Depressive Realism and Health Risk Accuracy:
The Negative Consequences of Positive Mood

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We examine the role of level of depression on updating of health-related risk estimates. Participants provided their risk of getting breast cancer before (baseline) and after (follow-up) receiving personalized (experiment 1) or standard (experiment 2) medical risk feedback. Although there were no significant differences in risk estimates at baseline, the follow-up risk estimates indicate that compared to non-depressives, depressives lowered their risk estimates such that they were more accurate or closer to the medical estimates provided in the risk feedback. In contrast to depressives, nondepressives with higher baseline risk estimates did not revise their follow-up risk estimates because they were in a positive mood after receiving the risk feedback.

There is considerable research on the factors influencing risk perceptions (e.g., Folkes 1988; Luce and Kahn 1999; Menon, Raghubir, and Schwarz 1995; Payne, Bettman, and Johnson 1992; Raghubir and Menon 1998). These studies indicate that people have a positivity bias, tending to underestimate their vulnerability to negative events. Because perceptions of invulnerability may be unrealistic in general or detrimental to undertaking preventative health behavior in particular, a common goal of public service announcements (PSAs) has been to communicate accurate risk information for a particular behavior.

Several recent studies suggest that whether risk information will be rejected or integrated to update prior beliefs may be a function of audience personality (e.g., Cooper, Agocha, and Sheldon 2000; Gerrard et al. 2000; Zuckerman and Kuhlman 2000). The link between PSAs and personality is based on the premise that acknowledging health risks may involve threat not only to one’s physical welfare but also to one’s self-regard. Therefore, with some forms of health threat, particularly those associated with risk-increasing behaviors (e.g., smoking), reactions may, in part, be directed to bolstering self-regard by reducing perceived risk. Consistent with this view, people with high self-esteem (Gerrard et al. 2000; Schaninger 1976), those who are more alienated or deviant (Friedman 2000), or sensation seekers (Hoyle, Fejfar, and Miller 2000) feel less vulnerable and show greater resistance to health threats.

This study examines whether another personality trait, depression, would moderate reactions to PSAs. We were interested in whether depressives (the label used for people who are not inpatients but are subclinically or moderately depressed) view PSA information differently from those who are not depressed. Because depressives are characterized in general by a negativity bias, and more specifically, by their negative construction of reality and their negative view of themselves (Beck 1976; Beck et al. 1979), they may engage in more processing of negative information in PSAs than those who are not depressed. Because depressives are characterized in general by a negativity bias, and more specifically, by their negative construction of reality and their negative view of themselves (Beck 1976; Beck et al. 1979), they may engage in more processing of negative information in PSAs than those who are not depressed. In contrast, nondepressives may be less susceptible to such biases, albeit in a positive manner. Previous research indicates nondepressives typically believe they are at lower risk (Perloff and Fetzer 1986; Taylor and Brown 1988) and are more likely to predict socially desirable rather than realistic futures (Sherman 1980). This view would suggest that PSAs containing positive, self-enhancing (negative) information would result in more message processing among those who are nondepressives (depressives).

However, an interesting phenomenon known as “depressive realism” suggests this message strategy may be too simplistic. Studies on depressive realism indicate that depressives are less biased processors of message information than people who are not depressed (Alloy and Abramson 1979). In a PSA context, the depressive realism effect would be manifested in more updating of prior risk beliefs among depressives as compared to nondepressives in a manner that...
is consistent with the PSA, even if the PSA contains positive (lower) risk information.

Because depressives have several negative characteristics, the depressive realism phenomenon seems counterintuitive. The negativity bias of depressives has led researchers to question the generalizability of the depressive realism phenomenon. Previous support for the depressive realism effect is undermined because subjects are typically required to increase risk estimates in order to be more accurate (Coyne and Gotlib 1983). Because depressives are by definition more pessimistic, it is unclear whether depressives are more accurate information processors or just have higher risk perceptions (Coyne and Gotlib 1983). In addition, Pacini, Muir, and Epstein (1998) question whether the depressive realism effect is generalizable to a meaningful context. They find that the depressive realism effect is more likely to be observed in trivial situations but not in more meaningful, emotional, or realistic situations.

Our experiments were motivated by two goals. First, to test whether the depressive realism phenomenon would be obtained in response to a PSA where one was required to lower the risk estimate or to be more positive to be accurate. Such a demonstration would overcome the tautological reasoning that depressives only outperform nondepressives when being more pessimistic increases accuracy. Second, because there is no empirical evidence on why depressive realism occurs, we examine why depressives (nondepressives) are more (less) willing to incorporate experimental feedback in future risk estimates. Data from two experiments support the depressive realism phenomenon, whereas data from the second experiment identifies the process underlying the depressive realism phenomenon.

**OBJECTIONS TO DEPRESSIVE REALISM**

The term “depressive realism” is used to describe a phenomenon in which depressives perform better than nondepressives. Considerable research indicates that depressives are more balanced in their self-perceptions (Coyne and Gotlib 1983; Ruhelaman, West, and Pusahow 1985; Watson and Clark 1984). Unlike nondepressives, depressives tend to (1) recall positive and negative self-relevant information with equal frequency (Kuiper and Derry 1982; Kuiper and Mac-Donald 1982), (2) show greater evenhandedness in their attributions of responsibility for positive and negative outcomes (Coyne and Gotlib 1983), and (3) offer self-evaluations that coincide more closely with evaluations by objective observers (Brown 1986). To sum, compared to nondepressives, depressives appear more likely to process self-relevant information in a balanced manner.

Despite dozens of published experiments and several reviews on the depressive realism phenomenon, there is still a debate on whether depressive realism is “real” (see reviews in Ackermann and DeRubeis 1991; Ruhelaman et al. 1985; Taylor and Brown 1988; Weary and Edwards 1994). Opponents of the depressive realism phenomenon have three key objections. Their first objection is the absence of emotional involvement or realism in the experimental setting. The second objection is the lack of independent empirically derived criteria of accuracy or realism. The third objection is the confound between accuracy and a negativity bias such that more accurate is also more negative. Given our first goal of testing the generalizability of the depressive realism phenomenon in a PSA context, each of these objections and potential solutions are discussed next.

**Consequential and Emotional Setting.** The first objection focuses on the unrealistic setting used to test depressive realism. Support for depressive realism is weakened when the same paradigms used to support depressive realism are used in more realistic, consequential, or emotionally relevant ways (e.g., Dennard and Hokanson 1986; Pacini et al. 1998). A study by Pacini et al. (1998) indicates that depressives are more accurate in trivial (low incentive = 10c/trial) situations. In more consequential situations (high incentive = $2/trial), they are no more or less accurate than nondepressives. A review of depression studies suggests that the depressive realism phenomenon is typically observed when the experimental conditions do not mirror reality or do not engage the subject in an emotionally relevant manner (Ackermann and DeRubeis 1991; Dunning and Story 1991).

In our study, we picked a subject that would be very emotional and consequential to our sample. Specifically, women between the ages of 40 and 60 were asked about their risk for getting breast cancer. The subject of breast cancer is emotionally involving, consequential, and relevant for our sample. The perception of breast cancer risk has become a topic of increasing importance, partly as a result of increasing public awareness and interest in genetic testing for breast cancer susceptibility and as a psychological construct that affects screening behavior (Croyle and Lerman 1995). If the Pacini et al. (1998) premise that there is a negative relationship between depressive realism and an emotional/consequential setting is valid, we should not find evidence for depressive realism in our study.

Our sample choice of older women also overcomes some objections to whether the depressive realism phenomenon is generalizable to more depressed populations. Most of the depressive realism findings reviewed in this article were based on college samples classified as moderately (subclinically) depressed as measured by Beck’s Depression Inventory. Our study is similar to previous studies on depressive realism in that we did not use clinical or inpatient samples. However, because older women are known to be more depressed than younger or college-level men and women (Nolen-Hoeksema et al. 1995; Nolen-Hoeksema, Larson, and Grayson 1999), our study tests whether the depressive realism phenomenon is observed in a more depressed sample.

**Objective Standard.** The second objection against the depressive realism phenomenon is the absence of independent, empirically derived criteria of accuracy or realism (Dunning and Story 1991). Studies on the effect of depression are accused of naive realism or relying on the assumption that one pattern of responses (e.g., an average estimate) is accurate for all subjects, despite the fact that
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everyday knowledge and life history may be very different for depressives and nondepressives (Dobson and Franche 1989; Dunning and Story 1991). One approach to overcoming this problem is to use what actually transpired as the criteria for judging the accuracy with which one might predict risk (Dunning and Story 1991). However, this method may result in subjects striving to meet their predictions. For example, nondepressives might take more precautions when they predict that they are not in danger between school and home.

In the first experiment, accuracy was not judged through a comparison of an average risk estimate applied uniformly to all respondents. Rather, an individualized risk estimate was calculated for each participant based on an empirically based medical algorithm (Gail et al. 1989). The variables included are current age, number of first-degree relatives with breast cancer, age at first live birth, age at menarche, and number of benign breast biopsy specimens. Computer software was used to calculate breast cancer risk estimates to provide personalized risk feedback to each participant.

We also gathered risk estimates before (baseline risk estimate) and after (follow-up risk estimate) subjects received the risk feedback. Thus, instead of imposing judgments on whether subjects’ responses were more or less accurate, we assessed the accuracy of the risk feedback recall and the extent to which respondents use the risk feedback to estimate their follow-up risk. The depressive realism phenomenon would be supported if when, compared to nondepressives, depressives’ follow-up risk estimates were more similar to the risk feedback.

Accuracy and Negativity. The third objection leveled against work on depressive realism focuses on the overlap between accuracy and the negativity bias. Although depressives may appear more accurate in many of their judgments, it is unclear whether this accuracy is the result of unbiased information processing or a negativity bias (Coyne and Gotlib 1983). Thus, depressives can be made to look sadder but wiser without being so.

To separate accuracy from a negative-response bias, we chose a context in which a more optimistic outlook would result in greater accuracy. As has been the case with national averages, our samples’ breast cancer risk estimates at baseline were about nine times higher than their medical estimate. Uniformly high breast cancer risk estimates, even among nondepressives, have been attributed to high levels of media exposure on breast cancer (Croyie and Lerman 1995). Thus, an increase in accuracy would require subjects to lower their risk estimate for breast cancer. If the negativity bias explanation is valid, depressives should exhibit lower accuracy because they are not inclined to reduce their breast cancer risk estimates. In contrast, we would obtain support for the depressive realism phenomenon if depressives exhibit greater accuracy than nondepressives by lowering their baseline risk estimate in response to the risk feedback. Stated formally,

H1: Compared to nondepressives, depressives’ follow-

up risk estimates will be more consistent with the risk feedback.

EXPERIMENT 1

Method

Participants. Fifty-five participants, all women between 40 and 60 recruited from local newspaper advertisements, participated in a study on women’s health issues. No mention of depression was made in the advertisement. Seventy-one percent were white, and 29% were African-American. The average age of the women was 47.5 (SD = 5.25), and 72% had a college education or greater. The mean medical (Gail) score for the sample was 2.97% (SD = .98), and the range of Gail scores was 90% to 5.40%.

Procedure. The study was conducted in two phases. In phase 1, women interested in participating were told that the purpose of the study was to gain insight into women’s reactions to health education materials that personalized the risk of getting breast cancer. In order to do this, they needed to answer a series of questions. First, they provided information on their baseline risk estimate by answering the following question: “On a 0 = no chance to 100 = certain to happen scale, what do you think is your chance of getting breast cancer?”

Second, they answered a series of questions on demographics (e.g., age, ethnicity, education), additional background checks (e.g., mammography history, attitudes toward public breast cancer information and mammograms, how many women they knew who had breast cancer), and the components needed to calculate each subject’s Gail score (age, family history, age of menarche, number of benign breast biopsies, and age of first live birth).

Third, participants completed questions from the Center for Epidemiology Depression (CES-D) scale (Radloff 1977). The CES-D scale, originally developed for the National Institute of Mental Health studies, has been judged the best screening instrument for symptoms of depression in adults (Myers and Weissman 1980). It has high levels of reliability and validity to detect both clinical (by the criteria of the Diagnostic and Statistical Manual of Mental Disorders, 3d ed. rev. [DSM-III]), and nonclinical symptoms of depression for a wide range of study populations (Radloff 1977; Roberts and Vernon 1983). The scale consists of 20 items (e.g., I did not feel like eating, my appetite was poor), with four rating points (0 = none of the time, 1 = a little of the time, 2 = a moderate amount of the time, and 3 = most of the time). The range of CES-D scores for our sample (11–34 out of a possible range of 0–60) is consistent with other samples (Boyd et al. 1982). The mean score for the sample was 18.58, and the standard deviation (SD) was 5.06.

Although the CES-D scores allow us to examine the effects of depression as a continuous variable, it is important to note that, without exception, previous research on the depressive realism phenomenon examines the differences
between respondents who were categorized as depressive or nondepressives based on a split in the sample depression scores (Alloy and Abramson 1979). Accordingly, the phenomenon and related hypotheses are all in terms of presence or absence of depression rather than the effects of level of depression. One of the advantages of the CES-D scale is that it has a validated cut-off point for identifying nondepressives and depressives (Nezlek, Imbrie, and Shean 1994). The cutoff score of 16 has been validated with DSM-III criteria for depression (Eaton and Kessler 1981) and has been validated with several populations (Boyd et al. 1982). In this study, 21 participants had a CES-D score below 16, and 34 participants had a CES-D score above 16. The mean CES-D score for the nondepressive group was 13.86 (SD = 1.65), and the mean for the depressive group was 21.50 (SD = 4.17; F(1,53) = 64.09, p < .001). Neither the group on demographics (e.g., ethnicity) or background checks (e.g., mammametry history, F's < 1). Therefore, neither the group on demographics (e.g., ethnicity) or background checks (e.g., mammametry history, F's < 1). Neither the Gail score components (F's < 1) nor the Gail score components were different for the two groups (Mnondep = 3.14, SD = 1.01; Mdep = 2.85, SD = 1.09, F(1,53) = 1.15, NS). Thus, we were comfortable examining the effects of depression both as a dichotomous variable and as a continuous variable.

Risk Feedback. Phase 2 occurred a week or two after phase 1. At the start of phase 2, participants received a one-page message that described the Gail et al. (1989) model, components of the model, and a personalized medical (Gail) risk estimate of getting breast cancer.

Follow-Up Risk Estimate. The same scale used to assess baseline risk was used to obtain the follow-up risk estimate. Specifically, participants marked their risk: "On a 0 = no chance to 100 = certain to happen scale, what do you think is your chance of getting breast cancer?"

Risk Feedback Recall. Participants were asked to write their Gail score.

Results

Two separate analyses were deemed necessary to capture the repeated use of the risk measure as well as the continuous nature of the variables. To capture the repeated use of the risk measure, we use ANOVA with depression as a two-level between-subjects factor and baseline and follow-up risk estimates as a within-subject factor (labeled "risk feedback") to reflect the risk feedback effect. To capture the continuous nature of the variables, we use regression with depression and baseline risk as independent variables and follow-up risk as the dependent measure. Both analyses revealed two significant main effects and a significant interaction effect.

Analysis of variance revealed the risk feedback succeeded in reducing follow-up risk (Mnondep = 26.31, SD = 18.82 vs. Mfollow = 14.99, SD = 17.67; F(1,53) = 13.60, p < .001), and depressives had lower risk estimates than nondepressives (Mnondep = 25.80, SD = 21.15 vs. Mdep = 17.47, SD = 14.76; F(1,53) = 4.06, p < .05). There was also a significant depression by risk feedback interaction (F(1,53) = 6.88, p < .001). The first bar chart in figure 1 contains the means for baseline and follow-up risk estimates in the nondepressive and depressive groups.

Simple effects indicated that although there were no differences in risk estimates for depressives and nondepressives at baseline (F(1,53) = .07, NS), consistent with hypothesis 1, depressives had lower follow-up risk estimates than nondepressives in line with the risk feedback (F(1,53) = 11.50, p < .001). There was no effect of depression on risk feedback recall (Mrecall = 2.89, SD = 1.07 vs. Marxact = 2.97, SD = .98; F(1,53) = .79, NS).

The regression analyses indicated that the higher the baseline risk, the higher the follow-up risk (β = .58, t = 4.59, p < .001), and the higher the depression score, the lower the follow-up risk (β = -.27, t = -2.37, p < .05). The interaction between baseline risk and depression on follow-up risk was significant (β = -.34, t = -2.71, p < .01). The negative coefficient for the relationship between the interaction term and follow-up risk estimates indicates nondepressives with high baseline scores did not lower their follow-up risk estimates. In contrast, depressives with high baseline scores lowered their follow-up risk estimates in accord with the risk feedback. Thus, our results from both analyses support depressive realism.

Discussion

The aforementioned results indicate the depressive realism phenomenon is generalizable to a meaningful context in which depressives were required to be optimistic to be accurate. Specifically, depressives lowered their follow-up risk so that it was more in line with their risk feedback than did nondepressives. The results also indicate that this effect was not based on superior memory of depressives because risk feedback recall did not vary by level of depression. We first examine the implications of this null effect from the viewpoint of other findings on recall in the depression literature, followed by a review of potential alternative explanations for the depressive realism phenomenon.

Our null depression effect on risk feedback recall is in contrast to the view that depressives display memory deficits for positive information (Craighead, Hickey, and De-Monbreun 1977; Werner and Rehm 1975) and nondepressives display memory deficits for negative information (Nelson and Craighead 1977). However, studies by Kuiper and his colleagues indicate that depressives tend to recall positive and negative self-relevant information with equal frequency (Kuiper and Derry 1982; Kuiper and MacDonald 1982). Together, these studies suggest memory differences between nondepressives and depressives seem to be a function of superior memory for negative information among depressives and a null depression effect for positive information. This premise is consistent with our recall results because our risk feedback containing a low risk estimate was likely construed as positive information.
FIGURE 1
EFFECT OF DEPRESSION ON RISK ESTIMATES

**Experiment 1:**

![Bar graph for Experiment 1](image)

**Experiment 2:**

![Bar graph for Experiment 2](image)
To explain our results in experiment 1, we asked ourselves the following question: Is it possible that depressives may engage in more effortful processing than nondepressives, but this effect is not manifested in superior recall? The literature identifies four different explanations for why depressives may engage in more effortful processing than nondepressives. Specifically, depressives are: (1) more likely to blame themselves and believe others will blame them for failure to comply with the experimental task, (2) more likely to interpret the risk feedback as a rejection cue or as an indication that their baseline risk estimate was wrong, (3) less confident in their baseline risk estimates, and (4) less likely to be in a positive mood during the experiment that provides a signal to work harder. We briefly discuss these potential mediators as a first step toward identifying which might best explain depressive realism in our research setting.

**Attributions.** Theorists have long established that people make attributions to satisfy a need for predictability and control (Weiner 2000). Noting the positive relationship between depression and lower perceived control, several researchers have demonstrated that depressives make more use of, and are more sensitive to, attributional information (Pacini et al. 1998; Weary and Williams 1990). In particular, depressives deny responsibility for positive outcomes but see themselves and fear others see them as accountable for negative outcomes (Abramson, Seligman, and Teasdale 1978; Beck 1991). In contrast, nondepressives attribute failure more than success to lack and discount the validity of the test (Martin, Abramson, and Alloy 1984; Weary and Williams 1990). Because external attributions for failure may be viewed as an ego-protecting defensive strategy, these findings imply depressives may not have the same arsenal of defensive strategies as nondepressives. These studies suggest the depressive realism occurs because, compared to nondepressives, depressives have higher internal attributions for noncompliance and fear that others will evaluate them negatively if they do not comply.

**Rejection Cue.** The literature suggests that depressives are more affected by self-threatening feedback than nondepressives. For example, depressives show a decrease in self-esteem after failure, whereas nondepressives show no such change (Boney-McCoy, Gibbons, and Gerrard 1999). The depression literature also indicates that depressives are often high in rejection sensitivity and neuroticism, which increases the tendency to scan the environment in search of rejection cues (Ayduk et al. 2000) and increases responsiveness to threat and punishment cues (Cooper et al. 2000). These studies suggest depressive realism occurs because depressives interpret the risk feedback as a rejection cue (or feedback that their baseline risk estimates are wrong) rather than as positive information (a lower risk estimate).

**Confidence.** A vast literature on calibration and accuracy indicates that people tend to exhibit the "overconfidence bias," that is, they exaggerate the extent to which what they know is accurate (Lichtenstein and Fischhoff 1980). The depression literature indicates the overconfidence bias may be stronger among nondepressives (Abramson et al. 1978; Golin et al. 1979; Golin, Terrell, and Johnson 1977). Specifically, nondepressives have a tendency to believe they have more personal control than can be justified. For example, nondepressives believe they have greater control if they throw dice than if someone else does it for them (Fleming and Darley 1986). In contrast, depressives do not exhibit an overconfidence bias (Dunning et al. 1990), especially in tasks requiring performance expectations and performance evaluations (Abramson et al. 1978; Golin et al. 1977, 1979). These studies suggest depressive realism occurs because depressives are less confident in their baseline risk estimates.

**Mood.** A fourth possible explanation for the depressive realism effect in our context may be that nondepressives are in a more positive mood after receiving the risk feedback than depressives. The mood literature suggests that people use their mood-as-information to signal how much processing is required (Schwarz 1990). A negative mood signals that the environment is problematic and that extra processing is required, whereas a positive mood signals that the environment is OK, so extra processing is not important. If such extra processing were required for more accurate updating of the follow-up risk estimate, this would explain why depressives who feel less positive are more accurate than nondepressives.

Together, these studies suggest four alternative processes for the depressive realism effect. We undertook two steps to identify which mediator explains the depressive realism process best. First, we provide evidence for depressive realism by replicating the findings in experiment 1. Second, we test the value of each explanation by testing whether the scale representing each explanation is a full or partial mediator.

**EXPERIMENT 2**

**Method**

**Participants.** Seventy-four participants, all women, recruited from a local college, participated in the study. The advertisement asked women between the ages of 25 and 40 to participate in a study on women's health issues. We chose a younger sample than experiment 1 (but older than the typical college student sample) to further test the generalizability of the depressive realism phenomenon. Ninety-eight percent were white, and 2% were African-American. The average age of the women was 31.3 (SD = 5.33), and 74% had a college education or greater.

**Procedure.** The same procedure used in experiment 1 was followed in experiment 2 with the addition of the potential mediating measures in phase 2. Women interested in participating were told that the purpose of the study was to learn more about what women thought about their health risks. No mention of depression was made in the advertisement.
Participants provided their baseline risk estimates, background information, and completed questions from the CES-D scale (Radloff 1977) in phase 1. In this study, 40 participants had a CES-D score below 16, and 34 participants had a CES-D score above 16. The range of CES-D scores for our sample (1–34 out of a possible range of 0–60) are consistent with other samples (Boyd et al. 1982). The mean score for the sample was 11.97, and the SD was 7.89. The lower depression mean is consistent with the younger sample used in experiment 2 (Nolen-Hoeksema et al. 1999).

Risk Feedback. Phase 2 occurred three weeks after phase 1. At the start of phase 2, all participants received a typical PSA that described the factors that increase risk of getting breast cancer along with standard age-based risk estimates (fig. 2). We used a standard PSA because we believed the personalized risk feedback in experiment 1 had succeeded in overcoming previous criticism related to using averages for measuring accuracy. A standard PSA would also further test the generalizability of the depressive realism phenomenon.

Mood. Panas-Now (Watson and Clark 1984) was used to assess mood immediately after PSA exposure. Participants were asked to read each mood item and indicate the extent to which they felt a certain way at the present moment. Twenty adjectives (interested, distressed, excited, upset, strong, guilty, scared, hostile, enthusiastic, proud, irritable, alert, ashamed, inspired, nervous, determined, attentive, jittery, active, and afraid) were used to measure mood on a scale of 1 (very slightly or not at all), 2 (a little), 3 (moderately), 4 (quite a lot), and 5 (extremely). Factor analysis yielded two factors: positive and negative mood. The two scales were reliable ($\alpha_{pos} = .93$; $\alpha_{neg} = .82$).

Follow-Up Risk Estimate. The same scale used to assess baseline risk was used to obtain the follow-up risk estimate. Specifically, participants marked their risk: “On a 0 = no chance to 100 = certain to happen scale, what do you think is your chance of getting breast cancer at age 80?” The correct answer was 10%. Although the PSA contained a range of risk scores, we did not ask them to provide a risk estimate. Specifically, participants marked their risk: “On a 0 = no chance to 100 = certain to happen scale, what do you think is your chance of getting breast cancer at age 80?” (1 = not at all confident/certain/sure are you that your first estimate (phase 1) is correct?” (1 = not at all confident/certain/sure, 5 = completely confident/certain/sure). The three-item scale was reliable ($\alpha = .95$).

Rejection Cue. Two questions were used to measure how participants reacted to risk feedback that was likely to be inconsistent with their prior risk beliefs: “Because the risk information provided was different from what I thought, I felt I had failed” (1 = not at all to 7 = extremely) and “Because the risk information provided was different from what I thought, I found the average chance of getting breast cancer by age 80” (1 = not at all upsetting to 7 = very upsetting). Because these two items were highly correlated, they were combined ($\alpha = .85$).

Attributions. We modified items from Nowicki-Strickland Adult Internal-External Control Scale (Nowicki and Duke 1974) to reflect our context. Participants checked “yes” or “no” for the following items: I did not use the risk information because it was not relevant to me; I would think of myself as having failed if I did not revise my risk on breast cancer; I don’t think anyone knows breast cancer risk estimates; I didn’t use the risk information because most public health information is not very credible; I believe most health problems will solve themselves if you just don’t fool with them; I only have myself to blame if I do not do what was expected of me; I did not find the risk information believable; I was given the risk information so that I would know my risk for getting breast cancer better; I am willing to take a chance and not believe the risk information; I would blame myself if I did not get the right answer; and I believe I know my risk better than the experimenter ($\alpha = .71$).

For the “fear of negative evaluation scale” (modified from Watson and Friend 1969), participants rated as true or false the following statements: I used the risk information because I worry what others may think of me; what others thought of me did not influence the way I used the risk information; I was afraid people would find fault with me if I didn’t use the risk information; the disapproval of others would have little effect on whether I used the risk information; if someone is evaluating me, I tend to expect the worst; and I am usually confident that others will have a favorable impression of me even if I disagree with them ($\alpha = .79$).

Results

The same pattern of results as experiment 1 was obtained in experiment 2. Analysis of variance with depression as a two-level between-subjects factor and baseline and follow-up risk estimates as a within-subjects factor (labeled risk feedback as in experiment 1) revealed three significant effects. The risk feedback succeeded in reducing follow-up risk ($M_{base} = 40.63$, $SD = 18.69$ vs. $M_{follow} = 25.11$, $SD = 18.64$; $F(1, 70) = 72.85$, $p < .001$), and depressives had lower follow-up risk estimates than nondepressives ($M_{nondep} = 34.63$, $SD = 19.78$ vs. $M_{dep} = 28.87$, $SD = 11.68$; $F(1, 70) = 4.06$, $p < .05$). There was also a significant depression by risk feedback interaction ($F(1, 70) = 21.73$, $p < .001$). Simple effects indicated that although there were no differences in risk estimates for depressives and nondepressives at baseline ($F(1, 72) = 1.60$, $p > .21$), consistent with hypothesis 1 (fig. 1), depressives had lower follow-up risk estimates than nondepressives in line with the risk feedback ($F(1, 70) = 16.98$, $p < .001$). Furthermore, similar to experiment 1, there was no effect of depression on risk feedback recall ($M_{recall} = 12.89$, $SD = 1.07$ vs. $M_{act} = 10.00$, $SD = 0$; $F(1, 72) = 1.07$, NS).
FIGURE 2
BRoEST CANCER RISK PUBLIC SERVICE ANNOUNCEMENT

Simply being a woman and getting older puts you at some risk for breast cancer.

What factors can increase risk for breast cancer?

One or more of the following conditions place a woman at higher than average risk for breast cancer:

- Personal history of prior breast cancer.
- Mother, sister, daughter or two or more close relatives, such as cousins with a history of breast cancer.
- A diagnosis of a breast condition that may predispose a woman to breast cancer, or a history of two or more breast biopsies for benign breast disease.
- Women age 45 or older who have at least 75 percent dense tissue on a mammogram are at some increased risk.
- A slight increase in risk for breast cancer is associated with having a first birth at age 30 or older.

Not having any of the above factors does NOT mean that you are “safe.” The majority of women who develop breast cancer do not have a family history of the disease, nor do they fall into any other high-risk category.

For more information about mammograms contact the Cancer Information Service at 1-800-422-6237
The regression analyses indicated that the higher the baseline risk, the higher the follow-up risk ($\beta = .63, t = 5.81, p < .001$), and the higher the depression score, the lower the follow-up risk ($\beta = -.35, t = -3.31, p < .001$). The interaction between baseline risk and depression on follow-up risk was significant ($\beta = -.48, t = -3.42, p < .01$). The negative coefficient for the relationship between the interaction term and follow-up risk estimates indicates that nondepressives with high baseline scores did not lower their follow-up risk estimates. In contrast, depressives with high baseline scores lowered their follow-up risk estimates in accord with the risk feedback. Thus, our results for the two studies exhibit the same pattern in support of depressive realism.

To check whether the interactive effect of baseline risk (B) and depression (D) on follow-up risk estimates is mediated by the effect of a process variable or mediated moderation, Baron and Kenny (1986) recommend three regression equations: (1) $B \times D \rightarrow$ follow-up risk, (2) $B \times D \rightarrow$ mediator, (3) $B \times D$, mediator $\rightarrow$ follow-up risk. All the coefficients need to be significant except for the coefficient for the $B \times D$ interaction in the last equation (Baron and Kenny 1986). In this manner, we assessed whether scales for attributions, fear of negative evaluation, rejection cue, confidence, negative mood, and positive mood mediate the relationship between the depression by baseline risk interaction and follow-up risk.

Only positive mood met the Baron and Kenny (1986) criteria when all six mediators were entered simultaneously. Separate analyses on positive mood as the only mediator also indicated that it was a significant full mediator: (1) $B \times D \rightarrow$ follow-up risk, $\beta = -.24, p < .01$, (2) $B \times D \rightarrow$ positive mood, $\beta = -.27, p < .02$, and (3) $B \times D (\beta = -.14, p > .22)$, positive mood ($\beta = .40, p < .01$) $\rightarrow$ follow-up risk. The negative coefficient for the relationship between the interaction term and the mediator (regression eq. 2) indicates that a combination of lower (higher) levels of depression and higher (lower) baseline scores evoked more positive mood or that mood was less positive among participants who had similar levels (low or high) of depression and baseline scores. The positive sign of the coefficient between positive mood and follow-up risk (regression eq. 3) indicates that the higher the positive mood, the higher or less accurate the follow-up risk estimate. Together, these results indicate that whereas depressives felt less positive after the risk feedback when they had high baseline risk estimates, nondepressives with high baseline estimates felt more positive upon receiving the same risk feedback. Furthermore, feeling less positive than nondepressives after the risk feedback, depressives with high baseline risk estimates lowered their follow-up risk estimate.

**DISCUSSION**

Our findings provide support for the depressive realism phenomenon in a consequential, emotional context. In questioning conventional wisdom for the undermining effects of depression on accuracy, depressive women in our study significantly reduced their baseline risk estimates after a personalized medical estimate (experiment 1) or a standard average medical estimate (experiment 2) informed them that their baseline risk estimate was higher than their medical estimate. In addition, our findings indicate that depressives were able to integrate the medical risk feedback better than nondepressives, despite the lower-than-expected medical risk estimate. In order to be accurate, depressives had to be optimistic in our study. These data clarify previous studies in which it is unclear whether superior depressive accuracy is the result of unbiased information processing or the result of a higher negativity bias (Coyne and Gotlib 1983).

Our study also supports the depressive realism effect in the face of an objective standard to judge accuracy. In contrast to studies that use observers' evaluation or prior predictions as standards, we used a medical algorithm (Gail score) to judge accuracy. We believe this procedure is better than using observer evaluations that may not be objective because observers are subject to their own cognitive biases (Ackermann and DeRubeis 1991). Our method is also better than an average estimate, or subjects' forecasts, because subjects may have control over whether they can meet their predictions (Dunning and Story 1991). We demonstrate that depressives are more accurate than nondepressives when they are provided with individual standards for accuracy.

Our findings challenge other criticisms of the depressive realism effect. First, in contrast to the premise that depressives ignore experimental feedback (Ackermann and DeRubeis 1991), we show that nondepressives, not depressives, ignored the risk feedback. Second, our findings question whether depressives neglect base rate information because our medical risk algorithm could be perceived as similar to base rate information (Dunning and Story 1991).

We also provide empirical evidence on the process underlying the depressive realism phenomenon. Although previous studies have provided theoretical accounts of the underlying process or identify moderating variables, they have not tested for mediation (Hoyle et al. 2000). Our mediation findings in experiment 2 indicate that depressive realism is a function of less positive mood in response to the experimental stimuli among depressives, and a less positive mood may send a signal to engage in extra processing. In our study, depressives with high baseline risk estimates were in a less positive mood after the risk feedback and subsequently revised their follow-up risk estimate in accord with the risk feedback. In contrast, nondepressives with high baseline risk estimates reported a more positive mood after the risk feedback and maintained their (high) risk estimate during follow-up.

Our mood findings support and extend previous accounts for the depressive realism phenomenon. In particular, it has been assumed that depressives engage in more effortful processing than nondepressives to counteract their typical maladaptive processing stemming from low self-acceptance, superstitious thinking, and distrust of others (Pucinelli et al. 1998) or to compensate for a perceived lack of control over life events (Edwards and Weary 1993). Our findings support the
depressive realism occurs because depressives are less likely
called the risk feedback as well as depressives, they might
processing is not critical because the environment is OK
ference from the risk feedback was that their baseline risk
as rewarding. This effect would be even stronger if de-
warding is missing. In an experimental setting, depressives
their feelings as meaning that something positive or re-
state such as depression is combined with a negative tran-
mood state (Gasper and Clore 1998). According to
appraisal theorists, the key meaning underlying depression
is the loss or absence of a reward (Ortony, Clore, and Collins
As a result, depressives should be inclined to interpret
their follow-up risk estimate. According to this explanation,
and their view that disconfirmation sensitive and level of
depression may be negatively correlated require further
exploration.

Practical Implications
Research on systematic biases in general, and systematic
biases in risk perception in particular, appears to be based
on the premise that people are motivated to maintain a po-
itive self-concept. Our study suggests that this assumption
may not hold for samples with a higher incidence of de-
pression such as older women (up to 35%) and adolescents
(up to 16%; Berkow et al. 1997). Accordingly, researchers
interested in, say, smoking or AIDS risks among adolescents
or cancer among women may want to consider examining
this basic assumption (Cohn et al. 1995; Greening 1997).
Our data suggest health messages should consider per-
sonality as a segmentation variable. In particular, our find-
ings indicate that risk communication should be tailored to
nondepressives and depressives. Specifically, nondepressi-
ves need a message that will encourage them to question
what they know. Providing them with a credible source (a
medical formula) did not shake their self-confidence in this
study. The literature identifies two approaches to increase
risk estimates by making related behavior more accessible
in memory. First, ask subjects to focus on instances of
AIDS-related behaviors before judging their own risk level
(Raghubir and Menon 1998). Second, elicit behavioral fre-
quencies at the subcategory level rather than at the category
level if relevant information is not easily accessible in mem-
ory (Menon 1997). Given our need to reduce risk estimates,
this approach suggests priming nondepressives to reject ac-
cessible information by pointing out the dissimilarities or
irrelevance between new and old information or framing the
risk intervention message in a new category so that they
would have less access to information in an old category.
For example, a message on mammography could start out
by informing women that the national average risk for heart
disease is much lower than the actual incidence of heart
attacks among women.

Although it is important that practitioners concern them-
selves with the risk communication format for depressives,
our studies show nondepressives chose to ignore the risk
feedback regardless of whether we use a personalized letter
from the medical center or a standard PSA. One interpre-
tation of this finding may be that nondepressives or those
who are more prone to a positivity bias are less likely to
be persuaded by social advertising (Raghubir and Menon 1998). More generally, our findings highlight the importance of considering alternative risk communication formats for nondepressives.

Our aim was to reduce risk estimates for breast cancer in line with medical risk estimates. A lower risk estimate is often cited as deterring diagnostic screening (Rogers 1975). However, counter to intuition, high risk estimates can also reduce the likelihood to use diagnostic screening because women believe they would rather not know or tempt fate (Croyle and Lerman 1995). The high prevalence of false positive results for mammography has been linked to higher vulnerability, increased test stress, and reduced test credibility (Luee and Kahn 1999). Thus, more realistic breast cancer risk estimates would enable women to make more informed decisions about diagnostic screening.

Limitations and Future Research

There is evidence that depressives are equally likely to expect desirable outcomes to occur but are more likely to predict the occurrence of aversive outcomes (Dunning and Story 1991). Because we were interested in separating the negativity bias from accuracy, we did not include a pessimistic medical risk estimate (e.g., women typically underestimate their risk of heart attacks) as alternative risk feedback in our design. This would be an interesting area for future research.

Our study does not attempt to understand why women are depressed nor why women do not exhibit a positivity bias for breast cancer risk. It does attempt to show whether the depressive realism phenomenon was generalizable to adult women who were making risk assessments in a consequential setting. There is considerable documentation on why older women are depressed. Factors include lower social status and power, more work hours per week, caring for children and the elderly simultaneously, not feeling valued for who they are, or their roles in their partnerships and families (Nolen-Hoeksema et al. 1999). Furthermore, age is positively related to depression, especially among women. This view is consistent with our findings in the second study that uses a younger sample with a lower depression mean.

Why did women in our sample overestimate their breast cancer risk when most people underestimate health risk? The overestimation in baseline risk estimates is consistent with national average breast cancer risk estimates. We replicate this result in experiment 2 with a younger sample of women. Several reasons may explain why women have higher than realistic breast cancer estimates. First, there is considerable evidence that women feel more vulnerable than men (Nolen-Hoeksema et al. 1995). Second, higher perception of breast cancer risk has been related to the increasing importance of the topic, partly as a result of greater accessibility of information on breast cancer in the popular press, increased corporate sponsorship, and concerns about diagnostic screening behaviors (Croyle and Lerman 1995). Another reason for higher risk perceptions may be related to its greater physical visibility (as opposed to, say, heart disease or ovarian cancer).

Higher availability of breast cancer risk information may also be responsible for higher breast cancer risk estimates among nondepressives. Our data indicate a null effect of depression on baseline risk estimates. This finding may seem counterintuitive when one considers that (pessimistic) depressives typically have higher-than-average risk estimates and (optimistic) nondepressives have lower-than-average risk estimates. It is possible that greater availability of breast cancer risk information has caused nondepressives to increase their risk estimates in par with depressives. Although the antecedents of breast cancer risk or depression were not objectives in our study, these two issues are important topics for future research.

Support for depressive realism may be a function of our stimuli because in contrast to self-traits, individuals have less control over the incidence of breast cancer (although there seems to be a general belief that diet and stress-reducing activities lower breast cancer risk). This premise is consistent with the view that depressives are more accurate when future events are uncontrollable (Dunning and Story 1991). For example, Weinstein (1980) demonstrates that the positivity bias is weaker if the event is not under the person's control (e.g., breast cancer vs. lung cancer). The logic is that out-of-control events are not subject to same level of positivity bias because manifestation of these outcomes does not reflect poorly on one's self-image. Similar effects have been observed for product risk; the person is more willing to accept a higher risk estimate if failure can be attributed to someone else (Folkes 1988; Menon and Johar 1997; Weiner 2000). The effect of personality traits on perceived risk and control deserves more attention.

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