The Automaticity of Affect for Political Leaders, Groups, and Issues: An Experimental Test of the Hot Cognition Hypothesis

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We report the results of three experimental tests of the “hot cognition” hypothesis, which posits that all sociopolitical concepts that have been evaluated in the past are affectively charged and that this affective charge is automatically activated within milliseconds on mere exposure to the concept, appreciably faster than conscious appraisal of the object.

We find support for the automaticity of affect toward political leaders, groups, and issues; specifically:

• Most Ss show significantly faster reaction times to affectively congruent political concepts and significantly slower response times to affectively incongruent concepts;
• These facilitation and inhibition effects, which hold for a cross-section of political leaders, groups, and issues, are strongest for those with the strongest prior attitudes, with sophisticates showing the strongest effect on “harder” political issues.
• Even semantically unrelated affective concepts (e.g., “sunshine,” “cancer”) have a strong effect on the evaluation of political leaders, groups, and issues.

We conclude with a discussion of the “so what?” question—the conceptual, substantive, and normative implications of hot cognition for political judgments, evaluations, and choice. One clear expectation, given that affect appears to be activated automatically on mere exposure to sociopolitical concepts, is that most citizens, but especially those sophisticates with strong political attitudes, will be biased information processors.

KEY WORDS: hot cognition, implicit attitudes, motivated reasoning, automaticity, affect
In this paper we report the results of three experimental studies testing a central postulate of our dual-process model of motivated political reasoning (Lodge & Taber, 2000; Taber & Lodge, 2001; Taber, Lodge, & Glather, 2001). This theory of motivated reasoning starts with the *hot cognition hypothesis* (Abelson, 1963), the claim that all sociopolitical concepts are affect laden (Bargh, 1994, 1997; Fazio & Williams, 1986; Fazio, Sanbonmatsu, Powell, & Kardes, 1986; Fiske, 1982; Lodge & Stroh, 1993; Lodge, McGraw, & Stroh, 1993; McGraw, Lodge, & Stroh, 1990; Morris, Squires, Taber, & Lodge, 2003). All political leaders, groups, issues, symbols, and ideas thought about and evaluated in the past become affectively charged—positively or negatively—and this affect is linked directly to the concept in long-term memory. This evaluative tally, moreover, comes automatically and inescapably to mind upon presentation of the associated object, thereby signaling its affective coloration (what Clore & Isbell [2001] call the “how-do-I-feel heuristic?” and what Sniderman, Brody, & Tetlock [1991] call the “likability heuristic”). At the moment one realizes that the letters B-U-S-H in a news headline refer to the President and not to a plant, one’s affect toward “W” Bush comes to mind along with his strongest cognitive associations.

Our theory of motivated reasoning couples together affect and cognition in long-term memory and brings them automatically to mind in the judgment process. Feelings become information. Affect imbues the judgment process from start to finish—from the encoding of information, its retrieval and comprehension, to its expression as a preference or choice. Should this theory of the automaticity of affect prove to be a reasonable approximation of how people routinely think about political objects, it would have important substantive and normative implications. One clear expectation—given that affect permeates all thinking and reasoning—is that most citizens most of the time will be biased reasoners, finding it difficult to evaluate new, attitude-relevant information in an evenhanded way (Redlawsk, 2002). This is what we have found in a series of experiments designed to explore the impact of motivated reasoning on political information processing (McGraw, Lodge, & Jones, 2002; McGraw, Fischle, Stenner, & Lodge, 1996; Taber & Lodge, 2001). People appear unable to break free of their prior sentiments when evaluating arguments on political issues, even when they are motivated to be impartial. They are apt to see congruent arguments as inherently stronger than those which are attitudinally incongruent; they spend time and cognitive resources counterarguing the points that challenge their priors; they seek to insulate themselves from challenging information by actively searching out congruent information. As a consequence of this motivated search and reasoning, their attitudes are prone to polarize in the face of a balanced set of pro and con information (Harton & Latané, 1997; Lord, Ross, & Lepper, 1979; Taber & Lodge, 2001), with all of these effects strongest for sophisticated citizens with the strongest political attitudes.

While there is strong experimental support for the automaticity of a wide range of social attitudes, at least toward people and groups (Bargh, 1997;
Greenwald et al., 2002; Wegner & Bargh, 1998), there have been no convincing tests of the hot cognition hypothesis in the political domain, and none at all for issues. Moreover, some scholars suggest that the evaluations of groups, and even more so issues, may not be processed in the same way as those for people (see Bassili & Roy, 1998; Levine, 2001; McGraw & Steenbergen, 1995). Citizens may store and rely on more and different types of considerations when evaluating groups (Hamilton & Sherman, 1996) or issues (Zaller & Feldman, 1992). When evaluating issues, citizens are said to see two or more sides to policy disputes, and their awareness of the pros and cons may prevent them from forming a summary judgment. If so, when called on to report an evaluation of the group or issue, they cannot depend on a simple affective linkage in memory, but will sample a fuller set of considerations that have been stored in memory about the object and then and there construct an evaluation from an integration of these considerations (Tourangeau, Rips, & Rasinski, 2000; Zaller & Feldman, 1992).

The studies reported here directly test the hot cognition question: are attitudes toward political leaders, groups, and issues evoked automatically or do they require a more effortful—and time-consuming—process of evaluative integration? We leave for the Conclusion the “so what?” question: when and for what citizens will the primacy of affect influence judgment, evaluation, and choice?

The Underlying Model of Hot Cognition

Before turning to our experimental tests, let us briefly review the cognitive architecture underlying our dual-process theory of political information processing (Lodge & Stroh, 1993; Lodge & Taber, 2000; Taber, 2003). A cornerstone of any model of political reasoning is the citizen’s preexisting knowledge and predilections. These long-term factors, functionally speaking, require a vast long-term memory (LTM) for storing facts, beliefs, and predispositions, and a mechanism for “moving” one’s knowledge about leaders, parties, and issues from LTM into working memory (WM) where it can be attended to (Barsalou, 1992; Rumelhart & Ortony, 1977; Sanford, 1986; Simon, 1957). Attention is very limited, perhaps to the magic number 7 ± 2 bits or chunks of information, hence the need for heuristics, habits, and other simplifying mechanisms for thinking and reasoning (Baumeister, Muraven, & Tice, 2000; Cialdini, 2001).

LTM is organized associatively, and it is useful to think of knowledge structures metaphorically in LTM as configurations of nodes linked to one another in a network of associations (Anderson, 1983) or if you prefer as neurons “bundled” together by weighted connections (Read & Miller, 1998; Smith, 1998). Were we able to tap a citizen’s complete political knowledge structure, there might be tens of thousands of conceptual nodes (among them one for George W. Bush) with a complex network of associations (perhaps his demographics, stands on issues, perceived traits, and maybe an inferential abstraction or two—e.g., that he is conservative). Links represent beliefs, the strength of which will vary. Moreover,
memory objects vary in accessibility—the ease with which a stored concept lying dormant in LTM can be retrieved into WM.

Figure 1 depicts a simple example of the architecture of one woman’s political knowledge (for a somewhat similar framework, see Greenwald et al., 2002). Note first that the self is the strongest node in the network and that identity (here, female, black) and self-esteem are the strongest links in the network. Positive and negative affect and basic identity nodes are distinguished in this representation because of their centrality in human information processing. As with more standard semantic network models, beliefs are represented as links among basic memory objects (e.g., “I am a Democrat,” “President Bush has ties to big business”). Attitudes appear as links between basic memory objects and positive
and/or negative affect. Ambivalence can be represented by allowing links to both positivity and negativity, as with “American” in Figure 1. The impact of context or priming on evaluations may also be depicted: see, for example, that if “jobs” is primed, “business” will be seen in a positive light, while in the context of “greed” “business” is evaluated negatively. This model, taking its lead from Fazio and his colleagues (Fazio, 1995; Fazio & Williams, 1986), brings affect center stage (Fiske, 1981; Marcus, Newman, & McKuen, 2001; Sears, 2001; Sears, Huddy, & Schaffer, 1986). All objects in LTM representing sociopolitical concepts are affect laden, with affect varying along three dimensions: positivity, negativity, and strength.

But how is information moved from LTM into WM? *Spreading activation* provides the mechanism. A node in LTM switches from being dormant to a state of readiness with the potential to be moved into WM when it is activated, either as a direct object of thought or because it is closely linked to an object of thought. The top panel of Figure 2 (adapted from Barsalou, 1992) depicts the activation process, with the Y-axis representing the level of activation of a given node in LTM and the X-axis representing time in milliseconds. The rise time from dormant-state to activation threshold is almost instantaneous (100–200 milliseconds). Though not depicted in Figure 2, activation also decays quite rapidly so that a given node will drop back to baseline in about a second if there is no further source of activation. Imagine a person reading about President Bush in a news-
paper headline. Without perceptible effort, the concept BUSH becomes activated and activation spreads along the network of links to related concepts, thereby priming strong semantic associations of BUSH (he is a REPUBLICAN) as well as beliefs (he is pro-business). For a few hundred milliseconds, these associated concepts remain in a heightened state of arousal, with any additional activation likely to push them over threshold and into WM.

It may be useful to think of priming through spreading activation as producing preconscious expectations. The bottom panel of Figure 2 shows the activation of associations under different priming conditions. Consider again the activation of the concept BUSH from a newspaper headline. Concepts associated with BUSH in LTM also receive activation, thereby raising their potential so that any subsequent processing which passes activation to these energized concepts will likely drive them over threshold. If a primed association (perhaps Bush’s Republican label or his stand on gun control) is “expected,” it takes substantially less processing to activate and has a better chance of getting into WM, of being processed faster, and thereby of “framing” the perception, recognition, and interpretation of subsequent information.

Conversely, spreading activation can inhibit the processing of unexpected categories (the bottom course in Panel b of Figure 2). When a concept is encountered unexpectedly, more bottom-up processing is necessary before it may pass threshold and enter WM. If the word “walnut” were processed initially, this would inhibit the recognition of semantically unrelated concepts (such as REPUBLICAN), which would thereby require more time and effort to process. Finally, the middle course in Panel b is a control or “baseline” condition in which no “expectations” are created by a prime. The nonword BBB, for example, which conveys no semantic expectations, would neither facilitate nor inhibit the recognition and categorization of subsequent concepts.

Simple though it be—essentially an affect-enabled ACTSTAR model (Anderson, 1983)—such node-link models with affective links can account for important characteristics of human information processing (Boynton & Lodge, 1994). Moving in step with contemporary thinking (Bargh, Chaiken, Govender, & Pratto, 1992; Fazio, 1995; Petty & Krosnick, 1995), we see attitudes as associations in memory between an object and an evaluation, with the term “object” being defined very broadly to include the representation in memory of people, places, ideas, symbols, things, and events. In the case of univalent attitudes, the summary evaluation is uni-dimensional, a single link from object to affect (or perhaps reciprocal links to positivity and negativity as with BUSH in Figure 1), representing a distillation of judgments made online as stimulus information is processed. The associative strength between an object (e.g., politician) and its evaluation (bad) is conceived as varying along a continuum from nil, an object with little or no affective association (from this perspective a “nonattitude”; Converse, 1970; Fazio & Williams, 1986) to objects with strong associative strength. Whereas nonattitudes require piecemeal, bottom-up construction, and weak attitudes require effortful
retrieval, the stronger the association between an object in memory and its affective evaluation the less time and effort needed to bring the attitude to mind, with objects carrying strong affective links being activated automatically on exposure (see Bargh et al., 1992; Bassilli & Roy, 1998; Fazio, 1992).

An Experimental Test of the Hot Cognition Postulate

To turn the notion of hot cognition from premise to hypothesis, let us set forth the experimental paradigm for empirically testing the postulate that affect is directly linked to its conceptual node and “travels” with it into WM spontaneously on mere exposure of the concept. The attitude priming paradigm, developed by Fazio and his colleagues (1986), is a spin off of the classic lexical decision paradigm (Collins & Loftus, 1975; Collins & Quillian, 1969) where, for example, an experimental subject (S) sees a “prime” word (e.g., “FLOWERS”) flashed on a computer screen for 200 milliseconds, followed 100 milliseconds later—when as shown in Figure 2 the concept’s activation is at peak—by a second string of letters (say “Clinton” or “rose” or “rospar”) which remains onscreen until the S makes a response, typically by pressing one button “as fast as possible without making too many errors” if the target is a legal English word, the other button if it is not. This is a nonreactive task; the subject is not asked directly whether the target is associated with the prime, whether a rose is a flower (indeed, though this is not usually a subliminal task, the prime is onscreen so briefly that the S may be only dimly aware of it), but rather whether the letters r-o-s-e form a word in English. An inference as to whether the target and prime are linked in the observer’s LTM is made on the basis of their reaction times in performing the (word/not-a-word) task. These and similar cognitive priming paradigms produce robust effects: facilitation (faster response times) to cognitively associated concepts; inhibition to unrelated concepts. What is more, these effects are automatic—they cannot be easily suppressed or overridden (Greenwald & Banaji, 1995; Neely, 1977).

But what about affect? Is one’s affect also activated when the concept it is linked to is primed? That is the hot cognition question. As depicted in Figure 3

Panel a:

Prime Displayed
200 ms

Target Displayed
100 ms

Reaction Time

Button Response (Positive or Negative)

Panel b:

“Cockroach”
200 ms

“Delightful” or “Disgusting”
100 ms

Reaction Time

Delightful @ 800 ms

Disgusting @ 500 ms

Figure 3. Affective Priming Paradigm.
we expose Ss to a prime word and then present a target word, but in this variant of the paradigm the Ss’ task is to press a button labeled “+” or “−” to indicate “as fast as possible without making too many errors” whether the target word has a positive or negative connotation. Here again, on each trial the name of an attitude object (e.g., POLITICIAN) is presented for 200 ms on the computer screen, followed by a 100 ms blank-screen interval. Then a target word—chosen for its unambiguous positive or negative connotation—is presented. The subject’s task is to indicate by a button press whether the target word is “good” or “bad” in meaning. The latency time from onset of target word to the S’s response is recorded. If the valence associated with the prime (e.g., DEMOCRATS) is the same as the valence associated with the target (e.g., CANCER), then response times to classify the target should be faster relative to a neutral baseline (facilitation); if prime and target valences are incongruent, however, response times should be slower (inhibition).

The elapsed time from the onset of the prime to the onset of the target is called the stimulus onset asynchrony (SOA) and is often—as in our experiment—varied to test for the automaticity of responses (note this is a manipulated factor not to be confused with a subject’s reaction time). Since conscious expectancies take at least 500 ms to develop (Neely, 1977; Posner & Snyder, 1975), any influence of the prime on response times to the target for SOAs significantly shorter than 500 ms must be “attributed to an automatic, unintended activation of the corresponding attitude” (Bargh et al., 1992, p. 894). At longer SOA—we use 1,000 ms for the long SOA condition in our experiments—these automatic activation effects will decay unless they are consciously maintained, which will happen only when subjects anticipate that such expectancies will be useful for subsequent information processing (Neely, 1977). Since conscious expectancies are not diagnostic of target valence in the attitude priming paradigm (good and bad targets are equally likely after each prime), we would not expect conscious expectancies to be formed, and it follows that we should not observe facilitation or inhibition effects under long SOA (Fazio, 1990, 1995).

By way of example, in Figure 3b, if COCKROACH were the prime and the target word was “disgusting” we would expect facilitation—a fast response time (here, on the order of 500 ms) to say “disgusting” is a negative word—because the prime and target are affectively congruent. Conversely, for all but entomologists, if the target word was “delightful,” we would expect inhibition—a slower RT (on average about 800 ms) to say “delightful” is a positive word—because the association is affectively incongruent. In terms of our architectural model (Figure 1), when a previously evaluated concept (say REPUBLICAN) is primed it passes activation to its linked evaluative node(s). Then, when an affectively congruent target appears (say “rainbow”), the “shared” evaluative node is already in a heightened state of arousal so the evaluative response is potentiated and thereby made more easily and faster; whereas, the response to an affectively incongruent target (e.g., “cancer”) would be unexpected and relatively inhibited. Note again that this is a nonreactive measure: the S’s task is to say whether the target word is
positive or negative, \textit{not} whether the word is or is not associated with the prime. This attitude-priming paradigm proves to be a strong test for discerning whether affect is activated automatically along with the concept itself.

The logic that we have just described for the attitude priming paradigm would appear to depend on an explicitly evaluative task—that is, experimental subjects are asked whether the target word is positive or negative—and this may limit the generality of the findings to cases where one is intentionally processing affective information. The studies we report are subject to this limitation, but it may be useful to note that others have established the automaticity of social attitudes in the absence of an explicitly evaluative task (De Houwer, Hermans, & Spruyt, 2001; Hermans, De Houwer, & Eelen, 1994), for example using a word pronunciation task to collect responses to the target (Bargh, Chaiken, & Raymond, 1996). Duckworth, Bargh, and Garcia (2002) have even found automatic affective priming for completely novel primes, which are found to evoke an affective response despite the absence of any prior attitude (e.g., unfamiliar abstract art). Despite this potential limitation, however, it is important to emphasize that the procedure we follow provides a genuinely nonreactive measure: Subjects neither intentionally nor consciously process the affective value of the \textit{prime} word, and it is evaluative affect toward the prime rather than the target that interests us.

\section*{General Experimental Procedures}

Following a pilot study (Lodge & Taber, 2000), three experiments were conducted to test the hot cognition hypothesis in the political domain using the affective priming paradigm. Since these studies are similar in design, differing in the political primes, targets, and treatment of SOA, we will discuss them together.

\textit{Procedures.} Undergraduate students in introductory political science courses at Stony Brook University received extra credit for their participation: Study 1, \(N = 80\); Study 2, \(N = 162\); Study 3, \(N = 95\). All studies were conducted in our Laboratory for Political Research on Windows-based personal computers using the experimentation software package \textit{EPrime}. Subjects completed the task singly in separate experimental rooms.

The experiments proceeded in stages: First, subjects received instruction and practice using a button response on a computer keyboard to indicate “as quickly as possible without making too many errors” whether the second of two words that appeared on the computer screen was “positive/good” or “negative/bad.” The first word (the \textit{PRIME}) always appeared in upper case and remained in the center of the screen for a brief interval of 200 ms, followed either by a 100 ms blank screen, for a short SOA of 300 ms, or by an 800 ms interstimulus interval, for a long SOA of 1,000 ms. The second word (the target) then appeared center-screen in lower case and remained until the S’s button press. Trials were separated by a two-second pause from the response key press to the onset of the next prime. Primes and targets are listed in Table 1.
Table 1. Primes and Targets

### Primes

#### Study 1

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<td>.02</td>
<td>.14</td>
<td>Welfare</td>
<td>.03</td>
<td>.04</td>
</tr>
<tr>
<td>Osama bin Laden</td>
<td>−.91</td>
<td>−.43</td>
<td>.12</td>
<td></td>
<td>Terrorists</td>
<td>−.80</td>
<td>−.46</td>
<td>.10</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pataki</td>
<td>.34</td>
<td>−.03</td>
<td>.21</td>
<td></td>
<td>Average</td>
<td>.11</td>
<td>−.11</td>
<td>.19</td>
<td>Average</td>
<td>.22</td>
<td>−.10</td>
</tr>
</tbody>
</table>

*Note.* Valence, Ambivalence, and Strength are all coded on the interval (−1, 1), with 0 neutral.

### Targets

<table>
<thead>
<tr>
<th>Study 1</th>
<th>Study 2</th>
<th>Study 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>appealing</td>
<td>magnificent</td>
<td>cancer</td>
</tr>
<tr>
<td>awful</td>
<td>marvelous</td>
<td>comedy</td>
</tr>
<tr>
<td>beautiful</td>
<td>miserable</td>
<td>funeral</td>
</tr>
<tr>
<td>delightful</td>
<td>painful</td>
<td>mutilate</td>
</tr>
<tr>
<td>horrible</td>
<td>repulsive</td>
<td>rainbow</td>
</tr>
</tbody>
</table>
As noted earlier, manipulation of the stimulus onset asynchrony (SOA) allows us to assess the automaticity of response within the attitude-priming paradigm (Fazio, 1990, 2001). What is important to note is that automatic facilitation and inhibition effects are predicted only for the reaction time responses to targets under the short SOA condition, when subjects do not have time to consciously establish expectancies. Referring back to Figure 2 depicting the activation cycle, recall that the 300 ms prime-to-target interval delivers the target word at or near peak activation, when automatic inhibition or facilitation effects should be strongest. After a long SOA of 1,000 ms, by contrast, we would expect little or no priming effect—any conscious expectancies that could be triggered by the prime after 500 ms would not be diagnostic of target valence in our studies.

Following the attitude-priming tasks, a computer-based survey was administered to collect explicitly: each S’s dichotomous (good-bad) ratings of the target words; each S’s rating of the positivity of the prime words in a Likert format and separately their negativity ratings of the prime words to allow us to measure ambivalence as well as valence of the primes (see Cacioppo, Gardner, & Berntson, 1997); each S’s ratings of their strength of attitude toward the prime words; basic demographics; and an open-ended political knowledge test in which, in addition to civics-type questions, we asked for the current or most recent office held by each of the political figures among the primes.

Measures and Data Manipulations. The valence of the prime was measured as the difference between the positive and negative evaluations of each prime for the given subject, dichotomized so that any difference greater than zero is coded 1 (net positive), any difference less than zero is coded 0 (net negative), and any difference equal to zero is set to missing. The 9 pt. prime strength measure was dichotomized around the scale midpoint (coded so that 0 denotes weak and 1 strong). Prime ambivalence was computed using the Griffin formula, which averages the positive and negative ratings and subtracts the absolute value of the difference between positive and negative ratings (Levine, 2001; Meffert, Guge, & Lodge, forthcoming; Thompson, Zanna, & Griffin, 1995), and then split at the scale midpoint (0 denoting low ambivalence, 1 high). Sophistication was measured as the number of correct responses on the political knowledge test (17 possible), subjected to a median split, with 0 coded for unsophisticates, 1 for sophisticates.

By their nature, reaction time data are highly positively skewed, and this skewness can affect group means in the analysis of variance. To correct for positive skewness in our data (Study 1, skewness = 3.59; Study 2 = 3.74; Study 3 = 2.83), we subjected the raw reaction time data to a natural log transformation (Bargh & Chartrand, 2000; Fazio, 1990, 1993). All statistical results reported below are computed on these natural log transformed reaction time data; it is worth noting, however, that the overall pattern of results emerges with or without this transformation. In addition, we eliminated trials involving targets that had been incorrectly rated in the survey (e.g., someone might say that “miserable” was
a good thing, in which case we excluded the trials for that subject in which mis-
erable was the target; .04% of trials across the three studies), and we eliminated trials in which there was an incorrect response to the target on the RT (error rate of 5% across the three studies).

**Primes and Targets.** In choosing primes for our studies we wanted (a) a sample of concepts that included political objects (persons, groups, and issues), (b) an approximately even split for our subjects between positive and negative primes, and (c) variance in the ambivalence measure. The primes varied across studies (see Table 1). For target words (selected from Bradley & Lang, 1999), the most important criterion was that they had clear and widely accepted evaluative implications, half of them positive and half negative.

**Hypotheses and Design.** Studies 1 and 2 were two (SOA, long vs. short) \( \times 2 \) (prime valence, positive vs. negative) \( \times 2 \) (target valence, positive vs. negative) mixed model designs with repeated measures on prime and target valence; Study 3 differed in that SOA was manipulated within subjects.

In each of the studies, we hypothesize that reaction times will be faster for affectively congruent prime-target concepts (pos/pos and neg/neg) than for incon-
gruent pairs (neg/pos and pos/neg). This is the basic hot cognition hypothesis. Critical to the hot cognition postulate is that one’s feelings are triggered *automatically* on the mere presentation of the concept; accordingly, the predicted facilitation and inhibition effects should only show up in the short SOA condition when priming activation is at peak. Operationally, our most basic hypothesis is repre-

tented by the three-way interaction, SOA x prime valence x target valence. Note that we have no expectations about differential effects for negative or positive primes or targets, but only about the affective congruence of prime-target pairs.

These projected analyses will be broken down by sophistication (a between subjects correlate) and attitude strength (within subjects). In general, we predict that political sophisticates and those with strong attitudes would be most likely to have formed online affective links for all of the political objects we use as primes and so we expect stronger results for sophisticates than for unsophisticates and for primes that evoke strong attitudes.

Finally, the basic reason given for the expectation that groups and issues are less likely to be linked to evaluative affect is that attitudes toward these objects are thought to consciously evoke pro and con considerations and consequently be more ambivalent than are attitudes toward persons. Therefore, in addition to com-
paring hot cognition for the three prime types, we will directly test the underly-
ing contention that implicit attitudes should be weaker for ambivalent primes.

**Results**

To examine whether evaluatively congruent prime-target pairs elicit faster reaction times than incongruently paired concepts in the short SOA condition but not the long SOA condition, we performed a 2 (SOA) \( \times 2 \) (prime valence) \( \times 2 \)
(target valence) mixed effects analysis of variance with repeated measures on the second and third factors for each experiment (in Experiment 3, SOA was also manipulated within subjects). We are also interested in the degree to which this basic interaction is conditioned on prime type (person, group, or issue), on the sophistication of the respondent, on strength of attitude toward the prime, and on ambivalence toward the prime, all of which entail higher order interaction analyses.

Following a presentation format that we use throughout this paper, results are depicted as bar graphs in sets of four bars, each representing an average raw RT for one of the basic groups defined by the prime by target valence interaction: from left to right, negative primes/negative targets, positive primes/positive targets, positive primes/negative targets, and negative primes/positive targets. (To facilitate interpretation, these bar charts depict raw reaction times, but because of positive skewness, statistical analyses are computed on log normal transformed RTs.) We expect the RTs to the attitudinally congruent concepts to be faster (facilitation) than RTs to attitudinally incongruent pairs (inhibition). The appropriate comparison is between the first and third bars (for negative targets) and between the second and fourth bars (for positive targets).

The Hot Cognition Hypothesis. Looking first at the basic prediction for Study 1 for all political primes, we find strong support for the hypothesized three way interaction of SOA, prime, and target, $F(1, 78) = 14.29, p < .001$, with no significant main effects. This result is captured in Figure 4a, which contrasts the basic expected pattern of facilitation and inhibition effects at short SOA, with no facilitation/inhibition effects at long SOA. Follow up contrasts confirm the apparent pattern in Figure 4a: under short SOA, responses to negative targets are significantly faster when preceded by negative primes, $t(45) = 2.02, p = .025$ (one-tailed), while positive primes elicit faster response times when paired with positive targets, $t(44) = 2.26, p = .02$. As predicted, similar contrasts for long SOA failed to reach significance. (To reduce redundancy, we will limit the remaining figures to the short SOA condition, though we will continue to report the full interactions in text.)

Experiment 1 provides strong support for the hot cognition hypothesis: affect it seems is triggered automatically on mere presentation of a political attitude object. Unfortunately, it is possible (though we think implausible) that the priming effect we demonstrate in Study 1 represents a semantic rather than evaluative association in memory for our subjects. That is, the trait adjectives used in Study 1—e.g., appealing, delightful, repulsive—may be semantically linked with some of the political primes, in which case this semantic association could generate the priming effect we observe. We know that people are prone to make trait inferences spontaneously (Park, 1989; Rahn, Aldridge, & Borgida, 1994; Rapport, Metcalf, & Hartman, 1989; Uleman & Bargh, 1989), based on little direct evidence, so perhaps their affective responses are cognitively mediated, that something the Democrats did led our Ss to infer that that they are “horrible” or...
“marvelous.” This is in fact the implication of the classic semantic network model—people store their trait inferences with the concept node in LTM. Accordingly, the prime “Giuliani” activates the network of associations linked to him and spreading activation energizes a connection to something he did that had been interpreted as “magnificent” and consequently the target word is now responded to quickly. While it is something of a stretch to see how the trait concepts would be semantically linked to such issues as “peace” and “taxes,” it is conceivable that groups and policies are metonymically “personalized” with trait attributes (Lakoff, 1991, 2001).

We believe that an alternative, primacy of affect (Murphy & Zajonc, 1993; Zajonc, 1980) interpretation of these results is more plausible. Perhaps cognitive and affective systems follow separate though likely interdependent pathways in the brain, with feelings following a quick and dirty route (Le Doux, 1996) that “prepares” a behavioral response before one’s cognitive associations reach con-

![Graph](image-url)
scious awareness. A strong test of this hypothesis within the attitude-priming paradigm would break any reasonable cognitive connection between the attitudinal prime and the target concepts. This is what we do in Experiments 2 and 3—the attitudinal primes are again political persons, groups, and issues, but the affective target words are now nouns selected from Bradley and Lang’s (1999) *Affective Norms for English Words*, chosen to be affectively unambiguous and semantically unrelated to the leaders, groups, or issues (e.g., comedy, miracle, rainbow, toothache; see Table 1). If we find facilitation effects for semantically unrelated but affectively congruent primes and targets (and inhibition for semantically unrelated but affectively incongruent associations), we will have an even more convincing demonstration of the automaticity of affect for political objects.

In addition, Study 3 introduces a within subjects manipulation on SOA (the same subjects do half their trials at long and half at short SOA) and a much expanded set of primes (see Table 1). The within subjects design on SOA increases statistical power for Study 3.

Figure 4b presents the results at short SOA for Studies 2 and 3. As predicted, the three-way interaction for SOA, prime valence, and target valence was highly significant in both studies (computed on log transformed data): Study 2: $F(1, 160) = 20.26, p < .001$; Study 3: $F(1, 94) = 20.40, p < .001$ (with all main effects insignificant). Planned follow-up contrasts confirm the pattern of Figure 4b: under a short SOA, when responses could only be automatic, positive and negative congruent pairs were significantly faster than incongruent pairs (Study 2: for positive targets, $t(82) = 5.19, p < .001$ [all one-tailed tests]; negative targets, $t(81) = 4.08, p < .001$; Study 3: positive primes, $t(100) = 2.43, p < .01$; negative primes, $t(100) = 4.21, p < .001$). Again, no contrasts were significant at long SOA.

Taken together, support for hot cognition across these three studies is striking. Averaged responses across a wide range of political primes show clear evidence of an automatic link in memory between a broad array of political concepts and positive or negative affect. Moreover, Studies 2 and 3 eliminate any purely semantic interpretation of these facilitation and inhibition effects. But what about our contingent hypotheses predicting the automaticity of affect for political persons, groups, and issues? And will sophisticates be found to be more prone to the effects of automatic affect on political attitudes than unsophisticates?

**Prime Types.** Because of the relatively small sample size in Study 1, let us focus on Studies 2 and 3. Figures 5a and 5b break our basic interactions down into the three prime types—persons (e.g., Colin Powell, George W. Bush, Giuliani, Hillary), groups (e.g., Democrats, Republicans, African Americans, terrorists), and issues (e.g., Affirmative Action, Death Penalty, Pro-Life, Gun Control; see Table 1 for the full sets).

The hot cognition hypothesis is supported for all the political prime types. Table 2 reports the ANOVA results for the SOA x prime x target interactions for studies 2 and 3, broken down by prime type, with all expected three-way interactions significant (issues marginally so for Study 3). Follow up contrasts, also
reported in Table 2, test the expected pattern of results: at short SOA for both positive and negative targets, congruent primes elicited significantly faster response times than did incongruent primes; whereas at long SOA, there was no significant difference between congruent and incongruent pairs. In short, we find experimental support for the automatic activation of an evaluative tally for a wide range of political persons, groups, and issues.

**Sophistication Effects.** The hot cognition hypothesis predicts these facilitation and inhibition effects to be contingent on the political sophistication of the respondent. Political sophisticates, we reason, have thought about and repeatedly evaluated most of the political primes in the past, while subjects whose political knowledge falls below the sample median are less likely to have formed affective links in memory, and therefore should not display the pattern of facilitation and inhibition that indicates automatic affect. In short, we would expect a significant four-way interaction among SOA, prime valence, target valence, and sophistication. Studies 2 and 3 provide enough statistical power to test for this four-way interaction.

Figure 5. RTs by Prime Type at Short SOA.
interaction (study 2 because of a large sample size and study 3 because SOA is manipulated within subjects).

The pattern of sophistication effects depicted in Figure 6, as well as the ANOVA and follow up contrasts reported in Table 3, shows an intriguing difference across studies. Study 2 found facilitation and inhibition effects, indicating hot cognition, regardless of level of sophistication. This counterfinding for the sophistication interaction held for all primes taken together and for person and groups; interestingly, sophistication was an important moderator of hot cognition for issue primes. In Study 3, by contrast, low-knowledge subjects were as predicted less likely than sophisticates to display automatic affect toward the full set of primes and for each prime type taken separately.

**Table 2. ANOVA Results by Prime Type, Studies 2 and 3**

<table>
<thead>
<tr>
<th>Prime Types</th>
<th>Study 2</th>
<th>Study 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Persons</td>
<td>$F(1,154) = 8.28$, $p = .005$</td>
<td>$F(1,91) = 17.47$, $p = .000$</td>
</tr>
<tr>
<td>Groups</td>
<td>$F(1,78) = 3.95$, $p = .051$</td>
<td>$F(1,77) = 11.77$, $p = .001$</td>
</tr>
<tr>
<td>Issues</td>
<td>$F(1,135) = 11.23$, $p = .001$</td>
<td>$F(1,70) = 2.26$, $p = .137$</td>
</tr>
</tbody>
</table>

Follow Up Contrasts for Congruent vs. Incongruent Prime-Target Pairs at Short SOA

<table>
<thead>
<tr>
<th>Prime Types</th>
<th>Study 2</th>
<th>Study 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Positive Targets</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Negative Targets</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Persons</td>
<td>$t(80) = 2.02$, $p = .023$</td>
<td>$t(80) = 2.20$, $p = .016$</td>
</tr>
<tr>
<td>Groups</td>
<td>$t(43) = 2.39$, $p = .011$</td>
<td>$t(43) = 1.29$, $p = .102$</td>
</tr>
<tr>
<td>Issues</td>
<td>$t(73) = 2.54$, $p = .006$</td>
<td>$t(73) = 4.59$, $p = .000$</td>
</tr>
</tbody>
</table>

Note. These analyses contrast RTs for congruent pairs (e.g., positive-positive) with those for incongruent pairs (e.g., negative-positive) to test the hypothesis that congruent pairs are faster than incongruent pairs.

**Table 3. ANOVA Results by Sophistication, Studies 2 and 3**

<table>
<thead>
<tr>
<th>Prime Types</th>
<th>Study 2</th>
<th>Study 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Primes</td>
<td>$F(1,158) = 1.52$, $p = .192$</td>
<td>$F(1,93) = 21.14$, $p = .000$</td>
</tr>
<tr>
<td>Persons</td>
<td>$F(1,152) = 0.26$, $p = .633$</td>
<td>$F(1,90) = 4.17$, $p = .044$</td>
</tr>
<tr>
<td>Groups</td>
<td>$F(1,76) = 0.13$, $p = .717$</td>
<td>$F(1,76) = 8.82$, $p = .004$</td>
</tr>
<tr>
<td>Issues</td>
<td>$F(1,133) = 3.19$, $p = .076$</td>
<td>$F(1,69) = 5.70$, $p = .020$</td>
</tr>
</tbody>
</table>

Follow Up Contrasts for Congruent vs. Incongruent Prime-Target Pairs at Short SOA, All Primes

<table>
<thead>
<tr>
<th>Sophistication</th>
<th>Study 2</th>
<th>Study 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Positive Targets</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Negative Targets</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sophisticates</td>
<td>$t(46) = 4.81$, $p = .000$</td>
<td>$t(46) = 3.75$, $p = .000$</td>
</tr>
<tr>
<td>Unsophisticates</td>
<td>$t(35) = 2.32$, $p = .013$</td>
<td>$t(34) = 1.80$, $p = .041$</td>
</tr>
</tbody>
</table>

Note. These analyses contrast RTs for congruent pairs (e.g., positive-positive) with those for incongruent pairs (e.g., negative-positive) to test the hypothesis that congruent pairs are faster than incongruent pairs.
This overall pattern lends credence to the theoretical expectations underlying the formation of OL tallies in suggesting that sophisticates, because of their interest in politics, have formed crystallized attitudes to a fuller set of political issues. Note that the person and group primes used in Study 2 are “easier,” more mainstream, and more likely to have been thought about and evaluated in the past by our subjects than are many of the primes in Study 3 (Cobb & Kuklinski, 1997). Virtually all New Yorkers in the aftermath of the 2000 election, regardless of level of sophistication, would have given some thought to George Bush, Al Gore, Hillary Clinton, and Rudy Giuliani. Similarly, most everyone would have formed an attitude about such mainstream groups as Democrats, Republicans, and politicians. Consider now the broader and more difficult sample of primes used in Study 3. In addition to the mainstream political persons, groups, and issues, we purposely included the somewhat obscure (even locally!) New York mayoral candidates, the NAACP and NRA, and a range of issues such as “counterterrorism,” the “death penalty,” and “pro-life” that we (knowing the quality of our undergraduates) can easily imagine many subjects not having thought much about or evaluated in the past. These “harder” primes—and especially the issue primes—seem on their face to have required more thought than unsophisticates were likely to have invested. Moreover, the issues may be more likely to induce ambivalence when they are evaluated, a point to which we now turn (on the other hand, Table 1 shows that averaging across all subjects ambivalence and attitude strength did not line up neatly by prime type).

**Ambivalence and Attitude Strength.** One of the more interesting theoretical arguments made about the automaticity of affect is the contention that ambivalent attitudes may require a different processing mechanism and a different pattern of linkages in LTM than simpler univalent attitudes (Bassili & Roy, 1998; Levine, 2001; McGraw & Steenbergen, 1995). We agree. Recall in Figure 1 we repre-
sented an ambivalent attitude toward Americans as links to both positive and negative affect. Priming an ambivalent attitude object should pass activation to both positivity and negativity. Predictably, this dual activation should generate neither strong facilitation nor strong inhibition effects.

Both Studies 2 and 3 confirm the importance of well-formed, accessible, or “crystallized” attitudes. As shown in Figure 7, neither ambivalent nor weak primes elicit significant facilitation/inhibition effects at short SOA that would indicate automatic hot cognition, but we do find automatic affect for both unambivalent and strong primes. These descriptive results are confirmed in the ANOVA analyses and contrasts reported in Table 4. In both studies, the four way interactions among SOA, prime valence, target valence, and ambivalence were significant as were the four-way interactions among SOA, prime valence, target valence, and attitude strength. Planned follow-up contrasts showed that unambivalent and strong primes led to the expected pattern of facilitation for affectively consistent targets and inhibition for affectively inconsistent targets, while ambivalent and
weak primes did not show evidence of automatic affect (indeed the pattern was generally reversed, with longer RTs for congruent than incongruent pairs).

**Conclusion**

In all three experiments we have documented the automaticity of affect across a broad range of political concepts. We find consistent support for the hot cognition hypothesis for political leaders, groups, and issues (especially among those with the strongest attitudes, and for nonambivalent primes, and for sophisticates in the evaluation of “hard” political issues). But why is this important? Of what possible significance can this first split second of information processing be? There are two fundamental implications of this research for political science. First, we believe that the hot cognition postulate promises a partial answer to a puzzle of long concern to political scientists—the problem of rational action by citizens in a democracy (Kinder, 1998; Page & Shapiro, 1992; Sniderman, 1993; Taber, 2003). Second, the primacy and automaticity of affect kick-start the processes that spark motivated biases when citizens encounter attitudinally contrary information (Ditto & Lopez, 1992; Huang & Price, 2001; Lord, Ross, & Lepper, 1979; McGraw et al., 1996; Munro et al., 2002; Sigelman & Sigelman, 1984; Taber & Lodge, 2001).

Our field has not been kind to the democratic citizen. Normative democratic theory imposes heroic expectations about the capacity and motivation of *homo politicus*, while modern empirical research finds many citizens to be *homo-not-so-sapiens*. Surveys consistently find respondents to be distressingly ignorant of and uninterested in things political. How, one might ask, can democracy survive

### Table 4. ANOVA Results by Ambivalence and Attitude Strength, Studies 2 and 3

<table>
<thead>
<tr>
<th>Interaction</th>
<th>Study 2</th>
<th>Study 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>SOA x Prime x Target</td>
<td>$F(1,99) = 6.01$</td>
<td>$F(1,53) = 7.85$</td>
</tr>
<tr>
<td>SOA x Prime x Target</td>
<td>$F(1,64) = 5.78$</td>
<td>$F(1,33) = 11.32$</td>
</tr>
</tbody>
</table>

<p>| Follow Up Contrasts for Congruent vs. Incongruent Prime-Target Pairs at Short SOA |
|-----------------------------------|------------------|------------------|</p>
<table>
<thead>
<tr>
<th>Primes</th>
<th>Positive Targets</th>
<th>Negative Targets</th>
<th>Positive Targets</th>
<th>Negative Targets</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ambivalent</td>
<td>$t(57) = -1.80$, $p = .039$</td>
<td>$t(57) = -1.24$, $p = .110$</td>
<td>$t(89) = -1.12$, $ns$</td>
<td>$t(89) = -0.96$, $ns$</td>
</tr>
<tr>
<td>Unambivalent</td>
<td>$t(81) = 4.90$, $p = .000$</td>
<td>$t(81) = 4.86$, $p = .000$</td>
<td>$t(98) = 9.00$, $p = .000$</td>
<td>$t(98) = 5.94$, $p = .000$</td>
</tr>
<tr>
<td>Weak</td>
<td>$t(52) = -2.44$, $p = .009$</td>
<td>$t(51) = -0.74$, $ns$</td>
<td>$t(91) = -1.89$, $p = .030$</td>
<td>$t(88) = -0.15$, $ns$</td>
</tr>
<tr>
<td>Strong</td>
<td>$t(60) = 3.56$, $p = .001$</td>
<td>$t(60) = 4.80$, $p = .000$</td>
<td>$t(96) = 4.32$, $p = .000$</td>
<td>$t(74) = 3.52$, $p = .001$</td>
</tr>
</tbody>
</table>

Note that positive t-values indicate the expected facilitation and inhibition effects, while negative t-values indicate a reverse pattern: faster RTs for inconsistent than for consistent pairs.
if large majorities lack the basic wherewithal to behave as rational citizens? Perhaps the most serious theoretical challenge to the ability of citizens to behave as fully rational creatures concerns their limited capacity to process information (Simon, 1981). At minimum, it seems, citizens must be able to form attitudes, impressions, and evaluations and choose among political leaders, groups, and ideas.

Unfortunately, the level of ignorance and apathy found regularly in public opinion surveys (and among our participants on the knowledge test!) calls into question even this basic requirement of rational action. In our view, one of the most exciting—and paradoxical—implications of the hot cognition hypothesis is the notion that people internalize simple summary evaluations, formed spontaneously as part of an online evaluation process, as they encounter political information. Once formed, such running tallies (or more accurately, links) provide a ready-made liking heuristic to guide future behavior (Cialdini, 2001; Marcus, Neuman, & MacKuen, 2001; Sears, 2001; Slovic, Finucane, Peters, & MacGregor, 2002; Sniderman, Brody, & Tetlockk, 1991). To the extent that such affective links spontaneously provide an evaluative distillation of the stream of information to which the citizen has been exposed, they would seem to offer a fast and relatively simple way around the rationality dilemma (Lodge, Steenbergen, & Brau, 1995). Moreover, unlike most work on heuristic information processing, which offers the promise of low-information rationality, hot cognition, and the online model may provide high information rationality in the sense that evaluative tallies appear to reflect a summing up of one’s prior evaluations, a distillation of the evaluative implications of most if not all relevant information one has been exposed to (Betsch, Plessner, Schwieren, & Gutig, 2001; Taber, 2003).

Because affect comes to mind automatically at the earliest stages of information processing, we would expect affect to have an immediate “primacy effect” on subsequent processing, such that one’s prior attitudes will powerfully constrain the interpretation, depth of processing, and evaluation of new information, as well as one’s ultimate course of action. While it seems highly unlikely that the evaluative tallies deposited in memory through time will be an unbiased reflection of experience, the critical questions become where and when will citizens be motivated reasoners (Kunda, 1990)? In a series of complementary experiments we repeatedly find (Taber & Lodge, 2001)—as do others in nonpolitical domains (Ditto & Lopez, 1992; Edwards & Smith, 1996; Lord, Ross, & Lepper, 1979; Munro et al., 2002)—that one’s prior attitudes are quite resistant to change. Even when motivated to be even-handed, “to leave their feelings aside,” people find it near impossible to view political policy arguments dispassionately (on gun control, affirmative action, federal support for the arts, etc.). Those holding strong attitudes actively counterargue contrary facts, figures, and interpretations while uncritically accepting attitudinally congruent information—a disconfirmation bias—and they actively seek out supporting information so as to bolster and protect their priors—a confirmation bias. Moreover, both selective biases lead to
attitude polarization, especially among the sophisticated and those with strong priors.

To what degree do our findings on the longer-term consequences of automatic affect undermine rationality? To the extent that motivated biases like those described above overwhelm the objective quality of information, the “hot cognition heuristic” may not be much of a solution to the rationality puzzle. But such “biases” may be innocuous, even useful, when they stop at healthy skepticism, allowing new information to have an independent impact on the evaluation process. When does automatic affect lead to rational skepticism and when does it drive irrational bias? This is a prime question on our agenda for future research.

The experiments reported here find robust facilitation and inhibition effects for political leaders, groups, and issues, complementing research in psychology on the automaticity of nonpolitical attitudes (Bargh et al., 1992; Fazio, 1992; Greenwald & Banaji, 1995). The results of our Experiments 2 and 3 in which we find that the predicted prime valence x target valence interactions even hold (in fact appear stronger) when the targets are semantically unrelated to the primes cannot be readily explained by purely cognitive models. There is no discernable semantic link between, say, Gore or Bush, and “rainbow,” “toothache,” or “mutilate,” yet the responses are speeded up significantly when the prime and target concepts are affectively congruent and slowed down when attitudinally incongruent. Certainly these results offer strong support for the prevalence of hot cognition in political information processing (Marcus, Newman, & MacKuen, 2001). But what are the implications of these findings for the underlying theoretical architecture of political attitudes?

One possibility, following Zajonc’s (1980, 1984) account of the primacy of affect, is that the cognitive and affective systems are separable and somewhat independent (though perhaps architecturally interrelated as depicted in our Figure 1). There is some neurological evidence (e.g., LeDoux, 1996) that the affect system is easily and swiftly sparked and once activated generates a “quick and dirty” approach-avoidance reaction to the situation (JUMP, before you know if it is a stick or a snake), with conscious, deliberative appraisal following moments later. From this perspective the automatic affective response is primary and may or may not (depending on individual and situational factors) be overturned by a later conscious, cognitive assessment (Devine, 1989; Murphy & Zajonc, 1993).

A related perspective—complementary to Zajonc’s independent systems—sees response competition as a plausible explanation for the attitudinal priming effect (DeHouwer, 2001). By this account, attitudes are inexorably linked to behavior. Attitudinal objects automatically potentiate a bivalent behavioral response. Mere exposure to an attitudinal object “readies” an immediate approach-avoidance behavioral response. When the prime and target are affectively congruent the behavioral response to the target is speeded up, but when the pairing is affectively incongruent the prepared response must first be inhibited, then redirected, and is consequently slowed down. In this light, a negative attitudinal object
readies an “avoidance” response, which, were it followed by a contrary signal (a “false alarm”), the “to-be-emitted” action must first be stopped and an alternative forward-looking “go” response initiated.

Both Zajonc’s independent-systems perspective and the response competition explanation accomplish a long-sought desideratum of social science—they directly link attitudes to behavior. What is critical from our perspective is that political beliefs, feelings, intentions, and actions will, if repeatedly associated, become interconnected in a network of interdependencies that becomes “automatized” in everyday thinking, feeling, and acting, only becoming disassociated in pathological cases (Gazzaniga, 1992, 1998). From this perspective Damasio (1994, 2002) is right in claiming, “the brain is a feeling machine for thinking” and William James (1890) was right in believing that “thinking is for doing.”

ACKNOWLEDGMENTS

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REFERENCES


The perception of threat and the experience of anxiety are distinct but related public reactions to terrorism. Anxiety increases risk aversion, potentially undercutting support for dangerous military action, consistent with terrorists’ typical aims. Conversely, perceived threat increases a desire for retaliation and promotes animosity toward a threatening enemy, in line with the usual goals of affected governments. Findings from a national telephone survey confirm the differing political effects of anxiety and perceived threat. The minority of Americans who experienced high levels of anxiety in response to the September 11 attacks were less supportive of aggressive military action against terrorists, less approving of President Bush, and favored increased American isolationism. In contrast, the majority of Americans who perceived a high threat of future terrorism in the United States (but were not overly anxious) supported the Bush administration’s antiterrorism policies domestically and internationally.

Psychological reactions to terrorism play a pivotal role in understanding public support for government antiterrorist policies. As Crenshaw argues: “The political effectiveness of terrorism is importantly determined by the psychological effects of violence on audiences” (1986, 400). In an area of research characterized by disagreement over the definition and objectives of terrorism, there is pervasive agreement that the effects of terrorism extend well beyond its immediate victims and physical destruction to include a much broader target population (Crenshaw 1986; Long 1990; Wardlaw 1982).

There are differing psychological reactions to external threat, however, and these reactions shape support of government policies designed to combat terrorism. Based on a review of the literature below, we draw a critical distinction between perceived threat and the anxiety it can elicit. The political importance of this distinction between perceived threat and anxiety rests on their typical psychological effects: anxiety leads to an overestimation of risk and risk-averse behavior (Lerner and Keltner 2000, 2001; Raghunathan and Pham 1999) whereas external and perceived threat increase support for outwardly focused retaliatory action (Herrmann, Tetlock, and Visser 1999; Jentleson 1992; Jentleson and Britton 1998).

The distinction between perceived threat and anxiety is intimately tied to the major objectives of terrorists and governments in countries targeted by terrorism. A major function of terrorist violence is to instill anxiety in a target population; this anxiety then places pressure on political elites to negotiate and make concessions with terrorists in order to mollify their frightened citizens (Friedland and Merari 1985; Long 1990). Long argues that terrorists often “use the unreasonable fear and the resulting political disaffection it has generated among the public to intimidate governments into making political concessions in line with its political goals” (1990, 5). In this sense, terrorists may have a good grasp of psychological reality. The intended effects of terrorism are consistent with the psychological link between anxiety and risk aversion.

These motives contrast starkly, however, with the need of governments in vulnerable countries to take forceful action against terrorists. As Berry puts it: “A target that is incapable of responding to terrorism will lose public support and lessen its capabilities and confidence to...”
Threat not only promotes intolerance but also leads to support for punitive action against threatening groups. In past research on foreign policy attitudes, Americans have supported overseas military action in direct proportion to the threat posed by a foreign aggressor to U.S. interests (Herrmann, Tetlock, and Visser 1999; Jentleson 1992; Jentleson and Britton 1998). Terrorist threat is also associated with support for aggressive military action among Israelis (Friedland and Merari 1985). Arian (1989), for example, found a direct link between the perceived likelihood of war and a preference for an increase in military power over peace negotiations among Israelis in the 1980s. The degree of external threat posed by various outgroups also predicts how negatively Israelis feel towards group members, confirming the general link between terrorist threat and heightened prejudice (Bar-Tal and Labin 2001).

These findings point to the surprisingly consistent effects of external and perceived threat across a broad range of studies. Both types of threat lead to the vilification of the source of threat, limit support for government actions that might assist members of the threatening group, promote support for belligerent solutions directed at threatening individuals or groups, and heighten in-group solidarity. The effects of threat are especially impressive given its varied definition and measurement. Even more impressive, when threat is manipulated experimentally we find it to be not just a correlate but a clear cause of ethnocentrism, intolerance, and a desire for retaliation (Grant and Brown 1995; Herrmann, Tetlock, and Visser 1999; Marcus et al. 1995).

Before concluding that external and perceived threat always promotes support for belligerent action against an aggressor, however, we should examine the concept of threat more closely. Most research, with the exception of some in Israel, has not examined powerful physical threats, such as terrorism, that are likely to arouse high levels of anxiety. We now review some specific psychological effects of anxiety, including heightened risk perception and risk aversion (Lerner et al. 2003; Lerner and Keltner 2000, 2001; Raghunathan and Pham 1999), which may help to explain why an external threat sometimes fosters support for belligerent and risky policies (Gordon and Arian 2001; Kahneman and Tversky 1979) and at other times leads to conciliatory or risk-averse behavior (Arian 1989; Niemi, Mueller, and Smith 1989).

The Distinctive Effects of Anxiety

Recent research in psychology points to three specific effects of anxiety that differ from the general response to perceived threat (Lerner and Keltner 2000, 2001;
Mathews and Macleod 1986). First, anxiety worsens cognitive functioning because it diverts attention to threatening stimuli and increases cognitive preoccupation with threatening sources, shifting attention and resources away from nonthreatening stimuli (Eysenck 1992; MacLeod and Mathews 1988; Mathews and McLeod 1986; Mogg et al. 1990; Yiend and Mathews 2001). Anxiety has especially detrimental effects on tasks that involve working memory such as reading comprehension and specific word recall (Eysenck 1992). Anxiety can improve some limited cognitive functions—anxious individuals may more readily detect additional environmental threat, for example (Byrne and Eysenck 1995)—but, overall, cognitive functioning is impaired by high anxiety.

Second, anxious individuals tend to perceive higher levels of risk than those who are not anxious (Butler and Mathews 1983; Eysenck 1992; Lerner and Keltner 2000, 2001). Anxiety is especially likely to increase the perceived risks associated with personally relevant negative events (Butler and Mathews 1983, 1987). According to Lerner and Keltner (2000, 2001) anxiety produces a sense of uncertainty and lack of control that elevates future judgments of risk. Anxiety may also increase perceived risk because it heightens the salience of self-relevant negative thoughts (MacLeod, Williams, and Bekerian 1991). In general, anxiety increases the perceived risk of negative self-relevant events, but not necessarily events that lack personal relevance.

Third, anxiety increases risk aversion because anxious individuals are motivated to reduce their anxiety, leading to a preference for less risky options (Lerner and Keltner 2001; Raghunathan and Pham 1999).

The differing psychological effects of anxiety and perceived threat shed light on reactions to government antiterrorism policies. Perceived threat is likely to increase the desire for government retaliation against an enemy, whereas anxiety will undercut this support to the extent that the proposed retaliatory action is seen as personally dangerous and risky. The divergent political consequences of anxiety and perceived threat should be most pronounced on government military action that could be seen to incite future terrorist retaliation. There should be no such conflict between anxiety and perceived threat, however, when it comes to support for domestic actions such as heightened internal surveillance which Americans view as aimed at guilty others, not at them (Huddy, Khatib, and Capelos 2002). Anxious individuals are thus unlikely to feel personally threatened by domestic antiterrorism policies and may actually be more supportive of them than the nonanxious.

Factors Linked to Heightened Threat Perceptions

Perceived threat and anxiety have distinct psychological and political effects. They also have somewhat different antecedents. Direct personal experience with terrorism has an especially powerful effect on the development of anxiety and related psychological symptoms (Gordon and Arian 2001; Galea et al. 2002; Piotrkowski and Brannen 2002; Schuster et al. 2001; Silver et al. 2002). The link between personal experience and anxiety may arise because individuals who are physically closest to a terrorist incident experience the event as more vivid, leading to heightened emotional arousal (Lowenstein et al. 2001). Such experiences may also arouse a sense of personal vulnerability, leading to the development of anxious thoughts about one’s physical well-being. Personal experience can elevate threat perceptions as observed by Fischhoff and colleagues (2003), but we expect the link between personal experience and anxiety to be greater than that between personal experience and perceived threat.

Perceived threat and anxiety are distinct reactions, but they are also related for obvious reasons. Someone who disputes the existence of any future terrorist threat is unlikely to feel anxious about terrorism. Of course, not everyone who perceives a threat will necessarily feel anxious. There are several factors that influence the development of both reactions. Gender is the most powerful of these. Women express higher levels of anxiety and perceive greater risks associated with war and terrorism (Arian and Gordon 1993; Fischhoff et al. 2003; Lerner et al. 2003; Skitka, Bauman, and Mullen 2004). Lower levels of education have also been found to increase anxiety and the perceived risk of terrorism (Friedland and Merari 1985; Skitka, Bauman, and Mullen 2004). There are two differing explanations for this link: highly educated individuals have greater facility with probabilistic information and can better reason about the future likelihood of a terrorist attack and personal victimization (Edwards 1983), and lower levels of education are associated with greater life stressors which reduce a sense of control and heighten responses to threatening events (Fischhoff et al. 2003; Perilla, Norris, and Lavizzo 2002; Vaughan 1993).

Hypotheses

We study Americans’ reactions to the threat of terrorism to better understand the political effects of perceived...
threat and anxiety on support for government antiterrorist policies. Data are drawn from the Threat and National Security Survey (TNSS), a national telephone survey that assessed reactions to the events of September 11. We test the following hypotheses: (1) Perceived threat is distinct from anxiety and has differing determinants, although the two are related. (2) Anxiety is linked to other psychological symptoms of distress but perceived threat is not. (3) Anxiety lowers knowledge about the event and its aftermath because of its tendency to impair cognitive functioning. (4) Anxiety and perceived threat lead to differences in support for antiterrorism policies: Perceived threat increases negative views of Arabs and leads to support for policies that strike out at the enemy. In contrast, anxiety reduces support for any retaliatory policies that could jeopardize American security. (5) Perceived threat increases support for homeland security policies designed to minimize future risk, even when such policies violate support for civil liberties; anxiety may also foster support for homeland security policies because such policies are designed to minimize the future risk of terrorism.

**Results**

**Sample**

The survey was conducted via telephone with 1,549 adults aged 18 or older between early October 2001 and early March 2002. The sample was drawn as a random-digit-dial (RDD) weekly rolling cross-section with roughly 100 individuals interviewed each week throughout the time period. The first month of data was collected by Shulman, Ronca, and Bukuvalis and the remainder by the Stony Brook University Center for Survey Research. The cooperation rate for the survey was 52%.²

**Distinct Reactions to External Threat**

Americans exhibited a range of responses to September 11, as seen in Table 1. The survey included two questions that tapped the perceived threat of future terrorism to the nation: “How concerned are you that there will be another terrorist attack on the United States in the near future?” and “How concerned are you that terrorists will attack the United States with biological or chemical weapons?”

²Respondents were of similar income level to the national population but were slightly more middle-aged, somewhat better educated, slightly less black, and somewhat more female, in line with other national telephone surveys (Brehm 1993). Post-stratification weights based on 2002 CPS figures for education, gender, and geographic region did not alter frequencies for key variables by more than 1 percentage point. The data remain unweighted in all reported analyses.

Levels of perceived national threat were quite high: 86% reported that they were very or somewhat concerned about another attack, and 84% were very or somewhat concerned about the threat of biological or chemical attacks. Perceived personal threat was assessed with one question: “How concerned are you personally about you yourself, a friend, or a relative being the victim of a future terrorist attack in the United States?” A surprisingly high 68% of respondents reported being very or somewhat concerned about being personally affected by a terrorist attack; 31% were very concerned.

Americans perceived a high level of terrorist threat to themselves and the nation, but varied in the degree to which they felt anxious. Respondents were asked “How much, if any, have the terrorist attacks shaken your own sense of personal safety and security?” A small minority (almost 18%) of the sample said that the attacks had shaken their sense of personal safety and security a great deal, although an additional 34% said that it had shaken them some. That left 47% who said the attacks had little or no effect on their sense of safety and security. Respondents were also asked how often they had felt four anxiety-related emotions: anxious, scared, frightened, or worried. Almost half reported feeling anxious or worried at least sometimes, and a small minority reported feeling these emotions very often. In addition, just under a third reported feeling scared or frightened sometimes or very often. But that left a majority who did not feel frightened or scared, or felt that way only occasionally. On average, the perceived threat of terrorism was more widespread than feelings of anxiety in the aftermath of 9/11.

To verify empirically the distinction between anxiety and perceived threat, several confirmatory factor analyses (and all subsequent analyses) were estimated using Mplus which has more robust estimators for categorical and ordinal variables (such as 4-point measures of anxiety and perceived threat) than other covariance structure model programs (Muthen and Muthen 2001). Mplus estimates the link between discrete observed indicators and an underlying continuous latent variable as probit or ordered probit functions, using estimated latent thresholds instead of interval scores for the discrete indicators. This method is used to arrive at factor loadings for all anxiety and threat variables. All models were estimated via weighted least squares, which is more appropriate than maximum likelihood for models with discrete variables (see Muthen and Muthen 2001). We also report robust standard error estimates.

The four anxiety items and feeling shaken by the 9/11 attacks were expected to load on one anxiety factor and the three perceived threat items to load on another, to yield two distinct but related dimensions. An initial factor
Table 1  Levels of Perceived Threat and Anxiety

<table>
<thead>
<tr>
<th></th>
<th>Very Concerned</th>
<th>Somewhat Concerned</th>
<th>Not Very Concerned</th>
<th>Not at All Concerned</th>
<th>DK/NA</th>
</tr>
</thead>
<tbody>
<tr>
<td>How concerned are you that there will be another terrorist attack on U.S. soil in the near future?</td>
<td>49.8%</td>
<td>36.5</td>
<td>9.7</td>
<td>3.5</td>
<td>0.4</td>
</tr>
<tr>
<td>How concerned are you that terrorists will attack the United States with biological or chemical weapons?</td>
<td>47.3%</td>
<td>37.4</td>
<td>10.2</td>
<td>3.8</td>
<td>1.3</td>
</tr>
<tr>
<td>How concerned are you personally about you, yourself, a friend, or a relative being the victim of a future terrorist attack in the United States?</td>
<td>30.8%</td>
<td>37.1</td>
<td>19.8</td>
<td>11.2</td>
<td>1.2</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>A Great Deal</th>
<th>Some</th>
<th>A Little</th>
<th>Not at All</th>
<th>DK/NA</th>
</tr>
</thead>
<tbody>
<tr>
<td>How much, if any, have the terrorist attacks shaken your own sense of personal safety and security?</td>
<td>17.8%</td>
<td>34.2</td>
<td>23.4</td>
<td>23.8</td>
<td>0.9</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Very Often</th>
<th>Sometimes</th>
<th>Not Very Often</th>
<th>Never</th>
<th>DK/NA</th>
</tr>
</thead>
<tbody>
<tr>
<td>As you think about the terrorist attacks and the U.S. response, how often have you felt . . .</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Anxious?</td>
<td>11.4%</td>
<td>35.7</td>
<td>27.9</td>
<td>23.6</td>
<td>1.4</td>
</tr>
<tr>
<td>Scared?</td>
<td>7.9%</td>
<td>23.3</td>
<td>28.7</td>
<td>38.3</td>
<td>0.8</td>
</tr>
<tr>
<td>Worried?</td>
<td>13.0%</td>
<td>36.9</td>
<td>25.9</td>
<td>22.9</td>
<td>1.2</td>
</tr>
<tr>
<td>Frightened?</td>
<td>5.6%</td>
<td>24.5</td>
<td>27.4</td>
<td>41.3</td>
<td>1.2</td>
</tr>
</tbody>
</table>

Note: All entries are percentages.

analysis confirmed that the four emotion items tap a single anxiety dimension. We then tested the null hypothesis that anxiety is indistinguishable from perceived threat. In this model, a single latent factor was assumed to account for the covariance among all eight of the reactions to 9/11 listed in Table 1. This one-factor model was a very poor fit to the data. The ratio of the chi-squared value to the degrees of freedom was over 69, a very large value. The RMSEA was .215, well above the acceptable .10 value, and the residuals were substantial.3

A two-factor model in which anxiety and perceived threat formed distinct factors proved a much better fit to the data. The normed fit indices for this model were close to 1.0, but other indicators of fit were less satisfactory. The chi-squared value to the degrees of freedom ratio was quite large (30), the RMSEA was .14, and there were some large residuals. Model diagnostics suggested that a concern about the personal risk of terrorism may load on the anxiety factor, providing further empirical acknowledgement of the link between anxiety and personal risk. And a shaken sense of personal safety and security appeared to load on the perceived threat factor, suggesting that feeling shaken indicates a mixture of anxiety and perceived threat. Adding these two parameters produced the two-factor model shown in Table 2, which is a slight modification of our original expectations. The chi-squared/degrees of freedom ratio decreased to 5.6, and the RMSEA dropped to .056, indicating a very good fit.4

Perceived threat and anxiety form two distinct factors in this revised and improved model. Perceived threat is most clearly defined by the two questions on concerns about future terrorist attacks on the United States. Concerns about the likelihood of personal (family and friends) consequences of terrorism is a somewhat weaker indicator of threat; feeling that the attacks had shaken one's

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3The RMSEA (root mean squared error of approximation) provides a measure of model discrepancy per degree of freedom and thus adds a penalty for adding parameters. Values of RMSEA less than .10 indicate a good fit of the model to the data and values less than .05 indicate a very good fit (Browne and Cudek 1993).

4In addition, the normed fit indices are virtually at their maximum: CFI = .997, TLI = .995.
TABLE 2  Factor Structure of Threat Items

<table>
<thead>
<tr>
<th>Factor 1: Perceived Threat</th>
<th>Factor 2: Anxiety</th>
</tr>
</thead>
<tbody>
<tr>
<td>Coefficient</td>
<td>Factor Loading</td>
</tr>
<tr>
<td>How concerned are you that there will be another terrorist attack on U.S. soil in the near future?</td>
<td>1.00</td>
</tr>
<tr>
<td>How concerned are you that terrorists will attack the United States with biological or chemical weapons?</td>
<td>.96 (.03)</td>
</tr>
<tr>
<td>How concerned are you personally about you yourself, a friend, or a relative being the victim of a future terrorist attack in the United States?</td>
<td>.56 (.03)</td>
</tr>
<tr>
<td>How much, if any, have the terrorist attacks shaken your own sense of personal safety and security?</td>
<td>.37 (.03)</td>
</tr>
<tr>
<td>As you think about the terrorist attacks and the U.S. response, how often have you felt . . .</td>
<td>1.00</td>
</tr>
<tr>
<td>Anxious?</td>
<td>1.30 (.03)</td>
</tr>
<tr>
<td>Scared?</td>
<td>1.20 (.03)</td>
</tr>
<tr>
<td>Worried?</td>
<td>1.29 (.03)</td>
</tr>
<tr>
<td>Frightened?</td>
<td></td>
</tr>
</tbody>
</table>

Note: Coefficients are weighted least squares estimates for a two-factor latent variable model with categorical observed variables. Robust standard errors are in parentheses.

sense of personal safety and security is an even weaker indicator, although still statistically significant. Anxiety is most strongly defined by the four emotion items, with somewhat stronger factor loadings for scared and frightened than worried and anxious. A sense of shaken personal safety and security loads on the anxiety factor, but not as strongly. Finally, personal threat is a significant but relatively weak indicator of anxiety. Although two of the eight questions have significant coefficients on both factors, the two factors are quite distinct with an estimated correlation of .57. The correlation between the perceived threat and anxiety factors is not a function of their two shared indicators. When the model is reestimated with neither of the shared indicators included, the estimated correlation between the factors is .58.

To ensure the robustness of the factor model shown in Table 2, it was estimated separately for the period up to the end of December 2001 and from January 1 till mid-March and then again for respondents who had up to a high school degree and those with at least some college. The estimated parameters were virtually identical in these subsamples and the two-factor solution was a much better fit than a one-factor model in each case. The two latent factors of anxiety and perceived threat are used in all subsequent analyses.

Determinants of Perceived Threat and Anxiety

Confirmatory factor analyses verify that anxiety and perceived threat are distinct reactions to the threat of terrorism. We now turn to their determinants to assess whether anxiety and perceived threat can also be distinguished on the basis of their differing origins. The estimates shown in Table 3 are based on regression equations in which latent factors measuring perceived threat and anxiety are regressed onto the other factors in the table. The $R^2$ value is estimated in this and all subsequent analyses because
the dependent variables are categorical or latent factors. Latent factors for perceived threat and anxiety have no intrinsic scales and are standardized so that a unit on each represents a change of one standard deviation. The regression coefficients in Table 3 thus indicate the change in a standard deviation unit of each latent variable as the predictor increases by one unit (all analytic variables are coded 0 to 1 except for age, education, religious attendance, and weeks after 9/11.

Physical and emotional proximity to the attacks was expected to arouse anxiety but have less impact on perceived threat. There is some support for this prediction. Knowing someone who was killed or hurt in the attacks increased both anxiety and perceived threat, but had greater impact on anxiety as expected. Living near the terrorist attacks had significant effects on both anxiety and perceived threat (see also Skitka, Bauman, and Mullen 2004), although close physical proximity had greatest impact on anxiety. Perceptions of threat were higher in the northeast than in the rest of the nation with no additional impact of living in the New York area. Anxiety, on the other hand, was significantly higher among those living in the New York metropolitan area but not in the Northeast more generally. This latter finding is consistent with other studies that find heightened psychological reactions to 9/11 among those who lived closest to the attacks.

Several other factors differentially influenced the development of anxiety and threat, helping to confirm their distinctiveness. Younger people felt more anxious than older people, although there were no significant age-related differences in perceived threat. Republicans experienced somewhat less anxiety than Democrats, feeling reassured perhaps by the presence of George W. Bush as president. Women perceived somewhat higher levels of threat but felt much more anxious than men. In addition, several factors influenced threat but not anxiety. Blacks were somewhat more likely than whites to assess a higher risk of terrorism, although they did not experience higher levels of anxiety. And authoritarianism lead to higher levels of perceived threat but only slightly higher levels of anxiety, consistent with evidence that authoritarianism is linked to greater sensitivity to threat (Lavine et al. 2002).

Finally, there was a slight decline in perceived threat and anxiety over time, but the effect is nonlinear. Perceived threat and anxiety declined more rapidly after 9/11 but showed little further decline after the New Year, consistent with the findings of other national studies (see Davis and Silver 2003). Moreover, there is no change over time in support for government national security policy or the dynamics of policy support. Anxiety and threat have the same impact on national security policy throughout the study period (as indicated by nonsignificant interactions between anxiety and time and perceived threat and time in all policy analyses). As a consequence, we omit time as a variable from subsequent analyses.

### Anxiety and Depression

To further validate the distinction between perceived threat and anxiety, we consider its link to depression, which shares common negative affect with anxiety but should not be especially related to perceived threat (Clark and Watson 1991). The survey included three indicators of depression: feeling depressed, having trouble concentrating, and having trouble sleeping. Table 4 contains the results of an equation predicting depression—a latent variable with a unit fixed at one standard deviation.

Anxiety strongly predicts symptoms of depression, but depression is unrelated to perceived threat as expected. Holding all else constant, an increase of one standard

---

**Table 3: Determinants of Perceived Threat and Anxiety**

<table>
<thead>
<tr>
<th></th>
<th>Perceived Threat</th>
<th>Anxiety</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>β</td>
<td>s.e.</td>
</tr>
<tr>
<td>Age (10 years)</td>
<td>.029</td>
<td>.019</td>
</tr>
<tr>
<td>Education</td>
<td>-.039</td>
<td>.013</td>
</tr>
<tr>
<td>Gender (female)</td>
<td>.29</td>
<td>.06</td>
</tr>
<tr>
<td>Income &lt;$25,000</td>
<td>.27</td>
<td>.08</td>
</tr>
<tr>
<td>Race/Ethnicity</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>.36</td>
<td>.11</td>
</tr>
<tr>
<td>Hispanic</td>
<td>.23</td>
<td>.12</td>
</tr>
<tr>
<td>Other</td>
<td>.24</td>
<td>.15</td>
</tr>
<tr>
<td>Authoritarianism</td>
<td>.48</td>
<td>.10</td>
</tr>
<tr>
<td>Party ID (Republican)</td>
<td>-.10</td>
<td>.09</td>
</tr>
<tr>
<td>Ideology (Conservative)</td>
<td>.13</td>
<td>.10</td>
</tr>
<tr>
<td>Attend religious services</td>
<td>.002</td>
<td>.012</td>
</tr>
<tr>
<td>Knowledge missing</td>
<td>.16</td>
<td>.07</td>
</tr>
<tr>
<td>Northeast</td>
<td>.27</td>
<td>.12</td>
</tr>
<tr>
<td>NY Metro area</td>
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<td>.17</td>
</tr>
<tr>
<td>Weeks after 9/11</td>
<td>-.093</td>
<td>.032</td>
</tr>
<tr>
<td>Weeks2</td>
<td>.003</td>
<td>.001</td>
</tr>
<tr>
<td>Estimated R2</td>
<td>.13</td>
<td></td>
</tr>
</tbody>
</table>

*Note:* Coefficients are weighted least squares estimates for the latent dependent variables defined by the factors shown in Table 2. The latent dependent variables have units of one standard deviation. Coefficients in bold have z-scores greater than 2. The equations also contained a dummy variable for those who would not report their income. The coefficient for this variable was small in both equations.

---

5 Authoritarianism was measured as a preference for obedience, respect, and good manners among children over more expressive and creative values (Feldman and Stenner 1997).
deviation in anxiety leads to a .69 standard deviation increase in depression symptoms. Levels of depression are higher among women, the less affluent, and nonwhites, especially Hispanics, and lower among the better educated (see also Schlenger et al. 2002). Proximity to the attacks—living in the New York metro area and knowing someone hurt or killed in the attacks—also leads to higher reported levels of depression. Authoritarians report higher levels of depression than nonauthoritarians, and Republicans are significantly less likely to report symptoms of depression than are Democrats.

### Anxiety and Knowledge

Anxiety typically worsens cognitive functioning and may impair learning about the attacks and subsequent events because it clogs working memory with anxious thoughts that limit the comprehension and retention of new information (Eysenck 1992). This hypothesis is tested with responses to four items concerning knowledge of Afghanistan, Islam, and Osama Bin Laden, which were combined to yield a 5-point knowledge measure.

The knowledge equation is estimated as an ordered probit with a five-level categorical dependent variable (see Table 4). Since probit coefficients have no straightforward interpretation, we also present the expected change in the probability of correctly answering at least three of the four knowledge questions as each predictor varies across its range. Estimated thresholds were calculated for all probit analyses reported and can be obtained from the authors. As predicted, anxiety is linked to less accurate knowledge, but perceived threat is not. This cannot be explained by lower levels of news attention among anxious

---

**Table 4** Determinants of Depression and Knowledge

<table>
<thead>
<tr>
<th></th>
<th>Depression</th>
<th>Knowledge</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient</td>
<td>Coefficient</td>
</tr>
<tr>
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<td>−.05 (.05)</td>
</tr>
<tr>
<td>Anxiety</td>
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<td>−.18 (.06)</td>
</tr>
<tr>
<td>Age (10 years)</td>
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<td>.084 (.018)</td>
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<tr>
<td>Education</td>
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<td>−.60 (.06)</td>
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<td></td>
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<td>Other</td>
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<td>Authoritarianism</td>
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</tr>
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<td>Ideology (Conservative)</td>
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</tr>
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<td>Religious services</td>
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<td>.002 (.012)</td>
</tr>
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<td>Know missing</td>
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<td></td>
</tr>
<tr>
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<td></td>
</tr>
<tr>
<td>City 400,000+</td>
<td>.06 (.08)</td>
<td></td>
</tr>
<tr>
<td>Northeast</td>
<td>.28 (.09)</td>
<td></td>
</tr>
<tr>
<td>Weeks after 9/11</td>
<td>−.036 (.006)</td>
<td></td>
</tr>
<tr>
<td>Estimated R²</td>
<td>.69</td>
<td>.30</td>
</tr>
</tbody>
</table>

*Note: Coefficients for depression symptoms are weighted least-squares estimates for the latent variable described in the text. The latent variable has a unit of one standard deviation. Coefficients for knowledge are weighted least-squares probit estimates. Robust standard errors are in parentheses. Coefficients in bold have z-scores greater than 2. Changes in probability for knowledge are differences in the probability of correctly answering at least three out of four questions as each predictor variable ranges from low to high as described in footnote 6.*

---

6 The specific range for each variable is: age, 20 years to 80 years old; education, 11 years to 20 (postgraduate degree) years; gender, male to female; race/ethnicity, white to black/Hispanic/other; authoritarianism, lowest to highest score; party identification, strong Democrat to strong Republican; ideological identification, very liberal to very conservative; attendance at religious services, zero to eight times per month. Perceived threat and anxiety varied from the 5th to 95th percentile. Changes in probability were computed holding all other variables constant at their mean for an independent and ideologically moderate white male.
individuals, since anxious individuals actually watched somewhat more TV news than those who were not anxious (r = .10, p < .05). This finding is consistent with evidence from Marcus, Neuman, and Mackuen (2000) that anxiety increases information-seeking behavior. The remaining predictors of knowledge are consistent with the usual determinants of political knowledge: increasing education, age, gender (male), and lower levels of authoritarianism (Delli Carpini and Keeter 1996).

Taken together, evidence on the measurement, determinants and correlates of anxiety and perceived threat demonstrate that they constitute distinct reactions to the external threat of terrorism. We turn next to consider their political effects.

Support for Military Intervention and Presidential Approval

Perceived threat and anxiety were expected to have opposing effects on support for military initiatives and overseas engagement in line with evidence that threat increases the desire for retaliation whereas anxiety leads to heightened estimates of risk, especially self-relevant risks such as retaliatory attacks against Americans on U.S. soil. We also expected perceived threat to enhance, and anxiety to diminish, support for President Bush’s handling of the terrorist crisis, because of his ready endorsement of a strong military response to the 9/11 attacks. Consistent with other national polls taken at this time, 90% of respondents approved of the way Bush was handling his job, 72% supported increased military action even if it meant significant U.S. casualties, 84% believed it would be best if the United States took an active part in world affairs, and 63% felt the United States should take the leading role in solving international problems (Gaines 2002; Huddy, Khatib, and Capelos 2002).

Table 5 displays probit estimates that confirm the differing policy implications of perceived threat and anxiety. Higher levels of perceived threat are associated with greater support for U.S. military intervention, U.S. overseas involvement, and approval of Bush, in line with evidence that threat promotes retaliation. In contrast, anxiety has the opposite effect, decreasing approval of President Bush’s handling of the situation and increasing opposition to military action and overseas involvement,

<table>
<thead>
<tr>
<th>Table 5</th>
<th>Determinants of Bush Approval, Military Intervention, and World Involvement</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Bush Approval</td>
</tr>
<tr>
<td></td>
<td>Coefficient</td>
</tr>
<tr>
<td>Perceived Threat</td>
<td>.30 (.09)</td>
</tr>
<tr>
<td>Anxiety</td>
<td>-.27 (.11)</td>
</tr>
<tr>
<td>Age (10 years)</td>
<td>.044 (.035)</td>
</tr>
<tr>
<td>Education</td>
<td>.027 (.025)</td>
</tr>
<tr>
<td>Gender (female)</td>
<td>-.07 (.11)</td>
</tr>
<tr>
<td>Race/Ethnicity</td>
<td></td>
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<td>Black</td>
<td>-.59 (.16)</td>
</tr>
<tr>
<td>Hispanic</td>
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<td>Other</td>
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</tr>
<tr>
<td>Authoritarianism</td>
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</tr>
<tr>
<td>Party ID (Republican)</td>
<td>.96 (.21)</td>
</tr>
<tr>
<td>Party ID (Conservative)</td>
<td>.34 (.19)</td>
</tr>
</tbody>
</table>

Note: Coefficients are weighted least-squares probit estimates. Robust standard errors are in parentheses. Coefficients in bold have z-scores greater than 2. Changes in probability are differences in the probability of approving of Bush’s performance or supporting interventionist policies as each predictor variable ranges from low to high as described in footnote 6.
consistent with psychological evidence that it promotes risk aversion.\(^7\)

The effects of perceived threat and anxiety are substantial, as can be seen from the estimated changes in probability in Table 5, and are especially striking given the relatively high overall levels of support for Bush and overseas military action. For the three policy variables, the estimated effect of perceived threat (as reflected by the change in probability) is larger than any other predictor; the effects of anxiety are also comparatively large. It is important to note that marginal changes in probability are always small when predicted probabilities approach 1 (or 0) due to the functional form of the probit model. This helps to explain the limited effects of partisanship, perceived threat, and anxiety on Bush approval. For white males who are at the mean on all other independent variables apart from partisanship, the predicted difference in approval in favor of strong Republicans and strong Democrats is only .12; among such respondents 86% of Democrats approve of Bush compared to 98% of Republicans. The predicted change in Bush approval is only slightly smaller for threat and anxiety: there is an increased probability of .10 as perceived threat goes from low to high, and a decrease in probability of .08 for a comparable change in anxiety (for a white, male independent when all other variables are held at their mean value).

At somewhat lower levels of Bush approval the marginal effects of perceived threat are considerably larger. For example, a white, strong Democrat male who perceives no threat is .19 less supportive of Bush than a similar individual who perceives high threat, when all other variables are held at their mean. For the same type of individual, Bush support drops by \( -.14 \) as anxiety moves from its lowest to highest value. The corresponding changes for a black male who is a strong Democrat are .30 (threat) and \( -.22 \) (anxiety). For this black male Democrat (with mean values on all other variables) the predicted support for Bush is .79 when anxiety is low and .57 when it is high.

In addition to the effects of threat and anxiety, there are several other factors that influence Bush approval and increase support for overseas military involvement. Republicans were significantly more likely than Democrats to support military action; conservatives were stronger supporters than liberals of military action and were more supportive of the United States taking a leading role in world affairs. Black respondents were consistently more opposed to U.S. military and overseas involvement than were whites.

**Threat and Reactions to Arabs**

Americans who perceived a high future threat of terrorism not only supported aggressive action against the enemy, they were also more likely to negatively stereotype Arabs and support restrictive immigration and intensified surveillance policies directed at Arabs and Arab-Americans, in line with the expected effect of threat on out-group vilification. There was overwhelming support (85%) for toughening restrictions on visas for foreign students and other visitors to the United States. Just under half of all respondents (48%) believed that Arabs who apply for entry to the U.S. should undergo more intensive security checks than people from other countries. And 29% felt the government should put Arabs and Arab-Americans in the United States under special surveillance. In addition to these three policy questions, respondents were asked how well the following characteristics describe most Arabs: trustworthy, violent, honest, and extremist. Table 6 contains the results of probit estimates examining the origins of support for the three policy variables and regression estimates of the determinants of stereotyping. The dependent variable in the stereotyping equation is a latent variable inferred from the four indicators with units equal to one standard deviation.

The effects of perceived threat are consistent across the four equations. Perceived threat heightened support for policies that would restrict the number of foreign visitors to the United States and single out Arabs for special attention after entry and when applying for visas. Moving from low to high levels of perceived threat produces the largest increase in the probability of support for all three policies of any of the independent variables in Table 6, with only one exception (the positive effects of age on support for visa restrictions). Threat also intensifies negative stereotypes of Arabs.

In contrast, anxiety has no substantial impact on policies directed at Arabs or the endorsement of Arab stereotypes. Two coefficients for anxiety approach conventional levels of statistical significance—greater security checks and stereotyping—but in neither case does anxiety have a sizeable impact on policy views. Anxiety was not expected to decrease support for policies such as increased Arab surveillance because such policies do not pose a personal risk to the majority of Americans. We had suggested\(^7\)

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\(^7\)The political effects of anxiety which are based on feelings toward the “terrorist attacks and the U.S. response” are not simply caused by individuals who report negative feelings because they opposed U.S. policy. When the same respondents were asked to report their feelings almost a year later (in October 2002) in reference to “anti-U.S. terrorists” but not the U.S. response, there was a strong link between anxiety measured at both time points (\( r = .54 \)). Moreover, anxiety about terrorists, assessed in October 2002, was significantly linked to opposition to the Iraq war assessed at the same time in regression analyses with appropriate controls.
that anxiety might increase support for greater restrictions on those most closely linked to the 9/11 attacks, but we find no evidence for that prospect. Overall, these findings suggest that anxiety leads to an avoidance of risky action but does not promote support for retaliation or proactive policies to reduce risk.

Several other consistent effects in Table 6 deserve mention. Across all three policy variables, younger people are less supportive of restrictions on foreign visitors’ access to the United States and on Arab access in particular; they are also less willing than older people to support special surveillance of Arabs and Arab-Americans in the United States. Authoritarians and conservatives are consistently supportive of restrictive immigration policies and hold more negative stereotypes of Arabs than nonauthoritarians and liberals, respectively.

### Threat and Civil Liberties

Perhaps the most controversial policy discussions immediately after 9/11 centered on the government’s desire to increase domestic surveillance and limit certain freedoms in order to deal with the possibility of domestic terrorism. Survey respondents were asked their views on two policies that were widely discussed after 9/11. A slight majority (56%) supported a government-mandated national identification card. Substantially fewer people (31%), however, were in favor of allowing the government to monitor the personal telephone calls and emails of ordinary Americans. Both trends are consistent with findings from national polls conducted after 9/11 (Huddy, Khatib, and Capelos 2002). We also asked respondents whether they were more concerned that the government would fail to enact strong antiterrorism laws or that new laws would excessively restrict civil liberties. The public was split on this trade-off with 45% concerned that new antiterrorism laws than such laws would place undue restrictions on civil liberties. The impact of threat and anxiety on support for these policies is presented in Table 7.

Perceived threat consistently increased support for domestic antiterrorism policies. Support for a national identification card and government monitoring of telephones and email rose significantly as the perceived threat increased. Similarly, threat was linked to a greater concern about the failure to enact strong antiterrorism measures than such laws would place undue restrictions on civil liberties. In all three equations, an increase in the threat of future terrorism produced the largest shift in the probability that someone would support civil liberties restrictions of any of the independent variables in Table 7.

### Table 6  Determinants of Anti-Arab Policy Preferences and Attitudes

<table>
<thead>
<tr>
<th></th>
<th>Surveillance of Arab/Americans</th>
<th>More Security Checks for Arab Visitors</th>
<th>Greater Restrictions on Visas</th>
<th>Stereotyping</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient</td>
<td>Change in Probability</td>
<td>Coefficient</td>
<td>Change in Probability</td>
</tr>
<tr>
<td>Perceived Threat</td>
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<td>.23</td>
<td>.17 (.06)</td>
<td>.19</td>
</tr>
<tr>
<td>Anxiety</td>
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<td>.01</td>
<td>.12 (.07)</td>
<td>.11</td>
</tr>
<tr>
<td>Age (10 years)</td>
<td>.060 (.025)</td>
<td>.13</td>
<td>.053 (.023)</td>
<td>.13</td>
</tr>
<tr>
<td>Education</td>
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<td>-.07</td>
<td>-.028 (.015)</td>
<td>-.10</td>
</tr>
<tr>
<td>Gender (female)</td>
<td>-.12 (.08)</td>
<td>-.04</td>
<td>-.15 (.07)</td>
<td>-.06</td>
</tr>
<tr>
<td>Race/Ethnicity</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>-.26 (.16)</td>
<td>-.09</td>
<td>.08 (.14)</td>
<td>.03</td>
</tr>
<tr>
<td>Hispanic</td>
<td>-.29 (.17)</td>
<td>-.10</td>
<td>.16 (.15)</td>
<td>.06</td>
</tr>
<tr>
<td>Other</td>
<td>-.30 (.22)</td>
<td>-.10</td>
<td>.05 (.17)</td>
<td>.02</td>
</tr>
<tr>
<td>Authoritarianism</td>
<td>.41 (.11)</td>
<td>.15</td>
<td>.29 (.10)</td>
<td>.11</td>
</tr>
<tr>
<td>Party ID (Republican)</td>
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<td>.08</td>
<td>-.04 (.11)</td>
<td>-.01</td>
</tr>
<tr>
<td>Ideology (Conservative)</td>
<td>.40 (.13)</td>
<td>.14</td>
<td>.38 (.12)</td>
<td>.15</td>
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<tr>
<td>Estimated R²</td>
<td>.14</td>
<td>.10</td>
<td>.21</td>
<td>.13</td>
</tr>
</tbody>
</table>

Note: Coefficients for stereotyping are weighted least-squares estimates for the latent variable described in the text. The latent variable has a unit of one standard deviation. Coefficients for the policy variables are weighted least-squares probit estimates. Robust standard errors are in parentheses. Coefficients in bold have z-scores greater than 2. Changes in probability for the policy variables are differences in the probability of supporting restrictions on Arabs and Arab-Americans as each predictor variable ranges from low to high as described in footnote 6.
## Table 7 Determinants of Policy Preferences on Civil Liberties

<table>
<thead>
<tr>
<th></th>
<th>National ID Card</th>
<th></th>
<th>Phones and Email</th>
<th></th>
<th>Security vs. Civil Liberties</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient</td>
<td>Change in Probability</td>
<td>Coefficient</td>
<td>Change in Probability</td>
<td>Coefficient</td>
<td>Change in Probability</td>
</tr>
<tr>
<td>Perceived Threat</td>
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<td>.24</td>
<td>.16 (.05)</td>
<td>.16</td>
<td>.18 (.06)</td>
<td>.20</td>
</tr>
<tr>
<td>Anxiety</td>
<td>.10 (.06)</td>
<td>.09</td>
<td>.10 (.06)</td>
<td>.07</td>
<td>.05 (.08)</td>
<td>.04</td>
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<td>.061 (.020)</td>
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<td>.07</td>
<td>.04 (.06)</td>
<td>.01</td>
<td>.11 (.08)</td>
<td>.04</td>
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<td>Race/Ethnicity</td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
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<td>-.01</td>
<td>-.01 (.11)</td>
<td>.00</td>
<td>-.15 (.15)</td>
<td>-.06</td>
</tr>
<tr>
<td>Hispanic</td>
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<td>.04</td>
<td>.27 (.13)</td>
<td>.10</td>
<td>-.23 (.16)</td>
<td>-.09</td>
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<td>Other</td>
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<td>.01</td>
<td>.11 (.15)</td>
<td>.04</td>
<td>.08 (.18)</td>
<td>.03</td>
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<td>Authoritarianism</td>
<td>.26 (.08)</td>
<td>.10</td>
<td>.30 (.08)</td>
<td>.10</td>
<td>.28 (.11)</td>
<td>.11</td>
</tr>
<tr>
<td>Party ID (Republican)</td>
<td>-.05 (.10)</td>
<td>-.02</td>
<td>.19 (.10)</td>
<td>.06</td>
<td>.45 (.13)</td>
<td>.17</td>
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<tr>
<td>Ideology (Conservative)</td>
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<td>.18</td>
<td>.43 (.10)</td>
<td>.14</td>
<td>.26 (.13)</td>
<td>.10</td>
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<tr>
<td>Estimated R²</td>
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<td>.11</td>
<td>.11</td>
<td>.11</td>
<td></td>
<td></td>
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</tbody>
</table>

*Note: Coefficients are weighted least-squares probit estimates. Robust standard errors are in parentheses. Coefficients in bold have z-scores greater than 2. Changes in probability are differences in the probability of supporting restrictions on civil liberties as each predictor variable ranges from low to high as described in footnote 6.*

Unlike military policies, anxiety was not expected to decrease support for policies that restricted civil liberties. There is a clear link between anxiety and opposition to military action. But civil liberties policies are unlikely to result in retaliatory terrorist action and should not arouse personal concerns among most Americans. We had, in fact, suggested that anxiety could increase support for such policies because they might reduce the risk of terrorism. But as we found for policies concerning immigrants, anxiety was not associated with increased support for domestic antiterrorism policies. The coefficients for anxiety are all positive in Table 7, but none are statistically significant at conventional levels, and none of the effects are substantively large. Perceived threat increases support for heightened surveillance policies but anxiety does not. This provides further confirmation that anxious individuals are risk averse but do not actively support precautionary policies, a point to which we return in the discussion.

### Discussion and Conclusions

The effects of terrorism depend heavily on how a targeted public responds, and, as demonstrated in this study, not everyone responds to the threat of external terrorism in the same way. Most Americans perceived a high level of future terrorist threat to the nation (Smith and Rasinski 2002), but only a minority expressed considerable anxiety in response to the 9/11 attacks. And these related but differing psychological reactions to the external threat of terrorism—perceived threat versus anxiety—had very different effects on public support for antiterrorism policies. As perceived threat increased, there was heightened support for a wide range of domestic and international government actions to combat the threat of terrorism, including overseas military action, a curtailment of civil liberties, and increased surveillance and tighter immigration restrictions for Arabs. In contrast, those who experienced high levels of anxiety were less supportive of aggressive military action against terrorists, generally favored increased American isolationism, and disapproved more of President Bush’s performance.

These findings raise important questions about the basis of public support for government antiterrorism measures. Our analysis suggests that a perception of high terrorist threat will likely promote public support for aggressive national security policy. The Bush administration seemed aware of this link and issued terrorist alerts into the early months of 2002, perhaps helping to explain why the perceived risk of terrorism remained relatively high throughout the study period. At the same time, this strategy holds clear risks for government officials who wish to take aggressive action against terrorists. To the
extent that terrorist warnings elevate Americans’ levels of anxiety, they could also undercut support for overseas military action. In order to garner public support for military action, the government must make people aware of the risk of terrorism without unduly scaring them.

Government officials involved in antiterrorism policy face an easier challenge in gaining support for domestic policies, however. Anxiety and threat do not act as countervailing forces on support for civil liberties policies as they do for aggressive international policies. As perceptions of threat increase, people become significantly more supportive of measures that restrict the rights of groups broadly associated with terrorism, and policies that limit civil liberties for all citizens more generally (for similar findings see Davis and Silver 2003, 2004). Over the long term, perceived threat provides support for belligerent overseas military action. In contrast, absence of any concern about future terrorist incidents, Americans who thought there was very little future risk of terrorism supported U.S. overseas military action and tighter restrictions on student visas, helping to explain high levels of support for many antiterrorist policies. This probably reflects an immediate response to the attacks of September 11 in the absence of any concern about future terrorist incidents. But Americans who do not perceive a significant threat of future terrorism may be less inclined to support continued military action and restrictions on civil liberties over the long term. We could not detect any such decline in policy support over the time span of this study (which ended in early March, 2002) but a reduction of perceived threat remains a potential source of opposition to sustained government action within the United States and overseas.

The findings from this study lend further insight into the future trajectory of support for antiterrorism measures in the United States when we consider the potential effects of anxiety. Security threats in this and other studies increase support for military action (Jentleson 1992; Jentleson and Britton 1998; Herrmann, Tetlock, and Visser 1999). But anxious respondents were less supportive of belligerent military action against terrorists, suggesting an important source of opposition to military intervention. In the aftermath of 9/11, several factors were consistently related to heightened levels of anxiety and related psychological reactions, including living close to the attack sites (Galea et al. 2002; Piotrkowski and Brannen 2002; Silver et al. 2002), and knowing someone who was hurt or killed in the attacks (in this study). It is difficult to say what might happen if the United States were attacked again in the near future. Based on our results, it is plausible that a future threat or actual attack directed at a different geographic region would broaden the number of individuals directly affected by terrorism and concomitantly raise levels of anxiety. This could, in turn, lower support for overseas military action. In contrast, in the absence of any additional attacks levels of anxiety are likely to decline slowly over time (we observed a slow decline in this study), weakening opposition to future overseas military action.

Since our conclusions are based on analysis of reactions to a single event in a country that has rarely felt the effects of foreign terrorism, we should consider whether they can be generalized to reactions to other terrorist incidents or to reactions under conditions of sustained terrorist action. Our answer is a tentative yes, although there is no conclusive evidence on this point as yet. Some of our findings corroborate evidence from Israel, a country that has prolonged experience with terrorism. For example, Israeli researchers find that perceived risk leads to increased vilification of a threatening group and support for belligerent action (Arian 1989; Bar-Tal and Labin 2001). There is also evidence that Israelis experienced fear during the Gulf War, especially in Tel Aviv where scud missiles were aimed (Arian and Gordon 1993). What is missing, however, is any evidence that anxiety tends to undercut support for belligerent antiterrorism measures under conditions of sustained threat. For the most part, Israeli research has not examined the distinct political effects of anxiety.

In conclusion, the findings from this study provide significant new evidence on the political effects of terrorism and psychological reactions to external threat more generally. Many terrorism researchers have speculated
that acts of terrorist violence can arouse fear and anxiety in a targeted population, which lead to alienation and social and political dislocation. We have clear evidence that the September 11 attacks did induce anxiety in a sizeable minority of Americans. And these emotions were strongly associated with symptoms of depression, appeared to inhibit learning about world events, and weakened support for overseas military action. This contrasted, however, with Americans’ dominant reaction, which was a heightened concern about future terrorist attacks in the United States that galvanized support for government antiterrorist policy. In this sense, the 9/11 terrorists failed to arouse sufficient levels of anxiety to counteract Americans’ basic desire to strike back in order to increase future national security, even if such action increased the short-term risk of terrorism at home. Possible future acts of terrorism, or a different enemy, however, could change the fine balance between a public attuned to future risks and one dominated by anxiety.

References


Abstract One of the most noted phenomena in social and political decision-making is the occurrence of a framing effect. For example, on problems involving risky choices, individuals tend to act risk-averse when the problem is framed in terms of gains (e.g., saving lives, making money) and risk-seeking when the same problem is instead framed in terms of losses (e.g., deaths, losing money). Scholars have begun to identify the processes underlying framing effects as well as the conditions under which framing effects occur. Yet, extant work focuses nearly exclusively on cognitive processes, despite growing recognition of the importance of emotion in general decision-making tasks. In this paper, we explore the impact of emotional states on risk attitudes and framing. We find that emotions significantly influence both individuals’ tendencies to take risks and the impact of a frame on risky choices (e.g., emotions amplify or depress a frame’s impact). The precise role of emotions depends on the problem domain (e.g., a life-death or a financial decision), and the specific type of emotion under study. Moreover, in contrast to much work in political science, we show that emotions need to be distinguished beyond their positive or negative valence, as different negative emotions exert opposite effects. Our results accentuate the importance of integrating emotions into research areas traditionally dominated by more cognitive perspectives.

Keywords Framing · Emotion · Decision-making · Risk · Experiment · Public opinion
Citizens’ political preferences form the foundation of democratic governance. It is thus not surprising that scholars devote a considerable amount of attention to studying the processes by which individuals form their political attitudes. The portrait that emerges from these studies makes clear that, in forming their opinions, citizens do not satisfy the requisites of the “ideal rational decision-maker.” As Berelson (1954, pp. 309–311) and his colleagues explained over fifty years ago: “The democratic citizen is expected…to have arrived at his principles by reason and to have considered rationally the implications and alleged consequences of the alternative proposals…[Yet] it appears that a sense of fitness is a more striking feature of political preference than reason and calculation” (emphasis added).

Two notable ways in which citizens deviate from this bygone ideal is by invoking emotion in their decisions, as opposed to pure reason, and by exhibiting susceptibility to framing effects. Some types of framing effects occur when individuals’ preferences shift due to arbitrary variations in information rather than reflecting calculations across all conceivable dimensions of a decision. Political behavior research on each of these topics—emotion and framing—has proven quite progressive over the last decade (e.g., Marcus 2003; Druckman 2004; Chong and Druckman 2007). Yet, surprisingly, few political behavior scholars have explored the relationship between emotion and framing, with the latter being construed almost always in purely cognitive terms.

In what follows, we explore the relative impact of emotions as opposed to frames as well as the moderating effects of emotions on how people process frames. We focus on the impact of emotion on the framing of risky choice problems, which is a variant of valence or equivalency framing. Equivalency framing effects occur when “logically equivalent phrases cause individuals to alter their preferences (Tversky and Kahneman 1981, 1987)... typically involve[ing] casting the same information in either a positive or a negative light…” (Druckman 2004, p. 671). An example of an equivalency framing effect is when people reject a policy program when told that it will result in 5% unemployment but prefer it when told that it will result in 95% employment.¹ Numerous authors highlight the relevance of these types of framing effects for studies of voting and public opinion, campaigns, policy-making, foreign-policy decision-making, coalition bargaining, judicial decision-making, and a variety of other topics (e.g., Quattrone and Tversky 1988; McDermott 1998; Bartels 2003; Levy 2003; Druckman 2004).

In the next section, we discuss extant work on risky choice, particularly emphasizing the role of framing in shaping such choices. We then turn to a

¹Druckman (2004, p. 672) explains that these types of equivalency framing effects “differ from value or issue framing effects, as studied in the political communication literature… Issue framing effects refer to situations where, by emphasizing a subset of potentially relevant considerations, a speaker leads individuals to focus on these considerations when constructing their opinions. For example, describing a hate group in terms of free speech as opposed to public safety causes people to base their rally opinions on free speech instead of public safety considerations.” For studies on issue framing and emotion—with particular focus on how different frames lead to alternative emotional reactions—see Brewer (2001), Gross and D’Ambrosio (2004), Marcus et al. (2005), Gross (2006), Gross and Brewer (2006). Also see Lerner and Keltner (2001), who explore emotions and equivalency framing effects. Some of their results are similar to ours although we look across domains, at preference confidence, and at different specific emotions.
discussion of how emotion not only affects risk preferences but also how it might interact with frames in shaping risky decisions. We also briefly discuss how emotions might influence the confidence individuals maintain in their expressed preferences. From these discussions, we derive predictions about the competing and interactive effects of frames and emotions, and about the impact of emotions on preference confidence. We test our expectations with two experiments. After presenting the results, we conclude with a brief discussion of implications. Our findings reveal the importance of incorporating emotions into studies of cognitive framing (both in terms of competing and interactive effects), how emotions need to be distinguished beyond their positive and negative valences, how the specific role of emotions and frames vary across problem domains, and how emotions influence the confidence one has in his or her preferences.

Risky Choice and Framing

Politicians regularly face risky choices in decisions that range from whether to go to war to deciding which policies will maximize their odds of being reelected. Citizens make risky decisions every time they vote for one candidate instead of another. The framing of risky choices plays a major role in any such decision-making pursuit. As is well known from the work on prospect theory conducted by Kahneman and Tversky \((1979, 1984)\), frames strongly influence risk propensity, such that people appear quite risk-averse when approaching gains, and remain much more risk-seeking when confronting losses.

One of the most persistent criticisms of prospect theory is that it lacks a theory of framing. Payne, Bettman, and Johnson \((1993)\) succinctly captured this sentiment when they stated, “clearly, the development of a theory of framing is badly needed” \(\text{(e.g., Riker 1995; Wittman 1995; Druckman 2001a,b)}\). In the fifteen years since Payne et al.’s statement, there has been progress. Scholars have begun to identify mediational processes and moderating variables. For example, Jou et al. \((1996)\) present a theory that posits cognitive accessibility processing as underlying framing effects. Others show that framing effects are less likely to occur when the respondent is a male \(\text{(Fagley and Miller 1997)}\), has high cognitive ability \(\text{(Stanovich and West 1998)}\), or briefly thinks through his or her decision \(\text{(Takemura 1994)}\). Moreover, framing effects tend to disappear when a decision-maker provides a compelling rationale for his or her decision \(\text{(Sieck and Yates 1997)}\).\(^2\) Additionally, Kanner \((2004)\) shows that framing can be manipulated by secondary actors by either changing the actor’s confidence about future outcomes, or discounting the utility of a given choice.

Notably absent from this list, however, is significant study into emotional factors that might compete and/or interact with framing effects. De Martino et al. \((2006, p. 684)\) explain that framing studies “emphasize the operation of analytic processes in guiding choice behavior. However, more intuitive or emotional responses can play a key role…” The authors go on to show, using fMRI technology, that framing effects

\(^2\) For contextual limitations, see Bless et al. \((1998)\), Druckman \((2004)\).
are associated with amygdala activity in the brain, “suggesting a key role for an emotional system” (also see Lerner and Keltner 2001; Chang 2007). Similarly, Loewenstein et al. (2001, p. 274) explain, “feelings play a much more prominent role in risk decision-making than they are given credit for by the cognitive-consequentialist tradition of judgment and decision-making research.” They continue by arguing that (p. 280), “the decision-theoretic approach to decision-making under risk has largely ignored the role played by emotions…very little attention has been given to the impact of emotions…” (also see Shiv et al. 2005, p. 438).

As mentioned, we explore two ways in which emotions influence the framing of risky choices. First, emotional response may provide a competing influence (with the frame) on cognitive interpretations of choice. Second, emotional considerations might either exacerbate or ameliorate the influence of a particular frame itself. In other words, emotion may provide an important key in explaining variance in framing effects, including susceptibility to them. Our goal is not to negate earlier cognitively focused work, but rather to supplement it by integrating emotion into an understanding of framing effects. We next describe how emotional states can directly affect individuals’ risk assessments, and then how emotions might exacerbate or depress the impact of a frame.

**Emotion and Risk**

A growing literature suggests that emotions can affect risk assessments in systematic and predictable ways. This work goes back, at least, to Bower (1981) who posited a relationship between mood and memory, suggesting that mood congruence enhanced memory (e.g., individuals in a bad mood proved more likely to remember negative events while those in a good mood more readily recalled positive experiences). Johnson and Tversky (1983) extend this logic to argue that specific emotions could affect risk assessment. They show that positive emotions trigger optimistic risk assessments, while negative emotions lead to more pessimistic risk assessments. This proved true even when the specific emotion resulted from factors wholly unrelated to the issue whose risk individuals assessed. For example, sad people tend to inflate their risk of negative events like getting cancer more than happy people (also see Hsee and Weber 1997). In short, this work suggests that positive emotions, such as enthusiasm, tend to lead to greater risk-seeking because people become more optimistic about future outcomes when they are feeling good. Similarly, negative emotions, such as anxiety, tend to make individuals more pessimistic about future outcomes, and this can lead to risk aversion.

More recent work in psychology accentuates the importance of moving beyond general positive and negative mood states into the study of discrete emotions. DeSteno et al. (2000), for example, demonstrate that mood states increase the perceived likelihood of emotion specific (i.e., beyond just positive or negative) congruent future events. For instance, an angry individual will perceive future events that generate anger (a negative emotion) as likely but sad events (also a negative emotion) as less likely to occur. DeSteno et al. argue that this effect results
from the informational value provided by particular emotional states (also see Bodenhausen et al. 1994; Mitchell et al. 2001). This interpretation is consistent with Clore’s (see Schwarz and Clore 2003, for a review) affect-as-information model of mood.

Lerner et al. (2003, p. 144) emphasize the “importance of examining specific emotions rather than global (positive–negative) feelings.” They reference Smith and Ellsworth’s (1985) appraisal-tendency theory to argue that emotions become an implicit perceptual lens for interpreting situations. From this perspective, negative emotions, such as fear or distress, that generate anxiety stem from appraisals of uncertainty and the need for situational control, while other negative emotions, such as anger or hostility, that generate aversion emerge from certainty married to the same need for control. Because of this, hostility or anger produces optimism about future outcomes (due to certainty) and risk-seeking choices, possibly because of the desire for revenge in the face of a clear target. In contrast, distress or fear leads to pessimistic judgments (due to uncertainty) of the future and greater risk aversion as individuals strive to figure out who or what is hurting them, and how they can best stop the threat (Lerner and Keltner 2000; Lerner and Keltner 2001; Lerner et al. 2004). The central point to take from this work lies in the critical importance of differentiating specific negative emotions that vary in their concomitant certainty and consequential affect on risk (e.g., distress or anxiety versus anger or aversion).

While much prior work in political science ignores such distinctions between types of negative emotions (e.g., Abelson et al. 1982; Marcus 1988; Marcus et al. 2000; Brader 2006; Brader et al. 2007), some recent work recognizes the varying impact of negative emotions. Notably, MacKuen et al. (2005, p. 3) emphasize the need to distinguish the negative emotion of anxiety from another negative “emotional response not previously considered... the emotion of aversion,” which

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3 In later work, DeSteno et al. (2004) apply their model to persuasive communication, arguing that specific emotions can alter the effectiveness of certain appeals based on their emotional framing. They note that the “influence of affective states on judgments of likelihood may not occur as a function of simple valence.” In other words, the extent to which an emotional state will influence a particular judgment depends on the event and the context, as well as the specific emotion elicited. Based on their earlier findings, they suggest that persuasive attempts will be more successful when they are framed to match the emotional state of the observer. Mayer et al. (1992) also investigated mood congruent judgments in a series of experiments designed to show the generalizability of the phenomenon. They showed that individuals judge attributes to be more characteristic, and judge outcomes to be more likely, when mood is congruent. In addition, they note that negative affect deflates the perceived risk of a positive event (also see, e.g., Berkowitz and Harmon-Jones 2004).

4 This view mirrors Slovic’s (1999) argument that emotions serve an orienting function. In this view (Slovic et al. 2004, 2005), risk is understood and acted upon in two fundamental, and profoundly different, ways. The first approach considers risk as feelings; the second understands risk as analysis. The first represents a rapid, intuitive, often unconscious experiential response or reaction to perceived danger or threat; this interpretation constitutes the “affect heuristic” (Finucane et al. 2000). Because this system evolved to aid humans in survival, it remains the most common and automatic way for individuals to assimilate risk. The second approach considers risk as an analytic problem requiring normative solutions involving rationality and logic. This process is relatively slower, and requires much greater conscious effort. While many advocates of this approach consider emotional responses to risk as irrational, more recent work in neuroscience demonstrates that these systems work largely in parallel and take direction from each other (Damasio 1994).
includes feelings of anger and disgust (e.g., Marcus et al. 2000, p. 168; Marcus 2003). For some tasks, different positive emotions can have distinct effects as well.\(^5\) However, we are unaware of any expectations (e.g., from the work on which we build) of variation in distinct positive emotions when it comes to risk attitudes or framing effects. As far as we know, positive emotions, such as enthusiasm, should generally lead to risk-seeking as suggested above.

**Emotions as a Framing Moderator**

Aside from directly influencing risk assessments, emotional states can condition the impact of a given frame. Marcus et al.’s (2000) influential model of affective intelligence posits that people tend to become more involved and interested when they feel positive emotions such as enthusiasm, but actually become more attentive to external stimuli, information-seeking, and open to attitude change when they are experiencing negative emotions that generate anxiety (e.g., distress) (also see Brader 2006). Marcus et al. (2005) assert that people with a high level of anxiety or distress should prove more susceptible to preference change because they are more attuned to external information (e.g., frames). The logic behind this is that anxiety signals a “sense of danger and novelty… [and] alerts people to stop, think, and adjust behavior” (Marcus et al. 2005, p. 950). More concretely, anxiety triggers the surveillance system of evaluation which “monitors the environment for novel and threatening stimuli…[and] interrupts habitual routine and engages thought” (Marcus et al. 2000, p. 53). Increased attention to the environment means increased power of information and how that information is framed (as opposed to reliance on long-standing risk-attitudes).

In contrast, negative emotions of aversion, such as anger, will not have this effect. “[A]version is an actively negative emotion distinct from both anxiety and the absence of enthusiasm (Learner and Keltner 2001)… when people experience an aversive response… they will avoid exposure to learning [i.e., information]” (MacKuen et al. 2005, pp. 7–8). Aversive emotions like anger, then, trigger the disposition system rather than the surveillance system; the disposition system leads people to be less attuned to the external environment.\(^6\) (For a detailed discussion of the causes and consequences of anger, see Berkowitz and Harmon-Jones 2004a, b). Similarly, increases in positive emotions such as enthusiasm tend to temper the impact of external information such as alternative frames because it also activates the disposition system.\(^7\) Notably, then, as with our expectations regarding direct effects on risk-attitudes, we expect different negative emotions to have distinct effects in moderating the frame’s impact with aversive emotions like anger having

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\(^5\) Lerner and Keltner (2000, pp. 477–480) offer an example of expected differences among the positive emotions of pride and surprise (but not with regard to risk attitudes) (also see Lerner and Small 2002, p. 300).

\(^6\) We thank an anonymous reviewer for guidance on this point.

\(^7\) This is consistent with Kowert and Hermann (1997) who find that experimental participants who are low on measures of the negative emotion anxiety appear less susceptible to framing effects.
similar effects to positive emotions like enthusiasm, but different effects than negative anxiety emotions such as distress.

**Preference Confidence**

A final topic which we explore is preference confidence. While citizens’ preferences themselves play an important role in democratic governance, preferences also matter because they often determine subsequent behaviors (e.g., policy decisions, voting, and participation). The likelihood that an individual’s behaviors will reflect his or her preferences dramatically increases as the individual gains confidence in the preference. Increased confidence in a particular preference also leads individuals to deepen their commitment to the preference, to ignore and not pursue additional information, and to resist persuasion (e.g., Sieck and Yates 1997, p. 218; Visser et al. 2003, pp. 135–136; Druckman 2004).

As far as we know, the role of emotions in affecting preference confidence has gone unexplored. It seems plausible and consistent with our theoretical discussion that the certainty that comes with aversion or anger (and possibly enthusiasm) will increase confidence while the uncertainty common with anxiety will depress confidence. This prediction also would be consistent with work showing that anger relies on heuristic processing, in ways similar to positive emotions (Moons and Mackie 2007).8

**Predictions**

It is relatively straightforward to derive predictions from these research programs on emotion and risk, and emotion and information seeking. As should be clear, both literatures point to distinct effects for positive emotions, negative aversive emotions, and negative anxious emotions. In terms of operationalizing each specific type of emotion, we follow prior work. For the positive emotion, we focus on enthusiasm which has been central to work by Marcus and his colleagues on the affective intelligence model (e.g., Marcus et al. 2000, 2005). We also follow Marcus et al. (2005, p. 962) by focusing on distress to capture anxiety. Our negative aversion emotion is anger (see, e.g., Watson et al. 1988; Marcus et al. 2000, p. 168; Lerner and Keltner 2001; Lerner et al. 2003).9 Our specific predictions are as follows.

1. *Enthusiasm will be positively correlated with risk-seeking behavior, regardless of frame.* We expect this finding because positive emotions like enthusiasm lead to optimistic appraisals of risk, regardless of the consequentialist data. So when an individual is presented with a positive frame, he or she will expect a positive outcome to be particularly likely given his or her current mood congruent state.

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8 We thank an anonymous reviewer for this insight.

9 Lerner and Keltner (2000, 2001) focus on fear rather than distress, but as they make clear, the same logic applies to analogous emotions like distress (i.e., in terms of their implications for certainty and control) (also see Schwarz 2000; Nabi 2003).
However, when presented with a negative frame, the individual will see the negative outcome as particularly unlikely given the incongruence between the frame and his or her present level of enthusiasm.

2. **Distress and anger will produce distinct impacts on risk-seeking behavior (due to the aforementioned difference in uncertainty).** First, distress will be negatively correlated with risk-seeking behavior. Second, anger will be positively correlated with risk-seeking. This would also be consistent with adopting a negative frame (also see Lerner and Keltner’s 2000, p. 146).

3. **Exposure to a negative (dying or losing) frame will be positively correlated with risk-seeking behavior.** This is the conventional framing prediction that negative frames lead to risk-seeking choices while positive frames produce a tendency toward risk-aversion (Tversky and Kahneman 1981, 1987).

4. **Distress and anger will produce distinct impacts on the frames. First, distress will enhance the impact of frames.** We expect this effect to be particularly pronounced in the negative frame due to congruence enhancement. This hypothesis derives from the expectation that, as explained, distress generally increases attention to external information. Second, **anger will temper the impact of the frame, due to its operation through the disposition system.**

5. **Enthusiasm will temper the impact of frame.** We expect this effect to be particularly pronounced in the positive frame because of congruence effects.

6. **Anger and enthusiasm will increase preference confidence while distress will depress confidence.**

To test our expectations, we implemented two experiments. We now describe each.

**Experiment 1**

**Participants, Design, and Procedure**

In the first experiment, a total of 214 individuals participated in the main part of the study in exchange for a cash payment and a snack. We recruited participants from a large, public university and the surrounding urban community by taking out newspaper advertisements, advertising in classes, sending e-mails, posting flyers, and contacting local community organizations. We invited participants to take part in a preference formation study at the university’s Political Psychology Laboratory. While students constituted a majority of the sample, a substantial numbers of non-students also participated (approximately 35%).

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10 This prediction finds a counterpart in the decisional paralysis so characteristic of depression. Anxiety, distress, and depression often intertwine. And, indeed, there may be some good evolutionary reasons why paralysis in feelings of distress promotes survival. Freezing in fear or distress, for example, often prevents predators from attacking.

11 The average age was about 26. Other sample statistics include: 57% females, 88% Caucasian, 52% self-identified Democrats, and 27% self-identified Republicans.
Each participant responded to two randomly ordered framing problems and a background questionnaire. We used two classic and widely cited risky-choice framing problems: (1) a problem that focuses on the outbreak of a disease (Tversky and Kahneman 1981, p. 453), and (2) a problem that focuses on how to invest a community grant (Tversky and Kahneman 1987, pp. 74–75). For the disease problem, we randomly exposed some respondents to the following positively framed description and question:

Imagine that the U.S. is preparing for the outbreak of an unusual Asian disease, which is expected to kill 600 people. Two alternative programs to combat the disease have been proposed. Assume that the exact scientific estimates of the consequences of the programs are as follows:

If Program A is adopted, 200 people will be saved.
If Program B is adopted, there is a 1/3 probability that 600 people will be saved, and a 2/3 probability that no people will be saved.
Which of the two programs would you favor?

Two elements of this problem are worth noting. First, the two choices—Program A and Program B—are equivalent in expected values. Both would result in saving 200 people in expectation (i.e., 1/3*600 = 200), but Program A has a certain or risk-averse outcome whereas Program B has an uncertain or risk-seeking outcome. Second, the programs are framed in terms of how many lives will be saved. This differs from the version that other recipients (randomly) receive where, after receiving the same exact initial description, they are told of the same programs but framed negatively in terms of people dying:

If Program A is adopted, 400 people will die.
If Program B is adopted, there is a 1/3 probability that nobody will die, and a 2/3 probability that 600 people will die.
Which of the two programs would you favor?

The investment problem had two analogous (randomly distributed) versions. The positive frame asked:

Imagine that the community where you live was given a $3,000 government grant for future community development. The community must however immediately invest the grant in one of two programs, and everyone agrees that the estimated impact of each program is as follows:

If Program Y is adopted, your community will gain $1,000.
If Program Z is adopted, there is a 50% chance that your community will gain $2,000 and a 50% chance that your community will gain nothing.
Which program would you vote for—Program Y or Program Z?

Respondents actually responded to four randomly ordered problems, however, two of these problems are not relevant to this study. There also were randomly assigned experimental conditions that are not relevant to this study, and thus, we do not discuss them (see Druckman 2004 for discussion). Like other studies that include multiple problems (e.g., Fagley and Miller 1997; Jou et al. 1996), we instructed participants to treat each problem independently and imagine that they were being confronted with each scenario (see Frisch 1993, p. 419; Levin et al. 2002 on using multiple problems).

We use a variation of the Tversky and Kahneman’s (1987, pp. 74–75) investment problem.
Again, notice Program Y is a risk-averse choice while Program Z constitutes a risk-seeking option (that is equivalent in expected outcome). The negative frame stated:

Imagine that the community where you live was given a $3,000 government grant for future community development. The community must however immediately invest the grant in one of two programs, and everyone agrees that the estimated impact of each program is as follows:

If Program Y is adopted, your community will lose $1,000.
If Program Z is adopted, there is a 50% chance that your community will lose nothing and a 50% chance that your community will lose $2,000.

Which program would you vote for—Program Y or Program Z?

We used two problems from different domains (i.e., life and money) because prior work has shown that framing effects sometimes vary by topic. People tend to be more risk-seeking with problems involving life-death choices than with problems involving public property, personal money, or investment (Wang 1996 p. 153; Jou et al. 1996, pp. 8–9; Kühberger et al. 1999, pp. 221–222).\(^{14}\)

As should be clear, for each problem, participants expressed a preference for one of two alternatives (i.e., Program A or B, or Program Y or Z). A framing effect occurs when, compared to individuals who receive a positive frame, individuals who receive a negative frame are significantly more likely to express a preference for the risk-seeking alternative (see Druckman 2001a). For example, in the original disease experiment, Tversky and Kahneman (1981, pp. 453) found that those who received the negative (dying) frame were significantly more likely to express a preference for the risk-seeking Program B (78%), than those who received the positive (saved) frame (28%). This difference in the percentages is called a preference shift (e.g., a 50% preference shift). In the original investment experiment, Tversky and Kahneman (1987, pp. 74–75) find 64% of respondents opt for Program Z (the risk-seeking alternative) when given the negative frame and only 28% do so when given the positive frame.

We randomly assigned each participant to either receive both problems using a positive frame (\(N = 101\)) or both problems using a negative frame (\(N = 113\)).\(^ {15}\) (The problems themselves appeared in a random order.) Participants dealt with one problem at a time and expressed a preference for each problem by checking an alternative on a separate page that followed the particular problem. For each problem, participants also recorded how confident they were that their “choice is the best possible choice.” This item allows us to explore preference confidence, as previously discussed.

Participants also completed a background questionnaire that included our emotion measures as well as other demographic and social questions. To gauge the

\(^{14}\) This variation stems from differences in the location of lives and money on individuals’ utility curves (see Fagley and Miller 1997 pp. 361–362; also see Fagley and Miller 1990; Marteau 1989; Levin and Chapman 1990; Levin et al. 1998).

\(^{15}\) Consistency in the frame used follows prior work (Fagley and Miller 1997).
participants’ emotional states, we used select items from Watson et al.’s (1988) well known PANAS scale. For example, we measured distress by asking respondents: “To what extent do you feel distressed right now, at this present moment?” Respondents chose one of five response options: “very slightly,” “a little,” “moderately,” “quite a bit,” or “extremely.”

We coded the variable so that higher scores indicate greater distress. We used analogous items to measure anger and enthusiasm.

The background questionnaire included other items that prior work has shown to be relevant to risk attitudes and/or framing. We measured propensity toward risk using questions from Zuckerman’s risk taking scale (see Bromiley and Curley 1992, p. 123). Specifically, we include four items (scenarios) to which a person can report a risk-averse or risk-seeking response. We then added together risk-averse responses to create a risk aversion scale. While we expect more risk taking individuals to make more risk-seeking choices and more risk neutral individuals to be more susceptible to framing effects, past research on the effects of risk propensity has been inconclusive (see, e.g., Fagley and Miller 1990, p. 505; Bromiley and Curley 1992; Druckman 2001b).

We also account for expertise since some have posited that experts differ from non-experts in risk attitudes (Bajtelsmit 1999, Unpublished manuscript), and/or susceptibility to framing effects (e.g., Larrick et al. 1993). We construct our expertise measure by combining two commonly used constructs (in the behavioral decision-making literature). First, we measured need for cognition by using Bizer et al.’s (2000) scale, where higher scores indicate greater need for cognition. Second, we asked participants to report the number of economics and statistics courses they had taken (Larrick et al. 1993). We classified participants as experts if they were above the median for both need for cognition and the number of classes.

16 These questions clearly tapped into the current emotional state of the subject, which could have reflected either “state”-based (i.e., the subject is transiently enthusiastic at the moment) or “trait”-based tendencies (the subject is endemically anxious in most situations). We remain agnostic as to the source of the emotion in this context, although future research might explore potential differences in the impact of state versus trait based emotion on risky decision-making. Obviously, this test examines the impact of background emotion on current choice. Experiment 2, reported below, experimentally manipulates background emotion to induce equivalency in emotional states in order to examine their impact on choice.

17 As we will shortly discuss, the background questionnaire included various expertise measures such as need for cognition items and questions about statistics background. We worried that asking these items before having the participants complete the problems could prompt them to view the problems in a more statistical sense and exert more thought, which could substantially change the impact of the problems (see Bless et al. 1998 for evidence of such effects). For this reason, participants filled out the background questionnaire after all participants in a given session finished the problems (the typical session had ten participants). We did, however, include another condition that randomly assigned participants to complete the background questionnaire without taking part in the framing problems. Forty participants were assigned to this condition (these participants were not included in the aforementioned 214 who were in the “main part” of the experiment). The average emotion scores for these participants were nearly identical to the scores of participants who read the problems first. Specifically, the respective averages for the participants in the main experiment and the forty participants in the non-treatment condition are as follows. For anger: 1.78 (SD: 1.13; n: 213) and 1.79 (1.06; 40); for distress: 1.87 (1.06; 213) and 1.85 (1.04; 40); and for enthusiasm: 3.75 (2.76; 213) and 3.77 (.82; 40). This confirms that, as our hypotheses assume, the problems themselves did not affect the participants’ emotional states, and thus, any emotion effects stem from emotion influencing reactions to the problems.
since we expect a moderating effect only for individuals who possess both motivation and ability (e.g., Payne et al. 1993, p. 112).

The impact of gender on framing effects has long puzzled researchers, and thus, we include a dummy variable that identifies female participants. Fagley and Miller (1990, p. 507, 1997) find that women exhibited framing effects while men did not in both monetary and life domains. However, Druckman (2001a) did not replicate this and in general the results are mixed. Evidence also suggests that males tend to be more optimistic than females (Lerner et al. 2003, p. 146) and will thus prefer the risky options (risk-seekers) (Kowert and Hermann 1997, p. 623; Bajtelsmit, 1999, Unpublished manuscript). Finally, we control for whether the participant is a self-identified student or non-student (recall that our sample included a mix of students and older non-students).\footnote{The results are the same if we instead use a continuous measure of age. We opt for the dichotomous measure since it is mostly meant to test for differences between student and non-student participants.}

Framing Results

We begin by investigating for the presence of framing effects (e.g., hypothesis 3). In Tables 1 and 2, we present crosstabs of preferences by frame received for each problem. Both show significant framing effects; exposure to the negative (dying) frame in the disease problem leads 67% of respondents to opt for the risk-seeking alternative, compared with just 45% who received the positive (saving) frame ($z = 3.24; p < .01$ for one-tailed difference of proportions test). The analogous percentages for the investment problem are 66% and 28% ($z = 5.55; p < .01$). Also, consistent with the aforementioned tendency towards risk-seeking in life-death problems, we see greater risk-seeking behavior, across frames, in the disease problem (57%) than in the investment problem (48%) ($z = 1.87; p < .05$).

To explore the impact of emotions and the other variables, we use logit regression. The dependent preference variable is coded such that 0 equals a positive or risk-averse preference while 1 equals a negative or risk-seeking preference. Similarly, frame is coded with 0 indicating exposure to a positive or gains frame and 1 identifying participants exposed to the negative or losses frame. A significant positive coefficient on the frame variable would indicate a framing effect. All other variables are coded as noted above, and are re-scaled to be between 0 and 1.\footnote{The results are the same when using alternative models, such as ANOVA.} We display the results in Table 3.

The first regression, which is for the disease problem, shows a significant main framing effect—those who received the negative frame express significantly more negative or risk-seeking preferences. The results also support most of the emotion hypotheses. Anger leads to more risk-seeking behavior (hypothesis 2), distress leads to less risk-seeking behavior (hypothesis 2), and enthusiasm leads to more risk-seeking behavior (hypothesis 1). The distinct directional impacts of anger and distress accentuate the importance of distinguishing negative emotions.
To explore the moderating effects of emotions, we interact each emotion variable with the frame variable; a significant coefficient indicates a moderating effect. Interestingly, we see evidence that two of the emotional states moderate framing effects. Enthusiasm tempers the framing effect such that those who are more

<table>
<thead>
<tr>
<th>Table 1</th>
<th>Preference by frame for the disease problem (Experiment 1)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Negative (risk-seeking) preference</td>
</tr>
<tr>
<td></td>
<td>67% (n = 76)</td>
</tr>
<tr>
<td></td>
<td>33% (37)</td>
</tr>
<tr>
<td>Total</td>
<td>100% (113)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Table 2</th>
<th>Preference by frame for the investment problem (Experiment 1)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Negative (risk-seeking) preference</td>
</tr>
<tr>
<td></td>
<td>66% (75)</td>
</tr>
<tr>
<td></td>
<td>34% (38)</td>
</tr>
<tr>
<td>Total</td>
<td>100% (113)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Table 3</th>
<th>Determinants of risk-seeking preferences (Experiment 1)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Independent variable</td>
<td>Disease problem</td>
</tr>
<tr>
<td>Negative frame</td>
<td>1.75* (1.00)</td>
</tr>
<tr>
<td>Anger</td>
<td>1.49* (.92)</td>
</tr>
<tr>
<td>Distress</td>
<td>−2.68** (1.13)</td>
</tr>
<tr>
<td>Enthusiasm</td>
<td>2.81** (1.12)</td>
</tr>
<tr>
<td>Anger × frame</td>
<td>−1.56 (1.35)</td>
</tr>
<tr>
<td>Distress × frame</td>
<td>3.82** (1.49)</td>
</tr>
<tr>
<td>Enthusiasm × frame</td>
<td>−4.21** (1.38)</td>
</tr>
<tr>
<td>Risk aversion tendency</td>
<td>−.50 (.67)</td>
</tr>
<tr>
<td>Female</td>
<td>.82 (.54)</td>
</tr>
<tr>
<td>Non-student</td>
<td>.87 (.55)</td>
</tr>
<tr>
<td>Expert</td>
<td>−.73 (.54)</td>
</tr>
<tr>
<td>Risk neutrality × frame</td>
<td>−.67 (.68)</td>
</tr>
<tr>
<td>Female × frame</td>
<td>1.27* (.72)</td>
</tr>
<tr>
<td>Non-student × frame</td>
<td>.07 (.77)</td>
</tr>
<tr>
<td>Expert × frame</td>
<td>.57 (.71)</td>
</tr>
<tr>
<td>Constant</td>
<td>−.55 (.75)</td>
</tr>
<tr>
<td>Log likelihood</td>
<td>−113.09</td>
</tr>
<tr>
<td>Number of observations</td>
<td>200</td>
</tr>
</tbody>
</table>

*Note: The dependent variable is coded so that 0 = choice of the positive risk-averse alternative and 1 = choice of the negative risk-seeking alternative. Table entries are logit coefficients with standard errors in parentheses. ** p ≤ .05; * p ≤ .1 for two-tailed tests

To explore the moderating effects of emotions, we interact each emotion variable with the frame variable; a significant coefficient indicates a moderating effect. Interestingly, we see evidence that two of the emotional states moderate framing effects. Enthusiasm tempers the framing effect such that those who are more
enthusiastic tend to be less affected by the frame (the interaction is highly significant and negative) (hypothesis 5). The opposite is the case for distress, as predicted (hypothesis 4)—increased distress leads to a greater impact of the frame. Finally, anger does not display a significant moderating effect (and its sign is in the opposite direction of what was predicted). This may not be too surprising since distress may be a more relevant negative emotion than anger given the life-death nature of problem (i.e., death may cause people to become upset). The differing results for distress and anger also support our claim that it is necessary to distinguish types of negative emotions when it comes to their effects on attention to external information.

To gauge the substantive main effect, we assume average values on all variables, except distress. In this case, if an individual has no distress (=0), then he or she has nearly a 60% chance of opting for the risk-seeking alternative. The same individual’s probability of being risk-seeking drops to .51 if his or her level of distress rises to the midpoint (=.5), and the all the way to .43 if he or she is maximally distressed (=1).20 This substantial tendency away from risk-seeking and toward risk aversion is heightened even further (by quite a bit) for individuals who receive the gains or save frame since higher levels of distress enhance the impact of the frame.

The precise nature of the interaction between distress and the save frame can be seen in Fig. 1, which plots the expected probability of opting for the risk-seeking alternative for different levels of emotion (minimum, median, and maximum), contingent on frame. (We do this for distress and enthusiasm since those have significant main effects and interactions.) The figure shows that as distress increases, the size of the framing effect substantially widens. Those with no distress (minimum) display a small framing effect (i.e., a .76 probability of risk-seeking for the save frame and .47 for the die frame) while those with maximal distress (maximum) show enormous susceptibility to the frames (with respective probabilities of .89 and .08). We also see that the previously discussed main effect of distress in reducing risky choice occurs only for those exposed to save frame. (The marginal increase for those exposed to the die frame is not significant.)

![Fig. 1](image.png)  
**Fig. 1** Probability of risk-seeking preference (Experiment 1 Disease Problem)

20 We used Clarify to compute these probabilities (as well as ones subsequently reported). Standard errors are available from the authors.
We see the inverse relationship with enthusiasm—those with no enthusiasm (minimum) display a significant framing effect (.70 and .18) while those with maximal (maximum) enthusiasm not only show no framing effect in the predicted direction but they actually flip their risk-preferences (.38 and .72) (i.e., maximally enthusiastic people display more risk-seeking in the save frame). Additionally, we see that the aforementioned main effect of enthusiasm enhancing risk-seeking only occurs for those exposed to the save frame.

Apart from the emotion variables, the only other significant finding is that females are more susceptible to framing effects (the interaction is significant and positive). This is consistent with some prior work, although, as mentioned, gender effects have been inconsistent overall (see Fagley and Miller 1990, 1997). It also shows that variations in emotion by gender do not wholly explain the impact of gender (see Lerner et al. 2003). The lack of significance for the other variables is interesting insofar as prior work focuses on risk proclivity, expertise, and age rather than emotion. Our results suggest that emotions play a much more salient role.

The second column in Table 3 presents the results for the investment problem. Frame again has a significant impact with the negative framing leading to substantially more risk-seeking behavior. This time, however, only one of the three emotions directly affects risk attitudes; enthusiasm leads to more risk-seeking behavior (as predicted by hypothesis 1). Moreover, none of the emotion variables moderate framing effects. As for the controls, the results show that females are significantly more risk-averse as are younger people. We suspect the age finding reflects the fact, for the student participants, that smaller amounts of money are more meaningful, leading to risk aversion.

We find the different results across domains intriguing. Variation in risk attitudes by problem area has received little attention, beyond the aforementioned expectation of greater risk-seeking behavior on life and death problems. Moreover, we know of no work that explores differences in the impact of emotion on risk attitudes by problem domain. There is, however, some evidentiary basis for the greater impact of life and death decisions over other types of choice in terms of exerting an impact on risky choice in risk-sensitive optimal foraging theory, which suggests that risk-seeking increases the closer an organism comes to subsistence level survival (McDermott et al. 2008). Our finding that negative emotions mattered only on a life and death problem and not a financial problem may reflect the inherently emotional nature of life-death decisions, or may suggest that such decisions remain less susceptible to abstract decision-making and conscious override. In contrast, investment decisions, especially when not involving the person’s own money, are not particularly distressing or anger provoking. The significance of enthusiasm on the investment problem may be akin to an irrational exuberance (or greed) that occurs when it comes to decisions involving money.

Our results suggest that emotions affect risk attitudes and in some domains moderate framing effects. Moreover, different negative emotions can have opposite effects; emotions should not be grouped simply based on valence. The impact of emotions will depend on the specific emotions (beyond valence), the issue at hand (problem domain), and the relationship under study (moderating or direct effect).
As mentioned, after expressing their preferences, participants were asked to rate the confidence they had in their expressed preference on a 7-point scale ranging from a low score of “not confident at all” to “moderately confident” to a high score of “very confident.” We examine preference confidence by using ordered probit regressions with confidence scores as the dependent variable. The key independent variables are our three emotion variables. Recall that we expect that anger and enthusiasm will increase confidence while distress will decrease it.

In our analyses, we also include a variable called “frame agreement” that measures whether a participant’s preference on a given problem matches the preference that would be predicted from the frame the participant received. For example, agreement occurs if a participant who received the negative (positive) frame expressed a preference for the risk-seeking (risk-averse) program (i.e., it equals 1 if the participant’s preference agreed with the frame and 0 if not).\(^{21}\) We expect that possibly being affected by the frame will generate overconfidence since it mitigates conflict (see Tetlock 1986; Druckman 2004; also see Kuklinski et al. 2000; Payne et al. 1993, p. 209). We also include the female variable as well as the non-student and expertise variables (the latter two may increase preference confidence (e.g., Sieck and Yates 1997)).\(^{22}\)

The results in Table 4 show that emotions affect preference confidence.\(^{23}\) Specifically, higher levels of distress cause individuals to be less confident in their preferences for both problems. On the investment problem, anger also is significant, Table 4 Determinants of preference confidence (Experiment 1)

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>Disease problem</th>
<th>Investment problem</th>
</tr>
</thead>
<tbody>
<tr>
<td>Frame agreement</td>
<td>.30* (.16)</td>
<td>−.001 (.16)</td>
</tr>
<tr>
<td>Anger</td>
<td>.30 (.27)</td>
<td>.49* (.28)</td>
</tr>
<tr>
<td>Distress</td>
<td>−.43* (.27)</td>
<td>−.41* (.27)</td>
</tr>
<tr>
<td>Enthusiasm</td>
<td>.37 (.28)</td>
<td>.34 (.27)</td>
</tr>
<tr>
<td>Female</td>
<td>−.64** (.16)</td>
<td>−.04 (.16)</td>
</tr>
<tr>
<td>Non-student</td>
<td>−.05 (.16)</td>
<td>.08 (.16)</td>
</tr>
<tr>
<td>Expert</td>
<td>.21 (.15)</td>
<td>.01 (.15)</td>
</tr>
<tr>
<td>Log likelihood</td>
<td>−350.42</td>
<td>−312.98</td>
</tr>
<tr>
<td>Number of observations</td>
<td>206</td>
<td>206</td>
</tr>
</tbody>
</table>

Note: The dependent variable is coded so that higher scores indicated increased confidence. Table entries are ordered probit coefficients with standard errors in parentheses. ** \(p \leq .05\); * \(p \leq .1\); \(+\) \(p \leq .13\) for two-tailed tests. Cutpoints are available from the authors.

Preference Confidence

While a score of 1 is not sufficient evidence that the frame had a causal impact, it is a necessary condition and is the most direct measure available.

We also ran regressions with dummy variables indicating the order in which the individual received the problem (e.g., the disease problem first or second). These variables do not affect the substantive results we present here.

In Table 4, we also note significance when \(p \leq .13\) for a two-tailed test, since on both problems this allows us to note that distress is very close to significant (and would be if we used a one-tailed test).
leading to increased confidence. While these results are consistent with our expectations, we also find no significant effect for enthusiasm (although it is signed in the predicted direction).

While noting the speculative nature of our findings, we also highlight four implications. First, the results suggest that negative emotions may matter more than positive emotions when it comes to preference confidence, perhaps reflecting a negativity bias of some sort. This asymmetry would not be uncommon or atypical; loss aversion robustly demonstrates the greater impact of loss on risk propensity overall. Second, the results again accentuate the importance of distinguishing negative emotions from one another, according to variations in control and uncertainty. Third, they also reveal that reasoning may differ across problem domain. Fourth, even if emotions do not directly affect preference formation, they may still be consequential—in the case of the investment problem, anger and distress did not influence the alternative chosen but did affect confidence in that decision. On the disease problem, we also see two other variables are significant—frame agreement and being female.

**Experiment 2**

The design of our second experiment mimicked that used in the first experiment with one major exception. This time instead of measuring individuals’ emotions with standard items, we experimentally induced emotional variations between participants. This approach enables us to assess the possibility that our experiment 1 emotion results stem from some other unmeasured variable (e.g., a spurious relationship) (Lerner and Keltner 2000, pp. 486–487). It also might provide insight into the relative impact of chronic versus temporary emotional states. Emotions expressed in the first experiment may have reflected either dispositional variations between individuals or situational—shorter term—distinctions that individuals happen to be experiencing. In our second experiment, emotional variations are presumably temporary (i.e. “state”-based as opposed to “trait”-based) as determined by the experimental manipulation.

**Participants, Design, and Procedure**

A total of 185 individuals participated in the experiment, after having completed an unrelated survey. Participants were non-student community members who had been asked to take part in an unrelated voting behavior study (for details on the sample, which was largely representative of the community, see Druckman 2007, unpublished manuscript). After completing the unrelated study, we asked a sub-sample of individuals to take part in another study (i.e., the experiment described here) in exchange for a cash payment.

We randomly assigned each participant to one of six experimental conditions that varied the frame—using either a positive frame ($N = 91$) or a negative frame ($N = 94$)—and the emotional inducement—priming enthusiasm, anger, or distress.
Our emotional inducement technique followed the approach used by Lerner and Keltner (2001, p. 152) (also see Lerner et al. 2003; DeSteno et al. 2004). Specifically, prior to receiving the framing problems, participants were asked to “Imagine a few things that make you feel [“enthusiastic or excited”/“angry”/ “distressed or upset”].” We then asked participants to answer the same two framing problems we used in the prior study (i.e., the disease and the investment problems).

Results

We display the results for each problem in Tables 5 and 6. We present the percentages of respondents who reported risk-seeking preferences. We need not include control variables given random assignment to the emotional inducements.24

We find significant overall framing effects, across conditions, for both problems. In the disease problem, 66% of participants in the negative frame condition opted for the risk-seeking alternative while 52% did so in the positive frame condition ($z = 1.98; p < .05$). The analogous percentages for the investment problem are 66% and 48% ($z = 2.35; p < .01$). We do not, however, replicate our prior finding of significantly greater overall risk-seeking behavior in the disease problem (compared to the investment problem).

Enthusiasm again leads to significant increases in risk-seeking behavior, across frames, with 68% displaying such preferences in the disease problem and 66% in the investment problem. Both these percentages significantly exceed the amount of risk-seeking displayed by those in the distress condition (for the disease problem, $z = 2.71; p < .01$; for the investment problem, $z = 1.94; p < .05$). We find mixed results for the anger group; in the disease problem, as predicted, the anger condition (64%) is significantly greater than the distress condition (43%; $z = 2.35; p < .01$), but this is not the case in the investment problem where the respective percentages are 57% and 48% (although it is in the correct direction; $z = 1.0; p < .20$). Overall, then, we have strong support across problems that enthusiasm promotes risk-seeking behavior relative to distress, and some support for the hypothesis that anger does the same and thus differs in its impact from the negative emotion of distress. As before, we find slightly different dynamics across problem domains with the disease problem displaying stronger emotion effects.

In terms of emotion and framing, we find evidence consistent with prediction that negative emotions have distinct effects on framing. Distress enhances framing and anger tempers it. For both problems, the distress group displays significant framing effects (for the disease problem, $z = 2.47; p < .01$; for the investment problem, $z = 1.62, p < .05$) while the anger group does not (for the disease problem, $z = .98; p < .20$; for the investment problem, $z = 1.02; p < .20$). The results on enthusiasm are more mixed—in the disease problem, enthusiasm clearly vitiates framing as the effect disappears, but a nearly significant framing effect remains in the investment problem ($z = 1.49; p < .07$). We thus again find distinctions across domains.

24 Given the nature of experiment 2, we were unable to measure some of the relevant controls used in experiment 1 including risk neutrality and expertise.
We explore preference confidence in Table 7, using a similar approach as in experiment 1. The design of our experiment means we can only investigate relative confidence in the different emotion conditions. Since we predict that anger and enthusiasm will increase confidence, and distress will depress it, we include dummy variables for the former two variables which reveal the effects relative to the distress condition. The results show that for both problems, anger and enthusiasm prompted participants to exhibit significantly greater confidence than those in distress condition. We also again find that frame agreement enhances confidence.

In sum, evidence from our second experiment—that used a distinct sample and manipulation—confirms what we found in the first study. The framing of problems affects risk preferences, but so do emotions. Moreover, different negative emotions have divergent effects both in terms of their direct impact on risk preferences and on how they interact with the frames. This accentuates the importance of incorporating discrete emotions in the study of risky framing. We also find that emotional states influence preference confidence, which might in turn impact the relationship between preferences and behavior. Finally, the precise impact of emotions and frames may vary across problem domains.

### Table 5 Risk-seeking preferences by condition for the disease problem (Experiment 2)

<table>
<thead>
<tr>
<th></th>
<th>Negative (dying) frame</th>
<th>Positive (saving) frame</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Enthusiasm</td>
<td>69% (32)</td>
<td>68% (28)</td>
<td>68% (60)</td>
</tr>
<tr>
<td>Anger</td>
<td>70% (33)</td>
<td>58% (36)</td>
<td>64% (69)</td>
</tr>
<tr>
<td>Distress</td>
<td>59% (29)</td>
<td>26% (27)</td>
<td>43% (56)</td>
</tr>
<tr>
<td>Total</td>
<td>66% (94)</td>
<td>52% (91)</td>
<td>59% (185)</td>
</tr>
</tbody>
</table>

### Table 6 Risk-seeking preferences by condition for the investment problem (Experiment 2)

<table>
<thead>
<tr>
<th></th>
<th>Negative (loss) frame</th>
<th>Positive (gain) frame</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Enthusiasm</td>
<td>74% (31)</td>
<td>56% (27)</td>
<td>66% (58)</td>
</tr>
<tr>
<td>Anger</td>
<td>64% (33)</td>
<td>51% (35)</td>
<td>57% (68)</td>
</tr>
<tr>
<td>Distress</td>
<td>59% (29)</td>
<td>37% (27)</td>
<td>48% (56)</td>
</tr>
<tr>
<td>Total</td>
<td>66% (93)</td>
<td>48% (89)</td>
<td>57% (182)</td>
</tr>
</tbody>
</table>

We explore preference confidence in Table 7, using a similar approach as in experiment 1. The design of our experiment means we can only investigate relative confidence in the different emotion conditions. Since we predict that anger and enthusiasm will increase confidence, and distress will depress it, we include dummy variables for the former two variables which reveal the effects relative to the distress condition. The results show that for both problems, anger and enthusiasm prompted participants to exhibit significantly greater confidence than those in distress condition. We also again find that frame agreement enhances confidence.

In sum, evidence from our second experiment—that used a distinct sample and manipulation—confirms what we found in the first study. The framing of problems affects risk preferences, but so do emotions. Moreover, different negative emotions have divergent effects both in terms of their direct impact on risk preferences and on how they interact with the frames. This accentuates the importance of incorporating discrete emotions in the study of risky framing. We also find that emotional states influence preference confidence, which might in turn impact the relationship between preferences and behavior. Finally, the precise impact of emotions and frames may vary across problem domains.

### Table 7 Determinants of preference confidence (Experiment 2)

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>Disease problem</th>
<th>Investment problem</th>
</tr>
</thead>
<tbody>
<tr>
<td>Frame agreement</td>
<td>.24* (.15)</td>
<td>.30** (.16)</td>
</tr>
<tr>
<td>Anger</td>
<td>.51** (.19)</td>
<td>.54** (.19)</td>
</tr>
<tr>
<td>Enthusiasm</td>
<td>.58** (.20)</td>
<td>.47** (.20)</td>
</tr>
<tr>
<td>Log likelihood</td>
<td>−340.35</td>
<td>−308.60</td>
</tr>
<tr>
<td>Number of observations</td>
<td>185</td>
<td>181</td>
</tr>
</tbody>
</table>

*Note: The dependent variable is coded so that higher scores indicated increased confidence. Table entries are ordered probit coefficients with standard errors in parentheses. ** p ≤ .05; * p ≤ .1; * * p ≤ .13 for two-tailed tests. Cutpoints are available from the authors.*
Recall that in our second experiment, due to the inducements, we can be confident that the emotions reflect temporary states. Thus, the similarity of our results across the two experiments suggests either that the emotions measured in experiment 1 also reflect temporary states, or that chronic emotions (perhaps captured in experiment 1) and temporary emotions have the same effects.

Towards a Theory of Emotion and Framing

In its original incarnation, Kahneman and Tversky labeled their nascent theory of decision-making under conditions of risk, “value theory” (Kahneman 2000). They did this, in part, to highlight the distinction between decision utility and experienced utility, noting the important difference between thinking and feeling. This differentiation in many ways presaged Slovic et al.’s (2004) delineation of risk as analysis and risk as feelings. While prospect theory progressed in a more cognitive fashion, representing the apex of the revolution it signified in psychology, the underlying notion of experienced utility was not lost altogether. Indeed, Kahneman has gone on to investigate the impact of emotion on assessments of well-being and happiness in his more recent work (Kahneman et. al. 1999). And framing effects can encompass such experiential utility.

One of the ways in which framing effects inculcate emotion is through the process of mental accounting (Thaler 1980). Mental accounting offers a description of how people seek to organize, trade-off, evaluate, assess and rationalize various options and events in their lives. Mental accounts induce strong emotions when they come to be closed, and individuals must draw up a kind of psychic balance sheet for particular experiences. In this way, choosing to instigate or terminate a mental account creatively incurs punishments or rewards, whose timing is often at least partly determined by the actor (Kahneman 2000). In normative judgments, sunk costs should never be counted in rendering decisions concerning future actions, but emotional considerations often pull in an opposite direction, and individuals may choose to delay shutting a particular mental account in order to put off the day of reckoning with past failures. Given previous discussion, it should not be surprising that these emotional and analytical processes can occur simultaneously on different tracks, all the while moving in opposite directions. At some point, the parallel mental processes must arrive at the same station and come to a halt, but conscious processes can sometimes delay that moment of evaluation and assessment.

In this light, emotion can clearly serve as a basis, at least in part, for a more comprehensive understanding of the origins of framing effects (Bueno de Mesquita and McDermott 2004). Emotion can provide a foundation upon which the framing of particular options can be based or constructed, thus influencing decision prior to choice, as well as during choice itself. While prospect theory may not present an explicit theory of framing, the indication of experiential utility points the way to at least one mechanism by which individuals may choose the frames they adopt as default values, through processes of mood congruence.

As with other aspects of prospect theory, the impact of emotion on frames may depend on domain (e.g., Forlani 2002). This may help explain why negative frames
induced more risk-seeking regardless of emotion. For example, distress may have more of an impact in life and death decisions than those concerning investment precisely because the former presents more of an imminent threat of loss in the vast majority of situations; for one thing, loss of life represents a threat encompassing a much broader scope than even the prospect of financial collapse (a theory of domain effects, however, still eludes us). Further, anger may not exert the kind of impact expected precisely because it does not easily map onto a gain/loss divide. Indeed, anger may represent an internal calculus of a relational welfare trade-off ratio, by which individuals assess whether or not others are treating them with the respect they believe they deserve (Tooby et al., n.d.). Rather, anger appears to increase the biased nature of information search, which reduces the impact of a presented frame, precisely because the angry perceiver imposes an internal filter designed to seek biased confirmation and assimilation of preexisting beliefs (Lord et al. 1979). This strategy may exist, in part, to perpetuate the mood state in order to maintain a motivation to fight if necessary.

Conclusion

Our results, combined with previous literature, suggest the merit of analyzing the impact of specific, discrete emotions on particular outcomes of interest, including their impact on framing effects. Further, the effect of these emotions on particular outcomes of interest may vary by context and domain, such that issues representing more critical, or familiar, challenges may produce systematically different effects. In this case, for example, the disease problem appeared more evocative than the investment one. Loss aversion suggests that losses hurt more than equal gains please; similarly, it may be the case that emotions may exacerbate the assessment of potential losses more than they might influence the assessment of prospective gains.

Overall, emotion clearly affects risk propensity in ways previously underappreciated. And further, emotion can also moderate framing effects in particular contexts (also see Lerner and Keltner 2001; Chang 2007). Importantly, different negative emotions, such as distress and anger, exert opposite effects, at least in some domains. Anger encourages greater risk-seeking, while distress encourages a more cautious approach. The study of decision-making needs to incorporate, explicitly, the effect of specific emotions on particular issues at hand. In so doing, however, it is important that researchers resist exploring a proliferation of emotions. The theoretical work on which we drew suggests that, in the case of negative emotions, there will be differences based on the extent to which the emotion generates anxiety as opposed to aversion. While we focused specifically on distress and anger, we would expect our results to be the same if we had instead looked at fear (an anxiety provoking emotion) and hostility (an aversion emotion), for example (see Lerner and Keltner 2001; however, also see Roseman et al. 1994). Future work also needs to more explicitly explore the origins of emotions and emotional reactions (e.g., Strelau et al. 2002).

Emotions serve motivating functions. As such, discrete emotions can encourage individuals to adopt particular frames for choice, establish internal feedback loops
for experienced utility, and create foundations for the assessment of events through mental accounting. While cognitive biases clearly tell part of the story about the mechanisms by which individuals make rapid and efficient decisions, emotional motives must also enter the narrative to provide a more complete picture of the intertwined processes by which humans make judgments and choices about the world around them.

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References


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 participants report an overt judgment (e.g., Schwarz, 1990; Schwarz & Clore, 1983, 1988). In most instances, this misattribution effect probably occurs by default and with little conscious deliberation (Schwarz, 1990). Yet, an analogous misattribution effect often fails to emerge 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).

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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 & Carlton, 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, Seta, & Crella, 1990; Strack, 1992). If the perceiver possesses sufficient cognitive resources, this corrective process is postulated to occur on-line as the perceiver forms and updates the summary evaluation of the target person.

Who possesses the cognitive capacity to engage in on-line correction and who does not? We speculate that a key dimension involves individual differences in processing efficiency. The logic behind this distinction is simple. Individuals who can process the target information in an efficient manner are less likely to fully exhaust their cognitive resources when doing so. As such, they should be more likely to possess the supplemental resources needed to engage in correction. In contrast, individuals who process the target information in an inefficient manner should be more likely to fully exhaust their cognitive resources when processing this information. As such, these individuals should be less likely to possess the resources needed to engage in correction. In sum, we predicted that inefficient processors would fail to engage in correction and reveal a mood assimilation effect. In contrast, efficient processors of the candidate information should show evidence of correction. This might reduce the assimilation effect or, alternatively, produce a contrast effect if these perceivers overcorrect for the biasing influence of mood (see Wyer & Budesheim, 1987, for a related formulation).

Evidence of the moderating role of expertise, a key determinant of processing efficiency, has already been presented by Srull (1983). He demonstrated that mood during exposure to information about an automobile produced assimilation effects among nonexperts. This effect was eliminated (not reversed) among experts who possessed a great deal of prior knowledge about automobiles. Srull maintained that experts encode stimulus information in a richer and more efficient manner and, as such, are less susceptible to the influence of extraneous factors such as mood. Experts in Srull's experiment appeared to simply discount or ignore irrelevant affective criteria when they were computing a judgment (see also Strack, 1992). Because mood does not enter into the judgmental computation in the first place, the mood assimilation effect is simply eliminated for experts. The correction process we postulate, which involves adjustment for mood-induced bias after it has already occurred, will produce this pattern if participants correctly estimate the initial biasing influence of mood. If participants happen to possess an exaggerated subjective theory of mood-induced bias, correction for perceived bias will result in overcorrection. Overcorrection will produce a contrast effect in which positive mood produces a more negative evaluation of the object than does negative mood (see Wyer & Budesheim, 1987, for a related formulation).

Our model is perhaps more closely related to Martin's (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 concealed. 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-
ence (e.g., conceived as an alternative to a prime of self-confident), judgments are contrasted from the originally primed concept. Thus, Martin's model pertains to conditions in which (a) the priming manipulation activates material that is relevant to interpreting the target information, (b) the target information is sufficiently ambiguous to allow for alternative semantic interpretations, and (c) the valence of the alternative interpretation is opposite to the valence of the primed interpretation.

Although our conceptualization was inspired by Martin's (1985, 1986; Martin et al., 1990) set-reset model, it differs in terms of the specific psychological processes postulated. Whereas Martin's model focuses on the effects of a cognitive prime, our model focuses on the effects of mood during exposure to the target person. Moreover, our model posits that mood can directly influence the evaluation of stimulus material possessing a fixed semantic interpretation (e.g., antiabortion), rather than biasing the relative accessibility of alternative interpretations of this material. That is, mood misattribution directly influences on-line evaluations of the target person, independent of how this information is semantically interpreted. In our experiments, the target information was unambiguous (e.g., "views abortion as murder," "wants to make abortion illegal") and, as such, did not afford alternative interpretations. Furthermore, the semantic content of the mood induction was irrelevant to the target information and thus was unlikely to prime a concept that was relevant to interpreting this information. Thus, instead of suggesting that mood biases the relative accessibility of alternative semantic interpretations of the stimulus material, we propose that mood misattribution directly influences evaluations of the target information.

Our model of correction also differs from the one Martin (1985, 1986; Martin et al., 1990) proposed. In Martin's model, correction involves using "a set of probe cues to access a new concept with which the individuals interpret the target information" (Martin et al., 1990, p. 28). Evaluation of the target is then guided by this new interpretation. In our model, corrections are made by adjusting on-line evaluations of the target person to compensate for the perceived biasing influence of mood. Thus, if participants believe a positive mood has inflated initial target evaluations by a given amount, correction entails simply subtracting this same amount to arrive at adjusted evaluations of the candidate. Of course, if participants overestimate the initial biasing influence of mood, they will subtract more than they should when correcting for these evaluations. This can result in a contrast effect of mood on judgments. In any case, this correction process does not involve retrieval of an alternative concept to be used as a basis for a revised interpretation. In sum, although the currently proposed model was inspired by Martin's set-reset model, it delineates a distinct psychological process that occurs under conditions that differ from those examined by Martin.

Who Possesses Adequate Resources to Engage in Correction? Assessing the Moderator

As noted previously, we speculate that individuals who process the candidate information in an efficient manner are more 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, 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 expert's 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 evaluated during the same task. 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 $2 \times 2$ 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, unknowingly 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.2 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 pretext, 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 2 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 reinstalation 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 indicator 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.3 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 agreements.

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, Abelzon, & 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 covaried.

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 meet 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 \(\times\) 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 \(\times\) 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.
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.\textsuperscript{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 Galiphol and Sophia'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 $2 \times 2$ (negative vs. positive mood) X (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.

**Table 2**

<table>
<thead>
<tr>
<th>Total recall</th>
<th>Negative</th>
<th>Positive</th>
</tr>
</thead>
<tbody>
<tr>
<td>High</td>
<td>54.97</td>
<td>30.81</td>
</tr>
<tr>
<td>(Efficient)</td>
<td>(6)</td>
<td>(7)</td>
</tr>
<tr>
<td>Low</td>
<td>47.86</td>
<td>54.99</td>
</tr>
<tr>
<td>(Inefficient)</td>
<td>(6)</td>
<td>(4)</td>
</tr>
</tbody>
</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.

**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

\textsuperscript{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 x 2 x 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:

You will be given some information to read about a political candidate. However, because this will only take a few minutes, the principal investigator has agreed to allow Dr. Richard Mann, a psychologist from the University of Massachusetts, to run a brief study during this time. Dr. Mann needs a sample of American college students’ personal experiences to use in constructing stimulus materials for a study next semester.” 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 conditions) 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.
Table 3

Adjusted-Mean Evaluations as a Function of Political Expertise, Delay, and Mood (Experiment 3)

<table>
<thead>
<tr>
<th>Political expertise</th>
<th>5-min delay</th>
<th>8-day delay</th>
<th>Pooled</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Negative mood</td>
<td>Positive mood</td>
<td>Negative mood</td>
</tr>
<tr>
<td>High</td>
<td>5.87</td>
<td>4.63</td>
<td>5.54</td>
</tr>
<tr>
<td>(Efficient)</td>
<td>(23)</td>
<td>(15)</td>
<td>(32)</td>
</tr>
<tr>
<td>Low</td>
<td>4.53</td>
<td>4.94</td>
<td>4.73</td>
</tr>
<tr>
<td>(Inefficient)</td>
<td>(14)</td>
<td>(26)</td>
<td>(30)</td>
</tr>
</tbody>
</table>

Note. The evaluation scale ranges from 1 to 11 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.

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 \( (M = 3.58) \), \( F(1, 189) = 21.35, p < .001 \).

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 \( (M = 4.79) \), \( F(1, 184) = 5.34, p < .05 \). 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

\(^7\) 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. 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 ($F_3 < 1$).

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 ($ps > .20$).

**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 perceiver 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 online interpretation of overcorrection and contrast.

The novelty of the presently reported findings and the
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; Iser 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 effect prior to judgmental computation (see Strack, 1992, for this distinction). Ignoring or discounting mood-induced effect 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; Srull, 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 10

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10 It may be true that the precorrected influence of mood is weaker for individuals of high expertise (or high recall), and this might be due to individual differences in systematic processing. However, to engage in the correction process that results in contrast, high-expertise (or high-recall) individuals must possess the necessary supplemental resources. Unlike processing efficiency, individual differences in systematic processing of the candidate information cannot account for the differential availability of supplemental resources (i.e., resources that have not been fully exploited in arriving at a precorrected evaluation of the candidate). Moreover, if our individual-differences measures captured individual differences in systematic versus heuristic processing, low-recall (or low-expertise) individuals should have been more likely to rely on partisan or ideological stereotypes as a heuristic when evaluating the candidate (Ottati, 1990; Ottati, Terkildsen, & Hubbard, 1994). In addition, high-recall (or high-expertise) individuals should have been more likely to systematically evaluate the candidate in terms of his specific issue positions. In fact, our individual-differences measures failed to moderate the effect of party, ideology, or issues in all three experiments.
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 little 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.

References


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Does information irrelevant to government performance affect voting behavior? If so, how does this help us understand the mechanisms underlying voters’ retrospective assessments of candidates’ performance in office? To precisely test for the effects of irrelevant information, we explore the electoral impact of local college football games just before an election, irrelevant events that government has nothing to do with and for which no government response would be expected. We find that a win in the 10 d before Election Day causes the incumbent to receive an additional 1.61 percentage points of the vote in Senate, gubernatorial, and presidential elections, with the effect being larger for teams with stronger fan support. In addition to conducting placebo tests based on postelection games, we corroborate these aggregate-level results with a survey that we conducted during the 2009 NCAA men’s college basketball tournament, where we find that surprising wins and losses affect presidential approval. An experiment embedded within the survey also indicates that personal well-being may influence voting decisions on a subconscious level. We find that making people more aware of the reasons for their current state of mind reduces the effect that irrelevant events have on their opinions. These findings underscore the subtle power of irrelevant events in shaping important real-world decisions and suggest ways in which decision making can be improved.

Irrelevant events affect voters’ evaluations of government performance

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We build on this research to show that events that government had nothing to do with, but that affect voters’ sense of well-being, can affect the decisions that they make on Election Day. We extend the psychological and decision sciences literatures by showing the effect of individual well-being on judgment outside the laboratory setting, in a real-world situation where collective stakes are high (even if the individual stakes may not be). In two different domains, our evidence indicates that voters’ personal sense of well-being—as determined by events that are unrelated to political and economic affairs—affects their evaluations of their elected representatives.

Given the relatively small costs to any individual of making a mistake, we might expect voters to make a wide variety of errors. At the same time, extant research has implicitly assumed that voters at least clear the relatively low standard of rationality implied by the ability to exclude entirely irrelevant events from the decision-making process. Whereas previous political science and economics research has advanced on the assumption that voters do not allow irrelevant events to affect their decisions, the psychological literature makes an association between voter well-being and decision making not only possible, but likely. Voters who are in a positive state of mind on Election Day are likely to use their mood as a signal for the incumbent party’s success (8) and access positive memories about the incumbent party (9) and/or interpret past actions taken by the incumbent party more favorably (10). Additionally, positive emotions may cause voters to be more satisfied with the status quo (e.g., refs. 11 and 12). Those voters may then be more likely to choose the incumbent party in the election.

To test whether irrelevant events affect voters’ decisions, we consider a unique quasi-experimental context: local sports outcomes. These game outcomes create an ideal variable for testing the hypothesis that voters’ decisions are affected by events separate from politics, because (i) they have been shown to significantly affect people’s well-being, either directly or via mood contagion in social networks (13–16), and (ii) they are unrelated to public affairs. No government response would be expected in response to game outcomes and the public would almost certainly not relate them to incumbent performance. Moreover, we find that voters respond to the random, unexpected outcome of game outcomes, further illustrating that voters appear to be responding to short-term emotional stimuli as opposed to responding to a team’s overall strength. Additionally, we find little evidence of a difference between private and public schools once fan interest is accounted for, suggesting that government involvement in collegiate athletics is not driving voter decision making. The random components of sports outcomes stand in stark contrast to even seemingly random events such as natural


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disasters, where incumbents may not have direct control over the event itself, but may be plausibly held responsible by voters for either preparation or response.

We analyze the relationship between prelection college football outcomes and the electoral performance of the incumbent party with aggregate-level data (study 1). Additionally, we collected original survey data during the 2009 NCAA men’s basketball championships to corroborate our results at the individual level (study 2) and embedded an experimental manipulation to show that the effect of externally-induced mood on political judgments can be eliminated when subjects are explicitly exposed to the irrelevant information, consistent with previous laboratory research (6). The aggregate-level study is intended to show that effects previously found in the laboratory actually exist in the real world in a consequential domain. The survey experiment allows us to test for a mechanism underlying our aggregate results.

**Results**

**Study 1: Presidential, Gubernatorial, and Senate Elections, 1964–2008.** We analyzed county-level election results from presidential, gubernatorial, and Senate elections between 1964 and 2008. We assessed the influence of irrelevant events on voting decisions by measuring the impact of prelection local college football outcomes (see Table S1 for a complete description of the teams involved in these games) in the county on the incumbent party’s vote share. We define the incumbent’s vote share to be either the vote share of the incumbent officeholder (sitting president, governor, or senator) or the new candidate of the current officeholder’s party (i) to remain consistent with the extant literature in political science and economics and (ii) because an exogenous shock to voter well-being is hypothesized to influence voters’ satisfaction with the status quo, which is represented by the incumbent party.

We further demonstrate robustness by using home team point spreads from the betting market. The point spreads can be used to estimate a team’s chances of winning the game before the game occurs (19). By conditioning on the ex ante probability of victory, we can construct an independent variable that isolates the surprise component of game outcomes, which is by definition uncorrelated with the other independent variables. This quasi-experiment enables us to estimate the effect of the exogenous shock to well-being.

We replicate our fully specified regressions (including fixed effects and demographic controls) and additionally control for the expected number of wins, thereby isolating the surprise component of the game outcomes (Table 1, row 6). Not surprisingly, the effect size increases somewhat, as voters appear to respond more to the surprise component of the game outcomes than they do to the component that is captured by the relative strengths of the two teams. Controlling for the expected number of wins, the effect of a win on incumbent party vote share is 1.61 percentage points (P = 0.05).
Table 1. Effect of a football victory on the incumbent party’s vote share

<table>
<thead>
<tr>
<th>Date of game</th>
<th>Preelection games (pooled)</th>
<th>1 wk before</th>
<th>Week of election</th>
<th>2 wk after</th>
<th>Postelection games (pooled)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td></td>
<td>1.13*</td>
<td>0.81</td>
<td>0.80**</td>
<td>−0.09</td>
</tr>
<tr>
<td>(0.58)</td>
<td></td>
<td>(0.58)</td>
<td>(0.34)</td>
<td>(0.60)</td>
<td>(0.66)</td>
</tr>
<tr>
<td>Include demographics</td>
<td></td>
<td>1.70***</td>
<td>1.12***</td>
<td>1.17***</td>
<td>0.46</td>
</tr>
<tr>
<td>(0.57)</td>
<td></td>
<td>(0.48)</td>
<td>(0.34)</td>
<td>(0.55)</td>
<td>(0.60)</td>
</tr>
<tr>
<td>Include fixed effects</td>
<td></td>
<td>1.47**</td>
<td>1.05*</td>
<td>1.10***</td>
<td>0.43</td>
</tr>
<tr>
<td>(0.58)</td>
<td></td>
<td>(0.53)</td>
<td>(0.37)</td>
<td>(0.53)</td>
<td>(0.51)</td>
</tr>
<tr>
<td>High-attendance teams</td>
<td></td>
<td>3.35***</td>
<td>2.20*</td>
<td>2.42***</td>
<td>0.46</td>
</tr>
<tr>
<td>(1.04)</td>
<td></td>
<td>(1.28)</td>
<td>(0.66)</td>
<td>(0.96)</td>
<td>(1.33)</td>
</tr>
<tr>
<td>Championship teams</td>
<td></td>
<td>2.63***</td>
<td>2.94**</td>
<td>2.30***</td>
<td>0.23</td>
</tr>
<tr>
<td>(1.14)</td>
<td></td>
<td>(1.30)</td>
<td>(0.70)</td>
<td>(1.03)</td>
<td>(1.42)</td>
</tr>
<tr>
<td>Include game expectations</td>
<td></td>
<td>2.59***</td>
<td>0.78</td>
<td>1.61***</td>
<td>−0.90</td>
</tr>
<tr>
<td>(0.88)</td>
<td></td>
<td>(0.98)</td>
<td>(0.58)</td>
<td>(0.90)</td>
<td>(0.81)</td>
</tr>
</tbody>
</table>

Dependent variable is vote share for the incumbent party. Regression SEs, corrected for clustering at the county level, are in parentheses. Senator is the excluded category for the office. Each of the first three rows builds on each other. In other words, in rows 2–6, demographic controls are included. Rows 3–6 all include both fixed effects and demographic controls. The fourth and fifth rows report the estimated effect obtained by summing the coefficient for the wins variable and an interaction between the wins variable and the high-attendance and championship dummy, respectively. n = 1,632 and n = 1,659 for preelection games and postelection games, respectively. Due to the availability of point spread data only back to 1983, n = 838 and n = 856 for preelection games and postelection games, respectively, when controlling for the probability of a victory.

*P < 0.10, **P < 0.05; ***P < 0.01 (two-tailed).

0.01, see column 4 of Table S7 for full regression results). The effect may be somewhat stronger for the games occurring the week before the game than for the games immediately preceding the election, although the effects are not statistically distinguishable (P = 0.21). Moreover, the coefficient on the expected number of wins is near zero, indicating that the surprise component of game outcomes drive our findings. Again, we find that the effect size is similar across public and private schools (see column 5 of Table S7 for full regression results).

Throughout our analyses, we define incumbent to be either the incumbent officeholder or the new candidate of the current officeholder’s party. Another possibility that we considered is that the incumbent presidential party could benefit in Senate elections from local team success. Our regression results provide some evidence that this may indeed be the case, so that at least in Senate elections, it appears to be both the incumbent presidential party and the incumbent Senate party in the state that benefit when the local football team wins. (If the incumbent presidential party’s vote share in Senate elections is used as the dependent variable in a regression where we include county and year fixed effects as well as county demographics, we obtain a coefficient of 2.05 with a SE of 0.99.)

Study 2: Survey Experiment. We conducted a survey with an embedded experiment during the 2009 NCAA men’s college basketball tournament. Subjects were Americans living in areas where there were college basketball teams participating in the tournament. Also known as “March Madness,” the tournament consists of 64 teams and six rounds of games. It is a single elimination tournament, meaning that each game is critical and likely to induce strong emotional reactions among fans. One advantage of the survey data over the aggregate data is that we do not have to assume that support for a team is necessarily tied to geographic location as we are able to ask respondents to name their favorite team. We interviewed respondents immediately after the third and fourth rounds of the tournament (the “Sweet Sixteen” and “Elite Eight” games) and before the fifth round (the “Final Four”). Half of the respondents (treatment group) were randomly assigned to receive the outcomes of their team’s games before answering a question about presidential job approval. The other half (control group) received no information about their team’s performance.

Results. As with the college football outcomes, we constructed a measure of the random component of wins, defined as the actual number of wins the team experienced during the third and fourth rounds minus the expected number of wins as determined by the betting markets. We obtain similar results if we simply use the game outcomes as opposed to isolating the random part of those events. As we anticipated, each additional adjusted win experienced by respondents significantly increased approval of President Obama’s job performance, with the effect size being 2.3 percentage points (P = 0.04). Hence, these survey results conform with what we observed in the field data—changes in well-being induced by the surprise component of sporting events affect people’s evaluations of the incumbent. We find no difference in the effect of basketball victories for private and public schools (see Tables S8 and S9 for full regression results).

Further evidence can be found by examining people who are strong supporters of their teams and who were closely following the tournament. Among these intense fans, the effect of an adjusted win was 5.0 percentage points (P = 0.008). Among nonintense fans, adjusted wins insignificantly increased Obama approval by only 1.1 percentage points (P = 0.41). The 3.9 percentage point difference in effect size between these two subgroups of respondents is significant at the 10% level (P = 0.07).

The survey data also allowed us to demonstrate that the effect of mood on political decision making appears to be subconscious. By randomly treating some individuals with information about the outcomes of their team’s games, we are able to test whether making the event (the game outcome) immediately salient decreased its subconscious effect, as psychological research has found that making subjects aware of the reasons for their mood decreases the tendency to misattribute those moods (6). Among respondents in the control group, the effect of an adjusted win was 4.6 percentage points (P = 0.003). Conversely, the effect of basketball outcomes on the treatment group (which was explicitly told the score(s) of the game(s)) was basically zero (B = 0.00, P = 0.96). The 4.6 percentage point difference between treatment conditions is also statistically significant (P = 0.04). The results show that making the game outcomes salient eliminated their impacts. By moving subconscious considerations into the forefront, the experimental prime allowed people to decouple their mood change induced by their team’s fortunes from the political object of judgment (President Obama).

Discussion

These results provide evidence that voting decisions are influenced by irrelevant events that have nothing to do with the competence or
effectiveness of the incumbent government. As discussed above, analyzing the effects of sporting outcomes provides a cleaner test than other environments considered in previous research, because no government action is taken or would be expected to be taken in preparation for or in response to game outcomes. Our findings, summarized in Fig. 1, are consistent across our aggregate- and individual-level results, indicating that these findings are likely to generalize to related environments. These results thus suggest potential new ways for researchers to open the black box and understand the processes underlying voters’ decisions. For example, researchers and election observers have long noted that incumbents’ prospects for reelection are tied to the health of the economy. We have shown evidence for a mechanism underlying this empirical regularity that is not about rational voters processing relevant information. Another reason why we observe the strong correlation between economic performance and the probability of incumbent reelection may be that voters’ general sense of well-being serves as a conduit between the state of the economy and electoral outcomes.

Our findings suggest a variety of important implications for understanding the cognitive processes underlying voting behavior. If unrelated events affect political judgment, a voter’s opinions and feelings in any given area are likely to affect that voter’s perceptions of other aspects of an incumbent’s performance or personality. For example, a voter who is presented with negative information about the local economy may perceive a separate news story about the president’s foreign policy in a less positive light. Alternatively, a negative campaign advertisement designed to elicit fear or anger may affect voters’ assessments of a candidate’s performance in office. Our results thus have implications for understanding elite incentives and strategies to manipulate voters’ perceptions of their own well-being. Events and information themselves may not be paramount in explaining election results. Rather, what may be most important is how campaigns use those events to affect voters’ perceptions of both their own well-being and the well-being of others to whom they are socially connected, given the spillover effects of mood.

However, the individual-level study points to a possible underlying mechanism that also suggests that the effect of mood induced by irrelevant events on voting is potentially fragile. Once the game outcome is made salient, its effect on political choice is eliminated. In other words, it appears that moving affect tied to an event from the subconscious to the conscious may allow people to reject irrelevant information because people then understand that their current state of well-being is unrelated to an incumbent’s performance in office.

Future research can build upon these findings in at least two ways. First, it would be interesting to explore the conditions under which voters base their decisions more on policy-relevant concerns as opposed to irrelevant factors. For example, more politically-engaged or knowledgeable voters may be more likely to consider factors related to government performance and candidate quality. Further, characteristics of officeholders—such as proximity to the situation or electoral skill—may influence voter responses. Second, scholars can assess the social consequences of affective voting with respect to public policy. Our results focus mainly on individual judgment and decision making and only indirectly suggest an effect on policy outcomes.

In summary, these findings illustrate that important real-world decisions can be influenced by shifts in affect caused by events that are orthogonal to the decision at hand. Although such influences can be interpreted in a negative light, highlighting that the influences of mood can be disruptive, they also play positive roles. Theorists have found that emotions are adaptive (20, 21), facilitating evaluative judgments when affective reactions are caused by the object of evaluation (6, 22, 23) and promoting attentiveness and deliberation when one senses that a task is not going well (24). For example, political scientists have argued that emotions can promote more competent decision making and more deliberative reasoning (25–28). This research provides an initial look at how affect from irrelevant events influences important decisions with significant social and economic consequences. In doing so, it suggests that these generally adaptive tendencies to subconsciously use affect as information can lead to surprising and important outcomes.

**Materials and Methods**

**Study 1. Data.** We analyze data on voting behavior, college football outcomes, and county-level demographics for the counties or county-equivalent units that have Bowl Championship Series (BCS) teams in the United States. These 62 teams come from the six major Division I Football Bowl Subdivision (FBS) football conferences: the Atlantic Coast Conference, the Paciﬁc Ten, the Big Ten, the Big Twelve, the Big East, and the Southeastern Conference. The only team in our sample that does not play in a BCS conference is Notre Dame, an independent school with a rich football tradition (see Table S1 for additional information on the teams). We consider only the counties in

**Fig. 1.** Summary of effects of sporting outcomes on election results. (A) Study 1: effect of college football outcomes on incumbent party vote share. 1964–2008. (B) Study 2: effect of college basketball outcomes on presidential approval, 2009 NCAA tournament.
which the teams are located—in no case are there multiple counties associated with a team.

For the voting data, we consider presidential, senatorial, and gubernatorial election results at the county level from 1964 to 2008, as reported by Congressional Quarterly's Voting and Elections Collection. (The first year of the presidential election data is 1964, the first year of the gubernatorial election data is 1970, and the first year of the senatorial election data is 1974. We also collected data from the previous election cycle for all three race types for use as a lagged version of the dependent variable.) All uncontested races are excluded from the analysis. (We also excluded “jungle” primary elections in Louisiana, as well as special elections and elections in which the incumbent party is a third party.)

To cover the same time frame as the voting data, we collected college football results from 1964 to 2008 to construct our key independent variable in the voting regressions: the number of wins for the college football team in the county in the 2 wk preceding the election. (All of our college football data came from an online database run by James Howell. The dataset contains game scores for college football games between 1869 and 2008 and can be accessed at http://homepages.cae.wisc.edu/~dwilson/rsfc/history/howell. All bye weeks were dropped from the dataset; treating byes in the same manner as losses does not change the results substantively or statistically.) Losses and ties are treated the same. Data on games for the 5 wk before Election Day through 2 wk after the election were collected.

To improve the efficiency of our estimates, we control for a number of socioeconomic factors that are associated with voting behavior. Specifically, we include the following county-level demographic characteristics in our regression models: median household income, percentage of high school graduates (normalized for each year), percentage of African-Americans, a measure of how rural the county is (measured by farms per capita), and percentage of unemployed (29). For years where the data are not available in ref. 29, we obtain our data from the Census Bureau’s County and City Data Book. When data are unavailable for a given year, estimates are interpolated from the closest available years. Using data from the Census Bureau’s Small Area Income and Population Estimates program, we also control for county-level population. (The data can be accessed at http://www.census.gov/hhes/www/saipe/index.html.) We find that the means of the demographic variables are similar between counties that experienced wins and those that experienced losses.

Analysis. We first conduct a simple difference test to assess the impact of college football outcomes on incumbent vote share. In other words, we estimate the following difference-in-difference:

\[
\text{Impact}_i = (V_{it} \text{(win)} - V_{it-1} \text{(win)}) - (V_{it} \text{(loss)} - V_{it-1} \text{(loss)}).
\]

\[V_{it} = \eta_i + \beta_i W_{it} + \beta_1 P_{it} + \beta_2 G_{it} + \beta_3 H_{it} + \beta_4 W_{it} + \epsilon_{it},\]

where \(V_{it}\) represents the vote share of the incumbent party in percentage points in county \(i\) at time \(t\) after a win and loss at time \(t\), respectively. Similarly, \(V_{it-1} \text{(win)}\) and \(V_{it-1} \text{(loss)}\) represent incumbent vote share in county \(i\) at time \(t - 1\), the previous election cycle, in counties that experienced a win and loss at time \(t\), respectively.

In addition, we also estimated a fully specified regression model via ordinary least squares (OLS),

\[V_{it} = \eta_i + \beta_1 W_{it} + \beta_2 P_{it} + \beta_3 G_{it} + \beta_4 H_{it} + \gamma X_{it} + \epsilon_{it},\]

where \(X_{it}\) is a vector of demographic and economic control variables, \(\eta_i\) and \(\epsilon_{it}\) are county and year fixed effects, respectively, and \(\epsilon_{it}\) is random error. The inclusion of fixed effects controls for the possibility that places that tend to have stronger football programs may also have the tendency to support incumbents. Although it appears that college football outcomes are exogenous to the fixed effects, which are not necessary to obtain unbiased coefficients, the fixed effects ensure that we are isolating the effect that is isolated in the data. The fact that we have also isolated the random component of county-level population.

To determine whether our results are spurious, we conduct a series of additional tests. First, we perform a test of placebo effects, in which we ensure that games played after Election Day do not have any effect on the incumbent party’s vote share.

Second, we condition on the probability of victory before the game takes place—which can be estimated using point spreads from the betting market —to isolate the random component of game outcomes, which are by definition uncorrelated with omitted variables such as team strength. We collected data on point spreads extending back to 1985 from Covers.com that we used to estimate this probability. For example, if Ohio State is favored to beat a team by 20 points, the market is indicating that Ohio State is very likely to win the game. Academic statisticians (19) have developed a simple formula to translate point spreads (\(x\)) into victory probabilities (\(E(\text{win})\))

\[E(\text{win}) = \Phi(-x/13.85)\]

We use the estimated probabilities of victory to construct a variable that represents the deviation of actual wins in the two preelection games from the expected number of wins before the games occurred. This variable is a continuous variable that has support from \(-2\) (two losses when the team was almost certain to win both games) to \(+2\) (two wins when the team was almost certain to lose).

Study 2. Participants. Participants were members of Survey Sampling International’s Internet panel. Human subjects approval was granted by the Institutional Review Board of Stanford University on February 20, 2009 (Protocol 16116). Informed consent was received by all participants. The subject pool consisted of 3,040 residents of regions with college basketball teams that had made it to the third round. A “region” with a competing team was defined as a county that has a Sweet Sixteen team, along with its 10 nearest counties (as determined by county centroids) in the same state. The survey was conducted over the Internet between March 30, 2009, and April 3, 2009, immediately after the third and fourth rounds of the tournament (the Sweet Sixteen and Elite Eight games) and before the fifth round (the Final Four). The games took place between March 26 and March 29.

Question wording is provided in Table S10.

Procedures. Respondents were first asked to select their favorite team from a list of the 16 competing teams. If they selected “none of the above,” they were assigned a favorite team on the basis of their geographic location. Respondents were then asked “How supportive are you of team name?” and “How closely have you been following the NCAA college basketball tournament?” Respondents were also given the choice to answer with “No one or all” and “How close are you to the Final Four?” The survey was conducted over the Internet between March 30, 2009, and April 3, 2009, immediately after the third and fourth rounds of the tournament (the Sweet Sixteen and Elite Eight games) and before the fifth round (the Final Four). The games took place between March 26 and March 29. Full question wordings are provided in Table S10.

Results. We use the estimated probabilities of victory to construct a variable that represents the deviation of actual wins in the two preelection games from the expected number of wins before the games occurred. This variable is a continuous variable that has support from \(-2\) (two losses when the team was almost certain to win both games) to \(+2\) (two wins when the team was almost certain to lose).

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Study 2. Participants. Participants were members of Survey Sampling Interna-

(game outcomes and the mention of sports), making it hard to interpret the estimated treatment effect.

Analysis. To test the main effect of adjusted wins on presidential approval, we estimated the logistic regression equation

\[ A_i = \alpha + \beta_1 W_i + \gamma X_i + \epsilon_i, \]  

where \( A_i \) represents a dichotomous measure of presidential approval (approve, disapprove), \( W_i \) represents the number of wins experienced by the team subtracted by the expected number of wins as determined by the betting market, \( X_i \) represents a vector of demographic and political controls (gender, age, race, education, employment status, and party identification), and \( \epsilon_i \) represents random error. We used a similar formula to the one in Eq. 4 in study 1 to convert betting spreads to win probabilities, but with a SD of 10.9 following previous research on college basketball (30). To calculate effect sizes, we reestimated Eq. 5 using a linear probability model.

To test the effect of the experimental prime, we estimated the logistic regression equation

\[ A_i = \alpha + \beta_1 W_i + \beta_2 P_i + \beta_3 (W_i \times P_i) + \gamma X_i + \epsilon_i, \]  

where \( P_i \) is a dummy variable representing whether the respondent was assigned to the treatment group. The effect of the prime is represented by \( \beta_3 \). We can similarly estimate the moderating effect of being an intense fan (i) by substituting \( i \) for \( P_i \) in Eq. 6. Effect sizes were again estimated using linear probability models.

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