covariates, in both regressions, the same patterns of significance were observed (the following results refer to the latter of the two models). Higher scores in the second round were associated with higher scores in the first round ($\beta = 0.65$, $SD = 0.05$, $p < .001$), with higher ratings of pleasure in playing video games ($\beta = 0.17$, $SD = 0.06$, $p = .005$), and with higher confidence in the pre-game estimates ($\beta = 0.09$, $SD = 0.04$, $p = .030$). Sex was also a significant covariate in the direction of higher scores in the second round for males compared to females ($\beta = 0.32$, $SD = 0.08$, $p < .001$), while experience with video games was negatively correlated with scores in the second round ($\beta = -0.18$, $SD = 0.07$, $p = .005$).[11] The negative mood measure was not a significant predictor ($\beta = 0.01$, $SD = 0.04$, $p = .831$).

[10] The negative mood measure was created by means of a principal component analysis (PCA) of the five mood ratings (happiness rating reversed). Both Bartlett's test of sphericity ($p < .001$) and Kaiser-Meyer-Olkin measure of sampling adequacy (overall MSA = .81, all item's MSA ≥ .77) allowed the PCA to proceed. Parallel analysis suggested a structure with a single factor, accounting for 63% of the variance and with all items loading adequately on it (all factor loadings ranging from .73 to .85). The negative mood measure was computed as the resulting factor scores.

[11] In this regression model, the score in the second game was predicted controlling for the score in the first one, making it conceptually equivalent to a test of the improvement between the two games. Because of this, the negative relationship between the second score and the experience with video games could be due to the fact that the more experienced participants scored higher in the first game, leaving less room for improvement in the second one.

The same pattern of results was observed when the analyses considered only the participants who believed that they could do better in the second round (i.e., participants who estimated a higher score in the second round and who stated that such improvement would be due at least in part to their ability; see Tables A6 and A7).

Table 3.1
Mean performance scores (and SDs) in the first and second game of Experiment 3, according to the type of counterfactual modification produced

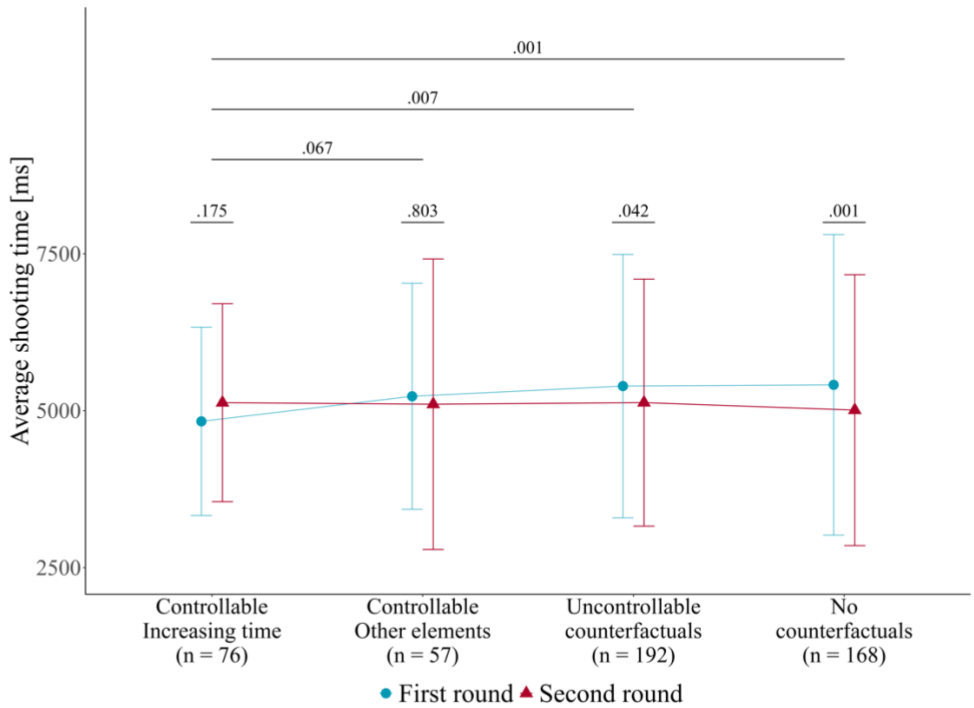
Counterfactual modification	First game	Second game
Controllable (*n* = 133)	40.9 (20.3)	48.0 (21.2)
Uncontrollable (*n* = 192)	37.2 (19.4)	41.4 (22.6)
No-counterfactual (*n* = 168)	37.9 (20.2)	45.1 (22.2)

Performance Improvement Via the Content-Specific Pathway

To assess whether the preliminary condition for the content-specific pathway held, participants who produced controllable counterfactuals were further divided based on whether their modification focused on not rushing their shots versus on other elements. Then, a linear mixed model was fit to investigate possible changes in the shooting times between the first and the second game based on the type of counterfactual produced by participants (see Figure 3.1 and Tables A8 and A9). The model included the squared-root transformed average shooting time as the dependent variable, the type of counterfactual produced, the game considered and their interaction as fixed effects, and the random intercepts for participants (which, when included, significantly improved the fit of the model, $\chi^2(1) = 451.92$, $p < .001$). Results showed a significant main effect of the game, $b = -0.478$, $p = .044$, 95% *CI* [-0.941 – -0.016], indicating that, overall, average shooting times were lower in the second game compared to the first, and no main effect of the counterfactual produced, $p = .852$. However, these effects were qualified by a significant interaction, $\chi^2(1) = 14.66$, $p = .002$, which was explored with a series of (Bonferroni corrected) contrasts (See Tables A10-A11). First, it emerged that participants who produced uncontrollable or no counterfactuals were in line with the general tendency to decrease shooting time in the second round (*p* = .042 and *p* = .001, respectively). This was not the case, however, for participants who produced controllable modifications, both those concerning the time taken to shoot or other controllable elements (*p* = .175 and *p* = .803, respectively), even if the former group was the only one to show an increase in shooting time in the second as compared to the first game. Second, we compared the difference in shooting times between the second and the first game for participants who produced a counterfactual about this aspect with the differences for participants who produced other types of modifications (positive values of the difference indicated an increase in shooting time from the first to the second game). It emerged that the contrasts were significant when the former group of participants, who increased their shooting time in the second game (*M* = 298, *SD* = 1,211; values in millisecond), was compared to those who produced an uncontrollable counterfactual (*M* = -264, *SD* = 1426, *p* = .007) or no counterfactual (*M* = -402, *SD* = 1,390, *p* = .001, respectively), who decreased their shooting times. Instead, the contrast was not significant when they were compared to participants who produced controllable counterfactuals on other elements (*M* = -126, *SD* = 1,559, *p* = .067), who still showed a decrease in shooting times, even if smaller in magnitude. This last result might be due to the fact that at least some of the controllable modifications produced by this latter group of participants were focused on actions (e.g., paying more attention to game features) that, if implemented, might have produced an increase in the time taken to shoot (see also Figure A1).

Then, as a further test of the content-specific pathway, it was investigated whether participants who produced a counterfactual about increasing their shooting time exhibited a significant relation between an increase in shooting time and an improvement in their performance in the second game. A regression model was fit (adjusted R^2 = 0.40), revealing a significant increase in the second game score as the difference in shooting times increased (β = 0.211, *p* = .013, 95% *CI* 0.045 – 0.376), even after controlling for the score obtained in the first game (β = 0.587, *p* < .001, 95% *CI* 0.420 – 0.755; see Table A12). Thus, the analysis suggests that, among participants who produced a counterfactual about not rushing their shots, the more the shooting time was increased the more the performance in the second game improved. However, it is important to note that the overall improvement of participants who produced a counterfactual about increasing shooting time was not significantly greater than that of other participants. Indeed, a regression model similar to that used to test the content-neutral pathway (but in which controllable counterfactuals were further divided, for a comparison of four groups: no vs. controllable about time vs. controllable about other elements vs. uncontrollable counterfactuals) did not account for the score obtained in the second round significantly better than a model including only the covariates, $F(3, 477) = 1.59$, $p = 0.191$, adjusted $R^2 = 0.45$, with strong evidence in favor of the latter ($BF_{10} = 0.09$). See Table A13 for further details.

Figure 3.1
Average shooting time in the first and second rounds, depending on the counterfactual produced



Note. Error bars represent standard deviations.

Discussion

Experiment 3 showed that simply thinking counterfactually about how one's performance could have been better does not produce sizeable improvements of performance in the same task, not even when the modification focused on controllable rather than uncontrollable elements. Thus, our results align with those that failed to find evidence of a content-neutral effect of counterfactuals (e.g., Branch & Anderson, 2020; Petrocelli et al., 2012; Summerville et al., 2019). Moreover, this finding was obtained employing a task characterized by a complex causal domain and by the possibility to improve performance in immediate repetitions of it. Also, the level of perceived opportunity did not appear to have an influence on the beneficial effects of counterfactual thinking. On the other hand, the findings that participants who produced a modification about not rushing their shots tended to actually increase their shooting time (more than participants who made uncontrollable or no modifications), and that the more these participants increased their shooting time the more they improved their scores in the second game (with respect to the first one), could seem consistent with the prediction related to the content-specific pathway of FT. Indeed, it may be interpreted as a controllable counterfactual modification helping to prepare for the future by increasing the likelihood of taking a specific, desired action, which leads to an improvement in a subsequent performance. However, the observed improvement in performance was not significantly greater than that experienced by participants who generated different types of counterfactuals or no counterfactuals at all. Taken together, these results suggest that producing and implementing a controllable counterfactual does not confer any specific preparatory advantage by itself. Rather, it underlines, as also suggested by the advocates of FT (Roese & Epstude, 2017), that greater attention should be devoted to the process of causal analysis that shapes the level of understanding of the experienced event. Such causal analysis is central to counterfactual thinking and may even be prompted by the request to engage in this form of mental simulation. It can also arise independently of the elicitation of a counterfactual thought, potentially explaining why participants who did not generate counterfactuals improved to the same extent as those who produced (and implemented) a controllable modification or other types of counterfactuals. Thus, while our aim was to explore potential beneficial effects associated with the content-specific pathway in a complex causal domain, future research should examine the underlying causal mechanisms behind such effects. In addition to this, it should be noted that the number of participants who produced a counterfactual about the shooting time was rather small and that the obtained result is at odds with that of Myers et al. (2014, Study 2). In their study, participants who thought they would have done better in a word-completion puzzle if they had taken more time to solve it, did not increase their persistence during a repetition of the task any more than control participants did. A possible reason for this discrepancy is that, due to the nature of the task, participants might have had the impression that taking more time would have been helpful but perhaps without clear ideas on what could have been done with that extra time. In our experiment, participants might have envisioned more specific actions that could have been implemented. Indeed, some of them produced modifications such as, “If only I’d spent a bit more time anticipating where the target was going to be and aiming a little better for it”. This would make their counterfactuals more akin to implementation intentions, thus favoring their enactment (Gollwitzer, 1999; in line with this possibility, Myers and colleagues reported that manipulating implementation intentions significantly affected participants’ persistence).

Among possible limitations of Experiment 3, it is worth mentioning that the content of the counterfactuals produced by participants was not manipulated, but, rather, they were free to choose how to complete the counterfactual prompt provided. Because of this “self-selection” of participants who spontaneously focused their modifications on a controllable element rather than on an uncontrollable element (and, even more specifically, the self-selection of participants who spontaneously focused their controllable modifications on shooting time rather than on another element), it cannot be established whether, and to what extent, the implementation of the action considered in the counterfactual was driven by personal characteristics, rather than by the counterfactual thought per se. Future studies might try to overcome this limitation, even if addressing this problem is extremely challenging, since a procedure in which participants are forced to consider a particular counterfactual might fail to find a content-specific effect not because such an effect does not exist, but because participants might not be convinced by the proposed counterfactual (Myers et al., 2014; Petrocelli et al., 2011). A second limitation of the experimental task is the impossibility of assessing whether controllable modifications other than those focused on not rushing the shots were implemented when repeating the task. To overcome this limitation, future works could employ modified versions of the task introducing a greater variety of elements that can serve as targets for objectively measurable controllable counterfactuals. For instance, participants could be given the option to spend a certain number of points to reduce the current shot's difficulty or to adjust the parameters determining the game variables themselves, linking more challenging configurations to higher rewards. Overall, such variations would allow a better mapping of the correspondence between the modifications considered and their enactment, and would provide more conclusive evidence of the beneficial effect of counterfactuals via the content-specific pathway. Additionally, this approach would expand the range of potential controllable modifications, allowing for an even more rigorous assessment of the extent to which individuals tend to prioritize controllable versus uncontrollable modifications. It would be necessary to exercise caution, however, as a disproportionate increase in the availability of controllable counterfactuals might undermine the representativeness of the task for real-life scenarios, which are typically characterized by a mixture of possible controllable and uncontrollable modifications, and lead to an inflated production of the former due to overexposure.

Chapter 4 – Interest for Non-Preparatory Counterfactual Information

The results presented in Chapters 2 and 3 cast doubts on the extent to which counterfactual thinking mainly serves a preparatory function. However, a further point that has recently been stressed by the proponents of FT is that the theory's predictions will more likely apply to counterfactuals that have been produced spontaneously rather than induced, as it often happens in experimental contexts (Roese & Epstude, 2017). While the reasoning behind this point was not made explicit, it likely regards the concern that counterfactuals produced following external prompts may have not been produced in more ecological settings, and therefore they fail to show a preparatory valence because their production is motivated by goals that do not align with those typically promoting the consideration of counterfactual scenarios (i.e., preparatory ones). Thus, to further inform the debate regarding the function of counterfactual thinking, more attention should be devoted to the spontaneous forms of this type of mental simulations. Some methodologies have been proposed to investigate spontaneous counterfactuals, such as asking participants to list their thoughts without making reference to the concept of counterfactual thinking (e.g., McEleney & Byrne, 2006) or taking phenomena like the false recognition of previously non-presented statements as proxies of the spontaneous activation of this form of mental simulation (Smallman et al., 2022). However, such methodologies would either require the recruitment of a prohibitively high number of participants, or they may not be easily applicable to the kind of experiments needed to precisely test the predictions of FT, such as the ones presented in Chapters 2 and 3. Indeed, these methodologies often require the use of rather simplified stimuli, such as vignettes in which a specific element (e.g., an action by the protagonist that deviated from her routine) lures the attention of the participants and, consequently, directs their counterfactual thinking towards considering a specific modification (see, for example, Smallman et al., 2022). In this way, knowing in advance what counterfactual thought the participants will likely produce, the experimenters can look for signs indicating that such production has happened, without the need of asking participants to make any thought explicit. As we have discussed in the previous chapters, however, properly evaluating the preparatory function of counterfactuals necessitate that individuals navigate rich environments where several possible counterfactual modifications are available to be considered, making it difficult to predict which, if any, will be evaluated by them.

Nevertheless, some results present in the literature can shed some light on the extent to which the preparatory function is involved in spontaneous, or at least intrinsically motivated, instances of counterfactual thinking. For example, participants in Branch and Zickar (2021) were asked to self-evaluate the perceived function of a series of spontaneously generated thoughts, and it emerged that counterfactuals were reportedly produced more to regulate mood rather than to aid action-planning and decision making. Also, findings from the literature on information seeking show that people are interested in finding out what outcome they would have experienced if they had made a different decision in the past, even when such information is non-instrumental, meaning it cannot assist in achieving better results in subsequent decisions. This phenomenon is particularly interesting for the debate regarding the central role of the preparatory function in more spontaneous forms of counterfactual thinking. The fact that people display an interest in discovering "what would have been" even in the absence of preparatory goals suggests that such goals may not be necessary to observe genuine instances of counterfactual thoughts. Consequently, exploring individuals' pursuit of non-instrumental counterfactual information (i.e., information about the outcome of forgone options) offers the opportunity to analyze the topic of the function of counterfactual thinking from a different perspective. Furthermore, investigating this behavior can offer insights on another frequently cited function of counterfactual thinking, namely emotion regulation, which has been proposed to characterize counterfactual thoughts in situations where preparatory goals are less prominent (Roese & Epstude, 2017).

Non-Instrumental Counterfactual Information Seeking

Contrary to models proposing that information is sought only when it allows the maximization of rewards, the psychological literature on information seeking has extensively shown that people look for information even when it is non-instrumental (e.g., Bennett et al., 2016; Charpentier et al., 2018; Liew et al., 2022). In those studies, non-instrumentality was operationalized, for example, as feedback on otherwise undisclosed outcomes of independent rounds in a stock market task, or as the possibility to find out in advance the outcome of a lottery that would have been in any case revealed later on. Strikingly, in some cases people were even willing to acquire non-instrumental information when they expected it to have a negative impact on their emotional state and when they had to incur costs to acquire it (e.g., FitzGibbon et al., 2021; Hsee & Ruan, 2016). Having ascertained that people do care for non-instrumental information, the literature is now investigating what factors make them more or less likely to look for it. As reported by various authors (e.g., Fitzgibbon & Murayama, 2022; Mechera-Ostrovsky et al., 2023; van Lieshout et al., 2020) an explanation that is often offered to account for this behavior pertains to emotion, and especially regret, regulation.

In this regard, it is first important to distinguish cases in which the potential regret experienced by individuals is anticipated versus post-decisional (Zeelenberg & Pieters, 2007). In the former, individuals may not yet have made a decision between various options, or they might not yet know how good or bad the outcome of their decision is, and so they can only simulate the extent to which they would regret a certain decision if it were to turn out poorly; in the latter, instead, individuals are aware of the outcome of the decision they made, and so the regret they possibly experience is actual rather than simulated.

Starting from situations in which a decision has yet to be made or in which the outcome of a decision is not yet known, it appears that people tend to look for information when they expect it to elicit pleasant emotions or to reduce unpleasant ones, but they avoid it when they think it will elicit unpleasant emotions or reduce pleasant ones (e.g., Hertwig & Engel, 2016; Sharot & Sunstein, 2020; Sweeny et al., 2010). Various findings support, more or less directly, this hypothesis. For example, Melnyk and Shepperd (2012) found that people were more likely to avoid acquiring information about being affected by a medical condition when they anticipated that doing so would lead them to experience regret, but this tendency was reversed when they expected that regret would have been experienced for not having pursued said information. Or, similarly, Gigerenzer & Garcia-Retamero (2017) found that people who are more susceptible to the effect of anticipated regret (measured as the tendency to select options that prevent knowing the outcome of a forgone choice; Zeelenberg, 1999) are also more likely to state that, if offered the possibility to know when their partner would die or whether their marriage would end in a divorce, they would refuse this possibility.

When considering situations in which the outcome of the choice made, but not that of the forgone option(s), is known, however, the relation between information seeking and regret appears to be different. In those cases, various studies found that people were willing to expose themselves to information even when they foresaw it could exacerbate the regret they may have experienced. For example, in Shani and Zeelenberg (2007) participants were asked to read vignettes regarding a number of different contexts, from stock market investments to holiday planning. In those vignettes, participants were depicted as having made a decision, but also as having doubts about whether the decision they made yielded the best possible outcome. Across several studies, it emerged that the more regret participants reported they would feel in the scenarios described, the more they thought they would seek out information about the forgone alternatives, even if such information could reveal they indeed made a poor decision and even if it had no instrumental value for the future. Another example is provided by FitzGibbon et al. (2021). In their case, participants were asked to complete a series of trials of a modified version of the Balloon Analogue Risk Task (BART). This computerized task consists in pumping air into a balloon, with each pump increasing by a certain amount a bonus payment that participants would receive at the end of the trial. However, in every trial a random, undisclosed “safe limit” is set: if participants exceed that limit with their number of pumps, the balloon explodes and no bonus payment is awarded for that trial. Given the structure of the task, three scenarios are possible: the worst one is that in which participants make the balloon explode, losing all the bonus payment; the intermediate one is that in which the participants stop pumping too soon, therefore winning less than the maximum amount they could have aimed for; the optimal one is that in which participants stop pumping right before the limit, maximizing the bonus they could get in that trial. In their modified version of the BART, Fitzgibbon and colleagues asked participants to state how many pumps they wanted to perform at the beginning of each trial and then showed them whether or not, based on the number of pumps selected, the balloon had exploded. Independently from whether the balloon had exploded or not, participants were given the possibility to check which was the safe limit for that trial. Thus, checking in a trial where the balloon exploded would give information about how close or far one was to winning the bonus, while checking in a trial where the balloon did not explode would give information about how much of the maximum potential bonus one missed. Given that it was statistically unlikely to select a number of pumps very close to the safe limit, the authors noted that the latter cases would be the ones more frequently exerting a negative effect on participants' emotional state, because the information would often reveal they missed a sizeable amount of the potential bonus (while in trials where the balloon exploded the information would often show they had greatly rather than narrowly exceeded the safe limit, potentially making them experience some form of relief). Importantly, given that the safe limit was determined randomly in each trial, acquiring this information had no practical value. The results showed that participants consistently checked what the safe limit was, even when the acquisition of this information was subject to various forms of cost, such as small physical efforts, time delays, or giving up part of the bonus payment gained, and even if the information often had a negative emotional impact on them. Finally, similar findings can be found in Study 1 by Summerville (2011). There, participants played a card-drawing game. The game consisted in choosing to draw a card from one of two identical decks, and the cards constituting the decks would determine, if drawn, the gain or loss of a certain number of points. After having drawn a card, people could also look at what would have happened if they had chosen the other deck. It was observed that people looked at the forgone deck more often when they had experienced a loss rather than a win, therefore exposing themselves to the risk of confirming they had made a poor decision and, consequently, of exacerbating regret.

Psychologists have advanced some hypotheses on why people exhibit this seemingly paradoxical search for information that might heighten feelings of discomfort. One proposal (Shani & Zeelenberg,

2012) suggests that, when people suspect the choice they made is not optimal, they will find themselves in the unpleasant condition of ruminating about what could have happened had they chosen differently. Trying to resolve this condition is what then pushes people to seek for non-instrumental information: in the best case, they may find out that making a different choice would have resulted in an even worse outcome, experiencing relief; but even if the information confirms that their decision was indeed a poor one, potentially exacerbating regret, this might help them reduce intrusive thoughts about "what could have been".

This focus on the role of regret, which, by definition, is a counterfactual emotion (Zeelenberg & Pieters, 2007), draws a straightforward connection between part of the research line on non-instrumental information seeking and counterfactual thinking. Indeed, the act of looking for information about a forgone alternative has been termed *counterfactual seeking* by Summerville (2011). To be more precise, one could further distinguish this phenomenon (that, with a slight variation, will be called *counterfactual information seeking* in this thesis) based on the instrumentality of the information to be acquired: if the information can help guide future decisions, it should be referred to as *instrumental* counterfactual information seeking; if, instead, the information is not useful in any way to improve the outcome of future decisions, it should be referred to as *non-instrumental* counterfactual information seeking, or, adopting a label proposed by FitzGibbon et al. (2021), as *counterfactual curiosity*.

As mentioned in the introduction of this chapter, non-instrumental counterfactual information seeking is an interesting phenomenon to investigate relative to the debate regarding the importance of the preparatory function in counterfactuals. Indeed, looking for information about the outcomes of forgone alternatives is the epitome of counterfactual thinking: while in most cases we can only wonder about what would have happened if we had chosen differently, people who took part in studies on counterfactual information seeking had the possibility to actually find out this information. At the same time, the non-instrumental nature of the information allows to exclude that individuals seek it for preparatory purposes. Therefore, we conducted a series of experiments on non-instrumental counterfactual information seeking, as this approach provides an alternative perspective for examining the function of counterfactual thinking, in which the instances of counterfactual thoughts considered are intrinsically motivated. In addition to this, the experiments presented in the following paragraphs can offer some insights also on the hypothesis, proposed by various authors in the literature, that non-instrumental counterfactual information seeking might be exhibited, predominantly, in a regret management fashion. Importantly. we have investigated this phenomenon in populations of younger and older adults: this allowed us, on the one hand, to increase the generalizability of our findings, but it also allowed to start providing some data on possible age differences in the interest to acquire information about counterfactual scenarios, on which, to our knowledge, no work is present in the literature (in general, studies investigating counterfactual thinking in older adults appear to be lacking; for an exception, see De Brigard et al., 2016). Given the inclusion of both younger and older adults in our experiment, in the next section a brief overview of age differences in information seeking is provided.

Investigating Age Differences in Counterfactual Information Seeking

The psychological literature has produced various results regarding possible age differences in terms of information seeking, but those findings were often mixed and not directly applicable to the phenomenon of counterfactual information seeking. For example, works on information search[12] showed that older adults tend to spend more time than younger ones searching for information (e.g., Johnson, 1990; Mata et al., 2007; Queen et al., 2012), but at the same time the amount of information they access is somewhat smaller compared to younger adults (Mata & Nunes, 2010), even if not all results align with this findings (e.g., Liu et al., 2020). Psychologists working on information search have also reported that older adults search more for positive rather than negative information compared to younger ones (e.g., Löckenhoff & Carstensen, 2007, 2008), a finding that aligns with

[12] In this thesis, *information seeking* is used as a general term indicating all instances of behaviors through which a piece of information is acquired. *Information search* is used instead more specifically to refer to the phenomenon investigated by works that employed attribute matrices as an experimental paradigm, where participants evaluate information about the attributes of various options to decide among one of such options.

older adults' general preference for positive rather than negative information (*positivity effect*; Mather & Carstensen, 2005), even if, again, recent studies failed to replicate this result (e.g., Levin et al., 2021). In any case, the kind of behavior investigated by the literature on information search is quite different from counterfactual information seeking: in the former, people have to choose what piece of information to attend to in order to decide between multiple options, while in the latter it is probed their interest in finding out the outcome of a forgone option after they have already made a decision and found out about its outcome. Thus, one should exert caution in transferring the findings from one research setting to the other.

A similar point can be made also about works like the one by Hertwig et al., (2021) on deliberate ignorance, that is the choice, not forced by external factors, to avoid accessing information that is available at no or negligible cost. In their study, Hertwig and colleagues asked participants to imagine whether they would want to know the answer to potentially self-threatening questions such as possible genetic predisposition to various diseases. They found that, as age increased, people appeared to be less willing to acquire such information. As a possible explanation of this result, the authors proposed that, for older adults, goals related to preserving their emotional well-being may take precedence over those related to preparing for future events, promoting a tendency to attend more to information bearing positive rather than negative effects on their emotional state (i.e., the positivity effect). Once again, the results by Hertwig and colleagues are not directly applicable to counterfactual information seeking: in their case, people were evaluating whether to acquire information about an undisclosed outcome that was not the result of a choice made by them, while counterfactual information seeking regards the acquisition of information about the outcomes of options people discarded when making a decision.

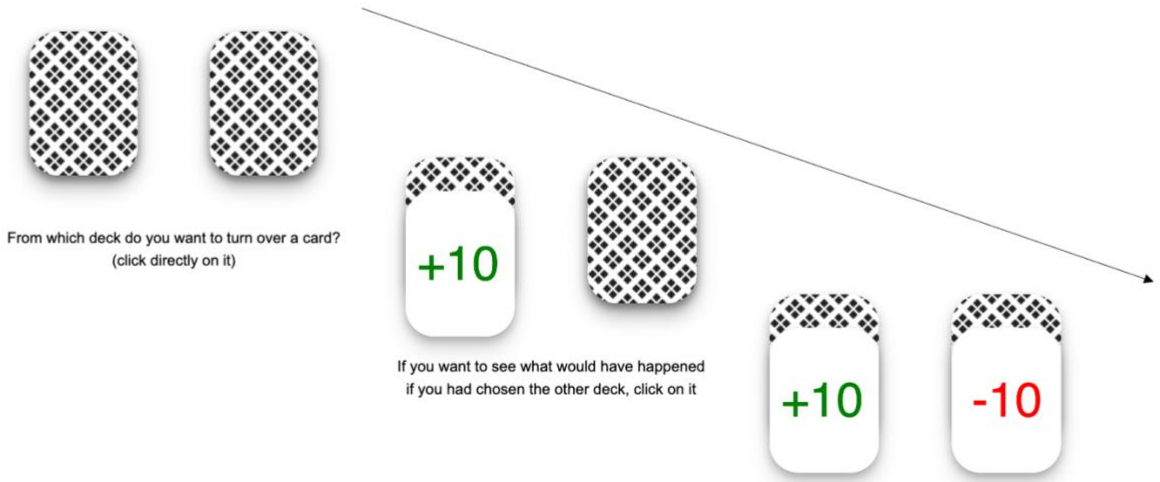
It appears that no work has specifically investigated possible age differences in counterfactual information seeking. However, some studies explored the impact of externally provided counterfactual information on behaviors and emotions on younger and older adults (Brassen et al., 2012; Tobia et al., 2016). Of particular interest for our aims are the results of Brassen et al. (2012).

In their study, it was investigated whether responsiveness to regret differed between younger and older adults. Participants played a game similar to the BART described above (with a series of boxes to be opened one at a time rather than a balloon to inflate), but in this case counterfactual information, that was again non-instrumental in nature, was automatically provided to them (so, there was no actual information seeking). It was found that younger adults tended to become more risk-taking (i.e., they opened more boxes) in subsequent trials the larger the missed opportunity was (i.e., the further they stopped from the safe limit in the previous round, as signaled by the counterfactual information received). This tendency, instead, was not observed in older adults. This difference could not be attributed to a deficit in the ability to think counterfactually on part of the older population, as, like younger adults, they rated to feel more regretful the larger the missed opportunity was. The findings by Brassen and colleagues, that the authors explained in terms of a strategic processing of information aimed at preserving emotional well-being, thus suggest that younger and older adults might handle regret differently. Then, if, as proposed by Shani and Zeelenberg (2012), regret management is a crucial driver of non-instrumental counterfactual information seeking, younger and older adults may exhibit differences in the pursuit of said information predominantly when regret-inducing outcomes are experienced, albeit it is difficult to anticipate the direction of this possible difference.

The Experimental Task

The task we developed to study non-instrumental counterfactual information seeking was an adaptation of the abovementioned card-drawing game used in Study 1 by Summerville (2011). Participants went through a series of rounds in which they were presented with two decks (see Figure 4.1 for a graphical representation of a round). Depending on the experiments, the decks were made up of "winning", "losing", and "neutral" cards, which, respectively, increased, decreased, or left unaffected an endowment (in pence) provided to participants at the beginning of each round. Before the task started, participants were shown the exact composition of the decks they would be playing with, in order to make them fully aware of the outcomes they could experience. In each round, first the two decks were shuffled and then participants were asked to choose from which deck to turn over the top card. Each card had the same probability of ending at the top of the deck and thus of being turned over by participants. After having seen the outcome of their choice, based on which their endowment for the round was updated, they also had the possibility to look at what card they would have drawn if they had selected the other deck. Importantly, since the rounds were independent, the counterfactual information regarding the forgone outcome was non-instrumental. At the end of the task, one round was randomly selected, and the participant received a bonus payment calculated as the starting endowment plus or minus the amount won in the round.

Figure 4.1
Graphical representation of a round of the card-drawing game



This task has several features that made it preferable to other tasks that have been used in the literature on non-instrumental counterfactual information seeking. First, some previous studies assessed information seeking behavior by means of self-reported ratings of how likely one would have looked for a certain piece of information (e.g., Shani & Zeelenberg, 2007; Summerville, 2011, Study 2). However, accurately evaluating one's intention to look for information, especially when considering hypothetical scenarios, might not always be straightforward. In our task, instead, there was a clear behavioral display of counterfactual information seeking, that is the decision to look at the top card of the forgone deck. Importantly, participants were only told that they had the possibility to acquire the counterfactual information, but we did not offer them any incentive to do so, in an attempt to keep the information seeking behavior as genuine as possible. Second, given the theorized centrality of regret for non-instrumental counterfactual information seeking, and for possible age-related differences in this behavior, we considered important to ensure that, in our task, participants could experience such emotion. Since regret is an emotion deriving from the realization that a personal, deliberate choice could have turned out better if one had decided differently (Zeelenberg & Pieters, 2007), asking participants to reflect on hypothetical scenarios, as it was done in other studies (e.g., Shani & Zeelenberg, 2007; van Dijk & Zeelenberg, 2007), might not be suitable for our aims. In our task, instead, the participants were asked to make real choices in which the outcomes were self-relevant, as they could determine the amount of the bonus payment participants would receive at the end of the task. Additionally, as it will be described in Experiment 5, the task can be readily modified to make feelings of regret more or less salient. This feature is not shared by other types of tasks, such as the modified version of the BART used by FitzGibbon et al., (2021). Certainly, the BART may be more suitable to induce regret compared to a vignette-based experimental design, thanks to the participants being actively involved in determining the outcome they experience. However, one might argue that the fact that each number of pumps had an equal probability of being set as the safe limit might reduce the personal responsibility, and, consequently the regret, felt by participants, who had no way of estimating when the balloon would explode (this is a feature that distinguishes this version of the BART from the original one of Lejuez et al., 2002, where the probability to encounter the safe limit increased as the number of pumps increased).

In the next paragraphs, Experiments 4, 5, and 6 will be presented. A playable version of the card-drawing game, together with the datasets and analyses scripts of the three experiments can be found at https://osf.io/56xjm/. See Appendix B for further details on the experiments.

Experiment 4 – Random Choice

In Experiment 4, we investigated whether individuals would exhibit interest for non-instrumental counterfactual information and if such possible interest would be affected by the outcome experienced (negative vs positive) and the age group of the participant (younger vs older adults). First, considering previous findings, we expected to observe a substantial tendency to look for this type of information. More specifically, based on the regret management account, we anticipated that individuals would be more likely to seek such information after experiencing negative, regret-inducing outcomes as opposed to positive ones. Concerning potential age-related differences, if the way in which younger and older adults manage regret plays a role in non-instrumental counterfactual information seeking, we might expect to observe an interaction between age group and outcome experienced, with the two groups differing mostly after negative rather than positive outcomes. However, predicting the exact nature of this interaction remains challenging, leaving room for various speculations. One possibility is that if older adults are more inclined than younger ones to disengage from the experience of regret, as proposed in Brassen et al. (2012), and if seeking counterfactual information serves primarily as a mean to manage this emotion, they might exhibit this behavior to a relatively lesser extent.

Methods

Participants

The minimum sample size for Experiment 4 was estimated using a simulation approach implemented in R (Green & MacLeod, 2016; Kumle et al., 2021). The *a priori* power analysis suggested that, considering a game made up of a total of 10 rounds, a sample of 180 participants should provide 82% power for detecting a small-to-medium effect size (log odds ratio = 0.64, corresponding to Cohen's d = 0.35 according to Sánchez-Meca et al., 2003) for the interaction between outcome experienced and age group. A total of 181 native-English-speaker participants from the UK were recruited, with a Prolific approval rate of at least 90%. Of these, 91 were recruited among the population of younger adults (i.e., age = 18 – 40, for all three experiments; 55% females, M_{age} =

28.9, SD_{age} = 6.40) and 90 among the population of older adults (i.e., age ≥ 65, for all three experiments; 51% females,[13] M_{age} = 69.6, SD_{age} = 3.91). The upper age limit of 40 years for the sample of younger adults correspond to the median age of the UK population in 2021 (Office for National Statistics, 2023), while the lower age limit of 65 years for the sample of older adults reflects the threshold used by other works that investigated the effect of aging on information seeking (e.g., Levin et al., 2021; Queen et al., 2012). Participants received monetary compensation of £0.40, in line with the payment suggested by Prolific, plus the amount of bonus payment they won during the experiment.

Materials and Design

In this experiment, the two decks used in the card-drawing game were identical, each comprising a winning card of the value of 10 pence and a losing card of the value of 10 pence. To account for possible losses, at the beginning of each round participants were provided with an endowment of 10 pence. Thus, the total bonus payment they could obtain was either 0 pence (if they turned over a losing card in the round selected for the bonus) or 20 pence (if they turned over a winning card in the round selected for the bonus). Participants played a total of 10 rounds of the card-drawing game. In Experiment 4 (as well as in Experiment 5), acquiring counterfactual information about the forgone deck was free (i.e., participants did not incur costs to acquire the information).

Procedure

At the beginning of the experimental procedure, participants received instructions on how to play the game and on how the bonus payment would be computed. Additionally, they were fully informed about the composition of the two decks they were going to play with, so that they could have clear expectations about what card they could get during each round (and what card they could have gotten by selecting the forgone deck). They then proceeded to play the game, at the end of which one round was randomly selected to compute the bonus payment. The selected round was presented again to the

[13] For one participant in the older adults group information about sex was not available.

participants, showing the outcome they had experienced and offering them the possibility to check what would have happened had they chosen the forgone deck, independently from whether they had already sought the counterfactual information in that round during the game. Participants' demographics were provided by Prolific.

Results

Overall, the two age groups were balanced in terms of outcomes experienced, both considering the entire game (younger adults: 49% losses; older adults: 49% losses) and the round selected for the bonus payment (younger adults: 51% losses; older adults: 44% losses).

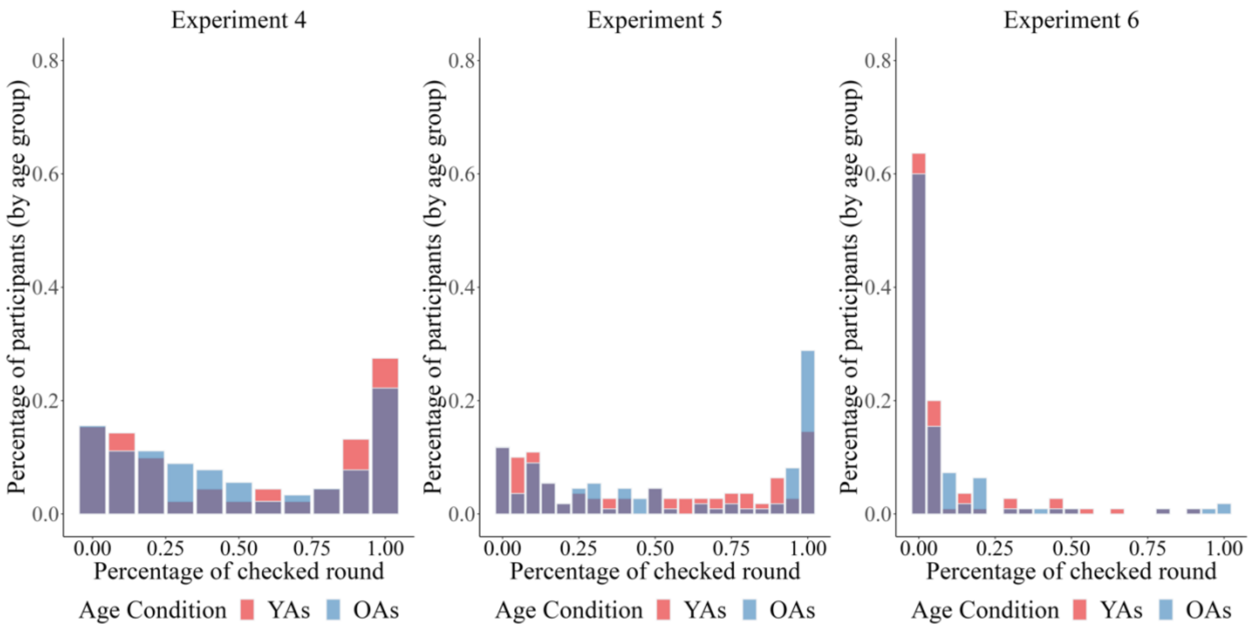
To evaluate the tendency, in the two age groups, to check the forgone deck throughout the game, a generalized linear mixed model (GLMM) was fit including, as the dependent variable, a binary variable indicating whether participant had checked the forgone deck in the various rounds of the game, and, as predictors, age group, the outcome experienced in the various rounds and their interaction (see Table B1 for details). Random intercepts for participants were included in the model, as their inclusion significantly increased its fit, $\chi^2(1) = 893.56$, $p < .001$. Results indicated, first of all, that the fixed intercept of the model was not significant, $p = .283$. This means that, overall, the tendency to look at the forgone deck (51% of all observation) was not significantly different from 50% (i.e., a value of zero in terms of log odds ratio), as also indicated by a Bayesian test of proportions ($BF_{10} = 0.10$, indicating strong evidence in favor of the null hypothesis). Then, a significant main effect of the outcome experienced was observed, $OR = 0.69$, $p < .001$, 95% CI [0.60 – 0.79], with the forgone deck being checked more frequently after having experienced a loss (56%) than after a gain (47%). No significant effect of age group, $p = .354$, nor of the interaction between age group and the outcome experienced, $p = .774$, were observed ($BF_{10} < 0.001$, indicating extreme evidence against the model including the effects of age group). Additionally, as it can be seen from Figure 4.2, the two age groups presented similar distributions with respect to the frequency with which the various participants checked the forgone deck. The results did not change when including the mean-centered number of the round as a covariate in the model. In that case, the covariate was significant, *OR* = 0.81, $p < .001$, 95% CI [0.77 – 0.85], indicating that the tendency to check the forgone deck decreased as the game proceeded (see Table B2 for details). Similar results to the ones observed in the GLMM emerged when a logistic regression was run using the same set of predictors but using, as the dependent variable, a binary variable indicating whether participants had checked the forgone deck when the round selected for the bonus payment was presented again at the end of the game (see Table B3).

Following these analyses, it was also checked whether participants exhibited any pattern of switching behavior. Indeed, we were interested in studying non-instrumental counterfactual information seeking and, accordingly, we built the card-drawing game so to make counterfactual information about the forgone deck non-instrumental. However, it was possible that participants erroneously suspected that the outcomes they experienced followed some undisclosed rule (e.g., the likelihood of turning over a losing card from a deck increases after having turned over a winning card from the same deck in the previous round), thus motivating the adoption of switching behaviors. If this was the case, participants may have perceived counterfactual information about the forgone deck as possessing instrumental value. To assess this possibility, another GLMM was fit, including age group, the outcome experienced in the previous round and their interaction as predictors, but using, as the dependent variable, a binary variable indicating whether participants had switched deck compared to the previous round (see Table B4 for details).[14] Also in this case, random intercepts for participants were included in the model, $\chi^2(1) = 56.96$, $p < .001$. Results indicated a significant effect of age group, *OR* = 0.70, $p < .001$, 95% CI [0.60 – 0.82], with older adults switching significantly less (28%) than younger ones (42%). Importantly, the outcome experienced did not affect switching behavior, neither at the main effect level, $p = .763$, nor in interaction with age group, $p = .108$ (BF_{10} = 0.002, indicating extreme evidence against the model including the effects of previous outcome).

[14] This model was fit on a dataset in which the first round of the game was excluded, as in that case there were no data about switching behavior compared to the previous round.

Figure 4.2

Percentage of participants in Experiments 4, 5, and 6 who checked the forgone deck in a certain percentage of rounds, divided by age group (YAs: younger adults; OAs: older adults)



Discussion

Experiment 4 aimed at investigating people's interest for non-instrumental counterfactual information and the extent to which such interest was modulated by the valence of the outcome experienced and by the age group of individuals. We replicated the finding by previous works (e.g., FitzGibbon et al., 2021; Summerville, 2011) that participants do look for counterfactual information even when it has no instrumental value, and they do so more after having experienced a negative rather than a positive outcome, in line with what would be expected based on the regret management account. We did not find any significant difference between the two age groups, neither at the main effect nor at the interaction level. However, it should be considered that, in this experiment, participants made a choice that could be categorized as random: given that the composition of the two decks was identical, they had no reason to choose one over the other. This characteristic might have made regret less salient, as experiencing this emotion requires the perception of having had some degree of personal responsibility in the decision made and in the outcome that followed it (Zeelenberg & Pieters, 2007). Then, if changes in regret management are what determine differences in non-instrumental counterfactual information seeking between the two age groups, it is possible that the structure of the game employed in Experiment 4 was not adequate to detect such differences.

However, before moving to the subsequent experiment, it is worth noticing a point. If, as just hypothesized, we failed to observe differences between younger and older adults because regret was not salient in this experiment, we should not have also observed the significant increase in checking behavior following losses as compared to gains, a result that has been previously attributed to regret management as well (e.g., Shani & Zeelenberg, 2012; Summerville, 2011). Indeed, it cannot be held that we did not observe age-related differences because the experimental design used failed to elicit regret, but at the same time that we observed a preference to look for the information after negative events because those elicited more regret than positive ones. This might suggest that age-related differences in non-instrumental counterfactual thinking, if present, are not related to regret management, or, conversely, that regret management is not sufficient to explain non-instrumental counterfactual information seeking.

Experiment 5 – Deliberate Choice

Experiment 5 aimed again at investigating non-instrumental counterfactual information seeking in a sample of younger and older adults. However, given the relevance of the emotion of regret in the literature on this type of information seeking, in this experiment the task was modified so that regret could be more salient. Specifically, we introduced an element of deliberation in the game, consisting of giving participants the possibility to play each round with a risky or a safe deck, which could provide, respectively, more or less extreme outcomes in terms of pence that would be added or subtracted to the endowment. As in Experiment 4, we expected to observe a substantial tendency to examine the forgone deck after having experienced negative outcomes as opposed to positive ones. Concerning potential age-related effects, we hypothesized a possible interaction between the outcome experienced and age group, with different checking behaviors by younger and older adults primarily in presence of negative outcomes.

Methods

Participants

The minimum sample size for Experiment 5 was estimated using the same simulation approach employed for Experiment 4. The *a priori* power analysis suggested that, considering a game made up of a total of 20 rounds, a sample of 220 participants should provide 82% power for detecting a small-to-medium effect size (average log odds ratio = 0.52, corresponding to Cohen's d = 0.29) for the interaction between age group and outcome experienced. A total of 221 native-English-speaker participants from the UK were recruited, with a Prolific approval rate of at least 90%. Of these, 110 were recruited among the population of younger adults (59% females,[15] M_{age} = 29.7, SD_{age} = 6.03) and 111 among the population of older adults (56% females, M_{age} = 69.9, SD_{age} = 4.62). Participants received monetary compensation of £0.90, in line with the payment suggested by Prolific, plus the amount of bonus payment they won during the experiment.

Materials and Design

This experiment was similar to Experiment 4, but it presented some differences. In particular, the two decks used in the card-drawing game had the same expected value, but their compositions were different. One deck, called "safe" deck, consisted of five cards whose values (in terms of pence subtracted or added from the endowment) were the following: -30, -10, 0, +10, + 30. The other deck, called "risky" deck, consisted of five cards as well, but in this case the values of the outcomes were more extreme: -60, -20, 0, +20, +60. Because of this change in the outcomes participants could experience, to account for the worst loss possible the endowment provided to them at the beginning of each round was 60 pence. This means that, by selecting the risky deck, participants could double the endowment in that round (if they turned over a +60 card), but also lose it completely (if they turned over a -60 card), while by selecting the safe deck they would guarantee themselves a potential bonus payment of at least 30 pence for that round (if, in the worst-case scenario, they were to turn

[15] For one participant in the younger adults group information about sex was not available

over a -30 card). The two decks could be presented in either the left or the right part of the screen, with their position being randomly determined in each round. To help participants distinguish the two decks, the back of the cards composing them were differentiated (see Figure 4.3, in which the deck on the left is the safe one and the deck on the right is the risky one). Additionally, to help participants keep track of which cards they could find by selecting one deck, miniatures of those cards were shown above the respective decks. Participants played a total of 20 rounds.

Figure 4.3
Representation of how the safe (left) and risky (right) decks appeared to participants in Experiment 5 at the beginning of each round

Procedure

The procedure was identical to that of Experiment 4.

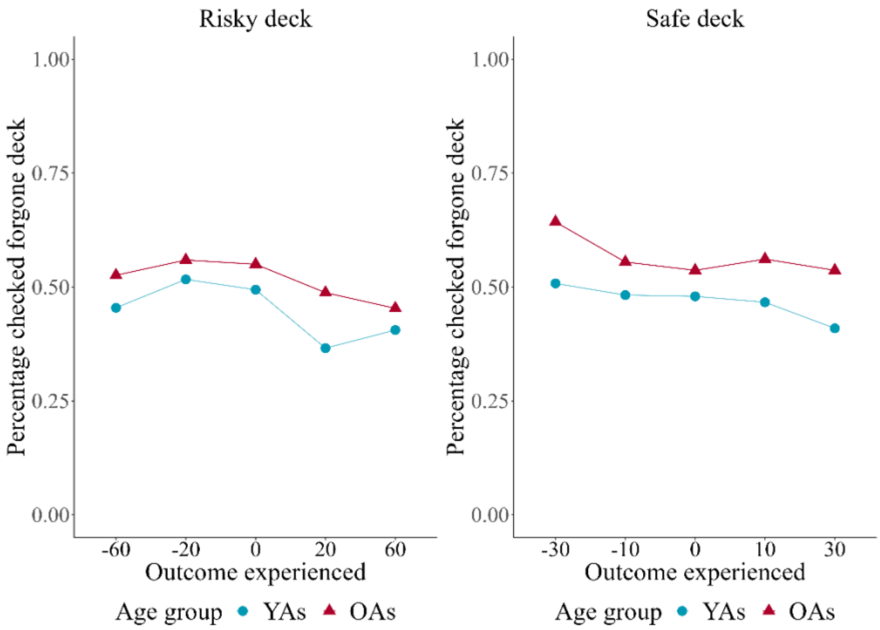
Results

The two age groups resulted balanced on various aspects. First, a GLMM was fit to compare the extent to which the two groups chose the risky versus the safe deck (with the inclusion of the random intercept term for the participants, $\chi^2(1) = 608.99$, $p < .001$). It resulted that younger (57%) and older adults (56%) were similar, $p = .910$, in their tendency to choose the risky deck somewhat more frequently than the safe deck ($BF_{10} = 0.02$, indicating very strong evidence against the model including the effect of age group). As a consequence, the outcomes experienced by the two age groups were also comparable (see Figure B1). Additionally, the distributions of younger and older adults who chose the risky deck in a certain percentage of rounds were similar as well (see Figure B2).

Then, counterfactual information seeking in younger and older adults was investigated by fitting a model structurally identical to the one presented in Experiment 4 (with random intercepts for participants, $\chi^2(1) = 2606.90$, $p < .001$). The model showed that, overall, the tendency to check the forgone deck (50%) was not different from 50%, as signaled by the fixed intercept failing to reach significance, $p = .314$, as well as by a Bayesian test of proportions ($BF_{10} = 0.04$, indicating strong evidence in favor of the null hypothesis). Additionally, a significant effect of the outcome experienced was observed again, $\chi^2(9) = 75.79$, $p < .001$, with post-hoc tests indicating that more negative outcomes tended to elicit more information seeking compared to positive outcomes (see Tables B5-B8 and Figure 4.4). In this case, the effect of age group was also significant, *OR* = 1.63, $p = .039$, 95% CI [1.03 – 2.60], with older adults being on average more likely to check the forgone deck (54%) compared to younger adults (46%). As it can be seen from Figure 4.2, this difference was likely driven by a higher percentage, among older adults, of participants who consistently checked the forgone deck in every round of the game. The interaction between the two predictors was not significant, $p = .984$. The results did not change when the round number was considered as a covariate. As in Experiment 4, it had a significant effect on probability of checking the forgone deck, *OR* = 0.87, $p < .001$, 95% CI [0.85 – 0.89], in the direction of decreasing it as the game moved to later rounds (see Table B9 for details). Also, a logistic regression was fit using the same predictors but, as the dependent variable, the binary variable indicating whether participants had checked the forgone deck when the round selected for the bonus payment was presented again at the end of the game. In this case, only the interaction between the age group and the outcome experienced reached significance, $\chi^2(9) = 21.60$, $p < .010$. However, interpreting this result may not be advisable, given the low number of observations in the various cells resulting from the intersection between the age group and outcomes experienced (e.g., older adults experiencing a +60), which produced high standard errors (see Table B10).

Figure 4.4

Percentage of rounds in which the forgone deck was checked, according to age group (YAs: younger adults; OAs: older adults) and outcome experienced in that round (divided between risky and safe decks)



Similarly to Experiment 4, these results were further explored by fitting a GLMM to assess participants' switching tendency (with random intercepts for participants, $\chi^2(1) = 316.56$, $p < .001$). The results showed that, in this case, younger (38%) and older adults (36%) switched decks from one round to the other to a similar extent, $p = .476$ ($BF_{10} < 0.001$, indicating extreme evidence against the model including the effect of age group). However, the effect of the outcome experienced in the previous round was significant, $\chi^2(9) = 113.99$, $p < .001$, and post-hoc tests suggest that this effect was due to a tendency of staying with the risky deck after having experienced a loss (see Tables B11-B14 and Figure B3). An additional model on switching behavior was fit to investigate the role of not just the outcome experienced in the previous round, but also of how this outcome compared to the forgone alternative. To this end, the dataset considered was restricted only to observations in which the participant had checked the forgone deck in the previous round. Then a variable was computed to capture the nature of the "counterfactual outcome" experienced in the previous round. This categorical variable had six levels: avoided loss by selecting the safe deck (i.e., cases in which the safe deck was selected and it was found that the outcome of the risky deck would have been worse);

double zero by selecting the safe deck (i.e., cases in which the safe deck was selected and both the experienced and forgone outcomes were zero); missed gain by selecting the safe deck (i.e., cases in which the safe deck was selected and it was found that the outcome of the risky deck would have been better); avoided loss by selecting the risky deck (i.e., cases in which the risky deck was selected and it was found that the outcome of the safe deck would have been worse); double zero by selecting the risky deck (i.e., cases in which the risky deck was selected and both the experienced and forgone outcomes were zero); missed gain by selecting the risky deck (i.e., cases in which the risky deck was selected and it was found that the outcome of the safe deck would have been better). The model was then fit predicting switching behavior based on age group, the counterfactual outcome experienced in the previous round and their interaction (with random intercepts for participants, $\chi^2(1) = 83.94$, $p < .001$). Consistent with what we found in the previous model, age group did not affect switching behavior, $p = .712$ ($BF_{10} < 0.001$, indicating extreme evidence against the model including the effect of age group), while the counterfactual outcome experienced in the previous round did, $\chi^2(5) = 22.90$, $p < .001$. Post-hoc tests suggest that this effect was driven by the tendency to avoid switching when experiencing a missed gain after having selected the risky deck (see Tables B15-B18 and Figure B4).

Discussion

Experiment 5 expanded the results of Experiment 4 to a condition in which participants' choices included an aspect of deliberation, given that they had to decide between a risky and a safe deck, a situation in which regret was more likely to be salient compared to when the choice was random. It was again observed that participants do seek non-instrumental counterfactual information, especially following negative rather than positive outcomes, but in this case also an effect of age group emerged: overall, independently from the outcome experienced, older adults were found more likely to check the forgone deck compared to younger adults. The fact that such a difference emerged when the choice was deliberate rather than random might, at a first glance, suggest that regret management is a candidate explanation for the effect of age over this type of counterfactual information seeking.

However, as it will be better pointed out in the General Discussion, if that was the case, we should have observed an interaction between age group and outcome experienced, with older adults differing from younger ones mostly after regret-inducing outcomes, that is the negative ones.

From the results regarding participants' switching behavior, one might wonder whether the tendency to switch less after having experienced a loss (especially with the risky deck) indicates that participants suspected the outcomes were not randomly determined, but rather they followed an undisclosed rule. In case they believed this, for them checking the forgone deck would have been instrumental to better predict subsequent outcomes from the various decks. However, if participants actually considered counterfactual information to be instrumental in the sense just outlined, we should have observed a high tendency to check the forgone deck independently from the outcome experienced (and not just after negative ones). Additionally, it should be noted that the highest percentages of switching following certain outcomes were around 50%, suggesting that switching behaviors, in those cases, were random rather than following a precise rule (see Figure B5).

In the General Discussion it will be also examined in greater details the effect of the counterfactual outcome over switching behavior. Albeit studying this aspect was not the main aim of the experiment, the finding that both younger and older adults appear to change their behavior following non-instrumental counterfactual information seems in contrast with what was found by Brassen et al. (2012), in which older adults did not exhibit such tendency.

Experiment 6 – Deliberate Choice, Costly Information

In Experiment 6, we further extended our results by investigating non-instrumental counterfactual information seeking in younger and older adults in a context where acquiring such information was subject to a cost. In this way, it was also possible to test the strength of the age-related difference observed in Experiment 5, exploring whether it would persist even when the information could be acquired only by spending part of a monetary endowment.

Methods

Participants

Since Experiment 6 was identical in terms of materials and structure to Experiment 5, a total of 220 native-English-speaker participants from the UK were recruited, with a Prolific approval rate of at least 90%. However, two participants were excluded as their self-reported age, differently from what was reported in the demographics provided by Prolific, i.e., below the age cutoff of 65 years.[16] Thus, the final sample included 218 participants, of which 110 were recruited among the population of younger adults (54% females[17], M_{age} = 29.8, SD_{age} = 6.40) and 108 among the population of older adults (49% females, M_{age} = 69.1, SD_{age} = 3.66). Participants received monetary compensation of £0.90, in line with the payment suggested by Prolific, plus the amount of bonus payment they won during the experiment.

Materials and Design

The version of the card-drawing game used in this experiment was identical to that employed in Experiment 5, but with two differences. First, while in Experiment 5 access to information about the forgone outcome was free (as in Experiment 4), in Experiment 6 participants had to pay 5 pence from their endowment to see what card they would have turned over if they had selected the other deck. Consequently, to account for possible losses and expenses to acquire the counterfactual information, the endowment provided at the beginning of each round was equal to 65 pence. In the instructions, it was explicitly stated that the rounds were independent, hence, purchasing this information did not have any impact on subsequent rounds. Second, in Experiment 5 the analysis of the checking behavior in the trial selected to compute the bonus payment were inconclusive due to the low number of observations for the various cases resulting from crossing the age group and the outcome experienced (e.g., older adults experiencing a +60). Thus, we removed the possibility to check the forgone deck

[16] This type of exclusion was not performed in Experiments 4 and 5 because, there, the question on self-reported age was not included.

[17] For one participant in the younger adults group information about sex was not available.

when the selected round was presented again at the end of the game.

Procedure

The procedure was identical to that of the previous experiments.

Results

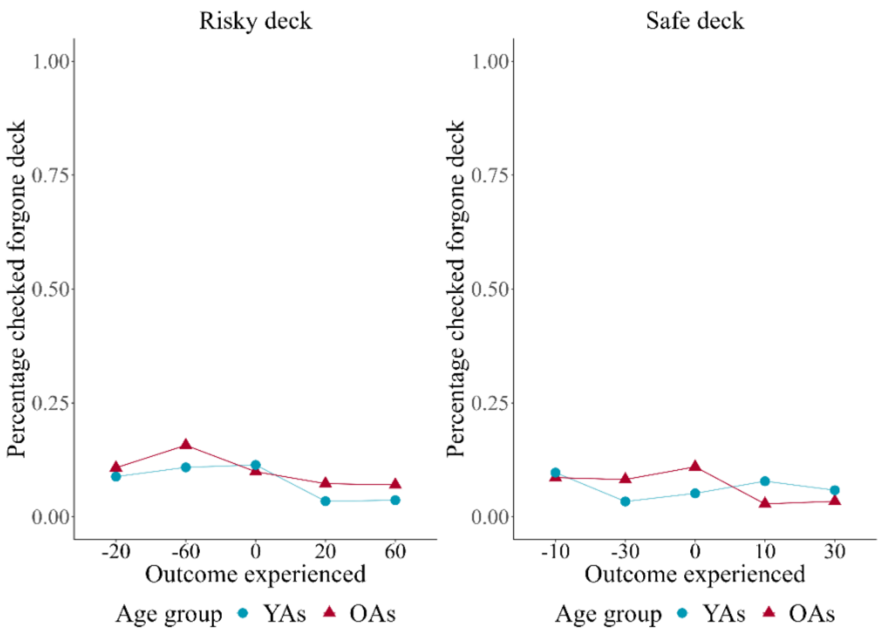
The first noticeable finding is that, in this experiment, the percentage of observations in which the participants checked the forgone outcome was, overall, much lower (8%) compared to what was observed in the previous experiments, even if not totally null. Indeed, a binomial test confirmed that this percentage was significantly higher than 5%, $p < .001$. For what concerns the general characteristics exhibited by the two age groups, choices by younger and older adults were again similar in many aspects, as was the case in the previous experiments.

First, a GLMM (with random intercepts for participants, $\chi^2(1) = 645.61$, $p < .001$) indicated that their tendency to choose the risky versus the safe deck was comparable, $p = .303$ ($BF_{10} = 0.03$, indicating very strong evidence against the model including the effect of age group), with both younger (63%) and older (59%) adults choosing the former more frequently and, consequently, experiencing a similar distribution of outcomes throughout the game (Figure B3). It should be noted that, from a qualitative point of view, younger adults were somewhat more consistent in their preference for selecting the risky deck throughout the game (Figure B4).

Then, a GLMM (with random intercepts for participants, $\chi^2(1) = 908.19$, $p < .001$) was fit to investigate how the age group, the outcome experienced, and their interaction affected the probability of looking for counterfactual information. First, as anticipated above, it emerged that the overall percentage of observations in which the forgone deck was checked was below 50%, as indicated by the significance of the fixed intercept, $OR = 0.004$, $p < .001$, 95% CI [0.002 – 0.010]. This was true for both younger (7%) and older adults (9%), as suggested by the main effect of the age group failing to reach significance, $p = .538$ ($BF_{10} < 0.001$, indicating extreme evidence against the model including the effects of age group). As it can be seen in Figure 4.2, the distributions of younger and older adults in terms of percentage of rounds in which they checked the forgone outcome were indeed similar. Instead, the effect of the outcome experienced persisted, $\chi^2(9) = 61.45$, $p < .001$, with post-hoc tests again suggesting that participants inspected the forgone deck more frequently after a negative rather than a positive outcome, in particular when they had selected the risky deck (see Tables B19-B22 and Figure 4.5). The interaction between the age group and the outcome experienced only approached significance, $p = .057$. The further GLMM that included the round of the game as a covariate failed to converge, likely due to low number of observations in which the forgone deck was checked.

Figure 4.5

Percentage of rounds in which the forgone deck was checked, according to age group (YAs: younger adults; OAs: older adults) and outcome experienced in that round (divided between the risky and safe decks)



Finally, a GLMM (with random intercepts for participants, $\chi^2(1) = 265.72$, $p < .001$) was fit to investigate whether the two age groups exhibited similar switching behaviors. The results showed that no effect of age group was present, $p = .592$ ($BF_{10} < 0.001$, indicating extreme evidence against the model including the effects of age group), while, as in Experiment 5, a significant effect of the outcome experienced in the previous round was observed, $\chi^2(9) = 142.36$, $p < .001$. Once again, this tendency appeared to be driven mainly by participants sticking to the risky deck after a loss (see

Tables B23-B26 and Figure B5). The model including the counterfactual outcome experienced in the previous round, instead, reported convergence errors, likely because of the low number of observations in which participants checked the forgone deck.

Discussion

Experiment 6 explored how non-instrumental counterfactual information seeking may be affected by the introduction of a cost to access said information. In particular, it was tested whether the difference, observed in Experiment 5, between the two age groups in terms of information seeking persisted even when participants had to pay to acquire non-instrumental counterfactual information. The results clearly indicated that this was not the case, and that both younger and older adults exhibited a strong decrease in their tendency to check the forgone deck compared to the previous experiments (even if, it should be noted, this decrease did not bring the number of observations in which participants checked the other deck to zero). Instead, we found once again that more negative outcomes made checking behavior more likely compared to positive ones. Compared to the previous experiments, in which counterfactual information was free, interpreting this result within the context of Experiment 6 should also take into account possible features deriving from the cost associated with the information. For example, losing 60 pence out of the initial endowment of 65 might have made participants more likely to also forgo the remaining 5 pence to satisfy their curiosity regarding the forgone deck, compared to when the loss was of a smaller amount. Thus, while the effect of the outcome experienced is similar in its direction to that observed in Experiments 4 and 5, it is possible that the underlying psychological motivations driving it differ.

General Discussion

The experiments presented in this chapter explored, in both younger and older adults, the extent to which people seek counterfactual information. It emerged that individuals, independently from their age group, consistently looked for this kind of information, especially after having experienced a negative outcome. Importantly, the information that our participants could access was non-instrumental in nature, a feature that excluded, by definition, that preparatory goals may be motivating participants to look for it. Additionally, while participants were offered the possibility to inspect the counterfactual outcome (and so this action may not be considered entirely spontaneous), they were never encouraged to do so, nor counterfactual thinking was ever mentioned. Thus, information seeking reflected at least a genuine interest by participants to find out about the counterfactual outcome of their choices. Taken together, these characteristics of our experimental design allow to conclude that people can and do experience a compelling desire to know what would have been even in absence of any reasonable preparatory advantage, up to the point that, albeit greatly reduced, this interest for non-instrumental counterfactual information persists even when people have to incur costs to acquire it. While our results do not exclude (nor aim to exclude) that, in certain cases, preparatory goals might be behind spontaneous instances of counterfactual thinking, they at least suggest that other motives can be equally potent in driving this form of mental simulation.

Also, conducting our investigations on samples of both younger and older adults provided some initial data on whether and how these two populations differ in their interest for counterfactual scenarios and, more in general, for non-instrumental information. Understanding how aging affects this type of information seeking can have important practical implications, as it has been suggested that preserving curiosity, which is deeply related to the search for non-instrumental information (Gottlieb & Oudeyer, 2018), may be a crucial component of a healthy aging process (Sakaki et al., 2018). What emerged from our results is that older adults appear more prone than younger ones to pursue said information, but only when it is relative to a deliberate choice they made and when it is free. The fact that this difference disappeared when participants had to incur costs to acquire the information suggests that it was not due to difficulties, by older adults, to inhibit the urge to know "what would have happened" (for a discussion on aging effects on inhibition, see Campbell et al., 2020). This set of results will have to be replicated by future studies, for example extending them to a population not recruited on platforms such as Prolific, which may include a sample of particularly high-functioning older adults. However, if confirmed, these findings will need to be further investigated to understand the underlying mechanisms promoting this difference between the two age groups. One possibility is that it is due to changes, throughout aging, in how people manage regret. Indeed, older adults appear to experience regret less negatively compared to younger adults (e.g., Tassone et al., 2019) and to put in action strategies to disengage from this emotion (e.g., Brassen et al., 2012; Wrosch & Heckhausen, 2002). Certainly, the fact that this result emerged when the decision was deliberate rather than random, that is, in a situation in which regret was more likely to be salient (Zeelenberg & Pieters, 2007), might corroborate this possibility. However, our data do not fully support this hypothesis: if differences in regret management were the motivation behind the age effect observed in Experiment 5, then we should have observed an interaction between age group and the outcome experienced, with older adults seeking more counterfactual information than younger ones after having experienced a negative rather than a positive outcome, that is, in cases in which regret was more likely to be experienced. Instead, we found a main effect of age group, with older adults being more curious than younger ones independently from the outcome they faced. More articulated versions of this emotion regulation hypothesis for age-related differences may still be evaluated as possible explanations for our data. For instance, if older adults are more sensitive to the possibility of ameliorating their emotional well-being compared to younger adults (e.g., Swirsky et al., 2023), they may seek counterfactual information also after positive outcomes as an attempt to further enhance the relative emotional valence of the outcome they experienced, clarifying why we observed a main effect of age rather than an interaction with the outcome experienced. However, it would also be advisable to consider other possible explanations. For example, it may be that counterfactual information, even when non-instrumental, is considered as more interesting by older rather than younger adults. On this note, Schutte and Malouff, (2019) found that, when making a decision between various options, inducing a sense of autonomy (i.e., the perception that a behavior is volitional rather than driven by external factors) increases the intention to seek out more information about the chosen option, especially in individuals low in general autonomy. Considering that older adults have been found to be lower than younger adults on dimensions likely related to autonomy, such as perceived control

(Robinson & Lachman, 2017), it is possible that the former might be more motivated to pursue counterfactual information concerning a deliberate choice (at least when such information is free). Or again, it may be the case that older adults, in general, experience counterfactual thinking as more compelling than younger ones, and this, in our experimental setting, would translate to a higher probability of checking the forgone deck. However, works that specifically investigated the effect of aging over the saliency of counterfactual scenarios are, to our knowledge, almost non-existent,[18] with the exception of one possibly related finding by De Brigard et al. (2016), in which older adults were found to perceive counterfactual simulations (as well as other forms of mental simulations) as more vivid compared to younger adults. Further investigations of this aspect are thus needed before putting forward this hypothesis.

As outlined above, the hypothesis of differences in regret management may not fully explain the result regarding the effect of age group observed in Experiment 5. The regret management explanation has been proposed to explain, more in general, the pursuit of non-instrumental counterfactual information, especially after negative outcomes (Shani & Zeelenberg, 2012; Summerville, 2011). Moreover, regret management is closely related to idea that counterfactuals may be used to regulate one's emotional state, a function that is often cited as the one mainly driving counterfactual thinking when preparatory goals are not particularly salient (e.g., Roese & Epstude, 2017). Also in that case, however, this hypothesis may not fully account for our data. In particular, from a regret management perspective, it is hard to explain why participants, for example in Experiment 5, exhibited high levels of information seeking even after the worst possible outcome (i.e., a loss of 60 pence), given that, in such situation, looking at the other deck could have only resulted in an exacerbation of regret. It is certainly possible to consider that, even in that scenario, acquiring information about the foregone outcome might help dealing with ruminative thoughts about

[18] Horhota et al., (2012) suggest that, based on their findings, younger adults appear to produce fewer counterfactual thoughts compared to older adults, at least in certain circumstances. However, it is not clear whether only counterfactual thoughts were included in their category "experiential-based responses" (i.e., the category of responses found to be produced less by younger adults), or if, under that label, they also grouped other responses not explicitly in a counterfactual form.

what could have been, as it was indeed proposed by Shani and Zeelenberg, (2012). However, if that is the case, it would be worth considering whether such findings could be more accurately explained, rather than in terms of regret management, as a consequence of a general increased activation of counterfactual thinking, which is known to be more common in situations generating negative rather than positive affect (Roese, 1997).

The present research also provides insights into a topic that was not the primary focus of our investigation. In both Experiment 5 and 6, participants tended to stick with the risky deck after having experienced a loss or, when the outcome of the forgone deck was also considered, after having experienced a missed gain. From our results, it appears that this tendency interested both younger and older adults alike, a finding that does not align with what was observed by Brassen et al. (2012). Also in their case, the task employed was made up of a series of independent rounds, and thus counterfactual information provided to participants was non-instrumental (as noted also in Tobia et al., 2016). Brassen and colleagues found that younger adults, but not older ones, tended to change their behavior in response to that counterfactual information, in particular becoming more risk-taking following missed gains. Instead, in Experiment 5, we observed that both younger and older adults tended to stick more with the risky deck after having experienced a missed gain rather than other types of counterfactual outcomes. For sure, the tasks employed in the present research and in the work by Brassen and colleagues (who used a BART-like task) are different in many aspects. For example, in our Experiment 5 the risk-taking nature of participants' behavior was related to selecting one course of action (i.e., playing with the risky deck) rather than another (i.e., playing with the safe deck); instead, in the task employed by Brassen and colleagues, there was no single action that could be categorized as risky, because in their case risk-taking was related to the propensity of opening a higher number of boxes. Also, our analysis considered only observations in which participants had checked, in the previous round, the outcome of the forgone deck. This means that participants who checked the counterfactual information more consistently contributed to a greater extent to the results observed. Thus, the difference with the results reported by Brassen and colleagues may also be due to individual characteristics of the participants considered. Future studies might try to explore what factors can account for the discrepancies between the two works.

Chapter 5 – Conclusions

The overarching goal of the present thesis was to test some of the implications stemming from the hypothesis that, when people engage in counterfactual thinking, most of the time they have a preparatory goal in mind (Roese & Epstude, 2017).

In Chapter 2, two experiments were conducted to assess the extent to which people's counterfactuals about a recent performance would focus on controllable versus uncontrollable elements, and to investigate possible factors able to account for the variability observed in previous studies relative to this dimension. Experiment 1 showed that greater difficulty of the task favors the production of uncontrollable modifications, while the results of Experiment 2 did not allow definitive conclusions regarding the effect of feedback. In Chapter 3, instead, we presented an experiment aimed at assessing the extent to which counterfactuals can aid performance improvement. Experiment 3 showed that simply thinking counterfactually about how one's performance could have been better does not produce sizeable improvements of performance in the same task, not even when the modification focused on controllable rather than uncontrollable elements. However, it was found that participants who considered, after the first round of the game, a modification concerning taking more time to shoot were then more likely to implement this behavior in the second round. This was related to an improvement in performance, which however was not significantly greater than that demonstrated by other participants. Importantly, these experiments were conducted using a novel experimental task, designed to take into account all the possible confounding factors that, as pointed out in recent years (Roese & Epstude, 2017), may have limited the relevance of previous findings not supporting FT's predictions. In Chapter 4, a series of three experiments were reported that offered insights on the relevance of preparatory goals to observe a genuine interest in seeking counterfactual information. In particular, Experiments 4, 5, and 6 were aimed at exploring, in both younger and older adults, non-instrumental counterfactual information seeking, that is the act of pursuing information about forgone alternatives even when this is not useful to improve the outcome of future decisions.

The results indicated, among other findings, that individuals of both age groups are considerably interested in acquiring that type of information even without being incentivized to do so, especially when they experience a negative outcome and when the information is free.

The results of the present work contribute to the debate on the preparatory value of counterfactual thinking. The first contribution relates to the types of counterfactuals people produce when reflecting on an event they personally experienced in the recent past. We confirmed previous findings showing that people tend to produce uncontrollable modifications when contemplating how their recent performance could have been better (Girotto et al., 2007; Pighin et al., 2011; 2021), and such tendency was found to be particularly pronounced after having tackled a challenging task (and, consequently, having performed poorly). As noted in Chapter 2, the appreciable proportion of controllable counterfactuals produced in the easy version of our experimental task (Experiment 1) allows us to exclude the possibility that uncontrollable modifications, prevailing in the other versions of the task, were due to a shortage of elements under participants' control. In fact, the variation in the proportions of controllable and uncontrollable counterfactuals within the same experimental paradigm suggests that not only were a good number of both types of modifications available, but also that the modifications produced by participants were influenced by factors beyond task characteristics. These findings appear to us difficult to reconcile under the theoretical framework of FT. If counterfactual thinking is mostly aimed at preparing for the future, and controllable rather than uncontrollable counterfactuals are the most suited for this scope, people should preferentially produce the former rather than latter, especially after failures. Yet, even if controllable modifications were available to be considered, we observed the opposite trend. In this regard, it may be pointed out that in Experiment 1, where the tendency to produce uncontrollable counterfactuals manifested most evidently, participants were not expected to (and, indeed, did not) repeat the game. Thus, in that situation there was no future event for which one should have prepared, and, because of this, uncontrollable modifications may have been produced. However, the percentage of uncontrollable counterfactuals in the Intermediate condition of Experiment 1 was fully comparable to that of Experiment 3, where the intermediate version of the game was employed as well, but in which case participants were informed they would repeat the task. Thus, the expected repeatability of the performance does not appear to have played a role in influencing the content of the counterfactuals produced.

The second major contribution of the present work is its examination of FT's claim that producing counterfactuals should have beneficial effects on future performances, a proposal that has received mixed support in previous studies. To our knowledge, the literature still lacked precise tests of the content-neutral and the content-specific pathway, especially ones that would address the concerns that have been raised about previous studies. What our results suggest is that generating a counterfactual modification (be it controllable or uncontrollable) does not appear to produce, *per se*, any particular benefit in terms of performance improvement. As discussed in Chapter 3, the overall picture that emerges is that a profound re-examination of the nature of these two pathways and of the mechanisms through which they operate is needed. Indeed, it may be the case that part of the conflicting findings that are present in the literature might be due to a lack of precision in the way in which the two pathways have been characterized, giving way to a certain degree of arbitrariness when it comes to defining what results should be expected based on them. This situation may be the result, in part, of the long history of FT, that during the years has undergone various changes to take into account the findings offered by new studies (Epstude & Roese, 2008, 2011; Roese, 1994; Roese & Epstude, 2017). Such changes, operated to modify specific aspects of the theory, may have not always been done organically, potentially introducing some ambiguity in certain aspects of it. Thus, more efforts should be devoted in the future to theoretically redefine and then empirically test the ways through which counterfactuals may aid individuals in future event. For example, as suggested in the discussion of Experiment 3, a potentially interesting aspect to consider is the level of understanding that participants have reached about an event they experienced in the past, which may altogether shape the content of a counterfactual and its effect on future behaviors. Or again, it would be interesting to better reason on the relationship between the content-neutral and the content-specific pathways. While it has been stated that they are not mutually exclusive (and that, in fact, they can interact with one another, see e.g., Epstude & Roese, 2008), it is not clear what factors would make a counterfactual more likely to act through the former or the latter. Theoretical and empirical work is needed to address this gap, as it is essential for a more complete understanding of how counterfactual thinking contributes to performance improvement, as FT proposes.

Finally, a third, more indirect contribution is represented by the results of Experiments 4 to 6. In line with previous works, people were found to voluntarily seek counterfactual information even when it was non-instrumental and, to a certain extent, even when they had to incur costs to acquire it. This suggests that counterfactual thinking, which is at the base of counterfactual information seeking, can be perceived as compelling also in situations where no preparatory goal can be active. Of course, the behavior studied in Experiments 4 to 6 cannot be deemed a proper instance of spontaneous counterfactual thinking, since participants were explicitly offered the possibility to consider the outcome of the forgone option. However, they also had no incentives to do that, so their willingness to invest time (and, to a certain extent, even part of a monetary endowment) to acquire such counterfactual information is at least indicative of a substantial, intrinsically motivated interest towards it. As noted in Chapter 4, running tightly controlled experiments aimed at investigating the main function served by spontaneous counterfactuals is extremely challenging, and it is thus possible that results pointing convincingly in the direction of a certain function rather than another may not be within reach. Nevertheless, the findings of Experiments 4 to 6 add to others, such as the ones based on self-report methods presented in Branch and Zickar (2021), in suggesting that preparatory goals need not to be present to experience compelling instances of counterfactual thinking. In this regard, it would be worth to explore in more detail the account focusing on an emotion regulation function as a possible motive behind behaviors such as non-instrumental counterfactual information seeking (e.g., Shani & Zeelenberg, 2012). While the experiments presented in Chapter 4 were not designed to provide a definitive test of this account, the results observed were only partly consistent with it, suggesting that further work may be needed to better define it. Finally, a more general point is worth being considered relatively to the debate around spontaneous versus prompted instances of counterfactual thinking: while it can make sense to distinguish them, it is essential to note that no study has directly compared spontaneously produced counterfactuals with those generated after a prompt. Until such data becomes available in the literature, it is advisable not to assume that findings from studies on prompted instances of counterfactual thinking would not apply to more spontaneous forms (as instead appears to be suggested by the proponents of FT, Roese & Epstude, 2017). It's possible that while spontaneous and prompted counterfactual thinking may differ in some respects, these differences would not undermine the significance of findings obtained from studying the latter form.

Should the present results be supported by future work, the notion that the primary function of counterfactual thinking is preparatory would likely fall by the wayside. But if counterfactuals are not produced in a preparatory fashion, what would be the main reason people wonder about alternatives to past events? While we recognize that counterfactual thinking is a goal-oriented process that can be employed to pursue a variety of aims, our data do not provide support for some often-cited functions. In particular, in Experiments 1 and 2, the consistent presence of uncontrollable counterfactuals runs counter to the argument that people produce these thoughts mainly to restore feelings of control, and the fact that such uncontrollable modifications are often internally focused does not fit with the idea that counterfactual thinking is used predominantly to protect self-esteem. Our view is that an explanatory, sense-making function is a promising candidate as the main driver of the production and the features characterizing counterfactuals (especially when in a conditional form). According to this, the purpose of the causal search at the core of counterfactual thinking would not be as much to understand how to do better in the future as to build a simple and efficient representation of the past that allows people to figure out why things turned out in one way rather than another. The idea that counterfactuals can serve a sense-making function is certainly not new (e.g., Byrne, 2016; Mandel, 2011; Roese & Vohs, 2012) and has points of contact with the long research tradition that aims at investigating the relationship between counterfactual thinking and causal reasoning (e.g., Frosch & Byrne, 2012; Gerstenberg et al., 2021; Mandel & Lehman, 1996; Quillien & Lucas, 2023; Wells &

Gavanski, 1989). In our opinion, this framework would offer a more coherent account of various results observed in the present work, especially referring to those presented in Chapters 2 and 3. For example, the production of uncontrollable counterfactuals would be well motivated whenever individuals considered an element outside their control as the main determinant for a past event that produced a certain outcome rather than a better one. In other words, in producing a counterfactual, the controllability of elements would not be a priority. What mattered most would be the perceived impact of these elements on the occurrence of the factual outcome in favor of a more desirable, counterfactual one (for a similar perspective, see Petrocelli et al., 2011). As suggested above, when the task tackled is particularly challenging, uncontrollable modifications – such as those that refer to the features that make the game difficult – might be more salient than possible controllable modifications. In such cases, individuals may have the impression that any action they could have implemented would not have made any difference. Accordingly, a counterfactual focused on said actions would present a poor explanation for such an unfavorable outcome (resulting in a semifactual, i.e., in an "even if…" thought in which the modification of an element of the past event would have still resulted in the same outcome; for a similar point see McCloy & Byrne, 2002), and it would, therefore, be less likely to be produced. Additionally, since formulating explanations is a crucial part of the ability to predict and control the future (e.g., Gopnik, 2000; Keil, 2006), an explanatory hypothesis would not exclude the idea that engaging in counterfactual thinking can also have beneficial effects on future performances. It is straightforward to consider how a counterfactual produced to explain a past event could have content-specific effects, just as suggested in FT. The more compelling it is that having implemented a certain action in the past would have led to a better outcome, the more likely it is that such an action will be implemented in the future. From this perspective, behavioral intentions would be determined by the general level of understanding achieved about the event under analysis, rather than by the production of specific counterfactual modifications. A consequence of this is that, even when counterfactuals concern elements outside people's control, the understanding acquired through the causal search underlying these counterfactuals might still provide individuals with ways to deal with such uncontrollable "obstacles," opening the possibility of future improvements. In any case, the preparatory effects of counterfactuals might then be one of the downstream consequences of a more general aim motivating the production of this type of mental simulation, that is, the goal of understanding and making sense of the situation experienced, a crucial driver for human cognition (Chater & Loewenstein, 2016). Future research should test the sense-making hypothesis proposed here, as well as its novel predictions. To do so, however, we believe that counterfactual thinking should be conceptualized and investigated differently from its study to this point, including moving away from the categorization of counterfactual thoughts into discrete qualitative classes (such as controllable vs. uncontrollable) and giving more attention to quantitative attributes like perceived plausibility (e.g., see De Brigard et al., 2021; Tasso & Cherubini, 2007). Perceived plausibility (in its various forms; see for example Connell & Keane, 2006) is central in the literature investigating how people evaluate explanations (e.g., Johnson et al., 2019; Khemlani et al., 2011; Read & Marcus-Newhall, 1993; Vrantsidis & Lombrozo, 2022; Zemla et al., 2017). The development of a measure to capture the plausibility of certain counterfactual scenarios would allow pitting against each other the sense-making and the preparatory accounts. With regard to the production of counterfactuals, indeed, the preparatory account would predict that, on average, a controllable modification should be preferred over an uncontrollable one, even if the former is less plausible than the latter. On the contrary, the sense-making account would predict that the more plausible of the two counterfactuals should be preferred, independent of their controllability. By orthogonally manipulating the controllability and the plausibility of counterfactual modifications, it would then be possible to assess which of the two accounts better captures the mental simulation of past events. In a similar vein, it might be interesting to also compare the sense-making and the emotion regulation accounts, for example by evaluating whether counterfactuals that are particularly suited to ameliorate one's emotional state (e.g., counterfactuals about externally uncontrollable elements that might be useful to avoid self-blame) are still produced even when deemed not particularly plausible.

As a final point, it should be noted that the sense-making hypothesis is being put forward mainly to account for features of counterfactual thinking such as its content and its effects on future behaviors, which were the focus of the experiments presented in Chapters 2 and 3. That is, for features that pertain to situations in which counterfactual thoughts are explicitly articulated, especially in a conditional format. However, while "if only" thoughts are by far the most studied instances of counterfactual thinking in the psychological literature, they do not represent the only exemplars of this kind of mental simulation. For example, the phenomenon investigated in Chapter 4, that is the search for counterfactual information, presents a form of counterfactual thought that is more implicit, arising seemingly automatically from the experience of a negative outcome and that probably does not entail any causal analysis of the past event, but just the urge to fill the gap in the knowledge of "what would have been" (related to this point, see Loewenstein, 1994, 2006). This is to say that, to produce a useful and sound hypothesis about the function of counterfactual thoughts, a first, necessary step would likely be to define what instances of the broader psychological process called "counterfactual thinking" we are interested to make sense of. More in general, this field of research would probably benefit from systematizing and expanding what is known about these different instances. In this way, it would be easier to understand, for example, whether counterfactual thinking is a homogenous process, in which the comparison between what is and what would have been only differ superficially across its various instances, or whether the differences between such instances are more profound, possibly suggesting the need to treat them as related but distinct phenomena (on this point, see Mandel, 2011).

References

Bacon, A. M., Walsh, C. R., & A. Briazu, R. (2020). Looking behind and looking ahead: Personality differences in counterfactual and prefactual thinking. *Imagination, Cognition and Personality*, *40*(2), 111–125. https://doi.org/10.1177/0276236620905810

Bacon, A. M., Walsh, C. R., & Martin, L. (2013). Fantasy proneness and counterfactual thinking. *Personality and Individual Differences*, *54*(4), 469–473. https://doi.org/10.1016/j.paid.2012.09.023

Bandura, A. (1977). Self-efficacy: Toward a unifying theory of behavioral change. *Psychological Review*, *84*, 191–215. https://doi.org/10.1037/0033-295X.84.2.191

Beck, S. R. (2020). The subject of children’s counterfactual thoughts. *Psychology of Consciousness: Theory, Research, and Practice*, *74*(4), 340–350. https://doi.org/10.1037/cns0000235

Beck, S. R., & Riggs, K. J. (2014). Developing thoughts about what might have been. *Child Development Perspectives*, *8*(3), 175–179. https://doi.org/10.1111/cdep.12082

Beck, S. R., Riggs, K. J., & Burns, P. (2011). Multiple developments in counterfactual thinking. In C. Hoerl, T. McCormack, & S. R. Beck (Eds.), *Understanding counterfactuals, understanding causation*. Oxford University Press.

Bennett, D., Bode, S., Brydevall, M., Warren, H., & Murawski, C. (2016). Intrinsic valuation of information in decision making under uncertainty. *PLOS Computational Biology*, *12*(7), e1005020. https://doi.org/10.1371/journal.pcbi.1005020

Branch, J. D., & Anderson, R. B. (2020, January 27). What could have been or what will be: A test of the functional theory of counterfactual thinking. *PsyArXiv*. https://doi.org/doi.org/10.31234/osf.io/a4p6t

Branch, J. G., & Zickar, M. J. (2021). Mental time travel into the episodic future, episodic past, and episodic counterfactual past in everyday life. *Applied Cognitive Psychology*, *35*(1), 293–299. https://doi.org/10.1002/acp.3765

Brassen, S., Gamer, M., Peters, J., Gluth, S., & Büchel, C. (2012). Don't Look Back in Anger! Responsiveness to missed chances in successful and nonsuccessful aging. *Science*, *336*(6081), 612–614. https://doi.org/10.1126/science.1217516

Broomhall, A. G., Phillips, W. J., Hine, D. W., & Loi, N. M. (2017). Upward counterfactual thinking and depression: A meta-analysis. *Clinical Psychology Review*, *55*, 56–73. https://doi.org/10.1016/j.cpr.2017.04.010

Brysbaert, M. (2019). How many participants do we have to include in properly powered experiments? A tutorial of power analysis with some simple guidelines. *Journal of Cognition*, *2*(1), 1–38. https://doi.org/10.5334/joc.72

Byrne, R. M. J. (2005). *The rational imagination. How people create alternatives to reality*. MIT Press.

Byrne, R. M. J. (2016). Counterfactual thought. *Annual Review of Psychology*, *67*, 135–157.

Byrne, R. M. J., & McEleney, A. (2000). Counterfactual thinking about actions and failures to act. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *26*(5), 1318–1331. APA PsycArticles®. https://doi.org/10.1037/0278-7393.26.5.1318

Byrne, R. M. J., Segura, S., Culhane, R., Tasso, A., & Berrocal, P. (2000). The temporality effect in counterfactual thinking about what might have been. *Memory & Cognition*, *28*(2), 264–281. https://doi.org/10.3758/BF03213805

Campbell, K. L., Lustig, C., & Hasher, L. (2020). Aging and inhibition: Introduction to the special issue. *Psychology and Aging*, *35*, 605–613. https://doi.org/10.1037/pag0000564

Catellani, P., & Bertolotti, M. (2014). The effects of counterfactual defences on social judgements. *European Journal of Social Psychology*, *44*(1), 82–92.

Catellani, P., & Covelli, V. (2013). The strategic use of counterfactual communication in politics. *Journal of Language and Social Psychology*, *32*(4), 480–489. https://doi.org/10.1177/0261927X13495548

Charpentier, C. J., Bromberg-Martin, E. S., & Sharot, T. (2018). Valuation of knowledge and ignorance in mesolimbic reward circuitry. *Proceedings of the National Academy of Sciences*, *115*(31), E7255–E7264. https://doi.org/10.1073/pnas.1800547115

Chater, N., & Loewenstein, G. (2016). The under-appreciated drive for sense-making. *Journal of Economic Behavior & Organization*, *126*, 137–154. https://doi.org/10.1016/j.jebo.2015.10.016

Connell, L., & Keane, M. T. (2006). A model of plausibility. *Cognitive Science*, *30*, 95–120. https://doi.org/10.1207/s15516709cog0000_53

Connolly, T., & Zeelenberg, M. (2002). Regret in decision making. *Current Directions in Psychological Science*, *11*(6), 212–216. https://doi.org/10.1111/1467-8721.00203

Coricelli, G., & Rustichini, A. (2010). Counterfactual thinking and emotions: Regret and envy learning. *Philosophical Transactions of the Royal Society B: Biological Sciences*, *365*(1538), 241–247. https://doi.org/10.1098/rstb.2009.0159

da Silva Frost, A., & Ledgerwood, A. (2020). Calibrate your confidence in research findings: A tutorial on improving research methods and practices. *Journal of Pacific Rim Psychology*, *14*, e14. Cambridge Core. https://doi.org/10.1017/prp.2020.7

Dahling, J. J., & Ruppel, C. L. (2016). Learning goal orientation buffers the effects of negative normative feedback on test self-efficacy and reattempt interest. *Learning and Individual Differences*, *50*, 296–301. https://doi.org/10.1016/j.lindif.2016.08.022

Davis, C. G., Lehman, D. R., Wortman, C. B., Silver, R. C., & Thompson, S. C. (1995). The undoing of traumatic life events. *Personality and Social Psychology Bulletin*, *21*(2), 109–124. https://doi.org/10.1177/0146167295212002

De Brigard, F., Gessell, B., Yang, B. W., Stewart, G., & Marsh, E. J. (2020). Remembering possible times: Memory for details of past, future, and counterfactual simulations. *Psychology of Consciousness: Theory, Research, and Practice*, *7*(4), 331–339. APA PsycArticles®. https://doi.org/10.1037/cns0000220

De Brigard, F., Giovanello, K. S., Stewart, G. W., Lockrow, A. W., O'Brien, M. M., & Spreng, R. N. (2016). Characterizing the subjective experience of episodic past, future, and counterfactual thinking in healthy younger and older adults. *Quarterly Journal of Experimental Psychology*, *69*(12), 2358–2375. https://doi.org/10.1080/17470218.2015.1115529

De Brigard, F., Henne, P., & Stanley, M. L. (2021). Perceived similarity of imagined possible worlds affects judgments of counterfactual plausibility. *Cognition*, *209*, 104574. https://doi.org/10.1016/j.cognition.2020.104574

De Brigard, F., & Parikh, N. (2019). Episodic counterfactual thinking. *Current Directions in Psychological Science*, *28*(1), 59–66.

De Brigard, F., Szpunar, K. K., & Schacter, D. L. (2013). Coming to Grips With the Past: Effect of Repeated Simulation on the Perceived Plausibility of Episodic Counterfactual Thoughts. *Psychological Science*, *24*(7), 1329–1334. https://doi.org/10.1177/0956797612468163

Ditrich, L., Landkammer, F., & Sassenberg, K. (2019). What if my colleague was wrong and I was right? The impact of counterfactual mindsets and interpersonal focus on written communication and decision making in a hidden profile task. *Acta Psychologica*, *192*, 118–125. https://doi.org/10.1016/j.actpsy.2018.11.003

Dixon, J. E., & Byrne, R. M. J. (2011). “If only” counterfactual thoughts about exceptional actions. *Memory & Cognition*, *39*(7), 1317–1331. https://doi.org/10.3758/s13421-011-0101-4

Doan, T., Denison, S., & Friedman, O. (2023). Two kinds of counterfactual closeness. *Journal of Experimental Psychology: General*. APA PsycArticles®. https://doi.org/10.1037/xge0001369

Effron, D. A. (2018). It could have been true: How counterfactual thoughts reduce condemnation of falsehoods and increase political polarization. *Personality and Social Psychology Bulletin*, *44*(5), 729–745. https://doi.org/10.1177/0146167217746152

Egan, S. M., & Byrne, R. M. J. (2012). Inferences from counterfactual threats and promises. *Experimental Psychology*, *59*(4), 227–235. https://doi.org/10.1027/1618-3169/a000147

Elster, J. (1999). *Alchemies of the mind*. Cambridge University Press.

Epstude, K., & Roese, N. J. (2008). The functional theory of counterfactual thinking. *Personality and Social Psychology Review*, *12*(2), 168–192. https://doi.org/10.1177/1088868308316091

Epstude, K., & Roese, N. J. (2011). When goal pursuit fails: The functions of counterfactual thought in intention formation. *Social Psychology*, *42*(2), 19–27. https://doi.org/10.1027/1864-9335/a000039

Epstude, K., Scholl, A., & Roese, N. J. (2016). Prefactual thoughts: Mental simulations about what might happen. *Review of General Psychology*, *20*(1), 48–56. https://doi.org/10.1037/gpr0000064

Escarti, A., & Guzman, J. F. (1999). Effects of feedback on self-efficacy, performance, and choice in an athletic task. *Journal of Applied Sport Psychology, 11*(1), 83–96. https://doi.org/10.1080/10413209908402952

Ferguson, H. J. (2012). Eye movements reveal rapid concurrent access to factual and counterfactual interpretations of the world. *The Quarterly Journal of Experimental Psychology*, *65*(5), 939–961. https://doi.org/10.1080/17470218.2011.637632

Fernandez, D. K., Gan, H. H. M., & Chan, A. Y. C. (2022). Comparing the functional benefits of counterfactual and prefactual thinking: The content-specific and content-neutral pathways. *Thinking & Reasoning*, *28*(2), 261–289. https://doi.org/10.1080/13546783.2021.1989034

Ferrante, D., Stragà, M., Girotto, V., & Walsh, C. (2013). Improving the past and the future: A temporal asymmetry in hypothetical thinking. *Journal of Experimental Psychology: General*, *142*(1), 23–27. https://doi.org/10.1037/a0027947

Fillon, A., Kutscher, L., & Feldman, G. (2021). Impact of past behaviour normality: Meta-analysis of exceptionality effect. *Cognition and Emotion*, *35*(1), 129–149. https://doi.org/10.1080/02699931.2020.1816910

FitzGibbon, L., Komiya, A., & Murayama, K. (2021). The lure of counterfactual curiosity: People incur a cost to experience regret. *Psychological Science*, *32*(2), 241–255. https://doi.org/10.1177/095679762096361

Fitzgibbon, L., & Murayama, K. (2022). Counterfactual curiosity: Motivated thinking about what might have been. *Philosophical Transactions of the Royal Society B: Biological Sciences*, *377*(1866), 20210340. https://doi.org/10.1098/rstb.2021.0340

Frosch, C. A., & Byrne, R. M. J. (2012). Causal conditionals and counterfactuals. *Acta Psychologica*, *141*(1), 54–66. https://doi.org/10.1016/j.actpsy.2012.07.001

Frosch, C. A., Egan, S. M., & Hancock, E. N. (2015). The effect of controllability and causality on counterfactual thinking. *Thinking & Reasoning*, *21*(3), 317–340. https://doi.org/10.1080/13546783.2014.976268

Galinsky, A. D., & Moskowitz, G. B. (2000). Counterfactuals as behavioral primes: Priming the simulation heuristic and consideration of alternatives. *Journal of Experimental Social Psychology*, *36*(4), 384–409. https://doi.org/10.1006/jesp.1999.1409

Gerlach, K. D., Dornblaser, D. W., & Schacter, D. L. (2014). Adaptive constructive processes and memory accuracy: Consequences of counterfactual simulations in young and older adults. *Memory*, *22*(1), 145–162. https://doi.org/10.1080/09658211.2013.779381

Gerstenberg, T., Goodman, N. D., Lagnado, D. A., & Tenenbaum, J. B. (2021). A counterfactual simulation model of causal judgments for physical events. *Psychological Review*, *128*(5), 936–975. https://doi.org/doi.org/10.1037/rev0000281

Gigerenzer, G., & Garcia-Retamero, R. (2017). Cassandra's regret: The psychology of not wanting to know. *Psychological Review*, *124*(2), 179–196. APA PsycArticles®. https://doi.org/10.1037/rev0000055

Gilovich, T., & Medvec, V. H. (1994). The temporal pattern to the experience of regret. *Journal of Personality and Social Psychology*, *67*, 357–365. https://doi.org/10.1037/0022-3514.67.3.357

Girotto, V., Ferrante, D., Pighin, S., & Gonzalez, M. (2007). Postdecisional counterfactual thinking by actors and readers. *Psychological Science*, *18*(6), 510–515. https://doi.org/10.1111/j.1467-9280.2007.01931.x

Girotto, V., Legrenzi, P., & Rizzo, A. (1991). Event controllability in counterfactual thinking. *Acta Psychologica*, *78*(1), 111–133. https://doi.org/10.1016/0001-6918(91)90007-M

Gleicher, F., Kost, K. A., Baker, S. M., Strathman, A. J., Richman, S. A., & Sherman, S. J. (1990). The role of counterfactual thinking in judgments of affect. *Personality and Social Psychology*

Bulletin, *16*(2), 284–295. https://doi.org/10.1177/0146167290162009

Goerke, M., Möller, J., Schulz-Hardt, S., Napiersky, U., & Frey, D. (2004). 'It’s not my fault-but only I can change it": Counterfactual and prefactual thoughts of managers. *Journal of Applied Psychology*, *89*, 279–292. https://doi.org/10.1037/0021-9010.89.2.279

Goldinger, S. D., Kleider, H. M., Azuma, T., & Beike, D. R. (2003). “Blaming the victim” under memory load. *Psychological Science*, *14*(1), 81–85. https://doi.org/10.1111/1467-9280.01423

Gollwitzer, P. M. (1999). Implementation intentions: Strong effects of simple plans. *American Psychologist*, *54*(7), 493–503. https://doi.org/10.1037/0003-066X.54.7.493

Gopnik, A. (2000). Explanation as orgasm and the drive for causal knowledge: The function, evolution, and phenomenology of the theory formation system. In *Explanation and cognition.* (pp. 299–323). The MIT Press.

Gottlieb, J., & Oudeyer, P.-Y. (2018). Towards a neuroscience of active sampling and curiosity. *Nature Reviews Neuroscience*, *19*(12), 758–770. https://doi.org/10.1038/s41583-018-0078-0

Green, P., & MacLeod, C. J. (2016). SIMR: An R package for power analysis of generalized linear mixed models by simulation. *Methods in Ecology and Evolution*, *7*(4), 493–498.

Hammell, C., & Chan, A. Y. C. (2016). Improving physical task performance with counterfactual and prefactual thinking. *PLoS ONE*, *11*(12), e0231278. https://doi.org/10.1371/journal.pone.0168181

Henne, P., Niemi, L., Pinillos, Á., De Brigard, F., & Knobe, J. (2019). A counterfactual explanation for the action effect in causal judgment. *Cognition*, *190*, 157–164. https://doi.org/10.1016/j.cognition.2019.05.006

Hertwig, R., & Engel, C. (2016). Homo Ignorans: Deliberately choosing not to know. *Perspectives on Psychological Science*, *11*(3), 359–372.

Hertwig, R., Woike, J. K., & Schupp, J. (2021). Age differences in deliberate ignorance. *Psychology and Aging*, *36*(4), 407–414. APA PsycArticles®. https://doi.org/10.1037/pag0000603

Horhota, M., Mienaltowski, A., & Blanchard-Fields, F. (2012). If only I had taken my usual route…: Age-related differences in counter-factual thinking. *Aging, Neuropsychology, and*

Cognition, *19*(3), 339–361. https://doi.org/10.1080/13825585.2011.615904

Hsee, C. K., & Ruan, B. (2016). The Pandora Effect: The power and peril of curiosity. *Psychological Science*, *27*(5), 659–666. https://doi.org/10.1177/0956797616631733

Johnson, M. M. S. (1990). Age differences in decision making: A process methodology for examining strategic information processing. *Journal of Gerontology*, *45*(2), P75–P78. https://doi.org/10.1093/geronj/45.2.P75

Johnson, S. G. B., Valenti, J. J., & Keil, F. C. (2019). Simplicity and complexity preferences in causal explanation: An opponent heuristic account. *Cognitive Psychology*, *113*, 101222. https://doi.org/10.1016/j.cogpsych.2019.05.004

Kahneman, D. (1995). Varieties of counterfactual thinking. In N. J. Roese & J. M. Olson (Eds.), *What might have been: The social psychology of counterfactual thinking* (pp. 375–396). Erlbaum.

Kahneman, D., & Miller, D. T. (1986). Norm theory: Comparing reality to its alternatives. *Psychological Review*, *93*(2), 136–135. https://doi.org/10.1037/0033-295X.93.2.136

Kahneman, D., & Tversky, A. (1982a). The psychology of preferences. *Scientific American*, *246*(1), 160–173. JSTOR.

Kahneman, D., & Tversky, A. (1982b). The simulation heuristic. In *Judgment under Uncertainty: Heuristics and Biases* (pp. 201–208). Cambridge University Press.

Keil, F. C. (2006). Explanation and understanding. *Annual Review of Psychology*, *57*, 227–254. https://doi.org/10.1146/annurev.psych.57.102904.190100

Khemlani, S. S., Sussman, A. B., & Oppenheimer, D. M. (2011). Harry Potter and the sorcerer's scope: Latent scope biases in explanatory reasoning. *Memory & Cognition*, *39*(3), 527–535. https://doi.org/10.3758/s13421-010-0028-1

Kumle, L., Võ, M. L.-H., & Draschkow, D. (2021). Estimating power in (generalized) linear mixed models: An open introduction and tutorial in R. *Behavior Research Methods*, *53*(6), 2528–2543. https://doi.org/10.3758/s13428-021-01546-0

Leiner, D. J. (2019). SoSci Survey (Version 3.1.06) [Computer software].

https://www.soscisurvey.de

Lejuez, C. W., Read, J. P., Kahler, C. W., Richards, J. B., Ramsey, S. E., Stuart, G. L., Strong, D. R., & Brown, R. A. (2002). Evaluation of a behavioral measure of risk taking: The Balloon Analogue Risk Task (BART). *Journal of Experimental Psychology: Applied*, *8*(2), 75–84. APA PsycArticles®. https://doi.org/10.1037/1076-898X.8.2.75

Levin, F., Fiedler, S., & Weber, B. (2021). Positivity effect and decision making in ageing. *Cognition and Emotion*, *35*(4), 790–804. https://doi.org/10.1080/02699931.2021.1884533

Liew, S. X., Embrey, J. R., Navarro, D. J., & Newell, B. R. (2022). Comparing anticipation and uncertainty-penalty accounts of noninstrumental information seeking. *Decision*, 247–267. https://doi.org/10.1037/dec0000179

Liu, X., Peng, H., & Pu, B. (2020). Age differences in the decision information search: The roles of task complexity and task relevance. *Cognitive Development*, *54*, 100877. https://doi.org/10.1016/j.cogdev.2020.100877

Löckenhoff, C. E., & Carstensen, L. L. (2007). Aging, emotion, and health-related decision strategies: Motivational manipulations can reduce age differences. *Psychology and Aging*, *22*(1), 134–146. APA PsycArticles®. https://doi.org/10.1037/0882-7974.22.1.134

Löckenhoff, C. E., & Carstensen, L. L. (2008). Decision strategies in health care choices for self and others: Older but not younger adults make adjustments for the age of the decision target. *The Journals of Gerontology: Series B*, *63*(2), P106–P109. https://doi.org/10.1093/geronb/63.2.P106

Loewenstein, G. (1994). The psychology of curiosity: A review and reinterpretation. *Psychological Bulletin*, *116*(1), 75–98. APA PsycArticles®. https://doi.org/10.1037/0033-2909.116.1.75

Loewenstein, G. (2006). The Pleasures and Pains of Information. *Science*, *312*(5774), 704–706. https://doi.org/10.1126/science.1128388

López-Astorga, M., Ragni, M., & Johnson-Laird, P. N. (2022). The probability of conditionals: A review. *Psychonomic Bulletin & Review*, *29*(1), 1–20. https://doi.org/10.3758/s13423-021-01938-5

Lucas, C. G., & Kemp, C. (2015). An improved probabilistic account of counterfactual reasoning. *Psychological Review*, *122*(4), 700–734. APA PsycArticles®. https://doi.org/10.1037/a0039655

Maloney, D. M., & Egan, S. M. (2017). The effect of autonomy on counterfactual thinking about controllable events. *Journal of Cognitive Psychology*, *29*(3), 337–351. https://doi.org/10.1080/20445911.2016.1277731

Mandel, D. R. (2011). Mental simulation and the nexus of causal and counterfactual explanation. In C. Hoerl, T. McCormack, & S. R. Beck (Eds.), *Understanding Counterfactuals, Understanding Causation: Issues in Philosophy and Psychology* (pp. 147–170). Oxford University Press. https://doi.org/10.1093/acprof:oso/9780199590698.003.0008

Mandel, D. R., & Lehman, D. R. (1996). Counterfactual thinking and ascriptions of cause and preventability. *Journal of Personality and Social Psychology*, *71*, 450–463. https://doi.org/10.1037/0022-3514.71.3.450

Markman, K. D., Lindberg, M. J., Kray, L. J., & Galinsky, A. D. (2007). Implications of counterfactual structure for creative generation and analytical problem solving. *Personality and Social Psychology Bulletin*, *33*(3), 312–324. https://doi.org/10.1177/0146167206296106

Markman, K. D., McMullen, M. N., & Elizaga, R. A. (2008). Counterfactual thinking, persistence, and performance: A test of the Reflection and Evaluation Model. *Journal of Experimental Social Psychology*, *44*(2), 421–428. https://doi.org/10.1016/j.jesp.2007.01.001

Mata, R., & Nunes, L. (2010). When less is enough: Cognitive aging, information search, and decision quality in consumer choice. *Psychology and Aging*, *25*, 289–298. https://doi.org/10.1037/a0017927

Mata, R., Schooler, L. J., & Rieskamp, J. (2007). The aging decision maker: Cognitive aging and the adaptive selection of decision strategies. *Psychology and Aging*, *22*, 796–810. https://doi.org/10.1037/0882-7974.22.4.796

Mather, M., & Carstensen, L. L. (2005). Aging and motivated cognition: The positivity effect in attention and memory. *Trends in Cognitive Sciences*, *9*(10), 496–502.

https://doi.org/10.1016/j.tics.2005.08.005

McCloy, R., & Byrne, R. M. J. (2000). Counterfactual thinking about controllable events. *Memory & Cognition*, *28*(6), 1071–1078. https://doi.org/10.3758/BF03209355

McCloy, R., & Byrne, R. M. J. (2002). Semifactual "even if" thinking. *Thinking & Reasoning*, *8*(1), 41–67. https://doi.org/10.1080/13546780143000125

McCrea, S. M. (2007). Counterfactual thinking following negative outcomes: Evidence for group and self-protective biases. *European Journal of Social Psychology*, *37*(6), 1256–1271. https://doi.org/10.1002/ejsp.462

McCrea, S. M. (2008). Self-handicapping, excuse making, and counterfactual thinking: Consequences for self-esteem and future motivation. *Journal of Personality and Social Psychology*, *95*(2), 274–292. https://doi.org/10.1037/0022-3514.95.2.274

McCulloch, K. C., & Smallman, R. (2014). The implications of counterfactual mind-sets for the functioning of implementation intentions. *Motivation and Emotion*, *38*(5), 635–644. https://doi.org/10.1007/s11031-014-9408-3

McEleney, A., & Byrne, R. M. J. (2006). Spontaneous counterfactual thoughts and causal explanations. *Thinking & Reasoning*, *12*(2), 235–255. https://doi.org/10.1080/13546780500317897

Mechera-Ostrovsky, T., Liew, S. X., & Newell, B. R. (2023). The role of risk, regret, and rejoice in non-instrumental information seeking. *Journal of Behavioral Decision Making*, *36*(1), e2294. https://doi.org/10.1002/bdm.2294

Melnyk, D., & Shepperd, J. A. (2012). Avoiding risk information about breast cancer. *Annals of Behavioral Medicine*, *44*(2), 216–224. https://doi.org/10.1007/s12160-012-9382-5

Mercier, H., Rolison, J. J., Stragà, M., Ferrante, D., Walsh, C., & Girotto, V. (2017). Questioning the preparatory function of counterfactual thinking. *Memory & Cognition*, *45*, 261–269. https://doi.org/10.3758/s13421-016-0660-5

Meyers-Levy, J., & Maheswaran, D. (1992). When timing matters: The influence of temporal distance on consumers' affective and persuasive responses. *Journal of Consumer Research*, *19*(3), 424–433. https://doi.org/10.1086/209312

Miller, D. T., & Gunasegaram, S. (1990). Temporal order and the perceived mutability of events: Implications for blame assignment. *Journal of Personality and Social Psychology*, *59*, 1111–1118. https://doi.org/10.1037/0022-3514.59.6.1111

Miller, T. (2019). Explanations in artificial intelligence: Insights from the social sciences. *Artificial Intelligence*, *267*, 1–38.

Myers, A. L., McCrea, S. M., & Tyser, M. P. (2014). The role of thought-content and mood in the preparative benefits of upward counterfactual thinking. *Motivation and Emotion*, *38*, 166–182. https://doi.org/10.1007/s11031-013-9362-5

Nasco, S. A., & Marsh, K. L. (1999). Gaining control through counterfactual thinking. *Personality and Social Psychology Bulletin*, *25*(5), 557–569. https://doi.org/10.1177/0146167299025005002

Office for National Statistics. (2023, March 31). *Age groups*. https://www.ethnicity-facts-figures.service.gov.uk/uk-population-by-ethnicity/demographics/age-groups/latest#median-age-by-ethnicity-over-time

Over, D. E., Hadjichristidis, C., Evans, J. St. B. T., Handley, S. J., & Sloman, S. A. (2007). The probability of causal conditionals. *Cognitive Psychology*, *54*(1), 62–97. https://doi.org/10.1016/j.cogpsych.2006.05.002

Özbek, M., Bohn, A., & Berntsen, D. (2017). Imagining the personal past: Episodic counterfactuals compared to episodic memories and episodic future projections. *Memory & Cognition*, *45*(3), 375–389. https://doi.org/10.3758/s13421-016-0671-2

Özbek, M., Bohn, A., & Berntsen, D. (2018). Why do I think and talk about it? Perceived functions and phenomenology of episodic counterfactual thinking compared with remembering and future thinking. *Quarterly Journal of Experimental Psychology*, *71*(10), 2101–2114. https://doi.org/10.1177/1747021817738731

Palan, S., & Schitter, C. (2018). Prolific.ac—A subject pool for online experiments. Journal of Behavioral and Experimental Finance, 17, 22–27. https://doi.org/10.1016/j.jbef.2017.12.004

Parikh, N., LaBar, K. S., & Brigard, F. D. (2020). Phenomenology of counterfactual thinking is dampened in anxious individuals. *Cognition and Emotion*, *34*(8), 1737–1745. https://doi.org/10.1080/02699931.2020.1802230

Parkinson, M., & Byrne, R. M. J. (2017). Counterfactual and semi-factual thoughts in moral judgements about failed attempts to harm. *Thinking & Reasoning*. https://doi.org/10.1080/13546783.2017.1345790

Petrocelli, J. V., & Crysel, L. C. (2009). Counterfactual thinking and confidence in blackjack: A test of the counterfactual inflation hypothesis. *Journal of Experimental Social Psychology*, *45*(6), 1312–1315. https://doi.org/10.1016/j.jesp.2009.08.004

Petrocelli, J. V., & Harris, A. K. (2011). Learning inhibition in the Monty Hall problem: The role of dysfunctional counterfactual prescriptions. *Personality and Social Psychology Bulletin*, *37*(10), 1297–1311. https://doi.org/10.1177/0146167211410245

Petrocelli, J. V., Percy, E. J., Sherman, S. J., & Tormala, Z. L. (2011). Counterfactual potency. *Journal of Personality and Social Psychology*, *100*(1), 30–46. https://doi.org/10.1037/a0021523

Petrocelli, J. V., Rubin, A. L., & Stevens, R. L. (2016). The sin of prediction: When mentally simulated alternatives compete with reality. *Personality and Social Psychology Bulletin*, *42*(12), 1635–1652. https://doi.org/10.1177/0146167216669122

Petrocelli, J. V., Seta, C. E., & Seta, J. J. (2013). Dysfunctional counterfactual thinking: When simulating alternatives to reality impedes experiential learning. *Thinking & Reasoning*, *19*(2), 205–230. https://doi.org/10.1080/13546783.2013.775073

Petrocelli, J. V., Seta, C. E., Seta, J. J., & Prince, L. B. (2012). "If only I could stop generating counterfactual thoughts": When counterfactual thinking interferes with academic performance. *Journal of Experimental Social Psychology*, *48*(5), 1117–1123. https://doi.org/10.1016/j.jesp.2012.03.017

Pighin, S., Byrne, R. M. J., Ferrante, D., Gonzalez, M., & Girotto, V. (2011). Counterfactual thoughts about experienced, observed, and narrated events. *Thinking & Reasoning*, *17*(2), 197–211.

https://doi.org/10.1080/13546783.2011.561598

Pighin, S., Byrne, R. M. J., & Tentori, K. (2021). "If only" counterfactual thoughts about cooperative and uncooperative decisions in social dilemmas. *Thinking & Reasoning*. https://doi.org/10.1080/13546783.2021.1961859

Queen, T. L., Hess, T. M., Ennis, G. E., Dowd, K., & Grühn, D. (2012). Information search and decision making: Effects of age and complexity on strategy use. *Psychology and Aging*, *27*(4), 817–824. APA PsycArticles®. https://doi.org/10.1037/a0028744

Quillien, T., & Lucas, C. G. (2023). Counterfactuals and the logic of causal selection. *Psychological Review*, Advance online publication. https://doi.org/10.1037/rev0000428

R Core Team. (2019). *R: A language and environment for statistical computing* (3.6.2) [Computer software]. R Foundation for Statistical Computing.

Raftery, A. E. (1995). Bayesian model selection in social research. *Sociological Methodology*, *25*, 111–163. JSTOR. https://doi.org/10.2307/271063

Read, S. J., & Marcus-Newhall, A. (1993). Explanatory coherence in social explanations: A parallel distributed processing account. *Journal of Personality and Social Psychology*, *65*, 429–447. https://doi.org/10.1037/0022-3514.65.3.429

Robinson, S. A., & Lachman, M. E. (2017). Perceived control and aging: A mini-review and directions for future research. *Gerontology*, *63*(5), 435–442. https://doi.org/10.1159/000468540

Roese, N. J. (1994). The functional basis of counterfactual thinking. *Journal of Personality and Social Psychology*, *66*(5), 805–818. https://doi.org/10.1037/0022-3514.66.5.805

Roese, N. J. (1997). Counterfactual thinking. *Psychological Bulletin*, *121*(1), 133–148. https://doi.org/10.1037/0033-2909.121.1.133

Roese, N. J., & Epstude, K. (2017). The functional theory of counterfactual thinking: New evidence, new challenges, new insights. In J. M. Olson (Ed.), *Advances in Experimental Social Psychology* (Vol. 56, pp. 1–79). Academic Press. https://doi.org/10.1016/bs.aesp.2017.02.001

Roese, N. J., & Hur, T. (1997). Affective determinants of counterfactual thinking. *Social*

Cognition, *15*(4), 274–290. https://doi.org/10.1521/soco.1997.15.4.274

Roese, N. J., Hur, T., & Pennington, G. L. (1999). Counterfactual thinking and regulatory focus: Implications for action versus inaction and sufficiency versus necessity. *Journal of Personality and Social Psychology*, *77*, 1109–1120. https://doi.org/10.1037/0022-3514.77.6.1109

Roese, N. J., & Olson, J. M. (1993a). Self-esteem and counterfactual thinking. *Journal of Personality and Social Psychology*, *65*(1), 199–206. APA PsycArticles®. https://doi.org/10.1037/0022-3514.65.1.199

Roese, N. J., & Olson, J. M. (1993b). The structure of counterfactual thought. *Personality and Social Psychology Bulletin*, *19*(3), 312–319. https://doi.org/10.1177/0146167293193008

Roese, N. J., & Olson, J. M. (1997). Counterfactual thinking: The intersection of affect and function. In M. P. Zanna (Ed.), *Advances in Experimental Social Psychology* (Vol. 29, pp. 1–59). Academic Press. https://doi.org/10.1016/S0065-2601(08)60015-5

Roese, N. J., Pennington, G. L., Coleman, J., Janicki, M., Li, N. P., & Kenrick, D. T. (2006). Sex differences in regret: All for love or some for lust? *Personality and Social Psychology Bulletin*, *32*(6), 770–780. https://doi.org/10.1177/0146167206286709

Roese, N. J., Smallman, R., & Epstude, K. (2017). Do episodic counterfactual thoughts focus on controllable action?: The role of self-initiation. *Journal of Experimental Social Psychology*, *73*, 14–23. https://doi.org/10.1016/j.jesp.2017.05.006

Roese, N. J., & Vohs, K. D. (2012). Hindsight bias. *Perspectives on Psychological Science*, *7*(5), 411–426. https://doi.org/10.1177/1745691612454303

Sakaki, M., Yagi, A., & Murayama, K. (2018). Curiosity in old age: A possible key to achieving adaptive aging. Neuroscience & Biobehavioral Reviews, 88, 106–116. https://doi.org/10.1016/j.neubiorev.2018.03.007

Sánchez-Meca, J., Marín-Martínez, F., & Chacón-Moscoso, S. (2003). Effect-size indices for dichotomized outcomes in meta-analysis. *Psychological Methods*, *8*(4), 448–467. APA PsycArticles®. https://doi.org/10.1037/1082-989X.8.4.448

Sanna, L. J., Parks, C. D., Meier, S., Chang, E. C., Kassin, B. R., Lechter, J. L., Turley-Ames, K. J., & Miyake, T. M. (2003). A game of inches: Spontaneous use ofcCounterfactuals by broadcasters during major league baseball playoffs. *Journal of Applied Social Psychology*, *33*(3), 455–475. https://doi.org/10.1111/j.1559-1816.2003.tb01906.x

Schacter, D. L. (2012). Adaptive constructive processes and the future of memory. *The American Psychologist*, *67*(8), 603–613. https://doi.org/10.1037/a0029869

Schacter, D. L., Benoit, R. G., Brigard, F. D., & Szpunar, K. K. (2015). Episodic future thinking and episodic counterfactual thinking: Intersections between memory and decisions. *Neurobiology of Learning and Memory*, *117*, 14–21. https://doi.org/10.1016/j.nlm.2013.12.008

Schutte, N. S., & Malouff, J. M. (2019). Increasing curiosity through autonomy of choice. *Motivation and Emotion*, *43*(4), 563–570. https://doi.org/10.1007/s11031-019-09758-w

Schwartz, N. (2011). Feelings-as-Information Theory. In P. A. M. Van Lange, E. T. Higgins, & A. W. Kruglanski (Eds.), *Handbook of Theories of Social Psychology*. Sage. http://digital.casalini.it/9781446269008

Segura, S., Fernandez-Berrocal, P., & Byrne, R. M. J. (2002). Temporal and causal order effects in thinking about what might have been. *The Quarterly Journal of Experimental Psychology Section A*, *55*(4), 1295–1305. https://doi.org/10.1080/02724980244000125

Shani, Y., & Zeelenberg, M. (2007). When and why do we want to know? How experienced regret promotes post-decision information search. *Journal of Behavioral Decision Making*, *20*(3), 207–222. https://doi.org/10.1002/bdm.550

Shani, Y., & Zeelenberg, M. (2012). Post-decisional information search: Balancing the pains of suspecting the worst with the comforts of knowing the worst. *Social Influence*, *7*(3), 193–210. https://doi.org/10.1080/15534510.2012.679219

Sharot, T., & Sunstein, C. R. (2020). How people decide what they want to know. *Nat Hum Behav*, *4*, 14–19. https://doi.org/10.1038/s41562-019-0793-1

Smallman, R., & Roese, N. J. (2009). Counterfactual thinking facilitates behavioral intentions.

Journal of Experimental Social Psychology, *45*(4), 845–852. https://doi.org/10.1016/j.jesp.2009.03.002

Smallman, R., & Summerville, A. (2018). Counterfactual thought in reasoning and performance. *Social and Personality Psychology Compass*, *12*(4), e12376. https://doi.org/10.1111/spc3.12376

Smallman, R., Summerville, A., & Lowe, J. C. (2022). The Spontaneous Counterfactual Inference task: Misremembering what might have been. *Personality and Social Psychology Bulletin*, 01461672221142181. https://doi.org/10.1177/01461672221142181

Son, Y., Buffone, A., Raso, J., Larche, A., Janocko, A., Zembroski, K., Schwartz, H. A., & Ungar, L. (2017). Recognizing counterfactual thinking in social media texts. *Proceedings of the 55th Annual Meeting of the Association for Computational Linguistics (Volume 2: Short Papers)*, 654–658. https://doi.org/10.18653/v1/P17-2103

Spellman, B. A. (1997). Crediting causality. *Journal of Experimental Psychology: General*, *126*, 323–348. https://doi.org/10.1037/0096-3445.126.4.323

Stanley, M. L., Cabeza, R., Smallman, R., & De Brigard, F. (2021). Memory and counterfactual simulations for past wrongdoings foster moral learning and improvement. *Cognitive Science*, *45*(6), e13007. https://doi.org/10.1111/cogs.13007

Stanley, M. L., Stewart, G. W., & Brigard, F. D. (2017). Counterfactual plausibility and comparative similarity. *Cognitive Science*, *41*(S5), 1216–1228. https://doi.org/10.1111/cogs.12451

Stevens, D., Anderson, D. I., O'Dwyer, N. J., & Williams, A. M. (2012). Does self-efficacy mediate transfer effects in the learning of easy and difficult motor skills? *Consciousness and Cognition*, *21*, 1122–1128. https://doi.org/10.1016/j.concog.2012.03.014

Stragà, M., Faiella, A., Santini, I., & Ferrante, D. (2022). "The game would have been better for me if…": Children's counterfactual thinking about their own performance in a game. *Thinking & Reasoning*, 1–35. https://doi.org/10.1080/13546783.2022.2130428

Stragà, M., & Ferrante, D. (2014). Will you train harder for the next marathon? The effect of counterfactual and prefactual thinking on marathon runners' intentions. *Proceedings of the Trieste*

Symposium on Perception and Cognition, 144–146. https://doi.org/hdl.handle.net/10077/10556%0A

Summerville, A. (2011). Counterfactual seeking: The scenic overlook of the road not taken. *Personality and Social Psychology Bulletin*, *37*(11), 1522–1533. https://doi.org/10.1177/0146167211413295

Summerville, A., Blue, J., Kirkmeyer, B. P., & Johnson, B. N. (2019). *Board 147: Coulda, woulda, will I?: An experimental investigation of counterfactual thoughts and intention generation in engineering student success*. 2019 ASEE Annual Conference & Exposition, Tampa, Florida. https://doi.org/10.18260/1-2--32262

Summerville, A., & Buchanan, J. (2014). Functions of personal experience and of expression of regret. *Personality and Social Psychology Bulletin*, *40*(4), 463–475. https://doi.org/10.1177/0146167213515026

Summerville, A., & Roese, N. J. (2008). Dare to compare: Fact-based versus simulation-based comparison in daily life. *Journal of Experimental Social Psychology*, *44*(3), 664–671. https://doi.org/10.1016/j.jesp.2007.04.002

Sweeny, K., Melnyk, D., Miller, W., & Shepperd, J. A. (2010). Information avoidance: Who, what, when, and why. *Review of General Psychology*, *14*(4), 340–353. https://doi.org/10.1037/a0021288

Swirsky, L. T., Sparrow, E. P., Sullivan, M. D., Valenzano, S. L., Chowdhury, S., & Spaniol, J. (2023). Age differences in motivated cognition: A meta-analysis. The Journals of Gerontology: Series B, 78(7), 1169–1181. https://doi.org/10.1093/geronb/gbad049

Szpunar, K. K., & Schacter, D. L. (2013). Get real: Effects of repeated simulation and emotion on the perceived plausibility of future experiences. *Journal of Experimental Psychology: General*, *142*(2), 323–327. APA PsycArticles®. https://doi.org/10.1037/a0028877

Tasso, A., & Cherubini, P. (2007). Differenza tra verità e asseribilità di condizionali fattuali e controfattuali [Difference between truth-value and assertability of factual and counterfactual conditionals]. *Sistemi Intelligenti*, *2*, 237–258. https://doi.org/10.1422/24892

Tassone, D., Reed, A. E., & Carstensen, L. L. (2019). Time may heal wounds: Aging and life regrets. *Psychology and Aging*, *34*, 862–866. https://doi.org/10.1037/pag0000381

Tetlock, P. E. (1998). Close-call counterfactuals and belief-system defenses: I was not almost wrong but I was almost right. *Journal of Personality and Social Psychology*, *75*, 639–652. https://doi.org/10.1037/0022-3514.75.3.639

Tobia, M. J., Guo, R., Gläscher, J., Schwarze, U., Brassen, S., Büchel, C., Obermayer, K., & Sommer, T. (2016). Altered behavioral and neural responsiveness to counterfactual gains in the elderly. *Cognitive, Affective, & Behavioral Neuroscience*, *16*(3), 457–472. https://doi.org/10.3758/s13415-016-0406-7

Trabasso, T., & Bartolone, J. (2003). Story understanding and counterfactual reasoning. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *29*, 904–923. https://doi.org/10.1037/0278-7393.29.5.904

Tyser, M. P., McCrea, S. M., & Knüpfer, K. (2012). Pursuing perfection or pursuing protection? Self-evaluation motives moderate the behavioral consequences of counterfactual thoughts. *European Journal of Social Psychology*, *42*, 372–382. https://doi.org/10.1002/ejsp.1864

van Dijk, E., & Zeelenberg, M. (2007). When curiosity killed regret: Avoiding or seeking the unknown in decision-making under uncertainty. *Journal of Experimental Social Psychology*, *43*(4), 656–662. https://doi.org/10.1016/j.jesp.2006.06.004

van Lieshout, L. L., de Lange, F. P., & Cools, R. (2020). Why so curious? Quantifying mechanisms of information seeking. *Curiosity (Explore vs Exploit)*, *35*, 112–117. https://doi.org/10.1016/j.cobeha.2020.08.005

Vrantsidis, T. H., & Lombrozo, T. (2022). Simplicity as a cue to probability: Multiple roles for simplicity in evaluating explanations. *Cognitive Science*, *46*(7), e13169. https://doi.org/10.1111/cogs.13169

Wagenmakers, E.-J. (2007). A practical solution to the pervasive problems of p values. *Psychonomic Bulletin & Review*, *14*(5), 779–804. https://doi.org/10.3758/BF03194105

Walker, R. J., Smallman, R., Summerville, A., & Deska, J. C. (2016). Motivated by us but not by them: Group membership influences the impact of counterfactual thinking on behavioral intentions. *Social Cognition*, *34*(4), 286–305. https://doi.org/10.1521/soco.2016.34.4.3

Walsh, C. R., & Byrne, R. M. J. (2004). Counterfactual thinking: The temporal order effect. *Memory & Cognition*, *32*(3), 369–378. https://doi.org/10.3758/BF03195831

Warren, G., Byrne, R. M. J., & Keane, M. T. (2023). Categorical and continuous features in counterfactual explanations of AI systems. *Proceedings of the 28th International Conference on Intelligent User Interfaces*, 171–187. https://doi.org/10.1145/3581641.3584090

Weiner, B. (1985). An attributional theory of achievement motivation and emotion. *Psychological Review*, *92*(4), 548–573. https://doi.org/10.1037/0033-295X.92.4.548

Wells, G. L., & Gavanski, I. (1989). Mental simulation of causality. *Journal of Personality and Social Psychology*, *56*(2), 161–169. https://doi.org/10.1037/0022-3514.56.2.161

Wells, G. L., Taylor, B. R., & Turtle, J. W. (1987). The undoing of scenarios. *Journal of Personality and Social Psychology*, *53*, 421–430. https://doi.org/10.1037/0022-3514.53.3.421

Winter, K., & Epstude, K. (2023). Motivational consequences of counterfactual mindsets: Does counterfactual structure influence the use of conservative or risky tactics? *Motivation and Emotion*, *47*(1), 100–114. https://doi.org/10.1007/s11031-022-09979-6

Woltin, K.-A., & Epstude, K. (2023). Should I have been more careful or less careless? The comparative nature of counterfactual thoughts alters judgments of their impact. *Cognition*, *235*, 105402. https://doi.org/10.1016/j.cognition.2023.105402

Wong, E. M., Galinsky, A. D., & Kray, L. J. (2008). The counterfactual mind-set: A decade of research. In K. D. Markman, W. M. P. Klein, & J. A. Surh (Eds.), *Handbook of Imagination and Mental Simulation* (pp. 161–174). Psychology Press.

Wrosch, C., & Heckhausen, J. (2002). Peceived control of life regrets: Good for young and bad for old adults. *Psychology and Aging*, *17*, 340–350. https://doi.org/10.1037/0882-7974.17.2.340

Zeelenberg, M. (1999). Anticipated regret, expected feedback and behavioral decision making. *Journal of Behavioral Decision Making*, *12*(2), 93–106. https://doi.org/10.1002/(SICI)1099-

0771(199906)12:2<93::AID-BDM311>3.0.CO;2-S

Zeelenberg, M., & Pieters, R. (2007). A Theory of regret regulation 1.0. *Journal of Consumer Psychology*, *17*(1), 3–18. https://doi.org/10.1207/s15327663jcp1701_3

Zeelenberg, M., & van Dijk, E. (2004). On the comparative nature of the emotion regret. In D. R. Mandel, D. J. Hilton, & P. Catellani (Eds.), *The Psychology of Counterfactual Thinking*. Routledge.

Zemla, J. C., Sloman, S., Bechlivanidis, C., & Lagnado, D. A. (2017). Evaluating everyday explanations. *Psychonomic Bulletin & Review*, *24*(5), 1488–1500. https://doi.org/10.3758/s13423-017-1258-z

Appendix A – Chapters 2 and 3

General Information

R Version and Packages Used

List of R packages used (R version: 3.6.1, 2019-07-05):

- **BayesFactor**
  Morey, R. D., & Rouder, J. N. (2018). BayesFactor: Computation of Bayes Factors for Common Designs (Version 0.9.12-4.2). https://cran.r-project.org/package=BayesFactor
- **broom**
  Robinson, D., Hayes, A., & Couch, S. (2021). broom: Convert Statistical Objects into Tidy Tibbles (Version 0.7.5). https://cran.r-project.org/package=broom
- **boot**
  Canty, A., & Ripley, B. (2021). boot: Bootstrap R (S-Plus) Functions. R package version 1.3-28.
- **car**
  Mayer, M. (2023). confintr: Confidence Intervals. R package version 1.0.2, https://CRAN.R-project.org/package=confintr
- **confintr**
  Fox, J., & Weisberg, S. (2019). An {R} Companion to Applied Regression (Third Edit). Sage. https://socialsciences.mcmaster.ca/jfox/Books/Companion/
- **DescTools**
  Signorell, A. et mult. alt. (2020). DescTools: Tools for descriptive statistics (Version 0.99.38). https://cran.r-project.org/web/packages/DescTools/index.html
- **dplyr**
  Wickham, H., Françoise, R., Henry, L., & Müller, K. (2020). dplyr: A Grammar of Data Manipulation (Version 1.0.2). https://cran.r-project.org/package=dplyr
- **emmeans**
  Lenth, R. (2023). emmeans: Estimated Marginal Means, aka Least-Squares Means. R package version 1.8.4-1. https://CRAN.R-project.org/package=emmeans
- **extrafont**
  Chang, W. (2022). extrafont: Tools for using fonts (Version 0.18). https://CRAN.R-project.org/package=extrafont
- **FSA**
  Ogle, D. H., Doll, J. C., Wheeler, P., & Dinno, A. (2021). FSA: Fisheries Stock Analysis. (R package version 0.9.1). https://github.com/droglenc/FSA
- **ggplot2**
  Wickham, H. (2016). ggplot2: Elegant Graphics for Data Analysis. Springer-Verlag. https://ggplot2-book.org/

- **ggpubr**
  Kassambara, A. (2020). ggpubr: “ggplot2” Based Publication Ready Plots (R package version 0.4.0). https://CRAN.R-project.org/package=ggpubr
- **gmodels**
  Warnes, G. R., Bolker, B., Lumley, T., & Johnson, R. C. (2018). gmodels: Various R Programming Tools for Model Fitting (R package version 2.18.1). https://cran.r-project.org/package=gmodels%0A
- **GPArotation**
  Coen, B. A., & Jennrich, R. I. (2005). Gradient Projection Algorithms and Software for ArbitraryRotation Criteria in Factor Analysis. *Educational and Psychological Measurement, 65*, 676–696. https://doi.org/10.1177/0013164404272507
- **lme4**
  Bates, D., Maechler, M., Bolker, B., & Walker S. (2015). Fitting Linear Mixed-Effects Models Using lme4. *Journal of Statistical Software, 67(1)*, 1-48. doi:10.18637/jss.v067.i01.
- **lmerTest**
  Kuznetsova A., Brockhoff P. B., & Christensen R. H. B. (2017). lmerTest Package: Tests in Linear Mixed Effects Models. *Journal of Statistical Software*, *82(13)*, 1-26. doi:10.18637/jss.v082.i13 <https://doi.org/10.18637/jss.v082.i13>.
- **plyr**
  Wickham, H. (2011). The split-apply-combine strategy for data analysis. *Journal of Statistical Software, 40(1)*, 1–29. http://www.jstatsoft.org/v40/i01/
- **psych**
  Revelle, W. (2020). psych: Procedures for Personality and Psychological Research (Version 2.0.9). Northwestern University. https://cran.r-project.org/package=psych
- **pwr**
  Champely, S. (2020). pwr: Basic Functions for Power Analysis (Version 1.3-0). https://CRAN.R-project.org/package=pwr
- **readxl**

  Wickham, H., & Brian, J. (2019). readxl: Read Excel Files (Version 1.3.1). https://cran.r-project.org/package=readxl
- **reshape2**
  Wickham, H. (2007). Reshaping Data with the reshape Package. *Journal of Statistical Software, 21*(12), 1-20. http://www.jstatsoft.org/v21/i12/
- **rstatix**
  Kassambara, A. (2021). rstatix: Pipe-Friendly Framework for Basic Statistical Tests (Version 0.7.0). https://cran.r-project.org/package=rstatix
- **sjPlot**
  Lüdecke, D. (2023). sjPlot: Data Visualization for Statistics in Social Science. R package version 2.8.14. https://CRAN.R-project.org/package=sjPlot
- **tidyr**
  Wickham, H. (2020). tidyr: Tidy Messy Data (Version 1.1.2). https://cran.r-project.org/package=tidyr

Instructions, Questions and Items Formulation

Below are the instructions, questions and items that were presented to participants in Experiments 1, 2, and 3, reported in the same order they were experienced. When it is not stated otherwise (in italics), the text was presented in all three studies.

1) INSTRUCTIONS: GENERAL

In this experiment you will be asked to play a target-shooting video game.

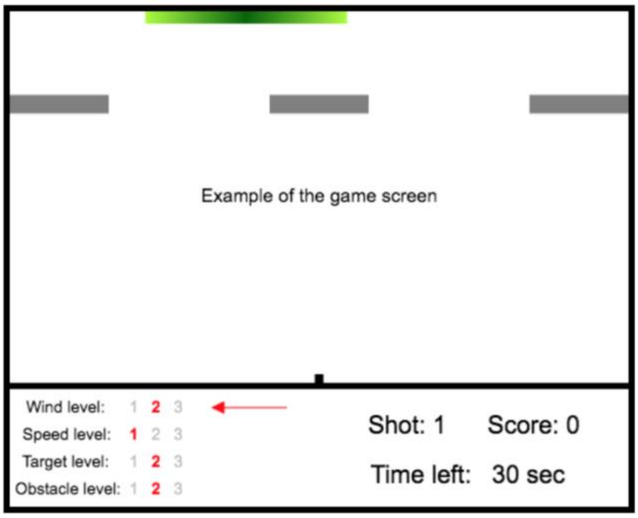
In Study 2 only, the formulation of this sentence was the following:
[In this experiment you will be asked to **play twice** a target-shooting video game.]

Please read the instructions carefully.

The aim of the game is to hit a target moving at the top of the screen with a bullet that is fired from the bottom, while avoiding some obstacles.

You have a total of 12 shots. Difficulty depends on four factors:

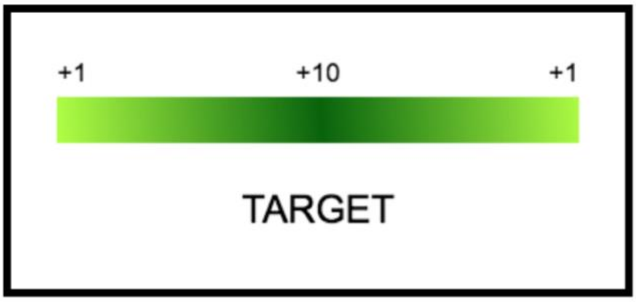
- the wind's strength (Wind level);
- the target's speed (Speed level);
- the target's size (Target level);
- the obstacles' width (Obstacle level).



The complexity of each of these factors varies randomly on three different levels (the current level will be indicated in red).

You have 30 seconds to make your shot. No bonus is given for shooting quickly, so take your time.

For each shot, your score will depend on whether you hit and where you hit the target. It can range from **0** (if the target is missed or the time runs out) to **10** (if you hit the bull's eye).



Before starting the game, two tutorials will explain how to aim and how the wind can affect the shots. You will also have 4 practice shots.

VERY IMPORTANT: in addition to the standard payment for your participation, **you can win an extra prize of 10 pounds**. At the end of the experiment, 5 participants will be randomly drawn, and the one with the highest score will receive the extra prize, so please try to do your best.

In Study 2 only, the formulation of this paragraph was the following:
[**VERY IMPORTANT**: in addition to the standard payment for your participation, **you can win up to two extra prizes of 5 pounds each**. At the end of the experiment, two sets of 5 participants will be randomly drawn (one set for the first game and one for the second). The participants with the highest score within each set will receive the additional money, so do your best in both games.]

If everything is clear, press "Next" to begin.

2) RATING EXPERIENCE AND PLEASURE WITH PLAYING VIDEOGAMES

Before starting the tutorials, please answer the following questions. In general:

a) How much experience do you have with video games?

   [Very limited / Very extensive]

b) How much do you like playing video games?

   [Not at all / Very much]

3) INSTRUCTIONS: AIMING AND SHOOTING TUTORIAL

Two actions are necessary to shoot the bullet.

- First, you must aim with the **mouse pointer**: Once fired, the bullet will head towards the position of the pointer, adjusted for the wind. You are free to move the pointer all over the game screen; moving the pointer after the bullet has been fired will not change its trajectory.

- Shoot by pressing the **"Shift" button** (either the right or the left one).

In the tutorial below, a thin line connects the bullet to the current position of the pointer, showing the trajectory that the bullet would follow if it was fired in that moment (in absence of wind). Please note that the line is present only in this tutorial, but not during the actual game.

You can now take 3 practice shots.

After the third shot, the "Next" button will appear at the end of the page, allowing you to proceed.

4) INSTRUCTIONS: WIND VIDEO TUTORIAL

Wind affects the trajectory of your shots. All the relevant information about it is presented in the lower part of the game screen. In particular:

- **Wind force** is indicated by the number in red. In each shot, the wind force can take one of three possible values, with higher numbers corresponding to higher intensities.

- **Wind direction** is indicated by the arrow drawn next to the wind force values. The arrow can point either left or right, showing the direction the wind is blowing.

In the video-tutorial below, there are 5 examples of wind conditions. The green line indicates the bullet’s trajectory in absence of wind when fired aiming right in front of the starting position.

Please note how different wind conditions affect the bullet’s trajectory.

5) INSTRUCTIONS: WIND HANDS-ON TUTORIAL

Here you can experience first-hand the same wind conditions you just saw in the video-tutorial.

6) PRACTICE SHOTS

You can now take 4 practice shots.

7) TOTAL SCORE ESTIMATION AFTER PRACTICE + CONFIDENCE ON ESTIMATE

During the practice you scored a total of %participant's practice score% points.

In the actual game, you will have 12 shots comparable to those of the practice rounds.

What do you think your overall score will be in the actual game?
(Indicate a number ranging from 0 to 120, trying to be as accurate as possible)

How confident do you feel about your overall score estimate?
[Not confident at all / Completely confident]

8) GAME

In Study 2, at this point, participants played the first of the two rounds of the game.

9) WAITING SCREEN

Present only in Study 1b

Please wait. The software is recording your score.

10) FEEDBACK

Present only in Study 1b for participants in the feedback-present condition who scored ≤ 90 points

Your overall score is **%participant's score%** points.

This score is **lower than the average score** obtained by the players that have already completed the game.

72% of the other players **scored better than you**

11) RATING PERFORMANCE SATISFACTION

How do you rate your performance in the game?
[Extremely bad / Extremely good]

12) COUNTERFACTUAL ELICITATION

Not present in the no-counterfactual condition of Study 2

Your overall score was %participant's score%.

Complete the following sentence with the first thought that comes to your mind.

"My score would have been **higher than %participant's score%,** if only…"

13) RATING EMOTIONS

Present only in Study 2

How do you feel about your performance?

a) [Unhappy / Happy]

In light of your performance, do you feel:

b) Angry [Not at all / Very much]

c) Disappointed [Not at all / Very much]

d) Ashamed [Not at all / Very much]

e) Regretful [Not at all / Very much]

14) TOTAL SCORE ESTIMATION SECOND GAME

Your overall score was %participant's score%.

Now imagine you are asked to play again the very same game you just played.
What do you think your overall score would be in this possible second game?
(Indicate a number ranging from 0 to 120, trying to be as accurate as possible)

In Study 2 only, the formulation of this paragraph was the following:
What do you think your overall score will be in the second game?
(Indicate a number ranging from 0 to 120, trying to be as accurate as possible)

In Study 2 only, participants were also asked to estimate their confidence in the estimate of the total score in the second round of the game:
How confident do you feel about your estimate?
[Not confident at all / Completely confident]

15) PERCEIVED OPPORTUNITY

Present only in Study 2

For the second game, you estimated a score that exceeds what you obtained in the first game by %estimate second game - participant's total score in the first game% points.

Do you think that this improvement, if achieved, would be due to your ability?
[Yes, at least in part / No, not at all]

16) SECOND GAME

Present only in Study 2

17) EDUCATION LEVEL

Highest education level obtained [Optional]:

- High school diploma
- Bachelor's degree
- Master's degree
- PhD or other

18) DEBRIEFING

Present only in Study 1b for participants in the feedback-present condition who scored ≤ 90 points

At the end of the game, you were informed that your score was lower than the average score obtained so far in this game, and that 72% of participants scored higher than you.

Actually, this is not true. This information was included only to investigate the possible effect of negative feedback on your thoughts about your score.

We apologize for this mild deception, but please note that it was necessary in light of the aims of this study. Note also that this will not affect in any way your probability of winning the extra prize, which will be assigned exactly as specified in the instructions (i.e., 5 participants will be randomly drawn, and the one with the highest score will win the prize).

Nevertheless, we realize that you might be disappointed to have been deceived. For this reason, if you prefer, you have the right to exclude your answers from our analyses, and this will not affect your payment for the participation in this experiment.

Please, let us know your decision by selecting one of the two following options:

- I allow you to use my anonymized data (i.e., include my answers in the analyses)
- I do not allow you to use my anonymized data (i.e., exclude my answers from the analyses)

Experiment 1 – Effect of Task Difficulty on Type of Counterfactual Produced

Logistic Regression

Table A1
Results of the logistic regression on the type of counterfactual produced (controllable vs. uncontrollable)

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept (easy)	-0.258	0.287	-0.900	.368	0.77	0.44 – 1.35
Exp cond intermediate (easy)	0.600	0.336	1.782	.075	1.82	0.95 – 3.55
Exp cond difficult (easy)	1.662	0.427	3.896	<.001	5.27	2.32 – 12.46
Exp cond difficult (intermediate)	1.062	0.369	2.882	.004	2.89	1.42 – 6.08
Experience videogame	0.034	0.255	0.132	.895	1.03	0.62 – 1.70
Enjoy videogame	-0.027	0.249	-0.110	.913	0.97	0.60 – 1.59
Mismatch expected performance	-0.323	0.184	-1.759	.079	0.72	0.50 – 1.03
Confidence expected performance	-0.354	0.159	-2.228	.026	0.70	0.51 – 0.96
Performance rating	0.185	0.193	0.955	.339	1.20	0.83 – 1.77
Expected improvement	-0.313	0.148	-2.117	.034	0.73	0.54 – 0.97
Age	0.213	0.139	1.539	.123	1.24	0.95 – 1.63
Sex male (female)	0.255	0.306	0.832	.405	1.29	0.71 – 2.37

Note. Dummy coding was used for all categorical predictors. Reference levels are reported between brackets. All continuous variables are standardized.

Experiment 2 – Effect of Negative Feedback on Type of Counterfactual Produced

Logistic Regression – Participants with Total Score ≤ 90 points

Table A2

Results of the logistic regression on the type of counterfactual produced (controllable vs. uncontrollable)

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept (feed-abs)	-0.227	0.203	-1.121	.262	0.797	0.53 – 1.18
Exp cond feedback-present (feed-abs)	0.347	0.237	1.463	.144	1.41	0.89 – 2.26
Total score (feed-abs)	-0.441	0.211	-2.089	.037	0.64	0.42 – 0.97
Total score (feed-pres)	-0.232	0.166	-1.394	.163	0.79	0.57 – 1.09
Exp cond * total score	0.209	0.232	0.904	.366	1.23	0.78 – 1.95
Experience videogame	-0.224	0.213	-1.054	.292	0.80	0.52 – 1.21
Enjoy videogame	0.109	0.200	0.542	.588	1.11	0.75 – 1.66
Mismatch expected performance	0.015	0.141	0.106	.916	1.02	0.77 – 1.34
Confidence expected performance	-0.260	0.126	-2.064	.039	0.77	0.60 – 0.99
Performance rating	0.197	0.141	1.394	.163	1.22	0.92 – 1.61
Expected improvement	-0.320	0.121	-2.643	.008	0.73	0.57 – 0.92
Age	0.131	0.109	1.209	.227	1.14	0.92 – 1.14
Sex male (female)	0.767	0.261	2.938	.003	2.15	1.30 – 3.62

Note. Dummy coding was used for all categorical predictors. Reference levels are reported between brackets. All continuous variables are standardized.

Logistic Regression – Participants with Total Score ≤ 85 points

The logistic regression performed in Experiment 2 was run again, excluding the participants who scored > 85 points (equal to the value below the top-scoring 10% of participants who played the easy version of the game in Experiment 1). The final sample comprised 358 participants (58% female, M_{age} = 29.3, SD_{age} = 6.23).

The full regression model, including experimental condition, total score and their interaction as predictors, was not significantly better than the one without the interaction term, $\chi^2(1) = 0.69$, p = .408, Nagelkerke $R^2 = 0.10$, and indeed the interaction term failed to reach significance (*OR* = 1.22, *p* = .409, 95% *CI* 0.76 to 1.96). Comparing the two models through the BIC approximation method further reported positive evidence in favour of the model without the interaction term ($BF_{10} = 0.07$). As can be seen from table S5, the experimental condition (*OR* = 1.41, *p* = .165, CI 0.87 to 2.28) also was not significant, while the total score was a significant predictor among participants in the feedback-absent condition (*OR* = 0.65, *p* = .044, CI 0.42 to 0.98) but not among those in the feedback-present one (*OR* = 0.79, *p* = .163, CI 0.56 to 1.10). Also, the expected improvement in a possible second game (*OR* = 0.73, *p* = .008, CI 0.57 to 0.92) and sex (*OR* = 2.15, *p* = .005, CI 1.30 to 3.62) were significant, while, as opposed to the analysis reported in the paper, the pre-game estimate of the total score (*OR* = 0.77, *p* = .070, CI 0.60 to 0.99) failed to reach significance.

Table A3

Results of the logistic regression on the type of counterfactual produced (controllable vs uncontrollable)

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept (feed-abs)	-0.208	0.210	-0.990	.322	0.813	0.54 – 1.22
Exp cond feedback-present (feed-abs)	0.341	0.245	1.390	.165	1.41	0.87 – 2.28
Total score (feed-abs)	-0.438	0.217	-2.017	.044	0.65	0.42 – 0.98
Total score (feed-pres)	-0.240	0.172	-1.394	.163	0.79	0.56 – 1.10
Exp cond * total score	0.198	0.240	0.826	.409	1.22	0.76 – 1.96
Experience videogame	-0.255	0.219	-1.166	.244	0.80	0.52 – 1.21
Enjoy videogame	0.109	0.207	0.528	.597	1.11	0.75 – 1.66
Mismatch expected performance	-0.017	0.146	0.116	.907	1.02	0.77 – 1.34
Confidence expected performance	-0.239	0.132	-1.813	.070	0.77	0.60 – 0.99
Performance rating	0.241	0.147	1.633	.102	1.22	0.92 – 1.61
Expected improvement	-0.328	0.125	-2.634	.008	0.73	0.57 – 0.92
Age	0.154	0.114	1.350	.177	1.14	0.92 – 1.14
Sex male (female)	0.773	0.276	2.803	.005	2.15	1.30 – 3.62

Note. Dummy coding was used for all categorical predictors. Reference levels are reported between brackets. All continuous variables are standardized.

Experiment 3 – Effect of Counterfactuals in Context of Repeated Performances

Pilot Study

Before running Experiment 3, it was necessary to test whether the performance in the target-shooting video game could be improved in an immediate repetition of the game. To this end, both the easy and the intermediate versions of the game were tested.

Method

Participants. A total of 30 participants were recruited on Prolific. Inclusion criteria and participants' compensation were the same as those reported for Study 2.

Procedure. Participants were randomly assigned to either the easy (n = 14) or intermediate (n = 16) conditions. The game was played twice, and after the first round of the game, participants immediately proceeded to the second one. No variables were measured except for game-related ones (i.e., participants' scores).

Results

Two paired-sample *t*-tests were run to investigate performance improvement from the first to the second game in the easy and intermediate conditions. Participants in the easy condition improved from the first (M = 55.8; SD = 19.2) to second round of the game (M = 67.6; SD = 15.5), $t(13) = -2.21$, $p = .046$, albeit to a lesser extent compared to those in the intermediate condition, who performed significantly better in the second (M = 40.4; SD = 21.1) compared to the first round (M = 28.9; SD = 18.7), $t(15) = -3.70$, $p = .004$.

Content-Neutral Pathway: All Participants

Multiple Linear Regression – Counterfactual vs. No-counterfactual

Table A4

Results of the multiple linear regression on the round score in the second game

Predictor	Estimate	*SE*	*t*	*p*	95% CI Estimate
Intercept (no-counterfactual)	-0.12	0.07	-1.681	.093	-0.270 – 0.021
Exp cond (no-counterfactual)	-0.065	0.071	-0.909	.363	-0.205 – 0.075
Total score first game	0.653	0.047	13.441	< .001	0.558 – 0.749
Experience videogame	-0.184	0.065	-2.816	.005	-0.312 – -0.056
Enjoy videogame	0.167	0.060	2.799	.005	0.050 – 0.284
Mismatch expected performance	-0.024	0.047	-0.524	0.601	-0.116 – 0.067
Confidence expected performance	0.089	0.040	2.218	.027	0.010 – 0.169
Performance rating	-0.045	0.054	-0.837	.403	-0.151 – 0.061
Expected improvement	0.016	0.037	0.429	.668	-0.057 – 0.089
Confidence expected improvement	-0.060	0.040	-1.484	.138	-0.139 – 0.019
Negative mood	0.014	0.044	0.313	.754	-0.073 – 0.100
Age	-0.058	0.034	-1.678	.094	-0.125 – 0.010
Sex male (female)	0.314	0.085	3.719	< .001	0.148 – 0.480

Note. Dummy coding was used for all categorical predictors. Reference levels are reported between brackets. All continuous variables are standardized.

Multiple Linear Regression – Controllable vs. Uncontrollable vs. No-counterfactual

Table A5

Results of the multiple linear regression on the total score in the second game

Predictor	Estimate	*SE*	*t*	*p*	95% CI Estimate
Intercept (no-counterfactual)	-0.130	0.073	-1.765	.078	-0.275 – 0.015
Exp cond controllable counterfactual (no-counterfactual)	0.035	0.088	0.405	.686	-0.137 – 0.207
Exp cond uncontrollable counterfactual (no-counterfactual)	-0.132	0.079	-1.676	.094	-0.288 – 0.022
Exp cond uncontrollable counterfactual (controllable counterfactual)	-0.168	0.085	-1.976	.049	-0.207 – 0.137
Total score first game	0.646	0.049	13.229	< .001	0.551 – 0.741
Experience videogame	-0.183	0.065	-2.812	.005	-0.311 – -0.055
Enjoy videogame	0.165	0.059	2.784	.006	0.049 – 0.282
Mismatch expected performance	-0.028	0.047	-0.611	.541	-0.120 – 0.063
Confidence expected performance	0.087	0.040	2.173	.030	0.008 – 0.166
Performance rating	-0.043	0.054	-0.801	.423	-0.149 – 0.063
Expected improvement	0.007	0.037	0.193	.847	-0.066 – 0.081
Confidence expected improvement	-0.059	0.040	-1.466	.143	-0.138 – 0.020
Negative mood	0.009	0.044	0.214	.831	-0.077 – 0.096
Age	-0.051	0.034	-1.476	.140	-0.118 – 0.017
Sex male (female)	0.324	0.084	3.837	< .001	0.158 – 0.490

Note. Dummy coding was used for all categorical predictors. Reference levels are reported between brackets. All continuous variables are standardized.

Content-Neutral Pathway: Considering Only Participants with Positive Perceived Opportunity

The analyses regarding the content-neutral pathway were run a second time including only those participants who believed they would improve in the second game (i.e., expected improvement higher than 0) and that such improvement would be due at least in part to their own ability (i.e., answered "yes, at least in part" to the question about participant's personal ability to improve). The new final sample thus consisted of 394 participants (43% female, M_{age} = 28.8, SD_{age} = 5.90), divided among the no-counterfactual (*n* = 126) and counterfactual conditions (*n* = 268, further divided between controllable, *n* = 112, and uncontrollable counterfactual, *n* = 156).

Multiple Linear Regression – Counterfactual vs. No-Counterfactual Conditions

Similar to what was observed in the main analyses, the model including the experimental condition was not significantly better than the one containing only the covariates, $F(1, 381) = 0.47$, $p = .495$ (see Table S16), and a Bayesian analysis confirmed this result, reporting moderate evidence in favor of the latter ($BF_{10} = 0.15$).

Table A6
Results of the multiple linear regression on the total score in the second game

Predictor	Estimate	*SE*	*t*	*p*	95% CI Estimate
Intercept (no-counterfactual)	-0.189	0.073	-2.562	.011	-0.334 – -0.044
Exp cond (no-counterfactual)	-0.058	0.085	-0.683	.495	-0.109 – 0.224
Total score first game	0.635	0.056	11.262	< .001	0.524 – 0.746
Experience videogame	-0.239	0.077	-3.096	.002	-0.391 – -0.087
Enjoy videogame	0.203	0.069	2.930	.004	0.067 – 0.339
Mismatch expected performance	-0.069	0.055	-1.267	.206	-0.176 – 0.038
Confidence expected performance	0.088	0.048	1.817	.070	-0.007 – 0.183
Performance rating	-0.045	0.063	-0.711	.477	-0.169 – 0.079
Expected improvement	-0.024	0.042	-0.580	.562	-0.106 – 0.058
Confidence expected improvement	-0.036	0.049	-0.719	.472	-0.133 – 0.062
Negative mood	0.025	0.051	0.484	.628	-0.076 – 0.125
Age	-0.093	0.040	-2.317	.021	-0.173 – -0.014
Sex male (female)	0.299	0.099	3.008	.003	0.103 – 0.494

Note. Dummy coding was used for all categorical predictors. Reference levels are reported between brackets. All continuous variables are standardized.

Multiple Linear Regression – Controllable vs. Uncontrollable vs. No-counterfactual

The results did not change when the counterfactual condition was further divided based on the type of counterfactual produced (no vs. counterfactual vs. uncontrollable counterfactuals). Again, there was no significant difference between the model containing the experimental condition and the one containing only the covariates, $F(2, 380) = 2.13$, $p = .121$ (see Table S17), a result confirmed by a Bayesian analysis showing moderate evidence in favor of the latter ($BF_{10} = 0.25$).

Table A7

Results of the multiple linear regression on the total score in the second game

Predictor	Estimate	*SE*	*t*	*p*	95% CI Estimate
Intercept (no-counterfactual)	-0.143	0.090	-1.595	.112	-0.319 – 0.033
Exp cond controllable counterfactual (no-counterfactual)	0.052	0.102	0.515	.607	-0.147 – 0.252
Exp cond uncontrollable counterfactual (no-counterfactual)	-0.137	0.094	-1.464	.144	-0.321 – 0.047
Exp cond uncontrollable counterfactual (controllable counterfactual)	-0.190	0.097	-1.945	.052	-0.381 – 0.002
Total score first game	0.627	0.056	11.126	< .001	0.516 – 0.738
Experience videogame	-0.240	0.077	-3.130	.002	-0.392 – -0.090
Enjoy videogame	0.202	0.069	2.923	.004	0.066 – 0.338
Mismatch expected performance	-0.073	0.054	-1.351	.178	-0.180 – 0.033
Confidence expected performance	0.084	0.048	1.732	.084	-0.011 – 0.178
Performance rating	-0.044	0.063	-0.694	.488	-0.167 – 0.080
Expected improvement	-0.034	0.042	-0.817	.414	-0.117 – 0.048
Confidence expected improvement	-0.035	0.049	-0.714	.476	-0.132 – 0.062
Negative mood	0.017	0.511	0.337	.737	-0.083 – 0.118
Age	-0.087	0.040	-2.153	.032	-0.166 – -0.008
Sex male (female)	0.320	0.100	3.211	.001	0.124 – 0.515

Note. Dummy coding was used for all categorical predictors. Reference levels are reported between brackets. All continuous variables are standardized.

Content-Specific Pathway: Effect on Second Performance

Average Shooting Times

Table A8
Mean and SD of the average shooting time in the first and second rounds of the game

Counterfactual produced	*n*	First round		Second round	
		M	*SD*	*M*	*SD*
Whole sample	493	5293	2100	5085	2018
Controllable – Increasing shooting time	76	4830	1500	5128	1577
Controllable – Other elements	57	5229	1801	5103	2314
Uncontrollable	192	5392	2098	5128	1967
No counterfactual (control condition)	168	5412	2394	5009	2158

Note. Means and standard deviations are reported in milliseconds.

Linear Mixed Model

Table A9
Fixed effect results of the linear mixed model on (square-root transformed) average shooting times

Predictor	Estimate	*SE*	*df*	*t*	*p*	95% CI estimate
Intercept	70.516	0.664	489	106.263	< .001	69.218 – 71.814
Type of CFT (Contr CFT – No time)	-0.819	1.244	489	-0.658	.511	-3.252 – 1.614
Type of CFT (Uncontr CFT)	-0.019	1.384	489	-0.014	.989	-2.727 – 2.688
Type of CFT (No CFT)	0.713	0.937	489	0.760	.447	-1.121 – 2.546
Game (Second game)	-0.478	0.236	489	-2.024	.044	-0.941 – -0.016
Type of CFT (Contr CFT – No time) * Game (Second game)	1.551	0.443	489	3.498	.001	0.684 – 2.417
Type of CFT (Uncontr CFT) * Game (Second game)	-0.306	0.493	489	-0.621	.535	-1.271 – 0.659
Type of CFT (No CFT) * Game (Second game)	-0.379	0.334	489	-1.135	.257	-1.032 – 0.274

Note. Deviation coding was used for all predictors. Reference levels were "controllable counterfactual about shooting time" for type of counterfactual and "first game" for game. In brackets are reported the levels to which the estimate value refers to (CFT: counterfactual; Contr CFT – No time = Controllable counterfactual not about shooting time; Uncontr CFT = Uncontrollable counterfactual).

Table A10

Contrasts on the average shooting times in the first and second game, divided by the type of counterfactual produced

Contrasts	Estimate	*SE*	*df*	*t*	*p*
Controllable counterfactual about time	2.144	1.060	489	2.022	.175
Controllable counterfactual not about time	-1.569	1.225	489	-1.281	.803
Uncontrollable counterfactual	-1.715	0.667	489	-2.570	.042
No counterfactual	-2.688	0.713	489	-3.768	.001

Note. *p* values are Bonferroni corrected

Table A11

Contrasts on the shooting times differences between the participants who produced a controllable counterfactual about shooting times and groups of participants who produced different modifications

Contrasts	Estimate	*SE*	*df*	*t*	*p*
Controllable counterfactual about time vs Controllable counterfactual not about time	3.713	1.620	489	2.292	0.067
Controllable counterfactual about time vs Uncontrollable counterfactual	3.859	1.253	489	3.080	0.007
Controllable counterfactual about time vs No counterfactual	4.832	1.278	489	3.781	0.001

Note. *p* values are Bonferroni corrected

Regression on Increase in Shooting Time and Performance Improvement

Table A12

Results of the multiple linear regression on the total score in the second game

Predictor	Estimate	*SE*	*t*	*p*	95% CI Estimate
Intercept	0.084	0.083	1.016	.313	-0.081– 0.250
Difference average shooting times	0.211	0.083	2.541	.013	0.045 – 0.376
Total score first game	0.588	0.084	6.991	< .001	0.420 – 0.755

Note: For this analysis, only participants who produced a counterfactual about not rushing their shots were considered (n = 76). All continuous variables are standardized.

Multiple Linear Regression – Controllable about time vs. Controllable about other elements vs. Uncontrollable vs. No-counterfactual

Table A13

Results of the multiple linear regression on the round score in the second game

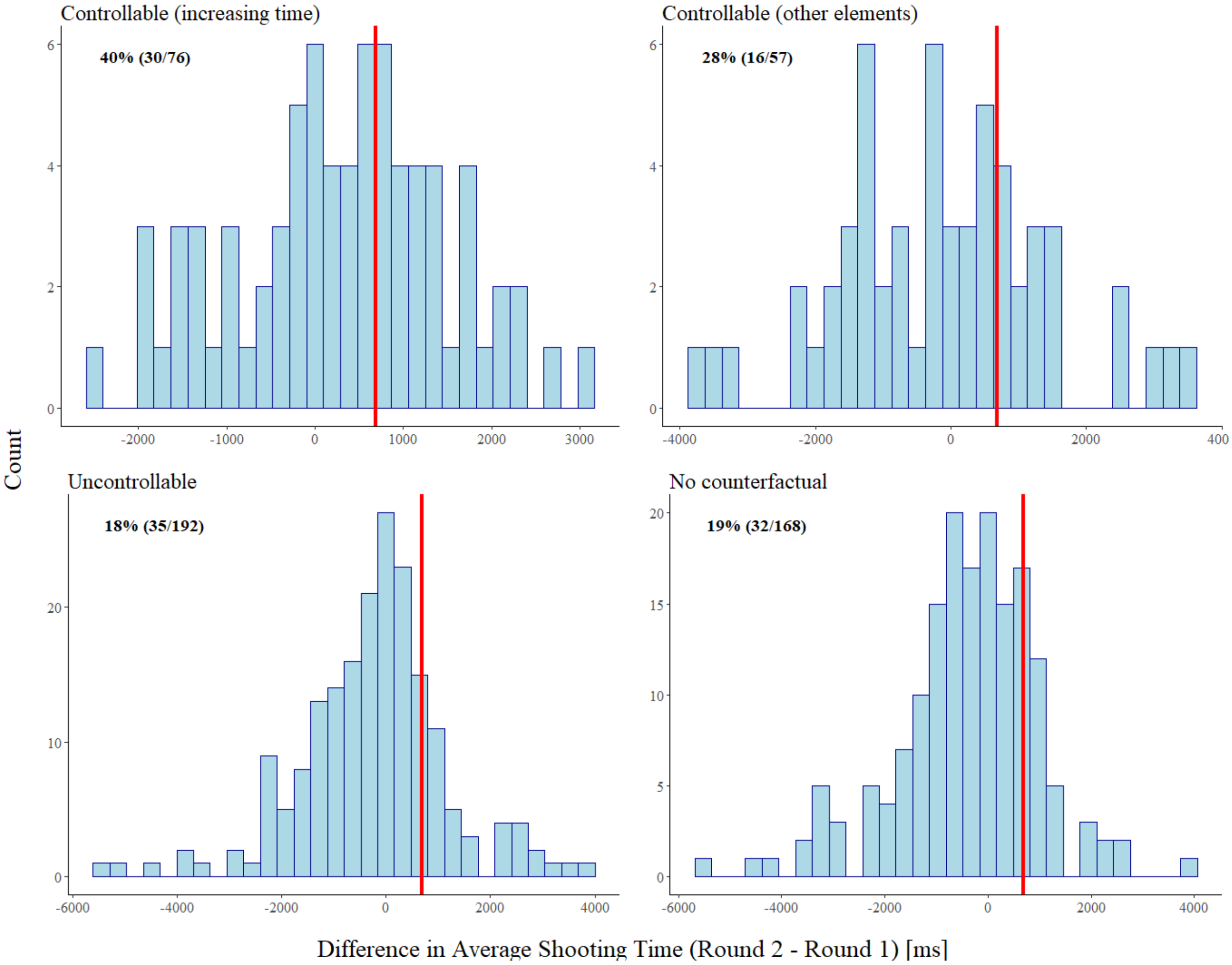
Predictor	Estimate	*SE*	*t*	*p*	95% CI Estimate
Intercept (no-counterfactual)	-0.107	0.096	-1.120	.263	-0.295 – 0.081
Controllable other (controllable time)	0.028	0.132	0.216	.829	-0.231 – 0.288
Uncontrollable (controllable time)	-0.156	0.102	-1.522	.129	-0.357 – 0.045
No-counterfactual (controllable time)	-0.023	0.105	-0.221	.825	-0.229 – 0.182
Total score first game	0.647	0.049	13.288	< .001	0.551 – 0.742
Experience videogame	-0.183	0.065	-2.807	.005	-0.311 – -0.055
Enjoy videogame	0.165	0.059	2.778	.006	0.048 – 0.282
Mismatch expected performance	-0.029	0.047	-0.626	0.531	-0.121 – 0.063
Confidence expected performance	0.087	0.040	2.168	.031	0.008 – 0.166
Performance rating	-0.042	0.054	-0.772	.441	-0.149 – 0.065
Expected improvement	0.007	0.037	0.195	.846	-0.066 – 0.081
Confidence expected improvement	-0.059	0.040	-1.451	.147	-0.138 – 0.021
Negative mood	0.011	0.044	0.237	.813	-0.077 – 0.098
Age	-0.050	0.034	-1.461	.145	-0.118 – 0.017
Sex male (female)	0.324	0.084	3.830	< .001	0.158 – 0.490

Note. Dummy coding was used for all categorical predictors. Reference levels are reported between brackets. All continuous variables are standardized.

Content-Specific Pathway: Evaluating Increase in Average Shooting Time

Figure A1

Proportion of participants who increased their average shooting time depending on the content of the counterfactual produced



Note. The red line indicates the median of the average differences in shooting time between the two games of all participants who increased their shot duration (683 ms). This value was taken as the threshold above which participants were considered to have meaningfully increased their shooting time in the second game.

Appendix B – Chapter 4

General Information

R Version and Packages Used

List of R packages used (R version: 4.2.2, 2022-10-31):

- **broom**
  Robinson, D., Hayes, A., & Couch, S. (2021). broom: Convert Statistical Objects into Tidy Tibbles (Version 0.7.5). https://cran.r-project.org/package=broom

- **car**
  Fox, J., & Weisberg, S. (2019). An {R} Companion to Applied Regression (Third Edit). Sage. https://socialsciences.mcmaster.ca/jfox/Books/Companion/

- **dplyr**

  Wickham, H., Françoise, R., Henry, L., & Müller, K. (2020). dplyr: A Grammar of Data Manipulation (Version 1.0.2). https://cran.r-project.org/package=dplyr

- **emmeans**

  Lenth, R. (2023). emmeans: Estimated Marginal Means, aka Least-Squares Means (R Package version 1.8.4-1). https://CRAN.R-project.org/package=emmeans

- **extrafont**

  Chang, W. (2022). extrafont: Tools for using fonts (Version 0.18). https://CRAN.R-project.org/package=extrafont

- **ggplot2**

  Wickham, H. (2016). ggplot2: Elegant Graphics for Data Analysis. Springer-Verlag. https://ggplot2-book.org/

- **gridExtra**
  Auguie, B. (2017). gridExtra: Miscellaneous Functions for "Grid" Graphics (R package version 2.3). https://CRAN.R-project.org/package=gridExtra

- **DHARMa**
  Hartig, F. (2022). DHARMa: Residual Diagnostics for Hierarchical (Multi-Level / Mixed) Regression Models (R package version 0.4.6). https://CRAN.R-project.org/package=DHARMa

- **lme4**
  Bates, D., Maechler, M., Bolker, B., Walker, S. (2015). Fitting Linear Mixed-Effects Models Using lme4. *Journal of Statistical Software, 67(1)*, 1–48. https://doi.org/10.18637/jss.v067.i01

- **nlme**
  Pinheiro, J., Bates, B., R Core Team (2022). nlme: Linear and Nonlinear Mixed Effects Models. (R package version 3.1-160). https://CRAN.R-project.org/package=nlme

- **readxl**

  Wickham, H., & Brian, J. (2019). readxl: Read Excel Files (Version 1.3.1). https://cran.r-project.org/package=readxl

Experiment 4 – Random Choice

Instructions

In this experiment you will be asked to play **10 rounds of a quick card-drawing game**.

At the beginning, you will be presented with two identical decks of cards, each of which consists of two cards: a “winning card” that allows a gain of 10 pence and a “losing card” that causes a loss of 10 pence.

When the game starts, each round will proceed as follows: The two decks will be shuffled. Then, your task will be to turn over the top card from one of these decks. You will then see whether the card you have turned over is a “winning card” or a “losing card”. After that, if you wish, you will be able to turn over the top card of the other deck to see what would have happened if you had chosen it.

Therefore, each round will end with either a gain or a loss of 10 pence. To account for any possible losses, in each round you will receive an **endowment of 10 pence**. Once all 10 rounds of the game are complete, one round will be randomly selected and its outcome will be honored with real money. Based on the card you turned over in that randomly selected round, **you will receive 0 pence** (= 10 endowment - 10 lost) **or 20 pence** (= 10 endowment + 10 won). In both cases, this amount will not affect the compensation due to you for your participation; that, of course, will remain the same.

(Next page)

Below you can see the composition of the two decks you will play with. They both consist of the same two cards: a “winning card” and a “losing card”.

GLMM – Checking behavior

Table B1

Fixed effect results of the GLMM on checking behavior throughout the rounds of the card-drawing game

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	0.266	0.248	1.074	.283	1.31	0.80 – 2.12
Age group (OAs)	-0.230	0.248	-0.927	.354	0.79	0.49 – 1.29
Outcome experienced (gain)	-0.375	0.072	-5.238	< .001	0.69	0.60 – 0.79
Age group (OAs) * Outcome experienced (gain)	0.020	0.071	0.287	.774	1.02	0.88 – 1.17

Note. Deviation coding was used for all predictors. Reference levels were "younger adults" for age group and "loss" for previous outcome. In brackets are reported the levels to which the estimate value refers to (OAs: older adults).

GLMM – Checking behavior, round as covariate

Table B2

Fixed effect results of the GLMM on checking behavior throughout the rounds of the card-drawing game

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	0.287	0.265	1.084	.278	1.34	0.79 – 2.24
Age group (OAs)	-0.242	0.265	-0.914	.361	0.79	0.47 – 1.32
Outcome experienced (gain)	-0.431	0.075	-5.710	< .001	0.65	0.56 – 0.75
Mean centered round	-0.213	0.026	-8.190	< .001	0.81	0.77 –0.85
Age group (OAs) * Outcome experienced (gain)	0.024	0.074	0.316	.752	1.02	0.88 – 1.18

Note. Deviation coding was used for all predictors. Reference levels were "younger adults" for age group, "loss" for previous outcome. In brackets are reported the levels to which the estimate value refers to (OAs: older adults).

Logistic regression – Checking behavior

Table B3

Results of the logistic regression on checking behavior in the round selected to compute the bonus payment

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	0.477	0.159	3.002	.003	1.61	1.18 – 2.21
Age group (OAs)	-0.141	0.159	-0.890	.374	0.87	0.63 – 1.18
Outcome experienced (gain)	-0.419	0.159	-2.637	.008	0.66	0.48 – 0.89
Age group (OAs) * Outcome experienced (gain)	0.244	0.159	1.534	.125	1.28	0.94 – 1.75

Note. Deviation coding was used for all predictors. Reference levels were “younger adults” for age group and “loss” for previous outcome. In brackets are reported the levels to which the estimate value refers to (OAs: older adults).

Generalized Linear Mixed Model (GLMM) – Switching behavior

Table B4

Fixed effect results of the GLMM on switching behavior throughout the rounds of the card-drawing game

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	-0.707	0.084	-8.457	< .001	0.49	0.42 – 0.58
Age group (older adults)	-0.356	0.082	-4.313	< .001	0.70	0.60 – 0.82
Previous outcome (gain)	-0.017	0.057	-0.302	.763	0.98	0.88 – 1.10
Age group * Previous outcome	0.092	0.057	1.608	.108	1.10	0.98 – 1.23

Note. Deviation coding was used for all predictors. Reference levels were “younger adults” for age group and “loss” for previous outcome. In brackets are reported the levels to which the estimate value refers to.

Experiment 5 – Deliberate Choice

Instructions

In this experiment, you will be asked to play **20 rounds of a quick card-drawing game**.

In the beginning, you will be presented with two decks of cards: one risky deck and one safe deck. The two decks consist of three types of cards: "winning cards" that produce a gain of a certain number of pence, "losing cards" that produce a loss of a certain number of pence, and "neutral cards" that produce neither a gain nor a loss of pence.

The difference between the two decks is that "winning cards" and "losing cards" yield more extreme outcomes in the risky deck compared to the safe deck (i.e., the number of pence that can be won, but also lost, is greater in the risky deck compared to the safe deck). On the next screen, you will be shown the exact composition of the two decks.

When the game begins, each round will proceed as follows. The two decks will be shuffled. Then, your task will be to turn over the top card from one of these decks. You will then see whether the card you have turned over is a "winning card", a "losing card", or a "neutral card". After that, if you wish, you will be able to turn over the top card of the other deck to see what would have happened if you had chosen it.

Depending on the number indicated on the card drawn, each round will end with a gain, a loss, or neither a gain nor a loss of pence (maximum possible gain or loss: 60 pence). To account for possible losses, in each round you will receive an **endowment of 60 pence**. Once all 20 rounds of the game have been completed, one round will be randomly selected and its outcome will be honored with real money. So, if, in the round that is later randomly selected, you choose the risky deck, your bonus payment could be **as little as 0 pence** (= 60 endowment - 60 lost) to **as much as 120 pence** (= 60 endowment + 60 won). On the other hand, if you choose the safe deck, your bonus payment could be **as little as 30 pence** (= 60 endowment - 30 lost) to **as much as 90 pence** (= 60 endowment + 30 won). In all cases, this amount will not affect the compensation due to you for your participation; that, of course, will remain the same.

(Next page)

Below, you can see the compositions of the risky deck and of the safe deck you will play with.

They both consist of five cards, each comprising two "winning cards", two "losing cards", and one "neutral card". In the risky deck, "winning cards" and "losing cards" yield more extreme outcomes, as compared to the safe deck.

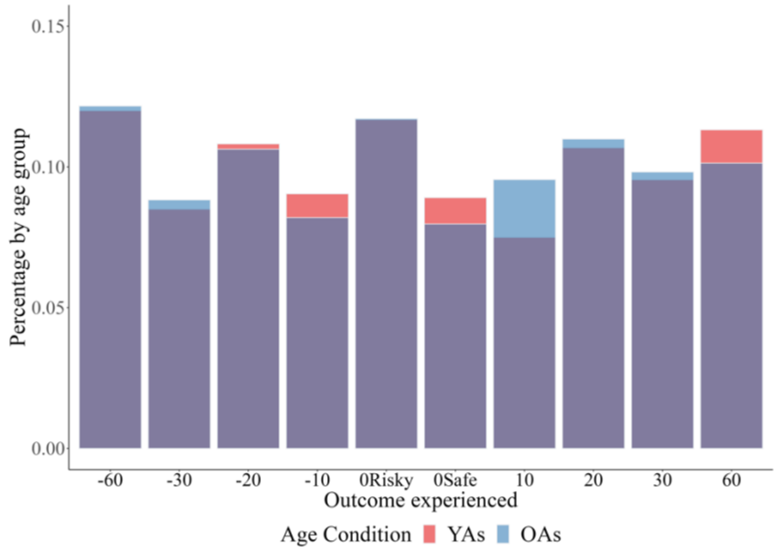
In each round, the risky deck and the safe deck may appear on the left or on the right.

To differentiate the two decks, the backs of their cards have different textures. Also, miniatures of the possible cards that could be drawn are represented above their respective decks.

Outcomes experienced

Figure B1

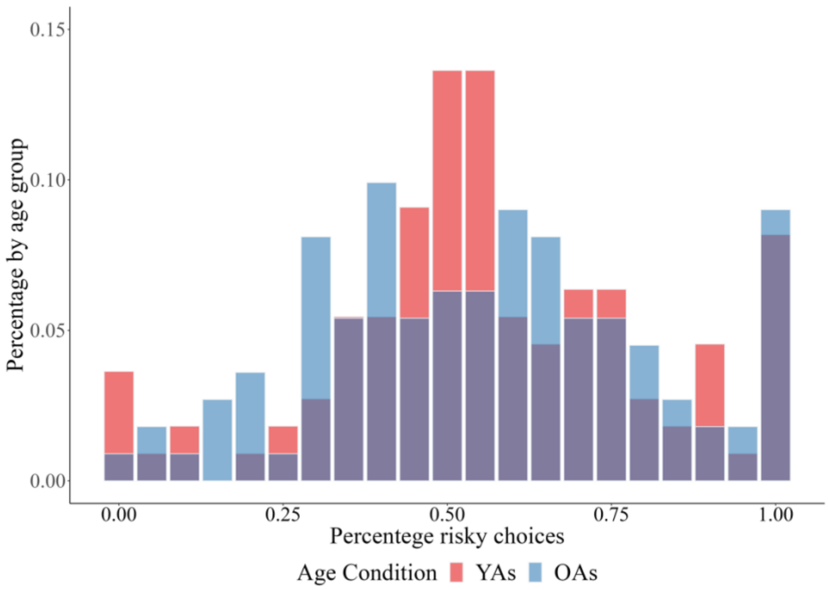
Percentage of the outcomes experienced throughout the rounds of the card-drawing game, according to age group (YAs: younger adults; OAs: older adults)



Deck chosen

Figure B2

Percentage of the number of rounds in which participants chose the risky deck, according to age group (YAs: younger adults; OAs: older adults)



GLMM – Checking behavior

Table B5

Fixed effect results of the GLMM on checking behavior throughout the rounds of the card-drawing game

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	0.239	0.237	1.007	.314	1.27	0.80 – 2.02
Age group (OAs)	0.490	0.237	2.065	.039	1.63	1.03 – 2.60
Outcome experienced (-60)	0.050	0.127	0.396	.692	1.05	0.82 – 1.35
Outcome experienced (-30)	0.339	0.151	2.251	.024	1.40	1.04 – 1.89
Outcome experienced (-20)	0.443	0.132	3.343	.001	1.56	1.20 – 2.02
Outcome experienced (-10)	0.312	0.151	2.062	.039	1.37	1.02 – 1.84
Outcome experienced (0R)	0.504	0.133	3.791	< .001	1.66	1.28 – 2.15
Outcome experienced (0S)	0.025	0.155	0.161	.872	1.03	0.76 – 1.39
Outcome experienced (10)	0.037	0.151	0.245	.806	1.04	0.77 – 1.40
Outcome experienced (20)	-0.441	0.142	-3.113	.002	0.64	0.49 – 0.85
Outcome experienced (30)	-0.613	0.145	-4.219	< .001	0.54	0.41 – 0.72
Age group (OAs) * Outcome experienced (-60)	0.050	0.127	0.392	.695	1.05	0.82 – 1.35
Age group (OAs) * Outcome experienced (-30)	0.035	0.151	0.233	.816	1.04	0.77 – 1.39
Age group (OAs) * Outcome experienced (-20)	0.043	0.132	0.327	.744	1.04	0.81 – 1.35
Age group (OAs) * Outcome experienced (-10)	-0.097	0.151	-0.642	.521	0.91	0.67 – 1.22
Age group (OAs) * Outcome experienced (0R)	0.081	0.133	0.607	.544	1.08	0.84 – 1.41
Age group (OAs) * Outcome experienced (0S)	-0.157	0.155	-1.014	.311	0.85	0.63 – 1.16
Age group (OAs) * Outcome experienced (10)	-0.012	0.151	-0.076	.939	0.99	0.74 – 1.33
Age group (OAs) * Outcome experienced (20)	0.066	0.142	0.466	.641	1.07	0.81 – 1.41
Age group (OAs) * Outcome experienced (30)	0.059	0.145	0.409	.683	1.06	0.80 – 1.41

Note. Deviation coding was used for all predictors. Reference levels were “younger adults” for age group and “+60” for outcome experienced. In brackets are reported the levels to which the estimate value refers to (OAs: older adults; 0R: Zero card drawn from Risky deck; 0S: Zero card drawn from Safe deck).

Table B6

Omnibus effects of the GLMM on checking behavior throughout the rounds of the card-drawing game

Predictor	Chisq	Df	*p*
Intercept	1.01	1	.318
Age group	4.27	1	.039
Outcome experienced	75.79	9	< .001
Age group * Outcome experienced	2.38	9	.984

Table B7

Percentage of rounds in which participants checked the forgone deck, divided by the age group and the outcome experienced in that round

Age group	Previous outcome	Checking percentage
Younger adults	-60	45%
	-30	51%
	-20	52%
	-10	48%
	0 – Risky deck	49%
	0 – Safe deck	48%
	10	47%
	20	37%
	30	41%
	60	41%
Older adults	-60	53%
	-30	64%
	-20	56%
	-10	55%
	0 – Risky deck	55%
	0 – Safe deck	54%
	10	56%
	20	49%

30	54%
60	45%

Table B8

Post-hoc tests (Bonferroni corrected) on the levels of the outcome experienced variable

Contrast	Odds ratio	*SE*	*z*	*p*
-60 / -30	0.75	0.16	-1.387	1
-60 / -20	0.68	0.13	-2.091	1
-60 / -10	0.77	0.16	-1.249	1
-60 / 0 Risky	0.64	0.12	-2.422	.694
-60 / 0 Safe	1.03	0.22	0.119	1
-60 / 10	1.01	0.21	0.063	1
-60 / 20	1.63	0.32	2.506	.549
-60 / 30	1.94	0.40	3.256	.051
-60 / 60	2.03	0.39	3.653	.012
-30 / -20	0.90	0.19	-0.487	1
-30 / -10	1.03	0.23	0.122	1
-30 / 0 Risky	0.85	0.18	-0.774	1
-30 / 0 Safe	1.37	0.31	1.378	1
-30 / 10	1.35	0.30	1.342	1
-30 / 20	2.18	0.48	3.543	.018
-30 / 30	2.59	0.57	4.316	.001
-30 / 60	2.70	0.59	4.578	< .001
-20 / -10	1.14	0.24	0.613	1
-20 / 0 Risky	0.94	0.18	-0.322	1
-20 / 0 Safe	1.52	0.33	1.920	1
-20 / 10	1.50	0.32	1.895	1
-20 / 20	2.42	0.49	4.394	< .001
-20 / 30	2.87	0.60	5.054	< .001
-20 / 60	3.00	0.60	5.500	< .001

-10 / 0 Risky	0.83	0.18	-0.897	1
-10 / 0 Safe	1.33	0.30	1.265	1
-10 / 10	1.32	0.29	1.232	1
-10 / 20	2.12	0.47	3.387	.032
-10 / 30	2.52	0.55	4.203	.001
-10 / 60	2.63	0.58	4.404	< .001
0 Risky / 0 Safe	1.61	0.35	2.191	1
0 Risky / 10	1.60	0.34	2.175	1
0 Risky / 20	2.57	0.52	4.702	< .001
0 Risky / 30	3.06	0.64	5.313	< .001
0 Risky / 60	3.19	0.64	5.825	< .001
0 Safe / 10	0.99	0.22	-0.054	1
0 Safe / 20	1.59	0.36	2.077	1
0 Safe / 30	1.89	0.42	2.872	.183
0 Safe / 60	1.98	0.44	3.069	.097
10 / 20	1.61	0.36	2.161	1
10 / 30	1.92	0.42	2.964	.137
10 / 60	2.00	0.44	3.174	.068
20 / 30	1.19	0.26	0.798	1
20 / 60	1.24	0.25	1.046	1
30 / 60	1.04	0.22	0.202	1

GLMM – Checking behavior, round as covariate

Table B9

Fixed effect results of the GLMM on checking behavior throughout the rounds of the card-drawing game

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	0.267	0.260	1.026	.305	1.31	0.78 – 2.17
Age group (OAs)	0.532	0.260	2.047	.041	1.70	1.02 – 2.84
Outcome experienced (-60)	0.036	0.134	0.273	.785	1.04	0.80 – 1.35
Outcome experienced (-30)	0.482	0.161	3.001	.003	1.62	1.18 – 2.22
Outcome experienced (-20)	0.459	0.140	3.265	.001	1.58	1.20 – 2.08
Outcome experienced (-10)	0.369	0.160	2.307	.021	1.45	1.06 – 1.98
Outcome experienced (0R)	0.474	0.140	3.377	.001	1.61	1.22 – 2.11
Outcome experienced (0S)	0.044	0.167	0.265	.791	1.05	0.75 – 1.45
Outcome experienced (10)	0.085	0.160	0.530	.596	1.09	0.80 – 1.49
Outcome experienced (20)	-0.537	0.150	-3.577	< .001	0.58	0.44 – 0.78
Outcome experienced (30)	-0.625	0.154	-4.052	< .001	0.54	0.40 – 0.72
Mean centered round	-0.140	0.009	-15.327	< .001	0.87	0.85 – 0.89
Age group (OAs) * Outcome experienced (-60)	0.071	0.134	0.531	.595	1.07	0.83 – 1.39
Age group (OAs) * Outcome experienced (-30)	0.044	0.160	0.272	.786	1.04	0.76 – 1.43
Age group (OAs) * Outcome experienced (-20)	0.014	0.140	0.103	.918	1.01	0.77 – 1.34
Age group (OAs) * Outcome experienced (-10)	-0.041	0.160	-0.256	.798	0.96	0.70 – 1.31
Age group (OAs) * Outcome experienced (0R)	0.105	0.140	0.749	.454	1.11	0.84 – 1.46
Age group (OAs) * Outcome experienced (0S)	-0.096	0.167	-0.576	.565	0.91	0.65 – 1.26
Age group (OAs) * Outcome experienced (10)	-0.029	0.160	-0.183	.855	0.97	0.71 – 1.33
Age group (OAs) * Outcome experienced (20)	0.019	0.150	0.125	.901	1.02	0.76 – 1.37
Age group (OAs) * Outcome experienced (30)	-0.016	0.154	-0.103	.918	0.98	0.73 – 1.33

Note. Deviation coding was used for all predictors. Reference levels were “younger adults” for age group and “+60” for outcome experienced. In brackets are reported the levels to which the estimate value refers to (OAs: older adults; 0R: Zero card drawn from Risky deck; 0S: Zero card drawn from Safe deck).

Logistic Regression – Checking behavior

Table B10

Results of the logistic regression on checking behavior in the round selected to compute the bonus payment

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	2.659	90.287	0.029	.977	14.29	0.60 – 5.03E+15
Age group (OAs)	-0.193	90.287	-0.002	.998	0.82	0.25 – 2.74
Outcome experienced (-60)	-1.161	90.289	-0.013	.990	0.31	3.71E-15 – 9.72
Outcome experienced (-30)	-1.739	90.288	-0.019	.985	0.18	1.31E-15 – 5.01
Outcome experienced (-20)	-1.932	90.288	-0.021	.983	0.14	1.56E-15 – 4.42
Outcome experienced (-10)	-1.739	90.288	-0.019	.985	0.18	2.56E-15 – 5.66
Outcome experienced (0R)	-2.057	90.288	-0.023	.982	0.13	1.38E-15 – 3.90
Outcome experienced (0S)	7.275	632.010	0.012	.991	1443.76	0.00 – 1.77E+08
Outcome experienced (10)	-1.966	90.289	-0.022	.983	0.14	1.5E-15 – 4.27
Outcome experienced (20)	-1.107	90.289	-0.012	.990	0.33	1.28E-15 – 8.36
Outcome experienced (30)	6.359	518.660	0.012	.990	577.51	0.00 – 4E+08
Age group (OAs) * Outcome experienced (-60)	0.305	90.289	0.003	.997	1.36	0.39 – 4.97
Age group (OAs) * Outcome experienced (-30)	0.526	90.288	0.006	.995	1.69	0.47 – 6.25
Age group (OAs) * Outcome experienced (-20)	-0.688	90.288	-0.008	.994	0.50	0.14 – 1.79
Age group (OAs) * Outcome experienced (-10)	-0.139	90.288	-0.002	.999	0.87	0.26 – 2.79
Age group (OAs) * Outcome experienced (0R)	-0.591	90.288	-0.007	.995	0.55	0.17 – 1.81
Age group (OAs) * Outcome experienced (0S)	7.825	632.010	0.012	.990	2502.78	7.07E-09 –6.55E+94
Age group (OAs) * Outcome experienced (10)	0.599	90.289	0.007	.995	1.82	0.53 – 6.18
Age group (OAs) * Outcome experienced (20)	1.206	90.289	0.013	.989	3.34	0.97 – 11.20
Age group (OAs) * Outcome experienced (30)	-8.355	518.660	-0.016	.987	0.00	NA – 8.26E+10

Note. Deviation coding was used for all predictors. Reference levels were "younger adults" for age group and "+60" for outcome experienced. In brackets are reported the levels to which the estimate value refers to (OAs: older adults; 0R: Zero card drawn from Risky deck; 0S: Zero card drawn from Safe deck).

GLMM – Switching behavior (previous outcome experienced)

Table B11

Fixed effect results of the GLMM on switching behavior throughout the rounds of the card-drawing game

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	-0.666	0.077	-8.641	< .001	0.51	0.44 – 0.60
Age group (OAs)	-0.054	0.076	-0.714	0.476	0.95	0.82 – 1.10
Previous outcome (-60)	-0.440	0.103	-4.282	< .001	0.64	0.53 – 0.79
Previous outcome (-30)	0.289	0.110	2.633	.008	1.33	1.08 – 1.65
Previous outcome (-20)	-0.423	0.109	-3.885	< .001	0.66	0.53 – 0.81
Previous outcome (-10)	0.294	0.111	2.656	.007	1.34	1.08 – 1.67
Previous outcome (0R)	-0.534	0.108	-4.968	< .001	0.59	0.47 – 0.72
Previous outcome (0S)	0.063	0.114	0.554	. 580	1.07	0.85 – 1.33
Previous outcome (+10)	-0.088	0.114	-0.778	.437	0.92	0.73 – 1.14
Previous outcome (+20)	0.034	0.104	0.331	.741	1.03	0.84 – 1.27
Previous outcome (+30)	0.043	0.107	0.399	.690	1.04	0.85 – 1.29
Age group (OAs) * Previous outcome (-60)	0.005	0.103	0.052	.959	1.01	0.82 – 1.23
Age group (OAs) * Previous outcome (-30)	-0.112	0.110	-1.019	.308	0.89	0.72 – 1.11
Age group (OAs) * Previous outcome (-20)	0.051	0.109	0.468	.639	1.05	0.85 – 1.30
Age group (OAs) * Previous outcome (-10)	0.017	0.111	0.155	.877	1.02	0.82 – 1.26
Age group (OAs) * Previous outcome (0R)	0.018	0.108	0.169	.866	1.02	0.82 – 1.26
Age group (OAs) * Previous outcome (0S)	-0.130	0.114	-1.139	.255	0.88	0.70 – 1.10
Age group (OAs) * Previous outcome (10)	0.021	0.114	0.186	.853	1.02	0.82 – 1.28
Age group (OAs) * Previous outcome (20)	-0.056	0.104	-0.536	.592	0.95	0.77 – 1.16
Age group (OAs) * Previous outcome (30)	0.071	0.107	0.666	.506	1.07	0.87 – 1.32

Note. Deviation coding was used for all predictors. Reference levels were “younger adults” for age group and “+60” for previous outcome. In brackets are reported the levels to which the estimate value refers to (OAs: older adults; 0R: Zero card drawn from Rrsky deck; 0S: Zero card drawn from safe deck).

Table B12

Omnibus effects of the GLMM on switching behavior throughout the rounds of the card-drawing game

Predictor	Chisq	Df	*p*
Intercept	74.66	1	< .001
Age group	0.51	1	.476
Previous outcome	113.99	9	< .001
Age group * Previous outcome	4.14	9	.902

Table B13

Percentage of rounds in which participants switched deck (i.e., passed from the safe deck to the risky one or vice versa) compared to the previous round, divided by the age group and the outcome experienced in the previous round

Age group	Previous outcome	Switching percentage
Younger adults	-60	29%
	-30	49%
	-20	29%
	-10	43%
	0 – Risky deck	27%
	0 – Safe deck	43%
	10	39%
	20	40%
	30	40%
	60	50%
Older adults	-60	28%
	-30	41%
	-20	28%
	-10	43%
	0 – Risky deck	25%
	0 – Safe deck	36%
	10	38%

20	32%
30	39%
60	53%

Table B14

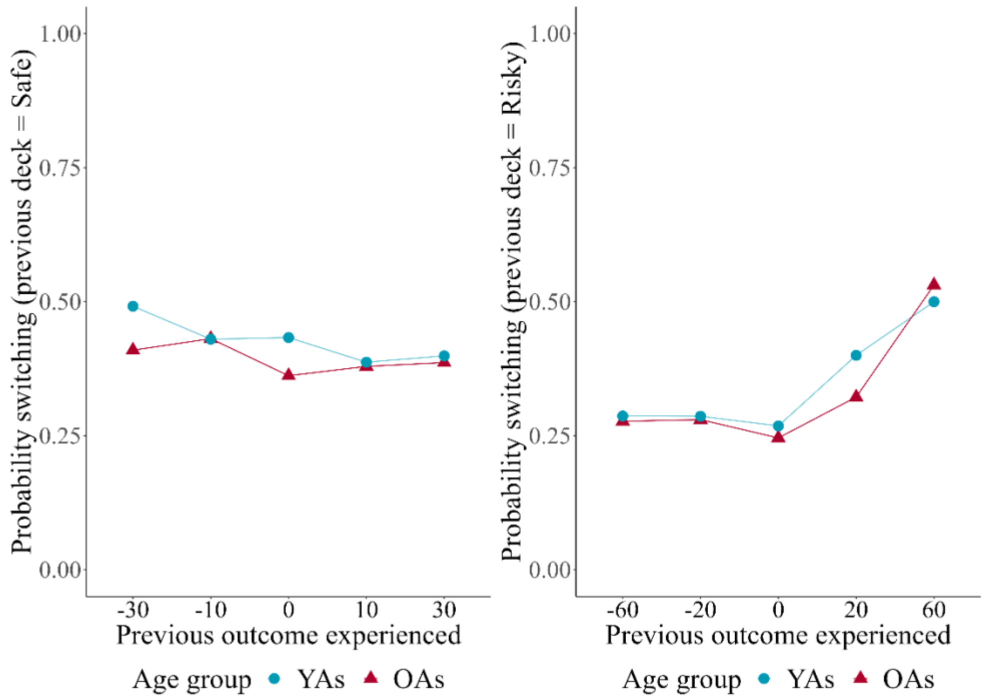
Post-hoc tests (Bonferroni corrected) on the levels of the previous outcome variable

Contrast	Odds ratio	*SE*	*z*	*p*
-60 / -30	0.48	0.08	-4.599	< .001
-60 / -20	0.98	0.15	-0.109	1
-60 / -10	0.48	0.08	-4.597	< .001
-60 / 0 Risky	1.10	0.17	0.612	1
-60 / 0 Safe	0.60	0.10	-3.084	.092
-60 / 10	0.70	0.11	-2.156	1
-60 / 20	0.62	0.09	-3.113	.083
-60 / 30	0.62	0.10	-3.096	.088
-60 / 60	0.30	0.05	-7.870	< .001
-30 / -20	2.04	0.33	4.346	.001
-30 / -10	0.99	0.16	-0.033	1
-30 / 0 Risky	2.28	0.37	5.066	< .001
-30 / 0 Safe	1.25	0.21	1.350	1
-30 / 10	1.46	0.24	2.257	1
-30 / 20	1.29	0.21	1.597	1
-30 / 30	1.28	0.21	1.532	1
-30 / 60	0.62	0.10	-2.967	.135
-20 / -10	0.49	0.08	-4.357	.001
-20 / 0 Risky	1.12	0.18	0.698	1
-20 / 0 Safe	0.62	0.10	-2.898	.169
-20 / 10	0.72	0.12	-1.997	1
-20 / 20	0.63	0.10	-2.898	.169
-20 / 30	0.63	0.10	-2.888	.174

-20 / 60	0.31	0.05	-7.502	< .001
-10 / 0 Risky	2.29	0.38	5.059	< .001
-10 / 0 Safe	1.26	0.21	1.379	1
-10 / 10	1.47	0.25	2.284	1
-10 / 20	1.30	0.21	1.616	1
-10 / 30	1.29	0.21	1.562	1
-10 / 60	0.63	0.10	-2.908	.163
0 Risky / 0 Safe	0.55	0.09	-3.577	.016
0 Risky / 10	0.64	0.11	-2.677	.334
0 Risky / 20	0.57	0.09	-3.646	.012
0 Risky / 30	0.56	0.09	-3.602	.014
0 Risky / 60	0.27	0.04	-8.268	< .001
0 Safe / 10	1.16	0.20	0.895	1
0 Safe / 20	1.03	0.17	0.177	1
0 Safe / 30	1.02	0.17	0.127	1
0 Safe / 60	0.50	0.08	-4.254	.001
10 / 20	0.88	0.14	-0.754	1
10 / 30	0.88	0.14	-0.801	1
10 / 60	0.43	0.07	-5.174	< .001
20 / 30	0.99	0.16	-0.052	1
20 / 60	0.48	0.07	-4.774	< .001
30 / 60	0.49	0.08	-4.552	< .001

Figure B3

Probability of switching deck based on the outcome experienced in the previous round, according to age group (YAs: younger adults; OAs: older adults)



GLMM – Switching behavior (previous counterfactual outcome)

Table B15

Fixed effect results of the GLMM on switching behavior throughout the rounds of the card-drawing game

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	-0.653	0.110	-5.955	< .001	0.52	0.42 – 0.65
Age group (OAs)	-0.040	0.109	-0.369	.712	0.96	0.78 – 1.19
CFTO (Avoided loss – Choosing risky deck)	0.229	0.125	1.840	.066	1.26	0.99 – 1.61
CFTO (Double zero – Choosing risky deck)	-0.580	0.281	-2.068	.039	0.56	0.32 – 0.97
CFTO (Missed gain – Choosing risky deck)	-0.238	0.120	-1.990	.047	0.79	0.62 – 1.00
CFTO (Avoided loss – Choosing safe deck)	0.220	0.124	1.768	.077	1.25	0.98 – 1.59
CFTO (Double zero – Choosing safe deck)	0.079	0.330	0.239	.811	1.08	0.57 – 2.07
Age group (OAs) * CFTO (Avoided loss - Risky)	-0.091	0.124	-0.732	.464	0.91	0.72 – 1.16
Age group (OAs) * CFTO (Double zero - Risky)	0.177	0.281	0.630	.529	1.19	0.69 – 2.07
Age group (OAs) * CFTO (Missed gain - Risky)	0.072	0.120	0.603	.546	1.07	0.85 – 1.36
Age group (OAs) * CFTO (Avoided loss - Safe)	-0.242	0.125	-1.940	.052	0.79	0.62 – 1.00
Age group (OAs) * CFTO (Double zero - Safe)	0.121	0.330	0.366	.714	1.13	0.59 – 2.16

Note. Deviation coding was used for all predictors. Reference levels were “younger adults” for age group and “Missed gain – Choosing safe deck” for counterfactual outcome. In brackets are reported the levels to which the estimate value refers to. (OAs; older adults; CFTO: Counterfactual outcome; 0R: Zero card drawn from Risky deck; 0S: Zero card drawn from Safe deck).

Table B16

Omnibus effects of the GLMM on switching behavior throughout the card-drawing game

Predictor	Chisq	Df	*p*
Intercept	35.46	1	< .001
Age group	0.14	1	.712
Previous outcome	22.90	5	< .001
Age group * Previous outcome	5.86	5	.320

Table B17

Percentage of rounds in which participants switched deck (i.e., passed from the safe deck to the risky one or vice versa) compared to the previous round, divided by the age group and the outcome experienced in the previous round

Age group	Counterfactual outcome	Switching percentage
Younger adults	Avoided loss – Choosing risky deck	42%
	Double zero – Choosing risky deck	23%
	Missed gain – Choosing risky deck	31%
	Avoided loss – Choosing safe deck	46%
	Double zero – Choosing safe deck	36%
	Missed gain – Choosing safe deck	44%
Older adults	Avoided loss – Choosing risky deck	36%
	Double zero – Choosing risky deck	24%
	Missed gain – Choosing risky deck	32%
	Avoided loss – Choosing safe deck	37%
	Double zero – Choosing safe deck	42%
	Missed gain – Choosing safe deck	43%

Table B18

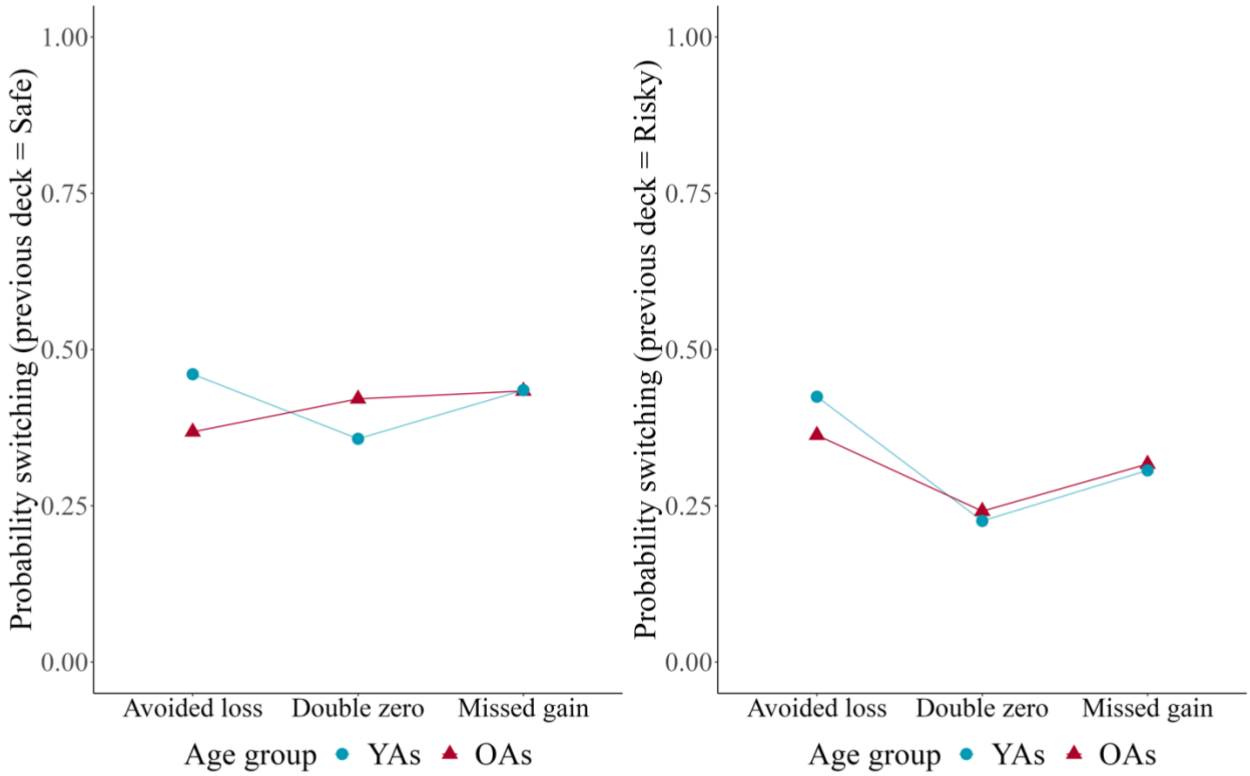
Post-hoc tests (Bonferroni corrected) on the previous outcome variable

Contrast	Odds ratio	*SE*	*z*	*p*
Avoided loss - Choosing risky / Double zero - Choosing risky	2.25	0.76	2.378	.261
Avoided loss - Choosing risky / Missed gain - Choosing risky	1.60	.23	3.308	.014
Avoided loss - Choosing risky / Avoided loss - Choosing safe	1.01	.15	.063	1
Avoided loss - Choosing risky / Double zero - Choosing safe	1.16	.47	.373	1
Avoided loss - Choosing risky / Missed gain - Choosing safe	.94	.14	-.408	1
Double zero - Choosing risky / Missed gain - Choosing risky	.71	.24	-1.014	1
Double zero - Choosing risky / Avoided loss - Choosing safe	.45	.15	-2.340	.289
Double zero - Choosing risky / Double zero - Choosing safe	.52	.26	-1.299	1
Double zero - Choosing risky / Missed gain - Choosing safe	.42	.14	-2.549	.162

Missed gain - Choosing risky / Avoided loss - Choosing safe	.63	.09	-3.248	.017
Missed gain - Choosing risky / Double zero - Choosing safe	.73	.29	-.791	1
Missed gain - Choosing risky / Missed gain - Choosing safe	.59	.08	-3.739	.003
Avoided loss - Choosing safe / Double zero - Choosing safe	1.15	.46	.350	1
Avoided loss - Choosing safe / Missed gain - Choosing safe	.93	.13	-.485	1
Double zero - Choosing safe / Missed gain - Choosing safe	.81	.33	-.524	1

Figure B4

Probability of switching deck based on the counterfactual outcome experienced in the previous round, according to age group (YAs: younger adults; OAs: older adults)



Experiment 6 – Deliberate Choice, Costly Information

Instructions

In this experiment, you will be asked to play **20 rounds of a quick card-drawing game**. Each round comes with an **endowment of 65 pence.**

In the beginning, you will be presented with two decks of cards: one risky deck and one safe deck. The two decks consist of three types of cards: "winning cards" that produce a gain of a certain number of pence, "losing cards" that produce a loss of a certain number of pence, and "neutral cards" that produce neither a gain nor a loss of pence.

The difference between the two decks is that "winning cards" and "losing cards" yield more extreme outcomes in the risky deck than in the safe deck (i.e., the number of pence that can be won, but also lost, is greater in the risky deck than in the safe deck). On the next screen, you will be shown the exact composition of the two decks.

Each round proceeds as follows. The two decks will be shuffled. Then, your task will be to turn over the top card from one of these decks. You will then see whether the card you have turned over is a "winning card", a "losing card", or a "neutral card". After that, if you wish, you can spend 5 pence to turn over the top card of the other deck and see what would have happened if you had chosen it. Since each round is independent, taking this action will not have any impact on your endowment or on the outcomes of subsequent rounds.

Depending on the number indicated on the card drawn, each round will end with a gain, a loss, or neither a gain nor a loss of pence (maximum values of the “winning” and “losing” cards: +/-60 pence). Once all 20 rounds of the game have been completed, one round will be randomly selected, and its outcome will be honored with real money. So, if, in the round that is later randomly selected, you choose the risky deck, your bonus payment could be **as little as 0 pence** (= 65 endowment - 60 lost - 5 if you checked the forgone deck) **to as much as 125 pence** (= 65 endowment + 60 won). On the other hand, if you choose the safe deck, your bonus payment could be as little **as 30 pence** (= 65 endowment - 30 lost - 5 if you checked the forgone deck) **to as much as 95 pence** (= 65 endowment + 30 won). In all cases, this amount will not affect the compensation due for your participation; that, of course, will remain the same.

(Next page)

Below, you can see the compositions of the risky deck and of the safe deck you will play with.

They both consist of five cards, each comprising two "winning cards", two "losing cards", and one "neutral card". In the risky deck, "winning cards" and "losing cards" yield more extreme outcomes, as compared to the safe deck.

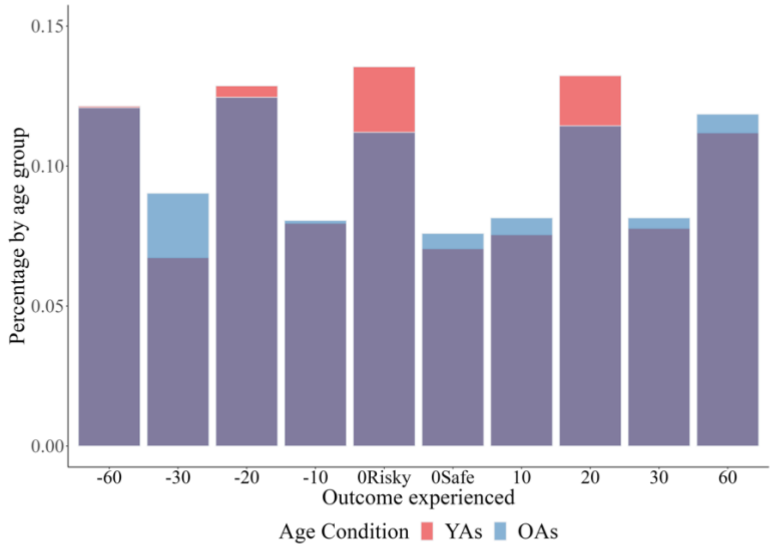
In each round, the risky deck and the safe deck may appear on the left or on the right.

To differentiate the two decks, the backs of their cards have different textures. Also, miniatures of the possible cards that could be drawn are represented above their respective decks.

Outcomes experienced

Figure B3

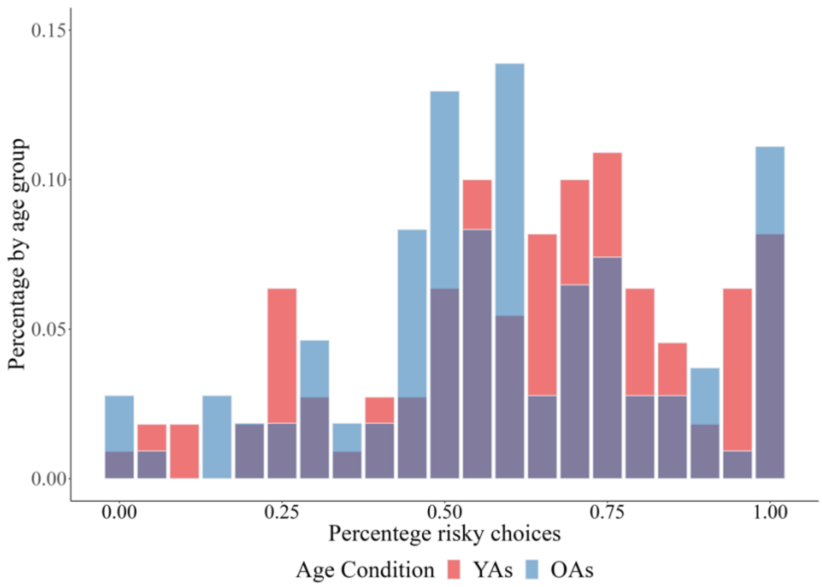
Percentage of the outcomes experienced throughout the rounds of the card-drawing game, according to age group (YAs: younger adults; OAs: older adults)



Deck chosen

Figure B4

Percentage of the number of rounds in which participants chose the risky deck, according to age group (YAs: younger adults; OAs: older adults)



GLMM – Checking behavior

Table B19

Fixed effect results of the GLMM on checking behavior throughout the rounds of the card-drawing game

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	-5.535	0.489	-11.327	< .001	0.004	0.002 – 0.010
Age group (OAs)	0.171	0.278	0.615	.538	1.186	0.689 – 2.044
Outcome experienced (-60)	0.848	0.205	4.141	< .001	2.335	1.563 – 3.488
Outcome experienced (-30)	0.378	0.301	1.256	.209	1.459	0.809 – 2.631
Outcome experienced (-20)	0.718	0.206	3.487	< .001	2.051	1.370 – 3.070
Outcome experienced (-10)	0.809	0.263	3.079	.002	2.247	1.342 – 3.761
Outcome experienced (0R)	0.127	0.216	0.587	.557	1.135	0.743 – 1.735
Outcome experienced (0S)	0.088	0.296	0.299	.765	1.093	0.611 – 1.953
Outcome experienced (10)	-0.214	0.336	-0.638	.524	0.807	0.418 – 1.559
Outcome experienced (20)	-0.758	0.265	-2.863	.004	0.469	0.279 – 0.787
Outcome experienced (30)	-0.612	0.342	-1.787	.074	0.543	0.277 – 1.061
Age group (OAs) * Outcome experienced (-60)	-0.082	0.205	-0.401	.688	0.921	0.617 – 1.375
Age group (OAs) * Outcome experienced (-30)	0.492	0.300	1.639	.101	1.636	0.908 – 2.948
Age group (OAs) * Outcome experienced (-20)	0.142	0.206	0.691	.489	1.153	0.770 – 1.725
Age group (OAs) * Outcome experienced (-10)	-0.259	0.262	-0.989	.323	0.772	0.462 – 1.290
Age group (OAs) * Outcome experienced (0R)	-0.118	0.216	-0.546	.585	0.889	0.582 – 1.358
Age group (OAs) * Outcome experienced (0S)	0.659	0.297	2.220	.026	1.932	1.080 – 3.456
Age group (OAs) * Outcome experienced (10)	-0.727	0.336	-2.165	.030	0.483	0.250 – 0.934
Age group (OAs) * Outcome experienced (20)	0.455	0.265	1.719	.086	1.576	0.938 – 2.646
Age group (OAs) * Outcome experienced (30)	-0.391	0.342	-1.142	.254	0.677	0.346 – 1.323

Note. Deviation coding was used for all predictors. Reference levels were “younger adults” for age group and “+60” for outcome experienced. In brackets are reported the levels to which the estimate value refers to (OAs: older adults; 0R: Zero card drawn from Risky deck; 0S: Zero card drawn from Safe deck).

Table B20

Omnibus effects of the GLMM on checking behavior throughout the rounds of the card-drawing game

Predictor	Chisq	Df	*p*
Intercept	128.30	1	< .001
Age group	0.38	1	.538
Outcome experienced	61.45	9	< .001
Age group * Outcome experienced	16.49	9	.057

Table B21

Percentage of rounds in which participants checked the forgone deck, divided by the age group and the outcome experienced in that round

Age group	Previous outcome	Checking percentage
Younger adults	-60	11%
	-30	3%
	-20	9%
	-10	10%
	0 – Risky deck	11%
	0 – Safe deck	5%
	10	8%
	20	3%
	30	6%
	60	4%
Older adults	-60	16%
	-30	8%
	-20	11%
	-10	9%
	0 – Risky deck	10%
	0 – Safe deck	11%
	10	3%
	20	7%

30	3%
60	7%

Table B22

Post-hoc tests (Bonferroni corrected) on the levels of the outcome experienced variable

Contrast	Odds ratio	*SE*	*z*	*p*
-60 / -30	1.60	0.61	1.234	1
-60 / -20	1.14	0.32	0.463	1
-60 / -10	1.04	0.35	0.112	1
-60 / 0 Risky	2.06	0.61	2.443	.656
-60 / 0 Safe	2.14	0.81	1.994	1
-60 / 10	2.89	1.21	2.535	.506
-60 / 20	4.98	1.70	4.703	< .001
-60 / 30	4.30	1.83	3.425	.028
-60 / 60	9.32	3.46	6.023	< .001
-30 / -20	0.71	0.27	-0.893	1
-30 / -10	0.65	0.27	-1.036	1
-30 / 0 Risky	1.29	0.50	0.640	1
-30 / 0 Safe	1.34	0.59	0.656	1
-30 / 10	1.81	0.87	1.234	1
-30 / 20	3.11	1.34	2.643	.370
-30 / 30	2.69	1.31	2.034	1
-30 / 60	5.83	2.64	3.887	.005
-20 / -10	0.91	0.31	-0.267	1
-20 / 0 Risky	1.81	0.54	1.994	1
-20 / 0 Safe	1.88	0.71	1.661	1
-20 / 10	2.54	1.07	2.221	1
-20 / 20	4.38	1.50	4.307	.001
-20 / 30	3.78	1.61	3.113	.083
-20 / 60	8.19	3.06	5.636	< .001

-10 / 0 Risky	1.98	0.70	1.917	1
-10 / 0 Safe	2.06	0.85	1.735	1
-10 / 10	2.78	1.26	2.269	1
-10 / 20	4.80	1.91	3.942	.004
-10 / 30	4.14	1.89	3.106	.085
-10 / 60	8.97	3.78	5.215	< .001
0 Risky / 0 Safe	1.04	0.40	0.099	1
0 Risky / 10	1.41	0.60	0.801	1
0 Risky / 20	2.42	0.84	2.542	.496
0 Risky / 30	2.09	0.90	1.709	1
0 Risky / 60	4.53	1.68	4.088	.002
0 Safe / 10	1.35	0.65	0.634	1
0 Safe / 20	2.33	0.98	2.012	1
0 Safe / 30	2.01	0.97	1.453	1
0 Safe / 60	4.36	1.95	3.302	.043
10 / 20	1.72	0.79	1.183	1
10 / 30	1.49	0.76	0.777	1
10 / 60	3.22	1.53	2.465	.617
20 / 30	0.86	0.40	-0.316	1
20 / 60	1.87	0.76	1.549	1
30 / 60	2.17	1.04	1.608	1

GLMM – Switching behavior (previous outcome experienced)

Table B23

Fixed effect results of the GLMM on switching behavior throughout the rounds of the card-drawing game

Predictor	Estimate	*SE*	*z*	*p*	Odds ratio	95% CI odds ratio
Intercept	-0.673	0.075	-8.976	< .001	0.51	0.44 – 0.59
Age group (OAs)	-0.040	0.074	-0.536	.592	0.96	0.83 – 1.11
Previous outcome (-60)	-0.579	0.110	-5.256	< .001	0.56	0.45 – 0.70
Previous outcome (-30)	0.772	0.118	6.526	< .001	2.16	1.72 – 2.73
Previous outcome (-20)	-0.748	0.109	-6.881	< .001	0.47	0.38 – 0.59
Previous outcome (-10)	0.492	0.114	4.314	< .001	1.64	1.31 – 2.05
Previous outcome (0R)	-0.421	0.103	-4.084	< .001	0.66	0.54 – 0.80
Previous outcome (0S)	0.241	0.120	2.005	.045	1.27	1.01 – 1.61
Previous outcome (+10)	0.072	0.118	0.610	.542	1.07	0.85 – 1.35
Previous outcome (+20)	-0.121	0.100	-1.213	.225	0.89	0.73 – 1.08
Previous outcome (+30)	0.048	0.116	0.416	.677	1.05	0.84 – 1.32
Age group (OAs) * Previous outcome (-60)	0.245	0.110	2.230	.026	1.28	1.03 – 1.59
Age group (OAs) * Previous outcome (-30)	-0.107	0.118	-0.903	.366	0.90	0.71 – 1.13
Age group (OAs) * Previous outcome (-20)	0.071	0.109	0.650	.516	1.07	0.87 – 1.33
Age group (OAs) * Previous outcome (-10)	-0.187	0.114	-1.640	.101	0.83	0.66 – 1.04
Age group (OAs) * Previous outcome (0R)	0.067	0.103	0.655	.512	1.07	0.87 – 1.31
Age group (OAs) * Previous outcome (0S)	-0.115	0.120	-0.957	.338	0.89	0.70 – 1.13
Age group (OAs) * Previous outcome (10)	-0.077	0.118	-0.655	.512	0.93	0.74 – 1.17
Age group (OAs) * Previous outcome (20)	0.071	0.100	0.712	0.476	1.07	0.88 – 1.31
Age group (OAs) * Previous outcome (30)	0.033	0.116	0.283	0.777	1.03	0.82 – 1.30

Note. Deviation coding was used for all predictors. Reference levels were “younger adults” for age group and “+60” for previous outcome. In brackets are reported the levels to which the estimate value refers to (OAs: older adults; 0R: Zero card drawn from Rrsky deck; 0S: Zero card drawn from safe deck).

Table B24

Omnibus effects of the GLMM on switching behavior throughout the rounds of the card-drawing game

Predictor	Chisq	Df	*p*
Intercept	80.58	1	< .001
Age group	0.29	1	.592
Previous outcome	142.36	9	< .001
Age group * Previous outcome	9.58	9	.386

Table B25

Percentage of rounds in which participants switched deck (i.e., passed from the safe deck to the risky one or vice versa) compared to the previous round, divided by the age group and the outcome experienced in the previous round

Age group	Previous outcome	Switching percentage
Younger adults	-60	20%
	-30	59%
	-20	21%
	-10	54%
	0 – Risky deck	28%
	0 – Safe deck	46%
	10	41%
	20	33%
	30	40%
	60	42%
Older adults	-60	27%
	-30	51%
	-20	23%
	-10	45%
	0 – Risky deck	28%
	0 – Safe deck	42%
	10	39%
	20	35%

30	39%
60	41%

Table B26

Post-hoc tests (Bonferroni corrected) on the levels of the previous outcome variable

Contrast	Odds ratio	*SE*	*z*	*p*
-60 / -30	0.26	0.04	-7.871	< .001
-60 / -20	1.19	0.19	1.049	1
-60 / -10	0.34	0.06	-6.374	< .001
-60 / 0 Risky	0.85	0.13	-1.008	1
-60 / 0 Safe	0.44	0.08	-4.722	< .001
-60 / 10	0.52	0.09	-3.797	.007
-60 / 20	0.63	0.10	-2.968	.135
-60 / 30	0.53	0.09	-3.687	.010
-60 / 60	0.44	0.07	-5.332	< .001
-30 / -20	4.57	0.78	8.903	< .001
-30 / -10	1.32	0.23	1.612	1
-30 / 0 Risky	3.29	0.55	7.166	< .001
-30 / 0 Safe	1.70	0.30	2.985	.127
-30 / 10	2.01	0.36	3.969	.003
-30 / 20	2.44	0.40	5.448	< .001
-30 / 30	2.06	0.36	4.151	.001
-30 / 60	1.70	0.28	3.231	.056
-20 / -10	0.29	0.05	-7.428	< .001
-20 / 0 Risky	0.72	0.11	-2.103	1
-20 / 0 Safe	0.37	0.06	-5.745	< .001
-20 / 10	0.44	0.07	-4.829	< .001
-20 / 20	0.53	0.08	-4.091	.002
-20 / 30	0.45	0.08	-4.718	< .001
-20 / 60	0.37	0.06	-6.490	< .001

-10 / 0 Risky	2.49	0.41	5.614	< .001
-10 / 0 Safe	1.29	0.22	1.444	1
-10 / 10	1.52	0.26	2.433	.673
-10 / 20	1.85	0.30	3.830	.006
-10 / 30	1.56	0.27	2.594	.427
-10 / 60	1.28	0.21	1.556	1
0 Risky / 0 Safe	0.52	0.09	-3.941	.004
0 Risky / 10	0.61	0.10	-2.974	.132
0 Risky / 20	0.74	0.11	-2.023	1
0 Risky / 30	0.63	0.10	-2.855	.194
0 Risky / 60	0.51	0.08	-4.499	< .001
0 Safe / 10	1.18	0.21	0.949	1
0 Safe / 20	1.44	0.24	2.187	1
0 Safe / 30	1.21	0.21	1.094	1
0 Safe / 60	1.00	0.16	-0.019	1
10 / 20	1.21	0.20	1.182	1
10 / 30	1.02	0.18	0.135	1
10 / 60	0.84	0.14	-1.055	1
20 / 30	0.84	0.14	-1.048	1
20 / 60	0.69	0.10	-2.515	.535
30 / 60	0.82	0.13	-1.208	1

Figure B5

Probability of switching deck based on the outcome experienced in the previous round, according to age group (YAs: younger adults; OAs: older adults)