

Affect in Judgement and Decision-Making:

The Role of Context

*Thesis submitted by Nick Sevdalis to University College London
for the fulfilment of the requirements of the
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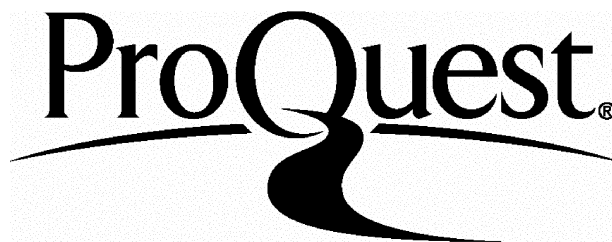
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Abstract

There is empirical evidence that when people make decisions they are not only interested in the material outcomes of the decisions but they also try to maximise their post-decisional pleasure and minimise their post-decisional pain. This strategy implies that people anticipate their post-decisional emotions. However, research has also shown that people overestimate their emotional reactions to future events – i.e. they predict them to be more intense than they actually are. This “impact bias” has been attributed to the fact that people tend to focus too much on the future events in question, thereby neglecting other future occurrences that will distract their attention and, as a consequence of this distraction, attenuate their emotional reactions. I tested the hypothesis that the influence of emotional anticipations on decisions is due to the “impact bias”. In other words, people overestimate how happy or unhappy they will become as a result of the outcomes of a decision. As a result of these mispredictions, when people face decisions with uncertain prospects they opt for alternatives that minimise the potential for future negative emotional reactions. In three scenario-based investment studies, I found evidence that people adopt a “regret-minimising” investment strategy only when they focus narrowly on the decision in question, but not when they see the decision in a broader context. In two negotiation studies and one study of risky choice, I found that people overpredicted their post-decisional emotional reactions to the outcomes of both tasks. In a more naturalistic study, I found that students overpredicted what their emotional reactions would be when they received feedback on academic assignments. Finally, in three studies on decision recall, I found that people who were narrowly focused on bad decisions reported feeling worse than people who put the decisions in the broader context of their lives.

To Jonck

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CHAPTER

1

Anticipated post-decisional affect:

Concepts and models

1. Introduction: Predicted and experienced utilities

As Robinson and Clore recently put it, “self report is the most common and potentially the best [...] way to measure a person’s emotional experiences (Robinson & Clore, 2002; p. 934). Building heavily on the availability and validity of self-reported affect, researchers in the Judgement and Decision-Making (JDM) literature have tried to model people’s choices and behaviours as functions of people’s anticipated emotional reactions to the outcomes of their decisions. Examples of these models are Regret Theory (e.g. Bell, 1982; Loomes & Sugden, 1982); Disappointment Theory (Bell, 1985b; Loomes & Sugden, 1986); and Mellers and her colleagues’ concept of Subjective Expected Pleasure (Mellers, Schwartz, & Ritov, 1999). In order to capture all these emotional considerations, Kahneman and his colleagues coined the term “predicted utility” (Kahneman & Snell, 1992; Kahneman, Wakker, & Sarin, 1997).

Over the last 15 years there has been empirical evidence that these anticipations indeed influence choices and behaviours (e.g. Mellers, 2000; Zeelenberg, 1999a). Since there is evidence that when people make decisions they try to maximise their post-decisional pleasure and minimise their post-decisional pain, the next step is to see what actually happens after the outcomes of the decision have materialised. In other words, once the outcomes of the decision become known, then the (pre-decisional) emotional anticipations become (post-decisional) emotional experiences. The transition from anticipated emotions to experienced emotions is conceptually captured by the transition from “predicted utility” to “experienced utility” (Kahneman & Snell, 1992; Kahneman et al., 1997).

Regarding the methodological approaches to the study of these utilities, three standard research practices have been established. The first one is to ask participants to experience a specific affective episode of either positive or negative valence. Pain

or, more generally, discomfort episodes seem to be the most widely preferred. For instance, participants have been asked to immerse their hands in cold water, (Kahneman, Fredrickson, Schreiber, & Redelmeier, 1993); to listen to annoying sounds (Ariely & Loewenstein, 2000); or to endure a heating device that has been adjusted on their arm (Ariely, 1998).

The second methodological approach is to ask participants to judge the degree of discomfort that the episode would entail either for themselves, or for somebody else, without actually experiencing the episode. For instance, participants have been asked to assess the discomfort associated with carrying a heavy suitcase (Varey & Kahneman, 1992); or to rate the degree of negative affect associated with a particular decision made by a hypothetical person (Connolly, Ordóñez, & Coughlan, 1997; Gilovich & Medvec, 1994; Kahneman & Tversky, 1982).

In either of these two methodological cases, after the participants have experienced or imagined the episode, they are then asked to provide the researcher either with a judgement about the degree of discomfort, pain or negative affect associated with the episode, or to make a choice as to whether they would be willing to undergo the episode again (or for the very first time, in the case of imagined episodes). Sometimes, participants are also asked to indicate the minimum amount of money they would demand in order to accept going through the aversive experience.

The third methodological approach is to construct choice sets that consist of options that differ regarding the affective value of their outcomes and then ask participants to make evaluations or choices from these choice-sets. Participants' preferences for some options rather than for others are then used to infer their motivation to maximise the positive affect that was associated with an option or to minimise the negative affect that was associated with another option. For instance,

participants have been asked to choose between regret-minimising and risk-minimising investment options (Zeelenberg & Beattie, 1997).

In all cases, the aim of the researcher is to determine whether a particular variable, or combination of variables, influences the utility judgements or behavioural choices of the participants. In most cases, before asking participants to make a judgement or a choice, the researcher has experimentally manipulated the presence or the level of a particular variable. Subsequently, the measures that are obtained are assumed to capture the effects of the manipulated variable on judgements, choices, or other behaviours.

2. Models of anticipated post-decisional affect in the laboratory

Affective influences at the pre-decisional stage emanate from one of three distinct categories: Mood-related influences, or automatic visceral reactions, or, finally, anticipated emotional reactions to decision outcomes (Hastie, 2001; Loewenstein, Weber, Hsee, & Welch, 2001; Mellers et al., 1998). As Loewenstein and his colleagues (2001; p. 268) put it, “to the extent that JDM research has addressed emotions, the emotions that have been taken into account are anticipated emotions”¹. Therefore, in what follows we shall focus on models of these anticipated affective reactions. For reviews of mood-related influences see Mellers et al. (1998; p. 453-454 – up to 1996) and Hastie (2001; p. 672 – up to 2000). For a review of the influences of automatic visceral influences see Loewenstein (1996) and Loewenstein et al. (2001).

Lopes (1987) and Elster and Loewenstein (1992) offer a bipolar view of the anticipatory period before the resolution of the uncertainty inherent in any risky

¹ For instance, Svenson’s (2003) integration of affective influences into the Differentiation Consolidation Theory (Diff Con) refers en passant to the avoidance of decision-related regret as a basic motivation for the post-decisional consolidation; however, the affective influences that Diff Con addresses explicitly are mood-related ones.

choice. According to the bipolar analysis, “hope and fear”, in Lopes’ terms or “savouring and dread”, in Elster and Loewenstein’s terms, operate throughout the pre-outcome period. These views are bipolar in the sense that they strongly differentiate between the valence of the expectations that the decision maker holds before the outcomes of his decision are known. Implicit in Lopes’ (1987) discussion and explicit in Elster and Loewenstein’s (1992) discussion is that these anticipatory states convey a utility of their own, which is essentially independent of the experienced utility conveyed by the decision outcome per se.

Other researchers, coming mostly from the economics tradition, have attempted to describe these affective anticipations in more formal terms – in other words, they have attempted to model them. So far these attempts have culminated in three models of anticipated affective reactions: Regret Theory (RT); Disappointment Theory (DT); and Decision Affect Theory – Subjective Expected Pleasure (DAT – SEP). In what follows we present separately each one of these models.

2.1. Regret Theory (RT)

The idea that people take into consideration the potential regret that is associated with a particular course of action was known well before the formal development of RT. It provided Wald (1939; 1950) and Savage (1951) with the basis for their minimax principle. The minimax principle postulates that a reasonable choice strategy is for the decision-maker to choose such a course of action that the maximum potential loss shall in any case be as small as possible. In other words, minimise the maximum loss, thus the name of the principle.

Janis and Mann (1977) devoted a whole chapter of their influential book to their concept of “anticipatory regret”, which is essentially the type of regret I am discussing here. In their words, “*anticipatory regret* is a convenient generic term to

refer to the main psychological effects of the various worries that beset a decision maker before any losses actually materialize...” (Janis & Mann, 1977; p. 222). Janis and Mann presented choice data straightforwardly explained in terms of regret aversion and they also argued that regret aversion has both positive effects (e.g. extensive information search) and negative effects (e.g. decision procrastination).

Finally, RT was independently developed by Bell (1982; 1983; 1985a) and Loomes and Sugden (1982; 1983; 1987a – see Larrick, 1993, for a review). RT postulates that

- a. Decision makers compare the actual outcomes of their actions with what those outcomes would have been, had they chosen differently; therefore, they experience either regret (when the comparison favours the foregone choice), or its positive counterpart, i.e. rejoicing (when the comparison favours the actual choice).
- b. The magnitude of their regret is an exponential function of the magnitude of the difference between the outcomes of the chosen and the outcomes of the foregone alternative. The same holds for rejoicing as well, the only difference being that the curve is less steep.²
- c. Decision makers anticipate these positive and negative post-decisional emotions (in other words, they forecast them) and take them into account when they make their choices.

Subsequent modifications to and expansions of the initial version of RT include

- a. The additional postulate by Bell that the outcome of the foregone alternative should in some way be communicated to the decision maker – i.e. there

² For a general theoretical perspective regarding the asymmetry between positive and negative outcomes see Kahneman and Tversky (1979); for a psychological explanation of this asymmetry see Taylor (1991).

should be some feedback on how good the outcome of the decision has been (Bell, 1983).

- b. The empirical documentation by Zeelenberg and his colleagues that risk aversion and regret aversion are orthogonal choice strategies – in other words, one does not necessarily follow from the other (Zeelenberg & Beattie, 1997; Zeelenberg, Beattie, van der Pligt, & de Vries, 1996).
- c. The conceptualisation of regret as a counterfactual emotion. Counterfactuals are “... mental representations of alternatives to the past...” (Roese, 1997; p. 133). Regret has thus been linked to Norm Theory (Kahneman & Miller, 1986; Kahneman & Tversky, 1982) and also to research on the cognitive appraisals and behavioural consequences of different emotions (Frijda, Kuipers, & ter Schure, 1989; Zeelenberg, van Dijk, Manstead, & van der Pligt, 1998).

2.2. Disappointment Theory (DT)

DT was developed by the same researchers that developed RT, i.e. Loomes and Sugden (1986) and Bell (1985b). Later, Gul (1991) proposed a different version of DT, with the intention of supplementing the strictly descriptive nature of the initial version of DT with a more normative theory. For the purposes of this review I shall treat all the above versions as roughly equivalent. Paralleling the postulates of RT, DT postulates that

- a. Decision makers form expectations regarding the outcomes of their decisions.
- b. Decision makers then compare the actual outcomes of their decisions with their earlier formed expectations; therefore, they experience either disappointment (when the actual outcomes fall below these expectations), or

its positive counterpart, i.e. elation (when the actual outcomes exceed these expectations).

- c. The magnitude of their disappointment is a function of a.) the discrepancy between the actual outcomes and the expected outcomes and b.) the probability of the occurrence of the expected outcomes. The same holds for elation, as well, the only difference being that the curve is less steep.
- d. Decision makers anticipate these positive and negative post-decisional emotions and take them into account when they make their choices.

The above outline makes clear two differences between RT and DT. Firstly, whereas in RT actual outcomes are evaluated against possible outcomes that might have been received had a different decision been made, in DT actual outcomes are evaluated against possible outcomes had a different “state of the world” occurred. This alternative state of the world is the expected state of the world. Secondly, disappointment depends on the likelihood of a possible outcome becoming the actual outcome. In other words, because the basis for the experience of disappointment is prior expectations, disappointment unlike regret is sensitive to surprises (Brandstätter, Kühberger, & Schneider, 1999; van Dijk & van der Pligt, 1997). I note, however, that, to the best of my knowledge, the issue of whether regret is also sensitive to the manipulation of the probability of the decision outcomes has never been directly addressed.

2.3. Decision Affect Theory (DAT) and Subjective Expected Pleasure (SEP)

Mellers and her colleagues (Mellers, Schwartz, Ho, & Ritov, 1997; Mellers et al., 1999) proposed a theory that conceptualises post-decision experienced utility as the experienced pleasure or pain that people report once the outcomes of their decisions have materialised. This conceptualisation is in line with the recent re-

framing of the concept of “experienced utility” as experienced pleasure or pain by Kahneman and his colleagues (Kahneman & Snell, 1992; Kahneman et al., 1997). Both approaches to experienced utility revive Bentham’s (1789 / 1996) original conceptualisation.

DAT postulates three distinct predictors of post-decisional affect (i.e. of experienced utility):

- a. The decision utility of the outcome (e.g. the monetary value of risky gambles). The higher the outcome utility, the higher the experienced positive affect.
- b. The decision maker’s expectations about the likelihood of occurrence of both the desirable and the undesirable outcomes. The more unexpected the outcomes, the more intense the emotional reactions to them – for both positive and negative outcomes.
- c. Finally, the counterfactual comparison evoked by the decision maker. This comparison is assumed to counterfactually evaluate the actual outcomes against the potential outcomes. “Upward” counterfactuals (i.e. counterfactuals that improve on reality) give rise to negative emotional reactions, whereas “downward” counterfactuals (i.e. counterfactuals that worsen reality) give rise to positive emotional reactions (Markman, Gavanski, Sherman, & McMullen, 1993).

The above outline makes clear that DAT is similar to DT. The major difference between the two is that DAT focuses on the prediction of post-decisional affect rather than the influence of the anticipation of this post-decisional experience on decision behaviour. However, this link is provided by SEP.

SEP is an extension of the DAT that moves from a pure description of post-decisional affect to a model that also predicts how affective anticipations influence

decisions. Essentially, SEP could be thought of as an attempt to integrate RT, DT and DAT into a bipolar model of anticipated affect. Borrowing concepts and postulates from RT and DT, SEP assumes that

- a. People form emotional anticipations regarding the outcomes of their decisions.
- b. These anticipations are taken into account when people make their choices.

SEP can be thought of as an integrative model because regret and disappointment effects are measured simultaneously; likewise, SEP can be thought of as a basically bipolar model because all self-reported affective reactions are conceptually collapsed into the overarching categories of “pleasure” and “pain”.

3. Empirical evidence: How influential are anticipated emotions on decision behaviours?

3.1. Empirical evidence for RT

The degree of empirical support RT has received so far is somewhat difficult to assess. As far as the initial economic models are concerned, the empirical findings have been rather mixed (for two short reviews of these findings see Larrick, 1993 and Zeelenberg, 1999a). Recent direct tests of the theory can be found in Inman, Dyer, & Jia (1997), Mellers et al. (1999), and Mellers (2000). This research provides support for the predictions of RT.

More generally, the regret-minimising (as contrasted to the risk-minimising) aspect involved in risky choices has received extended empirical support (for a recent review of some of the research described below, see Zeelenberg, 1999a). It has been shown to hold for

- a. Lottery tickets and gambles: People choose lotteries with minimum potential for post-decisional regret (Boles & Messick, 1995; Josephs, Larrick, Steele,

& Nisbett, 1992; Ritov, 1996; Zeelenberg et al., 1996); they are also unwilling to exchange a lottery ticket that they have been endowed with for fear that if it is the winning ticket, they will regret their decision (Bar-Hillel & Neter, 1996).

- b. Financial decisions: People opt for regret-minimising investment options (Butler & Highhouse, 2000; Zeelenberg & Beattie, 1997); they are regret-minimisers in dyadic negotiation tasks (Larrick & Boles, 1995; Zeelenberg & Beattie, 1997); they are willing to forego objectively good bargain opportunities in order to minimise the post-decisional regret that stems from them having missed a better bargain opportunity (a phenomenon that has been termed “inaction inertia” – Arkes, Kung, & Hutzler, 2002; Tykocinski, Israel, & Pittman, in press; Tykocinski & Pittman, 1998; 2001; Tykocinski, Pittman, & Tuttle, 1995); they time their purchasing decisions in order to avoid post-purchase regret (Cooke, Meyvis, & Schwartz, 2001; Inman & Zeelenberg, 1998; Simonson, 1992).
- c. Medical decisions: People choose not to vaccinate their children against life-threatening diseases if the vaccination decision entails post-decisional regret (Ritov & Baron, 1990; 1995; Yaniv, 2000); regret-avoidance also influences people’s decisions to take medical screening tests (Lechner, de Vries, & Offermans, 1997); to refrain from binge drinking (Murgraff, McDermott, White, & Phillips, 1999); and to use condoms during sexual intercourse (Richard, van der Pligt, & de Vries, 1995; 1996).
- d. Driving behaviour: People refrain from committing driving violations if they are primed with the post-violation regret (Parker, Manstead, & Stradling, 1995; Parker, Stradling, & Manstead, 1996).

- e. Decision avoidance: People procrastinate decisions that entail potential for high post-decisional regret (Beattie, Baron, Hershey, & Spranca, 1994).

In addition, experienced regret and regret-related counterfactual thinking have been shown to be an important consideration in people's lives in naturalistic settings (Gilovich & Medvec, 1994; 1995; Medvec, Madey, & Gilovich, 1995; Zeelenberg, van der Pligt, & Manstead, 1998). For instance, in samples of professional athletes, Medvec et al. (1995) showed that whereas bronze Olympic medallists counterfactually compared themselves to non-medallists, silver medallists counterfactually compared themselves to golden medallists. These comparisons resulted in the bronze medallists feeling happier than silver medallists – a rather perplexing finding.

3.2. Empirical evidence for DT

Loomes and Sugden (1987b) and Loomes (1988) provided some empirical support for their formulation of DT. As Zeelenberg and his colleagues pointed out research on the effect of anticipated disappointment on decision-making is scarce (Zeelenberg, van Dijk, Manstead, & van der Pligt, 2000). In the only published paper that tackles this question, van Dijk, Zeelenberg, and van der Pligt (2003) found that people indeed lower their expectations when self-relevant feedback on some decision of theirs is imminent.

In general, research on the lowering or the manipulation of expectations right before the disclosure of the decision outcome that can be thought of as indirectly supportive of DT's postulates about unmet expectations. On the one hand, lower expectations are, by definition, more likely to be met than higher expectations (Armor & Taylor, 1998). On the other hand, surprising negative outcomes are more aversive than expected negative outcomes (Feather, 1967; 1969; Spector, 1956;

Verinis, Brandsma, & Cofer, 1969). Taken together, these two assertions mean that the lower the initial expectations, the less likely they are to be disconfirmed and, as a logical corollary, the less likely is the decision maker to experience disappointment. This hypothesis has been supported by empirical findings (Carlsmith, 1962 – cited in Pyszczynski, 1982; Gilovich, Kerr, & Medvec, 1993; Loewenstein & Linville, 1986; Nisan, 1972; Pyszczynski, 1982; Shepperd, Ouellette, & Fernandez, 1996; Taylor & Shepperd, 1998)³.

Lowering of expectations can also occur *ex post facto*, thus taking the form of a hindsight effect (Fischhoff, 1975; Hawkins & Hastie, 1990): People have been found to re-evaluate downwards their initial chances of achieving a goal once this goal has been missed (Tykocinski, 2001). In addition to expectations, other elements of the past can also be subjected to this adjustment procedure: Ross and his collaborators (Conway & Ross, 1984; Ross, 1989; Ross & Newby-Clark, 1998) have shown that people revise their personal histories in the light of their current state of being. These revisions are made on the basis of relevant implicit theories of either stability or change. Finally, in order to be acceptable, the revision should satisfy a certain reasonability criterion (Armor & Taylor, 1998).

A second means to avoid disappointment is based on the generation of excuses for an aversive outcome: The decision maker can behave in a way that will provide him with a “good enough reason” for failure – i.e. a reason that will explain away the unfortunate outcome (e.g. Snyder & Higgins, 1988; for a review see Armor & Taylor, 1998; for early work on the “fear of failure” see Atkinson, 1964).

³ Another line of research that could be thought of as relevant here is research conducted on Temporal Construal Theory (TCT – Liberman & Trope, 1998; Trope & Liberman, 2000). According to TCT people approach decisions with desirability considerations when decision implications (i.e. decision outcomes) lie far in the future, whereas they switch to feasibility considerations as these implications gradually become more imminent. Although this reversal is explained in cognitive terms (contextualised versus de-contextualised construals for the distant and near future, respectively), I think that a motivational account (i.e. disappointment aversion) could also be valid (e.g. Gilovich et al., 1993; Shepperd et al., 1996; Taylor & Shepperd, 1998).

The literature on “egotism” (i.e. the motivation to protect one’s self – esteem against anticipated negative outcome feedback) provides empirical support for this defensive strategy (e.g. Berglas & Jones, 1978; Frankel & Snyder, 1978; Snyder, Smoller, Strenta, & Frankel, 1981).

Finally, dissonance-reduction-based reasoning has been found effective in reducing potential disappointment: The decision maker can convince himself that the outcome was not so desirable after all (Carlsmith, 1962 – In Pyszczynski, 1982; Festinger, 1957; Pyszczynski, 1982).

3.3. Empirical evidence for DAT – SEP

The initial experimental results are supportive of the model’s postulates (Mellers et al., 1999; Mellers, 2000). In addition, these results showed that SEP has greater explanatory power than both the minimax principle and RT (Mellers, 2000) – given Mellers et al.’s (1997) position that DAT (and therefore SEP) is similar to DT, there is no comparison between SEP and DT in terms of their relative explanatory power.

Shepperd and McNulty (2002) compared DAT with “cognitive consistency” theories (for instance, the theory of “cognitive dissonance”, Aronson, 1958; Festinger, 1957; see also Heider, 1958). These theories predict that unexpected outcomes feel worse than expected ones regardless of their positive or negative valence. This is because unexpected outcomes demonstrate people’s inability to generate accurate forecasts. In the three empirical studies that Shepperd and McNulty (2002) reported, these predictions were not supported, leading these researchers to the conclusion that DAT provides a more accurate model of affective experiences.

4. Should we or should we not anticipate post-decisional emotions? Some functional and normative considerations

Common in all the models of anticipated post-decisional affect is the notion that a decision maker who systematically anticipates post-decisional affect and factors these anticipations into the decision process behaves rationally. In other words, the inclusion of these anticipations in the decision utility function is taken to be beneficial and in no way does it constitute a sign of irrational behaviour.

For instance, Loomes and Sugden (1982) argue that there are two main reasons for their modification of the traditional utility function. The first reason is that the new utility function (i.e. the one that incorporates anticipated regret) offers a better account of the empirical findings. The second reason is that the decision maker who seeks to maximise this modified function behaves rationally.

A more elaborate argument on the rationality of regret anticipation comes from the functional analysis of counterfactual thinking in general – which, of course, encompasses regret and disappointment, since these are defined as counterfactual emotions. According to this analysis (Roese, 1997), counterfactual thinking can have both negative and positive aspects for the individual.

The negative aspect arises from contrast effects, which are assumed to be the first mechanism that produces counterfactual consequences for the individual. Any post-hoc comparison of an obtained outcome with its counterfactual alternative is capable of producing this type of effect; given, though, that these comparisons are more often than not of the “upward type”, they tend to result in exaggerated negative affect for the individual – and thus their generally negative aspect.

The positive aspect of counterfactual thinking arises from causal inferences, the second assumed mechanism for the generation of counterfactual consequences (Wells & Gavanski, 1989). Because they can be construed as conditional

propositions inherently related to an actual event (i.e. the event that is counterfactually construed), counterfactuals lead to causal inferences – therefore conveying information about causality. According to Roese’s analysis, this information is useful for the individual, since it allows for the future self-regulation of behaviour on the basis of the judgement of causality: If, for example, through the counterfactual construction of the event a particular omission is identified as the causal antecedent of a negative outcome, then it can be expected that in the future the recipient of the negative outcome will perform the initially omitted behaviour so as not to receive the same outcome again (see also Zeelenberg, 1999b for a similar argument and Mandel, 2003 for an empirical approach to this issue).

However, the functional aspect of counterfactual thinking has been strongly challenged by others. For instance, Howard (1992) strongly disagrees, arguing that preferences should only be based on a set of possible futures and not foregone and counterfactually construed ones. Moreover, in his attempt to qualify the verdict on the appropriateness or non-appropriateness of regret considerations, Burks (1946) long ago pointed out that the necessary prerequisite for regret to be reasonable is “[...] not merely [the assumption] that there are laws of nature, but that these laws are *more* than mere summaries of matters of fact (past, present, or future)” (Burks, 1946; p. 170, italics in the original). The property of being “more than mere summaries” implies that they should hold true irrespective of the actor’s actions (or inactions), so that the latter can meaningfully regret his promptness (or failure) to initiate a causal chain – since the presence or absence of this behaviour changes the state of the world he ends up in.

All the above said, and taking a broader perspective on the functionality / rationality issue, the controversy as to whether emotions in general (i.e. not only anticipated emotions) impede or enhance rational decision-making seems to remain

unresolved. A reason for this seems to us to be the fact that researchers often fail to distinguish between the different types of emotions they are considering. For example, Mellers (2000) compares Loomes and Sugden's (1982) with Damasio's (1994) views. This comparison, however, fails to take into account a fundamental difference, namely the fact that Loomes and Sugden focus on anticipated affect (i.e. a cognition-based emotion), whereas Damasio focuses on anticipatory affect or "gut feeling". Their overall convergence could be helpful in making the (rather over-generalising) point that "affect is beneficial for rational decision behaviour", but it would fail to address any aspects of that point specifically linked to either anticipated or anticipatory affect.

A final consideration within the functional and normative context that has only recently been brought into attention is the accuracy of the decision makers' affective anticipations. Two rather striking features characterise this issue. First, it has largely been ignored by researchers working within the field of anticipated emotions. Second, when it is mentioned as a critical aspect of the decision-making process (e.g. Mellers, 2000), it is not directly linked to the rationality of the subsequent decision. In other words, the normative or non-normative character of the decision is presented as independent of the prediction accuracy.

This neglect of the accuracy issue in the anticipated emotions literature results in the scarcity of published research that directly addresses this question. The aim of the second part of this review is to summarise the existing knowledge on affective self-forecasting.

CHAPTER

2

Affective forecasts:

How accurate are they?

1. Introduction: Types of self-forecasting

To the best of my knowledge, there has so far been no all-inclusive review of the area of self-forecasting. This is understandable, because, on the one hand the literature on self-forecasting seems vast, and on the other hand the diversity of the research that falls under the “self-forecasting” category makes any superimposed categorisation inadequate. For the purposes of this review, however, a working categorisation of the current research questions and results needs to be established. I propose that, overall, the area should be viewed as incorporating two major sub-categories: The cognitive self-forecasts and the affective self-forecasts.

Looking at cognitive self-forecasts, performance seems to be the dominant consideration. Therefore, I conceptualise this sub-category as inclusive of all instances of individual performance predictions, both within and outside the laboratory (e.g. task completion times; questions correctly answered out of an almanac; etc.). The accuracy of these forecasts has been discussed in terms of the overconfidence phenomenon. Simply put, when people are asked to make confidence judgements for their estimated performance at an upcoming task, their confidence judgements are higher than their actual performance scores. In other words, the overconfidence phenomenon implies that people overpredict their future performance. The empirical robustness of overconfidence has been established across a variety of tasks, in both naturalistic and laboratory settings (for reviews of the vast literature, see Harvey, 1994; Lichtenstein, Fischhoff, & Phillips, 1982; McClelland & Bolger, 1994; for a different view, see Gigerenzer, 1994).

Looking at affective self-forecasts, I have been able to distinguish three types: Moods and emotions; tastes and their changes; and visceral factors. In this context I refer to “visceral factors” as inclusive of hunger, pain, and fear forecasts. The anticipated emotional reactions I am primarily interested in fall into the “moods

and emotions” category. In what follows, I review the evidence on the accuracy of the three types of affective forecasts. For the relevant literature on cognitive forecasts see the reviews cited above.

A final methodological note is needed here. As will become apparent, a major difference between the research on affective influences on individual decision-making and the research on the accuracy of affective forecasts is that whereas the former has been theory-driven, the latter has largely been empirically-driven. In other words, JDM researchers started from the assumptions of RT; DT; and DAT – SEP and they collected empirical evidence that either supported or did not support these assumptions. Therefore, the emotion-related empirical research in the area of JDM is essentially a process of empirical validation of RT; DT; and DAT – SEP.

In contrast, researchers who looked at the accuracy affective forecasts started with practical concerns. For instance, health and clinical psychology researchers might want to know if normal participants predict less fear when they see a snake or a spider compared to phobic participants. Snake phobias or spider phobias are then modelled as overpredictions of the fear that pathological people associate with the phobic object. Moreover, it follows that a behavioural treatment that would reduce the overprediction of fear would be effective for the treatment of the phobia (for an overview, see Rachman & Bichard, 1988). Likewise, health psychology researchers might want to know if participants who avoid a medical treatment overpredict the pain that is related to their treatment compared to participants who do not avoid taking the treatment. Treatment avoidance is then modelled as an overprediction of pain and behavioural treatments focusing on the reduction of this overprediction are introduced (for an overview, see Rachman & Arntz, 1991).

Therefore, this stream of research has had applied, rather than basic, motivations. In the light of these motivations, I review the evidence on the accuracy of affective forecasts starting from the existing empirical evidence; I then move on to review some of the existing accounts of the inaccuracies that have been documented regarding these forecasts; finally, I focus on one of the suggested accounts and elaborate on a possible link between the JDM emotion-related research and the affective forecasts research.

2. Accuracy of affective self-forecasts

2.1. Visceral factors

According to George Loewenstein, the defining features of visceral factors are “[...] a direct hedonic impact and [...] an influence on the relative desirability of different goods and actions” (Loewenstein, 1996; p. 273). Though in the same broad domain, visceral factors differ from tastes in that a.) they have an independent hedonic impact; b.) they are correlated with well-defined external circumstances (e.g. fearful stimuli); c.) they change more rapidly; and d.) they are based on different neurophysiological mechanisms (Loewenstein, 1996; p. 273). Hunger, thirst, sexual desire, craving for a drug, and emotions like fear and physical pain are all examples of visceral factors. Unlike the other emotions, visceral factors exert their influence without any conscious cognitive mediation.

Regarding people’s perceptions of and reactions to visceral factors, Loewenstein (1996) points out first, that people fail to appreciate future visceral factors that will affect their behaviour and, second, that people also fail to predict accurately their own future behaviours under the influence of visceral factors. For instance, when people put the alarm clock out of immediate reach before they go to sleep, they fail to appreciate that the sleepiness of the next morning will motivate

them to stay in bed longer. By putting the alarm farther away they are essentially arranging a self-torment. However, if people fail to put the alarm far from the bed they may be underestimating the size of the influence of the sleepiness – in other words, they might never get out of bed on time. Essentially, then, these behaviours work in opposite directions: The first underestimates the pain associated with waking up the next morning; the second underestimates the influence of this pain on the next morning's effort to get out of bed.

The literature on self-control (e.g. Ainslie & Haslam, 1992; Elster, 1999; Elster & Skog, 1999; Schelling, 1984; 1992; Thaler & Shefrin, 1981) provides some philosophical insights regarding the emergence of these visceral influences and people's behaviours when they are faced with them. The mere idea of attempting to impose control on future behaviours seems to constitute an anomaly, since it implies that people are consciously willing to bind themselves against their own future preferences¹. The whole issue is captured in a very lively way in the following excerpt from Schelling (1984; p. 8):

“When we ask the mother who an hour ago was frantic with pain whether she is glad the anesthesia was denied her, I expect her to answer yes. But I don't see what that proves. If we ask her while she is in pain, we'll get another answer”.

The point here is again the erroneous forecasts people generate about either the feelings associated with the activation of a visceral factor, or the degree of influence the latter can exert on their future behaviours.

The visceral factors that are more closely related to my aims in this review are the aforementioned hunger, physical pain, and fear.

¹ In the self-control literature these predictions are commonly referred to as predictions of future preferences; since I have clearly stated that I endorse Loewenstein's (1996) differentiation between tastes and visceral factors, and for the sake of simplicity, in this paragraph I use the two terms interchangeably.

2.1.1. Hunger forecasts

The basic research question that has been attempted regarding the visceral state of hunger is whether its presence or absence has any effect on subsequent food-related choice behaviour. The typical study within this area comprises two groups of participants, one hungry and the other satiated (or at least one group “hungrier” than the other). The dependent measure is either actual or intended food consumption behaviour.

Nisbett and Kanouse (1968 – cited by Loewenstein, O’Donoghue, & Rabin, 2003) investigated the impact of hunger on shopping behaviour in a naturalistic setting – namely a supermarket. What they found was that the shoppers’ hunger (operationally defined as the temporal distance between the shoppers’ last meal and their visit to the supermarket) and the quantity of unwanted goods they purchased during their shopping trip were positively correlated.

This finding was replicated by Gilbert, Gil, and Wilson (in press). In their also naturalistic study they manipulated hunger by offering half their participants a muffin before they went into a grocery store. Among other findings, Gilbert et al. (in press) found that participants who had eaten the muffin eventually purchased a smaller number of unwanted food items than participants who had not eaten anything (34% versus 51%, for less and more hungry participants, respectively).

Similar findings were obtained by Read and van Leeuwen (1998), who showed that participants who are hungry at the point of choice (i.e. late in the afternoon) are more likely to choose unhealthy snacks to consume in a week’s time than participants who are satiated (i.e. immediately after lunch). Satiated participants favour y snacks instead.

Overall, research on hunger has empirically documented an influence of hunger on food-related choice behaviour.

2.1.2. Pain forecasts

The basic research question that has been attempted regarding the visceral state of pain is whether pain is accurately predicted or not. Pain has been induced and measured in laboratory situations or it has been measured in naturalistic settings with patients. In the typical study within this area, participants go through multiple consecutive pain trials. Before starting a trial, they are asked to forecast the intensity of pain they are about to experience. These forecasts are made on “Visual Analogue Scales” (VAS), which are horizontal graphs divided into distinct intervals and anchored at 0 and 100. The time-spans used are minutes (i.e. the time intervals between two consecutive trials).

Two criteria of accuracy have been used. According to the first criterion, a pain forecast is judged to be accurate if it falls within 2% or 5% (variable across studies) of the actual pain experience; if the forecast exceeds this limit it is declared inaccurate. According to the second criterion, experienced pain is compared to predicted pain. If the difference between the prediction and the experience is statistically significant, then the forecast is declared inaccurate; if the difference between the prediction and the experience is not significant, then the forecast is declared accurate. In the overview of the relevant literature that follows, I report empirical evidence regarding both criteria, wherever this evidence is available.

Looking at naturally occurring pain, in their review of the relevant literature Rachman and Arntz (1991) reported results from 29 chronic arthritis patients (Rachman & Lopatka, 1988); 30 headache patients (Rachman & Eyrl, 1989); and 40 menstruating women (Rachman & Eyrl, 1989). Arthritis patients made accurate forecasts in their first predictions ($D_{\text{predicted-experienced}} = 2.26$, n.s.), whereas using the 2%–5% criterion they were accurate in 56.86% of cases, overpredicting in 25.49%, and underpredicting in 17.65% of them. Moreover, a learning effect was found

across trials: The change in the forecasts was predicted by the degree of discrepancy in the preceding trial.

The headache study was done on students who were experiencing severe headaches. First predictions of pain were again accurate ($D_{\text{predicted-experienced}} = -1.14$, n.s.). Likewise, first predictions of relief due to medication were also accurate ($D_{\text{predicted-experienced}} = 2.83$, n.s.). Participants were accurate in 24% of the cases, overpredicting in 43%, and underpredicting in 33% of them. Higher pain predictions were associated with more intense pain experiences – therefore the predictive judgements appeared to have some predictive validity.

The menstruation study was done on female students who were experiencing severe menstruation symptoms. First pain predictions were accurate ($D_{\text{predicted-experienced}} = 3.78$, n.s.). The data on the first predictions of relief due to medication were not available. Participants were accurate in 18% of cases, overpredicting in 45%, and underpredicting in 37% of them. No learning effect was found in this study. These findings led Rachman and Eyrl (1989) to conclude that chronic pain (arthritis) is generally more accurately predicted than recurrent pain (headaches; menstrual pains).

Looking at experimentally induced pain, Arntz and van den Hout (1988) used minor electric shocks as the painful stimuli. One of the two groups they used were accurate in their first pain predictions ($D_{\text{predicted-experienced}} = 2.00$, n.s.), whereas the other group underpredicted pain in their first forecasts ($D_{\text{predicted-experienced}} = -14.00$, $p < .05$). There were no differences between the groups regarding the percentages of accurate and inaccurate forecasts: Participants were accurate in 26% of cases, overpredicting in 40%, and underpredicting in 34% of them – thereby exhibiting an unclear pattern, with both under- and overpredictions. A learning effect was also found: The forecasts became more accurate over trials.

Arntz and Lousberg (1990) also used electric shocks as the painful stimuli. Because of the experimental manipulations in this experiment, only the data of the control group are of interest here. Unfortunately, the predicted and experienced pain scores are only graphically depicted in the paper (see Arntz & Lousberg, 1990; pp. 21-22, graphs "a"). Visual inspection of the graphs reveals that participants began by underpredicting, continued with more or less accurate predictions, but, by the middle of the experiment, they turned to overprediction. Therefore, I (somewhat arbitrarily) conclude that overprediction was the prevalent finding from these data. A learning effect was also found. Overall, then, Arntz and Lousberg's (1990) findings are in line with Arntz and van den Hout's (1988) findings on electric shocks.

Rachman and Arntz's (1991) overall conclusion from their review is that people tend to overpredict pain. This conclusion is apparently based on the stringent proportional criterion of accuracy. However, the initial forecasts were accurate in five out of six cases (the only exception being the Arntz & van den Hout, 1988 control participants).

More recent research has looked at dental pain (Arntz, van Eck, & Heijmans, 1990), chronic low back pain (Arntz & Peters, 1995; McCracken, Gross, Sorg, & Edmands, 1993), and post-operative pain (Thomas, Robinson, Champion, McKell, & Pell, 1998). In the dental pain study (Arntz et al., 1990) participants were 40 people visiting their dentists. Participants were accurate in 21.25% of cases, overpredicting in 62.50%, and underpredicting in 16.25% of them. In this study an individual differences variable (namely, dental anxiety) was introduced for the first time. When participants were split in two groups depending on the amount of anxiety they were experiencing about their dental treatments, "high-anxiety" participants overpredicted their pain much more than "low-anxiety" participants

("high anxiety": $D_{\text{predicted-experienced}} = 22.20$; "low anxiety": $D_{\text{predicted-experienced}} = 3.90$ – averaged across two reported visits; significance levels not available, see Arntz et al., 1990; p. 36). A learning effect was also observed.

The anxiety variable was introduced again in the chronic low back pain study by McCracken et al. (1993). The 43 chronic low back pain patients were accurate in 42.00% of cases, overpredicting in 19.47%, and underpredicting in 38.42% of them. In this study, however, when participants were split in two anxiety groups "high anxiety" participants were accurate in 54% of cases, overpredicting in 20%, and underpredicting in 26% of them, whereas "low anxiety" patients accurate in 40% of cases, overpredicting in 16%, and underpredicting in 44% of them. In other words, the pattern of findings was the opposite of the Arntz et al. (1990) study. A learning effect was once more observed.

The discrepancy between McCracken et al.'s (1993) finding that chronic patients err in the direction of underprediction and Rachman and Lopatka's (1988) finding that chronic patients are more or less accurate, but, when they err, they tend to overpredict, led Arntz and Peters (1995) to conduct a further study. In this study they compared 20 chronic low back pain patients with controls in order to investigate any latent pattern in the chronic patients' responses. Their findings for the first trial show that whereas both groups tended to underpredict, the patients did so to a greater extent (patients: $D_{\text{predicted-experienced}} = -11.80$; controls: $D_{\text{predicted-experienced}} = -5.50$, significance levels not available). Moreover, patients' forecasts were consistently underpredictions, whereas controls' forecasts were accurate.

Finally, in a study of post-operative pain predictions, Thomas et al. (1998) asked 91 patients to predict how much pain they thought they would experience over a period of six days. The time-span between the forecasts and the experiences is not reported. The patients overpredicted their pain for the first three days, but they

were accurate in their pain predictions for days four to six. Moreover, female patients both underpredicted more and experienced more post-operative pain than male patients. Finally, predicted pain was a valid predictor of experienced pain when the former was high, but not when the former was low. This finding is in line with the fact that predictions were generally higher than experiences.

2.1.3. Fear and anxiety forecasts

The basic research question that has been addressed regarding the visceral states of fear and anxiety is whether fear and anxiety are accurately predicted or not. Both emotions have been induced and measured in laboratory situations and they have also been measured in naturalistic settings with patients. The typical study within the area is identical to the typical study within the area of pain research. The same comments apply regarding the criteria of accuracy. For qualitative reviews of the empirical findings, see Rachman and Bichard (1988), Rachman (1994) and Marks and DeSilva (1994).

Rachman and Levitt (1985) asked 13 student participants who had been screened for claustrophobia to predict their fear when they would enter a dark room. Unfortunately, the predicted and experienced fear scores are only graphically depicted in the paper (see Rachman & Levitt, 1985; p. 591, fig. 2). Visual inspection of the graphs reveals that students were inaccurate in their fear predictions, exhibiting an inconsistent pattern of over- and underpredictions. Valentiner, Telch, and Bolte (unpublished data – cited in Telch, Valentiner, & Bolte, 1994), however, documented overpredictions in fear forecasts for claustrophobics.

Rachman and Lopatka (1986a; 1986b) asked student participants who had been screened for snake phobia to predict their fear when they saw a snake. Overprediction was documented in both the first trial ($D_{\text{predicted-experienced}} = 16.13, p <$

.001; study 1) and at the aggregate level. The forecasts were accurate in 32.15% of cases, with overpredicting in 44.90% and underpredicting in 22.96% of them (averaged results from both studies). A learning effect was also observed, as participants became more accurate over trials.

Taylor and Rachman (1994a; 1994b) studied snake-phobics and spider-phobics. In the first study, 224 students who had been screened for snake phobia overpredicted their fear in two successive instances (1st trial: $D_{\text{predicted-experienced}} = 9.60, p < .001$; 2nd trial: $D_{\text{predicted-experienced}} = 26.80, p < .001$). In the second study, 100 students who had been screened for spider phobia predicted their fear accurately, but only when they were primed with a relevant cue (i.e. written account of an encounter with a spider) before the prediction ($D_{\text{predicted-experienced}} = -2.10, \text{n.s.}$). When the cue was irrelevant (i.e. a written account of an irrelevant experience) participants overpredicted their fear ($D_{\text{predicted-experienced}} = 7.30, p < .05$).

Rachman and Bichard (1988) reviewed two studies that were done on non-student samples. Rachman (1983) found that 21 parachuting trainees overpredicted their fear ($D_{\text{predicted-experienced}} = 8.09, p < .05$). McMillan and Rachman (1988) replicated these findings with a larger sample ($N = 105$; $D_{\text{predicted-experienced}} = 11.10, p < .01$). Both these results were based on one single parachuting instance.

The only study so far that has not replicated the finding that fear and anxiety are overpredicted is a study by Telch, Ilai, Valentiner, and Craske (1994). Telch et al. (1994) found that 37 student participants who had been screened for claustrophobia underestimated their fear. Telch et al. (1994) did a second study on a separate sample of claustrophobics, this time manipulating the proximity of perceived safety resources available to them (i.e. the distance from the door that would lead them out of the testing room). Telch et al. (1994) found that forecasts were accurate when perceived safety resources were proximal ($D_{\text{predicted-experienced}} =$

1.20, n.s.), whereas they were underpredictions when perceived safety resources were distant ($D_{\text{predicted-experienced}} = -4.10, p < .05$). Telch et al. (1994) concluded that underprediction of fear is more likely when perceived safety cues are absent or distant.

A similar line of research has investigated the accuracy of anxiety forecasts. Arntz et al. (1990), in the dental pain study that I described above, found that at the aggregate level anxiety was predicted more accurately than pain. Before the first visit to the dentist patients were accurate in 25% of cases, overpredicting in 60%, and underpredicting in 15% of them. Before the second visit, the accuracy percentages changed to 50% accuracy, with 35% overprediction and 15% underprediction. Focusing exclusively on clinical anxiety, Arntz, Hildebrand, and van den Hout (1994) showed that psychiatric patients (two studies; $N = 48$) tend to overpredict their anxiety (as might be expected). They also found a learning effect, as forecasts tended to become more accurate over trials.

2.2. Tastes and their changes

March (1978) argued that one of the prerequisites for rational choice is a “good guess” about uncertain future preferences. The uncertainty that surrounds future preferences arises from the fact that although the traditional microeconomic view of tastes postulates that they should be absolute, relevant, consistent, precise, and exogenously determined, tastes have been found to be relative, irrelevant, unstable, inconsistent, imprecise, and showing evidence of internalities.

The JDM literature has been more or less silent regarding the question of the accuracy of preference predictions. Varey and Kahneman’s observation almost ten years ago that “[...] little is known about the question of how accurately people predict their future tastes [...]” (Varey & Kahneman, 1992; p. 189) is indicative of

this state of neglect. The situation has not changed dramatically in the last ten years. In fact, Varey and Kahneman's (1992) paper is one of the few pieces of published research available that directly addresses this issue.

In Varey and Kahneman's pilot study, participants had to predict how much they would like a serving of ice-cream and a particular piece of music after experiencing them each day over the time periods of one day and one week. The general finding from this pilot study was that participants overpredicted how much their liking for these two consumption goods would decrease. In Varey and Kahneman's main study, participants had to predict how much they would like a serving of yoghurt and a particular piece of music after experiencing them each day over one and eight days. This task was more naturalistic, since participants filled in the relevant questionnaires at home and returned them after the last day of the study. The results of the main study replicated the results from the pilot: Participants were again inaccurate in their predictions. Not only did participants overpredict the decrease in their liking for that particular yoghurt and piece of music. In addition, some of them failed to accurately predict even the direction of the change. In other words, for some 29% of the participants liking for that particular yoghurt and piece of music increased, rather than decreased – but only 3% of them had predicted an increase.

A related phenomenon from the area of behavioural economics is the so-called "endowment effect". According to Thaler's (2000) conceptualisation of the phenomenon, people often demand more money to give up an object they possess (i.e. their "endowment") than they would be willing to pay to acquire it in the first place. Kahneman, Knetsch, and Thaler (1990; 1991), Loewenstein and Adler (1995), van Boven, Dunning, and Loewenstein (2000) and van Boven, Loewenstein, and Dunning (2003) provided empirical documentation for the phenomenon using

time-spans of only several minutes. Kahneman et al. (1991) showed that the effect is due not to an increased attractiveness of the possessed object, but to the enhanced pain that is linked to parting with it. Loewenstein and Adler (1995) and van Boven et al. (2000; 2003) further showed that participants do not predict this pain accurately. For instance, the selling prices participants offered when they were in possession of the object (e.g. a mug engraved with their school logo) were on average \$1.12 greater than their buying prices² (Loewenstein & Adler, 1995). Moreover, in another experiment, 50% of participants stated that they would like to revise their selling prices upwards after having acquired the object. Learning does not seem to be enough to counteract these inaccuracies (Kahneman et al., 1990; van Boven et al., 2003).

2.3. Future emotions and moods

The number of studies directly investigating the accuracy of affective forecasts is rather small. The first series of relevant studies is the one reported by Mitchell, Thompson, Peterson, and Cronk (1997). The task for their participants was to give pleasure ratings both before and during a trip to Europe (study 1); a Thanksgiving vacation (study 2); and a 3-week bicycle trip to California (study 3). The time span between predictions and experiences was one month for the first; one week for the second; and three weeks and two days for the third study. The results obtained by Mitchell et al. allowed them to coin the term “rosy prospection” for their participants’ forecasts: In all the three studies, and across all different time-spans, participants overestimated the degree of pleasure they would later be experiencing.

² I have averaged three within-participants elicited price differences, namely \$1.67 for the 1st experiment – Carnegie Mellon sample, \$1.17 for the 1st experiment – Pittsburgh sample and \$0.53 for the 2nd experiment; all these differences were significant.

A similar series of six studies was run by Gilbert, Pinel, Wilson, Blumberg, and Wheatley (1998). For five out of their six studies Gilbert and his colleagues used a different type of assessment of accuracy: Instead of comparing data within-participants (i.e. in a typical before-after way), they divided their participants into “forecasters” and “experiencers” and they assessed the discrepancies between the two groups. The greater these discrepancies, the larger the inaccuracy. The type of task they used was mixed: Half of the studies used naturalistic tasks (dating relationships in study 1; academic tenure in study 2; and election results in study 3), whereas the other half used typical laboratory tasks (negative personality feedback in study 4; story about baby’s death in study 5; and unsuccessful job application in study 6). In the first three studies they also contrasted the positive versus negative content of the affect forecasted. Finally, the time-span they used in their only within-participants measure of affect (study 6) was 10 minutes.

Gilbert et al. (1998) found that participants were accurate in their affective predictions about dating relationships, long-term tenure professors, and election winners, but inaccurate in their affective predictions for short-term tenure professors and election losers. In other words, the observed inaccuracies were associated with forecasts of negative affect only.

Therefore, in studies four to six Gilbert et al. focused on negative affect only and they found more empirical support for the inaccuracy argument. Participants were unable to predict that they would feel better after having received negative personality feedback from a fallible (compared to an infallible) source; after having read a blameworthy (compared to a blameless) story; and after having been rejected in an unfair (compared to a fair) manner. The final result reported from this series was the within-participants comparison of the sixth study, where again participants overpredicted the negative affect they would experience.

Wilson, Wheatley, Meyers, Gilbert, and Axsom (2000; study 3) provided more evidence of affective misprediction. In this within-participants study a time-span of two months was used and the event in question was an actual football game. Participants had to predict their affective reactions to the result of the game. Wilson et al. (2000) found that participants overpredicted both their positive affect (i.e. in case their team won) and negative affect (i.e. in case their team lost). Wilson et al. (2000) eventually coined the term “durability bias” to capture these affective overpredictions. Thus, Mitchell et al.’s (1997) findings were replicated for both negative affect and for a significantly shorter time-span between prediction and experience.

Similarly to Gilbert et al. (1998), a combination of before-after and “forecasters” versus “experiencers” design was also employed by Sieff, Dawes, and Loewenstein (1999) for the assessment of accuracy in predicting emotional reactions to HIV test results. In this naturalistic study, one group of participants predicted their emotional reactions to the test results and, four weeks after they found out their actual results, they were asked to report their emotional state (before-after and, at the same time, “forecasters” group). In addition to this group, another group of participants did not make any predictions, but only reported their emotional state four weeks after they had found out their actual results (“experiencers” group).

The within-participants analysis of the first group showed that they were fairly accurate in their predictions: Although predicted affect was higher than experienced affect, this discrepancy did not reach significance. In addition, predicted and experienced affective states were correlated (Pearson $r = .62$), thereby demonstrating a reasonable degree of judgement validity. The between-groups analysis, however, revealed a different pattern: Compared to the “experiencers”, the

“forecasters” significantly overpredicted both the increase in distress after a positive test result and the decrease in distress after a negative test result.

Tackling a slightly different research question, Totterdell, Parkinson, Briner, and Reynolds (1997) investigated people’s accuracy in predicting their moods. Totterdell et al. (1997) hypothesised that participants would not be particularly insightful in their forecasts of their own moods. This hypothesis was based on previous empirical findings by Wilson, Laser, and Stone (1982). Wilson et al. (1982) found that participants (“targets”) who were recording their everyday moods were not any more accurate than another group of participants (“observers”) who were also recording the “targets” moods. Totterdell et al.’s participants had to report their moods from home or work for 14 consecutive days. The time-spans between predictions and the experiences were half a day (more or less, since participants had to forecast their moods in the beginning of the day and rate their experienced moods by the end of it) and one week.

The results of the study partially confirmed Totterdell et al.’s hypothesis. On the one hand, participants were accurate in their predictions, with the errors being inconsistently both under- and overpredictions. However, when the experienced mood ratings were regressed on the predicted ratings, the explained variance of the model did not exceed 9%. Inclusion of other predictors (e.g. concurrent mood; hassles that occurred the same day or the day before; etc.) significantly improved the accuracy of the model. This pattern of findings suggests that, although accurate, mood forecasts did not contribute substantially to the prediction of subsequently experienced moods. In contrast, concurrent mood (i.e. mood fluctuations around the point of assessment) was the weightiest predictor of experienced moods. Finally, no difference was found across time-spans.

Mellers (2000) summarised the results of three naturalistic studies. Participants had to predict how they would be feeling after receiving their grades (four-month time-span); after receiving feedback on their weight-loss progress (one-week time-span); and after receiving the results of pregnancy tests (ten-minute time-span). Apart from the different time-spans, the other major difference between the three studies was the degree of familiarity with the outcome (high for the grades and weight loss studies; low for the pregnancy test study).

Mellers' findings were mixed. Whereas students were accurate in their predictions, pregnant women and dieters were not. In addition, across the three studies participants who were inaccurate tended to overpredict the pain associated with possible negative outcomes, but not the pleasure associated with possible good outcomes. In Mellers' words "Actual pleasure is greater than anticipated, especially for undesirable outcomes" (Mellers, 2000; p. 919).

Mellers et al. (1999) were the first to investigate the accuracy of post-decisional emotional anticipations. Mellers et al. used a gambling task and a one-week time-span and they found that participants were accurate in their predictions of post-decisional regret and disappointment (Pearson r s between predicted and experienced emotions varied between .96 and .98). However, participants underestimated the affective impact of "surprise effects" – in other words, their predictions of emotional reactions to outcomes that were judged unlikely to occur were lower than their corresponding experiences. There also were some outlying participants, who appeared unable to predict even the valence of their reactions.

Coughlan and Connolly (2001) replicated the finding that surprise effects are underestimated. In the second study, they asked bowlers to estimate their game satisfaction as a function of unexpected outcomes. Participants underpredicted both their satisfaction with surprisingly good outcomes ($D_{\text{predicted-experienced}} = .69, p < .001$)

and their dissatisfaction with surprisingly bad outcomes ($D_{\text{predicted-experienced}} = -.55, p < .02$). Coughlan and Connolly commented on these findings: “Surprises, in short, tend to have more of an amplifying effect on satisfaction ratings than subjects predicted they would” (Coughlan & Connolly, 2001; p. 11).

3. Theoretical accounts for the mispredictions

3.1. Short- and long-term projection biases

In an effort to provide a theoretical account of the pattern of findings obtained by Sieff et al. (1999), Loewenstein and his colleagues summarised empirical evidence that demonstrates that people consistently and across a variety of domains underestimate adaptation³ (Loewenstein et al., 2003; see also van Boven & Loewenstein, 2003) – in other words, people consistently underestimate their abilities to adapt to new circumstances. As a result of this underestimation, people exaggerate the impact of positive and negative future events on their overall well-being. The underprediction of adaptation and the accompanying overestimation of the impact of future events on one’s well-being are instances of what Loewenstein and his colleagues call “long-term projection bias”.

Loewenstein et al. (2003) also referred to the phenomenon of overprediction of negative affect that Gilbert and his colleagues documented as an instance of the “long-term projection bias”. Gilbert et al. (1998) found that people overpredict the negative affect that they will experience as a result of negative events that will happen to them in the future and they attributed these overpredictions to “immune neglect”. “Immune neglect” refers to people’s failure to appreciate the effectiveness of their “psychological immune system”, which is activated after negative

³ Which, interestingly, they refer to as “a common type of taste change” (Loewenstein et al., 2003; p. 1212). See my discussion of the “short term projection bias” that follows.

experiences in order to minimise the degree of negative affect. Because of this failure, people overestimate their future displeasures.

Additional empirical evidence for the validity of the “long-term projection bias” as an explanatory mechanism comes from a famous series of studies by Brickman, Coates, and Janoff-Bulman (1978). Brickman et al. (1978) used the “targets vs. observers” paradigm (commonly used in social judgement research) and they found that neither major positive events (e.g. winning a lottery), nor major negative events (e.g. paraplegia) changed the targets’ everyday lives for the better or for the worse to the extent that the observers imagined they would. In the line of the projection bias hypothesis, Brickman et al. (1978) argued that people perceive their financial states or states of health as changes relative to their present status quo – thereby overpredicting the impact of more money or less health in their lives. This explanation of the real-life empirical findings also falls well in line with Prospect Theory’s postulate that people judge levels of wealth and other utilities as transitions from some reference point and not in absolute terms (Kahneman & Tversky, 1979).

Alongside the “long-term projection bias”, Loewenstein and his colleagues also introduced a “short-term projection bias”⁴. Not only do people overestimate the impact of future events on their well-being; they also overestimate the degree to which their future preferences, tastes, and utilities in general will resemble their present ones. This overestimation results in erroneous predictions of what people will want or enjoy in the future. The difference between the “long-” and the “short-term projection bias” is the time frame of reference, which is much longer for the former bias than for the latter.

The “short-term projection bias” accommodates the empirical findings on the influence of hunger on behaviours. When people make their decisions under the

⁴ The distinction between the two appears to be empirical, rather than conceptual.

influence of a current state of hunger, they project this state into the future, thereby overpredicting how much they will want to eat in the future, and they behave accordingly (for instance, they buy things from the grocery store that they did not intend to buy; they choose unhealthy snacks rather than healthy snacks; etc.).

In addition, the “short-term projection bias” accommodates the findings of the research on the “endowment effect”. People fail to anticipate their attachment to the object they will be endowed with, because they project their current state of non-attachment to the object into the near future. Likewise, people also fail to see that if they did not have the object they would be much less attached to it – thereby attaching a lower value to it. These mispredictions provide the frame for the “egocentric empathy gaps” between “buyers” and “sellers” in the situations where the “endowment effect” has been studied that have been described by van Boven et al. (2000).

3.2. Erroneous shared theories

In their effort to account for the mispredictions of their participants’ liking for ice-cream, yoghurt, and music Varey and Kahneman (1992) argued that in order to predict their future tastes people rely on shared (or “lay”) theories. For instance, a shared theory might postulate that “continuous consumption of a certain good will eventually lead to a decline in its likeability” – i.e. a satiation effect. On the basis of such a theory, people predict that they will come to hate a serving of ice-cream after having had one every day for a week – which, however, is not the case.

Mispredictions of everyday moods have also been attributed to erroneous shared theories. An explanation of the findings that “targets” are not more accurate judges of their own moods than “observers” (Wilson et al., 1982) is that both “targets” and “observers” rely on the same theories to predict everyday mood

fluctuations. These theories predict, for instance, that people are overall in higher spirits on Fridays and Saturdays – which, as was the case with the satiation prediction, is not true.

3.3 Evolutionary explanations: “Better safe than sorry”

Looking for a psychological explanation for the empirically documented overprediction of pain and fear, Rachman and Arntz have claimed that the overestimations of pain and fear should be viewed as instances of a more generic tendency to overpredict aversive events – in other words, overprediction might be an innate characteristic of the way people and other animals deal with aversive, or threatening stimuli (for pain: Rachman & Eyrl, 1989; Arntz, 1996; for fear: Arntz, 1995; Rachman, 1994; Rachman & Eyrl, 1989).

Moreover, Rachman and Arntz have also argued that these overestimations are functional. Their rationale is that, since underpredicted pains and fears are more aversive and motivate more avoidance behaviours than accurately predicted pains and fears (e.g. Arntz & Hopmans, 1998; Arntz, van den Hout, van den Berg, & Meijboom, 1991; Arntz, van Eck, & de Jong, 1991), people have a “hardwired” motivation to avoid unpleasant surprises. Therefore, they “choose” to err in the direction of “safety” – hence the overpredictions.

Regarding the learning effects that have been documented (i.e. the pain and fear forecasts becoming less inaccurate over consecutive trials), Rachman and Bichard (1988) have argued that people have an innate “drive towards accuracy”. This “drive towards accuracy” has been founded on Rachman and Arntz’s (1991) argument that predictability and controllability are highly desirable features of the environment for both humans and animals. Therefore, once people have learnt that

their environment (or laboratory situation) is more or less predictable, they are better calibrated in their forecasts.

3.4. Focalism

In Wilson et al.'s (2000) studies of affective predictions, participants consistently over-predicted the duration of their affective reactions to future events – a phenomenon that was called “durability bias”. In their discussion of the “durability bias” Wilson and his colleagues suggested that the psychological mechanism that underlies this phenomenon is what they termed “focalism”. In Wilson et al.'s words, when people generate predictions about how they will be feeling after a future event “...people focus too much on the event in question and not enough on the consequences of other future events” (Wilson et al., 2000; p. 821; see also Gilbert & Wilson, 2000). The pattern of findings that Wilson and his colleagues obtained in their experimental studies provided the empirical documentation of focalism.

Expanding the breadth of “focalism”, Wilson and his colleagues also suggested that “focalism” might be the underlying cause of the reversal from desirability to feasibility considerations as the time of a future decision or behaviour becomes more imminent (Gilovich et al., 1995; Liberman & Trope, 1998; Trope & Liberman, 2000). Moreover, Wilson et al. (2000) suggested that focalism might operate retrospectively, as well as prospectively. They reported some preliminary data that document the durability bias when people were asked to recall how they felt with respect to a past event that had occurred to them (Wilson, Meyers, & Gilbert, 1999; see also Wilson, Meyers, & Gilbert, in press) – in other words, they found evidence for “retrospective focalism” (i.e. as opposed to “prospective focalism”). Wilson and his colleagues suggested that this retrospective focalism might also operate when people recall their life regrets (cf. Gilovich & Medvec,

1995). They further suggested that a way of reducing these regrets might be to prompt people to de-focus from their decisions and see their lives in a broader perspective – thereby realising that “there were myriad forces making it difficult to act in the desired way” (Wilson et al., 2000, p. 835).

Wilson et al. (2000) actually tested the effectiveness of such a de-focusing manipulation. Before their participants predicted their affective reactions to future events, some of them were asked to fill in a diary of events that were likely to happen to them concurrently with the event in question – i.e. the “focal” event. Wilson et al. (2000) found that participants who had filled in the diaries before predicting their affective reactions (i.e. “de-focused” participants) were more accurate in their predictions than participants who had not filled in the diaries (i.e. control participants). Moreover, Wilson et al.’s (2000) data provided support for the notion that the diaries function as reminders of other future occurrences, rather than as affective competitors – in other words, it was not the affective valence of the diary events that changed participants’ judgements, but rather the reminder that these events would frame the focal event.

Looking at interpersonal, rather than intrapersonal judgements, Schkade and Kahneman (1998) found empirical evidence for a mechanism they termed “focusing illusion”. In their words:

“When a judgment about an entire object or category is made with attention focused on a subset of that category, a focusing illusion is likely to occur, whereby the attended subset is overweighted relative to the unattended subset” (Schkade & Kahneman, 1998; p. 340).

According to Schkade and Kahneman, the “focusing illusion” was the mechanism that led their Midwesterner participants to judge Californians as happier than themselves (whereas Californian participants did not actually judge themselves

as happier than Midwesterners). The attended category in this case was the weather – generally better in California than in the Midwest.

Schkade and Kahneman (1998) also suggested that focalism might be the mechanism that underlies people's misjudgements about sudden changes in their own, or others' life circumstances – i.e. people's underpredictions of adaptation (cf. Brickman et al., 1978; Loewenstein & Schkade, 1999). To summarise, focalism has so far been empirically documented in three different areas. Firstly, in concurrent judgements of general happiness about others (Schkade & Kahneman, 1998). Secondly, in predictive judgements of general happiness about one's self (Wilson et al., 2000). Thirdly, in retrospective judgements of general happiness about one's self (Wilson et al., 1999). Moreover, provision of a frame of reference for the focal event via a diary-filling technique has been found effective in attenuating the influence of focalism in predictive judgements of general happiness about one's self.

Therefore, it appears that “focalism” and the “projection biases” are closely linked. On the one hand, “intrapersonal focalism” in the domain of affective self-prediction is arguably an instance of the “short-term projection bias”. On the other hand, “interpersonal focalism” in the domain of self-other discrepancies in judgements of happiness is arguably an instance of the “long-term projection bias”.

CHAPTER

3

The present research:

Rationale, hypotheses, and overview of empirical work

1. The present research: Aims and general hypotheses

The present work draws heavily on two currents of research. The first one is the research on anticipated post-decisional affect. According to all existing models, when people make decisions they forecast what their post-decisional affective reactions to the decision outcomes will be. Subsequently, people factor these forecasts into their utility functions – alongside the decision outcomes. People's motivation regarding these post-decisional affective reactions then is to minimise their post-decisional pain (cf. regret and disappointment) and to maximise their post-decisional pleasure (cf. rejoicing and elation). This is an assumption shared by all the theories that have attempted to model people's anticipations of post-decisional emotions.

The second current of research is the research on the accuracy of affective self-forecasts and, more specifically, the research on "focalism". The existing empirical findings show that focalism is one of the mechanisms that cause affective overprediction. People overpredict their affective reactions to future events because they are focused on the events in question. Because of this focus, people fail to notice that other occurrences (i.e. occurrences other than the focal event) will also occupy their attention in the future, thereby attenuating the emotional impact of the focal event.

The aim of the present research was threefold. Firstly, I wanted to test the possibility that people overpredict what their affective reactions to the outcomes of their decisions will be. The first research hypothesis was the following: People's predictions of the affective reactions that they will experience when they receive the outcomes of their decisions are overpredictions. In other words, people overestimate how happy or how unhappy they will become as a function of receiving the outcomes of their

decisions. In order to test this, I compared people's affective forecasts with their affective experiences.

Secondly, I wanted to test the possibility that the overpredictions of decision-related affect are due to focalism. The second research hypothesis was the following: People's predictions of the affective reactions that they will experience when they receive the outcomes of their decisions are overpredictions only when people are focused on the outcomes in question. When people see the outcomes of their decisions in a broader context (i.e. when they are de-focused from the outcomes), their forecasts of decision-related affect are accurate. In other words, people's overestimations of how happy or how unhappy they will become as a function of receiving the outcomes of their decisions are due to focalism. In order to test this, I compared focused people's affective forecasts with de-focused people's ones.

Thirdly, I wanted to test the possibility that the influence of affective anticipations on decision intentions or behaviours is due to focalism. The third research hypothesis was the following: People try to minimise the pain and maximise the pleasure that they will experience when they receive the outcomes of their decisions only when they are focused on the outcomes in question. When people see the outcomes of their decisions in a broader context (i.e. when they are de-focused from the outcomes), their forecasts of decision-related affect are less influential on their decision intentions or behaviours. In order to test this, I looked at people's choice behaviours and compared the predictive validity of focused people's affective forecasts with that of de-focused people's forecasts.

2. Overview of the empirical work

Ten empirical studies are summarised in this Thesis. Studies 1, 6, and 7 were designed to address the question whether people predict their future emotions accurately or not. Studies 2, 3, and 4 were designed as indirect tests of the question whether affective misprediction can be attributed to focalism; the same question was directly addressed in studies 5, 6, and 7. Studies 8, 9, and 10 investigated the presence of focalism in affective post-dictions – i.e. affective experience after the recall of a past event. Finally, studies 2 to 6 were designed to address the question whether the influence of affective self-forecasts on individual decision behaviours can be attributed to focalism.

In the first empirical chapter (Chapter 4), I report a real risky-choice study (Study 1). Participants were offered a choice between a safer but less rewarding task and a riskier but more rewarding task. The feedback on participants' actual performance was manipulated in order to introduce surprise effects. Regarding participants' observed task choice behaviour, I tested the predictions of RT; DT; and DAT – SEP by contrasting the effects of affective forecasts to the effects of performance expectations. Regarding participants' self-reported decision-related affect, I compared participants' affective predictions and their corresponding affective experiences across the different levels of performance feedback.

In the second empirical chapter (Chapter 5), I report three scenario-based investment studies. Studies 2 and 3 introduced two positive investment options, with post-decisional regret potential associated with one of them. Participants had to indicate their intentions to invest to each one of the two options. Regarding participants' self-reported intentions to invest, I contrasted the predictions of Prospect Theory to the

predictions of Regret Theory for participants who were either focused on the investment decision or de-focused from it. Study 4 introduced two negative investment options, with post-decisional regret potential associated with one of them. Participants had to indicate their intentions to invest to each one of the two options. As was the case in Studies 2 and 3, regarding participants' self-reported intentions to invest, I contrasted the predictions of Prospect Theory to the predictions of Regret Theory for participants who were either focused on the investment decision or de-focused from it.

In the third empirical chapter (Chapter 6), I report two negotiation studies. Study 5 was a hypothetical two-party negotiation with regret potential associated with some of the outcomes. Regarding participants' self-reported negotiation behaviour (i.e. monetary offers), I contrasted the behaviours of focused and de-focused participants. Study 6 was a real two-party negotiation with regret potential associated with some of the outcomes. Regarding observed negotiation behaviour (i.e. monetary offers), I compared participants' behaviour when they were focused on the outcome to their corresponding behaviour when they were de-focused from it. Regarding participants' self-reported decision-related affect, I compared participants' affective predictions and affective experiences when they were focused on the outcome to their corresponding predictions and experiences when they were de-focused from it. Regarding the influence of affective forecasts on game behaviours, I compared the influence of affective predictions on participants' negotiation behaviour when they were focused on the outcome to the corresponding influence of these predictions on participants' negotiation behaviour when they were de-focused from the outcome.

In the fourth empirical chapter (Chapter 7), I report a naturalistic study of academic performance, where the feedback on participants' performance was not

manipulated (Study 7). Students predicted their affective reactions to receiving their coursework marks. When they received their marks they recorded their reactions. Regarding participants' self-reported decision-related affect, I compared affective predictions and affective experiences of focused and de-focused students.

In the fifth empirical chapter (Chapter 8), I report three naturalistic studies of decision recall using self-elicited emotional stimuli (Studies 8, 9, and 10). In Study 8, participants recalled a decision of theirs that resulted in positive or negative outcomes and then they recorded how they were feeling about it. In Studies 9 and 10, participants recalled a decision of theirs that resulted in negative outcomes and recorded how they were feeling about it. Regarding participants' self-reported decision-related affect, I compared affective experiences of focused, de-focused, and otherwise distracted participants.

CHAPTER

4

**Anticipated post-decisional emotional reactions in a risky choice
situation:**

**From performance expectations and affective forecasts
to performance feedback and affective experiences**

Post-decisional regret considerations in risky choice tasks

Over the last 10 years, empirical research has documented evidence for the presence of regret considerations in risky choice situations. For instance, Ritov (1996) used simple gamble tasks and found that participants were regret-averse in their preferences for different gambles. Boles and Messick (1995) obtained a similar pattern of regret aversion using gamble tasks and interpersonal comparisons as the source of regret. Josephs et al. (1992) also replicated these results using gambles, but they failed to observe regret aversion in the domain of losses. Zeelenberg et al. (1996), however, observed regret aversion in risky choices in the domain of losses using a gamble task and a different manipulation to introduce regret considerations¹.

The research question that motivates the stream of research summarised above is the interplay between risk aversion and regret aversion as strategies that people employ when they are faced with a risky choice with uncertain outcomes. Whereas the initial conclusion was that regret aversion leads to risk aversion, the research by Zeelenberg and his colleagues demonstrated that risk aversion and regret aversion are independent choice strategies. Furthermore, the research summarised above makes the assumption that when people are faced with a risky choice with regret potential they forecast how they will be feeling as soon as a.) they receive the outcomes of their choices and b.) they find out the outcomes of their foregone choice (s). These forecasts, in turn, are assumed to guide people's choice behaviours.

A common criticism that the research outlined above has attracted is that the context-free gamble paradigm that has been used is a poor substitute for real-life situations where people might face choices with regret potential. Put in a different

¹ I shall consider in greater detail Zeelenberg and Beattie's (1997) empirical evidence regarding the presence of regret aversion in the domain of losses in Chapter 6.

way, this research has been criticised as being of limited generalisability (e.g. Zeelenberg & Beattie, 1997; p. 65).

Study 1: Aims and hypotheses

To summarise, the research on risky choice summarised above has three limitations. The first limitation is its potential lack of generalisability to real-life situations due to the exclusive use of the gamble paradigm. The second limitation is that it has narrowly focused on post-decisional regret considerations and neglected more general emotional considerations that might arise in a risky choice situation. The third limitation is that it has assumed the presence of regret considerations without actually investigating their emergence or their nature. For instance, how accurate are regret anticipations?

Study 1 was designed to address these criticisms. Specifically, the first aim of Study 1 was to generalise prior findings by providing a richer context in which risky choice can be studied. I offered participants a choice between an easier and a more difficult version of an anagram-solving task. Participants' reward for doing the task was contingent on their task selection and on their task performance. This reward contingency enabled me to regard the easy task as corresponding to a safe choice and the difficult task as corresponding to a risky choice.

The second aim of Study 1 was to scrutinise the assumption that affective forecasts guide risky choices. Closely related to this, the third aim of Study 1 was to provide a thorough investigation of the accuracy of these affective forecasts. I explicitly asked participants to generate affective forecasts before they chose the task they would do. When participants finished the task, I asked them to record their affective experiences. In this way I was able to investigate a.) the extent to which affective forecasts guide choice behaviour and b.) the accuracy of these forecasts. In

line with previous research, I expected that a.) participants' choices would be influenced by their anticipations of post-task emotions and b.) participants would overpredict their post-task emotions (cf. Mellers, 2000; Sieff et al., 2000; Wilson et al., 2000).

Finally, I gave participants feedback regarding their performance at the task. Performance feedback was manipulated, so that some participants thought that they could have done better, others that they could have done worse, and others that they would have done equally well had they made a different task choice. These counterfactual comparisons are conceptualised by RT; DT; and DAT – SEP as primary sources of post-decisional affect – therefore they are an essential component of the study of this type of affect. In addition, the interaction between task choice and performance feedback also allowed me to introduce and examine surprise effects in participants' affective judgements. Regarding these effects, I expected that participants would underestimate the impact of surprises on their experienced emotions (cf. Coughlan & Connolly, 2001; Mellers et al., 1999).

Method

Participants: Seventy-one participants volunteered for the study (mean age = 24.73 years; $SD = 6.26$ years; forty-eight females and twenty-three males). Participants were paid £8 for their participation (in this and other studies), plus any money they made from the anagram-solving task (up to £7 or £9).

Variables and Design: The design was a three independent groups design, with Feedback (Equal vs. Better vs. Worse) as the independent variable. There were 23-25 participants in each condition.

Regarding the dependent variables of the experiment, participants chose a task, predicted their task performance, and stated their confidence in their performance prediction. Participants also predicted what their emotional reactions would be when they received feedback on their performance. Participants used the Positive Affect Negative Affect Schedule (PANAS; Watson, Clark, & Tellegen, 1988) to record these predictions. The Positive Affect (PA) scale of the PANAS consists of the following 10 items: Attentive, interested, alert, excited, enthusiastic, inspired, proud, determined, strong, and active. I also included rejoicing and elation (i.e. emotions that have been of theoretical interest in the JDM domain). The Negative Affect (NA) scale consists of the following 10 items: Distressed, upset, guilty, scared, hostile, irritable, ashamed, nervous, jittery, and afraid. I also included regret, and disappointment. In what follows, I shall refer to rejoicing, elation, regret, and disappointment as “decision-related” affect.

Once participants were finished with the task, I recorded their actual performance, their perceptions of how difficult the task was, and their perceived expertise at the task. Participants also recorded their experienced emotions immediately after receiving performance feedback and also a couple of days later.

Materials and Procedure: In the first stage of the study, all participants were presented with a short booklet containing manipulations and dependent measures. All participants received the following instructions: *“For this study you will be presented with a choice between two tasks. The two tasks are quite similar; they both require anagram solving. They differ, however with respect to their difficulty,*

as one of the two is easier than the other. Your aim for either task is to solve as many anagrams as possible (100 in total), in 15 minutes. Your extra payment for participating in the study depends on your task performance. In other words, the more anagrams you solve, the more money you make. The maximum amount of money you can make if you choose the easy task is £7 (£0.07 per anagram); the maximum amount of money you can make if you choose the difficult task is £9 (£0.09 per anagram) – that is, the difficult task is approximately 29% more rewarding than the easy task. In addition, you will be asked to describe how you think you will be feeling after having finished the task and having found out how well you actually did. Once finished with your chosen task you will receive feedback on how well you actually did. In addition, you will find out how well you would have done, had you chosen the other task (whichever this task is). We will estimate this on the basis of your scores on the individual differences scales you have filled in already – or you will fill in before going through the anagrams. Of course, because this score is an estimate, we do not expect it to reflect your performance with 100% accuracy; however, it should be a fairly valid predictor of your performance at the non-chosen task. All this sounds slightly confusing, but it will become much clearer as you go through the study! There will also be a follow-up session, a couple of days after the first one. This last session can be done over the phone”.

Either before or after reading these instructions, participants completed some unrelated personality scales. Repeating the instructions, the experimenter informed participants that, on the basis of some combination of their scores on the personality scales, he would be able to estimate how well they could have done at the task they would not eventually choose. In this way, participants were told they could compare their actual performance at the task they chose with their estimated performance at the task they did not choose.

Once participants had read and heard the instructions and before they started solving the anagrams, they made all their predictions. Regarding their performance, participants predicted how many anagrams out of the 100 they thought they would solve and they also stated their confidence in this prediction. The confidence item read as follows: *“What do you think is the probability (0% – 100%) that the number of anagram you actually solve falls within 5 anagrams either way of the number you just now predicted (e.g. if you predicted 50 anagrams, then: 45 ← 50 → 55)?”* Participants gave their answers on an 11-point percentage scale, anchored at 0% = it will definitely not fall within 5 anagrams either way and 100% = it will definitely fall within 5 anagrams either way, through the 50% midpoint = falling within 5 anagrams either way is a 50-50 chance.

Regarding their post-task emotions, participants predicted how they thought they would be feeling after receiving feedback on their task performance. Participants were given three possibilities, pertaining to the relation between their actual performance at their chosen task and their estimated performance at the non-chosen task: They predicted how they would be feeling if their actual performance matched their estimated one; if their actual performance exceeded their estimated one; and, finally, if their actual performance fell below their estimated one. Participants predicted their immediate affective reactions (“immediate prediction”) and their affective reactions two days later (“delayed prediction”). The instructions for the “immediate prediction” were a modified version of the PANAS instructions (Watson et al., 1988; p. 1070), and read as follows (instructions for the “delayed prediction” in brackets): *“The list that follows consists of a number of words that describe different feelings and emotions. Read each item and then mark the appropriate answer into the space next to that word. Indicate to what extent you will be feeling this way immediately after you finish with the task (two days after having*

finished with the task), for each of the three described possibilities". The three possibilities outlined above read as follows: "If your actual performance at the chosen task matches your estimated performance at the non-chosen task" (feedback "equal"); "If your actual performance at the chosen task is higher than your estimated performance at the non-chosen task" (feedback "worse"); "If your actual performance at the chosen task is lower than your estimated performance at the non-chosen task" (feedback "better"). Participants also filled in the decision-related affect scales for each of these three possibilities.

Once they had finished with the predictions, participants chose one of the tasks (i.e. either the easy, or the difficult one). In reality, both versions were identical (i.e. they contained exactly the same anagrams). The only difference was that participants who opted for the easy task received an anagrams pack with "EASY" printed on the first page, whereas participants who opted for the difficult task received an anagrams pack with "DIFFICULT" printed on the first page instead.

Next, participants did the anagram-solving task that they had chosen. Participants who chose the easy task received the following instructions (instructions for participants who chose the difficult task in parentheses): *"In the five pages that follow you will find 100 anagrams. Your task is to solve as many of them as possible in 15 minutes. All the anagrams can be solved. Some of them can be correctly solved in more than one way. Note also that some of them are more difficult than others. If you find an anagram too difficult to work out, move on to the next one. Here is an example of a solved anagram: gear → rage. For each anagram you solve correctly (i.e. you produce a grammatically correct word) you will receive £ 0.07 (£0.09). Good luck!"*

In the next stage of the study, participants evaluated the task and their own expertise at it using the following scales: *“Overall, the anagram task was difficult”*; *“The anagram task was more difficult than I initially thought”*; and *“I am experienced with anagram solving”*. Participants gave their answers on 5-point scales, anchored at 1 = completely agree and 5 = completely disagree. In the time participants took to record their answers, the experimenter counted the correctly solved anagrams and calculated the amount of money that participants would be paid.

Next, all participants received the Feedback manipulation. “Feedback-equal” participants were told that on the basis of some combination of scores in the personality scales, the experimenter estimated that had they chosen the other version of the anagrams task (i.e. the easy version for participants who had chosen the difficult one; the difficult version for participants who had chosen the easy one) they would have done equally well (i.e. they would have solved roughly the same number of anagrams). “Feedback-better” participants were told the same cover story, except that the experimenter estimated that they would have done better (i.e. they would have solved more anagrams). Finally, “feedback-worse” participants were told the same cover story too, except that the experimenter estimated that they would have done worse (i.e. they would have solved fewer anagrams).

After having received this feedback information, participants recorded how they felt regarding their performance using the PANAS and the decision-related affect scales (“immediate experiences”). A couple of days later, participants were e-mailed or called on the phone by the experimenter and they were asked to say how they felt regarding their performance using the PANAS and the decision-related affect scales (“delayed experiences”).

Results

1. Anticipated emotions and choice behaviour

Participants were more likely to choose the easier than the more difficult version of the anagrams task [$\chi^2(1) = 4.07, p = .044$]. Next, I used hierarchical logistic regression modelling in order to predict choices of task on the basis of anticipated emotional reactions to task performance and confidence scores.

The 10 positive items and then 10 negative items of the PANAS were separately summed in order to obtain the overall PA and NA scores, respectively. Across the two predictive judgements (“immediate predictions”; “delayed predictions”) the PA scale had a mean inter-item reliability of .90 (Cronbach Alpha coefficient). The NA scale had a mean inter-item reliability of .92 (Cronbach Alpha coefficient).

I built a two-stage model, with performance prediction, confidence in performance prediction, and expertise entering in the first stage and anticipated emotional reactions entering in the second stage. These anticipated emotional reactions comprised both immediate and delayed predictions for PA, NA, and decision-related affect. As can be seen in Table 4.1, performance-related measures alone were not enough to predict task choice. The inclusion of post-game affective anticipations improved accuracy of prediction of the choices by 14.50%. Overall, more anticipated rejoicing after the completion of the task and higher perceived expertise led participants to choose the more difficult task. In contrast, more anticipated regret immediately after the completion of the task led participants to choose the easier task.

Table 4.1. Logistic regression modelling of task choice as a function of confidence and anticipated post-game emotions

1 st stage model			2 nd stage model		
(performance; confidence; expertise)	Predictors (b-weights)*	Model accuracy	(all the previous & anticipated emotions)	Predictors (b-weights)*	Model accuracy
$R^2 = .07-.09$; $\chi^2(3) = 4.42$, $p = .219$	Expertise (-.60)	64.50%	$R^2 = .34-.46$; $\chi^2(12) = 21.15$, $p = .048$	Rejoicing-IM (-1.72)** Regret-IM (1.21) Expertise (-1.05)	79.00%

*All predictors significant at $p < .05$; ** Abbreviations: IM: prediction of immediate emotions

2. Predicted and actual performance

Participants' predicted and actual performance at the anagram-solving task can be seen in Table 4.2. These scores were submitted to a Choice*Judgement mixed ANOVA, with Choice (Easy vs. Difficult) as the between-subjects factor and Judgement (Predicted vs. Actual) as the within-subjects factor.

The analysis yielded only a main effect of Judgement [$F(1, 68) = 83.76$, $p < .001$], such that, overall, participants overpredicted their performance. This effect was anticipated, since overestimation of skilled performance is a well-established empirical phenomenon (e.g. Harvey, 1994). Importantly, the analysis did not reveal any effect of Choice on performance. The absence of any such effect rules out motivational accounts of participants' performance (for instance, that participants underperformed at the difficult task in order to attribute their low scores at an objectively difficult task to lack of effort rather than lack of competence, etc.).

Table 4.2. Predicted and actual performance across Choice and Feedback conditions (SDs in parentheses)

	Easy task		Difficult task	
	Predicted	Actual	Predicted	Actual
F – Equal	43.93 (20.49)	22.86 (8.56)	36.50 (20.01)	29.10 (8.47)
F – Better	55.77 (20.40)	34.85 (12.65)	63.00 (10.59)	32.50 (7.89)
F – Worse	53.31 (18.52)	28.69 (8.56)	57.86 (19.97)	27.86 (7.93)

3. Predicted and experienced positive and negative affect (PANAS)

The 10 positive items, and then 10 negative items of the PANAS were separately summed in order to obtain the overall PA and NA scores, respectively. Across the four judgements (“immediate predictions”; “delayed predictions”; immediate experiences”; and “delayed experiences”) the PA scale had a mean inter-item reliability of .91, with Cronbach Alpha reliability coefficients ranging from .90 to .93. The NA scale had a mean inter-item reliability of .88, with Cronbach Alpha reliability coefficients ranging from .84 to .91.

Regarding the accuracy of participants’ affective judgements, since the effects of expectations and surprise that I am interested in depend entirely on a.) participants’ choice of task and b.) the type of feedback that participants received after they completed the task, I split the participants into six independent groups on the basis of their choice of task and my feedback manipulation. The resulting groups were the following: Easy/equal; easy/better; easy/worse; difficult/equal; difficult/better; difficult/worse. Participants’ predicted and experienced PA and NA across these six conditions can be seen in Table 4.3 (feedback-equal); Table 4.4. (feedback-better); and Table 4.5. (feedback-worse).

Next I submitted these affective judgements to an Affect*Judgement*Time ANOVA, with Affect (PA vs. NA), Time (Immediate vs. Delayed), and Judgement (Predicted vs. Experienced) as the within-subjects factors. In order to make the analysis easier to follow, I have broken down the findings on the basis of the above grouping.

3.1. Easy / equal (table 4.3.; upper panel): The analysis revealed an effect of Affect [$F(1, 12) = 7.26, p = .020$], such that participants reported overall more PA than NA. The analysis also yielded an effect of Time [$F(1, 12) = 13.48, p = .003$], such that immediate judgements of affect were higher overall than delayed judgements of affect. The absence of any effect of Judgement shows no evidence of affective misprediction in this group.

3.2. Difficult / equal (Table 4.3.; lower panel): The analysis revealed an effect of Affect [$F(1, 7) = 26.39, p = .001$], such that participants reported overall more PA than NA. The analysis also yielded an effect of Time [$F(1, 7) = 24.24, p = .002$], such that immediate judgements of affect were higher overall than delayed judgements of affect. These effects were qualified by a significant Affect*Time interaction [$F(1, 7) = 48.66, p < .001$]. Analysis of this interaction revealed that the immediate judgements of PA were higher than the delayed judgements of PA [$t(7) = 5.83, p = .001$], but that no difference between immediate and delayed judgements was observed for NA [$t(7) = .60, p = .567$]. The absence of any effect of Judgement again shows no evidence of affective misprediction in this group.

Table 4.3. Predicted and experienced PA and NA in the Feedback-equal groups (SDs in parentheses)

Easy task				
	Predicted PA	Experienced PA	Predicted NA	Experienced NA
Immediate	21.69 (9.15)	18.39 (8.68)	13.39 (3.55)	14.69 (5.07)
Delayed	15.62 (8.69)	16.46 (7.34)	11.00 (1.87)	13.15 (5.68)
Difficult task				
	Predicted PA	Experienced PA	Predicted NA	Experienced NA
Immediate	21.25 (5.70)	18.25 (5.87)	11.63 (2.83)	10.63 (1.06)
Delayed	13.13 (3.64)	12.63 (3.29)	11.50 (3.86)	10.38 (0.52)

3.3. *Easy / better* (table 4.4.; upper panel): The analysis revealed an effect of Affect [$F(1, 12) = 5.90, p = .032$], such that participants reported overall more PA than NA. The analysis also yielded an effect of Time [$F(1, 12) = 22.19, p = .001$], such that immediate judgements of affect were higher overall than delayed judgements of affect. More importantly, the analysis also yielded an Affect*Judgement interaction [$F(1, 12) = 22.72, p < .001$]. Participants overpredicted the NA [$t(12) = 4.43, p = .001$] and they underpredicted the PA [$t(12) = -3.37, p = .006$] that they experienced when they found out that they would have been better off choosing the more difficult task. The presence of this effect of Judgement shows evidence of misprediction of both PA and NA in this group.

Table 4.4. Predicted and experienced PA and NA in the Feedback-better groups (SDs in parentheses)

Easy task				
	Predicted PA	Experienced PA	Predicted NA	Experienced NA
Immediate	18.39 (4.03)	22.46 (6.98)	17.31 (6.52)	12.85 (3.93)
Delayed	14.54 (4.33)	19.39 (6.54)	15.00 (5.72)	12.69 (5.22)
Difficult task				
	Predicted PA	Experienced PA	Predicted NA	Experienced NA
Immediate	17.89 (2.37)	22.33 (7.40)	17.78 (6.70)	13.44 (5.62)
Delayed	14.33 (4.56)	21.11 (7.24)	15.56 (6.82)	12.56 (4.16)

3.4. *Difficult / better* (table 4.4.; lower panel): The analysis revealed a weak effect of Affect [$F(1, 8) = 4.44, p = .068$], such that participants reported overall more PA than NA. More importantly, the analysis also yielded an Affect*Judgement interaction [$F(1, 8) = 16.78, p = .003$]. Participants marginally overpredicted the NA [$t(8) = 2.18, p = .061$] and they underpredicted the PA [$t(8) = -4.02, p = .004$] that they experienced when they found out that they would have been better off choosing the easier task. The presence of this effect of Judgement shows evidence of misprediction of both PA and NA in this group.

3.5. *Easy / worse* (table 4.5.; upper panel): The analysis revealed two main effects (Affect and Judgement); three two-way interactions (Affect*Time; Affect*Judgement; and Time*Judgement); and, to qualify all the other effects, the analysis also yielded an Affect*Time*Judgement interaction [$F(1, 13) = 11.83, p = .004$]. Analysis of this interaction revealed that participants overpredicted only the immediate PA that they experienced when they found out that they would have been worse off choosing the more difficult task [$t(15) = 4.47, p < .001$]. No evidence of

misprediction was obtained for immediate NA [$t(15) = -1.02, p = .324$]; delayed PA [$t(15) = .63, p = .540$]; or delayed NA [$t(13) = -1.04, p = .319$]. The presence of this effect of Judgement shows evidence of overprediction of PA in this group.

Table 4.5. Predicted and experienced PA and NA in the Feedback-worse groups (SDs in parentheses)

Easy task				
	Predicted PA	Experienced PA	Predicted NA	Experienced NA
Immediate	29.00 (9.66)	21.29 (8.29)	11.07 (2.46)	12.00 (2.66)
Delayed	20.86 (8.43)	19.57 (9.20)	10.50 (0.94)	11.07 (1.69)
Difficult task				
	Predicted PA	Experienced PA	Predicted NA	Experienced NA
Immediate	33.00 (8.62)	24.86 (7.99)	14.74 (6.70)	14.43 (5.44)
Delayed	25.00 (6.73)	24.14 (8.90)	12.86 (6.72)	13.43 (4.39)

3.6. *Difficult / worse* (table 4.5.; lower panel): The analysis revealed a main effect of Affect [$F(1, 6) = 26.51, p = .002$], such that participants reported overall more PA than NA. The analysis also yielded two other main effects (Time; and Judgement), which were qualified by a weak Time*Judgement interaction [$F(1, 6) = 5.49, p = .058$]. Analysis of this interaction revealed that participants overpredicted only their immediate affective reactions [$t(6) = 4.76, p = .003$]. No evidence of misprediction was obtained for delayed affective reactions [$t(6) = .10, p = .928$]. The presence of this effect of Judgement shows evidence of overprediction of immediate affective reactions in this group.

4. Predicted and experienced decision-related affect

As was the case with the PANAS, participants' predicted and experienced regret, disappointment, rejoicing, and elation across the six groups were submitted to an Affect*Judgement*Time ANOVA, with Affect (Regret vs. Disappointment vs. Rejoicing vs. Elation), Judgement (Predicted vs. Experienced), and Time (Immediate vs. Delayed) as within-subjects factors. I present the findings of this analysis following the groupings used in the previous section.

4.1. *Easy / equal* (tables 4.6.1. & 4.6.2.; upper panels): The analysis revealed an effect of Time [$F(1, 12) = 7.26, p = .020$], such that immediate judgements of affect were overall higher than delayed judgements of affect. The absence of any effect of Judgement shows no evidence of affective misprediction in this group.

Table 4.6.1. Predicted and experienced regret and disappointment in the Feedback-equal groups
(SDs in parentheses)

Easy task				
	Predicted	Experienced	Predicted	Experienced
	regret	regret	disappointment	disappointment
Immediate	1.85 (1.35)	2.08 (1.04)	1.77 (1.01)	2.23 (1.36)
Delayed	1.23 (0.83)	1.54 (0.66)	1.69 (1.18)	1.46 (0.78)
Difficult task				
	Predicted	Experienced	Predicted	Experienced
	Regret	regret	disappointment	disappointment
Immediate	1.13 (0.35)	1.50 (0.93)	1.38 (0.74)	1.63 (0.92)
Delayed	1.25 (0.71)	1.25 (0.71)	1.13 (0.35)	1.25 (0.71)

4.2. *Difficult / equal* (tables 4.6.1. & 4.6.2.; lower panels): The analysis revealed an effect of Time [$F(1, 7) = 7.30, p = .031$], such that immediate judgements of affect were overall higher than delayed judgements of affect. This effect was qualified by a marginal Affect*Time*Judgement interaction [$F(3, 21) = 2.94, p = .057$]. Analysis of this interaction revealed that participants overpredicted the immediate rejoicing [$t(9) = 3.00, p = .015$] that they experienced when they found out that their choice of task did not affect their performance. No evidence of misprediction was obtained for the other decision-related emotions and time perspectives. The presence of this effect of Judgement shows evidence of overprediction of rejoicing in this group.

Table 4.6.2. Predicted and experienced rejoicing and elation in the Feedback-equal groups (SDs in parentheses)

Easy task				
	Predicted	Experienced	Predicted	Experienced
	rejoicing	rejoicing	elation	elation
Immediate	1.69 (1.03)	1.15 (0.56)	1.85 (1.28)	1.62 (0.96)
Delayed	1.31 (0.86)	1.23 (0.83)	1.15 (0.38)	1.39 (0.65)
Difficult task				
	Predicted	Experienced	Predicted	Experienced
	rejoicing	rejoicing	elation	elation
Immediate	1.63 (0.52)	1.00 (0.00)	1.50 (0.93)	1.38 (0.52)
Delayed	1.13 (0.35)	1.00 (0.00)	1.25 (0.71)	1.13 (0.35)

4.3. *Easy / better* (tables 4.7.1. & 4.7.2.; upper panels): The analysis revealed an effect of Affect [$F(3, 33) = 18.48, p < .001$], such that participants reported overall more regret and disappointment than rejoicing and elation. The analysis also yielded an effect of Time [$F(1, 11) = 12.48, p = .005$], such that immediate judgements of

affect were overall higher than delayed judgements of affect. More importantly, the analysis also yielded an Affect*Judgement interaction [$F(3, 33) = 13.99, p < .001$]. Participants overpredicted the regret [$t(12) = 3.03, p = .010$] and the disappointment [$t(12) = 5.20, p < .001$] and they underpredicted the rejoicing [$t(12) = -2.55, p = .025$] and the elation [$t(11) = 3.55, p = .005$] that they experienced when they found out that they would have been better off choosing the more difficult task. The presence of this effect of Judgement shows evidence of misprediction of decision-related affect in this group.

Table 4.7.1. Predicted and experienced regret and disappointment in the Feedback-better groups
(SDs in parentheses)

Easy task				
	Predicted	Experienced	Predicted	Experienced
	regret	regret	disappointment	disappointment
Immediate	3.17 (1.12)	2.17 (0.94)	3.17 (0.94)	1.83 (0.84)
Delayed	2.67 (1.30)	1.92 (1.17)	2.33 (0.89)	1.50 (0.67)
Difficult task				
	Predicted	Experienced	Predicted	Experienced
	Regret	regret	disappointment	disappointment
Immediate	2.00 (1.12)	2.22 (1.09)	3.33 (0.87)	2.33 (1.41)
Delayed	2.11 (0.93)	1.78 (0.97)	2.56 (1.13)	2.44 (1.33)

4.4. *Difficult / better* (tables 4.7.1. & 4.7.2.; lower panels): The analysis revealed a weak effect of Affect [$F(3, 24) = 12.20, p < .001$], such that participants reported overall more regret and disappointment than rejoicing and elation. More importantly, the analysis also yielded an Affect*Judgement interaction [$F(3, 24) = 3.41, p = .034$]. Participants marginally overpredicted the disappointment [$t(8) =$

1.97, $p = .084$] that they experienced when they found out that they would have been better off choosing the easier task. The presence of this weak effect of Judgement shows only fragile evidence of misprediction of disappointment in this group.

Table 4.7.2. Predicted and experienced rejoicing and elation in the Feedback-better groups (SDs in parentheses)

Easy task				
	Predicted	Experienced	Predicted	Experienced
	rejoicing	rejoicing	elation	elation
Immediate	1.08 (0.29)	1.50 (0.80)	1.00 (0.00)	1.83 (0.84)
Delayed	1.08 (0.29)	1.58 (0.79)	1.00 (0.00)	1.50 (0.67)
Difficult task				
	Predicted	Experienced	Predicted	Experienced
	rejoicing	rejoicing	elation	elation
Immediate	1.11 (0.33)	1.22 (0.44)	1.22 (0.67)	1.56 (0.88)
Delayed	1.00 (0.00)	1.33 (0.71)	1.11 (0.33)	1.67 (0.71)

4.5. *Easy / worse* (tables 4.8.1. & 4.8.2.; upper panels): The analysis revealed two main effects (Affect and Time); three two-way interactions (Affect*Time; and Affect*Judgement); and, to qualify all the other effects, it also yielded an Affect*Time*Judgement interaction [$F(3, 39) = 3.04, p = .040$]. Analysis of this interaction revealed that participants overpredicted the immediate rejoicing [$t(15) = 2.55, p = .022$] and the immediate elation [$t(15) = 3.09, p = .007$] that they experienced when they found out that they would have been worse off choosing the more difficult task and that they also underpredicted the immediate disappointment [$t(15) = -3.22, p = .006$] and the delayed disappointment [$t(13) = -2.92, p = .012$]

that they experienced in these circumstances. No evidence of misprediction was obtained for the other decision-related emotions and time perspectives. The presence of this effect of Judgement shows evidence of overprediction of rejoicing and elation and underprediction of disappointment in this group.

Table 4.8.1. Predicted and experienced regret and disappointment in the Feedback-worse groups
(SDs in parentheses)

Easy task				
	Predicted	Experienced	Predicted	Experienced
	regret	regret	disappointment	disappointment
Immediate	1.29 (1.07)	1.29 (0.83)	1.29 (0.61)	2.36 (0.93)
Delayed	1.29 (0.83)	1.36 (0.84)	1.07 (0.27)	1.79 (0.98)
Difficult task				
	Predicted	Experienced	Predicted	Experienced
	Regret	regret	disappointment	disappointment
Immediate	1.71 (1.25)	1.57 (0.79)	1.71 (1.11)	2.00 (1.15)
Delayed	1.29 (0.76)	1.57 (0.79)	1.29 (0.76)	1.57 (0.79)

4.6. *Difficult / worse* (tables 4.8.1. & 4.8.2.; lower panels): The analysis revealed a main effect of Time [$F(1, 6) = 10.74, p = .017$], such that immediate affective judgements were higher than delayed affective judgements. This effect was qualified by a significant Time*Judgement interaction that the analysis also yielded [$F(1, 6) = 6.25, p = .047$]. Analysis of this interaction revealed that only judgements of immediate rejoicing were higher than judgements of delayed rejoicing [$t(6) = 2.76, p = .033$]. More interesting was the main effect of Affect [$F(3, 18) = 4.95, p = .011$] that the analysis also revealed. Participants reported more overall rejoicing and elation than regret and disappointment. This effect was qualified by a significant

Affect*Judgement interaction that the analysis also yielded [$F(3, 18) = 3.22, p = .048$]. Analysis of this interaction revealed that participants overpredicted only the immediate rejoicing [$t(6) = 4.04, p = .007$] that they experienced when they found out that they would have been worse off choosing the easier task. No evidence of misprediction was obtained for the other decision-related emotions. The presence of this effect of Judgement shows evidence of overprediction of immediate rejoicing in this group.

Table 4.8.2. Predicted and experienced rejoicing and elation in the Feedback-worse groups (SDs in parentheses)

Easy task				
	Predicted	Experienced	Predicted	Experienced
	rejoicing	rejoicing	elation	elation
Immediate	2.14 (1.35)	1.36 (0.63)	2.71 (1.44)	1.57 (0.76)
Delayed	1.71 (1.00)	1.50 (0.76)	1.93 (1.00)	1.36 (0.75)
Difficult task				
	Predicted	Experienced	Predicted	Experienced
	rejoicing	rejoicing	elation	elation
Immediate	3.86 (1.21)	1.86 (1.07)	3.57 (1.62)	2.57 (1.27)
Delayed	2.00 (1.00)	1.71 (0.95)	2.57 (1.51)	2.29 (1.25)

Discussion

The findings of Study 1 address two different research questions². The first question is to what extent people's risky choice behaviours are guided by their anticipations of their emotional reactions and by their comparisons of actual

² The findings of Study 1 also relate to the question of overconfidence in forecasts of skilled performance. However, since this topic is not directly relevant to my purposes here, I shall not elaborate on these findings.

outcomes to counterfactual ones. The second question is how accurately people predict their emotions when a.) they receive outcomes surprisingly better than the ones that they expected; and b.) they receive outcomes surprisingly worse than the ones that they expected. In what follows, I summarise and comment on the findings of Study 1 that are relevant to these questions.

Anticipated affective reactions and task choice

Regarding this question, the findings of Study 1 are clear. As Prospect Theory predicts (Kahneman & Tversky, 1979), participants chose the easier task more often than the difficult task. Their choice of task partly depended on their subjective expertise in the task (i.e. in anagram-solving), but more so on the regret and rejoicing that they thought they would feel once they would receive the task outcomes and the task-relevant feedback. This finding is interesting for at least two reasons. Firstly, it replicates previous findings using a research setting that can be considered “richer” than the traditional gamble tasks that researchers in the domain have used. I take this setting to be richer in the sense that it involved participants’ evaluation and use of their skilled performance and also because it involved self-relevant performance feedback – which is very often the case in real-life situations.

Secondly, the above finding demonstrates that the influence of anticipated emotions on decision processes can be “emotion-specific” – in other words, some of the aspects of this influence cannot be captured by general measures of affect. This is in line with Zeelenberg, van Dijk, and Manstead’s (1998) finding that general happiness measures cannot capture the relationship between responsibility for a decision and post-decisional emotional experiences, whereas more specific measures of regret can. In a different domain, measure specificity in the assessment of subjective experiences and evaluations has also been identified as a factor that

enhances the predictive validity of the structural models of attitudes, as in the case of the Theory of Planned Behaviour (e.g. Ajzen, 1991).

Affective forecasts when outcomes are mildly surprising

When participants were told that they would have solved the same number of anagrams had they chosen the other task, participants who had chosen the easy task (easy/equal condition) were faced with a mild negative surprise (since with the same number of anagrams they could have made 29% more money), whereas participants who had chosen the difficult task (difficult/equal condition) were faced with a mild positive surprise (since with the same number of anagrams they could have foregone 29% of their reward).

These participants indeed experienced more rejoicing than they had anticipated – however, the effect was rather weak. However, participants in the easy task condition reported higher levels of positive than negative affect. I take these findings to indicate that, in general, participants in these two conditions did not bother too much with the (counterfactual) comparisons that I mentioned above. A number of factors may have led to this findings: a) the amounts of money involved were low; b) participants tried not to appear greedy to the experimenter; or c) participants did not perceive the intended boost (for those who did the easy task) or threat (for those who did the difficult task) to their self-esteem from the feedback.

Affective forecasts when outcomes are strongly negatively surprising

When participants were told that they would have been better off had they chosen the other task, participants who had chosen the difficult task (difficult/better condition) had good reasons to regret their choice. The task was seriously difficult: Overall, participants solved 29% of the anagrams, with the range of performance

starting from as low as 13% and not exceeding 60%. Unsurprisingly, these participants experienced more regret and disappointment than rejoicing and elation. Regarding the accuracy of their affective forecasts to this situation, participants overpredicted their negative affect and they also tended to overpredict their disappointment. In contrast, they underpredicted their positive affect.

Turning to participants who had chosen the easy task (easy/better condition), when they were given similar feedback they too had reasons to regret their choice, since they could have solved more anagrams and made more money had they chosen the difficult task. Given the difficulty of the task, however, the plausibility of the cover story for these participants must have been rather low; therefore, we expected milder effects for this group. However, these participants experienced more regret and disappointment than rejoicing and elation. Regarding the accuracy of their affective forecasts to this situation, participants overpredicted their negative affect, regret, and disappointment. In contrast, they underpredicted their positive affect, rejoicing, and elation.

These comparisons also reinforce the argument on specificity of affective measures that was made above: In both groups, whereas regret and disappointment scores were higher than the corresponding rejoicing and elation scores, the overall scores of positive affect were higher than the overall scores of negative affect.

Affective forecasts when outcomes are strongly positively surprising

When participants were told that they would have been worse off had they chosen the other task, and given the level of task difficulty, participants who had chosen the easy task (easy/worse condition) had good reasons to be happy with their choice. Unsurprisingly, these participants experienced more rejoicing and elation than regret and disappointment. Regarding the accuracy of their affective forecasts to

this situation, participants overpredicted their rejoicing and elation, but underpredicted their disappointment.

Turning to participants who had chosen the difficult task (difficult/worse), when they were given similar feedback they too had reasons to be happy with their choice, since they could have solved fewer anagrams and made less money had they chosen the easy task. Once again, given the difficulty of the task, the plausibility of the cover story for these participants must have been rather low; therefore, I expected milder effects for this group. However, these participants experienced more rejoicing and elation than regret and disappointment. Regarding the accuracy of their affective forecasts to this situation, participants overpredicted their rejoicing only.

Finally, regarding the argument on the specificity of affective measures, the findings from these two last groups are mixed. Whereas the overprediction of rejoicing and elation that was observed in the first group (easy/worse condition) was accompanied by an overprediction of positive affect, no evidence of misprediction of either positive or negative affect was obtained for the second group (difficult/worse condition).

Summary

Study 1 accomplished all of its aims. It provided evidence that studies that employ affective measures need to be explicit about the degree of specificity of these measures (see global indices vs. specific affective items). Study 1 also provided firm evidence that affective forecasts guide individual choice behaviour even though they are not accurate. Finally, Study 1 also showed that people overestimate the impact of major surprises, but that they are accurate when they judge the impact of minor surprises on their affective state.

CHAPTER

5

Anticipated regret in investment decisions:

An instance of focalism?

Post-decisional regret considerations in financial decision-making

Zeelenberg and Beattie (1997) found empirical evidence for the presence of regret considerations in investment decisions. Their participants read a scenario that involved a choice between two positive, non-matched (i.e. not equally attractive) investment options, a safer option and a riskier option. Half of the participants were informed that their (hypothetical, scenario-based) sister had already chosen the riskier of the two options. According to Zeelenberg and Beattie, this information provided participants with expectations of post-decisional information (or “feedback”, as they called it) on the performance of the riskier investment option – since no matter which option they opted for, at the end of the investment period they would find out how well the riskier investment had done from their sisters. The other half of the participants did not receive this expectation of feedback – therefore, they had no way of finding out how well the riskier investment had done.

Zeelenberg and Beattie’s (1997) reasoning was that participants who would not find out the outcome of the riskier option would prefer the safer investment. This is Prospect Theory’s prediction for risky choices in the domain of gains (Kahneman & Tversky, 1979). However, participants who would find out the outcome of the riskier option from their sister would prefer the riskier alternative. In line with Boles and Messick’s (1995) findings that a possible source of post-decisional regret is interpersonal comparison between one’s self and others, Zeelenberg and Beattie (1997) suggested that information on the outcome of the riskier investment would make the interpersonal comparison between participants and their sister salient. This, in turn, would make post-decisional regret considerations influential. These considerations would eventually lead to riskier investment preferences – in other words, participants

would try to minimise the likelihood that they received worse outcomes than their sisters by opting for the same investment as she had opted for. Zeelenberg and Beattie's (1997) empirical findings supported these predictions.

2. Studies 2 to 4: Aims and hypotheses

To summarise, the reasoning underlying the investment decisions of people who anticipate post-decisional feedback on an investment option is to block out any future information that could cause them to regret their initial decision. In line with Wilson et al.'s (2000) research on prospective focalism, I propose that these regret considerations can be attributed to focalism. Participants who face the possibility of regretting their decision in the future focus narrowly on this potential future regret, thereby discounting the other defining aspect of the decision – namely its riskiness. If this is true, then if participants are de-focused from the decision in question before they report their investment preferences, the influence of regret-minimisation on participants' investment decision should attenuate. The aim of the three studies summarised in this chapter was to test for this possibility.

Study 2

In Study 2 I presented participants with two uncertain positive investment options. One of the two options was riskier than the other. Participants were asked to indicate their intentions to invest in each of them. Half of the participants were expecting post-decisional feedback on the performance of the riskier investment (and their chosen investment if it was not the riskier one), whereas the other half of the participants were expecting post-decisional feedback on the performance of their

chosen investment only (whichever this option was). In addition to the expectation of feedback, half of the participants in each group were de-focused before recording their intentions to invest. Before recording their investment intentions, de-focused participants completed a prospective diary of events likely to happen to them in the investment time-span.

Regarding participants' investment intentions, I expected that focused participants would favour the riskier investment. This is the regret-minimising option that RT predicts and Zeelenberg and Beattie (1997) observed. I also expected that de-focused participants would favour the safer investment. This is the risk-minimising option that Prospect Theory predicts.

Method

Participants: One hundred and forty-eight students of the University of London volunteered to participate in the study (mean age = 24.75 years; $SD = 6.23$ years; seventy-eight females). Participants were not paid for their participation.

Variables and Design: The design was a 2*2 completely between-subjects design, with Anticipated Feedback (Choice only vs. Risky choice) and Diary (No-Diary vs. Diary) as independent variables. There were 37 participants in each condition.

The dependent variable was participants' intentions to invest in one of the two options.

Materials and Procedure: All participants were presented with a short booklet containing manipulations and dependent measures. On the first page of the booklet,

participants in the “choice-only” feedback condition were asked to imagine themselves as the protagonist in the following scenario (modified version of Zeelenberg & Beattie’s 1997 scenario; p. 66):

“Your uncle has just died and left you £1000. You now have to decide how to invest the money for 3 years. A friend has just told you about two kinds of investment; the first one is a Government Bond, which is guaranteed to pay back a total sum between £1000 and £1800 at the end of the three years. The second one is to invest the money for the same three years period in a High Interest Account, which is guaranteed to pay back a total sum between £1250 and £1350 at the end of the three years. You know for sure that at the end of the three years and in both instances, you will not be able to find out how much money you would have made if you had chosen the other one”.

The scenario for the “risky-choice” feedback condition read as follows:

“Your uncle has just died and left you £1000. You now have to decide how to invest the money for the following 3 years. Your uncle has also left your sister £1000; she has already made up her mind and invested her money for the same three years period in a Government Bond, which is guaranteed to pay back a total sum between £1000 and £1800 at the end of the three years. You can choose to invest your money in this kind of investment too. On the other hand, a friend has just told you about another kind of investment which you could choose, a High Interest Account, which is guaranteed to pay back a total sum between £1250 and £1350 at the end of the three years. You know for sure that at the end of the three years you will find out how much money you would have made if you had chosen the Government Bond, because your sister will tell you”.

On the second page of the booklet, participants in the “diary” condition received the following instructions: *“Now we would like you to think of and briefly describe the*

major events that are likely to occur to you in the next three years (feel free to describe as many events as you like)". Participants were given approximately three quarters of a page to describe the events that they could think of. Participants in the "no-diary" condition did not perform any filler task.

Finally, all participants went through the last page of the booklet, where they had to record their intentions to invest the money in one or the other option. The item read as follows: "*I intend to invest my £1000...*". Participants gave their answers on a 9-point scale, anchored at -4 = definitely in the High Interest Account and +4 = definitely in the Government Bond¹.

Results

Reported intentions to invest can be seen in Table 5.1. The intention scale was submitted to a Feedback*Diary ANOVA, with Feedback (Choice only vs. Risky choice) and Diary (No-Diary vs. Diary) as between-subjects factors. The analysis yielded an unexpected effect of Diary [$F(1, 144) = 8.71, p = .004$], such that "diary" participants were more risk-averse than "no-diary" participants.

The anticipated main effect of Feedback and the interaction between Feedback and Diary were not obtained.

¹ Asking participants to indicate which one of the two options they would choose might seem a more straightforward measure here. Research on the measurement of behavioural intentions, however, suggests that continuous measures of intentions should be used instead (unipolar, or bipolar, as is the case here). A self-reported intention to perform a behaviour corresponds to a subjective probability that the person will eventually exhibit the behaviour rather than to a certainty (e.g. Ajzen, 1991; 2002; Himmelfarb, 1993; Osgood, Suci, & Tannenbaum, 1957).

Table 5.1. Mean intentions to invest across conditions (*SDs* in parentheses)

	Choice only Feedback	Risky choice Feedback
No Diary	1.00 (2.77)	1.22 (2.44)
Diary	0.00 (2.55)	-0.30 (2.59)

Discussion

Study 2 failed to replicate the regret effect that Zeelenberg and Beattie (1997) demonstrated. The difference between this study and theirs was that whereas they used two unipolar scales for the two investment options (i.e. an 1-7 scale for the safer option and another 1-7 scale for the riskier option), I assessed investment intentions using one single bipolar scale. From a normative viewpoint, people's preferences for one investment over another should exhibit invariance to the representation of the problem. However, there is empirical evidence that different elicitation formats result in preference reversals between sets of options (for a classic account of preference reversals see Lichtenstein & Slovic, 1971; Slovic & Lichtenstein, 1983).

For instance, Mellers and her colleagues looked at participants' preferences for gambles presented in pairs using four different response formats (namely attractiveness ratings; buying prices; selling prices; and strength of preference judgements). They found that the different formats elicited different responses (Mellers, Chang, Birnbaum, & Ordóñez, 1992). More relevant to these findings is the research by Bazerman, Loewenstein, and White (1992). They found preference reversals between choices and evaluations of alternatives. More importantly, they found that people put more emphasis on their own payoffs when they chose between alternatives, whereas they put more emphasis on the relative value of the same payoffs (that stemmed from the

interpersonal comparison of payoffs for self versus payoffs for others) when they evaluated alternatives.

I suspect that this “choice vs. evaluation” discrepancy is the reason that I failed to replicate Zeelenberg and Beattie’s (1997) findings. The bipolar scale that I used resembles a choice task: My participants saw the two options at the two opposite ends of a continuum. With this format, as with any binary choice format, the intention to invest in one of the option automatically precludes the intention to invest to the other. On the other hand, the unipolar scales that Zeelenberg and Beattie (1997) used resemble more an evaluation (or strength of preference) task: Their participants evaluated the two options separately, such that “some” preference for one of them did not automatically preclude “some” simultaneous preference for the other one. Therefore, in Study 3, I sought to replicate the regret effect that Zeelenberg and Beattie observed using two unipolar scales.

Study 2 also demonstrated that the provision of a real-life context for the investment decision (via the de-focusing manipulation) led to uniformly risk-averse intentions. This finding is interesting because Prospect Theory’s postulate that people are risk-averse in the domain of gains is not enough to accommodate it. Participants in both the “no-diary” and the “diary” conditions were judging two positive investments, but only those in the “diary” condition exhibited risk-aversion. When I looked at the events that participants had mentioned in their diaries, the following major categories emerged: Getting a job (75%), graduating (53%), moving house (44%), continuing studies (37%), buying a house or a car (33%), starting a family (31%), and travelling (30%). These qualitative data seem to suggest the following tentative explanation for these participants’ risk-aversion: Most of the events that were mentioned in the diaries

involve a substantial financial commitment. It is, therefore, plausible that this commitment led these participants to be risk-averse. This explanation is in line with Slovic's (1964) idea that risk preferences are context-specific (see also Shapira, 1994). If this is the case, then it follows that different contexts (i.e. contexts evoked by qualitatively different diaries) would elicit different risk preferences. We tested this possibility in Study 3.

To summarise, the next steps that I took were, firstly, to try to replicate the regret effect that Zeelenberg and Beattie (1997) demonstrated and, secondly, to further investigate the contextualisation effect of de-focusing.

Study 3

In Study 3 I presented participants with two uncertain positive investment options. One of the two options was riskier than the other. Participants were asked to indicate their intentions to invest in each of them. Half of the participants were expecting post-decisional feedback on the performance of the riskier investment, whereas the other half of the participants were expecting post-decisional feedback on the performance of their chosen investment only (whichever this option was). As in Study 2, in addition to the expectation of feedback, some participants were de-focused before recording their intentions to invest.

In order to test whether different evoked contexts would give rise to different risk preferences (e.g. Shapira, 1994; Slovic, 1964), in this study I also manipulated the context that the de-focusing manipulation evoked. For half of the de-focused participants the evoked frame of reference for the investment decision was one of future

responsibilities; for the other half of the de-focused participants the evoked frame of reference for the investment decision was a more “carefree” one.

Moreover, I assessed the positive or negative valence of the diary-evoked contexts. In their initial investigation of focalism, Wilson et al. (2000) suggested two underlying de-biasing mechanisms that might be operating in the de-focusing stage. Since people are reminded of other occurrences that will take place together with the focal event, they are either distracted from the event (the “distraction hypothesis”), or they realise that the contextualising events will carry emotional consequences of their own and they will thereby influence people’s emotional state in addition to the focal event (the “affective competition hypothesis”). Wilson et al.’s (2000) data supported the distraction hypothesis. In Study 3 I attempted to discriminate between the two hypotheses. A pattern of reliable correlations between the valence of the diaries and the investment intentions would support the affective competition hypothesis. Absence of such pattern would fail to provide support for this hypothesis, thereby leaving only the distraction hypothesis.

Regarding the effects of the presence of context on participants’ investment intentions, I expected that focused participants would favour the riskier investment. For the de-focused participants, if the specific content of the de-focusing manipulation is irrelevant, then participants should uniformly take the safer option. However, if the specific content of the de-focusing manipulation is relevant, then participants should take the safer option when the context is one of forthcoming responsibilities and they should take the riskier option (or at least be risk-neutral) when the context is a carefree one. Regarding the effects of the type of context on participants’ investment intentions, following the findings of Study 2, I expected that their preference for the safer option

would be stronger in the “responsibilities-diary” condition than in the “carefree-diary” condition and, conversely, that their preference for the riskier option would be stronger in the “carefree-diary” condition than in the “responsibilities-diary” condition.

Method

Participants: One hundred and sixty-two students of the University of London volunteered to participate in the study (mean age = 22.36 years; $SD = 4.83$ years; one hundred and nineteen females). Participants were not paid for their participation.

Variables and Design: The design was a 2*3 completely between-subjects design, with Anticipated Feedback (Choice-only versus Risky-choice) and Diary (No-Diary vs. Responsibilities-Diary vs. Carefree-Diary) as independent variables. There were 27 participants in each condition.

The first dependent variable was participants’ intention to invest in the safe option (“safe”). The second dependent variable was participants’ intention to invest in the risky option (“risky”).

Materials and Procedure: All participants were presented with a short booklet containing manipulations and dependent measures. On the first page of the booklet, participants in the “choice-only” feedback condition were asked to imagine themselves as the protagonist in the following scenario:

“Your uncle has just died and left you £1000. You now have to decide how to invest the money for 3 years. A friend has just told you about two kinds of investment; the first one is a Government Bond, which is guaranteed to pay back a total sum between £1000 and

£1800 at the end of the three years. The second one is to invest the money for the same three years period in a High Interest Account, which is guaranteed to pay back a total sum between £1300 and £1500 at the end of the three years. You know for sure that at the end of the three years and in both instances, you will not be able to find out how much money you would have made if you had chosen the other one”.

The scenario for the “risky-choice” feedback condition read as follows:

“Your uncle has just died and left you £1000. You now have to decide how to invest the money for the following 3 years. Your uncle has also left your sister £1000; she has already made up her mind and invested her money for the same three years period in a Government Bond, which is guaranteed to pay back a total sum between £1000 and £1800 at the end of the three years. You can choose to invest your money in this kind of investment too. On the other hand, a friend has just told you about another kind of investment which you could choose, a High Interest Account, which is guaranteed to pay back a total sum between £1300 and £1500 at the end of the three years. You know for sure that at the end of the three years you will find out how much money you would have made if you had chosen the Government Bond, because your sister will tell you”.

On the second page of the booklet, one third of the participants (“responsibilities-diary”) received the following instructions: *“Now, we would like you to think of and briefly describe two major events that are likely to occur to you during the next three years and are likely to increase your personal responsibilities”.* Participants were given two lines to summarise each event. After each event, participants also filled in the following two scales: *“The event described above I regard as positive”* and *“The event described above I regard as negative”*. Participants gave

their answers on two 8-point scales, anchored at 1 = strongly agree and 8 = strongly disagree.

Another third of the participants (“carefree-diary”) received the following instructions: *“Now, we would like you to think of and briefly describe two major events that are likely to occur to you during the next three years but are unlikely to increase your personal responsibilities”*. After each event, participants also filled in the following two scales: *“The event described above I regard as positive”* and *“The event described above I regard as negative”*. Participants gave their answers on two 8-point scales, anchored at 1 = strongly agree and 8 = strongly disagree. Participants in the “no-diary” condition did not perform any filler task.

Finally, all participants went through the last page of the booklet, where they had to record their intentions invest the money in the two investments. The two items read as follows: *“I intend to invest my £1000 in the High Interest Account”* (“safe”); and *“I intend to invest my £1000 in the Government Bond”* (“risky”). Participants gave their answers on 8-point scales, anchored at 1 = definitely no and 8 = definitely yes.

Results

1. Intentions to invest

Reported intentions to invest can be seen in Table 5.2. The intention scales were submitted to a Feedback*Diary*Option mixed ANOVA, with Feedback (Choice only vs. Risky choice) and Diary (No-Diary vs. Responsibilities-Diary vs. Carefree-Diary) as between-subjects factors and Option (Safe vs. Risky) as a within-subjects factor. The analysis revealed an effect of Option [$F(1, 156) = 22.95, p < .001$], such that participants overall preferred the safer investment to the riskier one. This effect,

however, was qualified by the Feedback*Diary*Option interaction [$F(2, 156) = 3.23, p = .042$]. I split this interaction into three Feedback*Option ANOVAs – one for “no-diary” participants, one for “responsibilities-diary” participants, and one for “carefree-diary” participants.

For “no-diary” participants the analysis revealed only a Feedback*Option interaction [$F(1, 52) = 7.86, p = .007$]. Whereas “choice-only” feedback participants preferred the safer to the riskier investment [$t(26) = 3.41, p = .002$], “risky-choice” participants were indifferent between the two investments [$t(26) = -.64, p = .526$].

Table 5.2. Mean intentions to invest across conditions (SDs in parentheses)

	Choice-only Feedback		Risky-choice Feedback	
	Safe option	Risky option	Safe option	Risky option
No-Diary	5.78 (1.65)	3.74 (1.81)	4.37 (1.74)	4.78 (1.86)
Responsibilities-Diary	5.22(1.85)	3.70 (1.71)	5.78 (1.67)	3.37 (1.88)
Carefree-Diary	5.44 (1.93)	4.15 (2.03)	5.26 (1.91)	4.33 (2.06)

For “responsibilities-diary” participants the analysis revealed only a main effect of Option [$F(1, 52) = 18.06, p < .001$], such that participants overall preferred the safer to the riskier investment.

Finally, for “carefree-diary” participants the analysis also revealed a main effect of Option only [$F(1, 52) = 4.83, p = .032$], such that participants overall preferred the safer to the riskier investment.

2. Valence of diaries

For each event that participants reported in their diaries I computed a “positivity index” by subtracting the perceived negativity of the event from its perceived positivity. I then computed the correlations between intentions to invest (“safe”; “risky”) and the positivity indices. The resulting correlation coefficients were neither reliable (none was significant), nor sizeable (Pearson r s < .10).

Discussion

Study 3 replicated the regret effect that Zeelenberg and Beattie (1997) observed. Participants favoured their sister’s chosen investment more when they would receive feedback on that investment only, arguably, in order to minimise the chances that they would experience post-decisional regret at the end of the investment period.

This regret-minimising choice was not observed when participants put their investment decision in the broader context of their lives. De-focused participants favoured the safer alternative – as Prospect Theory predicts. Study 3, therefore, documents the presence of focalism in financial decision-making. Moreover, I found no evidence that the valence of the diaries influences subsequent self-reported intentions – a finding that corroborates Wilson et al.’s (2000) distraction hypothesis at the expense of the affective competition hypothesis.

Finally, I failed to document an influence of the evoked context on participants’ investment intentions – although, as I had anticipated, the participants’ preference for the safer option was stronger in the “responsibilities-diary” condition than in the “carefree-diary” condition. Assuming that the manipulation of context was strong and

relevant enough in order to induce the hypothesised context differences, Study 2 provided no firm evidence that the effect of de-focusing is context-specific.

The next step that I took was to attempt to replicate the focalism effect in the domain of losses.

Study 4

Study 4 was a replication of Studies 2 and 3 in the domain of losses. Participants were presented with two uncertain negative investment options. One of them was riskier than the other. Participants were asked to indicate their intentions to invest to each of them. Half of the participants were expecting post-decisional feedback on the performance of the safer investment, whereas the other half of the participants were expecting post-decisional feedback on the performance of their chosen investment only (whichever this option was). In addition to the expectation of feedback, half of the participants were de-focused before recording their intentions to invest. As in Study 3, the positive or negative valence of the diaries was also assessed.

Regarding participants investment intentions, I expected that focused participants would favour the safer investment (i.e. they would prefer the regret-minimising option, as RT suggests). I also expected that de-focused participants would favour the riskier investment (i.e. they would prefer the risk-maximising option, as Prospect theory suggests). Regarding the comparison between the distraction hypothesis and the affective competition hypothesis for focalism, I expected that the de-focusing effects would depend on the presence of the evoked context, rather than on the valence of it.

Method

Participants: Fifty students from various London universities volunteered to participate in the study (mean age = 23.42; $SD = 4.77$; thirty-one females). Participants were paid £5 for their participation (in this and other studies).

Variables and Design: The design was a 2*2 mixed design, with Anticipated Feedback (Choice-only vs. Safe-choice) as a between-subjects independent variable and Diary (No-Diary vs. Diary) as a within-subjects independent variable. There were 24-26 participants in each condition.

The first dependent variable was participants' intention to withdraw the money from the money market altogether ("safe"). The second dependent variable was participants' intention to withdraw the money from their current investment and reinvest it in another option ("risky").

Materials and Procedure: All participants were presented with a short booklet containing manipulations and dependent measures. On the first page participants in the "choice-only feedback" condition were asked to imagine themselves as the protagonist of the following scenario:

"Some time ago a distant relative of yours died and left you £1000. You decided to invest this money. After some investment market research, you eventually invested the money in a one-year Government Bond. Unfortunately however, because of the World Trade Centre events, your Government Bond is not doing very well. In order not to lose your money, a friend suggests two solutions. The first one is to withdraw the money from the one-year Government Bond right away and not reinvest in anywhere. In this

case you will suffer a certain loss of £300 – £500. The second solution is to withdraw the money from this particular one-year Government Bond and reinvest it for the remaining time in a private bond, the ABC Bond. In this case you will suffer a loss of £0 – £800. You know for sure that at the end of the investment period you will not find out how much money you would have saved had you chosen the other option (whichever that option is)”.

The scenario for the “safe-choice feedback” condition read as follows:

“Some time ago a distant relative of yours died and left you and your sister £1000. You decided to invest this money. After some investment market research, both of you eventually invested the money in a one-year Government Bond. Unfortunately however, because of the World Trade Centre events, your Government Bond is not doing very well. In order not to lose your money, your sister suggests the following solution: Withdraw the money from the one-year Government Bond right away. In this case you will suffer a certain loss of £300 – £500. In any case, that’s what your sister has already made up her mind to do with her own investment. There is, however, a second option. You can withdraw the money from this particular one-year Government Bond and reinvest it for the remaining time in a private bond, the ABC Bond. In this case you will suffer a loss of £0 – £800. You know for sure that at the end of the investment period you will find out how much money you would have saved had you chosen to withdraw your money from the Government Bond and not reinvest it; this is because your sister will tell you”.

On the second page of the booklet, participants were asked to indicate their investment intentions. The items read as follows: “*I intend to withdraw my money from the Government Bond and not reinvest it*” (“safe”); “*I intend to withdraw my money*

from the Government Bond and reinvest it in the ABC Bond ("risky"). Participants gave their answers on 8-point scales, anchored at 1 = definitely no and 8 = definitely yes.

Next, participants completed some unrelated personality scales and then they received the same investment scenarios again. This time, before indicating their intentions all participants went through the Diary manipulation. The diary instructions read as follows: *"Now, we would like you to think of and briefly describe two major events that are likely to occur to you during the following year"*. Participants were given two lines to summarise each event. After each event, participants also filled in the following two scales: *"The event described above I regard as positive"* and *"The event described above I regard as negative"*. Participants gave their answers on two 8-point scales, anchored at 1 = strongly agree and 8 = strongly disagree. Finally, participants went through the last page of the booklet, where they filled in the two intention scales once again.

Results

1. Intentions to invest

Reported intentions to invest can be seen in Table 5.3. The intention scales were submitted to a Feedback*Diary*Option mixed ANOVA, with Feedback (Choice only vs. Safe choice) as a between-subjects factor and Diary (Diary vs. No-Diary) and Option (Safe vs. Risky) as within-subjects factors. The analysis revealed only a main effect of Option [$F(1, 46) = 4.65, p = .036$], such that participants overall preferred the riskier to the safer option.

2. Valence of diaries

For each event that participants reported in their diaries I computed a “positivity index” by subtracting the perceived negativity of the event from its perceived positivity. I then computed the correlations between intentions to invest (“safe”; “risky”) and the positivity indices. The resulting correlation coefficients were neither reliable (none was significant), nor sizeable (Pearson r s < .09).

Table 5.3. Mean intentions to invest across conditions (*SDs* in parentheses)

	Choice-only Feedback		Safe-choice Feedback	
	Safe option	Risky option	Safe option	Risky option
No Diary	3.67 (1.93)	4.96 (2.12)	3.69 (1.81)	4.89 (2.29)
Diary	3.58 (1.98)	4.96 (2.16)	3.81 (2.15)	4.42 (2.28)

Discussion

As Prospect Theory predicts, participants preferred the riskier to the safer option in the domain of losses. In other words, Study 4 failed to document a regret effect in the domain of losses. Josephs, et al. (1992) also failed to document a regret effect in the domain of losses using a gamble task – whereas they obtained the effect in the domain of gains. Josephs et al. (1992) argued that the motivation that underlies people’s regret aversion is the motivation to preserve a sense of high self-esteem. A regrettable decision implies that the decision-maker is incompetent – hence the blow to one’s self-esteem and the resulting regret avoidance (see also Larrick, 1993, for the self-esteem argument). However, when a decision is certain to lead to losses the decision-maker

cannot appear competent, since, no matter how small the loss is, it is still a (regrettable) loss – hence there is no room left for regret avoidance to influence the decision process.

Zeelenberg et al. (1996; experiment 3), however, obtained a regret effect in the domain of losses, using also a gamble task and the anticipated feedback manipulation that I used. Their rationale was that the anticipation of a complete resolution of uncertainty that the post-decisional feedback provides gives rise to regret considerations regardless of gains or losses. Closer inspection of their data (Zeelenberg et al. 1996; pp. 155-156), however, suggests that the regret avoidance that they observed was only due to a weak preference for the safer gamble when feedback would be provided on the choice of that gamble [$\chi^2(1) = 3.56, p = .059$]. Zeelenberg et al.'s (1996) participants' preference for the risky gamble was not affected at all [$\chi^2(1) = .89, p = .346$]. I consider these findings only weak evidence for the empirical documentation of regret avoidance in the domain of losses. I am not arguing that it is not possible or meaningful to document regret effects in the domain of losses. However, with the task that was used, the regret effect was not found².

The absence of the regret effect in these findings precluded any test of focalism in the domain of losses. Interestingly, I found no evidence that the valence of the diaries influences subsequent self-reported intentions – a finding that corroborates the corresponding finding of Study 3 and supports Wilson et al.'s (2000) distraction hypothesis. However, the absence of an effect of focalism precludes a firm conclusion

² It should also be noted here that whereas a between-subjects design was used in Zeelenberg et al.'s (1996) experiment 3, in Study 4 I used a within-subjects design instead. If the inconsistency in the findings can be partly attributed to the choice of experimental design (as it has been done in the past regarding regret-related effects; see, for instance, Zeelenberg, van Dijk, & Manstead, 2000), it would also serve as further evidence of the non-robustness of regret effects in the domain of losses.

regarding the comparison between the distraction hypothesis and the affective competition hypothesis on the basis of these data.

General Discussion

Overall, the research that I summarised in this chapter provides some empirical evidence for the presence of focalism in investment decisions. Specifically, whereas participants who were focused on a positive investment decision with regret potential exhibited regret-aversion, participants who were de-focused from the investment decision in question did not exhibit regret-aversion (Study 3). Moreover, de-focused participants' behavioural intentions did not appear to be influenced by the positive or negative valence of the evoked frame of the investment decision (Study 3; Study 4). Finally, I did not observe either regret-aversion or focalism in negative investment decisions (Study 4).

The above findings provide partial empirical support for the hypotheses. Regret aversion was present when participants anticipated that they would find out how much better (or worse) off they would have been had they chosen an investment option different from the one that they actually chose. However, regret aversion was only present for decisions in the domain of gains. Furthermore, the elimination of regret aversion when participants were de-focused from the investment decision in question allows regret aversion to be attributed to focalism. In other words, when people are narrowly focused on the outcomes of an uncertain investment, they try to minimise the chances that they regret a current investment decision of theirs. However, when people see the same decision in a broader real-life frame, regret-aversion considerations seem to subside.

Finally, this series of studies revealed two other, unrelated effects. The first effect was the effect of response format on people's self-reported risk preferences (Study 2). A response format that resembles a binary choice task (i.e. unique bipolar intention scale for two investment alternatives) revealed different risk-preferences than a response format that resembles a strength-of-preference task (i.e. separate unipolar intention scales for the two investment alternatives). The second effect was the absence of regret aversion in the domain of losses. This null result both agrees (Josephs et al., 1992) and conflicts (Zeelenberg et al., 1996) with previous findings.

CHAPTER

6

Anticipated post-decisional emotional reactions in

Ultimatum Game bargaining:

An instance of focalism?

Post-decisional emotional considerations in Ultimatum Game bargaining

A task that has been extensively used for the study of people's behaviour in dyadic negotiation is the "Ultimatum Game" (UG). In the simplest form of the UG two players are allocated the roles of the "proposer" and the "responder". The proposer is then endowed with an amount of money, which she has to split between herself and the responder. Once the proposer has made an offer, the responder can either accept the split, in which case the two players share the amount accordingly, or reject the split, in which case neither of the players makes any money.

After the UG's first introduction into experimental research by Güth, Schmittberger, and Schwarz (1982), it has been extensively used by economists and psychologists alike (for reviews of the literature on UGs see Camerer & Thaler, 1995; Güth, 1995; Roth, 1995; Thaler, 1988). The reason that the UG has attracted all this interest is the following: On the one hand, proposers usually offer around 40% of the "pie" that has been allocated to them. On the other hand, responders usually reject offers of less than 20% of the "pie". These findings go against the predictions of standard microeconomic theory. According to these predictions, since any positive (i.e. non-zero) offer adds something to the responder's financial assets, the responder should accept it. It follows that the proposer should offer no more than the smallest amount possible (e.g. one penny, or one cent).

Since standard theory cannot account for observed behaviour in the UG, alternative accounts have been put forward. One suggestion is that proposers do not make offers that are too low because they behave according to a social norm of fairness. However, this explanation has been challenged. Research has demonstrated that, rather than being fair, proposers simply try to appear fair to the responders: When perceived fairness considerations are made redundant (for instance, by obliging responders to accept any offer that the proposer makes in a variant of the

UG that is called the “Dictator Game”), proposers’ offers are markedly lower. In other words, proposers try to anticipate the responders’ fairness considerations not because they are altruistically concerned with fairness, but because they try to maximise the chances that their offers will be accepted (for a review of the fairness / perceived fairness accounts see Camerer & Thaler 1995; Thaler, 1988).

Within the same motivational perspective, research has recently looked into anticipated post-game emotional reactions. Looking at the responder’s side, Pillutla and Murnighan (1996) introduced the “wounded spite / pride” model of UG behaviour. The model accommodates the existing behavioural observations by postulating that positive but small offers are rejected by responders who feel angry at the size of the pie that has been allocated to them by the proposer. Pillutla and Murnighan’s (1996) experimental findings support the predictions of the model. Importantly, the evidence that they obtained reveals experienced anger as a better predictor of offer rejection than perceived unfairness of the offer.

Looking at the proposer’s side, Zeelenberg and Beattie (1997; experiments 2a & 2b) investigated post-game regret considerations. According to their rationale, two things can go regrettably wrong for the proposer. The first thing is offering too little and, consequently, receiving a rejection. In this case the proposer obviously regrets offering too little. The second thing is offering too much; receiving an acceptance; but then finding out that the offer that the responder was ready to accept was lower than the offer that the proposer actually made. In this case the proposer regrets offering too much – in other words, the excessive offer is construed as a foregone gain. Zeelenberg and Beattie’s (1997) rationale for the introduction of the latter regret follows research on the “winner’s curse”, where negotiators find out at the end of the negotiation that they have paid more than their opponent’s reservation price (Thaler, 1994).

Zeelenberg and Beattie (1997) suggested that when making an offer the proposer tries to minimise both types of post-game regret that she might experience. In order to empirically test this suggestion, Zeelenberg and Beattie assigned all their participants the role of the proposer and asked them to make an offer to a responder. Before stating their offers, half of the participants were told that at the end of the game they would find out the responders' "minimum acceptable offer" (MAO) – in other words, they could anticipate post-game regret for offering too much. The other half of the participants would not receive any information on their responder's MAO – therefore, for them post-game regret for offering too much was not a possibility. Zeelenberg and Beattie's (1997) reasoning was that participants who knew that they would find out the responder's MAO would anticipate the regret associated with making an unnecessary large offer and that this anticipation would lead to smaller offers. Zeelenberg and Beattie's (1997) empirical findings supported this prediction.

Studies 5 and 6: Aims and hypotheses

To summarise, the regret-minimisation strategy that Zeelenberg and Beattie (1997) postulated implies that proposers generate an affective self-prediction – in other words, they try to anticipate a future emotional state of theirs. This affective prediction subsequently influences their negotiation behaviour. The strategy that underlies the negotiation behaviour of proposers who anticipate post-negotiation feedback on their opponents' MAO is to minimise the chances that they regret an initial allocation decision that was unnecessary large.

In line with Wilson et al.'s (2000) research on focalism, I propose that the observed regret considerations in UG situations can be attributed to focalism. Proposers who face the possibility of regretting a too generous allocation decision in the future focus narrowly on this potential future regret and adopt a regret-

minimising strategy. If this is true, then de-focusing participants from the allocation decision before they make their offers should attenuate the influence of regret-minimisation on participants' offers. The first aim of studies 5 and 6 was to test for this possibility.

The second aim of these studies was to provide a further empirical test of focalism. In their empirical tests of focalism and the de-focusing manipulation, Wilson et al. (2000) asked participants to predict how they would be feeling after the results a football game; after a hypothetical space tragedy; and, finally, after a hypothetical successful military operation of US forces in Iraq. The basic difference between these tasks and an UG negotiation task is that the latter is more controllable than the former. Empirical evidence suggests that task controllability influences the intensity of people's affective reactions to the outcomes of a task. For instance, Zeelenberg, van der Pligt, and de Vries (2000) demonstrated that people feel worse over an unfortunate outcome when the outcome is more attributable to their own behaviours than when the outcome is less attributable to their own behaviours.

Behaviour in an UG is more controllable compared to football games or other "external" events, because the outcome of the task (i.e. acceptance or rejection of the offer) largely depends on the participant's initial decision (i.e. the allocation decision). Therefore, I used the UG paradigm in order to test for the presence of focalism in predictions of post-game affective reactions.

Study 5

I presented participants with a hypothetical UG. All participants were allocated the role of the proposer. Then they made an offer to a hypothetical responder and they stated their confidence that the offer would be accepted. Half of the participants were told that at the end of the game they would find out the

responder's MAO; no reference to the responder's MAO was made to the other half of the participants. In addition to the expectation of the MAO information, half of the participants completed a diary of events that regularly occur to them in a typical day of theirs before making their offers and stating their subjective confidence.

Regarding participants' game behaviour, I anticipated that participants who expected information about the responder's MAO would make lower offers than participants who did not expect such information. Regarding the influence of affective forecasts on participants' game behaviours, I anticipated that the diary completion would attenuate the influence of anticipated post-game information about the responder's MAO on participants' offers. In other words, I anticipated that only focused participants would make regret-minimising offers.

Method

Participants: One hundred and forty-five participants volunteered for the study (mean age = 18.37 years; $SD = 3.00$ years; age data missing from three participants; one hundred and nineteen females; twenty two males; gender data missing from four participants). The majority of participants were recruited during a group laboratory demonstration session and were not paid. Others were recruited via notices in buildings of the University of London and were paid £4 for their participation (in this and other studies).

Variables and Design: The design was a 2*2 completely between-subjects design, with Anticipated Feedback (No-Feedback vs. Feedback) and Diary (No-Diary vs. Diary) as independent variables. There were between 32-41 participants in each condition. The dependent variables were participants' offers to the responders and participants' confidence that responders would accept the offers.

Materials and Procedure: All participants were presented with a short booklet containing manipulations and dependent measures. On the first page of the booklet, participants in both Feedback conditions received the following instructions: *"This questionnaire is actually a simulated paper-and-pencil game, with very simple rules. Two players participate, one of them being the "proposer"; the other one being the "responder". The proposer is given £10 in cash, and what he/she is asked to do is to allocate this money between himself/herself and the responder. Once he/she has made up his/her mind, the proposer makes an offer (i.e. he/she decides what amount he/she is willing to allocate to the responder)".*

Instructions for the "no-feedback" condition went on as follows: *"If the responder agrees with the proposed split, then the two players share the money according to the proposed split, and the game is over. If the responder does not agree with the proposed split, then neither of the two players gets any money at all, and the game is over"*. Instructions for the "feedback" condition went on as follows: *"If the responder agrees with the proposed split, then the two players share the money according to the proposed split, the game is over, and the responder lets the proposer know what is the minimum amount of money he/she would have accepted anyway. If the responder does not agree with the proposed split, then neither of the two players gets any money at all, the game is over, and the responder lets the*

proposer know what is the minimum amount of money he/she would have accepted anyway”.

The last part of the instructions was identical for the two Feedback conditions: *“What you are asked to do for this study is to imagine that you are the “PROPOSER”, and that you have been given £10 to share between yourself and an unknown responder. In the pages that follow, we would like you to say how much money you would be willing to allocate to the responder, if you were to actually play the ultimatum game, and fill in some other relevant scales, as well”.*

On the second page of the booklet, participants in the “diary” condition received the following instructions: *“Now before you make your offer we would like you to take some time and describe a typical day of yours during this term. Describe all kinds of activities in which you are usually involved throughout the day, beginning from what you usually do in the morning and going on till the moment you usually go to bed (e.g. eating, having class, being out with friends, sleeping, etc.). To help you with this task an hourly diary is provided. Fill in the diary following these instructions”.* Participants were provided with a 24-hour diary and they were asked to fill in the slot that appeared next to each hour of the day. Participants in the “no-diary” condition did not perform any filler task.

Finally, all participants went through the last page of the booklet, where they had to write down the amount of money (out of the allocated £10) that they were willing to offer to the responder. Participants also stated their confidence that their offers would be accepted. The confidence item read as follows: *“How likely is it that the responder would accept your offer?”* Participants gave their answers on an 11-point percentage scale, anchored at 0% = he/she would definitely reject it and 100% = he/she would definitely accept it, through the 50% midpoint = acceptance or rejection would be a 50-50 chance.

Results

Participants' offers and confidence judgements can be seen in Table 6.1. The offers and the confidence judgements were submitted to a Feedback*Diary*Judgement mixed ANOVA, with Feedback (No-Feedback vs. Feedback) and Diary (No-Diary vs. Diary) as between-subjects factors and Judgement (Offer vs. Confidence) as a within-subjects factor. The analysis yielded only a main effect of Judgement [$F(1, 141) = 1952.18, p < .001$]. This effect was anticipated and also it is theoretically uninteresting, since offers and confidence were measured on different scales.

Table 6.1. Mean offers (out of £10) and confidence across conditions (*SDs* in parentheses)

	Offers		Confidence	
	No-Feedback	Feedback	No-Feedback	Feedback
No-Diary	4.67 (1.10)	4.33 (1.29)	0.72 (0.22)	0.66 (0.24)
Diary	4.53 (1.20)	4.69 (1.00)	0.68 (0.26)	0.70 (0.22)

Discussion

Study 5 failed to replicate the regret effect that Zeelenberg and Beattie (1997) demonstrated. However, closer inspection of their findings reveals that, whereas anticipated feedback influenced offers, it actually failed to influence participants' anticipated regret if they made a too low offer or participants' anticipated regret if they made a too high offer (Zeelenberg & Beattie, 1997; pp. 70-71). Therefore, it appears that their attribution of the observed influence of anticipated feedback on offers to people's post-game regret considerations was not

empirically warranted. I am not arguing that anticipated regret does not influence UG bargaining behaviour. However, anticipation of feedback on the responder's MAO may not be enough to trigger these regret effects. In addition, since in Study 5 a hypothetical UG was used, the possibility that participants' low involvement with the experimental task contributed towards the null result cannot be excluded.

Since regret effects could not be inferred from participants' self-reported allocation intentions, it follows that Study 5 also failed to provide an empirical test for the presence of focalism in UG bargaining. In order to resolve these inconsistencies, Study 6 was designed to replicate this investigation in a real UG situation. In Study 6, I also took direct measures of participants' predicted and experienced post-game affective reactions, including regret.

Study 6

Participants were presented with two sequential UGs. All participants were allocated the role of the proposer. Then they made an offer to a responder (in reality, the experimenter) and they stated their confidence that the offer would be accepted. Half of the participants were told that at the end of the game they would find out the responder's MAO; no reference to the responder's MAO was made to the other half of the participants. In addition to the expectation of the MAO information, all participants were de-focused before playing the second game. Participants played both games first and then they found out the outcomes (all offers were actually rejected).

Participants were also asked to predict their post-game affective reactions upon receiving the outcomes of the games. As soon as both games were over, participants were asked to record their experienced affective reactions after

receiving the outcomes. Finally, participants were contacted a few days later and they were asked to record their post-game affective reactions again.

Regarding participants' game behaviour, I anticipated that participants who expected information about the responder's MAO would make lower offers than participants who did not expect such information. Regarding the influence of affective forecasts on participants' game behaviours, I anticipated that diary completion would attenuate the influence of anticipated post-game information about the responder's MAO on participants' offers. In other words, I anticipated that only focused participants would make regret-minimising offers. Finally, regarding participants' affective self-predictions, I anticipated that de-focused participants' forecasts would be more accurate than focused participants' forecasts.

Method

Participants: Forty-seven participants volunteered for the study (mean age = 23.68 years; $SD = 4.80$ years; twenty-eight females). Participants were paid £5 for their participation (in this and other studies).

Variables and Design: The design was a 2*2 mixed factorial design, with Anticipated Feedback (No-Feedback vs. Feedback) as a between-subjects factor and Diary (No-Diary vs. Diary) as a within-subjects factor. There were 23-24 participants in each condition.

Regarding the dependent variables of the experiment, for each game participants made an offer to the responder and they also stated their confidence that their offers would be accepted. Participants also predicted their post-game immediate and delayed emotional reactions if their offers were accepted and if their offers were rejected after each game. Participants used the PANAS (Watson et al.,

1988) and the “decision-related affect” items that were introduced in Study 1 to record these predictions. Participants also recorded their confidence that the experienced emotional state would match the predicted one.

Once both games were over and participants knew the outcomes, they reported their experienced emotional reactions (PA; NA; and decision-related affect). Finally, a few days later, participants reported their emotional reactions once again.

Materials and Procedure: All participants were presented with a short booklet containing manipulations and dependent measures. In the first stage of the experiment, participants in the “no-feedback” condition received the following instructions: *“This section of the questionnaire is actually a paper-and-pencil game. The game is very simple: You are playing with another participant of this study, whom you are not supposed to meet at any part of the game. You have been assigned the role of the “proposer”; the other participant has similarly been assigned the role of the “responder”. The proposer is given by the researcher £10 in cash, and what he/she is asked to do is to allocate this money between him/herself and the responder. Once he/she has made up his/her mind, the proposer writes down in the “AMOUNT PROPOSED” space on the questionnaire sheet the amount he/she is willing to allocate to the responder. Right after that, the researcher takes this sheet to the responder, who is waiting in another room. If the responder agrees with the proposed split, then he/she ticks the “I ACCEPT” option on that same sheet. The researcher gets back to the proposer, informs him/her on the outcome of the offer, gives him/her the agreed money, does the same with the responder, and the game is over. If the responder does not agree with the proposed split, then he/she ticks the “I DON’T ACCEPT” option on that same sheet. The researcher gets*

back to the proposer, informs him/her on the outcome of the offer, none of the two players gets any money at all, and the game is over”.

The instructions for the “feedback” condition were identical regarding the description of the game. However, in addition to the acceptance, or rejection of their offers by the responder, participants were also told: *“Whether the responder accepts or rejects the offer, he/she also writes down the amount for which he/she would have accepted the offer anyway in the “MINIMUM AMOUNT ACCEPTED” space on the questionnaire sheet. The researcher gets back to the proposer, informs him/her on the outcome of the offer and the minimum amount that would have been accepted by the responder, and the game is over”.*

Participants were then asked to write down the amount of money (out of the allocated £10) that they were willing to offer to the responder and to state their confidence that their offers would be accepted. The confidence item read as follows: *“How likely is it that the responder accepts your offer?”* Participants gave their answers on an 11-point percentage scale, anchored at 0% = he/she will definitely reject it and 100% = he/she will definitely accept it, through the 50% midpoint = acceptance or rejection is a 50-50 chance.

In the second stage of the experiment, participants were asked to predict how they thought they would be feeling immediately after the game and a couple of days later. Since at this stage participants did not know the outcomes of their offers, they predicted their emotional reactions for both outcome possibilities (i.e. acceptance and rejection of their offers). The instructions were a modified version of the PANAS instructions (Watson et al., 1988; p. 1070) and read as follows: *“The list that follows consists of a number of words that describe different feelings and emotions. Read each item and then mark the appropriate answer to the space next to that word”.* Participants were provided with two time perspectives and two outcome

contingencies. Instructions for each of these combinations read as follows: “[...] *immediately after finding out that your offer is accepted*” (“immediate acceptance”); “[...] *immediately after finding out that your offer is rejected*” (“immediate rejection”); “[...] *a week after finding out that your offer is accepted*” (“delayed acceptance”); and “[...] *a week after finding out that your offer is rejected*” (“delayed rejection”).

In the third stage of the experiment, participants completed some unrelated personality measures. In the fourth stage of the experiment, participants received the same UG instructions and all dependent measures again. This time, before participants made their offers, stated their confidence, predicted their emotional reactions, and stated their confidence in their affective forecasts, they received the following instructions: “*Now before you make your offer we would like you to take some time and describe a typical day of yours during this term. Describe all kinds of activities in which you are usually involved throughout the day, beginning from what you usually do in the morning and going on till the moment you usually go to bed (e.g. eating, having class, being out with friends, sleeping, etc.). To help you with this task an hourly diary is provided. Fill in the diary following these instructions*”. Participants were provided with a 24-hour diary and they had to fill in the slot that appeared next to each hour of the day. In addition to this diary, participants were asked to complete a second 24-hour diary that referred specifically to the day of the experiment.

In the fifth stage of the experiment, participants received the outcomes of the two games. The outcomes were manipulated, so that all participants were told that their offers were rejected¹. “No-feedback” participants were simply told that their

¹ It has been suggested that people dread post-decisional regret and disappointment (i.e. negative emotions) more than they savour post-decisional rejoicing and elation (i.e. positive emotions; e.g. Ritov & Baron, 1995). This is in line with the primary focus of anticipated affect theories on negative

offers had been rejected; “feedback” participants were told that their offers had been rejected and they were also given a MAO that the responder would have accepted. The MAO that was communicated to all participants exceeded their offers by £1. After receiving this information, participants recorded their emotional reactions to the rejection of their offers (PANAS and decision-related affect items).

The sixth stage of the experiment was arranged approximately a week later. Participants came back in the laboratory and reported their emotional reactions (PANAS and decision-related affect items) to the rejections of their offers. Participants were also given the opportunity to compare their experienced with their predicted affective judgements. The comparison item read as follows: *“To what extent do you think that your emotional state now matches the emotional state you initially predicted (i.e. at the first session of this study)?”* Participants gave their answers on an 11-point scale, anchored at -5 = my emotional state now is less intense than I predicted and $+5$ = my emotional state now is more intense than I predicted, through the 0 midpoint = my emotional state now matches perfectly what I predicted.

Results

1. Anticipated emotions and UG behaviour

Participants’ offers and confidence judgements can be seen in Table 6.2. The offers and the confidence judgements were submitted to a Feedback*Diary*Judgement mixed ANOVA, with Feedback (No-Feedback vs. Feedback) as a between-subjects factor and Diary (No-Diary vs. Diary) and Judgement (Offer vs. Confidence) as within-subjects factors. The analysis yielded

emotions rather than on positive ones and also with the concept of loss aversion. The assumption that people are more motivated to to avoid painful experiences than to pursue positive ones provided the rationale for rejecting all the offers in this experiment.

only a main effect of Judgement [$F(1, 45) = 1550.01, p < .001$]. This effect was anticipated and also it is theoretically uninteresting, since offers and confidence were measured on different scales.

Next, I used hierarchical regression modelling in order to predict offers in the two games on the basis of anticipated affective reactions to the outcomes of the games and confidence scores. The 10 positive items and the 10 negative items of the PANAS were separately summed in order to obtain the overall PA and NA scores, respectively. Across the eight predictive judgements (“immediate acceptance”; “immediate rejection”; “delayed acceptance”; and “delayed rejection” across two diary conditions) the PA scale had a mean inter-item reliability of .89, with Cronbach Alpha reliability coefficients ranging from .81 to .93. The NA scale had a mean inter-item reliability of .90, with Cronbach Alpha reliability coefficients ranging from .83 to .98.

Table 6.2. Mean offers (out of £10) and confidence as a function of Feedback and Diary (SDs in parentheses)

	Offers		Confidence	
	No-Feedback	Feedback	No-Feedback	Feedback
No-Diary	4.67 (0.92)	4.85 (0.51)	0.65 (0.17)	0.77 (0.13)
Diary	4.63 (0.77)	4.67 (1.16)	0.59 (0.16)	0.75 (0.14)

For both the “no-diary” and the “diary” game I built two-stage models, with confidence entering in the first stage and anticipated emotional reactions entering in the second stage. All four affective predictions (i.e. “immediate acceptance”; “immediate rejection”; “delayed acceptance”; and “delayed rejection”) of PA, NA,

and decision-related affect were included simultaneously in the second stage regression models.

As can be seen in Table 6.3, the inclusion of post-game affective anticipations improved accuracy of prediction of the offers for the “no-diary” game, but not for the “diary” game. For the “no-diary” game, more disappointment, less PA, and less elation after a possible rejection led participants to make higher offers. In other words, the worse participants thought that they would feel after receiving a rejection, the more they tried to avoid it by making higher offers.

Table 6.3. Regression modelling of offers as a function of confidence and anticipated post-game emotions

Offers	1 st stage model (confidence)	Predictors (b-weights)*	2 nd stage model (confidence & anticipated emotions)	Predictors (b-weights)*	Gain in accuracy
No-Diary UG	$R^2 = .14$; $F(1, 46) = 7.23$, $p = .010$	Confidence (.37)	$R^2 = .76$; $F(25, 21) = 2.65$, $p = .013$	Rejoic.-DR (1.12)** PA-IR (.86) Disappoin.-DR (.63) Confidence (.51) PA-DR (-.85) NA-IR (-.63) Elation-IR (-.53) Regret-IA (-.38)	$R^2 = .62$; $F(24, 21) = 2.26$, $p = .032$
Diary UG	$R^2 = .22$; $F(1, 45) = 12.46$, $p = .001$	Confidence (.47)	$R^2 = .63$; $F(25, 21) = 1.41$, $p = .213$	-----	$R^2 = .41$; $F(24, 21) = .96$, $p = .539$

*All predictors significant at $p < .05$; ** Abbreviations: IR: immediate rejection; IA: immediate acceptance; DR: delayed rejection; DA: delayed acceptance

At the same time, however, more rejoicing, more PA, and less NA after a possible rejection and also less regret after a possible acceptance also led participants to make higher offers. In other words, the better participants thought that they would feel after receiving a rejection and the worse they thought they would feel after receiving an acceptance, the more they tried to avoid rejection by

making higher offers. I come back to this pattern of contradictory findings in the Discussion. However, the most important feature of the results presented in Table 6.3 is that emotions predicted UG offers in the no-diary condition but not in the diary condition.

2. Predicted and experienced positive and negative affect (PANAS)

Across the four experienced judgements (“immediate rejection”; and “delayed rejection” across two diary conditions) the PA scale had a mean inter-item reliability of .92, with Cronbach Alpha reliability coefficients ranging from .90 to .95. The NA scale had a mean inter-tem reliability of .78, with Cronbach Alpha reliability coefficients ranging from .70 to .82.

Since all participants were told that their offers were rejected, in the analyses of emotional reactions I included only predictions about immediate and delayed affect contingent on rejection of the offers. Participants’ predicted and experienced emotional reactions to the outcomes of the two games were submitted to an Affect*Time*Judgement*Diary*Feedback mixed ANOVA, with Feedback (No-Feedback vs. Feedback) as a between-subjects factor and Affect (PA vs. NA), Time (Immediate vs. Delayed), Judgement (Prediction vs. Experience), and Diary (No-Diary vs. Diary) as within-subjects factors.

The analysis yielded three main effects (Affect; Time; and Diary), four two-way interactions (Judgement*Feedback; Time*Affect; Judgement*Affect; and Judgement*Diary), and, to qualify some of the other effects, it also yielded a Judgement*Time*Feedback interaction. Conceptually, these effects can be grouped as follows²:

² It should be noted here that the proposed grouping is *conceptual* in the sense that it does not follow the rigid rationale of the underlying statistical analyses. For instance, Diary and Judgement effects also interact. However, I have chosen to present these effects under separate headings, in order to

1. Effects that involve “Judgement”: They address the question of accuracy of affective judgements.
2. Effects that involve “Diary”: They address the question of de-focusing.
3. Effects that involve “Affect”: They address the differences between PA and NA.

In order to make the analysis easier to follow, I have analysed the interactions reported above on the basis of the above conceptualisation.

2.1. Accuracy of affective judgements: Effects that involve “Judgement”

I split the Judgement*Time*Feedback interaction [$F(1, 42) = 5.68, p = .022$] into two Judgement*Time ANOVAs – one for “feedback” participants and one for “no-feedback” participants. Results for both Feedback conditions can be seen in Table 6.4.

Table 6.4. Mean PA and NA as a function of Feedback, Judgement, and Time (*SDs* in parentheses)

	No Feedback		Feedback	
	Prediction	Experience	Prediction	Experience
Immediate	16.90 (4.29)	15.26 (4.33)	15.12 (2.85)	16.33 (4.23)
Delayed	14.07 (4.55)	14.55 (4.99)	12.92 (2.84)	13.81 (3.87)

For “feedback” participants, the analysis revealed only a main effect of Time [$F(1, 20) = 34.54, p < .001$], such that immediate affective judgements were higher than delayed.

For “no-feedback” participants the analysis revealed a main effect of Time [$F(1, 22) = 10.19, p = .004$] and a main effect of Judgement [$F(1, 22) = 8.56, p =$

highlight their relevance to the hypotheses under empirical test. Certainly other groupings of these results could also be suggested. These comments also apply to Section 3.

.008], both of which were qualified by the Time*Judgement interaction [$F(1, 22) = 6.88, p = .016$]. Whereas “no-feedback” participants overpredicted their immediate affective reactions [$t(23) = 3.11, p = .005$], no evidence of misprediction was obtained for their delayed affective reactions [$t(22) = -1.08, p = .292$].

2.2. De-focusing: Effects that involve “Diary”

Moving on to the Judgement*Diary interaction, [$F(1, 42) = 8.65, p = .005$], whereas participants predicted more intense affective reactions when they were focused than when they were de-focused [$t(46) = 4.02, p < .001$], their experienced affective reactions did not differ as a function of the de-focusing [$t(43) = .75, p = .459$]. These results can be seen in Table 6.5. and they demonstrate that the effect of diary completion is a reduction in the affective forecasts, rather than an influence in the affective experiences.

Table 6.5. Mean PA and NA as a function of Judgement and Diary (*SDs in parentheses*)

	Predictions	Experiences
No-Diary	15.12 (3.54)	14.93 (4.03)
Diary	14.45 (3.75)	15.04 (4.01)

2.3. PA vs. NA: Effects that involve “Affect”

Finally, decomposing of the Judgement*Affect interaction, [$F(1, 42) = 10.28, p = .003$] revealed that, whereas participants overpredicted their NA [$t(43) = -2.11, p = .041$], they underpredicted their corresponding PA [$t(43) = 3.91, p < .001$]. These results can be seen in Table 6.6.

Table 6.6. Mean PA and NA as a function of Judgement (*SDs* in parentheses)

	Predictions	Experiences
PA	15.07 (5.14)	16.56 (6.32)
NA	14.50 (4.53)	13.40 (3.68)

Analysis of the weaker Time*Affect interaction [$F(1, 42) = 3.81, p = .058$] revealed that, whereas participants reported equal levels of PA and NA immediately after the task [$t(46) = 1.38, p = .175$], they reported higher levels of PA than NA later on [$t(43) = -3.11, p = .003$]. These results can be seen in Table 6.7.

Table 6.7. Mean PA and NA as a function of Time (*SDs* in parentheses)

	Immediate	Delayed
PA	16.70 (5.51)	15.14 (5.67)
NA	15.29 (4.55)	12.58 (3.68)

3. Predicted and experienced decision-related affect

As was the case with the PANAS scores, participants' predicted and experienced decision-related affect was submitted to an Affect*Time*Judgement*Diary*Feedback mixed ANOVA, with Feedback (No-Feedback vs. Feedback) as a between-subjects factor and Affect (Regret vs. Disappointment vs. Rejoicing vs. Elation), Time (Immediate vs. Delayed), Judgement (Prediction vs. Experience), and Diary (No-Diary vs. Diary) as within-subjects factors.

The analysis yielded three main effects (Affect; Time; and Diary), six two-way interactions (Judgement*Feedback; Judgement*Time; Judgement*Affect; Judgement*Diary; Time*Diary; and Time*Affect), three three-way interactions

(Judgement*Diary*Feedback; Time*Judgement*Affect; and Time*Diary*Affect), and, to qualify some of the other effects, the analysis also yielded a Judgement*Diary*Affect*Feedback interaction. In what follows, I have analysed these interactions according to the conceptualisation that I introduced for the PA and NA indices. It should be noted, however, that the observed interaction between Judgement and Diary brings the questions of accuracy and de-focusing into a single analysis.

3.1. Accuracy of affective judgements and de-focusing: Effects that involve “Judgement” and “Diary”

I split the Judgement*Diary*Affect*Feedback interaction [$F(2.31, 96.98) = 4.14, p = .014$] in two Judgement*Diary*Affect ANOVAs – one for “feedback” participants and one for “no-feedback” participants.

For “feedback” participants the analysis only revealed a main effect of Affect [$F(1.33, 26.54) = 26.68, p < .001$], such that participants reported more regret and disappointment than rejoicing and elation. For “no-feedback” participants, the analysis revealed three main effects (Judgement; Diary; and Affect), two two-way interactions (Judgement*Diary; and Judgement*Affect), and, to qualify all these effects, the analysis also revealed a Judgement*Diary*Affect interaction [$F(2.36, 51.96) = 4.42, p = .013$]. I split this interaction into four Judgement*Diary ANOVAs – one for each emotion. These results can be seen in Table 6.8.

For regret, the analysis revealed only a Judgement*Diary interaction [$F(1, 22) = 5.74, p = .025$]. Whereas participants overpredicted their regret when they were focused on the game [$t(22) = 2.04, p = .054$], no evidence of misprediction of regret was obtained when participants were de-focused from the game [$t(22) = .46, p = .650$].

For disappointment, the analysis revealed a main effect of Judgement [$F(1, 22) = 8.02, p = .010$], such that participants generally overpredicted their disappointment. However, this effect was qualified by the Judgement*Diary interaction [$F(1, 22) = 9.10, p = .006$]. Whereas participants overpredicted their disappointment when they were focused on the game [$t(22) = 3.95, p = .001$], no evidence of misprediction of disappointment was obtained when participants were de-focused from the game [$t(22) = .79, p = .437$]. So, for negative decision-related affect, effects were as predicted.

For rejoicing and elation, the analysis failed to reveal any effects.

Table 6.8. Mean decision-related affect as a function of Judgement and Diary (SDs in parentheses)

		Predictions	Experiences
Regret	No-Diary	2.04 (1.08)	1.54 (0.69)
	Diary	1.74 (0.95)	1.67 (0.73)
Disappointment	No-Diary	3.09 (0.72)	2.30 (0.77)
	Diary	2.52 (0.86)	2.37 (0.88)
Rejoicing	No-Diary	1.33 (0.82)	1.28 (0.56)
	Diary	1.24 (0.54)	1.26 (0.56)
Elation	No-Diary	1.25 (0.59)	1.23 (0.52)
	Diary	1.21 (0.58)	1.19 (0.51)

3.2. More effects that involve "Diary"

I split the Time*Diary*Affect interaction [$F(2.61, 109.54) = 5.32, p = .003$] into four Time*Diary ANOVAs – one for each emotion. These results can be seen in Table 6.9.

For regret, the analysis revealed a main effect of Time [$F(1, 43) = 6.93, p = .012$], such that participants reported more regret immediately after the games than

later on. This effect was qualified by the weak Time*Diary interaction [$F(1, 43) = 3.67, p = .062$]. Whereas participants reported more immediate than delayed regret when they were focused on the game [$t(43) = 3.01, p = .004$], they reported equal immediate and delayed regret when they were de-focused from the game [$t(43) = 1.55, p = .129$].

For disappointment, the analysis revealed a main effect of Time [$F(1, 43) = 90.56, p < .001$], such that participants reported more disappointment immediately after the games than later on. The analysis also revealed a main effect of Diary [$F(1, 43) = 5.49, p = .024$], such that participants reported more disappointment when they were focused on the games than when they were de-focused from the games. However, both effects were qualified by the Time*Diary interaction [$F(1, 43) = 15.50, p < .001$]. Participants reported more immediate disappointment when they were focused on the game than when they were de-focused from it [$t(46) = 3.56, p = .001$]. De-focusing did not have any effect on judgements of delayed disappointment [$t(43) = .76, p = .452$].

For rejoicing and elation, the analysis failed to reveal any effects.

Table 6.9. Mean decision-related affect as a function of Time and Diary (*SDs in parentheses*)

		Immediate	Delayed
Regret	No-Diary	2.07 (0.99)	1.76 (0.68)
	Diary	1.93 (1.06)	1.80 (0.86)
Disappointment	No-Diary	3.23 (0.91)	2.06 (0.78)
	Diary	2.86 (1.04)	1.99 (0.75)
Rejoicing	No-Diary	1.19 (0.49)	1.24 (0.60)
	Diary	1.17 (0.46)	1.22 (0.55)
Elation	No-Diary	1.20 (0.50)	1.19 (0.50)
	Diary	1.18 (0.52)	1.16 (0.43)

3.3. Regret vs. Disappointment vs. Rejoicing vs. Elation: Effects that involve “Affect”

Finally, I split the weak Time*Judgement*Affect interaction [$F(2.23, 93.68) = 2.62, p = .072$] into four Time*Judgement ANOVAs – one for each emotion. These results can be seen in Table 6.10.

For regret, the analysis revealed a main effect of Time; this effect has been described above. This effect was qualified by the Time*Judgement interaction [$F(1, 43) = 6.50, p = .014$]. Whereas participants overpredicted their immediate regret [$t(46) = 2.39, p = .021$], no evidence of misprediction of delayed regret was obtained [$t(43) = -.28, p = .778$].

Table 6.10. Mean decision-related affect as a function of Judgement and Time (SDs in parentheses)

		Immediate	Delayed
Regret	Predictions	2.19 (1.11)	1.76 (0.74)
	Experiences	1.81 (1.16)	1.80 (0.92)
Disappointment	Predictions	3.24 (0.96)	2.11 (0.83)
	Experiences	2.85 (1.05)	1.93 (0.85)
Rejoicing	Predictions	1.14 (0.39)	1.19 (0.59)
	Experiences	1.23 (0.60)	1.26 (0.62)
Elation	Predictions	1.16 (0.46)	1.14 (0.48)
	Experiences	1.22 (0.56)	1.21 (0.52)

For disappointment, the analysis revealed a main effect of Time; this effect has also been described above. The analysis also revealed a main effect of Judgement [$F(1, 43) = 6.55, p = .014$], such that participants generally overpredicted their disappointment.

For rejoicing and elation, the analysis failed to reveal any effects.

4. Post-game emotional experiences

In order to further investigate the determinants of participants' post-game experienced affect, I first multiplied participants' offers by their corresponding confidence judgements that the offers would be accepted. I used these "weighted offers" (i.e. rather than the initial unweighted offers) in the analyses of experienced affective reactions. My rationale for doing this was that people's affective reactions to behavioural outcomes have been shown to depend on a.) the objective quality of the outcomes and b.) people's prior expectations regarding the likelihood of occurrence of possible outcomes. For instance, people who are holding high expectations regarding an upcoming outcome are more disappointed when they realise that the outcome falls below their initial expectations than people who are not holding any expectations at all (e.g. Klaaren, Hodges, & Wilson, 1994; for a review, see Wilson & Klaaren, 1992).

Applying this rationale to the UG behaviour under investigation, I reasoned that the more confident participants were about their offers, the worse they would feel as soon as they received the rejections. For instance, a rejection of a £4-offer should be more aversive for someone who was 80% confident that the offer would be accepted (weighted offer: £3.20) than for someone who was 60% confident that the offer would be accepted (weighted offer: £2.40).

I calculated correlations between participants' weighted offers and their immediate and delayed affective experiences. For the "no-diary" game, higher weighted offers were associated with lower delayed PA (Pearson $r = -.30$, $p = .048$) and lower delayed rejoicing (Pearson $r = -.29$, $p = .053$). For the "diary" game, higher weighted offers were associated with lower delayed regret (Pearson $r = -.39$, $p = .009$). I shall come back to this apparent shift in the influence of weighted offers on affective experiences in the Discussion.

5. Post-game comparisons of affective predictions

Participants had the opportunity to compare their pre-game emotional anticipations to their post-game emotional experiences with the following item: “*To what extent do you think that your emotional state now matches the one you predicted?*” Participants responded on a -5 (experienced less intense than predicted) to +5 (experienced more intense than predicted) scale. The mean response was -.86. This value is significantly lower than zero [$t(43) = -3.44, p < .001$], thereby indicating that participants experienced less intense emotions than their predictions.

Affective comparisons did not vary as a function of Feedback [$t(37.49) = -.95, p = .349$]. Moreover, correlations between comparisons, weighted offers, and confidence in affective predictions were neither significant, nor sizeable (all Pearson r s $< .23$). Finally, since participants overall overpredicted their post-game affective reactions and the mean comparison was downward, it appears that the comparisons were in the correct direction. This finding can be taken to indicate that participants had some insight into their affective experience. However, when I divided participants in those whose comparisons were appropriate (i.e. negative value; $n = 26$) and those whose comparisons were not (i.e. positive value or zero; $n = 18$), a chi-square failed to reveal any reliable difference between the number of participants in the two groups [$\chi^2(1) = 1.46, p = .228$]. In other words, I did not obtain any reliable evidence that participants’ affective comparisons were more likely to be appropriate than inappropriate.

Discussion

Study 6 was designed to test the following three hypotheses:

Hypothesis a.) Participants who expected information on the responder’s MAO would make lower offers than participants who did not expect such information.

Hypothesis b.) Diary completion would make participants' affective forecasts more accurate.

Hypothesis c.) Diary completion would attenuate the influence of anticipated post-game information on the responder's MAO on participants' offers.

In what follows, first, I discuss the findings of Study 6 that are related to each one of the above hypotheses. Second, I elaborate on the determinants of participants' emotional experiences.

Hypothesis a: Anticipated emotions and game behaviour

Study 6 showed that anticipated post-game emotions influence people's negotiation behaviours. However, the pattern of findings that emerged from Study 6 was more complicated than anticipated. On the one hand, participants made higher offers when they associated higher levels of disappointment and lower levels of positive affect with a rejected offer. The emerging factor here appears to be avoidance of painful rejections. This motivation reflects Zeelenberg and Beattie's (1997) idea that people in the UG situation try to minimise their regrets about offering too little (and having their offers rejected because of this). Moreover, participants made lower offers when they associated higher levels of regret with an accepted offer. The emerging factor here appears to be avoidance of painful acceptances. This motivation reflects Zeelenberg and Beattie's (1997) idea that when people in the UG situation try to minimise their regrets about offering too much.

On the other hand, participants made higher offers when they associated higher levels of rejoicing and positive affect and lower levels of negative affect and elation with a rejected offer. At first glance, it is rather hard to discern participants' considerations here – actually, these findings seem to directly contradict what I have

said so far. However, a recent development in the theoretical conceptualisation of post-decisional emotions could provide an account of such considerations.

The characterisation of a negotiation decision (or of any decision) as “good” or “bad” or “regrettable” involves a judgement about two different aspects of the decision: The decision outcome and the decision process. In other words, both good outcomes and regrettable outcomes can stem from conscious and effortful deliberation or from pure luck. Schematically then, the following four possibilities are open:

- a. Good process → good outcome
- b. Good process → regrettable outcome
- c. Regrettable process → good outcome
- d. Regrettable process → regrettable outcome

Connolly and Zeelenberg (2002) recently accommodated these possibilities within Decision Justification Theory (DJT; see also Connolly & Reb, 2003; Inman & Zeelenberg, 2002; Pieters & Zeelenberg, 2003). DJT explicitly discriminates between “outcome regret” and “decision regret”. The former regret stems from unfortunate outcomes, which did not necessarily result from bad decisions. The latter regret stems from bad decisions, which did not necessarily result in unfortunate outcomes.

I suggest, therefore, that participants who associated higher levels of rejoicing and positive affect and lower levels of negative affect and elation with a rejected offer reported their negotiation behaviour-related emotions, rather than their negotiation outcome-related emotions. At this stage and without additional empirical results this explanation cannot be but tentative. However, Wright, Ayton, and Djemal’s (2003) recent finding that people tend to infer the quality of a decision

process by the quality of the outcomes of that process provide some indirect empirical support for this argument.

Hypotheses b and c: Focalism, the accuracy of affective anticipations, and their influence on game behaviour

Whereas participants who were narrowly focused on the allocation decision generally overpredicted their post-game affective reactions, de-focused participants' affective predictions were in line with their post-game affective experiences. This finding, on the one hand, replicates Wilson et al.'s (2000) findings regarding the presence of focalism in prospective judgements of affect and, on the other hand, it extends Wilson et al.'s (2000) documentation of focalism to a controllable situation.

Not only were affective anticipations more accurate when participants were de-focused from the allocation decision, they also had less influence on participants' game behaviour. When participants were de-focused from the allocation decision, the influence of the emotional considerations on their offers was eliminated. The only predictor of participants' game behaviour was their subjective confidence that their offers would be accepted. This finding reflects people's motivation to maximise their chances of making some profit out of the negotiation. Moreover, the motivation to maximise profit was present even when the affective anticipations were salient (i.e. when participants were focused on the allocation decision).

At a more conceptual level, the moderating influence of the de-focusing manipulation on affective predictions seems to be the route via which de-focusing makes game behaviour relatively "immune" to anticipated post-game emotions. Schematically, the de-focusing from the allocation decision leads participants to construe the emotional impact of the game as more moderate than they would have

construed it had they been focused on the game. Once the affective anticipations have been attenuated, they lose their influence on people's negotiation behaviours.

Surprise effects and participants' emotional experiences

Surprise effects were present in participants' experienced emotional reactions. Regarding focused participants' emotions, those of them who were more confident that their offers would be accepted experienced less positive affect and less rejoicing when they found out that their offers had actually been rejected than participants who held lower expectations. This finding provides further empirical evidence for the assumption of DT and DAT – SEP that people feel better or worse when they receive the outcomes of their decisions depending on the level of their pre-decisional expectations about these outcomes.

Regarding de-focused participants' emotions, those of them who were more confident that their offers would be accepted experienced less regret when they found out that their offers had actually been rejected than participants who held lower expectations. The “behaviour-related emotions” versus “outcomes-related emotions” distinction that I introduced earlier seems to be applicable to this finding too. Participants who were more confident that they made a reasonable offer to the responder (i.e. an offer that they judged more likely to be accepted than to be rejected) had fewer regrets after receiving a rejection than participants who were less confident that they made a reasonable offer.

I take the fact that this shift from “outcomes-related emotions” to “behaviour-related emotions” was brought about by the de-focusing manipulation as indirect evidence for the relevance of the DJT for the findings. In agreement with the essence of de-focusing, de-focused negotiators appear to see beyond the immediate impact of negative negotiation outcomes. What determines their post-

negotiation pleasure is the quality of their negotiation behaviour – which does not fully determine negotiation outcomes. As was the case with the DJT conjectures in relation to the influence of anticipated emotions on game behaviour, this explanation remains tentative until direct further empirical tests are undertaken.

Finally, regarding people's self-insight into their affective experiences the findings of Study 6 were mixed. On the one hand, participants generally viewed their affective predictions too intense, as they should have done, since they overpredicted their post-game affective reactions. On the other hand, closer analysis of individual participants' responses revealed that they were not more likely to make the appropriate comparison than an inappropriate one.

General Discussion

Overall, the research reported in this chapter provides empirical evidence for the presence of focalism in negotiation decisions. Specifically, whereas participants were heavily influenced by their post-negotiation affective anticipations when they were focused on the outcomes of the negotiation, they were less influenced by these anticipations when they saw the outcomes of the negotiation in a broader, real-life frame (Study 6). Moreover, de-focused participants' predictions of post-negotiation affect were more accurate than the corresponding predictions of focused participants (Study 6). Taken together, these findings seem to suggest that affective forecasts, in general, are influential precisely because they are exaggerated.

Moreover, neither the anticipation of post-negotiation feedback, nor the de-focusing manipulation had a direct effect on participants' observed negotiation behaviour (Study 5; Study 6). It seems reasonable, therefore, to suggest that, whereas affective anticipations (when they are overpredictions) are instrumental in shaping people's negotiation behaviour, the anticipation of post-negotiation

feedback on the responder's intended behaviour and the de-focusing manipulation do not influence behaviour directly, but only through their influence on affective anticipations.

Finally, regarding participants' experienced affective reactions, this research provides evidence that they are susceptible to surprise effects. Failures that were more unexpected were more aversive than those that were less unexpected (Study 6).

This research also documented some effects that were not anticipated. Participants associated reasonable negotiation behaviours with higher levels of post-negotiation pleasure, even after a possible failure of the negotiation (Study 6). In addition, participants were actually happier when negotiation failures came after more reasonable behaviours than when they came after less reasonable behaviours (Study 6). I tentatively interpret these findings according to the model of dual affective evaluations that DJT suggests. People associate reasonable (i.e. good or otherwise justifiable) behaviours with more post-behaviour pleasure and unreasonable (i.e. bad or otherwise unjustifiable) behaviours with more post-behaviour pain – independently of the quality of the behavioural outcomes per se.

Taken together with the shift in focus from outcome-related emotions to behaviour-related emotions that was observed for de-focused participants (Study 6), these findings point towards a possibility of integrating DJT and the focalism mechanism. The hypothesis is the following: In situations where the perceived behavioural control is high, people who are narrowly focused on a decision are mostly influenced by their anticipated affective reactions to the decision outcomes, whereas people who adopt a wider frame of reference for the decision in question are mostly influenced by their anticipated affective reactions to the decision process. This hypothesis contrasts the temporary character of outcomes with the more

permanent character of decision processes and sees decision-makers as adopting one of two strategies: Either focusing on the “next day” of a particular decision (when outcomes become known and profits, regrets, etc. materialise) or focusing on the “next similar decision” (when a decision situation arises that demands a decision process that has been judged effective in the past irrespective of its interaction with external contingencies that determined the outcomes). This hypothesis remains to be empirically tested.

CHAPTER

7

Anticipated post-decisional emotional reactions in

a naturalistic setting:

An instance of focalism?

The accuracy of affective self-forecasts

Empirical research has documented evidence that people are inaccurate when they predict their future emotions. For instance, Mitchell et al. (1997) found that people overpredict how happy they will be during a vacation or when they go on a trip. Sieff et al. (1999) found that people overpredict how distressed they will be if they receive positive HIV tests results or how relieved they will be if they receive negative HIV test results. Mellers (2000) showed that people overpredict how distressed they will be if they receive (undesirable) positive results at a pregnancy test or negative feedback for their progress on a dieting programme or, conversely, how happy they will be if they receive negative results at the pregnancy test or positive feedback for their progress on the dieting programme. Wilson et al. (2000; study 3) demonstrated that people overpredict how happy they will be if their favourite football team wins an important game or how unhappy they will be if their team loses the game. Finally, Buehler and McFarland (2001) found that people overpredict how happy they will be if they receive unexpectedly high grades or how unhappy they will be if they receive unexpectedly low ones. Overall, the flavour of the research on “affective self-forecasting” is that people are inaccurate when they predict what their affective reactions to future events will be.

Further empirical research has documented focalism as one of the underlying mechanisms that contributes to affective mispredictions (see Chapter 2). In their discussion of the overpredictions of affect that they documented, Wilson and his colleagues suggested that when people generate predictions about how they will be feeling after a future event “...people focus too much on the event in question and not enough on the consequences of other future events” (Wilson et al., 2000; p. 821). The pattern of findings that Wilson and his colleagues obtained in their

experimental studies provided some initial empirical evidence supportive of the focalism account.

Working independently, Schkade and Kahneman (1998) also found empirical evidence supportive of the “focalism” account. Schkade and Kahneman (1998) attributed self-other discrepancies in judgements of general happiness in a mechanism that they called “focusing illusion”. In their words:

“When a judgment about an entire object or category is made with attention focused on a subset of that category, a focusing illusion is likely to occur, whereby the attended subset is overweighted relative to the unattended subset” (Schkade & Kahneman, 1998; p. 340).

The observation that Midwesterner participants judged Californians as happier than themselves (whereas Californian participants did not actually judge themselves as happier than Midwesterners) was attributed by Schkade and Kahneman (1998) to this “focusing illusion”. The attended category in this case was the weather – generally better in California, than in the Midwest.

Study 7: Aims and hypotheses

To summarise, focalism has been empirically documented both in predictive judgements of general happiness about one’s self (Wilson et al., 2000) and also in concurrent judgements of general happiness about others (Schkade & Kahneman, 1998). Study 7 was aimed as a further empirical test of focalism in controllable and natural circumstances. In this respect, Study 7 is an extension of Studies 1 and 6.

In Study 6, participants did two dyadic negotiation tasks, the outcomes of which they thought they were mostly in control of and they predicted their post-negotiation emotions. Before participants predicted their post-negotiation emotions for the second task they were de-focused from the outcomes of the negotiation.

Therefore, Study 6 was an empirical investigation of focalism in circumstances assumed to be controllable. These circumstances were laboratory-based: Participants negotiated in the laboratory environment; the feedback that they received was manipulated by the experimenter; and, finally, the negotiation task itself (i.e. the UG) is a typical laboratory-task. Therefore, all the criticisms regarding the generalisability of the findings of such experimental demonstrations that I mentioned in Chapter 4 apply here equally.

In Study 1, I investigated the accuracy of affective predictions and their influence on binary choice behaviour. The research setting was laboratory-based too. However, it involved a.) participants' evaluation and use of their skilled performance and b.) self-relevant performance feedback for the participants. Since these two features of the research setting of Study 1 represent features commonly present in real-life choice circumstances, they address to some extent the criticism of lack of generalisability that has been raised against the typical gamble tasks that have been used so far.

Combining features of Studies 1 and 6, I designed Study 7. In this study, I tested for the presence of focalism in predictions of affective reactions regarding results at an academic assignment. As was the case with the UGs that I used in Study 6, I consider class assignments more controllable than football games or other "external" events, because the outcome of the task (i.e. obtained marks) is largely a function of the participants' effort. I approached students and asked them to predict how they thought they would be feeling after receiving marks on assignments that they had to hand in for their seminar and laboratory classes. When students had received feedback on their work, I approached them again and asked them to report how they were actually feeling about their performance.

Regarding students' affective predictions, in line with the research that I summarised above, I anticipated that students would overpredict both their pleasure (if they did well) and their pain (if they did poorly). In addition, in line with Wilson et al.'s (2000) research on focalism, I propose that, if students are indeed inaccurate, these inaccuracies can be attributed to focalism. Students who are predicting what their affective reactions to their marks will be are narrowly focusing on receiving the marks and they neglect other occurrences that will occupy their minds at that time. If this is true, then if students are de-focused from receiving the marks before they make their affective predictions, these predictions should be better calibrated. The aim of study 7 was to test for this possibility.

Method

Participants: Thirty-nine participants from seminar and laboratory classes volunteered for the study (mean age = 19.65 years; $SD = 1.50$ years; age data missing from 16 participants; twenty-seven females; gender data missing for three participants). The majority of participants were not paid for the participation; some participants received £4 for participating in the study.

Variables and Design: The design was a two independent groups design, with Diary (No-Diary vs. Diary) as the independent variable. There were 12-27 participants in each condition.

Regarding the dependent variables of the experiment, participants predicted their performance and stated their confidence in their performance prediction. Participants also predicted what their emotional reactions would be when they received feedback on their performance. Participants used the PANAS (Watson et

al., 1988) and the decision-related affect items that I introduced in Study 1 to record these predictions. Participants also predicted the duration of their affective reactions.

When participants had received feedback on their assignments, I recorded their actual performance and their experienced emotions. Finally, some participants also recorded their experienced emotions a week later.

Materials and Procedure: All participants were presented with a short booklet containing manipulations and dependent measures. In the first stage of the study, all participants were told that the study investigated “*people’s perceptions of and reactions to everyday situations*” and that the situation that was selected for them was coursework writing. It was stressed to participants that study itself was completely unrelated to their seminar or laboratory classes and to the class assignments that they had to submit for these classes. In addition, the study was anonymous. Participants were assigned a code (that they worked out themselves, rather than the code been given by the experimenter) in order for the experimenter to be able to match their responses across the different stages of the study.

Once participants had been given this information, they were asked to make their predictions. Regarding their performance, participants predicted the mark that they would obtain for a particular piece of coursework that they were about to submit and they also stated their confidence in this prediction. The confidence item read as follows: “*What do you think is the probability (0% – 100%) that the mark you actually obtain falls within 5 marks either way of the mark you just now predicted (e.g. if you predicted 50, then: 45 ← 50 → 55)?*” Participants gave their answers on an 11-point percentage scale, anchored at 0% = it will definitely not fall within 5 marks either way and 100% = it will definitely fall within 5 marks either

way, through the 50% midpoint = falling within 5 marks either way is a 50-50 chance.

Regarding their post-feedback emotions, participants predicted what they thought their affective reactions would be after receiving the mark for their coursework. Since at this stage participants did not know what their actual marks would be, they predicted their emotional reactions for both lower than expected and higher than expected marks (i.e. negative and positive surprises). Participants were given five possibilities: They predicted how they would be feeling if their actual performance matched their estimated one; if their actual performance exceeded their estimated one by 5 marks; if their actual performance exceeded their estimated one by 10 marks; if their actual performance was lower than their estimated one by 5 marks; and if their actual performance was lower than their estimated one by 10 marks. The instructions were a modified version of the PANAS instructions (Watson et al., 1988; p. 1070) and read as follows: *“The list that follows consists of a number of words that describe different feelings and emotions. Read each item and then mark the appropriate answer in the space next to that word. Indicate to what extent you will be feeling this way immediately after being given your mark for this essay/lab-report”*. Instructions for each of the five possibilities outlined above read as follows: *“[...] if obtained mark equals predicted”* (“accurate prediction”); *“[...] if obtained mark is 5 points lower than predicted”* (“small over-prediction”); *“[...] if obtained mark is 10 points lower than predicted”* (“large over-prediction”); *“[...] if obtained mark is 5 points higher than predicted”* (“small under-prediction”); and *“[...] if obtained mark is 10 points higher than predicted”* (“large under-prediction”).

Participants also predicted the duration of their affective reactions after receiving the marks. The duration item read as follows: *“In the above list you just*

now described your emotional state as soon as you find out the mark for your essay/lab-report. For how long do you think you will be feeling this way after being announced your mark for your essay/lab-report? Give a rough estimate of the duration of your emotional reaction by filling in the appropriate number next to the word that best describes the duration of your feelings (e.g. 10 minutes, or 10 days, or 10 weeks, etc.). Again keep in mind that there are no right or wrong answers". Participants were provided with four duration items ("minutes"; "days"; "weeks"; and "months") with a space next to each one of them and they were instructed to fill in one of these spaces with their best estimate.

The only difference between the "diary" and "no-diary" participants was that before making the above predictions, participants in the "diary" condition received the following instructions: *"Now we would like you to take some time and describe a typical day of yours during this term. Describe all kinds of activities in which you are usually involved throughout the day, beginning from what you usually do in the morning and going on till the moment you usually go to bed (e.g. eating, having class, being out with friends, sleeping, etc.). To help you with this task an hourly diary is provided. Fill in the diary following these instructions"*. Participants were provided with a 24-hour diary and they had to fill in the slot that appeared next to each hour of the day. Participants in the "no-diary" condition did not perform any filler task.

The second stage of the study was arranged after participants received their marks. Participants reported their emotional reactions (PANAS and decision-related affect items) to the marks that they had received. Participants were also given the opportunity to compare their experienced with their predicted affective judgements. The comparison item read as follows: *"To what extent do you think that your emotional state now matches the emotional state you initially predicted (i.e. at the*

first session of this study)?” Participants gave their answers on an 11-point scale, anchored at -5 = my emotional state now is less intense than I predicted and $+5$ = my emotional state now is more intense than I predicted, through the 0 midpoint = my emotional state now matches perfectly what I predicted.

Finally, 14 “no diary” participants went through a third stage of the study. These participants were approached again approximately a week after they had received their marks and they were asked to record their emotional reactions once again.

Analysis: Since I had no way of knowing how well participants would do at the coursework task, the major methodological issue in this study was how to match the affective predictions with the predictions of performance. My approach was the following: I took a performance prediction to be accurate if the actual mark fell between zero and four marks (either way) of the predicted mark (i.e. a zero to four marks discrepancy). I took a prediction to be a “small under- / over-prediction” if the actual mark fell between five and nine marks (either way) of the predicted mark (i.e. a five to nine marks discrepancy). Finally, I took a prediction to be a “large under- / over-prediction” if the actual mark fell 10 marks or more and beyond (either way) the predicted mark (i.e. a ten marks and beyond discrepancy).

On the basis of this criterion, I chose which one of the five affective predictions would be the prediction to which the affective experience would be compared. When students received the marks, I calculated the deviation between expected and actual marks. Mark discrepancies between zero and four marks were matched with affective predictions for “accurate predictions” of performance; mark discrepancies between five and nine marks were matched with affective predictions for “small under- / over-prediction” of performance; and mark discrepancies of ten

marks or more were matched with affective predictions for “large under- / over-predictions” of performance. For instance, if a student had predicted that she would obtain 55% and she actually obtained 61%, I compared her experienced emotions against what she thought that she would be feeling “[...] if obtained mark is 5 points higher than predicted” – in other words, I considered her prediction a “small under-prediction”.

Results

1. Errors in performance prediction and confidence

Table 7.1 depicts two error indices: An “absolute error index” (AEI) and a “signed error index” (SEI). By subtracting participants’ absolute predicted performance from their absolute actual performance, I obtained the AEI. A one-sample *t*-test on this index revealed that participants indeed mispredicted their performance [mean AEI = 7.74; *SD* = 5.00; $t(33) = 9.02$, $p < .001$]. The diary manipulation did not have any effect on AEI [$t(32) = 1.41$, $p = .171$].

Likewise, by subtracting participants’ predicted performance from their actual performance, I obtained the SEI. A one-sample *t*-test on this index revealed that participants indeed underpredicted their performance [mean SEI = 3.44; *SD* = 8.63; $t(33) = 2.33$, $p = .026$]. As was the case with the AEI, the diary manipulation did not have any effect on SEI [$t(32) = .23$, $p = .817$].

Table 7.1. Errors in performance prediction and confidence across Diary conditions (*SDs* in parentheses)

	AEI	SEI	Confidence
No-Diary	8.50 (4.62)	3.67 (9.09)	0.69 (0.19)
Diary	5.90 (5.65)	2.90 (7.82)	0.68 (0.22)

Moreover, when I divided participants into those who predicted their performance accurately ($N = 9$), those who underpredicted it ($N = 19$), and those who overpredicted it ($N = 6$), I found that participants were more likely to underpredict their performance than overpredict it or predict it accurately [$\chi^2(2) = 8.18, p = .017$]. Therefore, performance underpredictions were a general trend rather than being due to individual differences in predictive accuracy (and to the resulting outlying error scores that such individual differences would entail).

2. Predicted and experienced positive and negative affect and decision-related emotions

The 10 positive items, and then 10 negative items of the PANAS were separately summed in order to obtain the overall PA and NA scores, respectively. Across the two judgements (one predictive and one experienced) the PA scale had a mean inter-item reliability (Cronbach Alpha) of .91. The NA scale had an inter-item reliability (Cronbach Alpha) of .89.

Participants' predicted and experienced PA and NA can be seen in Table 7.2. I compared affective predictions and experiences separately for "no-diary" and "diary" participants. Whereas "no-diary" participants overpredicted the PA that they experienced when they received their marks [$t(22) = 2.60, p = .016$], there was no misprediction for "diary" participants [$t(9) = -.47, p = .649$]. A similar analysis failed to reveal any evidence of misprediction of participants' NA in either diary condition ["no-diary": $t(22) = -1.03, p = .313$; "diary": $t(9) = -.10, p = .924$].

Table 7.2. Predicted and experienced PA and NA across Diary conditions (SDs in parentheses)

	No-Diary		Diary	
	Predictions	Experiences	Predictions	Experiences
PA	27.70 (11.46)	22.13 (8.06)	19.80 (5.69)	20.60 (7.44)
NA	13.48 (5.95)	14.57 (5.01)	13.90 (5.22)	14.10 (5.45)

Participants' predicted and experienced regret, disappointment, rejoicing, and elation can be seen in Table 7.3. As with the general indices of PA and NA, I compared affective predictions and experiences separately for "no-diary" and "diary" participants. Whereas "no-diary" participants overpredicted the rejoicing and elation that they experienced when they received their marks [for rejoicing: $t(22) = 4.49$, $p < .001$; for elation: $t(22) = 3.23$, $p = .004$], there was no mispredictions for "diary" participants [for rejoicing: $t(9) = 1.00$, $p = .343$; for elation: $t(9) = .69$, $p = .509$]. A similar analysis failed to reveal any evidence of misprediction of participants' regret or disappointment in either diary condition ["no-diary"; regret: $t(22) = -1.82$, $p = .083$; disappointment: $t(22) = -.97$, $p = .342$; "diary"; regret: $t(9) = .19$, $p = .853$; disappointment: $t(9) = 1.18$, $p = .269$]. Taken together, these analyses demonstrate that diary completion renders affective forecasts (of positive emotions) more accurate.

3. Post-feedback emotional experiences

In order to investigate the determinants of participants' post-feedback experienced affect, I first multiplied participants' estimated performance by their corresponding confidence judgements that this would be their actual performance. As was the case in Study 6, I used these "weighted predictions" (i.e. rather than the initial unweighted performance estimates) in the analyses of experienced affective

reactions. The rationale for doing this was introduced in detail in the relevant section of the results of Study 6 (p. 132), therefore it will not be repeated here.

Table 7.3. Predicted and experienced decision-related affect across Diary conditions (SDs in parentheses)

	No-Diary		Diary	
	Predictions	Experiences	Predictions	Experiences
Regret	1.34 (0.71)	1.74 (0.92)	1.80 (1.32)	1.70 (0.82)
Disappointment	1.65 (1.11)	1.91 (1.16)	2.10 (1.37)	1.70 (0.68)
Rejoicing	2.87 (1.60)	1.91 (1.04)	1.90 (1.52)	1.50 (0.97)
Elation	2.70 (1.46)	1.83 (0.83)	1.80 (1.48)	1.60 (0.97)

I calculated correlations between “no-diary” participants’ weighted performance estimates and their immediate affective experiences and found that higher weighted performance estimates were associated with lower experienced NA (Pearson $r = -.42$, $p = .046$). In other words, the more confident participants were in their (low) performance forecasts, the less pain they experienced when they received their marks.

4. Predicted duration of affective reactions and comparisons of affective predictions

Participants had the opportunity to compare their initial emotional anticipations to their post-feedback emotional experiences with the following item: “To what extent do you think that your emotional state now matches the emotional state you initially predicted (i.e. at the first session of this study)?” Participants responded on a –5 (experienced less intense than predicted) to +5 (experienced more

intense than predicted) scale. The mean response was $-.79$ ($SD = 1.73$). This value was significantly lower than zero [$t(32) = -2.62, p = .013$], thereby indicating that participants experienced less intense emotions than their predictions.

Affective comparisons did not vary as a function of Diary [$t(31) = .24, p = .810$], or Assessment [$F(2, 30) = .95, p = .399$]. Moreover, there was no association between comparisons and weighted performance estimates. Finally, since “no-diary” participants over-predicted their post-feedback pleasure and the mean comparison was downward, it appears that the comparisons were broadly appropriate. This finding can be taken to indicate that participants had some insight into their affective experience. However, when I divided “no-diary” participants ($N = 23$) into those who made appropriate comparisons (i.e. negative; $N = 13$) and those who made inappropriate ones (i.e. positive or zero; $N = 10$) a chi-square failed to reveal a reliable difference between the number of participants in the two groups [$\chi^2(1) = .39, p = .532$]. In other words, there was no firm evidence that participants were more likely to make affective comparisons appropriately than inappropriately.

4. Affective reactions across time

Fourteen “no-diary” participants completed the third stage of the study. In addition to the first two assessments, these participants also reported their emotions one week after they had received their marks. Again, the 10 positive items and the 10 negative items of the PANAS were separately summed in order to obtain the overall PA and NA scores. Across the three judgements that these participants generated (one predictive and two experienced) the PA scale had a mean inter-item reliability of .92, with Cronbach Alpha reliability coefficients ranging from .89 to .96. The NA scale had an inter-item reliability of .80 with Cronbach Alpha reliability coefficients ranging from .68 to .87.

Predicted and experienced PA and NA across the three assessments can be seen in Table 7.4. These judgements were submitted to an Affect*Time ANOVA, with Affect (PA vs. NA) and Time (Predictions vs. Immediate experiences vs. Delayed experiences) as within-subjects factors.

The analysis yielded a main effect of Affect [$F(1, 13) = 18.23, p = .001$], such that participants reported more PA than NA. However, this effect was qualified by the Affect*Time interaction that the analysis also revealed [$F(1.21, 15.68) = 6.81, p = .016$]. A trend analysis revealed that, whereas there was a reliable linear trend decreasing across time for PA [$F(1, 13) = 6.72, p = .022$], no reliable trend was found for NA [$F(1, 13) = 3.14, p = .100$].

Table 7.4. Predicted and experienced PA and NA across Time (*SDs in parentheses*)

	Time		
	Predictions	Immediate Experiences	Delayed Experiences
PA	28.64 (11.76)	23.64 (7.31)	20.79 (7.14)
NA	12.93 (4.39)	14.71 (4.92)	14.93 (3.89)

Turning now to these participants' decision-related affect, their predicted and experienced regret, disappointment, rejoicing, and elation can be seen in Table 7.5. As was the case for the aggregate indices of PA and NA, these judgements were submitted to a Affect*Time ANOVA, with Affect (Regret vs. Disappointment vs. Rejoicing vs. Elation) and Time (Predictions vs. Experiences-Immediate vs. Experiences-Delayed) as within-subjects factors.

Table 7.5. Predicted and experienced decision-related affect across Time (*SDs* in parentheses)

	Time		
	Predictions	Immediate Experiences	Delayed Experiences
Regret	1.29 (0.61)	1.86 (1.03)	1.64 (0.84)
Disappointment	1.57 (1.02)	1.93 (1.00)	1.64 (0.75)
Rejoicing	3.14 (1.61)	2.00 (1.18)	1.71 (0.91)
Elation	2.93 (1.49)	2.00 (0.96)	1.64 (0.93)

The analysis yielded a main effect of Time [$F(2, 26) = 4.48, p = .021$], such that participants' decision-related affect showed a reliable decrease across time [$F(1, 13) = 9.24, p = .009$]. This effect was qualified by the Affect*Time interaction that the analysis also revealed [$F(2.50, 32.54) = 6.68, p = .002$]. Whereas the analysis failed to document any effects for regret or disappointment, for rejoicing it yielded a main effect of Time [$F(1.44, 18.67) = 10.40, p = .002$]. Whereas participants' rejoicing decreased from predictions to immediate experiences [$F(1, 13) = 24.47, p < .001$], it remained stable from immediate experiences to delayed experiences [$F(1, 13) = .88, p = .365$].

Likewise, for elation the analysis revealed a main effect of Time [$F(2, 26) = 6.97, p = .004$]. Whereas participants' elation decreased from predictions to immediate experiences [$F(1, 13) = 8.29, p = .013$], it remained stable from immediate experiences to delayed experiences [$F(1, 13) = .88, p = .365$].

Discussion

Study 7 was designed to test the hypothesis that diary completion would make students' affective forecasts more accurate. The research setting employed to test this hypothesis was designed to be a naturalistic one: The research environment was the students' classrooms, rather than the laboratory. Moreover, the task itself

was an actual academic assignment that the students had to hand in as part of their term coursework. Finally, the performance feedback that the students received was also not manipulated. In what follows, first, I summarise the findings of Study 7 that are related to the accuracy hypothesis. Second, I elaborate on the determinants of participants' emotional experiences. Finally, I discuss the quest for naturalistic research settings in the study of judgement and decision-making.

Focalism and the accuracy of affective anticipations

Whereas students who were narrowly focused on receiving their marks overpredicted their post-feedback pleasure, de-focused students' predictions of pleasure were in line with their post-feedback pleasure experiences. This finding replicates the relevant findings of Study 6. Therefore, regarding the accuracy of affective self-forecasts, Study 7 replicates Wilson et al's (2000) findings related to the presence of focalism in prospective judgements of affect and also serves as an extension of their documentation of focalism to a controllable situation.

Surprise effects and participants' emotional experiences

Study 7 reported surprise effects qualitatively different from the ones that Study 1 and Study 6 documented. In Study 1, participants were genuinely surprised when, for instance, they chose the harder task and they found out that they could have performed worse with the easier one or when they chose the easy task and found out that they could have done better with the harder one. The emotional impact of surprises (both positive and negative) was generally overpredicted by these participants. Likewise, in Study 6, participants were also genuinely surprised when they made reasonable offers and received rejections. The emotional impact of these negative surprises was generally overpredicted by these participants too.

Unlike Studies 1 and 6, in Study 7 the outcomes that participants received (i.e. their marks) were not manipulated. However, participants appeared to be surprised here too. The main reason for their surprise was that in Study 7 participants underpredicted how well they would do at the coursework task. Therefore, when their actual performance exceeded their estimated one they were quite happy – actually, the more confident they were that their performance would be low when they estimated it, the less unhappy they were when they received their marks. I take these findings as a clear demonstration of “defensive pessimism” (e.g. Shepperd et al., 1996; Taylor & Shepperd, 1998; van Dijk et al., 2003): In order to avoid post-feedback disappointment with unrealistically high hopes, students chose to keep their expectations low – especially since coursework-feedback was more or less imminent.

Naturalistic research settings: Environments, tasks, and measures

The pattern of affective miscalculations that Study 6 documented in an artificial setting (i.e. the laboratory) using an artificial task (i.e. the UG) was also present in Study 1, in a similarly artificial setting (i.e. the laboratory) using a less artificial task (i.e. choice between more and less rewarding unknown task accompanied by performance-related feedback). This pattern of affective miscalculations was replicated again by Study 7 in a natural setting (i.e. the students’ seminar and laboratory classes) and with a purely naturalistic task (i.e. coursework writing).

The quest for naturalistic environments and naturalistic tasks has recently become a pressing one within the JDM research domain (e.g. Hastie, 2001). The practical consequence of this is that researchers are urged to choose designs that, to the best of their knowledge, simulate the situation outside the laboratory. Regarding

the research on affective self-forecasting, however, if one accepts the premise of ecological validity, then the question of task selection becomes very hard (if not impossible) to solve. In other words, it can be argued that the task of assigning a number to an affective experience has absolutely no meaning in the world outside the laboratory. Certainly people can talk about their affective experiences; they can also anticipate these experiences and create affective memories. However, assigning numbers to them cannot be claimed to be a generic or intuitive way of describing, predicting, or recalling these experiences – hence the difficulty of having an ecologically valid task of this sort.

A more general problem with the ecological validity approach is that the premise of “ecological validity” (i.e. the premise that the ultimate goal of psychological models is the parsimonious description of real-life behaviours) can itself be challenged. From a cognitive psychology or cognitive science viewpoint it can be argued that non-ecologically valid tasks can be at least as informative as ecologically valid ones in the search of the processes that underlie people’s behaviours (e.g. Goldstein, in press; Harvey, 1997a; 1997b).

For instance, when people with normal vision judge the length of a couple of lines under some conditions (e.g. when inward-looking arrows have been added to one of the lines, whereas outward-looking arrows have been added to the other one) they perceive one line as being longer than the other, whereas actually the two lines are of equal length (the Müller-Lyer illusion; e.g. Goldstein, 2002). Similarly, when people with average numerical ability are asked to predict the outcome of a single toss of a fair coin under some conditions (e.g. when the last five tosses have come out “tails”) they will predict that it is more likely that the coin toss will come out “heads” rather than “tails” (e.g. the representativeness heuristic; e.g. Tversky & Kahneman, 1982). Both these examples come from research areas that demonstrate

the presence of “illusions” (visual and cognitive) in human judgement and can be thought of as typical illustrations of the argument that non-ecologically valid tasks can provide valid insights into human judgement¹.

¹ However, whereas the assessment of visual illusions appears to be a straightforward issue, the assessment of cognitive illusions has triggered a substantial (and still on-going) discussion (e.g. Ayton & Wright, 1994).

CHAPTER

8

Post-decisional emotional reactions after recalled real-life decisions:

How do de-focusing manipulations work?

The accuracy of affective recall

Wilson et al. (2000) found that people estimate that their affective reactions to future events will be more intense than they actually are and they attributed these mispredictions to focalism (see Chapter 7). Focalism has also been found to influence people's judgements of their affective reactions to past events (Mitchell et al. 1997; Wilson, Meyers, & Gilbert, in press). For example, Mitchell et al. (1997) found that people's retrospective judgements of enjoyable events, such as bicycle trips and vacations, were more positive than their judgements that were contemporaneous with these events. Wilson et al. (2000) argue that "retrospective focalism" arises for the same reason as "prospective focalism": People pay too much attention to the focal event and not enough to the consequences of other events that occur at the same time as the focal event.

Wilson et al. (2000; study 1) demonstrated that asking people to complete a diary of activities in which they expected to be engaged around the time of the focal event reduced the intensity of the emotions associated with that event. They argued that the diary manipulation had its effect by reducing focalism: Specifically, they suggested that its effect is to reduce the extent to which people expect to be thinking about the focal event after it has occurred. I also obtained both indirect (Studies 2, 3, & 4) and direct (Studies 6 & 7) empirical evidence that the diary manipulation is effective in reducing focalism across a variety of research settings.

Regarding the way that the de-focusing effect of diary occurs, Wilson et al. (2000) suggested that it could be brought about in one of two ways. First, asking people to complete a diary may make them realise that that other events would occupy their thought and thereby distract them from thinking about the target event. As a result, they would ensure that they moderated their affective forecasts that they are asked to generate (the "distraction hypothesis"). Second, people may focus their

attention on the affective consequences of the other events rather than at the likelihood of being distracted by those events. In other words, they may believe that the affective consequences of the focal event will be diluted or cancelled out by the affective consequences of the other events occurring around the same time. As a result, they attenuate their affective forecasts of the focal event (the “affective competition hypothesis”).

In fact, Wilson et al. (2000; study 4) in their investigation of prospective focalism found that the affective valence of events in participants’ diaries had no effect on forecast affect for the focal event. Consequently, they concluded that the “affective competition hypothesis” was not viable; the “distraction hypothesis” provided the more plausible account of their diary manipulation. In the present research, the indirect test of the two hypotheses undertaken in Study 4 also did not provide any empirical grounds for the support of the “affective competition hypothesis”.

Studies 8 to 10: Aims and hypotheses

To summarise, for Wilson et al. (2000; p. 835), “Any time people think about how an event in the future or the past will or did affect them, they are likely too focus on that event too much and not enough on other occurrences that will or did occupy their thoughts and influence their behaviour”. Both their experiments and the present research provide empirical support for the focalism account of inaccuracies in affective forecasts. However, two further issues need to be addressed. First, there is only little direct evidence that retrospective judgements of affect are influenced by focalism (e.g. Mitchell et al., 1997; Wilson et al., in press). Second, there is no direct evidence that de-focusing people from the recalled event attenuates their affective judgements.

Therefore, the aim of the three studies reported in this chapter was twofold. First, the present studies were aimed as further empirical tests of Wilson et al.'s suggestion that focalism operates retrospectively, as well as prospectively. More specifically, I tested for the presence of focalism in delayed judgements of post-decisional emotional reactions. Second, the present studies were also designed to provide a direct empirical test of the underlying mechanism that leads to the effects of focalism and de-focusing.

Study 8

I asked participants to recall a real-life decision of theirs. Half of them were asked to recall a bad decision; the others were asked to recall a good decision. All participants were asked to report how they felt at the moment regarding the recalled decision. Half of the participants were de-focused before they reported their post-decisional emotions. De-focused participants completed a diary of events that had occurred to them at the same time as the recalled decision.

Regarding participants' post-decisional emotions, I anticipated that that de-focused participants would report lower levels of post-decisional affect than participants who remained focused on the recalled decision throughout the experiment.

Method

Participants: One hundred and twelve current and prospective students of the University of London volunteered to participate in the study (mean age = 19.53 years; $SD = 2.62$ years; ninety females). The majority of participants were recruited during a group laboratory demonstration session and were not paid. Others were

recruited via notices in buildings of the University of London and were paid £5 for their participation (in this and other studies).

Variables and Design: The design was a 2*2 completely between-subjects design, with Decision (Satisfactory vs. Unsatisfactory) and Diary (No-Diary vs. Diary) as independent variables. There were 28 participants in each condition. The dependent variable was post-decisional affect.

Materials and Procedure: All participants were presented with a short booklet containing experimental manipulations and dependent measures. On the first page of the booklet, participants in the “satisfactory decision” condition received the following instructions (instructions for the “unsatisfactory decision” condition in parentheses): *“First of all, we would like you to recall a decision of yours from your own life, the outcome of which led you to feel intense satisfaction (regret). Provide a brief description of this decision, such that the person who reads it understands why you feel satisfied with (regret) it”*. Participants were given three quarters of a page to summarise their decisions.

On the second page of the booklet, participants in the “diary” condition were requested to provide a diary. The instructions read as follows: *“Now, we would like you to write down as many events as you can that occurred to you at the same time as the decision you previously described”*. Participants were again given approximately three quarters of a page to summarise the events that they could recall. There was no filler task for participants in the “no-diary” condition.

Finally, all participants rated their post-decisional affect. For the “satisfactory decision” the affect item read as follows (the item for the “unsatisfactory decision” in parentheses): *“To what extent do you feel satisfied with*

(*regret*) the decision you described above?” Participants gave their answers on an 8-point scale, anchored at 1 = moderately and 8 = very much.

Results

Reported regret and satisfaction with the outcomes of the recalled decisions can be seen in Table 8.1. Two independent samples *t*-tests on these ratings revealed that whereas the Diary was effective in lowering reported regret [$t(46.59) = 1.97, p = .055$], it did not had any effect on reported satisfaction [$t(54) = .37, p = .711$].

Table 8.1. Mean post-decisional affect across conditions (*SDs* in parentheses)

	Satisfactory decision	Unsatisfactory decision
No-Diary	6.96 (1.23)	6.36 (1.42)
Diary	6.86 (0.89)	5.39 (2.17)

Discussion

The attenuation of reported regret by the diary manipulation that Study 8 documented shows that Wilson et al.’s (2000) distraction hypothesis can be extended to retrospective judgements of affect. The lack of an effect for retrospective judgements of positive affect replicates the asymmetry between positive and negative affect that Wilson et al. (2000) also documented. Wilson et al. (2000) argued that the asymmetry arises because positive emotional states are more fragile than negative ones.

Before concluding that the diary manipulation did indeed produce a distraction effect, I need to exclude the possibility that it disrupted emotional processing of the focal event that had been retrieved from memory. Study 9 was designed to do this.

Study 9

In Study 8 the diary was inserted between the recall of an emotional event and the assessment of the emotions associated with that event. If the act of retrieval of an emotional event itself elicits some emotional response and if that response is used in the assessment, then interposing the diary completion task between event retrieval and assessment may have its effect by displacing the cues used for assessment from working memory. In other words, the effect of the diary may have been to interfere with emotional processing (i.e. rather than to contextualise it as the distraction hypothesis suggests).

In Study 9 participants were again asked to recall a past decision of theirs, the outcomes of which led to either positive, or negative affect, and describe how they felt about it at present. Some of the participants were de-focused by completing a diary of events that had occurred to them at the same time as the recalled decision. Some other participants were given an anagram-solving task to solve before reporting their post-decisional emotions. Finally, some participants reported their post-decisional emotions immediately after the recall of the decision.

If the interference hypothesis is correct then interposing tasks irrelevant to the retrieved emotional event should also displace information required for emotional assessment from working memory. Any task that has this effect should produce interference. In this study, I examined whether solving anagrams has any effect on retrospective judgements of the emotions associated with the target event.

Regarding participants' post-decisional emotions, if the distraction hypothesis is correct, then I anticipated that de-focused (i.e. diary) participants would report lower levels of post-decisional affect than participants who either remained focused on the recalled decision throughout the experiment, or did the anagram-solving task. However, if the interference hypothesis is correct, then I

anticipated that both de-focused (i.e. diary) participants and participants who solved anagrams would report lower levels of post-decisional affect than participants who remained focused on the recalled decision throughout the experiment.

Method

Participants: One hundred and ninety-two current and prospective students of the University of London volunteered to participate in the study (mean age = 19.08 years; $SD = 4.55$ years; one hundred and forty-seven females). The majority of participants were recruited during a group laboratory demonstration session and were not paid. Others were recruited via notices in buildings of the University of London and were paid £3 for their participation.

Variables and Design: The design was a 2*3 completely between-subjects design, with Decision (Satisfactory vs. Unsatisfactory) and Condition (No-Diary vs. Diary vs. Anagrams) as independent variables. There were 32 participants in each condition. The dependent variables were the measures of affect. I used the PANAS (Watson et al., 1988) and the decision-related affect items that I introduced in Study 1 to record participants' post-decisional affect.

Materials and Procedure: All participants were presented with a short booklet containing experimental manipulations and dependent measures. On the first page of the booklet, participants were told that the study investigated “*people’s perceptions of and reactions to everyday situations*”. On the second page of the booklet, participants in the “satisfactory decision” condition received the following instructions (instructions for the “unsatisfactory decision” condition in parentheses): “*First of all, we would like you to recall a decision of yours from your own life, the*

outcome of which led you to feel intense positive affect (negative affect). Provide a brief description of this decision, such that the person who reads it understands why you feel good (bad) about it". Participants were given approximately three quarters of a page to summarise their decisions.

On the third page of the booklet, one third of the participants ("diary" condition) were then requested to provide a diary; the instructions read as follows: *"Now, we would like you to recall and write down as many events as you can that occurred to you at the same time as the decision you previously described"*. Participants were again given approximately three quarters of a page to summarise the events that they could recall.

Another third of participants ("anagrams" condition) were asked to solve 13 four-letter anagrams; the instructions read as follows: *"Now we would like you to solve the following anagrams. The first one has been worked out for you, as an example. All of the anagrams can be solved"*. The example was: *"ARTY → TRAY"*. There was no filler task for participants in the "no-diary" condition.

Finally, all participants went through the last two pages of the booklet, where they had to fill in the PA and NA scales and the decision-related affect scales. The instructions were a modified version of the PANAS instructions (Watson et al., 1988; p. 1070) and read as follows: *"The list that follows consists of a number of words that describe different feelings and emotions. Read each item and then mark the appropriate answer in the space next to that word. Indicate to what extent you are feeling this way, with respect to the decision you just now described. Keep in mind that there are no right or wrong answers"*. Participants gave their answers on 5-point scales, anchored at 1 = very slightly or not at all and 5 = extremely.

Results

1. Post-decisional decision-related emotions

Regret, rejoicing, disappointment, and elation were submitted to a Decision*Condition*Affect mixed ANOVA, with Decision (Satisfactory vs. Unsatisfactory) and Condition (No-Diary vs. Diary vs. Anagrams) as between-subjects factors and Affect (Regret vs. Disappointment vs. Rejoicing vs. Elation) as a within-subjects factor. This analysis yielded two main effects (Decision and Affect), and two two-way interactions (Affect*Decision, and Affect*Condition), all of which were qualified by the interaction between Decision, Condition, and Affect [$F(6, 558) = 3.68, p < .001$]. I split this interaction into two Affect*Condition ANOVAs – one for “satisfactory” and one for “unsatisfactory” decisions.

Decision-related affect for satisfactory decisions can be seen in Table 8.2. The analysis revealed only the anticipated main effect of Affect [$F(1.99, 185.04) = 107.08, p < .001$], such that participants reported more rejoicing and elation than regret and disappointment.

Table 8.2. Mean decision-related affect “satisfactory decision” conditions (*SDs in parentheses*)

	No-Diary	Diary	Anagrams
Regret	1.25 (0.67)	1.13 (0.42)	1.47 (0.88)
Disappointment	1.25 (0.57)	1.22 (0.55)	1.44 (0.80)
Rejoicing	3.06 (1.08)	3.16 (1.11)	2.72 (1.46)
Elation	3.00 (1.34)	3.09 (1.30)	3.13 (1.39)

Decision-related affect for unsatisfactory decisions can be seen in Table 8.3. The analysis revealed again the anticipated effect of Affect [$F(1.76, 163.29) = 130.53, p < .001$], such that participants reported more regret and disappointment than rejoicing and elation. However, this effect was qualified by the

Affect*Condition interaction [$F(3.51, 163.29) = 4.55, p = .003$]. I divided this interaction into four one-way ANOVAs – one for each decision-related emotion.

For regret, the analysis revealed an overall effect of Condition [$F(2, 93) = 3.08, p = .051$]. Analyses of contrasts further revealed that “diary” participants reported lower levels of regret than “no-diary” participants [$t(93) = 1.98, p = .050$] and that “anagrams” participants also reported lower levels of regret than “no-diary” participants [$t(93) = 2.28, p = .025$].

For disappointment the pattern was similar. The analysis revealed an overall effect of Condition [$F(2, 93) = 4.59, p = .013$]. Analyses of contrasts further revealed that “diary” participants reported lower levels of disappointment than “no-diary” participants [$t(93) = 1.93, p = .056$] and that “anagrams” participants also reported lower levels of disappointment than “no-diary” participants [$t(93) = 2.99, p = .004$].

Table 8.3. Mean decision-related affect “unsatisfactory decision” condition (*SDs in parentheses*)

	No-Diary	Diary	Anagrams
Regret	4.31 (1.06)	3.69 (1.36)	3.59 (1.34)
Disappointment	3.53 (1.41)	2.84 (1.51)	2.47 (1.34)
Rejoicing	1.09 (0.39)	1.44 (1.08)	1.50 (1.02)
Elation	1.03 (0.18)	1.44 (0.77)	1.47 (0.84)

For elation, the analysis revealed an overall effect of Condition [$F(2, 93) = 4.34, p = .016$]. Analyses of contrasts further revealed that “diary” participants reported higher levels of elation than “no-diary” participants [$t(34.30) = -2.95, p =$

.006] and that “anagrams” participants also reported higher levels of elation than “no-diary” participants [$t(33.73) = -2.88, p = .007$].

Finally, for rejoicing the overall analysis failed to reveal a reliable effect.

2. Post-decisional positive and negative affect (PANAS)

The 10 positive items and the 10 negative items of the PANAS were separately summed, in order to obtain the overall PA and NA scores. Across the six experimental conditions the PA scale had a mean inter-item reliability of .81, with Cronbach Alpha reliability coefficients ranging from .60 to .90. The NA scale had a mean inter-item reliability of .83, with Cronbach Alpha reliability coefficients ranging from .75 to .89.

PA and NA scores were submitted to a Decision*Condition*Affect mixed ANOVA, with Decision (Satisfactory vs. Unsatisfactory) and Condition (No-Diary vs. Diary vs. Anagrams) as between-subjects factors and Affect (PA vs. NA) as a within-subjects factor. The analysis yielded two main effects (Decision and Affect), and a two-way interaction (Affect*Decision), all of which, however, were qualified by the marginal interaction between Decision, Condition, and Affect [$F(2, 185) = 2.37, p = .10$]. I split this interaction into two Affect*Condition ANOVAs – one for “satisfactory” and one for “unsatisfactory” decisions.

PA and NA for satisfactory decisions can be seen in Table 8.4. The analysis revealed only the anticipated main effect of Affect [$F(1, 92) = 348.14, p < .001$], such that participants reported more PA than NA.

Table 8.4. PA and NA across “satisfactory decision” conditions (*SDs* in parentheses)

	No-Diary	Diary	Anagrams
PA	34.59 (6.71)	34.81 (7.71)	34.69 (7.58)
NA	16.09 (5.81)	16.54 (5.04)	17.75 (7.17)

PA and NA for unsatisfactory decisions can be seen in Table 8.5. The analysis revealed again the anticipated effect of Affect [$F(1, 93) = 19.14, p < .001$], such that participants reported more NA than PA. A weak Affect*Condition interaction [$F(2, 93) = 2.58, p = .081$] qualified this effect. I split this interaction into two one-way ANOVAs – one for PA and one for NA.

For NA, the analysis failed to reveal an effect of Condition. However, for PA the analysis revealed an overall effect of Condition [$F(2, 93) = 5.44, p = .006$]. Analyses of contrasts further revealed that “diary” participants reported higher levels of PA than “no-diary” participants [$t(41.96) = -2.76, p = .009$] and that “anagrams” participants also reported higher levels of PA than “no-diary” participants [$t(42.51) = -3.48, p = .001$].

Table 8.5. PA and NA across “unsatisfactory decision” conditions (*SDs* in parentheses)

	No-Diary	Diary	Anagrams
PA	15.16 (4.10)	20.25 (9.61)	21.44 (9.35)
NA	24.69 (8.18)	24.50 (8.75)	24.28 (9.35)

Discussion

The results of Study 9 unequivocally support the interference hypothesis. Effects of the diary completion task and the anagrams task were the same: Both of them resulted in people judging their emotions associated with unsatisfactory focal events as less negative. They showed less regret and disappointment and more

elation and rejoicing. General positive affect was significantly increased by both manipulations (though, curiously, negative affect remained unaffected).

Before concluding that the diary completion task acts to interfere with the contents of working memory in the manner outlined above, I need to exclude an alternative explanation of the results of this study. The anagrams that were used were very simple: Virtually all participants solved them. Hence it is possible that success in the anagrams task temporarily elevated participants' moods and this boost in their positive affect transferred to their retrospective judgements of affect associated with the focal event that they had retrieved from memory. In other words, feeling good about the anagrams task may have made them feel better about their unsatisfactory decisions. Thus the argument is that the effects of the two interfering tasks were the same but occurred via different routes. Diary completion de-focused participants by making them aware that they would be distracted from the focal event by other events (Wilson et al., 2000); anagram-solving produced a mood elevation that transferred to participants' assessment of their affect associated with the retrieved focal event. Study 10 was designed to distinguish between the "single route model" (i.e. both tasks produce an interference effect) and the "dual route model" (i.e. diary completion has its effect via distraction but anagram solving has its effect via affect transfer).

Study 10

I assumed that the extent of the affect transfer does not depend on the source of the affect. Thus for the "dual route model" to hold, affect should be elevated by anagram solving but not by diary completion (or, at least, it should be elevated more by anagram solving). Thus I examined whether mood increased more between the start of the experiment and the completion of the interfering task in the anagrams

condition than in the diary completion condition. To check that mood monitoring itself does not affect de-focusing, I compared the no-diary / no-mood-monitoring condition that was included in Studies 8 and 9 with a no-diary / mood-monitoring condition in which participants' moods were monitored in the absence of any task interposed between focal event retrieval and assessment of affect.

In addition to the dependent variables used in Study 9, I also included participants' perceptions of their personal responsibility for the outcome of the focal event and of the degree to which that outcome was influenced by situational factors. Previous research has linked the feeling of personal responsibility for a outcome with experienced regret (Zeelenberg et al., 1998; Zeelenberg, van Dijk, van der Pligt, Manstead, van Empelen, & Reinderman, 1998) and so it is possible that diary completion has its effect by causing participants to regard the unfortunate events less as their own fault, which, in turn, decreases the degree of negative affect that they associate with that event. This "responsibility reduction hypothesis" is an alternative to Wilson et al.'s (2000) distraction hypothesis in the dual route model. It predicts that judgements of personal responsibility will be lower after diary completion, whereas the distraction hypothesis is neutral in this respect.

Method

Participants: Eighty-five students of various London Universities volunteered to participate in the study (mean age = 25.78 years; *SD* = 8.25; sixty females). The majority of participants were paid £8 for their participation (in this and other studies). Other participants were not paid for their participation.

Variables and Design: The design was a four independent groups design, with Condition (No-Diary / No-Mood-monitoring vs. No-Diary / Mood-monitoring vs.

Diary / Mood-monitoring vs. Anagrams / Mood-monitoring) as the independent variable. There were 20-22 participants in each condition.

The first set of dependent variables comprised the measures of affect. I used the PANAS (Watson et al., 1988) to record participants' moods and the PANAS accompanied by the decision-related affect items that were introduced in Study 1 to record participants' post-decisional affect. The second set of dependent measures comprised the ratings of perceived personal responsibility for the outcome of the decision (PR) and perceived contribution of situational factors to the outcome of the decision (SF).

Materials and Procedure: All participants were presented with a short booklet containing manipulations and dependent measures. On the first page of the booklet, participants were told that the study investigated “*people’s perceptions of and reactions to everyday situations*”. On the second page of the booklet, participants in the mood monitoring conditions were asked to record their current mood using the PANAS. The instructions were a version of the standard PANAS instructions (Watson et al., 1988; p. 1070), and read as follows: “*The list that follows consists of a number of words that describe different feelings and emotions. Read each item and then mark the appropriate answer in the space next to that word. Indicate to what extent you are feeling this way right now, that is at the present moment. Keep in mind that there are no right or wrong answers*”. Participants gave their answers on 5-point scales, anchored at 1 = very slightly or not at all and 5 = extremely. There was no filler task for participants in the “no-diary/no mood monitoring” condition.

On the third page of the booklet, all participants received the following instructions: “*First of all, we would like you to recall a decision of yours from your own life, the outcome of which led you to feel intense negative affect. Provide a brief*

description of this decision, such that the person who reads it understands why you feel bad about it". Participants were given approximately three quarters of a page to summarise their decisions.

Next, participants in the "diary/mood-monitoring" condition were requested to complete a diary; the instructions read as follows: "*Now, we would like you to think of and briefly describe TWO MAJOR EVENTS that occurred to you at the same time as the decision you described*". Participants were given two lines to summarise each event.

Participants in the "anagrams/mood-monitoring" condition were asked to solve 13 four-letter anagrams; the instructions read as follows: "*Now we would like you to solve the following anagrams. The first one has been worked out for you, as an example. All of the anagrams can be solved*". The example was: "ARTY → TRAY". There was no filler task for participants in the "no-diary/mood-monitoring" and "no-diary/no-mood-monitoring" conditions.

As soon as participants had finished with the recall task ("no-diary" conditions), or the recall and the diary tasks ("diary" condition), or the recall and the anagrams tasks ("anagrams" condition), they were asked to record their mood once again, using the same PA and NA scales. Instructions were the same as before. The order of the items in the scales was randomised across the stages of the experiment. Again, there was no filler task for participants in the "no-diary/no-mood-monitoring" condition.

Finally, all participants went through the last two pages of the booklet, where they had to fill in the PA and NA scales, the decision-related affect scales, and the perceived responsibility scales. Instructions for the affect scales were very similar to the ones described above, the only difference being the following: "... *Indicate to what extent you are feeling this way, with respect to the decision you just*

now described". The personal responsibility (PS) item read as follows [the situational factors (SF) item in parentheses]: "*To what extent do you think you (situational factors) are responsible for the bad outcome of the decision you described above?*" Participants gave their answers on two 8-point scales, anchored at 1 = not at all and 8 = very much.

Results

1. Moods before and after the experimental tasks

In order to measure participants' moods before and after the tasks, the 10 positive items and the 10 negative items of the PANAS were separately summed, resulting in four mood measures: PA before the recall task ("PA-before"); NA before the recall task ("NA-before"); PA after the recall, or diary, or anagrams tasks ("PA-after"); and NA after the recall, or diary, or anagrams tasks ("NA-after"). Across the three experimental conditions the PA scale had a mean inter-item reliability of .78, with Cronbach Alpha reliability coefficients ranging from .67 to .88. The NA-before scale had a mean inter-item reliability of .80, with Cronbach Alpha reliability coefficients ranging from .75 to .82. The PA-after scale had a mean inter-item reliability of .91, with Cronbach Alpha reliability coefficients ranging from .89 to .93. The NA-after scale had a mean inter-item reliability of .81, with Cronbach Alpha reliability coefficients ranging from .75 to .89.

PA-before and NA-before and PA-after and NA-after scores can be seen in Table 8.6. These scores were submitted to a Condition*Time*Affect mixed ANOVA, with Condition (No-Diary / No-Mood-monitoring vs. No-Diary / Mood-monitoring vs. Diary / Mood-monitoring vs. Anagrams / Mood-monitoring) and Time (Before vs. After) as between-subjects factors and Affect (PA vs. NA) as a within-subjects factor.

The analysis yielded a main effect of Affect [$F(1, 61) = 109.00, p < .001$], such that participants reported overall higher levels of PA than NA. This effect was qualified by an interaction between Time and Affect [$F(1, 61) = 8.46, p = .005$] that the analysis also revealed. Whereas participants' PA was lower after the tasks than before [$t(63) = 2.72, p = .009$], their corresponding NA tended to be higher after the tasks than before [$t(63) = -1.91, p = .060$].

Importantly, there was no suggestion of an interaction between Time and Condition. The reduction of participants' moods over the course of the experiment was no greater when participants completed a diary as an interfering task than when they solved anagrams as their interfering task. In other words, there was no indication that anagram solving boosted participants' moods more or depressed them less than diary completion. Therefore, I have no evidence for a dual route model; for parsimony reasons, I maintain the single route model based on the "interference hypothesis".

Table 8.6. PA and NA before and after the tasks across "mood-monitoring" conditions (*SDs* in parentheses)

	No-Diary	Diary	Anagrams
PA-before	28.51 (8.27)	26.32 (5.25)	26.27 (6.23)
NA-before	14.91 (5.05)	14.24 (4.70)	16.59 (6.03)
PA-after	25.43 (9.47)	24.23 (7.84)	25.46 (9.19)
NA-after	16.14 (5.13)	15.24 (4.25)	18.05 (7.93)

2. Post-decisional decision-related affect

Reported decision-related emotions can be seen in Table 8.7. Regret, rejoicing, disappointment, and elation were submitted to a Condition*Affect mixed

ANOVA, with Condition (No-Diary / No-Mood-monitoring vs. No-Diary / Mood-monitoring vs. Diary / Mood-monitoring vs. Anagrams / Mood-monitoring) as a between-subjects factor and Affect (Regret vs. Disappointment vs. Rejoicing vs. Elation) as a within-subjects factor. The analysis yielded a main effect of Affect [$F(1.51, 122.28) = 51.88, p < .001$], such that participants reported more regret and disappointment than rejoicing and elation. The analysis also yielded a main effect of Condition [$F(3, 81) = 8.72, p < .001$], such that participants in the “no-diary/no-mood-monitoring” condition reported higher levels of decision-related affect than the other three conditions.

Table 8.7. Mean decision-related affect across conditions (*SDs* in parentheses)

	No-Diary / No-Mood- monitoring	No-Diary / Mood- monitoring	Diary / Mood- monitoring	Anagrams / Mood- monitoring
Regret	4.14 (1.11)	2.91 (1.64)	2.52 (1.47)	3.00 (1.45)
Disappointment	4.10 (1.26)	2.48 (1.66)	2.14 (1.42)	2.59 (1.50)
Rejoicing	1.48 (1.08)	1.61 (0.90)	1.29 (0.64)	1.36 (0.73)
Elation	1.52 (0.75)	1.58 (1.03)	1.41 (0.70)	1.41 (0.67)

These main effects were qualified by a significant Condition*Affect interaction [$F(4.53, 122.28) = 3.11, p = .014$]. I split this interaction into four one-way ANOVAs – one for each decision-related emotion.

For regret, the analysis revealed an overall effect of Condition [$F(3, 81) = 5.00, p = .003$]. Analyses of contrasts further revealed that “diary/mood-monitoring” participants reported lower levels of regret than “no-diary/no-mood-monitoring” participants [$t(81) = -3.67, p < .001$] and that “anagrams/mood-monitoring” participants also reported lower levels of regret than “no-diary/no-mood-

monitoring” participants [$t(81) = -2.62, p = .011$]. Importantly, participants in the “diary/mood-monitoring” and “anagrams/mood-monitoring” conditions reported same levels of regret as “no-diary/mood-monitoring” participants’ [“no-diary/mood-monitoring”-“diary/mood-monitoring” contrast: $t(81) = -.86, p = .391$; “no-diary/mood-monitoring”-“anagrams/mood-monitoring” contrast: $t(81) = .22, p = .828$].

For disappointment the pattern was similar. The analysis revealed an overall effect of Condition [$F(3, 81) = 7.32, p < .001$]. Analyses of contrasts further revealed that “diary/mood-monitoring” participants reported lower levels of disappointment than “no-diary/no-mood-monitoring” participants [$t(81) = -4.31, p < .001$] and that “anagrams/mood-monitoring” participants also reported lower levels of disappointment than “no-diary/no-mood-monitoring” participants [$t(81) = -3.36, p = .001$]. Importantly, participants in the “diary/mood-monitoring” and “anagrams/mood-monitoring” conditions reported same levels of disappointment as “no-diary/mood-monitoring” participants’ [“no-diary/mood-monitoring”-“diary/mood-monitoring” contrast: $t(81) = -.74, p = .464$; “no-diary/mood-monitoring”-“anagrams/mood-monitoring” contrast: $t(81) = .26, p = .799$].

For elation and rejoicing the analysis failed to reveal a reliable effect.

3. Post-decisional positive and negative affect

The 10 positive items and the 10 negative items were separately summed, in order to obtain the PA and NA scale scores, respectively. Across the four experimental conditions the PA scale had a mean inter-item reliability of .90, with Cronbach Alpha reliability coefficients ranging from .87 to .93. The NA scale had a mean inter-item reliability of .88, with Cronbach Alpha reliability coefficients ranging from .85 to .91.

PA and NA scores can be seen in Table 8.8. PA and NA scores were submitted to a Condition*Affect mixed ANOVA, with Condition (No-Diary / No-Mood-monitoring vs. No-Diary / Mood-monitoring vs. Diary / Mood-monitoring vs. Anagrams / Mood-monitoring) as a between-subjects factor and Affect (PA vs. NA) as a within-subjects factor. The analysis yielded a main effect of Condition [$F(3, 81) = 5.72, p = .001$], such that participants in the “no-diary/no-mood-monitoring” condition reported higher levels of affect than the other three conditions.

Table 8.8. PA and NA across conditions (*SDs* in parentheses)

	No-Diary / No-Mood- monitoring	No-Diary / Mood- monitoring	Diary / Mood- monitoring	Anagrams / Mood- monitoring
PA	22.57 (9.16)	21.05 (8.87)	22.29 (9.11)	21.50 (8.41)
NA	31.05 (10.01)	20.71 (8.60)	17.81 (7.69)	22.77 (10.04)

This main effect was qualified by the Condition*Affect interaction [$F(3, 81) = 3.20, p = .028$]. I split this interaction into two one-way ANOVAs – one for PA and one for NA.

For PA the analysis failed to reveal a reliable effect. For NA the analysis revealed an overall effect of Condition [$F(3, 81) = 8.56, p < .001$]. Analyses of contrasts further revealed that both “diary/mood-monitoring” participants reported lower levels of NA than “no-diary/no-mood-monitoring” participants [$t(81) = -4.80, p < .001$] and that “anagrams/mood-monitoring” participants also reported lower levels of NA than “no-diary/no-mood-monitoring” participants [$t(81) = -3.16, p = .002$]. Importantly, participants in the “diary/mood-monitoring” and “anagrams/mood-monitoring” conditions reported same levels of NA as “no-diary/mood-monitoring” participants’ [“no-diary/mood-monitoring”-“diary/mood-

monitoring” contrast: $t(81) = -1.03, p = .305$; “no-diary/mood-monitoring”-
“anagrams/mood-monitoring” contrast: $t(81) = .66, p = .513$].

4. Assessment of personal responsibility

PR and SF ratings can be seen in Table 8.9. These ratings were submitted to a Condition*Responsibility mixed ANOVA, with Condition (No-Diary / No-Mood-monitoring vs. No-Diary / Mood-monitoring vs. Diary / Mood-monitoring vs. Anagrams / Mood-monitoring) as a between-subjects factor and Responsibility (PR vs. SF) as a within-subjects factor. The analysis yielded only a main effect of Responsibility [$F(1, 81) = 9.26, p = .003$], such that participants perceived the unfortunate decision as a result of their own responsibility, rather than a result of external factors. Importantly, there was no suggestion of an interaction between Responsibility and Condition. Thus there was no evidence that people considered themselves less responsible for the outcome after they had completed the diary. Therefore, there was no support for the “responsibility reduction hypothesis”. Given that I have already reported mood-monitoring analyses that reject dual route models, this is to be expected.

Table 8.9. PR and SF ratings across conditions (*SDs* in parentheses)

	No-Diary / No-Mood- monitoring	No-Diary / Mood- monitoring	Diary / Mood- monitoring	Anagrams / Mood-monitoring
PR	6.14 (1.79)	5.91 (1.70)	6.00 (1.84)	6.55 (1.47)
SF	5.23 (2.00)	5.43 (2.23)	5.14 (1.91)	4.50 (1.92)

Discussion

Study 10 allowed the examination of participants' moods before and after the decision recall task. The comparison of moods revealed a change in participants' moods over the interval between stating the experience and stating their ratings of affect. It showed that their originally good mood became worse. This finding replicates findings from mood induction research, where it is assumed that a particular mood-state can be induced if participants imagine events that brought about emotions similar to the desired mood-state (e.g. Brewer, Doughtie, & Lubin, 1980; Schwarz & Clore, 1983; for a review, see Westermann, Spies, Stahl, & Hesse, 1996). Importantly for my purposes, however, the deterioration of moods was not greater in the diary completion condition. As a result, there was no scope for differential affect transfer between conditions. This serves to eliminate the dual route model from consideration.

When moods were monitored, there was no difference between either the "diary" condition and the "no-diary" condition or between the "anagrams" condition and the "no-diary" condition. Thus monitoring of moods eliminated the de-focusing effects found in Studies 8 and 9. However, negative affect (regret, disappointment, NA) was lower in the "no-diary / mood-monitoring" condition than in the "no-diary / no-mood-monitoring condition". In other words, mood monitoring itself had the same de-focusing effect that diary completion and anagram solving had in Studies 8 and 9. It appears that any mental activity (i.e. diary completion; anagram-solving; mood-monitoring) that is interposed between retrieval of the decision and judging the affect associated with that decision serves to reduce the level of the judged affect.

Finally, ratings of personal responsibility for the decision outcome and of the effects of situational factors on that outcome did not depend on the experimental

condition. Thus Study 10 provided no evidence that responsibility reduction caused the reduction in rated affect in the “diary” conditions of Studies 8 and 9.

General Discussion

Studies 8 to 10 were not designed to study retrospective focalism directly: I did not measure participants’ levels of affect at the time of the original decision and so I could not show that levels of affect associated with that decision when it is recalled at a later date are elevated. However, focalism of this type has been reported previously (e.g. Mitchell et al., 1997; Wilson et al., in press). The results are not in conflict with these findings or with Wilson et al.’s (2001) argument that both prospective and retrospective focalism occur because people pay too much attention to the focal event and not enough attention to other events that provide a context for the focal one.

Wilson et al. (2000) showed that focalism is decreased by requiring people to complete a diary of their activities around the time of the focal event. In other words, the diary completion manipulation lowered the level of affect associated with the focal event. Studies 8 to 10 were designed to explore the reason for this “de-focusing” (i.e. affect-lowering) effect. In Study 8, I demonstrated that the effect of diary completion that Wilson et al. (2000) had shown for prospective judgements can be extended to retrospective judgements too (albeit only those pertaining to unsatisfactory events). Wilson et al. (2000) argued that such effects occur because completing the diary makes people realise that other events that provide context to the focal one would also influence their level of affect. Once this realisation has taken place, people lower their affective judgements to allow for the effects of this distraction (the distraction hypothesis).

Study 9 showed that solving anagrams has the same effect as completing a diary. It is rather implausible to argue that solving anagrams would make people realise that the focal event is embedded in other affect-inducing events. Although parsimony should have led me to reject the distraction hypothesis as an explanation of the diary completion effect at this juncture, I did consider a dual route model in which the distraction hypothesis was retained for diary completion and an affect-transfer hypothesis was proposed to explain the effects of anagram solving.

Study 10 showed that solving anagrams had no more favourable effects on participants' moods than completing diaries. This rendered the affect-transfer hypothesis untenable. Furthermore, mood-monitoring itself was found to have the same affect-lowering properties as diary completion and anagram solving. Rather than retaining the distraction hypothesis for diary completion and developing other hypotheses to explain why anagram solving and mood monitoring produce similar effects, it would be better to provide a unitary account for all these phenomena.

Obviously, the retaining of the interference hypothesis for retrospective judgements of affect carries some implications for my interpretation of the effects of diaries in the studies of anticipated affect. In the light of the present findings, it could be argued that diary completion did not broaden the way people thought about the anticipated outcomes of their decisions (as Wilson and his colleagues have argued). It rather interfered with the processing that led to these judgements. If this is true, then I would expect that anagram solving would have the same effects on affective anticipations as in affective experiences.

In order to test this alternative for affective anticipations, I asked 53 participants (mean age 18.34, $SD = 3.61$ years; forty-seven females) to answer the following question: *"Think about the next six months of your life: Is there a decision that you will probably need to take during these six months and that will have a*

negative impact on your feelings (i.e. it will result in you feeling low)?” after participants had described their decisions, 17 of them (“diary” condition) completed a diary of events likely to happen to them in the same time span (i.e. in the next six months), whereas another 17 (“anagrams” condition) solved the same four-letter anagrams that I used in studies 9 and 10. Nineteen participants (“control” condition) did not undergo any filler task. Finally, all participants reported what they thought their emotions would be regarding the decisions that they had previously described using the PANAS.

Analysis of these data replicated the findings of Study 8 and thus provided further support for the interference hypothesis. Whereas there was no reliable effect of Condition (Control vs. Diary vs. Anagrams) on participants’ self-reported NA [$F(2, 50) = .33, p = .723$], the analysis revealed a reliable effect on their self-reported PA [$F(2, 50) = 3.20, p = .049$], such that “diary” and “anagrams” participants taken together ($M = 3.24, SD = .60$) reported higher levels of PA than “control” participants [$M = 2.78, SD = .94; t(50) = -2.18, p = .034$].

On the basis of all the above findings, it appears that all the effects occur when a task that requires cognitive processing is interposed between people’s recall of a past decision and their judgements of their affect associated with that decision. Without an interfering task, affect judgements are high; with one, they are lower. My proposal is that recalling a decision leads properties associated with it (including affect-related ones) to be placed in working memory. While they are in working memory, these properties can be assessed for strength, valence, etc. However, requiring people to perform a cognitive task between loading the properties into working memory and assessing those properties interferes in some way with their assessment.

Just how the interference occurs depends on how working memory is conceptualised. If one considers that its limitations arise from its restricted storage capacity (e.g. Miller, 1956), one would argue that information required for the intervening tasks displaces (some of) the information required for affect assessment. If one argues that it is limited in terms of the activation available for focal attentional processing (e.g. Cantor & Engle, 1993), one would say that performing the intervening task (partially) de-activates the memory representations required for affect assessment. If one believes that it is limited because of a shortage of the inhibitory resources needed to suppress activation of information that is no longer relevant (e.g. Bjork, 1989; Hasher & Zacks, 1988), one would propose that irrelevant information from the interfering task adds noise to the processing that subserves affect assessment. Whatever the precise mechanism, my claim is that an interfering task acts to disrupt the processing that underlies the judgements of affect.

Disruption of processing implies that output will be closer to whatever the default value is prior to any processing. Intervening tasks do not so much lower levels of judged affect as prevent them from moving as far from their default value as they would otherwise. What would the default value have been in my experiments? It is most likely to have been the centre of the rating scales (i.e. 2.50). In the absence of information about true affect levels, this would tend to minimise mean squared error. Inspection of Tables 8.1, 8.3, and 8.7 does indeed reveal that ratings are closer to this default value in interfering task conditions when the effect of the intervening task is significant (i.e. for unsatisfactory decisions).

Diary completion effects in studies of prospective judgements of affect are likely to arise for exactly the same reasons as those in studies of retrospective judgements of affect. For example, Wilson et al. (2000; study 3) asked football fans in their experimental group to complete a diary and then to forecast their affect

levels just after their team had won or lost a future fixture. Insufficient inhibitory resources (Bjork, 1989; Hasher & Zachs, 1988) would ensure that diary-related events were still active in working memory during the period in which affective forecasts were made. The resulting interference lowered these forecasts for the reasons just outlined. In Wilson et al.'s (2000; study 3) control group, no cognitive task preceded affective forecasting. Without interference, uninterrupted processing allowed ratings to move further from their default (baseline) values. Hence they were higher.¹

Judgements of affect related to satisfactory outcomes did not appear to be subject to the focalism that affected those related to unsatisfactory outcomes. They remained close to their default values even when no interference was present (Table 8.3). For example, while regret associated with an unsatisfactory decision was rated 4.31 on the five-point scale in the No-Diary condition of Study 9, elation associated with a satisfactory decision was only rated as 3.00 on the same scale in the same condition. Effects of interference cannot lower ratings if they are already close to their default value.

¹ Of course, processing without interference does not have to lead to ratings different from the default values: When the control group of Wilson et al.'s (2000) football study were asked to predict what their affect would be some days after the game, their ratings were close to baseline.

CHAPTER

9

General Discussion

1. Main hypotheses of this research: Overview of the empirical findings

As stated in Chapter 3, the aim of the present research was to investigate the accuracy of people's anticipations of post-decisional emotions and also the influence of these emotional anticipations on people's observed decision-behaviours. In what follows, I outline the hypotheses of the present research again and then summarise the empirical findings that are relevant to each one of them.

First hypothesis. When people are focused on the possible outcomes of their decisions or choices, their predictions of the emotions that they will experience when they receive the outcomes of their decisions are overpredictions.

This hypothesis is grounded in the research on "affective forecasting" (see Chapter 2; section 2). This research has provided ample empirical support for the claim that people generally exaggerate when they predict what their emotional reactions to future events will be, or when they predict how easy or difficult they will find it to adapt to changes in their current life circumstances.

The present research provided empirical support for this hypothesis. Participants who found out that they would have done worse had they chosen a different version of an anagram-solving task overpredicted their happiness and participants who found out that they would have done better overpredicted their frustration (Study 1). Likewise, participants who received surprising rejections in an UG negotiation task overpredicted their frustration (Study 6) and, in general, negotiators overpredicted the post-negotiation emotions that the negotiation outcome triggered (Study 6). Finally, students who received surprisingly good marks for the coursework that they submitted overpredicted their happiness (Study 7) and, in general, students overpredicted the emotions that receiving their coursework marks triggered (Study 7).

Second hypothesis. When people put the possible outcomes of their decisions in a broader context (i.e. when they see them as sources of pleasure or pain among other sources of pleasure or pain that they are face with in their everyday lives) their predictions of the emotions that they will experience when they receive the outcomes of their decisions are accurate.

This hypothesis is grounded in the research on focalism (see Chapter 2; section 3.4). Focalism is one of the mechanisms (among others; see Chapter 2; sections 3.1. to 3.3.) to which affective mispredictions have been attributed. According to Wilson et al. (2000), who provided initial empirical support for the existence and the influence of focalism in affective self-prediction, “Any time people think about how an event in the future or the past will or did affect them, they are likely to focus on that event too much and not enough on other occurrences that will or did occupy their thoughts and influence their behaviour” (Wilson et al., 2000; p. 835). The implication for decision-related emotions is that if people are led to realise that the outcomes of their decisions are embedded in a big mosaic of unrelated occurrences that will compete for their attention when the decision outcomes materialise, their predictions of the intensity of the decision-related emotions will not be exaggerated (i.e. they will be accurate).

The present research provided empirical support for this hypothesis too. Participants who received surprising rejections at an UG negotiation task predicted their frustration accurately when they put the negotiation outcomes in the broader perspective of their everyday lives (Study 6). More generally, negotiators predicted accurately the post-negotiation emotions that the negotiation outcome triggered when the latter was contextualised (Study 6). Likewise, students who received surprisingly good marks for the coursework that they submitted predicted their happiness accurately when they put the coursework feedback in the broader

perspective of their everyday lives (Study 7). As was the case with the negotiators, students predicted accurately the emotions that the dissemination of their coursework marks triggered when the latter was contextualised (Study 7).

Moreover, the present research documented findings that extend this hypothesis to affective reactions after recalled decisions. Participants associated less pain with past unfortunate decisions of theirs when they recalled these decisions in the broader perspective of their lives at the time when the decisions were made than when they recalled them in the absence of this context (Studies 8 and 9).

Third hypothesis. People actively try to minimise the pain and maximise the pleasure that they think they will experience when they receive the outcomes of their decisions only when they are narrowly focused on the decision outcomes in question. When people put the possible outcomes of their decisions in a broader context (i.e. when they see them as sources of pleasure or pain among other sources of pleasure or pain that they are face with in their everyday lives) their effort to minimise post-decisional pain and maximise post-decisional pleasure subsides.

This hypothesis is grounded in the models of post-decisional affective reactions (RT; DT; and DAT – SEP; see Chapter 1) and also in the research on focalism. RT, DT, and DAT – SEP share the postulate that when people make decisions, they anticipate their post-decisional emotions and they actively try to maximise the pleasure and minimise the pain that they will experience once the outcomes of their decisions materialise. Moreover, these models also claim that these anticipated emotions depend not only on the factual valence of the outcomes, but also on the counterfactual comparisons between obtained and foregone outcomes, and chosen and rejected decision behaviours.

However, if people exaggerate their affective reactions to future events, it follows that their pre-decisional anticipations of the post-decisional emotional impact of the outcomes of their decisions are exaggerated too. If this is the case, then it can be argued that these affective anticipations have an influence on decision behaviour exactly because they are exaggerated. Therefore, if in a given decision situation people are led to contextualise their decision outcomes, their anticipation of the impact of these outcomes on their overall well-being will be accurate and, thereby, these anticipations will be less influential on decision behaviour.

The present research provided empirical support for this hypothesis too. Participants who were faced with a risky choice between two investment options chose the investment that minimised their post-decisional regret when they were narrowly focused on the investment outcomes, but they chose the investment that minimised risk when they saw the investment within the context of their everyday lives (Study 3). Likewise, negotiation behaviour of participants who were narrowly focused on the negotiation outcome was a function of their anticipated pleasure and pain after a possible failure of the negotiation, but a function of their effort to make the most out of the negotiation when they saw its outcome within the context of their everyday lives (Study 6).

2. A secondary hypothesis of this research: Overview of the empirical findings

In the investigation of the effects of focalism on affective predictions and decision behaviours I used the following manipulation to contextualise participants' decisions: Participants were asked to complete diaries of events contemporaneous with their decisions. These diaries were prospective when the consequences of the decision would show in the future (Studies 2 to 7), or retrospective when the consequences of the decision had already shown (Studies 8 to 10).

This manipulation was first introduced as a way to contextualise people's judgements by Wilson and his colleagues (Wilson et al., 2000). In fact, these researchers showed that diary completion is an effective way to attenuate the effects of focalism on people's prospective judgements of affect. Regarding the exact way the diaries function, Wilson et al. (2000) suggested two possibilities. First, asking people to complete a diary may make them realise that other events would occupy their thoughts and thereby distract them from thinking about the target event. As a result, they would ensure that they moderate their affective forecasts that they are asked to generate (the "distraction hypothesis"). Second, people may focus their attention on the affective consequences of the other events rather than at the likelihood of being distracted by those events. In other words, they may believe that the affective consequences of the focal event will be diluted or cancelled out by the affective consequences of the other events occurring around the same time. As a result, they attenuate their affective forecasts of the focal event (the "affective competition hypothesis").

In their original investigation of prospective focalism, Wilson et al. (2000) found no empirical support for the "affective competition hypothesis". Consequently, they concluded that the "distraction hypothesis" provided the more plausible account of their diary manipulation. I also failed to obtain evidence in support of the affective competition hypothesis: The valence of the diaries that participants completed before choosing between a regret minimising and a risk minimising investment option had no effect on their investment intentions (Study 3).

However, in the investigation of retrospective focalism, I found that participants who completed a diary did not report lower post-decisional pain than participants whose affective processing was simply interfered with with an anagram-solving task (Study 9). In other words, the empirical results supported an

“interference hypothesis” at the expense of the distraction hypothesis. An additional (ad hoc) empirical test of the interference hypothesis provided converging evidence that the same pattern arises in prospective judgements of affect as well – thus consolidating the effect further.

Moreover, when I assumed a dual route model for the emergence of the effects of distraction and interference, I failed to find any supportive empirical evidence (Study 10). Finally, participants who underwent identical experimental tasks and procedure, but who were monitoring their moods throughout the experiment also reported lower post-decisional emotions than participants who were focused on the recalled decision (Study 10). This last finding provided further empirical support for the interference hypothesis: Any mental activity that is interposed between decision recall and assessment of post-decisional emotions associated with it serves to reduce the levels of these emotions^{1, 2}.

3. Theoretical implications and directions for future research

The empirical findings that were outlined above carry theoretical implications. These implications concern, first, the research on modelling of anticipated post-decisional affect and, second, the research on affective self-forecasting. In what follows, I consider these implications separately for each one of these research areas.

¹ An interesting experimental possibility is the following: Mental activities need not be interposed between decision recall and affective judgements; they can precede the recall / anticipation task.

² The fact that the interference hypothesis seems to provide a more parsimonious account for these observations does not automatically render the distraction account uninteresting. I agree with Gilbert’s stance that, after all, “... *lots* of things could lead people to moderate their predictions, and we wouldn’t expect a single mechanism [i.e. focalism] for all of them” (D. T. Gilbert, personal communication, December 11, 2003).

Modelling anticipated post-decisional affect: What about accuracy?

Over the last 15 years, the modelling of anticipated post-decisional emotional reactions has become one of the “hot” topics in the JDM area. Evidence for this is the attention that the relevant models, especially RT, have been attracting among researchers. The immediate output of this upsurge in research is ample empirical evidence that anticipated emotions do indeed have an influence on people’s decision behaviours. Moreover, the latest reviews of the empirical work and theoretical directions of the JDM field explicitly refer to the topic (Hastie, 2001; Mellers et al., 1998), and there has been at least one review devoted to RT (Zeelenberg, 1999a).

However, this proliferation of empirical research has not been accompanied by further theoretical development of the initial models. This lag between empirical data and their formal modelling has resulted in the following paradox: Whereas the interest in anticipated post-decisional emotions was sparked by formal conceptualisations of the function of these emotions, these conceptualisations do not seem to have been affected by the empirical findings – at least not to the extent that one would hope. The area, therefore, seems to be at a loss for an overarching formal model.

The first attempt at such a model was the one by Mellers’ and her colleagues, who introduced DAT – SEP (Mellers, 2000; Mellers et al., 1997; Mellers et al., 1999). Their model suggests an interesting conceptual combination of regret and disappointment effects under the generic term “subjective expected (dis-) pleasure”; however, this combination is still in need of empirical validation. The second interesting suggestion that Mellers and her colleagues have put forward is that, if decision-makers actually generate affective forecasts and if these forecasts are influential on their subsequent behaviour, then these forecasts ought to be

accurate. In other words, what is the point in the decision-makers predicting how they will be feeling after receiving the outcomes of their decisions, if these predictions are not in line with their actual emotional experiences?

I believe that the accuracy question is an important but neglected one. I agree with Mellers' stance that, should affective forecasts be granted a functional role to play in people's decisions (cf. Roese, 1997; Zeelenberg, 1999b), they should also be accurate. However, the question of whether people are accurate forecasters of their own decision-related emotional reactions is in need of empirical investigation. The present research should be regarded as a first systematic attempt to address this question. The findings show that decision-makers are generally inaccurate – at least, in the decision circumstances that were studied. Moreover, their inaccuracies are more often than not overpredictions: Decision-makers exaggerate the impact of some decision outcomes rather than others on their subjective well-being.

This pattern of findings does not stand in isolation in the literature. Research on affective self-forecasting (i.e. forecasting of future affective states, not necessarily related to the reception of some decision outcomes) has also documented that people in general exaggerate the degree to which their subjective well-being will be elevated by positive events or be depressed by negative events that will happen to them in the future. This pattern in affective self-prediction has recently been termed "impact bias" – in other words, people exaggerate the impact of future events on their well-being, hence the bias (for a review, see Wilson & Gilbert, 2003).

These empirical results need further replication – not least because the research on affective self-forecasting does not explicitly address the question of accuracy of post-decisional affective anticipations. However, taken together with the results of the present investigation, the above pattern of affective overprediction

appears to conflict with the assumption of forecast accuracy put forward by Mellers. In turn, if this is the case, then the claimed functional and rational aspect of affective anticipations (e.g. Roese, 1997; Zeelenberg, 1999b) need to be re-considered.

The impact bias in affective self-forecasting: What about optimism?

When people think about the future, they exaggerate the impact of future events on their subjective well-being – hence the overpredictions and the emergence of the impact bias. The bias has been documented in people's affective self-predictions regarding both positive (e.g. Mitchell et al., 1997) and negative future events (e.g. Gilbert et al., 1998).

The presence of the bias in prospective judgements about positive events is in agreement with the research on optimism. This research has identified two types of optimism (for a review of optimism, see Armor & Taylor, 1998): “Dispositional optimism” refers to the observation that some people are more optimistic than others. “Optimism in specific expectations” refers to the following inter-related observations:

a.) People think that they would receive better outcomes than the average person would receive, or has been shown to receive, in the same situation. For instance, Sherman (1980) found that people thought that they would behave in a more socially desirable way in given situations than others who have been in the same situations. In his programmatic research on optimism, Weinstein (e.g. 1980; 1982; 1984) found that people believe that future health problems will affect them less than the average person and that the future will be generally rosy, especially for themselves. Since these self-perceptions totally disregard actual base rate information about the events in question, Weinstein termed this optimism

“unrealistic” (for a review of the research on “unrealistic optimism”, see Weinstein & Klein, 1996).

b.) People (“targets”) are more optimistic about their future achievements than observers are about the targets’ achievements. For instance, Buehler, Griffin, and Ross (1994) found that when target-participants were asked to judge how much time it would take them to finish an assignment, they generated predictions that were more optimistic than the predictions of observer-participants about the targets’ times. Buehler, Griffin, and Ross (1995) also found that when target-participants were asked to judge the future course of their personal relationships, they judged these relationships as much better and much more likely to remain stable over time than observer-participants judged those targets’ relationships.

When people judge that a future event will boost their well-being more than it actually does, the overprediction is in line with these optimistic self-perceptions: Since the future will be good, especially for one’s self, a good event cannot have but a big positive impact – hence the misprediction. However, when people judge that a future event will be more damaging to their well-being than it actually is, the overprediction seems to conflict with the optimistic self-perceptions outlined above: Since the future will be good, especially for one’s self, how can an isolated bad event have a big negative impact?

I suggest the following explanation for this conundrum: People overpredict the impact of negative events on their well-being exactly because they perceive a rosy future for themselves – in other words, the impact bias for negative events is due to a contrast effect³. For instance, when a participant predicts her emotional reactions to a failed negotiation, the failure is (or is perceived as) an infrequent

³ For a discussion of contrast effects in social judgements see Sherif and Hovland (1961), Sherif and Sherif (1967), and Sherif, Sherif, and Nebergall (1965).

negative event embedded in an otherwise rosy perspective. The fact that this event has been singled out in the experimental situation contributes to the perception of its affective consequences as worse than they will actually be.

This explanation does not conflict with the focalism account of affective mispredictions. The impact bias is observed when people think about the target event in isolation – in other words, when they are focused on it. When people contextualise the target event (for instance, when they are asked to complete a diary of other events that are bound to happen concurrently with the target event) the impact bias is eliminated. Likewise, Weinstein and Lachendro (1982) found that when participants were asked to think carefully about others' circumstances, unrealistic optimism regarding personal future misfortunes was reduced. Weinstein and Lachendro (1982) concluded that unrealistic optimism is not due to the lack of relevant information about others, but to people's egocentrism. In other words, self-optimism arises because people are focused on their own circumstances, thereby neglecting others' circumstances. Consequently, when people are de-focused from themselves (for instance, when they are asked to think in detail about others) unrealistic optimism is attenuated.

Weinstein and Lachendro's (1982) manipulation and findings bear strong resemblances to the de-focusing manipulations and findings of the present research. Therefore, in addition to the distinction between prospective and retrospective focalism, I propose a distinction between "intra-individual" and "inter-individual" focalism. The former is the focalism introduced by Wilson and his colleagues and tested in the present research. The latter is the focalism introduced by Schkade and Kahneman (1998) and tested by Weinstein and Lachendro (1982) in the context of unrealistic optimism. These conjectures regarding the link between optimism and focalism await empirical testing.

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