Effects of Mood During Exposure to Target Information on Subsequently Reported Judgments: An On-Line Model of Misattribution and Correction

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Three experiments investigated the effects of participants' mood during exposure to target information on delayed judgments of the target. Participants were exposed to a mood induction immediately before they acquired information about a political candidate and then reported their evaluation of the candidate at a later time. Effects of mood on judgments were moderated by 2 individual-differences measures that can be interpreted in terms of processing efficiency. These were political expertise and total recall for the candidate information, with higher scores on these indices interpreted as reflecting more efficient processing. Among low-expertise (or low-recall) perceivers, mood produced an assimilation effect on evaluative judgments. Among high-expertise (or high-recall) perceivers, mood produced a contrast effect on judgments. When pooling across these individual differences, mood exerted no influence on judgments. These findings are consistent with an on-line model of mood misattribution and overcorrection.

A growing body of literature demonstrates that mood states significantly influence social judgments. This effect can be mediated by two distinct process mechanisms: mood-congruent priming and mood misattribution (Clore, 1992; Forgas, 1994, 1995). In the first case, mood indirectly influences judgments by priming mood-congruent concepts that bias the encoding (Bower, Gilligan, & Monteiro, 1981), interpretation (Bower, 1981), or retrieval (Isen, Shalker, Clark, & Karp, 1978; Teasdale & Fogarty, 1979) of information that bears on judgments. In the second case, misattribution of mood to the object of judgment exerts a more direct influence on judgments (Schwarz, 1990; Schwarz & Clore, 1983). In this latter case, mood itself is the information that directly bears on judgments. In both of these instances, mood produces an assimilation effect in which positive moods elicit more positive judgments of an object than do negative moods. This article focuses on conditions in which mood exerts a direct misattribution effect on evaluative judgments.

Research confirms that misattribution effects occur when mood is manipulated at the time individuals are initially exposed to the object of judgment (e.g., Clore, Parrot, Schwarz, & Wilkin, 1990). The present research was motivated by the belief that mood misattribution is indeed a prevalent phenomenon that colors people's evaluations, even when they first acquire information about an object. We speculate, however, that this effect may elude researchers who neglect individual differences that moderate this effect. Specifically, we propose that mood produces an assimilation effect when perceivers lack the necessary resources to correct for the biasing influence of mood. Conversely, we propose that mood can produce a contrast effect when perceivers possess sufficient cognitive resources to engage in correction but inadvertently overcorrect for the biasing influence of mood. When individual differences in available resources are neglected, these effects can cancel, resulting in no evidence of misattribution.

In the model we propose, misattribution and correction effects are postulated to occur for evaluations of the target that are made on-line, during exposure to the target information. As such, our model is restricted to conditions in which perceivers are in fact motivated to evaluate the target on-line. In the present experiments, participants were exposed to a mood induction before they acquired information about a political candidate and then reported their evaluation of the candidate at a later point in time. However, immediately prior to receiving information about the candidate, participants were explicitly told that they would later be asked to evaluate the candidate. Previous research confirms that these instructions lead participants to covertly evaluate each piece of information as it is encountered, form and update an integrated summary evaluation as each piece of information is acquired, and store the summary evaluation in memory. This summary evaluation is later retrieved and used as a basis for judgments when participants are asked to report them (Hastie & Park, 1986; Lodge, McGraw, & Stroh, 1989).
Mood During Exposure to Target Information: Misattribution and Correction

Our model of assimilation is simply an on-line variant of Schwarz and Clore’s “affect as information” hypothesis (Schwarz, 1990; Schwarz & Clore, 1983; Schwarz, Strack, Kommer, & Wagner, 1987; see also Wyer & Carlston, 1979). In accordance with their conceptualization, we assumed that individuals use their affective reaction as relevant information when they make evaluative judgments. Thus, when evaluating an object, individuals may simply ask themselves, “How do I feel about it?” In so doing, feelings elicited by factors that are objectively unrelated to the object of judgment can be misattributed to it. Schwarz and Clore (1983) focused on misattribution effects that occur at the time a person reports a judgment. For example, if individuals are asked to evaluate a film they saw a week earlier, they may use their current mood as information about the film and evaluate it in a mood-congruent manner. We propose that a similar process can operate when mood is manipulated at the time individuals are initially exposed to the object of judgment and evaluations of the object are made on-line. For example, mood during exposure to a film might affect evaluations of the film that are made on-line while it is being watched. If these evaluations are retrieved and used as a basis for judgments reported a week later, mood during exposure to the film might produce mood-congruent judgments even when these judgments are reported a week later. This departure from Schwarz and Clore’s original model is, in a sense, no departure at all. It is simply assumed that affect can be used as an informational basis for covert evaluative judgments that are made on-line prior to the report of an overt judgment. Moreover, insofar as the covert summary evaluation is stored and later retrieved when participants are asked to report a judgment, this effect should remain apparent even for delayed judgments that occur after the mood state has “worn off.” We label this the on-line affect as information hypothesis.

As suggested by the on-line affect as information hypothesis, we assumed that assimilation is the default outcome when the induction procedure elicits an affectively involving mood state and participants do not possess sufficient cognitive resources to engage in correction (Petty, Gleicher, & Baker, 1991; Petty & Wegener, 1993; Srull, 1983; Strack, Schwarz, & Gschneidinger, 1985). If participants possess adequate resources, however, we assumed that they will try to correct for the biasing influence of mood. This should occur for two reasons. First, because these individuals possess supplemental resources that are not fully absorbed by the initial process of evaluating the target, they are more likely to reflect on the potential biasing influence of mood (see Petty, Priester, & Wegener, 1994, for the role of awareness in correction). Second, the actual act of correction entails an additional cognitive step (or steps) that requires a sufficient amount of cognitive capacity (Martin, 1985, 1986; Martin et al., 1990) set-reset model of social judgment. Although Martin’s model does not concern the effects of mood per se, it can account for assimilation and contrast effects elicited by a cognitive prime that immediately precedes exposure to target information. In Martin’s model, the effect of this prime on subsequent judgments is mediated by its prior effect on the semantic interpretation of ambiguous information about the target. For example, the sentence “He was well aware of his ability to do many things well” may be interpreted differently depending on whether this sentence is preceded by the prime self-confident or conceited. Martin contended that those who possess limited cognitive resources simply use the primed concept as a basis for interpretation, resulting in judgments that are assimilated toward the prime. Those who possess adequate resources “partial out” the biasing influence of the prime and search for an alternative concept with which to interpret the stimulus information. Because the alternative concept is of opposite va-
likely to possess the supplemental resources needed to engage in correction. Conversely, individuals who process this information in an inefficient manner should be less likely to possess the resources needed to engage in correction. Research suggests that individual differences in processing efficiency do indeed play an important role in political information processing. It is a considerable challenge, however, to obtain an adequate measure of this construct.

We used as our guide in this matter a rather extensive body of research concerning expertise in political information processing (e.g., Fiske & Kinder, 1981; Fiske, Kinder, & Larter, 1983; Judd & Krosnick, 1989; Lodge, Steenbergen, & Brau, 1995; McGraw, Lodge, & Stroh, 1990) as well as related work concerning the role of prior knowledge in information processing more broadly defined (e.g., Anderson, 1983; Bargh, 1994; Chase & Simon, 1973; Fazio, Chen, McDonel, & Sherman, 1982; Smith & Lerner, 1986). An application of basic research findings to the candidate evaluation task we investigated suggests that political experts will exhibit two forms of procedural efficiency. First, prior knowledge that is relevant to evaluating each issue position (e.g., the perceivers own position) is more likely to be available or accessible among experts. Moreover, experts possess more frequent experience or practice with evaluating issue stances. Under these conditions, evaluation of each issue stance should occur with increased speed and decreased resource allocation (see Anderson, 1983; Bargh, 1994; Bargh & Pietromonaco, 1982; Fazio et al., 1982; Markus, 1977; Smith, 1994; Smith & Lerner, 1986, for related evidence). Second, experts should be more skillful at combining these evaluations when forming and updating the on-line summary evaluation. An experts previous practice with evaluating political figures should reduce this combinatorial process to a relatively routine procedure (see Bargh, in press). Proceduralization and compilation should enable experts to perform this procedure with increased speed and decreased resource allocation (Anderson, 1983). Politically applied research supports these assumptions. This research suggests political expertise is strongly associated with efficient political information processing (Fiske & Kinder, 1981; Judd & Krosnick, 1989) and, more specifically, efficient on-line evaluation of a political candidate (McGraw et al., 1990). The same cannot be said for alternative measures of cognitive ability (e.g., education and general intelligence) that are nonspecific with regard to the political domain (see Cantor & Kihlstrom, 1989, for the utility of domain-specific measures).

Although our primary intent was to capture individual differences in processing efficiency, it is important to note that political expertise is also strongly associated with memory for issue stances in candidate evaluation tasks (e.g., Fiske et al., 1983; Lodge et al., 1995; see also Chase & Simon, 1973; Srull, 1983, for comparable evidence in nonpolitical domains). Our own analysis, presented in Experiment 3, replicated this frequently reported finding. Although we were not specifically interested in modeling this empirical association, a variety of cognitive mechanisms may be involved. Increased availability or accessibility of political knowledge structures may enable experts to more effectively encode each individual issue stance, resulting in increased recall for this information (see Bargh, in press; Bargh & Thein, 1985; Higgins, King, & Mavin, 1982; Markus, 1977, for related evidence). This may occur because prior political knowledge provides a contextual basis for organ-
izing and interpreting the issue information, stimulus information that might otherwise appear confusing or incomprehensible (Wyer & Carlston, 1979; see also Bransford & Johnson, 1972, for the effect of manipulating contextual knowledge on recall). Alternatively, novices may encode specific issue stances just as effectively as experts. However, heightened speed or efficiency may enable experts to encode more of the issues (see McClain, 1983, for related evidence). Regardless of the mediating process, it is clear that political expertise is strongly associated with recall for issue stances in candidate evaluation tasks. We capitalized on this empirical finding by substituting a total-recall measure for political expertise when a direct measure of political expertise was unavailable.

In sum, political expertise and recall for candidate information are strongly associated, and research suggests political expertise is strongly linked to processing efficiency in candidate evaluation tasks. Hence, although use of individual-differences measures precluded us from making definitive claims about causal processes and underlying cognitive mechanisms, the weight of existing evidence suggests that individual differences in expertise and recall are associated with processing efficiency. We therefore assumed that processing efficiency may be reasonably indexed by these measures.

Overview

Three experiments examined the effect of mood at exposure on subsequently reported judgments of a target person. In all three cases, we predicted that mood misattribution would produce an assimilation effect on judgment among perceivers who processed the target information in an inefficient manner. Among perceivers who engaged in highly efficient processing of the target information, we predicted that evidence of correction would emerge. We expected that correction would reduce or eliminate the assimilation effect or, alternatively, that overcorrection would produce a contrast effect. We tested the model by using a political candidate as the target object.

In Experiments 1 and 2, the delay between exposure to the candidate and the report of an overt judgment was held constant at 7 and 8 days, respectively. In both of these cases, total recall for the candidate information served to operationalize processing efficiency. The primary purpose of these two experiments was to establish the generality of the postulated effects using different manipulations of mood, capitalizing on real-world events in one case and a laboratory manipulation in the other. Experiment 3 provided further confirmation of the conclusions drawn in the first two experiments by using a measure of political expertise to operationalize processing efficiency. Furthermore, Experiment 3 provided additional evidence that correction occurs at the time information is received and not at the time of overt judgment. All three experiments yielded findings consistent with the proposed model.

Experiment 1

Experiment 1 examined the effect of a mood state induced by a real-life event characterized by considerable personal involvement. Specifically, mood was elicited by actual feedback given to students regarding their performance on a midterm class exam. As noted previously, we assumed that efficient processing would be indirectly reflected by the number of candidate issue positions that participants accurately recalled. One potential advantage of using total recall to operationalize processing efficiency is that it taps the participants' ability to process political information in the specific candidate evaluation task investigated (see Cantor & Kihlstrom, 1989, for the utility of task-specific measures). Furthermore, unlike an a priori measure of political expertise, this task-specific measure avoids any potential contamination that may result from having the participants complete a political knowledge test prior to reading about the candidate.

Delay between exposure to the candidate and the report of a judgment was held constant at 7 days. This was done because we wanted to focus on the effects of mood during information acquisition, not mood at the time of overt judgment. The 7-day delay ensured that mood at exposure would not persist to the time of overt judgment and, as such, avoided a potential confound of mood at exposure with mood at the time judgments were reported. Furthermore, inclusion of a delay allowed us to gauge whether or not mood at exposure produces effects on judgment that persist over time.

Method

Participants and Design

Eighty college students participated to obtain extra credit for an undergraduate course in political science. Immediately after receiving their midterm grade (assumed to elicit a mood state), participants read about a candidate's stands on 12 issues. Seven days later, participants freely recalled the candidate's stands on the issues and reported their global evaluation of the candidate. Mood and total recall (efficiency) served as the independent variables. Global evaluation of the candidate served as the dependent variable. The design was constructed as a 2 X 2 between-subjects factorial containing two variables: mood at exposure (negative vs. positive) and total recall (low vs. high).

Preliminary Data

Early in the semester, participants evaluated "a variety of issue positions taken by a number of different candidates." This information was ostensibly being collected because "the political science department is in the process of collecting normative data concerning where the student body stands on a variety of issues." The 50 issue positions ostensibly pertained to a number of different candidates and, as such, did not serve to create a coherent impression of any individual candidate. Participants rated each issue position as + (agree), 0 (neutral), or - (disagree). The 50-item set contained 12 issue positions that, unknown to the participants, would serve as the target candidate's issue positions later in the semester. These 12 positions were presented to participants in the main experiment in a manner to be described. On the basis of the participants' ratings of these 12 issues, an "issue agreement" score was computed by simply adding up the number of positively evaluated issues and subtracting the number of negatively evaluated issues. After providing the issue ratings, participants reported

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1 This is especially true when one is controlling for the amount of time allotted to encoding the stimulus material (McClain, 1983). Because all participants were exposed to the candidate information for the same amount of time, this variable was essentially held constant in our experiments.
their level of party identification on a scale ranging from 1 (strong democrat) to 7 (strong republican). Participants also reported their ideology on a 3-point scale labeled liberal, moderate, and conservative, as well as their level of interest in politics on a scale ranging from 1 (not at all) to 4 (extremely interested). Lastly, participants reported their overall grade point average (GPA) in political science.

Procedure and Primary Measures

Session 1. At the start of class midway through the semester, participants received their midterm grade. We assumed that those whose grade was lower than their GPA in political science would be in a negative mood whereas those who received a grade that was equal to or higher than their GPA would be in a positive mood. We did not perform a manipulation check for two reasons. First, asking participants to report their mood state might have alerted them to the fact that their mood state was of concern to the experimenter. This could produce correction effects for reasons that have nothing to do with the hypotheses in question. Research by Schwarz and Clore (1983) suggests that this is an important concern. Merely mentioning to participants the possible source of their mood, even without asking them to report it, can eliminate mood effects on judgment (Schwarz & Clore, 1983). Second, Parrot and Sabini (1990) already verified that an operationalization virtually identical to the present one produces pronounced differences in mood.

Immediately after receiving their midterm grade, participants were introduced to an experiment concerning “how people form impressions of a political candidate” and were told they would receive information about a past member of the U.S. House of Representatives. Participants were also told the following:

In order to mimic the real world in which you do not vote for or against a candidate immediately after learning of the candidate’s stands on several issues, you will be given an opportunity to evaluate this candidate at a later point in time.

On this pretense, participants were given a one-page summary of the candidate’s stands on the issues. It was explained that this information was garnered from a review of news articles pertaining to the candidate, the candidate’s public statements during the previous 12 months, and roll call votes from the U.S. House of Representatives. The synopsis consisted of 12 issue positions that were among the 50-item set assessed early in the semester. These 12 issue positions concerned the National Aeronautics and Space Administration (NASA), Central America, defense, women’s rights, capital punishment, school prayer, nuclear power, crime, welfare, abortion, gun control, and housing. The candidate endorsed a conservative position on most of these issues. Each issue stance was stated in a clear and unambiguous manner. For example, the candidate’s position on capital punishment was “favors reinstallation of the death penalty.” His position on abortion was “views abortion as murder. Favored a house bill (HR232) to eliminate all federal dollars to fund abortion.” Participants were given 5 min to read the summary, after which the experimental forms were collected.

Session 2. Seven days later, participants were asked to freely recall the candidate’s issue positions and report their global evaluation of the candidate. As noted previously, participants’ total-recall scores (gist scoring criterion) served as an indicative of processing efficiency. To ensure that the high-recall and low-recall groups significantly differed in terms of processing efficiency, it was necessary to perform an upper versus lower quartile split on the total-recall scores. Participants in the middle two quartiles were excluded from the analysis. Global evaluation of the candidate, the dependent variable, was assessed using the feeling thermometer. Participants reported a number ranging from 0 to 100, with higher numbers indicating favorable or warm feelings toward the candidate and lower numbers indicating unfavorable feelings.

Lastly, mood at the time of overt judgment was assessed using three measures. First, participants indicated “how you feel at this moment” on a scale ranging from 1 (extremely happy) to 7 (extremely unhappy). Next, participants indicated “how you would describe your mood at this moment” on a scale ranging from 1 (extremely good mood) to 7 (extremely bad mood). Participants then rated their mood using the mood thermometer by reporting a number ranging from 0 to 100, with lower numbers indicating “you don’t feel in a very good mood” and higher numbers indicating “you feel in a good mood.”

Results and Discussion

Our hypotheses concerned the effects of mood during exposure to the candidate information (Session 1), not mood at the time judgments were reported (Session 2). Preliminary analyses were performed to verify that mood at judgment (Session 2) was in fact unrelated to differences in mood produced by the midterm grades conveyed in Session 1. Each mood index, assessed at the end of Session 2, was analyzed as a function of assumed mood in the first session (negative vs. positive) and total recall (low vs. high). No effects were significant in any of the analyses (p > .15). These results confirm that mood at exposure was not confounded with mood at the time of overt judgment.

Effects of Mood on Candidate Evaluation

We expected that misattribution of mood would produce an assimilation effect on candidate evaluation among low-recall (inefficient) perceivers. We predicted that correction would reduce or eliminate the assimilation effect or, alternatively, that overcorrection would actually produce a contrast effect among high-recall (efficient) perceivers. Taken together, these predictions were expected to result in a two-way interaction between mood and total recall. Candidate evaluation scores were submitted to a 2 (negative vs. positive mood) × 2 (low vs. high total recall) between-subjects analysis of covariance (ANCOVA) with party identification, ideology, and on-line issue agreement

2 The expected level of performance on an exam is determined by prior performance (Birkel & Straub, 1977; Fishbein, Raven, & Hunter, 1963), here reflected by the student’s GPA. Furthermore, affective reactions to an outcome are determined by whether the actual outcome exceeds or falls below the expected or prior outcome level (Hsee, Abelsohn, & Salovey, 1991). Student feedback indicated this particular course had a reputation for being more difficult than the average course. The average grade for this course over the years was below the university average. Thus, we assumed that students were happy to receive a grade equivalent to their GPA. Because this coding scheme is based on subtracting the political science GPA from the midterm grade, any component of the midterm grade that may be associated with political expertise or processing efficiency has been exacerbated.

3 When an indirect measure of a construct is being used, use of extreme groupings helps ensure that the two groups differ in terms of the hypothetical construct of interest. Furthermore, participants in the middle two quartiles recalled exactly three and four issues, respectively. Thus, the only other viable alternative was to perform a median split. This would have guaranteed that half of the low-recall group differed from half of the high-recall group by only one recall item. Clearly, this alternative approach fails to ensure that the two groups significantly differ in terms of the hypothetical construct of interest.
as covariates. Supplementary analyses verified that the assumption of homogeneity of regression slopes was met (i.e., there was no evidence that the covariates interacted with the independent variables). 4

The ANCOVA revealed only one significant effect, an interaction between total recall and mood, $F(1, 33) = 6.98$, $p < .02$. This effect was robust enough to persist even when an analysis of variance (ANOVA) that excluded the covariates was performed, and raw score means produced the same pattern as the adjusted means. Adjusted-mean evaluations as a function of total recall and mood are shown in Table 1. Low-recall participants evaluated the candidate more favorably in the positive mood condition ($M = 50.46$) than in the negative mood condition ($M = 38.09$). However, high-recall participants evaluated the candidate more favorably in the negative mood condition ($M = 51.34$) than in the positive mood condition ($M = 40.57$). Thus, mood appeared to produce an assimilation effect among low-recall participants (inefficient processors) and a contrast effect among high-recall participants (efficient processors). Planned comparisons of the adjusted means revealed that the assimilation effect was significant ($p < .05$), whereas the contrast effect did not reach significance ($p = .15$). The absence of a significant mood main effect is instructive. The failure to consider moderating individual differences can produce the mistaken impression that mood at exposure fails to influence subsequently reported judgments of a target person.

When interpreting the moderating role of total recall in the aforementioned analysis, we assumed that the ability to efficiently process political information is the key factor. An alternative possibility is that mood effects are moderated by motivation to process political information. To test this possibility, we replicated the previously reported analysis substituting political interest for the moderating variable. 5 Political interest roughly reflects individual differences in motivation to process political information. Consistent with our speculation that processing efficiency is more closely tied to ability than motivation in this context, this analysis did not yield a significant interaction between political interest and mood ($p > .25$).

**Does Mood at Exposure Produce Biased Recall?**

If mood at exposure elicits selective retrieval of mood-congruent information at the time judgments are reported, two findings should emerge. First, the mood manipulation should elicit mood differences that persist and remain apparent at the time judgments are reported (i.e., mood at exposure should be confounded with mood at the time judgments are reported). Second, the likelihood of recalling positive information should be greatest in the positive mood condition, whereas the likelihood of recalling negative information should be greatest in the negative mood condition. In fact, neither of these findings emerged. As noted previously, mood at the time judgments were reported was unaffected by the mood manipulation. Moreover, there was no evidence of mood-congruent recall. Specifically, a recall-bias score was computed by subtracting the proportion of negative issues recalled from the proportion of positive issues recalled. Positive scores on this index reflect a bias toward recalling positively evaluated issues, and negative scores reflect a bias toward recalling negatively evaluated issues. In fact, recall bias was unaffected by mood at exposure ($P < 1$).

**Experiment 2**

We performed a replication of Experiment 1 for two reasons. First, it seemed desirable to test our hypothesis using a more direct experimental manipulation of mood. Second, it is possible that candidate evaluation judgments were contaminated by having participants recall the issues prior to reporting judgments in Experiment 1. Experiment 2 addressed both of these methodological considerations.

**Method**

Experiment 2 was performed using 46 college students. The procedure and the measures were virtually identical to those used in Experiment 1. Once again, preliminary measures of issue agreement, party identification, ideology, and political interest were collected early in the semester. Midway through the semester (Session 1), mood was manip-

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4 Effects were tested using the most conservative, unique sums of squares approach. The design and the data met the necessary criteria specified by Kirk (1982, pp. 719-720) for usage of ANCOVA. Specifically, the covariates were measured prior to the mood elicitation. Ideology and issue agreement were orthogonal to all effects tested. Party identification was orthogonal to both mood and the Mood x Total Recall interaction. Furthermore, there is no theoretical basis for assuming that total recall (or political expertise) causally determines the direction of party identification. All covariates were independent of the predicted Mood x Total Recall interaction. Under these conditions, neither the ANOVA nor the ANCOVA estimates of this interaction are biased (Cook & Campbell, 1979). Furthermore, the ANCOVA of candidate evaluation judgments was replicated including a memory-based issue predictor as an additional covariate. If biased recall mediates the effect of mood on candidate evaluation, inclusion of an issue agreement predictor based exclusively on recalled issues should eliminate the mood effect. In fact, inclusion of this covariate merely served to increase the significance of the effects reported.

5 The distribution of responses to the political interest item precluded us from performing an upper versus lower quartile split when using political interest as the moderator. In all three experiments, only a median split was feasible given the distribution of responses to this item. Because this does not produce extreme groupings that correspond to those used for total recall (and political expertise), this evidence against a motivational interpretation of the moderator is only tentative.

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**Table 1**

*Adjusted-Mean Evaluations as a Function of Total Recall and Mood (Experiment 1)*

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<thead>
<tr>
<th>Total recall</th>
<th>Mood</th>
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<tbody>
<tr>
<td></td>
<td></td>
<td>Negative</td>
<td>Positive</td>
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<tr>
<td>High (Efficient)</td>
<td>51.34</td>
<td>40.57</td>
<td></td>
</tr>
<tr>
<td>Low (Inefficient)</td>
<td>38.09</td>
<td>50.46</td>
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*Note.* The evaluation scale ranges from 0 to 100 with higher numbers reflecting a more positive evaluation of the candidate. The analysis used party identification, ideology, and issue agreement as covariates. Numbers in parentheses represent sample sizes.
ulated, and participants read about the political candidate. Eight days later (Session 2), participants evaluated the candidate and recalled his issue positions. To ensure that the high- and low-recall groups significantly differed in terms of processing efficiency, it was again necessary to perform an upper versus lower quartile split on the total-recall scores.6

In this case, mood was experimentally manipulated. Prior to reading about the candidate, participants were told that they would be participating in an unrelated study "being performed by Richard Mann, who was doing research on American films at a nearby university." Participants were told that, after viewing each of the three film clips, they would be "asked to answer a few straightforward questions." Half of the participants viewed movie footage designed to evoke a positive mood state, whereas the remaining participants viewed movie footage designed to evoke a negative mood state. This movie footage was taken directly from Martin, Ward, Achee, and Wyer's (1993) study. It consisted of three movie segments lasting a total of 20 min. In both conditions, the first segment was taken from a car chase scene in the movie Bullitt. This segment was included to draw the participants' attention away from the overall affective tone of the film clips and thus to decrease the likelihood that participants would guess the movie footage was designed to evoke a mood state. The remaining two segments were taken from Stripes and Splash in the positive mood condition and from Galipol and Sophie's Choice in the negative mood condition. We did not perform a mood manipulation check for two reasons. First, asking participants to report their mood state might have alerted them to the fact that their mood state was of concern to the experimenter. This could produce correction effects for reasons that have nothing to do with the hypotheses in question. Second, Martin et al. (1993) already verified that this manipulation produces pronounced differences in mood.

Another distinguishing feature of Experiment 2 concerns the order in which free recall and candidate evaluations were measured. In this case, candidate evaluations were assessed prior to recall. Delay between exposure and overt judgment was held constant at 8 days.

Results and Discussion

Preliminary analyses were performed to verify that mood at judgment was in fact unrelated to differences in mood produced by the mood manipulation in Session 1. Each mood index, assessed at the end of Session 2, was analyzed as a function of manipulated mood in Session 1 (negative vs. positive) and total recall (low vs. high). No effects were significant in any of the analyses (p > .25). This means mood at exposure was not confounded with mood at the time of overt judgment.

Candidate evaluation scores were again submitted to a 2 (negative vs. positive mood) × 2 (low vs. high total recall) between-subjects ANCOVA with party identification, ideology, and on-line issue agreement as covariates. As predicted, the ANCOVA revealed only one significant effect, an interaction between total recall and mood, F(1, 16) = 5.91, p < .05. Adjusted-mean thermometer ratings as a function of total recall and mood are shown in Table 2. Low-recall participants evaluated the candidate more favorably in the positive mood condition (M = 54.99) than in the negative mood condition (M = 47.86). High-recall participants evaluated the candidate more favorably in the negative mood condition (M = 54.97) than in the positive mood condition (M = 30.81). Thus, mood again appears to have produced an assimilation effect among participants who engaged in inefficient processing (low recall) but a contrast effect among participants who engaged in efficient processing (high recall) of the candidate information. In this case, planned comparisons of the adjusted means revealed that the contrast effect was significant (p = .02), whereas the assimilation effect did not reach significance. We replicated this analysis substituting political interest for the moderating variable. Consistent with our speculation that processing efficiency is more closely tied to ability than motivation, this analysis failed to yield a significant interaction between political interest and mood (p > .45).

As noted previously, mood at the time of overt judgment was unaffected by the manipulation of mood at exposure as well as its interaction with total recall. Thus, the previously reported effects cannot readily be explained in terms of a tendency to retrieve mood-congruent information at the time of overt judgment. Analyses of recall-bias scores (operationalized as they were in Experiment 1) also failed to yield any significant effects due to mood at exposure (F < 1). Thus, once again there was no evidence that mood at exposure produced mood-congruent recall.

Experiment 3

The first two experiments provided preliminary support for the proposed model. For individuals who recalled little target information (low efficiency), mood at exposure produced an assimilation effect on judgments of the target. For individuals who recalled a great deal of target information (high efficiency), mood at exposure produced an opposite contrast effect on judgments. Furthermore, these effects could not be readily explained in terms of mood-congruent recall of the target information. One purpose of Experiment 3 was to establish the generality of these effects by using a different operationalization of processing efficiency. Specifically, a political information test was administered to participants before they read about the candidate. Political experts possessing a great deal of

<table>
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<th>Table 2</th>
<th>Adjusted-Mean Evaluations as a Function of Total Recall and Mood (Experiment 2)</th>
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<tr>
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<td>M</td>
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<tr>
<td>Total recall</td>
<td></td>
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<tr>
<td>High</td>
<td>54.97</td>
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<tr>
<td>(Efficient)</td>
<td>(6)</td>
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<tr>
<td>Low</td>
<td>47.86</td>
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<tr>
<td>(Inefficient)</td>
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</table>

Note. The evaluation scale ranges from 0 to 100 with higher numbers reflecting a more positive evaluation of the candidate. The analysis used party identification, ideology, and issue agreement as covariates. Numbers in parentheses represent sample sizes.

6 Participants in the middle two quartiles recalled exactly two and three issues, respectively. Thus, the only other viable alternative was to perform a median split. This would have guaranteed that half of the low-recall group differed from half of the high-recall group by only one recall item. Clearly, this alternative approach fails to ensure that the two groups significantly differ in terms of the hypothetical construct of interest.
prior knowledge about politics were assumed to process the candidate information in an efficient manner; whereas political novices were assumed to process this information in an inefficient manner.

Experiment 3 also included a manipulation of delay between exposure to the candidate information and the subsequent report of a judgment. This was done to assess when the expert participants correct for the biasing influence of mood. Suppose participants attempt to correct for mood-induced bias only after they have been asked to report a judgment. Then contrast effects among experts should be even more strongly pronounced in a short-delay condition than in a long-delay condition. This is because participants should be more likely to remember the mood induction in the short-delay condition and, as such, should be more likely to correct for it. However, if the corrective process occurs for covert evaluative judgments made on-line, evidence of correction should not be contingent on delay between information acquisition and the report of a judgment. In this case, correction should be equally evident regardless of whether these covert evaluations are retrieved and used as a basis for a judgment reported soon or long after reading about the candidate.

Lastly, Experiment 3 was designed to eliminate serial position effects when testing for biases in memory for the candidate’s issue positions. Specifically, the order of the candidate’s specific issue positions was varied randomly across participants. Furthermore, in addition to including a free-recall measure of memory, Experiment 3 included a recognition measure of memory for the issues. Free recall, because it is strongly dependent on the participants’ ability to retrieve information from memory, provides the necessary data for testing selective-retrieval effects. Correct recognition is primarily sensitive to fluctuations in input or encoding (Srull, 1984). As such, the recognition measure allows one to test for selective-encoding effects.

Method

Participants and Design

Two hundred and eighty-two college students participated. Half participated to gain extra credit. The other half participated to fulfill a course requirement. Mood at exposure, political expertise (efficiency), and delay served as the independent variables. Global evaluation of the candidate served as the dependent variable. The design was constructed as a $2 \times 2 \times 2$ between-subjects factorial containing three variables: mood (negative vs. positive), political expertise (low vs. high), and delay (5 min vs. 8 days).

Procedure and Primary Measures

With some exceptions, the procedure was analogous to that used in the first two experiments. Early in the semester, participants evaluated a variety of issue positions and reported their level of party identification, ideology, and political interest. Measures of issue agreement, party identification, and political interest were identical to those used in the previous two experiments. In this case, ideology was measured on a scale ranging from 1 (very liberal) to 7 (very conservative). A measure of political expertise was also administered at this time. This measure consisted of 20 multiple-choice questions that tested the participants’ knowledge of political figures (e.g., Douglas Wilder, Pat Schroeder) and political organizations (e.g., North Atlantic Treaty Organization [NATO], Federal Reserve Board). Correct responses to these items were summed to arrive at an overall political expertise score. This variable was dichotomized by performing an upper versus lower quartile split. Participants in the middle two quartiles were excluded from the analysis.

Session 1. Later in the semester, participants reported to an experimental suite where each participant was seated in a separate room. Participants were told the following:

Under this pretense, participants were given 15 min to complete a Life Event Inventory. Half of the participants were instructed to write about a recent experience that made them happy, whereas the other half wrote about an experience that made them sad. Participants were instructed to include as many vivid details as possible, so as to ensure that the manipulation evoked a strong affective state. Again, a mood manipulation check was omitted for two reasons. First, we did not want to alert participants to the fact that their mood state was of concern to the experimenters. This could produce correction effects for reasons that have nothing to do with the hypotheses in question. Second, Schwarz and his associates (Schwarz & Clore, 1983; Strack et al., 1985) already verified that this manipulation produces pronounced differences in mood. When used to elicit affect at the time of overt judgment, this mood manipulation produces assimilation effects on judgment (Schwarz & Clore, 1983; Strack et al., 1985).

After completing the life event inventory, participants were introduced to an ostensibly unrelated experiment concerning “how people form impressions of political candidates.” The instructions were essentially identical to those used in the first two experiments. After participants had finished reading about the candidate, half of them were reminded to return 8 days later to complete the experiment (long-delay condition). The remaining participants (short-delay condition) then completed a 5-min distractor task that required them to draw a map of the university campus. This was done to ensure that the mood elicited at exposure would not persist until the moment when these participants reported an overt judgment (i.e., to avoid a confounding of mood at exposure with mood at overt judgment).

After completing the distractor task, these participants completed the first response form, wherein they reported their global evaluation of the candidate. Two items assessed global evaluation of the candidate. The first item asked participants to indicate “how much you like the candidate” on a scale ranging from 1 (dislike very much) to 11 (like very much). The second item asked participants to indicate “whether you feel negatively or positively toward the candidate” on a scale ranging from 1 (feel negatively) to 11 (feel positively). Responses to these items were averaged to arrive at an overall index of global candidate evaluation.

After completing these judgments, participants were asked to recall the candidate’s issue positions. Then they completed a second form that included a recognition measure of memory for the candidate’s issue positions. The recognition test contained six items that correctly identified six of the candidate’s issue positions and six distractors. Finally, measures of mood at the time of overt judgment, which were identical to those administered in the previous two experiments, were administered.

Session 2. Participants in the delay condition completed the two candidate response forms during a session that took place 8 days later.
Results and Discussion

In Experiment 3, political expertise replaced total recall as an operationalization of processing efficiency. We first sought to verify that political expertise and total recall do indeed tap a common underlying construct. Replicating previous research (Lodge et al., 1995), a preliminary analysis of the current data supported this assumption. Political experts (M = 4.67) recalled significantly more issues than political novices (M = 3.58), F(1, 184) = 21.35, p < .001.

Preliminary analyses were also performed to verify that mood at judgment was in fact unrelated to differences in mood produced by the mood manipulation that immediately preceded exposure to the candidate. Each mood index, assessed at the time of overt judgment, was analyzed as a function of manipulated mood during exposure (negative vs. positive), expertise (low vs. high), and delay (5 min vs. 8 days). No effects were significant in any of the analyses (p > .20). These results confirm that mood at exposure was not confounded with mood at overt judgment. This held true for both the 5-min and the 8-day delay condition.

Effects of Mood on Candidate Evaluation

Global evaluation scores were submitted to a 2 (negative vs. positive mood) × 2 (low vs. high expertise) × 2 (5-min vs. 8-day delay) between-subjects ANCOVA with party identification, ideology, and on-line issue agreement as covariates. Supplementary analyses again revealed that the assumption of homogeneity of regression slopes was met (i.e., there was no evidence that the covariates interacted with the independent variables).7

The ANCOVA revealed only one significant effect, an interaction between political expertise and mood, F(1, 184) = 9.03, p < .003. This effect was robust enough to persist even when an ANOVA that excluded the covariates was performed. Adjusted-mean candidate evaluations as a function of delay, political expertise, and mood are shown in Table 3. The two-way interaction was decomposed through the use of planned comparisons of the adjusted means, pooling across delay. These comparisons revealed that political novices evaluated the candidate more favorably when they were in a positive mood (M = 5.39) than when they were in a negative mood (M = 4.63), F(1, 184) = 3.77, p < .05. Conversely, experts evaluated the candidate more favorably when they were in a negative mood (M = 5.70) than when they were in a positive mood (M = 4.79), F(1, 184) = 5.24, p < .05. Thus, consistent with the findings obtained in the first two experiments, mood produced an assimilation effect for political novices and a contrast effect for experts. Note that the main effect of mood, collapsed over levels of expertise, was once again nonsignificant. Moreover, there was no evidence that delay moderated the effect of mood or its interaction with political expertise. We replicated this analysis, substituting political interest for the moderating variable. Consistent with our speculation that processing efficiency is more closely associated with ability than motivation in this context, political interest failed to moderate the mood effect (p > .35).

If expertise and total recall tap a common moderating dimension, the expertise by mood interaction should be eliminated when total recall is included as an additional moderating variable in the model. This is because, when the present unique sums of squares approach is used, each effect is tested after controlling for other effects in the model. To test this possibility, we first performed a linear transformation on the total-recall scores in the short-delay condition. This transformation produced a distribution of adjusted recall scores in the short-delay condition possessing the same mean and standard deviation as the distribution of total-recall scores in the long-delay condition.8 This produced a common recall index for the entire sample that was unaffected by the delay manipulation. Global evaluation

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7 Effects were tested using the most conservative, unique sums of squares approach. Once again, the design and the data met the necessary criteria specified by Kirk (1982, pp. 719-720) for usage of ANCOVA. Specifically, the covariates were measured prior to the manipulation of mood and delay. The covariates were independent of all terms involving expertise and the predicted Mood × Expertise interaction. Under these conditions, neither the ANOVA nor the ANCOVA estimates of the predicted interaction are biased (Cook & Campbell, 1979). Furthermore, the ANCOVA was replicated including the memory-based issue agreement score as an additional covariate. Inclusion of this covariate merely served to increase the significance of the effects reported.

8 For 1 participant in the short-delay condition, this transformation produced a negative total-recall value (—0.34). Because it is not possible to recall less than zero issue positions, this value was recoded to equal zero.
scores were then examined as a function of a full factorial containing the following independent variables: mood (negative vs. positive), expertise (low vs. high), delay (5 min vs. 8 days), and total recall (a continuous variable). Party identification, ideology, and on-line issue agreement were entered as controls.7 As anticipated, this analysis eliminated the expertise by mood interaction effect, F(1, 176) = 1.04, p = .31. This finding suggests that expertise and total recall tap a common moderating dimension.

Are Mood at Exposure Effects Mediated by Biased Encoding or Retrieval?

As noted previously, the experimental conditions did not differ in terms of mood at the time of overt judgment. Thus, the previously reported assimilation and contrast effects could not readily be explained in terms of selective retrieval of mood-congruent material at the time of overt judgment. A direct test of selective recall was performed by analyzing recall-bias scores (operationalized as they were in Experiments 1 and 2) as a function of delay, expertise, and mood. If recall is mood congruent, a main effect of mood should emerge in this analysis. Alternatively, if the assimilation and contrast effects on judgment are mediated by selective recall, a corresponding expertise by mood interaction should emerge when the biased-recall index is analyzed. In fact, both effects were nonsignificant (p > .05).

An analogous test of selective-encoding effects was performed by analyzing recognition-bias scores as a function of delay, expertise, and mood at exposure. The recognition-bias score was computed by subtracting the proportion of negative issues correctly recognized from the proportion of positive issues correctly recognized. Positive scores on this index reflect a bias toward recognizing positively evaluated issues, whereas negative scores on this index reflect a bias toward recognizing negatively evaluated issues. If encoding is mood congruent, a main effect of mood should emerge in this analysis. Alternatively, if the assimilation and contrast effects on judgment are mediated by selective encoding, a corresponding expertise by mood interaction should emerge when the recognition-bias index is analyzed. In fact, both effects were nonsignificant (p > .20).

General Discussion

The results of all three experiments revealed a consistent pattern. Mood produced an assimilation effect on judgment among low-expertise (or low-recall) perceivers. Mood produced a contrast effect among high-expertise (or high-recall) perceivers. When these two perceivers groups were pooled, mood exerted no influence on judgments of the target. This pattern of findings was demonstrated to generalize across three different operationalizations of mood. Although the use of individual-differences measures precluded us from definitively isolating the process mechanism that determined these effects, the weight of existing evidence led us to assume processing efficiency was the key moderating mechanism in all three experiments. Namely, we interpreted these findings to mean that mood produced an assimilation effect among perceivers who processed the candidate information in an inefficient manner, whereas mood produced a contrast effect among perceivers who processed this information in an efficient manner.

We proposed a tentative model that can account for these findings. This model pertains only to conditions that promote on-line evaluation of the target. Specifically, we postulated that mood during exposure to target information is misattributed to the target and, as such, influences on-line evaluations of the target. We labeled this the on-line affect as information hypothesis to emphasize its link to Schwarz and Clore's (1983) original formulation. If perceivers process the target information in an inefficient manner, and thereby possess insufficient resources to engage in correction, on-line evaluations of the target are assimilated toward the mood state. These on-line evaluations are stored in memory and later retrieved for purposes of reporting a judgment. This produces a mood assimilation effect even for delayed judgments that are reported after the mood state has "worn off." Low-expertise or low-recall individuals, in our view, exhibit this pattern of assimilation because they process the target information in an inefficient manner.

If perceivers process the target information in a highly efficient manner, and thereby possess adequate resources to engage in correction, we postulate that they attempt to correct for the biasing influence of mood. However, because perceivers may overestimate the initial biasing influence of mood, they may inadvertently overcorrect for this influence. This can produce a contrast effect in which positive mood elicits a more negative target evaluation than does negative mood. In our view, high-expertise or high-recall individuals exhibit this pattern of correction because they process the target information in a highly efficient manner. Our results indicate that this contrast effect is not contingent on the amount of delay between exposure to target information and the subsequent report of an overt judgment. If participants attempt to correct for mood-induced affect only after they have been asked to report an overt judgment, delay should attenuate the contrast effect. That is, participants should be less likely to remember the mood elicitation procedure after delay and should therefore be less likely to correct for its influence. Yet, if the corrective process occurs for covert evaluative judgments made on-line, correction should be equally evident regardless of whether these covert evaluations are used as a basis for an immediate or a delayed report of judgment. The lack of any contingency with delay supports our on-line interpretation of overcorrection and contrast.

Given the novelty of the presently reported findings and the

7 A simpler tactic would involve performing an ANCOVA in which total recall and its interaction with mood are entered as additional covariates. Unfortunately, this simpler tactic grossly violates the statistical assumptions associated with the use of ANCOVA. It is inappropriate to include a covariate (total recall) by independent variable (mood) interaction term as a covariate when ANCOVA is being used (Kirk, 1982). An interactive regression procedure was used to test the expanded model described in the text because total recall served as a continuous independent variable in the design. Dichotomous independent variables were coded using effect coding (−1 vs. 1), and the continuous variable was coded using the most direct analog to effect coding (from −1 for lowest recall to 1 for highest recall). When this interactive regression approach was used to test the original model (omitting the total-recall variable), this regression procedure produced exactly the same effects reported in the original ANCOVA analysis.
possibility that our individual difference measures may be associated with other dimensions that can be distinguished from processing efficiency, it is important to consider the merit of our preferred interpretation against alternative conceptualizations. We consider four possibilities. These are mediation by cognitive motivation, mood-induced priming, standard of comparison, and simple discounting, respectively. Each of these alternatives is associated with a distinct interpretation of the moderating variable.

**Motivation Versus Ability**

One concern centers around whether cognitive ability or motivation was the key moderator in our experiments. Although both ability and motivation promote correction (Martin, 1985, 1986; Martin et al., 1990; Petty et al., 1994), we speculate that ability to efficiently process information was the key moderator in our experiments. Need for cognition, a motivational variable, failed to moderate the effect of mood on judgment in a replication of our experiments (Isbell, 1995). Furthermore, we replicated all of our analyses, substituting political interest for the moderating variable. Political interest roughly reflects individual differences in motivation to process information. Consistent with our speculation that the moderator is more closely tied to ability, political interest failed to moderate the effect of mood on judgment in all three experiments. Perhaps most important, our experimental instructions instilled an evaluation objective with anticipated delay, a processing objective that motivates all participants to exert maximal effort in evaluating the candidate on-line (McGraw et al., 1990; see Srull & Wyer, 1983, for related evidence). These conditions essentially control for motivation by setting it at the highest possible level for all participants. Of course, it is probably impossible to completely disentangle individual differences in ability and motivation in this domain. This is because the ability to process political information is, in part, an acquired ability that is developed by previous exposure and practice with political information-processing tasks. Politically motivated individuals may expose themselves to more political information and therefore become more efficient with such tasks (see Cantor & Kihlstrom, 1989, for a related argument).

**Mediation by Mood-Induced Priming**

It is important to distinguish our interpretation of assimilation, which involves on-line mood misattribution, from alternative accounts of assimilation that are mediated by mood-induced priming (see Clore, 1992; Forgas, 1994, 1995, for this distinction). These alternative accounts suggest that the effect of mood on judgment is mediated by mood-congruent encoding, interpretation, elaboration, or retrieval of target information. For instance, according to the selective-encoding hypothesis (Bower et al., 1981; Brown & Taylor, 1986), mood-congruent material is more likely to be encoded than is mood-incongruent material. Alternatively, perceivers may interpret ambiguous information about the target in a mood-congruent manner (Bower, 1981). Because judgments are based on these encodings and interpretations, both of these mechanisms should result in a mood assimilation effect. Another account suggests mood elicits mood-congruent cognitive responses when perceivers elaborate on the stimulus material (Petty et al., 1991). If these biased elaborations determine global evaluation of the candidate, this should also produce a mood assimilation effect. A somewhat different account posits that mood at the time of judgment increases the accessibility of mood-congruent features of the object (Blaney, 1986; Isen et al., 1978). If judgments are based on the retrieval of specific target information, this accessibility bias should also produce a mood assimilation effect on judgments.

None of these interpretations seem particularly plausible in the candidate evaluation task we investigated. Analyses of the memory data yielded no evidence to suggest our mood effects were mediated by selective encoding or retrieval of mood-congruent material. Moreover, the target information was extremely unambiguous in our experiments (e.g., “views abortion as murder,” “wants to make abortion illegal”). Under these conditions, the stimulus information does not afford biased interpretation in terms of alternative concepts. Thus, it seems implausible to argue mood-congruent encoding, interpretation, or retrieval mediates the effects we report. This is not surprising. The issue positions were unambiguous, and the candidate adopted a typical conservative position on the vast majority of issues. Under such conditions, these effects are typically limited (Forgas, 1994, 1995).

An interpretation of our findings in terms of mood-congruent elaboration is also problematic. First, mood-congruent biases in elaboration are limited when the target information is unambiguous or typical (Forgas, 1994, 1995). As we just noted, the candidate information possessed both of these qualities. Second, mood-congruent elaboration should be most likely to mediate mood effects among individuals who engage in extensive cognitive elaboration (Petty et al., 1991). This suggests that mood-congruent elaboration should have produced an assimilation effect among the high-expertise (or high-recall) individuals in our experiments. In fact, we obtained a contrast effect for these individuals. As R. E. Petty (personal communication, July 5, 1995) noted, however, this does not mean that the high-expertise (or high-recall) individuals failed to engage in extensive elaborative processing in our experiments. In this regard, it is important to distinguish mood-congruent elaboration from corrective elaboration. Indeed, our model is consistent with the assumption that high-expertise individuals engaged in extensive corrective elaboration.

**Standard of Comparison**

It is important to distinguish our interpretation of the contrast effect, which involves overcorrection, from alternative accounts of contrast that involve a standard of comparison (see Schwarz & Bless, 1992, for elaboration on this distinction). Although affectively involving mood manipulations do not typically produce standard of comparison effects, less involving induction procedures that resemble a mood manipulation can produce such effects (see Schwarz & Strack, 1991, for a review). In these instances, the content of the induction procedure serves as a standard of comparison. For example, an induction that involves thinking about negative life events in the remote past can increase ratings of present life satisfaction.
(Strack et al., 1985). Alternatively, sitting in an unpleasant room can increase satisfaction with one's own room or housing arrangement (Schwarz et al., 1987). These effects occur when the manipulation primes a separate cognitive standard (e.g., past life events or an unpleasant room) that falls in the same class as the object being judged (e.g., present life events or one's own room). They do not commonly occur when the induction primes material that falls in a class that is completely unrelated to the object of judgment. For example, sitting in an unpleasant room does not typically increase satisfaction with one's own automobile.

One might argue that high-expertise (or high-recall) perceivers categorized the candidate's issue positions in a more precise manner and, as such, were more likely to differentiate these issue positions from a standard of issue positions that was primed by the mood induction. This might explain why contrast effects were localized to the high-expertise (or high-recall) segment of our sample. Although we cannot unequivocally reject this interpretation, we believe it is less plausible than the one we proposed. For one thing, the semantic content of the mood induction was completely apolitical in our experiments. Therefore, it is unlikely that our induction procedure primed a separate standard that fell in the same class as the object that was judged. That is, it is unlikely that our mood manipulation primed a standard of political issue positions against which the candidate's issue positions were evaluated. Furthermore, mood was manipulated using methods specifically designed to evoke a strong affective reaction. As noted previously, standard of comparison effects are typically restricted to induction procedures that provoke little if any affective response in the perceiver (Strack et al., 1985). Moreover, although the present model pertains to mood during exposure to target information, standard of comparison effects are typically demonstrated when the standard is salient at the time of overt judgment. In sum, the conditions we investigated were opposite to those that commonly elicit standard of comparison effects. Thus, although a standard of comparison interpretation of our contrast effect cannot be unequivocally rejected, it seems less plausible than the overcorrection process we postulated.

**Correction Versus Discounting**

Correcting for mood-induced bias, after it has already contaminated judgment, is not the only strategy available to participants attempting to avoid this form of contamination. An alternative strategy might involve discounting or ignoring mood-induced affect prior to judgmental computation (see Strack, 1992, for this distinction). Ignoring or discounting mood-induced affect will simply eliminate mood effects on judgment. The effect of correction, however, depends on the extent to which the participants accurately assess the initial biasing influence of mood. If the participants underestimated this influence, correction will be insufficient and will merely reduce the mood assimilation effect. If the participants accurately estimate the biasing influence of mood, correction will completely eliminate mood effects. However, if participants possess an exaggerated subjective theory of mood-induced bias, they will overcorrect for mood-induced bias and reveal a contrast effect. A key distinction between correction and discounting strategies is that, unlike correction, simple discounting cannot account for the contrast effect we reported.

The inability to account for our contrast effect in terms of simple discounting bears directly on our assumption that processing efficiency, not systematic processing, was the key moderator in our experiments. If high-expertise (or high-recall) individuals simply engaged in more systematic processing of the candidate information, they should have been less likely to rely on heuristics when evaluating the candidate (Chaiken & Stangor, 1987). Reliance on affect as information, in this view, is a heuristic evaluation strategy that these individuals simply avoided. Although this form of discounting might account for a reduction in the mood assimilation effect among these individuals, it cannot account for the contrast effect we reported.

Although simple discounting cannot account for the contrast effect we reported, it may account for the reduction or elimination of mood assimilation effects reported in other studies (Schwarz & Clore, 1983; Strull, 1983). For example, Schwarz and Clore demonstrated that individuals reported lower levels of life satisfaction during rainy weather. This effect was eliminated, not reversed, when participants were reminded of the actual source of their mood (the weather) before they were asked to report their life satisfaction. This finding raises an important question: Under what conditions might individuals engage in discounting versus correction?

One possibility is that discounting effects never occur. Namely, an apparent discounting effect may indicate that cognitively efficient participants overcorrected whereas less efficient participants failed to correct at all, with these two effects canceling when pooling across processing differences. A second possibility is that discounting is the preferred avenue of avoiding bias when it is relatively easy for participants to correctly distinguish affect elicited by the mood induction from other more relevant criteria before judgments are computed. Under these circumstances, participants can more easily set aside irrelevant affect before computing a judgment. In Schwarz and Clore's (1983) study, the participant's task involved retrieving previously acquired information about one's life, assessing its implications for life satisfaction, and reporting a judgment. The information contained in the participant's transitory mood was
not previously acquired nor was it retrieved from memory. These differences should enable participants to more easily distinguish affect associated with previous life experiences from affect elicited by their momentary mood state. This is especially likely when participants are reminded of the actual source of their present mood (the weather) before they are asked to even consider or retrieve previously acquired information that is relevant to judgment. In our experiments, mood, the acquisition of judgment-relevant information, and on-line judgment all occurred at the same time. These conditions may have failed to equip the perceiver with a priori markers that punctuated the distinction between mood and more relevant information. As such, it is less likely that participants would be able to discount mood-induced affect before computing a judgment (see Clore, 1992, for a related conceptualization).

We speculate that the ability to separate relevant judgmental criteria from irrelevant affective criteria may also be enhanced when relevant judgmental criteria fail to elicit "hot" affective reactions in the perceiver. Under these conditions, the perceiver may more easily distinguish affect elicited by the mood induction from the "cold" cognitive criteria that are more directly relevant to judgment. This speculation may speak to the discrepancy between Srull's (1983) findings and our own. Srull reported that mood assimilation effects were eliminated, not reversed, for expert perceivers. This discrepancy could be due to the fact that the target we investigated (a person) elicited more affect than Srull's target of judgment (an automobile). Issue positions such as "views abortion as murder" may indeed have elicited strong affective reactions in our experiments. The description of an automobile, in contrast, is less likely to elicit strong affective reactions. Experts in Srull's study might have made an easy a priori distinction between "cold" cognitive criteria that were relevant to judgment and "hot" affective criteria that were elicited by the mood induction. As such, they simply may have ignored or discounted mood-related affect prior to computing a judgment. In our experiments, the distinction between mood-induced affect and target-induced affect may have been more easily blurred or confused. Under such conditions, misattribution effects may be unavoidable. Furthermore, when correcting for this unavoidable bias, participants may inadvertently correct for affect elicited by the candidate in addition to affect elicited by the mood induction (see Martin, 1986, for a related conceptualization). This particular form of overcorrection, which involves a kind of reverse misattribution of target-induced affect to the mood induction, may occur only when the target elicits a strong affective reaction.

Conclusion

It is commonly assumed that mood misattribution during exposure to an object can have an enduring influence on attitudes toward the object. Indeed, this assumption underlies many practices that are prevalent in contemporary society. Advertisements include contextual cues that are designed to evoke a positive affective reaction. Political strategists use "feel good" campaign techniques, and so on. Yet, empirical support for this assumption has often remained elusive in experimental studies (Clore et al., 1990). We offered a tentative model that may shed some light on this conundrum. Specifically, we proposed affective states elicited during exposure to an object do indeed have an enduring influence on attitudes toward the object. However, the direction of this influence may be diametrically opposite for different segments of the population. Assimilation effects, which are commonly assumed to exist, may occur only for individuals who possess high expertise in the domain of judgment or for individuals who recall little target information. Our results suggest that an opposite contrast effect can emerge for individuals who possess high expertise in the domain of judgment or for individuals who recall a great deal of target information. Although our use of expertise and recall measures precluded us from definitively isolating the causal process mechanism responsible for this reversal, the weight of our evidence suggests processing efficiency is the key moderating mechanism. In our view, low-expertise (or low-recall) participants exhibit an inefficient processing style that precludes them from possessing the necessary resources to engage in correction. Conversely, we contend high-expertise (or high-recall) individuals process the target information in a more efficient manner. As such, they are more likely to possess the necessary resources to engage in correction. In doing so, however, they may inadvertently overcorrect for the biasing influence of mood. This can produce a mood contrast effect on judgments. When pooling across these individual differences, these effects can cancel, resulting in no evidence of mood misattribution. Approaches that neglect these individual differences can therefore produce the mistaken impression that mood at exposure fails to influence subsequently reported evaluations of the target.

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