How do you feel now? On the perceptual distortion of extremely recent changes in anger

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HIGHLIGHTS
• We examine affect-based retrospective biases for extremely recent events.
• We show that people show systematic biases in assessment of recent changes in anger.
• We provide insight into revenge and its relation to in-group dynamics.

ABSTRACT
Previous research on retrospective biases in emotion has been largely concerned with mistakes that are made when people are asked to recall temporally distant affective experiences (e.g. those that occurred weeks or months ago). However, far less is known about people’s abilities to accurately track extremely recent shifts in affective experience. Across three experiments, we show that people consistently distort perception of a very recent change in anger after being reminded of a historical act of revenge (i.e. the assassination of Osama bin Laden). Consistent with the implications of the “revenge paradox” (Carlsmith, Wilson, & Gilbert, 2008) these reminders made participants more angry. However, participants believed that this act of revenge had made them less angry—the exact opposite of what happened—provided that their psychological allegiance to the ingroup had been primed. We discuss the implications of our findings in previous research on the interconnections between emotional experience and social categorization processes (Mackie, Maimer, & Smith, 2009), as well as the role of revenge in protecting the interests of the ingroup (Fehr & Gachter, 2002).

Introduction
Research and theory on affective forecasting (Wilson, Wheatley, Meyers, Gilbert, & Axsom, 2000) have shown that people commit a multitude of errors when they are asked to predict their own emotions. In a study reported by Gilbert, Pinel, Wilson, Blumberg, and Wheatley (1998), for example, researchers asked participants to predict how they would feel in the aftermath of a romantic breakup. Although participants correctly predicted that the breakup would have a negative impact, their actual reactions were neither as intense, nor as long lasting, as anticipated.

Interestingly, people often make these and other types of errors in relatively familiar domains, in which they have a great deal of personal experience. Consider the aforementioned study on romantic breakups by Gilbert et al. (1998). In that study, many participants (who were in their late teens or early twenties) were likely to have already experienced the pain of a romantic breakup. Intuitively, one might imagine that such experience might lead people to realize that their mental models about emotional breakups were wrong, thus allowing them to make more accurate predictions in the future. However, even if participants had gained such experience, the data provided little indication that they had been able to learn from it. Ayton, Pott, and Elwakili (2007) provided more direct evidence of people’s failure to learn from personal experience, when they asked participants to imagine how they would feel after failing a future driving test. Results showed that “experienced” individuals (i.e. those who had previously failed a driving test) were no more accurate in their predictions than those who had never experienced this event.

The affective forecasting literature thus suggests that people make the same types of mistakes over and over again. This state of affairs calls to mind the perspective of Marcus Cicero, who wrote, “Any man can make mistakes, but only an idiot persists in his error.” With all due respect to Mr. Cicero, we would not go so far to suggest that persistent error in affective forecasting represents a lack of intelligence per se. Rather, such mistakes can often reflect foundational limitations in human information processing (cf. Wilson & Brekke, 1994). Moreover, people can sometimes be accurate in their affective forecasts (Wilson & Gilbert, 2003) and there are some conditions in which people can...
learn from their own previous emotional history (Brown & McConnell, 2011). Nevertheless, given that people often make the same mistakes (even in familiar domains), this raises an important question: what makes it so hard to learn from personal experience?

Currently, the most popular explanation is that such errors are attributable to long-term memory biases. In particular, just as people often manifest errors when they attempt to reconstruct the past (Schacter, 2001) this penchant for error has been shown to extend to cases in which people recall their own emotional histories (for reviews, see Levine, Lench, & Safer, 2009; Robinson & Clore, 2002). For example, suppose that people were, in fact, capable of correctly recalling that the pain of their romantic breakup was much shorter than originally anticipated. If so, they could use these accurate memories as a learning experience, increasing the chance that they would make better predictions in the future. However, given that we often distort memories of our own past, this could perpetuate errors when we attempt to forecast the future. Stated another way, people are, in a sense, “learning from the past”. However, the problem lies in the fact that they are remembering the wrong past, one that reflects a memory-based distortion of what really happened.

On the assessment of extremely recent changes in mood

In nearly all of the research on retrospective memories of emotion, there is considerable delay between (a) the event originally occurred and (b) when retrospective retrieval occurs, often on the order of days, weeks, or even months (cf. Levine et al., 2009). Given the well-known limitations of long-term memory (see above), it is not surprising to see that errors in emotional memories are so common. However, it is possible that people might show greater accuracy under shorter time frames?

Few studies have examined short-term biases in affective retrospection, but Van Boven and Robinson (2012) represents one notable exception. When male and female participants were asked to recall the intensity of a very recent affective experience (i.e. that happened twenty minutes earlier), researchers observed stereotypic biases in the judged intensity of affect, such that (a) men, compared to women, judged their anger as more intense, and (b) women, compared to men, judged their sadness as more intense. Notably, these biases were only evident when, at the time of recall, participants were placed under cognitive load, or when the gender stereotype had been experimentally primed.

Given the short time period involved in the Van Boven and Robinson (2012) paradigm, it seems unlikely that the observed errors were due to biases in long term memory. Rather, it seems more likely that their results reflected a bias in how participants interpreted their own emotional experience. In particular, given that emotional experience is often somewhat ambiguous (cf. Bem, 1972), inferences about previous emotional change may involve some degree of social construction (Martin & Tesser, 1992), in which people rely on the implications of any relevant mental constructs that might be accessible at the time of judgment (cf. Wyer & Srull, 1989). The plausibility of this explanation is bolstered further by the fact that these biases emerged when resources were scarce, or when the relevant (gender-based) expectations had recently been primed, which are precisely the kinds of conditions under which heuristic, accessibility-driven processing is most likely to occur (Higgins & Bargh, 1987).

On the possibility of extremely short term biases in emotional valence

Although Van Boven and Robinson’s (2012) findings are important, they demonstrated these biases in the realm of intensity: “how much” of a given emotion participants had felt. Precise recollection of how much emotion one felt in the past (e.g. “exactly how sad were you after watching that movie?”) may require cognitive effort, something that people may not have the ability/motivation to exert (Wilson & Brekke, 1994). Relevant, too, is the fact that many of the affective forecasting errors demonstrated in the literature bear on people’s inabilities to predict emotional intensity (cf. Wilson & Gilbert, 2003). In general, then, accurate appraisal of emotional intensity may simply be a relatively difficult thing to do.

However, what about valence? Suppose that a recent event put you in a bad mood. When reflecting upon this event, would you be accurate in assessing this change in affect? Intuition suggests that, at least for the basic issue of valence—did X make you feel better, or worse?—recollection should be accurate. Moreover, such ability should be especially good if the event in question had happened just moments ago. In this article, we offer a counterintuitive prediction, in that people can make a mistake of valence, even for extremely recent events. To our knowledge, ours is the first program of research to address this kind of error. The goal of this article was to demonstrate the existence of these biases as well as provide some insight into the mechanisms by which they occur.

The present research

Our curiosity in these matters was stimulated by a line of research by Carlsmith, Wilson, and Gilbert (2008). As these researchers demonstrate, people expect that revenge feels good (“revenge is sweet”). However, this is a domain in which people make a blatant affective forecasting error. In particular, revenge actually makes people feel worse, not better. According to Carlsmith et al. (2008), this is because retribution against the transgressor can trigger thoughts about the misdeeds for which that person is being punished in the first place. This, in turn, can make people feel even worse than they otherwise would. In other words, “revenge can prolong peoples’ hedonic reactions to a transgression because punishing others can cause people to continue to think about (rather than to forget) those whom they have punished” (Carlsmith et al., 2008, p. 1324). It is important to emphasize, however, that the Carlsmith et al. (2008) model stipulates that such negative affect is stimulated by the thoughts about the original transgression, not by the act of revenge itself.

In this article, we were interested in an issue that was not addressed by Carlsmith et al. (2008): would people recognize that a revenge-related event had just made them feel worse? Aside from the intuitive sense that people are able to figure this out, research suggests that people often notice when expectations have been violated (Roese & Sherman, 2007). Hence, people might be able to ascertain that revenge had made them angry, precisely because this change was unexpected. However, such optimism might be tempered by a long line of research showing that self introspection is more complex, and more prone to error, than one might imagine (Bem, 1972; Chaiken & Baldwin, 1981; Dijksterhuis, Bos, Nordgren, & van Baaren, 2006; Salancik & Conway, 1975; Stanovich, 2013; Wilson & Schooler, 2008; Wilson & Stone, 1985). Hence, although one could make a case for optimism in terms of retrospective accuracy in our paradigm, this literature led us to take a more pessimistic position. We predicted that participants, despite the seemingly “optimal” conditions for accuracy, would mistakenly conclude that exposure to an act of revenge made them feel better, even though they actually felt worse.

On the relevance of anger to the revenge paradox

In the introduction of their paper, Carlsmith et al. (2008) place considerable emphasis on cultural expectations as to the cathartic potential of revenge, especially as it pertains to anger. In particular, they suggest that “there is widespread acceptance of the notion that aggression, either directed against the target of one’s ire or displaced to some other object, relieves the tension, and thus the anger, that had been pent up inside” (Carlsmith et al., 2008, p. 1316). Hence, even though Carlsmith et al. (2008) sometimes frame the revenge paradox in global terms (e.g. as a general expectation to “feel better”), these considerations suggests that these expectations are rooted in an expected reduction
in negative affect—and anger in particular—rather than an increase in positive affect.

As for the actual consequences of revenge, the Carlsmith et al. (2008) model suggests, too, that anger may often be relevant. There is a wealth of evidence suggesting that people feel anger when they perceive that others have violated social norm(s), especially when those actions harm others (e.g., Alickie, 2000; Goldberg, Lerner, & Tetlock, 1999; Russell & Giner-Sorolla, 2011; Schlenker, 1997; Tetlock et al., 2007; Weiner, 1995). This is not to say that perception of wrongdoing cannot trigger other types of negative feelings. However, across all of the studies presented in this article, the effects obtained with anger were, by far, the most stable. The stability of our findings with respect to anger is consistent with the fact that intentional social norm violations, especially when such actions harm the self and/or ingroup members, should consistently elicit increased feelings of anger. Ironically, then, the type of emotion for which people might most consistently expect hedonic benefit—a cathartic-like reduction of anger—represents the very type of emotion domain along which people are actually likely to feel worse.

Summary of research goals

The present research adds to existing knowledge about retrospective biases in emotion (Levine et al., 2009) with particular emphasis on the possibility that people might show lack of insight into extremely recent changes in mood. In principle, this research could have been conducted in several different substantive domains. However, in order to generate a persuasive set of findings, we choose one domain, and continue this focus across a programmatic line of studies. The affective dynamics associated with revenge (Carlsmith et al., 2008) represented an excellent match to our objectives, for several reasons.

For one thing, we wished to “set the bar” low to facilitate the chance that participants might maintain some degree of retrospective accuracy. To our knowledge, researchers have identified only one domain in which people hold incorrect expectations about valence, and that is revenge. Moreover, the present research allowed us to show retrospective biases with a rather familiar type of emotion. It would be one thing to show retrospective biases for relatively subtle/esoteric emotional experiences, such as ennui or Schadenfreude. It is quite another thing, however, to demonstrate bias for anger, arguably one of the least subtle of all human emotions.

Finally, apart from retrospective assessment, our research provides more insight into the dynamics of revenge, a topic of scholarly interest in its own right (de Quervain et al., 2004). Hence, while our main goal was to learn more about retrospective biases, a secondary but still important goal was to learn more about revenge. As we shall show, this endeavor builds upon but also adds to previous research on the interconnections between emotional experience and social categorization/social identity processes (Mackie, Maimer, & Smith, 2009) and highlights the role of revenge in protecting the interests of the ingroup (cf. Fehr & Gächter, 2002).1

Preliminary Study One

The killing of Osama bin Laden was widely perceived by many Americans as justifiable payback for his role in 9/11 attacks (Bowden, 2012). Moreover, although President Obama did not actually use the word “revenge” when announcing bin Laden’s death, his description leaves little doubt that bin Laden’s death was retaliation for bin Laden’s earlier attacks: “On nights like this one, we can say to those families who lost loved ones to al Qaeda’s terror: justice has been done”. It is safe to say, then, that thoughts about bin Laden are associated with people’s memories of the 9/11 attacks, and vice versa. Given that Americans are still likely to associate a negative emotion with the 9/11 attacks (Lambert et al., 2010), the Carlsmith et al. (2008) model suggests that reminders about the killing of Osama bin Laden’s death would actually make people feel worse, not better.

We conducted an initial preliminary study to validate this assumption. Using a sample of American residents drawn from the MTurk website (N = 232), participants were randomly assigned to read a short historical account of the Olympic Games devoid of any political content or, alternatively, a passage of similar length which described the main events surrounding the capture and death of Osama bin Laden (e.g., “Osama bin Laden, the mastermind of the most devastating attack on American soil in modern times and the most hunted man in the world, was killed in a firefight with United States forces in Pakistan.”). Afterwards, all participants completed a standard mood inventory, where they rated their ambient mood along a scale that ranged from 0 to 6. Our primary interest in this study was in constructing a composite measure of anger, based on an average of angry, mad, irritated, and irate (alpha = .80). Consistent with the framework offered by Carlsmith et al. (2008), we found greater anger among participants who were reminded (vs. not reminded) of the revenge killing of bin Laden, Ms = 2.38 vs. 1.51, F(1, 230) = 38.38, p < .001, r² = .14.2

Preliminary Study Two

The results from our first preliminary study represents a conceptual extension of the Carlsmith et al. (2008) model, showing that an after-the-fact reminder of the bin Laden assassination made participants feel worse. But why, then, do people believe revenge to have positive hedonic benefit? Social anthropologists have speculated that revenge plays an important functional role, to (a) punish those who threaten ingroup interests and to (b) dissuade others from considering such harm in the future (de Quervain et al., 2004; Fehr & Fischbacher, 2003; Fehr & Gächter, 2002). This explains not only why Americans might be motivated to take revenge on bin Laden, but also why they would expect hedonic benefit from doing so.

As work in social identity theory has shown, the tendency to support the ingroup represents a fairly robust effect, as this extends to many different types of ingroups, including one’s own country (Tajfel, 1981). Nevertheless, the strength of ingroup favoritism can vary across situations as well as across individuals (cf. Brewer, 1999; Hertel & Kerr, 2001). This point is relevant to current concerns because some of our additional pilot work in this area with a separate group of participants (Eadeh, Peak, & Lambert, 2013) revealed a tendency for many—but not all—of our participants to frame the assassination of bin Laden in positive terms, using language (e.g. “I’m proud of America’s ability to protect itself and smash those who would seek to harm us”) reminiscent of the kind of unabashed, militaristically-themed patriotism often associated with right-wing authoritarianism (Altemeyer, 1988). In other cases, however, participants framed the event in a more negative way, such as questioning whether the use of force in this particular context was justified (e.g. “The United States has no right to take matters in their own hands”). Rather than representing a liability in our research, we saw this as an opportunity to gain some causal leverage in understanding the dynamics underlying subjective beliefs about revenge. In particular, if (a) the expected hedonic benefits of revenge are rooted in protecting ingroup interests (Fehr & Gächter, 2002), then (b) these

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1 The chronology of data collection for all studies reported in this article is as follows: (a) Preliminary Study 1: July 2011 to August 2011; (b) Preliminary Study 2: January 2013; (c) Experiment 1: December 2011; (d) Experiment 2: January 2013; (e) Experiment 3: January 2013; (f) Supplemental study: June 2013.

2 Carlsmith et al. (2008) suggest that the paradoxical downside of revenge—the capacity of such acts to make people feel worse—is restricted only to those participants who personally carried out the act of revenge. At first blush, this might seem to suggest that we would not obtain analogous effects in our paradigm, given that none of our participants were personally involved in taking retribution on Osama bin Laden. However, the mission to assassinate bin Laden was clearly taken on behalf of the American public. Hence, even though very few Americans played any direct role in the act of revenge, they were almost certainly likely to be self-involved on a psychological level.
expectations should be stronger if attitudes towards the relevant ingroup were salient than if they were not.

To test these predictions, a separate sample of American participants (N = 60) were first asked to indicate their agreement with a randomized series of statements along a scale ranging from 1 to 7, with higher numbers indicating greater agreement. Participants in the control condition responded to a series of ten statements that pertained to their level of extraversion (e.g. In general, I would regard myself as an introvert; I tend to be outgoing and make friends easily). In contrast, participants in the experimental condition responded to an assortment of items which, collectively, implicated the kinds of aggressively-infused patriotism that is the signature of the authoritarian mindset (e.g. What our country needs most are disciplined citizens that follow national leaders in unity; I am proud to be an American; I love my country; What our country really needs is a strong, determined leader who will crush evil and take us back to our true path; Being an American is central to my sense of who I am).

Participants’ responses to these items were highly correlated and an index based on an average of all items (alpha = .87) yielded an average score of 5.20 indicating a moderate level of agreement with the ingroup prime items.

All participants were then asked, as part of an ostensibly unrelated task, to generate affective forecasts of how they would feel if they were reminded of bin Laden’s death. We were particularly interested in participants’ predictions with respect to how angry, as well as how mad, this reminder might make them feel. For anger, participants were asked to “imagine that you were vividly and powerfully reminded of the death of Osama bin Laden. To what extent would this type of reminder increase, decrease, or have no effect on the degree to which you feel ANGRY?” This was then followed by a scale that ranged from 0 (it would make me feel LESS angry) to 100 (it would make me feel MORE angry). Participants also completed a very similar task, this time substituting the word mad in the relevant part of the question. We then formed a composite measure of anger-related predictions based on an average of these two items (alpha = .87).

In this task, a rating of “50” indicated the belief that the bin Laden article had no effect, and ratings higher or lower than 50 indicated a belief that this would produce, respectively, an increase or decrease in the targeted emotion. Our framework suggests that participants would believe that the bin Laden reminder should make them less angry, but only if they had been assigned to the ingroup priming condition. As predicted, participants assigned to the ingroup prime condition believed that the bin Laden reminder would make them less angry (M = 35.95), which was not true of participants assigned to the control condition (M = 49.68), F(1, 56) = 6.47, p < .01, η² = .10. Follow-up analyses revealed that ratings in the ingroup prime condition were significantly below the midpoint, t(29) = −3.78, p < .001, but this was not true for the ratings made by control participants, t < 1.0.4

Summary of preliminary studies

The type of paradigm used in our preliminary studies was clearly different from the studies reported by Carlsmith et al. (2008). For example, our studies involved an after-the-fact reminder of an historical example of revenge, whereas Carlsmith et al. (2008) focused on self-initiated revenge against defectors in a modified prisoner’s dilemma game. Despite these and other differences, the results from the two foregoing studies provide convergence with the framework offered by Carlsmith et al. (2008). When participants were actually reminded of an act of revenge (cf. Preliminary Study One), this elicited a general increase in anger. When a separate group of participants (cf. Preliminary Study Two) was asked to predict how they would respond to such a reminder, participants expected that this would make them less angry. However, anticipated hedonic benefits of revenge were evident only when participants were assigned to an ingroup prime condition that was designed to activate the kinds of aggressive patriotism that we had observed in our previous pilot work (Eadeh et al., 2013).

This latter contingency is consistent with research on the functional role of revenge in protecting the interests of the ingroup (Fehr & Fischbacher, 2003; Fehr & Gächter, 2002). In other words, if the expected hedonic benefits of revenge are rooted in a desire to protect ingroup interests, then it stands to reason that these expectations might be especially strong if these motives had recently been primed than if they had not. This aspect of our findings is relevant, too, to a point we made in the context of discussing the results obtained by Van Boven and Robinson (2012). In particular, asking people to formulate an expectation about their future emotional state represents a kind of inference process that is likely to be affected, at least in part, by the cognitive accessibility of relevant mental constructs (Wyer & Srull, 1989).

Overview of main experiments

Our main studies investigated whether participants would be able to accurately detect their actual emotional response to a reminder of the bin Laden assassination. In other words, would these “freshly angered” participants be able to accurately recognize the true impact of revenge? These considerations are not easily addressed using between-subject designs. This is because between-subjects designs often require conflu- ent counterfactuals, in which people are asked to imagine how they might feel if they had been assigned to a different condition, and to contrast those feelings compared to their current feelings (e.g. compared to how you might have felt if you hadn't read the bin Laden article, do you think you might be more, or less angry, than you are feeling now?). Although this approach is interesting, forcing participants to consider such counterfactuals can yield findings that may be difficult to interpret.

An alternative approach is to use a within-subjects design that does not require participants to engage in counterfactual thinking. This is the approach we took here. In particular, we obtained a baseline measure of emotion prior to the target article along with an assessment of emotion immediately after reading the article. One advantage of such designs is that they allow greater leverage in tracking actual (i.e. pre vs. post) changes in emotional trajectory. After obtaining indices of participants’ actual change in affect, we asked participants to indicate how they think the article affected them. The basic elements of our design are summarized in Fig. 1.

Pre vs. post designs can, in some cases, raise potential concerns about two sources of measurement error (Shadish, Cook, & Campbell, 2002). In some scenarios, people might wish to appear consistent between pretest and posttest responses. Motivation for consistency could potentially hinder one’s ability to detect any true changes that may have occurred. Alternatively, people may possess a different motivation, to convey the impression that they changed, perhaps due to beliefs that their ratings are supposed to be different. In this latter case, this could produce an apparent shift in judgment, even in the absence of true, underlying change.

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3 The kind of beliefs/attitudes under concern here involves a blend of patriotism and certain facets of conservatism (i.e. right wing authoritarianism). Nevertheless, while acknowledging this overlap, it simply reflects a reality of contemporary American politics: in other words, the kind of militaristic-themed patriotism under concern here is, de facto, an intrinsic part of right wing authoritarianism, and vice versa. Hence, although it would

4 In theory, one could also conduct within-condition correlational analyses among participants who were assigned to the ingroup prime condition, to the extent that there would be some variation in participants’ level of agreement to the statements in question. However, when we conducted this sort of correlational analysis, we did not observe any consistent relationship between overall scores on this index of “aggressive patriotism” and the kinds of responses that participants gave later in this or any of the other studies in this paper. Hence, in the results to be reported ahead, our focus was on a group level of analyses, comparing and contrasting responses as a function of experimental condition (ingroup prime vs. control), with the prediction that participants would manifest stronger bias in affective forecasting in the former compared to the latter case.
There is a relatively straightforward way of ameliorating such concerns, and that is to use within-subject designs in combination with between-subjects designs (cf. Shadish et al., 2002). If one can show similar results using both designs, this reduces the probability that one’s findings reflect the emergence of such artifacts. This point is relevant to the present research because, in Preliminary Experiment One—which used a between-subjects design—we have already shown that a reminder of the bin Laden assassination made participants feel worse. Hence, this alleviates concern that our results represented some sort of spurious or artificial result of using a within-subject design.

On the distinction between actual vs. subjective change in emotional experience

Our measure of actual change is a statistical index, in which we, as researchers, calculate the change in mood from one point (pre-article) to another (post-article). One might wonder if participants, themselves, could engage in a self-initiated mental subtraction, in which they use their own responses to our task as a basis of inferring their emotional response to the bin Laden article (e.g. I rated my anger as a “4” on the pretest, a “6” on the posttest, so I guess the article made me more angry). However, our reading of the emotional introspection literature suggested that this sort of “mental subtraction” would probably be beyond the capabilities of most people, at least in the context of this study.

Two additional features of our design further minimized the chance of such mental subtraction. First, the experiment was administered on a computer, preventing participants from “flipping back” to previous ratings, as they could potentially do in a pencil and paper format. Second, our task presented a large number of individual emotion items in both the pre and the post-article task, and we employed two independent randomizations of order for each block of judgment. This meant that, for any given participant, the randomization that occurred for the pre-article block was different from the post-block items, and vice versa. Hence, although implementation of the pre and post-article measures of emotion allowed us, as researchers, to easily calculate emotional change, this is not something that participants would be able to easily complete, at least with any degree of precision.

A note on actual changes in positive emotion

As noted earlier, the main focus of the present research was on anger, as this was the type of emotional experience for which we predicted the most reliable contrast between actual vs. subjective change. In order to show that our findings were specific to anger (and not negative affect in general), we also report findings as they bear on actual vs. subjective changes with respect to anxiety and sadness.

Although we did not have any specific predictions with respect to positive emotions (e.g. feelings of pride, satisfaction, happiness), we also included measures of these emotional states for exploratory purposes. As it turned out, however, we failed to find any consistent pattern of results for this latter class of affective experience. Most important for present purposes, we were unable to find any consistent evidence that

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Incentives for retrospective accuracy (Experiments 2 and 3)

![Fig. 1. Schematic outline of experimental paradigm for Experiments 1–3.](image)

Experiment one

We had two predictions for this study. First, we predicted that, on average, participants would experience higher levels of anger after being reminded of the bin Laden assassination compared to how they felt before. Second, we predicted systematic bias in subjective change, such that participants would not necessarily be aware that their anger had increased. Given the results of Preliminary Study Two, we expected that this retrospective appraisal error would be most pronounced among participants whose psychological allegiance to the ingroup had been primed in an earlier task. Hence, our design allowed for the possibility that participants might show at least some accuracy in retrospective appraisal. However, if such accuracy did arise, we expected that it would be constrained to the control group.

Because the ingroup prime was positioned before participants read the article about Osama bin Laden (see Fig. 1), it is theoretically possible that this prime could affect actual responses. As it turned out, however, the ingroup prime affected subjective beliefs about changes in anger, but not actual changes in anger. That is, the actual effects of anger were the same, regardless of whether connections to the ingroup were primed or not. This aspect of our results is important, because it shows that priming the emotional schema has the capacity to change retrospective appraisals of emotion, independent of what actually happened. We shall discuss these considerations in more detail in the General discussion section.

Method

Participants and design

The study sample consisted of 129 American participants (62 males and 67 females) drawn from Amazon.com’s Mechanical Turk program. The design contained one between-subjects factor (patriotism prime vs. control) along with one within-subjects factor, pertaining to the timing of the two measures of mood (pre vs. post article). None of our reminder of the bin Laden assassination produced any reliable changes with respect to positive emotion. Given that our primary focus was on contrasting actual vs. subjective change in emotional experience, analyses of positive emotion were not actually germane to the present framework, in light of the fact that we were unable to find reliable evidence of actual change in the first place. For this reason, our primary analyses in all of the studies to be reported below focus on anger along with the correlated emotions of anxiety and sadness.5

5. The failure to find any consistent boosts in positive emotion following our reminder of the bin Laden assassination might seem puzzling, given that this mission was widely viewed as a successful military operation in which “justice was served” to a notorious terrorist threat. Although such puzzlement is understandable, one should keep in mind that this is the whole point of the Carlsmith et al. (2008) model. All of our intuitions strongly suggest that revenge should feel positive. However, our own data, coupled with that reported by Carlsmith et al. (2008), suggests that these intuitions may be erroneous. This is not to say that researchers will never have any chance of finding actual hedonic benefits of revenge. However, it may be much more difficult than one might think to find evidence of such effects.
effects were significantly contingent on gender. Thus, analyses are collapsed over this factor.

Baseline assessment of mood
At the beginning of the experimental session, participants completed the “Mood Questionnaire” task, accompanied by the following instructions: “In this initial task, we are interested in what sort of mood you might be in right now. In other words, what sorts of emotions and feelings are you experiencing at the moment? To assess these feelings, we are going to present a series of items on the screen. For each one, please indicate the extent to which it captures how you feel RIGHT NOW.” Participants then responded to a randomized series of 27 items: interested, bored, edgy, happy, proud, alert, irritated, satisfied, mad, upset, tense, sad, pleased, relaxed, unhappy, angry, apprehensive, determined, irate, dejected, anxious, comfortable, jittery, nervous, worried, confident, and calm. For each item, participants selected any number between 1 (not at all) and 6 (very much so) that best represented their current feelings. The primary focus of our research was on anger and thus our primary measure of baseline mood contained an average of angry, mad, irritable, and irate. However, we also formed composite measures of anxiety (based on an average of jittery, nervous, worried, anxious, tense, edgy, and apprehensive) as well as sadness (based on an average of sad and unhappy). All three of these indices had good levels of internal reliability, all alphas > .75.

Ingroup priming manipulation
The priming manipulation was identical to that used in Preliminary Study Two.

Target article
All participants were read a relatively short passage about the capture and death of Osama bin Laden. Aside from a few minor changes in wording (none of which affected its substance), this passage was identical to the passage that participants had read in Preliminary Study One.

Post-article assessment of mood
Immediately after reading the article, participants completed a second set of mood measures. The structure and format of this task was very similar to the first (pre-article) measure of mood, with slight modification. In particular, we were interested in capturing participants’ reactions to the newspaper article: Now that you have read that article, we would like you to indicate your emotional reaction to it. In other words, as you read the article, how did it make you feel? For each of the items that appear on the screen, select the response that best captures your emotional reaction to the article. Please note that, unlike the first emotion rating task, we are specifically interested in your emotional reaction to the article, in particular.

Our primary purpose was to compare baseline anger (as assessed in the pre-article measure) to post-article anger. For this reason, we constructed post-article composites using the same items that we used to construct the pre-article composites. We utilized a parallel approach to form pre/post measures of the other two indices (anxiety and sadness). As expected, given the short time span between these measures (along with the fact that they were constructed using the identical mood items), pre and post-measures of mood tended to be moderately correlated. This was true for the two sets of anger ($r = .24$) and sadness ($r = .33$) ratings, with slightly higher correlations in the case of anxiety ($r = .51$), all $ps < .01$.

Assessment of subjective beliefs about the emotional impact of the target article
Participants then completed two tasks, both of which were designed to assess the subjective impact of the bin Laden article on their emotions. The first task was designed to provide a global snapshot of how participants believed the article made them feel. The second task, which used more sensitive, emotion-specific assessment of change, was expected to provide more precise leverage on participants’ subjective assessments of change.

Global appraisals of changes in mood
In the first task, participants were presented with a series of six statements, each of which offered a different perspective in terms of the presumed impact of the article on emotion (i.e., it did not affect me at all; not sure; it produced a small amount of positive emotion; it produced a large amount of positive emotion; it produced a small amount of negative emotion; it produced a large amount of negative emotion). Participants were told to select the statement(s) that best captured how they thought the article had affected them and were explicitly told that they could select more than one statement if they wished.

Emotion-specific appraisals of change
A second task was included to provide more specific information as to emotion-specific changes in mood. In particular, we probed participants’ beliefs as to whether the article made them feel less vs. more of a particular emotion in question (or, alternatively, whether they believed that the article had no impact at all). Of particular interest was our assessment of anger. In this case, the question consisted of an initial prompt—Compared to how you felt at the beginning of the study, to what extent did the article make you feel angry?—followed by a scale that ranged from 0 (made me feel much LESS angry) to 10 (made me feel much MORE angry) with the midpoint (5) explicitly marked with “had no impact either way.” The identical format was used for assessments of sadness as well as anxiety, with the substitution of the relevant term (sad, anxious) in the relevant part of the question. Questions were presented in a different randomized order for each participant.

A note about the calculation of effect sizes
As Feingold (2009) notes, there is considerable variation in how researchers compute/report effect sizes for within subjects designs. For example, although many social scientists rely on the GLM procedure to generate effect size indices, this approach does not take into account autocorrelation (i.e., the relation between the pretest and posttest; Cohen, 1988). Hence, where relevant, the effect sizes to be reported below represent the adjusted values, after using the procedures recommended by Morris and DeShon (2002).

Results
Actual changes in mood
Consistent with predictions—and replicating Preliminary Study One—the bin Laden article evoked higher anger ratings on the post-test ($M = 2.09$) compared to the pre-test ($M = 1.68$), $F(1, 127) = 10.53$, $p < .001$, $\eta^2 = .08$. Hence, on the average, participants felt angrier after reading about the revenge killing of bin Laden compared to how they felt before. These effects were not contingent on the manipulation of ingroup salience. That is, participants were more angry after reading about the bin Laden article compared to how they felt beforehand, and this was true regardless of whether attitudes towards the ingroup were salient ($M_s = 1.90$ vs. 1.56) or not ($M_s = 2.30$ vs. 1.81), $F < 1.0$ for the interaction with the priming manipulation.$^6$

These changes were specific to anger. Participants did express slightly greater sadness after reading the article compared to how they felt before ($M_s = 2.08$ vs. 1.98), but this effect was not significant, $F < 1.0$.

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6 Closer inspection of these means reveals a tendency for participants to express generally lower levels of anger (regardless of whether these ratings were made before or after reading the bin Laden article) if they had been assigned to the ingroup (vs. control) prime, ($M_s = 1.73$ vs. 2.06), $F(1, 127) = 4.09$, $p = .05$. (A similar finding emerged in Experiment 2.) Although we did not predict this effect (and have no compelling explanation for why it arose), this effect has no statistical or theoretical bearing on the finding of main interest, which is that participants were more angry after reading the article about bin Laden compared to how they felt before, regardless of whether participants were assigned to the ingroup or control prime.
Additional analyses indicated a tendency for participants to be more anxious after reading the article compared to how they felt before (Ms = 2.23 vs. 2.04), F(1, 127) = 3.82, p < .05, ηp2 = .01. However, this effect disappeared once anger was controlled for, F < 1.0.

Descriptive analyses of participants’ overall beliefs about the emotional impact of the article

Recall that our first assessment of subjective belief asked participants to select among an array of statements as to how they think the article affected them (see above). As noted earlier, participants were told that they could select more than one of the statements if they wished. The majority of participants (n = 99; 75%) selected one statement, with some participants (n = 27; 21%) picking two. (Of the remaining participants, three participants picked three statements, and one participant picked all six.) This fact does not compromise the meaningfulness of counting the frequency that certain statements were chosen, as it meant only that the sum total of percentages in the analyses to follow somewhat exceeded 100%.

We anticipated that this task would provide only an initial snapshot of participants’ beliefs about their emotional reactions, and for this reason, we present these data for descriptive purposes only. (Preliminary analyses revealed no reliable contingency of these data on the ingroup prime, and hence we collapse over this variable here.) The relevant percentages are shown in Fig. 2. It is worth noting that the majority of participants believed the article affected them, insofar as relatively few participants selected the statement “did not affect me at all” or “not sure”.

Among the participants who believed the article affected them, our framework suggests that participants should be more likely to believe the article had induced positive, rather than negative, feelings. This was indeed the case. The most frequently chosen statement was “a small positive effect” (n = 56; 42%) with a sizeable number of participants selecting “a large positive effect” (n = 26; 20%). In contrast, fewer participants indicated that the article had impacted them negatively, and this was true for those indicating that it had produced “a small negative effect” (n = 39; 29%), with an even smaller number of participants indicating that it had produced “a large negative effect” (n = 11; 9%). Hence, even though there was some variability in subjective perceptions, the dominant trend was that the impact of the bin Laden article on their mood was positive. Indeed, collapsing over whether they believed the effect to be “small” or “large”, participants were much more likely to indicate that the article elicited positive as opposed to negative emotions (65% vs. 37%).

To reiterate, this task was deliberately designed to provide only global indications of subjective impact, and it may not have been particularly sensitive in terms of picking up on emotion-specific dynamics of retrospective assessment. (Such insensitivity could also have explained why the participants’ responses to this task did not vary as a function of the ingroup priming manipulation.) The emotion-specific assessment of change to be reported below was expected to yield more leverage in this regard.

Emotion-specific beliefs about change

Our second subjective change task employed a 0–10 “sliding scale” task, with the midpoint the (5) labeled “no impact either way”. Ratings higher or lower than that midpoint indicated that the article had made them feel more or less, respectively, of the targeted emotion. In Fig. 3, we present subjective changes in affect for anger, anxiety, and sadness as a function of experimental condition. The effects observed with anger were as predicted, with participants in the ingroup (vs. control) prime condition revealing a significantly stronger tendency to believe that the article made them feel less angry, (M = 4.21 vs. 5.15), F(1, 127) = 5.10, p < .05, ηp2 = .04. Follow-up analyses revealed that anger ratings were significantly below the midpoint of the scale in the ingroup prime condition, confirming our conclusion that participants in that condition believed that the article made them less angry, t(66) = −2.67 p < .05. Anger ratings were not significantly different from the scale midpoint in the control condition, t(61) < 1.0, p > .25.

As for sadness, the only reliable effect to emerge from these analyses was a tendency for participants to believe that the article made them less sad (M = 4.58), a rating which was significantly lower than the midpoint, t(128) = −2.05, p < .05. However, these ratings of sadness were not contingent on the ingroup prime, F < 1.0. Ratings of anxiety (M = 4.74) were not significantly different from the midpoint (p > .15) and the ingroup prime had no effect on these ratings, F < 1.0.

Discussion

These findings provide new insight into the limitations of people’s ability to accurately assess extremely recent changes in their affective experience. In the present study, participants’ actual mood clearly shifted in a more negative direction, especially with respect to higher levels of anger. But yet, immediately afterwards, participants expressed a modest but statistically significant belief that the article made them less angry, provided that they had been assigned to our ingroup prime condition. The fact that this “disconnect” was obtained among participants in this condition is consistent with our view that cultural expectations about the cathartic-like benefits of revenge are rooted in dynamics
associated with protection of the ingroup (de Quervain et al., 2004; Fehr & Fischbacher, 2003; Fehr & Gächter, 2002).

Experiment two

Insight into the dynamics of bias can be derived by systematically demonstrating the factors that can increase, as well as decrease, the likelihood of such bias. In Experiment 1, we identified one factor that can apparently increase retrospective bias in the realm of revenge: the salience of one's psychological allegiance to the ingroup. In Experiment 2, we sought to replicate this effect, by again showing greater likelihood of error in the presence of an ingroup prime.

However, Experiment 2 went beyond our earlier study, as we also included an additional manipulation that could potentially decrease the likelihood of bias. In particular, we randomly assigned participants to condition such that they either were, or were not, given explicit prompts to be accurate. This manipulation was based on the principle that inaccurate social perception can often be attributable to a lack of motivation to arrive at a correct answer (Kunda, 1990), and that bias can be potentially be minimized through the instantiation of explicit prompts for accuracy (e.g. Chaiken, Liberman, & Eagly, 1989; Fiske & Neuberg, 1990; Hart et al., 2009; Kunda, 1990; Neuberg, 1989; Thompson, Roman, Moskowitz, Chaiken, & Bargh, 1994).

In pursuing this goal, we wished to manipulate participants' motivation for retrospective accuracy, independent of how they actually responded to the bin Laden article. For this reason, our experimental manipulation of accuracy was positioned after participants had responded to the bin Laden article (i.e. after the second, post-article measurement of mood; cf. Fig. 1). This aspect of our design allowed us to rule out the possibility that this manipulation played some role at an earlier stage, such as increasing their on-line attentiveness to any ongoing shifts in mood. In summary, Experiment 2 employed an additional prompt at the beginning of the task. Half of the participants were randomly assigned to receive the same, relatively generic instructions used in Experiment 1. However, the other half of our participants were provided with an additional prompt at the beginning of the task:

In our research, we were interested in the extent to which people can accurately reflect on their previous emotional experiences. We were particularly interested in the extent to which people can correctly recall and track recent changes in their own mood. Hence, we would now like you to tell us a little more about your emotional reaction to the article you just read, about Osama bin Laden. As you answer these questions, do your best to provide the most accurate picture of how this article actually affected you.

After receiving these two types of instructions (accuracy vs. control), participants then completed a series of queries about subjective changes in emotion that were generally similar to that used in Experiment 1. However, we greatly expanded the number of questions used to assess such inferences about change. Four of these emotions/mood items pertained to anger (angry, irate, mad, and aggressive), five pertained to anxiety (anxious, afraid, nervous, uneasy, and worried), and three pertained to sadness (sad, dejected, and pessimistic). All of these composites were reliable (all alphas > .80). As in Experiment 1, these composite measures of subjective change were strongly correlated with one another (all ps < .01).

**Results**

Applying Experiment 1, participants’ level of anger immediately after reading the bin Laden article was higher (M = 21.84) compared to their level of anger just before doing so (M = 16.32), F (1, 108) = 7.03, p < .01, ηp² = .02. This within subject effect was the same regardless of whether participants had earlier been assigned to the ingroup prime (Ms = 17.88 vs. 13.56) or the control condition (Ms = 25.82 vs. 19.08), F < 1.0 for the moderating effect of priming condition. Because the accuracy manipulation occurred after the post-article measurement of mood (cf. Fig. 1), there was no reason to suspect that the change in anger would be contingent on the subsequent manipulation of instructional set, and no interaction of this sort was found, F < 1.0.

**Method**

**Participants and design**

A total of 112 American participants (47 males, and 65 female) were recruited through the MTurk website. The design included two between-subjects factors, including the same ingroup priming manipulation used in our first two studies as well as instructional set (accuracy vs. control), the latter of which was positioned in the context of the “subjective change” task. Once again, gender did not qualify any of the effects reported below and hence analyses are collapsed over this factor.

**Procedure**

Paralleling Experiment 1, participants were assigned to the ingroup (vs. control) prime condition, followed by (a) an initial mood assessment, (b) the bin Laden article, (c) post-article mood assessment, and (d) appraisals of subjective change in emotion. Also paralleling our first experiment, participants were given an explicit baseline during subjective ratings, as they indicated how the article affected their emotions relative to their feelings at the beginning of the experiment.

However, this procedure included a few modifications. This included minor changes in the specific items used to construct the pre and post-article measures of mood, which included our composite measures of (a) anger, which were based on an average of angry, mad, irate, and aggressive, (b) anxiety, based on an average of anxious, afraid, nervous, uneasy, and worried, and (c) sadness, based on an average of sad, dejected, and pessimistic. As in Experiment 1, all composites were highly reliable (all alphas > .80) and, as before, each pair of mood composites was significantly correlated with one another. All of these ratings were made along a scale ranging from 0 to 100; participants were explicitly told to select the midpoint of the scale (50) if they were not sure how they felt at that moment.

A more substantive change pertained to the instructions accompanying the subjective change task. Half of the participants were randomly assigned to receive the same, relatively generic instructions used in Experiment 1. However, the other half of our participants were provided with an additional prompt at the beginning of the task:

In our research, we were interested in the extent to which people can accurately reflect on their previous emotional experiences. We were particularly interested in the extent to which people can correctly recall and track recent changes in their own mood. Hence, we would now like you to tell us a little more about your emotional reaction to the article you just read, about Osama bin Laden. As you answer these questions, do your best to provide the most accurate picture of how this article actually affected you.

After receiving these two types of instructions (accuracy vs. control), participants then completed a series of queries about subjective changes in emotion that were generally similar to that used in Experiment 1. However, we greatly expanded the number of questions used to assess such inferences about change. Four of these emotions/mood items pertained to anger (angry, irate, mad, and aggressive), five pertained to anxiety (anxious, afraid, nervous, uneasy, and worried) and three pertained to sadness (sad, dejected, and pessimistic). All of these composites were reliable (all alphas > .85). As in Experiment 1, these composite measures of subjective change were strongly correlated with one another (all ps < .01).

**Results**

**Actual changes in mood**

Replicating Experiment 1, participants’ level of anger immediately after reading the bin Laden article was higher (M = 21.84) compared to their level of anger just before doing so (M = 16.32), F (1, 108) = 7.03, p < .01, ηp² = .02. This within subject effect was the same regardless of whether participants had earlier been assigned to the ingroup prime (Ms = 17.88 vs. 13.56) or the control condition (Ms = 25.82 vs. 19.08), F < 1.0 for the moderating effect of priming condition. Because the accuracy manipulation occurred after the post-article measurement of mood (cf. Fig. 1), there was no reason to suspect that the change in anger would be contingent on the subsequent manipulation of instructional set, and no interaction of this sort was found, F < 1.0.
No significant effects of any sort were found for actual changes in sadness or anxiety, all ps > .10.

Subjective changes in mood
As in Experiment 1, subjective beliefs about changes in mood were assessed using a 0–10 scale, with scores below vs. above the midpoint (5) indicating a belief that the article had produced a decrease or increase, respectively, of the mood in question. Setting aside (for the moment) the effects of our experimental manipulations, we found a general tendency for participants to believe that the bin Laden article had, in essence, put them in a better mood by reducing their level of negative affect. At this level of analysis, this effect appeared to generalize across the three different indices of negative mood, as seen by the fact that all three subjective change ratings were significantly lower than the scale midpoint (i.e. 5.0), including ratings of anger (M = 4.02), anxiety (M = 4.36), and sadness (M = 3.95), all three ps < .001 for the relevant t-test, using the midpoint value of 5.0 as the test value. Of more interest, however, was the potential contingency of these ratings on our experimental manipulations, including the accuracy manipulation as well as the ingroup prime. We consider each of these effects in turn below.

Effects of the accuracy manipulation
Did the accuracy manipulation alter the pattern of subjective judgments noted above? It did not. Indeed, ratings of anger were nearly identical, regardless of whether participants were assigned to the accuracy vs. control condition (Ms = 3.99 vs. 4.04), F < 1.0 for the effect of the accuracy manipulation. This consistency was also observed in the case of anxiety (Ms = 3.98 vs. 3.90), both Fs < 1.0. Nor did we observe any reliable interactions of these ratings on our experimental manipulations, including the accuracy manipulation in conjunction with the ingroup prime, all ps > .15.

Effects of the ingroup priming manipulation
Initial analyses revealed the same asymmetry involving anxiety that we observed in Experiment 1. In contrast to our anger effects, there was a small tendency for participants to believe that the bin Laden article had made them more anxious, regardless of whether participants were assigned to the accuracy vs. control condition (Ms = 4.42 vs. 4.28). Although this effect was not significant (p > .20), it meant that, as in Experiment 2, anxiety represented a suppressor variable (Cohen & Cohen, 1975), given that the priming manipulation had the opposite effect on anger. In the analyses to be reported below on anger, therefore, we statistically controlled for anxiety. In the case of anger, we observed, as predicted, a significant effect of the ingroup prime, such that participants exhibited a stronger tendency to believe that the bin Laden article had made them less angry if they had been assigned to the priming manipulation than if they had not (Msadjusted = 3.80 vs. 4.25), F (1, 107) = 3.96, p < .05, ɳ² = .04.

Follow-up analyses revealed that both of these adjusted anger ratings were significantly below the midpoint (5.0), indicating that participants believed their anger had been reduced, regardless of condition. Clearly, however, these beliefs were stronger among participants who had been assigned to the ingroup prime condition. (Analyses of subjective changes in anger without controlling for anxiety revealed a similar pattern of means across the ingroup vs. control condition [M = 3.87 vs. 4.18], but this difference was not reliable. F (1, 108) = .84, p > .25.) The effects of the prime on sadness were not reliable, regardless of whether anxiety was controlled for or not, both ps > .20. No significant effects were found with anxiety, and this was true regardless of whether anger and/or sadness were controlled for, all effects ns.

Discussion
Experiment 2 replicated several findings from Experiment 1. Exposure to the bin Laden article again produced a significant increase in actual anger relative to how participants felt prior to reading that passage. However, when participants were asked for their beliefs as to how their anger had shifted, they gave little indication of any awareness that the article might have increased their level of anger. Moreover, as in Experiment 1, participants’ distortion of their retrospective assessment of anger was most pronounced when ingroup attitudes had been primed, as such participants showed, on average, a tendency to assert that their level of anger had decreased, which is the opposite of what actually happened.

Experiment 2 went beyond Experiment 1 by also including an experimental manipulation—an explicit prompt for accuracy—in a desire to investigate whether the aforementioned bias in retrospective appraisal could be minimized or eliminated. Our inclusion of this prompt was driven by previous research, which suggested that increased motivation for accuracy may have the potential, at least, to reduce bias (Chaiken et al., 1989; Fiske & Neuberg, 1990; Hart et al., 2009; Kunda, 1990; Neuberg, 1989; Thompson et al., 1994). However, the inclusion of this accuracy prompt had little effect. In interpreting this null effect, it is important to keep in mind that the nature of our accuracy prompt was directly patterned after, and used wording that was virtually identical to, the accuracy instructions first used by Neuberg (1989) and that has been shown to be effective in numerous other studies in the literature (see above). Hence, this addresses concerns that there is something intrinsically weak about the kind of accuracy prompt used here.

The more relevant question, in our view, is why the accuracy prompt did not have any effects in this particular type of setting and for this particular type of bias. This way of framing the issues is helpful because previous research suggests that any given incentive for accuracy may, or may not, be effective, depending on the circumstances in which it is employed. In particular, explicit prompts to reduce bias may have limited efficacy to the extent that people are not aware that they may, in fact, be making an error (Wilson & Brekke, 1994). By extension, this raises the possibility that, in our own paradigm, our participants were not actually aware of their proclivity for bias in the first place. In other words, the bias observed in our research may reflect a failure of ability to arrive at a correct inference, as opposed to a failure in motivation (Wilson & Brekke, 1994). An alternate explanation, however, is that our accuracy prompts were simply too weak to produce any change in how participants responded. In our final study, we employed a more blatant attempt to reduce this retrospective bias.

Experiment three
In Experiment 3, we provided half of our participants with a forewarning of the revenge paradox, explicitly informing them, in advance, of the reason why supposedly “favorable” acts of revenge might lead people to ruminate on the misdeeds of the person against whom one is taking retribution. In essence, we were letting these participants know that revenge may not be so sweet after all. To the extent that this warning would be effective, this might make it more likely for participants to realize, in retrospect, that the reminder of the bin Laden article could have negative hedonic consequences.

The forewarning in question was extremely blatant. Indeed, short of literally telling participants, point blank, that they should expect to feel angry, it is hard to imagine how we could have been more explicit. For this very reason, we knew in advance that this manipulation carried the risk of a strong demand effect. In particular, in our effort to “debias” participants with a very blatant forewarning, we knew that any observed reduction in bias might simply be the result of participants correctly guessing the hypothesis of our study. As it turned out, however, we still continued to find evidence of the same, robust bias in retrospective assessment, despite the presence of this blatant forewarning.
Summary of design

As in Experiment 2, our design employed a 2 × 2 factorial design, involving the orthogonal manipulation of two independent variables. As before, one of these independent variables pertained to the presence vs. absence of the ingroup prime, using a methodology identical to that of our earlier studies. Once again, we predicted greater subjective bias for inferences of anger change if the salience of ingroup allegiance had been primed than if it had not. As in our earlier study, the other independent variable pertained to a manipulation that was employed in the service of testing whether the bias in question could be minimized or eliminated. In this experiment, however, we included a much more direct debiasing intervention, one that directly called participants’ attention to the revenge paradox (see ahead for details).

Method

Participants and design

A total of 151 American participants (71 male, 80 female) were recruited through the Mechanical Turk website. There were two between-subjects factors, pertaining to (a) the presence (vs. absence) of the ingroup prime at the beginning of the study, as well as (b) the presence (vs. absence) of the forewarning about the revenge paradox provided in the context of the subjective belief task. As in our earlier studies, none of our effects were contingent on gender.

Procedure

With only one exception, all of the elements of the study were identical to Experiment 2. The one exception concerned the nature of the experimental manipulation of instructional set, just prior to the task in which participants were asked to express their subjective beliefs about change. Half of the participants were given the same generic instructions as those received by participants in the control condition of Experiment 2. The other half of the participants were given the following instructions:

Previous research has shown that revenge can sometimes lead people to think about the previous misdeeds/crimes of the person being punished. In this experiment, this suggests that prompting people to think about the assassination of Osama bin Laden might remind them of the various acts of terrorism for which he was responsible, such as his role in the attacks on the World Trade Center on September 11, 2001.

Participants were then explicitly asked to indicate the extent to which “reading the article about the death of bin Laden reminded you of his role in committing previous terrorist acts, such as the 9/11 attacks”. Participants were asked to render their judgments along a scale that ranged from 0 (not at all) to 100 (very much so). Analyses of participants’ response to this scale indicated a dominant tendency for our participants to affirm that the article had, in fact, induced them to think about the 9/11 attacks, as seen by the fact that the median response was extremely high (M = 77.0). Following this task, all participants were asked to complete the same block of subjective change ratings as was used in Experiment 2.

Results

Actual change in mood

Replicating Experiments 1 and 2, participants’ level of anger immediately after reading the bin Laden article was higher (M = 22.88) compared to their level of anger just before doing so (M = 15.45), F (1, 147) = 13.15, p < .001, η² = .09. Moreover, as in our earlier studies, this pattern was not contingent on the ingroup priming manipulation. That is, participants reported higher levels of anger after reading the article compared to how they felt before, and this was true regardless of whether participants were assigned to the ingroup prime condition (Ms = 24.18 vs. 17.45) or the control condition (Ms = 21.54 vs. 13.37), F < 1.0 for the contingency of the priming variable. Given that the manipulation of the instructions for the retrospective assessment task occurred after the assessment of actual changes in mood, no effects of that manipulation on this effect were expected and none were found, all effects ns. No significant changes in mood emerged with respect to anxiety or sadness, Fs < 1.0.

Subjective change in mood

As in our previous studies, the scale for this task ranged from 0 to 10, with a meaningful midpoint (5.0), such that ratings higher vs. lower than this point indicating a perceived increase vs. decrease in the specified emotion, respectively. As such, it is again useful to initially consider the mean level of these ratings, prior to considering their contingency on the experimental manipulations. As in Experiment 2, participants believed that the bin Laden article had put them in a better mood as seen by ratings of anger (M = 4.38) sadness (M = 4.27), and anxiety (M = 4.41). Each of these ratings was significantly below the midpoint, ps < .001 for the relevant t-tests, treating the midpoint of the scale (5.0) as the test value. As in our earlier studies, the effect of our ingroup prime was clearly specific to anger, as we note below.

Effect of the ingroup prime

We predicted, and found, that participants would show a stronger belief in a cathartic-like reduction of anger if participants had been assigned to the ingroup prime (vs. control) condition, Ms = 4.11 vs. 4.67. Although this difference approached statistical significance (p = .07) it was, as in Experiments 1 and 2, strengthened after controlling for anxiety, F (1, 146) = 4.62, p < .05, η² = .03. Follow up analyses revealed that the rating of subjective beliefs for anger was significantly different from the midpoint (5.0) in the ingroup prime condition (p < .01) but not for the control condition (p > .10). No significant effects were observed with ratings of anxiety or sadness, all effects ns.

Effect of the forewarning variable

Did our blatant forewarning reduce participants’ tendency to reveal errors in retrospective assessment? It did not. (To maintain consistency with the previous analyses, anxiety was controlled for in these analyses as well.) In particular, the predicted difference between ingroup (vs. control) prime was observed (a) among those participants who had been assigned to the “forewarning” condition, Ms = 4.28 vs. 4.61 for ratings in the ingroup vs. control prime condition, respectively as well (b) participants who had not been given this forewarning, Ms 3.98 vs. 4.67. Simple effects tests revealed no impact of the forewarning variable when analyses were conducted separately for those participants who had been assigned to the ingroup prime condition, F (1, 75) = 1.09, p = .30, and analogous analyses among participants assigned to the control condition also failed to reveal any effects, F (1, 70) = .04, p = .88.

Ruling out an alternative explanation

The interpretation of our main findings is based on the assumption that exposure to the bin Laden article resulted in a bona-fide increase in anger which was still in place by the time participants finished reading the article. However, there is an alternative explanation. Suppose, for the moment, that (a) participants felt some degree of anger while they were actually reading the article but (b) by the time they had actually finished the article, their feeling towards this act of revenge was actually quite positive. In other words, it is possible that participants were motivated to rationalize the killing of bin Laden and, even though the article could have triggered some initial anger, they could well have emerged at the end of this reading exercise in a comparatively cheery mood (e.g. “well, it was ultimately worth it, all things being equal”). If this alternative were true, it would undermine our claim that there was a substantial divergence between reality and subjective belief.
Relevant, too, is the fact that the wording of the post-article probe in Experiments 1–3 did not, strictly speaking, ask participants how they felt after reading the article. Rather, it asked them how they felt while they were reading the article.

Although this alternative is possible, it would require the assumption that participants were experiencing an extremely rapid change in affective experience, involving a shift from feeling bad (while reading the article) to feeling good (immediately afterwards), all within a matter of a minute or less. This seems unlikely. Also inconsistent with this alternative are the data from Preliminary Study One. In that study, participants were randomly assigned to read one of two articles (bin Laden vs. control) and, immediately afterwards, were asked to complete a mood measure that explicitly asked them to report how they felt at that moment (i.e. after reading the article). Those data clearly showed that participants’ level of anger was significantly higher compared to the control condition. Thus, those data clearly seem to suggest that the bin Laden article had produced an increase in anger that had not dissipated by the time that participants had finished the article.

In order to provide further evidence in support of our account, we conducted an additional study with a separate group of participants (N = 46). The design of this study was virtually identical to that of the prime condition from Experiment 1, but with one notable modification, making the instructions for the post-task measurement of mood to make it more parallel to that of our Experiment 1, but with one notable modification, the instructions for the post-task measurement of mood to make it more parallel to that of our first preliminary study: “Now that you have read the preceding article, we would like you to indicate your current emotional state. In other words, we would like you to tell us how you feel RIGHT NOW”. Consistent with our preferred interpretation, we again found a significant increase in anger, but not anxiety or sadness. In particular, after forming a pre as well as a post-article composite measure of anger, analyses revealed (after controlling for sadness and anxiety) significantly higher levels of anger after participants read the article compared to how they felt before. Ms = 16.82 vs. 12.58) = F (1, 44) = 4.85, p < .01. Our data thus indicates that the bin Laden article elicited a significant increase in anger, regardless of whether these data were collected in the context of a between or within-subject design, and regardless of the exact wording of the post-task assessment.

**General discussion**

In a recent article about assessment of personal change, Libby, Eibach, and Gilovich (2005) observed that there is “a rich subjectivist and constructivist tradition in social psychology suggesting that perceptions of personal change can be powerfully influenced by various subjective and contextual factors” (p. 50). In the case of Libby et al. (2005), these researchers were interested in the dynamics of perceived changes in personality. Nevertheless, this “constructivist” perspective is clearly relevant to the present paradigm as well, insofar as we provide new insights into the process by which people arrive at a subjective understanding of their own past emotional trajectory which, under certain conditions, was quite different from what actually happened.

This is not to say, of course, that we are the first researchers to demonstrate the potential for emotion-based retrospective bias. However, previous research has been almost exclusively concerned with the proclivity of people to demonstrate long-term memory bias for affective experiences that occurred some time ago, on the order of days, week, or even months (cf. Levine et al., 2009). We could locate only one set of studies that have examined the possibility of retrospective distortion for an extremely recent event, reported by Van Boven and Robinson (2012). However, Van Boven and Robinson (2012) demonstrated distortions only in the realm of recalled intensity (i.e. how much of a given emotion/mood was felt in the past). However, one could argue that questions about emotional intensity, especially in cases in which one is looking for a precise answer (e.g. “exactly how sad did that event make you feel?”) are fairly challenging. By way of contrast, the basic question of valence—did X make you feel better or worse—would seem to be much easier. We show in this research that, even in this “easy case”, there is still ample opportunity for bias to emerge.

**On the generalizability of our findings**

Our findings bolster but also extend findings obtained by Carlsmith et al. (2008), showing that the “revenge paradox” generalizes to research paradigms quite different from that investigated in their earlier work. Our findings are also conceptually consistent with the “functional view” of revenge, rooted in its motivation to aggressively protect the interests of the ingroup (de Quervain et al., 2004; Fehr & Fischbacher, 2003; Fehr & Gächter, 2002). In particular, we found the most consistent evidence of distortion among participants who had been primed with a relevant aspect of their feelings towards the ingroup. This sort of contingency is precisely what one might expect, given the relevance of ingroup dynamics to the dynamics of revenge, and vice versa. The contingency of our results on the ingroup prime proved to be highly robust, as we replicated this effect four times, once in Preliminary Study 2, and again in our three main studies (Experiments 1–3).

This aspect of our research is also relevant to research on intergroup emotion theory (Mackie et al., 2009), which recognizes that social categorization processes may play an important role in the way that people process information about emotional experience (Moons, Leonard, Mackie, & Smith, 2009). As Moons et al. (2009) note, “the mere activation of group membership triggers common emotional experiences... in the face of group relevant events” (p. 761). Consistent with this perspective, the subjective benefits of revenge are, as we have noted, likely to be intertwined with people’s motivation to protect ingroup interests (Fehr & Fischbacher, 2003). When psychological associations with the ingroup are made salient, a relevant “revenge schema” is likely to become activated, which is associated with a sense that revenge is the right thing to do, with an expectation that it will make us feel better (see also Carlsmith, Darley, & Robinson, 2002). The ironic aspects of this process, as Carlsmith et al. (2008) have noted—and as we also show—are that revenge does not actually make people feel better. Nevertheless, the likelihood that we engage in any given action is likely to be driven by its perceived hedonic benefits, and as long as people expect revenge to feel good, they are likely to keep engaging in that behavior.

**On the specificity of our ingroup prime on subjective changes in anger**

In all three experiments, our ingroup prime affected subjective, but not actual, changes in anger. In our view, this aspect of our findings supports our view that the processes underlying these two types of measures are quite different. To begin, consider our measurement of subjective change. It is worth noting at the outset that this measure was essentially probing participants’ meta-perceptions about their own emotional history. In other words, our subjective change measure was not actually measuring emotional experience per se. Rather, it was measuring participants’ beliefs about their emotions. This point is important because Van Boven and Robinson (2012) have already shown that such meta-perceptions appear to be guided, at least in part, by heuristic/accessibility-based processes, in the sense that people rely on the implications of any relevant mental constructs that happen to be accessible at the time of judgment. The fact that our own assessment of subjective change was systematically influenced by a priming manipulation is thus conceptually consistent with the prior results by Van Boven and Robinson (2012), despite the obvious differences across our paradigms.

Now consider the way that we measured actual change. Here, we were not measuring beliefs about emotion, as in the case of our subjective index. Rather, we were simply asking participants to tell us how they were feeling at any given moment, which corresponds to how most laypeople, as well as social psychologists (cf. Schwarz & Clore, 2007) think about mood. In contrast to the subjective change measure, these measures did not require participants to formulate any sort of
personal theory as to how their emotions might have changed. In other words, we were tapping the experiential system (Strack, 1992), in which participants were simply be asked to report their “gut feelings” at that point in time. All in all, therefore, measurement of actual change was derived from a relatively simple set of questions about current mood that participants likely found quite easy to answer.

One of the implications of the social cognition literature is that people are more likely to rely on heuristic (i.e. top-down) processes when the task is relatively difficult and/or when there is ambiguity as to how one should respond (Wyer & Srull, 1989). Although we did not measure task difficulty, it seemed likely that our subjective change measure did require considerable more guesswork on the part of participants, more so than simply asking participants to tell how they were feeling. In other words, propositional beliefs about emotional change most likely involve more top-down social construction (cf. Martin & Tesser, 1992) than measures of emotional experience itself. This could thus explain why the subjective change index was more susceptible to our priming manipulation compared to the latter. Although this explanation seems reasonable, it is important to acknowledge that this may not be the only reason why our priming manipulation was more likely to influence the subjective (vs. actual) change indices. Future research would thus be useful in gaining more insight into these matters. Regardless of the exact reason(s) for this asymmetry, however, all three experiments showed that it is possible to alter people’s beliefs about their emotional experience, independent of what actually happened.

On the magnitude of actual changes in anger

The observant reader may have already noted that the absolute level of anger following the bin Laden article was not particularly high. However, a main concern in this article was in the actual change in anger. When actual change was determined within the context of a between subjects design (cf. Preliminary Study One), the effect size of actual change was large (Cohen’s $d = .81$). The adjusted effect sizes for our within subjects studies (i.e. after taking into account correlations between the pre and post measures) were as follows: (a) Experiment 1: $d = .24$; (b) Experiment 2: $d = .26$, (c) Experiment 3: $d = .34$, and (d) our supplemental study (presented above): $d = .19$. For the actual change in anger, the average effect size was thus .37, which represents a small-to-medium sized effect (Cohen, 1988).

By conventional standards our effects were not large, although their magnitude was not altogether different from that often observed in the social sciences (Cohen, 1988; Richard, Bond, & Stokes-Zoota, 2003). Also of note, Abelson (1995) has offered caution against sole use of effect size as a criterion of importance, arguing that one should also take into account other factors, such as the extent to which the data provide leverage in showing support for a novel theoretical model, especially when that model offers counterintuitive predictions (see also Prentice & Miller, 1992). Hence, while it is true that the size of our effects is modest, we believe that the importance of our findings rest on their ability to provide consistent, converging support for our theoretical model as this model bears on the propensity of some fairly surprising biases in retrospective emotional appraisal.

On the implications of our “debiasing” attempts

In the case of Experiment 2, we used an explicit accuracy prompt very similar to those used in previous investigations (e.g. Fiske & Neuberg, 1990; Neuberg, 1989; see also Kunda, 1990; Thompson et al., 1994). In the case of Experiment 3 we used a more direct “forewarning”, which provided a blatant and clear explanation of the revenge paradox. Neither of these interventions successfully eliminated the observed bias. As with all null results, it is necessary to be cautious in offering conclusions as to why these manipulations did not mitigate the observed bias. Although future research is obviously needed to provide more direct support for this idea, our preferred interpretation is that the bias in question reflected a lack of awareness, rather than a failure in motivation to arrive at a correct answer. Stated another way, it is not so much the case that participants lacked the motivation to “see” the bias in question; rather, they were not aware of the bias in the first place. This would explain why two different interventions—the latter of which was fairly blatant—failed to eliminate the tendency for error.

The failure of these two different interventions cannot, of course, rule out the possibility that there might be a different intervention that might be more effective. However, one challenge in such an endeavor is to select a manipulation powerful enough to minimize bias while, at the same time, being subtle enough to avoid raising alternative explanations involving demand effects. For example, if the bias were shown to be eliminated by a direct reference to the error (e.g. “if you think that the passage might have made you more angry, you are probably mistaken”) it would not at all be clear if this reflected a bona-fide improvement in accuracy, as opposed to participants simply using this prompt as a direct cue to shift their responses in a more negative direction, regardless of their internal beliefs.

On the correlation between objective vs. subjective change

The “constructivist” approach to perception (cf. earlier quote by Libby et al.) in no way implies that people are completely out of touch with reality. Indeed, even as they emphasized the potential disconnect between perceived and actual change, Libby et al. (2005) were careful to note that a “powerful determinant of perceived change is how much one has actually changed” (p. 50; emphasis added.) Hence, in the present paradigm, the degree to which participants perceive that the bin Laden article made them more angry may be predictive, to a certain extent, to the degree to which it actually made them angry, and vice versa. However, the strength of this relationship should be greater in the control condition, compared to that observed in the ingroup prime condition. In other words, given that our observation of bias was mostly constrained to the ingroup prime condition, one should naturally expect that the correlation between objective and subjective change would tend to be somewhat smaller in that condition (although perhaps not literally zero).

In fact, this is precisely what our own findings showed.Collapsed over Experiments 1–3 ($N = 399$), we found a moderately strong correlation between objective vs. subjective change in anger, $r = .46$, $p < .001$. However, further probing revealed that this correlation was stronger among those participants who were assigned to the control condition ($N = 196, r = .54$) compared to the participants who were assigned to the ingroup prime condition ($N = 203, r = .37$). Although both of these correlations were significant (both $p < .001$), a Fisher $r$ to $Z$ test verified that the former correlation was reliably larger than the latter $Z = 2.14, p < .05$.

Taken in combination with our primary analyses, these data support a fairly straightforward interpretation of our findings. On the average, participants showed greater discrepancy between actual vs. subjective changes in anger if they had been assigned to the ingroup prime than if they had not. This suggests that, for participants in the ingroup prime condition, they were relying to a relatively greater extent on their “top-down” expectations about revenge. For this very reason, this would lead one to expect to see a smaller correlation between objective vs. subjective changes in anger compared to participants assigned to the control condition, and this is indeed what we found. However, it would be a mistake to conclude that the participants in the ingroup prime condition were completely oblivious to reality. In particular, our data showed (as did the findings obtained by Libby et al., 2005) that a strong predictor of subjective change is how much people actually changed. What our findings do show, however, is that the connection between subjective belief and actual change was relatively smaller when participants’ psychological allegiance to the ingroup had been primed.
Directions for future research

As with any other program of research, it is important to acknowledge that our findings raise, but do not answer, a number of important issues that must ultimately be addressed in future research. To begin, our findings suggest the possibility of a “continuity of error” by which retrospective biases in the assessment of past emotions could be used as a basis for formulating incorrect affective forecasts about relevant, future events. In the present paradigm, for example, it is interesting to imagine how participants might respond if they were asked (after responding to the bin Laden article) to predict how they would feel if they had learned about the assassination of another notorious terrorist. We would predict that such affective forecasts would be most strongly predicted by participants’ subjective beliefs about their emotional reactions to the bin Laden event, as opposed to any actual changes in emotional experience.

It is also worth emphasizing that the hedonic downside of revenge, as noted by Carlsmith et al. (2008) is somewhat indirect. That is, it is not the act of revenge per se that makes people feel bad. Rather, it is the extent to which thinking about revenge tends to activate thoughts about the original misdeeds of the transgressor. In theory, it might be possible to disentangle these two elements. One advantage of this separation is that it leaves open the possibility that acts of revenge could indeed be associated with positive emotion, provided that one were able to successfully isolate (a) the emotional associations with thevengeful act from (b) the emotional associations with the original transgression. In practice, however, this may prove to be difficult. For example, it would require that one figure out a way of priming thoughts about killing Osama Bin Laden without also priming memories of the 9/11 attacks. This may prove to be extremely difficult, if not impossible. Nevertheless, future research may need to consider the merits of separating these elements out, perhaps in other test domains more conducive to such an approach.

Another important goal of future research is to employ variations on our ingroup priming manipulation, in order to gain further leverage on the mechanisms responsible for exacerbating the errors we observed in retrospective assessment. Our preferred interpretation is that the ingroup prime activated what might be called a “revenge script”, in which injuriousness against the ingroup trigger motivation for punishment, which leads, in turn, to an expected cathartic relief (schematically: original infraction → anger → punishment → reduced anger). If this view is correct, more direct activation of this script (in a previous and ostensibly unrelated context) should produce findings similar to those found here.

Another interesting issue for future research is whether there might be some people who are more accurate than others in retrospective assessment. It would be of interest, too, to see if people who generally have greater awareness and/or skill in processing information about their own emotional reactions (e.g. Salovey, Mayer, Goldman, Turvey, & Palfai, 1995) tend to be the same people who show better-than-average skill in affective forecasting. These and other future lines of work should yield interesting and important insight into the dynamics of accuracy and bias as they pertain to affective experience.
