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The relationship between counterfactual thinking and emotional reactions to event outcomes: Does one account fit all?

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By enabling a comparison between what is and what might have been, counterfactual thoughts amplify our emotional responses to bad outcomes. Well-known demonstrations such as the *action effect* (the tendency to attribute most regret to a character whose actions brought about a bad outcome) and the *temporal order effect* (the tendency to undo the last in a series of events leading up to a bad outcome) are often explained in this way. An important difference between these effects is that outcomes are due to decisions in the action effect, whereas in the temporal order effect outcomes are achieved by chance. In Experiment 1, we showed that imposing time pressure leads to a significant reduction in the action but not in the temporal order effect. In Experiment 2, we found that asking participants to evaluate the protagonists ("who ought to feel worse?") led to a significant reduction in the temporal order but not in the action effect. The results suggest that the action and temporal order effects require different explanations and are consistent with other work that suggests that when decisions lead to bad outcomes a comparison of decision quality is an important determinant of the emotional response attributed to the protagonists. The stimulus materials used in our experiments may be downloaded from pbr.psychonomic-journals.org/content/supplemental.

Counterfactual thoughts concern states of affairs that were once possible but never came to pass. Despite their imaginary status, consideration of counterfactual possibilities has been shown to play an important role in phenomena as varied as victim compensation decisions (Miller & McFarland, 1986), attributions of causality (Wells & Gavanski, 1989), and the priming of analytical thought (Kray, Galinsky, & Wong, 2006). In this article, we concern ourselves with experimental investigations of the role played by counterfactuals in emotional reactions to event outcomes. In particular, we examine the generality of a simple account of the relationship between counterfactuals and emotional reactions (see Byrne, 2005; Kahneman & Miller, 1986). According to that account, some events have greater emotional consequences than others because they more readily give rise to a counterfactual, and a comparison between reality and an easily imagined counterfactual alternative leads to emotion amplification.

The first experimental demonstration that we focus on is the action effect in judgments of regret (Gilovich & Medvec, 1995; Kahneman & Tversky, 1982). In Kahneman and Tversky's original demonstration of the effect, participants were presented with the following scenario.

> Mr. Paul owns shares in Company A. During the past year he considered switching it to stock in Company B, but he decided against it. He now finds that he would have been better off by $1,200 if he had switched to the stock of Company B. Mr. George owned shares in Company B. During the past year he switched to stock in Company A. He now finds out that he would have been better off by $1,200 if he had kept his stock in Company B.

The overwhelming majority of participants in this and many subsequent studies attributed more regret to the actor. Kahneman and Miller's (1986) simple and elegant explanation was that inaction is often the norm, whereas action is abnormal, and in order to undo an outcome, counterfactuals tend to be generated that mutate abnormal rather than normal antecedents. A comparison between the actual outcome for the actor and an easily imagined counterfactual—in which he doesn't switch his stock and doesn't lose money—leads to the attribution of greater regret to Mr. George.

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In a different demonstration, Miller and Gunasegaram (1990) gave participants the following scenario.

> Imagine two individuals (Jones and Cooper) who are offered the following very attractive proposition. Each individual is asked to toss a coin. If the two coins come up the same (both heads or both tails), each individual wins $1,000. However, if the two coins do not come up the same, neither individual wins anything. Jones goes first and tosses a head; Cooper goes next and tosses a tail. Thus, the outcome is that neither individual wins anything.

Of the participants, 86% predicted that Cooper, the character who went second, would experience more guilt. The temporal order effect, participants' tendency to undo the last in a chain of events, has been replicated several times (Byrne, Segura, Culhane, Tasso, & Berrocal, 2000; Walsh & Byrne, 2004). It occurs when there are more than two events leading up to the outcome (Segura, Fernandez-Berrocal, & Byrne, 2002), and when the last event is the first mentioned in the sequence (Byrne et al., 2000). Once again, a very simple explanation may be given of these effects: Since the first event in the sequence tends to be presupposed (see Miller & Gunasegaram, 1990), it is easier to generate a counterfactual for the character who went last, and the comparison between reality and the easily imagined counterfactual for that character results in greater regret being attributed to Cooper.

Demonstrations such as the action and temporal order effects were initially used to support claims about regularities in the aspects of reality that people mutate in order to undo an outcome (Kahneman & Miller, 1986). More recently, however, they have been cited as evidence for the extension to the domain of counterfactual thinking (see Byrne, 2005) of the *mental model theory of reasoning*, which holds that people reason by constructing mental models of possibilities consistent with the premises. When applied to counterfactual thinking, the model theory explains phenomena such as the action and temporal order effects by recourse to cognitive economy. For example, Walsh and Byrne (2004) have proposed a computational model of the temporal order effect, in which people represent the factual state of affairs and, to reduce the load on working memory, only a subset of the possible counterfactuals. In Miller and Gunasegaram's (1990) scenario, the factual state of affairs is that Jones tossed heads, Cooper tossed tails, and they lost. According to Walsh and Byrne's account, the counterfactual model that participants are likely to construct is one in which Jones tossed heads, Cooper tossed heads, and they won. Because Cooper's outcome is mutated in the counterfactual model, he is expected to feel more guilt than Jones is. Note that people could, in principle, construct other counterfactual models, but to reduce the load on memory the model theory explanation claims that they do not do so.

Byrne and McEleney (2000) have given a similar explanation for the action effect. When people read the investment scenario, they are said to construct mental models corresponding to predecision states of affairs for the actor and the nonactor. They also construct a model of the postdecision state of affairs for the actor. Because the pre- and postdecision states of affairs are the same for the nonactor, inaction is more economically represented than is action. When participants come to imagine each character's emotional response to the outcome, the representation of the action contained in the model for the predecision state of affairs becomes the basis for a counterfactual in which the actor did not sell his shares and did not lose money. Although participants could, in principle, construct mental models corresponding to the counterfactual possibilities for the nonactor, to reduce the load on memory they tend not to do so.

These explanations for the experimental phenomena are intuitively appealing. However, in the case of the action effect, there are reasons for suspecting that the explanation is too simple. For example, there have been demonstrations that the action effect is observed only when participants can compare the actor and the nonactor, and related claims that the comparison concerns decision quality (Feeney & Handley, 2006; N'gbala & Branscombe, 1997). Although Zhang, Walsh, and Bonnefon (2005) have suggested that the effect is sometimes not observed in a between-participants design because participants in the different conditions use the response scale differently, other studies (Feeney & Handley, 2006) have shown that when participants read about and rate the characters in sequence, an action effect is observed only when participants read about the actor first. It is unclear how a measurement error explanation would account for these findings. There is clearly disagreement about whether the very simple account that we outlined at the outset works for the action effect.

There has been no similar debate about the nature of the temporal order effect, perhaps because the characters play a game of chance rather than making a decision. Thus, no comparison is possible between these characters on the basis of their decision making. Feeney and Handley (2006) have suggested that the comparison between the decision to act and the decision not to act requires more complex model sets than those suggested by Byrne and colleagues. However, in the absence of comparison between the characters on the basis of their decisions, one might predict that simpler model sets would be constructed in thinking about the temporal order scenario.

To test this intuition, in our first experiment we used a speeded-response manipulation. We asked participants to respond as quickly as possible, or forced them to take more time to consider their decisions about which character felt worse. We reasoned that if the action effect required more complex mental representations and comparison processes than did the temporal order effect, speeding people up might reduce susceptibility to the action effect but not to the temporal order effect. On the other hand, the simple account that we outlined at the outset claims that we observed both effects because there is a readily available counterfactual for one character but not for the other. Such an account would appear to predict that the effect of our timing manipulation should be the same for both phenomena.

EXPERIMENT 1

Method

Participants. In this experiment, 32 female and 32 male undergraduates at Durham University participated.

Design. The experiment had a 2 × 2 mixed design. The between-participants manipulation was time. Participants either had unlimited time to answer the questions about the scenario or were encouraged to answer as quickly as possible. The within-participants variable was scenario type. Participants answered questions about action effect and temporal order effect scenarios.

Materials and Procedure. We constructed 16 scenarios. Eight of these were modeled on action effect scenarios already in the literature (see Gilovich & Medvec, 1995; Kahneman & Tversky, 1982; Landman, 1987) and 8 on temporal order effect scenarios (see Byrne et al., 2000; Miller & Gunasegaram, 1990). For 4 of the action effect scenarios, participants were asked the usual question about which character was likely to feel the most regret, and for 4 of the temporal order effect scenarios, participants were asked the standard question about which character was likely to feel worse. These scenarios may be downloaded as supplemental materials for this article.

The remaining four action and four temporal order effect scenarios were fillers. Once they had read each of these filler scenarios, participants were asked to make a judgment about the characters. For example, in one action effect scenario concerning two characters whose relationships break down after each has an affair but only one of them confesses, participants were asked which character was more likely to have children in the future. These items were included in an attempt to ensure that participants carefully processed each of the experimental scenarios.

Scenarios were presented, one at a time, via PowerPoint on a laptop computer. Once they had read the scenario, participants pressed the space bar and a question printed in black appeared. Once the text had turned to red, participants were allowed to call out their answers, which were recorded by the experimenter. In the speeded condition the text changed color after 2 sec, whereas in the delayed condition the change occurred after 20 sec.

The instructions for both conditions explained that each scenario required participants to make a judgment between two characters and that they would be given as much time as necessary to read the scenarios. They were told that once they had taken as long as they needed in order to read the scenarios they should cause the question to appear by pressing the space bar. Participants in the speeded condition were asked to make their judgment as quickly as possible after the text had changed color. Participants in the delayed condition were instructed to take as much time as they needed. In both conditions, once the question changed color, the scenario disappeared from the screen, leaving only the question behind.

Participants were randomly allocated to conditions, and they carried out the experiment individually. They read the scenarios in one of two random orders; for each scenario, we counterbalanced the order of description for the characters.

Results

We calculated the number of scenarios in which each participant had chosen the actor and the number of scenarios in which each one had chosen the character who went second. These means, broken down by scenario type and condition, are presented in Table 1.

A 2 × 2 mixed-design ANOVA carried out on a mean number of effect-consistent choices revealed a significant main effect of scenario type [$F(1,62) = 16.601$, $MS_e = 6.125$, $p < .001$] and a significant main effect of time [$F(1,62) = 38.049$, $MS_e = 16.531$, $p < .001$]. The interaction was also significant [$F(1,62) = 21.683$, $MS_e = 8.00$, $p < .001$], suggesting that the action and temporal order phenomena are affected differently by speeded responding. Post hoc tests on the means involved in the interaction confirmed this suggestion. Participants chose the actor significantly less in the speeded condition ($M = 2.06$; $SD = .564$) than in the delayed condition ($M = 3.28$; $SD = .634$) [$t(62) = 8.121$, $p < .001$]. However, the difference between the speeded ($M = 3$; $SD = .672$) and delayed ($M = 3.22$; $SD = .659$) conditions in the tendency to choose the character who went second was nonsignificant [$t(63) = 0.193$, $p > .05$].

Table 1
Means and Standard Deviations of the Agency and Temporal Order Scores, Broken Down by Condition

	Speeded		Delayed		Overall	
Scenario	*M*	*SD*	*M*	*SD*	*M*	*SD*
Action	2.06	.564	3.28	.634	2.67	.856
Temporal	3.00	.672	3.22	.659	3.11	.669

Note—In each case, the maximum score is 4.

Single sample *t* tests were performed on the scores from each condition of the design to investigate whether the data demonstrated significant action and temporal order effects compared with those expected by chance (i.e., showing the effect in two out of the four scenarios). In the delayed condition, these tests revealed significant action [$t(31) = 11.428$, $p < .001$] and temporal order effects [$t(31) = 10.459$, $p < .001$]. However, although we found a significant temporal order effect in the speeded condition [$t(31) = 8.418$, $p < .05$], there was no evidence of an action effect [$t(31) = 0.626$, $p = .536$].

Discussion

The results of this experiment confirmed our predictions: There was no evidence of an action effect in the speeded condition, but there was a clear temporal order effect. In addition, our timing manipulation had a significant effect on rates of the action effect but no effect on rates of the temporal order effect. These results are consistent with the claim that, whereas the temporal order effect requires a simple comparison between what is and what could have been for the character who goes last, the action effect is determined by a between-decision comparison process that operates on a more complex representation of counterfactual possibilities (see Feeney & Handley, 2006).

In Experiment 2, we employed a different manipulation, designed to show that the phenomena are different. This time we predicted that our experimental manipulation would result in a significant reduction in the number of participants susceptible to the temporal order effect.

EXPERIMENT 2

Our manipulation in Experiment 2 was of the nature of the judgment that participants made about the characters in the scenario. We asked participants to predict who would feel worse, or have the most regret, or to make an evaluative judgment about who *ought* to feel worse or have the most regret; we also gave them the opportunity to indicate that the characters should feel equally bad. Our

claim is that the action effect is dependent on a comparison of quality between the decision to act and the decision not to act. If this is so, asking participants to evaluate the protagonists should result in the judgment that the actor ought to feel the most regret because of his bad decision. This prediction holds even when participants have the option to say that there ought to be no difference between the characters. On the other hand, for the temporal order effect, a decision-quality comparison is impossible. Hence, when participants are asked to evaluate the protagonists, the addition of the extra option should lead to a reduction in the size of the effect. It is unclear how to extend the simple account of the phenomena that we described in the introduction in order to explain any such finding.

Method

Participants. In this experiment, 193 female and 50 male students at Queen's University Belfast participated.

Materials and Procedure. Each participant read either the version of Kahneman and Tversky's (1982) investment scenario or the version of Miller and Gunasegaram's (1990) coin scenario that had been used in Experiment 1. Three versions of each scenario were constructed. The baseline version of each was identical to that used in the previous experiments, where participants were asked "Which character will feel the most regret?" or "Which character will feel worse?" and were required to choose one or the other character. In the control version, the standard scenario and question were used, and it was possible to indicate that the characters would feel equal regret, or feel equally bad. In the experimental version, participants were asked, "Which character ought to feel the most regret?" or "Which character ought to feel worse?" and the additional response option was included. We counterbalanced the order in which the characters appeared in the scenarios and the order of the answer options.

Results

The number of participants who selected each option for each of the three versions of the action and temporal order scenarios is presented in Table 2. In the baseline condition, clear action and temporal order effects of approximately equal size [$\chi^2(1) = 2.27, p = .13$] were demonstrated, with the majority of participants selecting the actor and the second player. Thus, with these materials and this sample, it was possible to convincingly demonstrate both the basic effects.

A chi-square to test the mutual independence of the three variables in this study (see Everitt, 1977) was significant [$\chi^2(4) = 17.51, p < .005$]. To test our hypotheses about the nature of the interaction, for each scenario type we analyzed the frequency with which people did and did not show the basic effect in the control and experimental conditions. For the action effect, the distribution of these frequencies did not differ from that expected by chance [$\chi^2(1) = 0.99, p = .32$]. Although the addition of a third option appeared to reduce the size of the action effect overall, a substantial number of participants selected the actor in both conditions. For the temporal order effect, on the other hand, the distribution of frequencies did differ from that expected by chance [$\chi^2(1) = 8.04, p < .005$]. Once again, although the addition of a third option led to a decrease in the number of participants selecting the second player in the control condition relative to the baseline condition, almost half of the participants in the control condition selected the second player. However, few participants in the experimental condition selected the second player. A further chi-square test revealed that for the control condition, distribution of the frequencies in the action and temporal order scenarios did not deviate significantly from chance [$\chi^2(1) = 2.04, p = .15$]. A similar comparison in the experimental condition was statistically significant [$\chi^2(1) = 10.25, p = .001$]. Thus, asking who ought to feel the most regret, or feel worse, results in a substantial, if somewhat reduced, action effect, but significantly attenuates the size of the temporal order effect.

Discussion

Compared with a control where an equivocal response option is provided, an experimental condition with an additional response option in which people are asked who ought to feel the most regret or who ought to feel worse reduces the size of the temporal order effect, but not the action effect. This finding is consistent with the claim that the action effect is, at least in part, the result of a quality comparison between the decision to act and the decision not to act. Because the outcome achieved by the characters in temporal order effect scenarios is due to chance, asking who ought to feel worse results in high rates of equivocal responding. It is unclear how an account which claims that both effects are due to a single comparison between reality and a counterfactual state of affairs could ever predict that the same experimental manipulation would cause only one of the effects to disappear.

GENERAL DISCUSSION

Our results suggest that the action and temporal order effects may resist attempts to explain them in the same

Table 2
Number of Participants in Experiment 2 Selecting Each Option, Broken Down by Scenario Type and Condition

		Condition		
			Experimental	
Phenomenon	Option	Baseline	Control	Experimental
Agency	Nonactor	7	1	2
	Actor	34	24	19
	Both	–	16	19
Temporal	1st Player	12	6	5
	2nd Player	26	18	6
	Both	–	18	30

terms (see Byrne, 2005). The action effect is reduced under speeded responding and persists when people are asked to evaluate the characters. However, the temporal order effect is immune to speeded responding but disappears when people are asked to evaluate the protagonists. The results of Experiment 1 in particular are problematic for the mental model theory of thinking, which explains the action and temporal order effects by claiming that, to minimize the load on memory, people represent a counterfactual only for the actor and for the character who went second. Given that a principle of cognitive economy is applied to explain both phenomena, it is unclear why they should behave so differently under conditions of speeded responding.

The results of Experiment 2 are perhaps less troublesome for the theory, since the experiment was designed to explain how people answer predictive (i.e., who will feel a certain way) rather than evaluative (i.e., who ought to feel a certain way) questions about the protagonists. However, it is unclear how the theory might be extended to capture the results of Experiment 2. Note here that the mental model account of the temporal order effect (Walsh & Byrne, 2004) is unchallenged by any of our results and is important because it provides a clearly worked-out explanation for the intuitively appealing claim that the most recent in a chain of events will be undone in order to undo a bad outcome.

Apart from what they tell us about action and temporal order effects, our results suggest psychological differences between the evaluation of chance outcomes and outcomes that are arrived at by a decision. For the former, it is the outcome that is all-important, and people's emotional reactions appear to be determined by a comparison between the actual bad outcome and an imagined better outcome. For outcomes that are the result of decisions, the quality of the decision is also important (see also Connolly & Zeelenberg, 2002); when the same bad outcome befalls two decision makers, people appear to compare the quality of their decisions. A challenge for accounts of counterfactual thinking is to specify mental representations and comparison processes that are sufficiently rich to support judgments about decisions as well as judgments about outcomes.

AUTHOR NOTE

Experiment 1 was carried out at Durham University by the first two authors under the supervision of the third author, as partial fulfillment of the requirements for their BSc in Applied Psychology. Correspondence concerning this article should be addressed to A. Feeney, School of Psychology, Queen's University Belfast, University Road, Belfast BT7 1NN, Northern Ireland (e-mail: a.feeney@qub.ac.uk).

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SUPPLEMENTAL MATERIALS

The scenario stimuli we used may be downloaded as supplemental materials from pbr.psychonomic-journals.org/content/supplemental.

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