

## Context and the Interpretation of Likelihood Information: The Role of Intergroup Comparisons on Perceived Vulnerability

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Four experiments investigated how people's perceptions about a group's (e.g., women's) vulnerability to a disease are influenced by information about the prevalence of the disease in a comparable group (e.g., men). Participants read symptom and prevalence information about fictitious diseases before answering questions regarding target group vulnerability. Participants used the prevalence rate for a nontarget group as an immediate comparison standard for intuitively interpreting the degree of vulnerability of a target group, resulting in robust contrast effects. Experiments 3 and 4 illustrated that these contrast effects can cause a person's intuitive perceptions about a group's vulnerability to selected diseases to conflict with his or her knowledge of the prevalence rates for the diseases. The results support a distinction between 2 components of psychological uncertainty—beliefs in objective probability and more intuitive perceptions of certainty.

Uncertainty plays a key mediating role in decisions within many domains, including legal, political, health, business, and personal. The importance of the uncertainty construct is reflected in a variety of theories in social psychology, health psychology, and decision making. For example, the health belief model (Becker, 1974), theory of reasoned action (Ajzen & Fishbein, 1980), and many other expected-utility style models assume that when deciding what to do, people consider the likelihoods of various outcomes (i.e., uncertainty) and the anticipated values of those outcomes. Although uncertainty has been an invaluable construct in these and many other theories, the common treatment of uncertainty—as a unitary construct that can be adequately represented and measured as a numeric probability value—deserves closer examination (see, e.g., Gigerenzer, 1994; Howell & Burnett, 1978; Teigen, 1994, 2001; Teigen & Brun, 2000; Windschitl, 2000, 2002; Windschitl & Wells, 1996, 1998; Zimmer, 1983).

Recent research suggests that the construct of uncertainty can be fruitfully divided into two components—beliefs about the objective probability of an event and more intuitive perceptions about whether an event will occur (see, e.g., Windschitl & Weber, 1999; Windschitl & Wells, 1998; Windschitl & Young, 2001). According to this distinction, beliefs about objective probability are primarily determined by deliberative and rule-based information processing. That is, when attempting to determine the objective likelihood of an event, people intentionally use information that they deem to be relevant, and they attempt, when possible, to integrate that information according to their understanding of learned rules (e.g., mathematical rules, modes ponens; see Sloman,

1996, and Epstein, 1994, for further description of rule-based processing). Intuitive perceptions of certainty are influenced by a wider array of processes. Although these perceptions can sometimes reflect the influence of deliberative, rule-based processes, they are also influenced by more nondeliberative and associative processes that are not necessarily bounded by or consistent with a person's understanding of formal rules relevant to likelihood assessment.

Under many circumstances, intuitive perceptions of certainty and beliefs about the objective probability of an event are in agreement. However, recent investigations have revealed factors that can differentially affect the two components of uncertainty. For example, studies of the ratio-bias phenomenon demonstrate that people's intuitive optimism about winning a drawing can be influenced by a manipulation of the number of ways the drawing can be won, even if the manipulation does not have a similar influence on beliefs about objective probability (see Denes-Raj & Epstein, 1994; Kirkpatrick & Epstein, 1992; Pacini & Epstein, 1999). Participants in one study preferred to draw from a bowl containing 10 winning and 90 nonwinning beans versus a bowl containing 1 winning and 9 nonwinning beans even though they were aware that both bowls offered a 10% chance of winning (Kirkpatrick & Epstein, 1992). Another example comes from studies of the alternative-outcomes effect and the equiprobability effect. These studies illustrate that manipulations to the distribution of alternative outcomes can influence intuitive certainty regarding a focal outcome (as measured on nonnumeric certainty scales and related judgments) even when those same manipulations do not influence participants' beliefs about the numeric probability of the focal outcome (Teigen, 2001; Windschitl & Wells, 1998; Windschitl & Young, 2001). For example, participants in one study expressed less certainty that a chocolate-chip-loving girl would draw her preferred cookie when the relevant cookie jar was said to contain two chocolate chip cookies and seven oatmeal cookies than when the jar was said to contain two chocolate chip cookies, one oatmeal, one raisin, one butterscotch,

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one rum, one peanut butter, one pecan, and one sugar cookie (Windschitl & Wells, 1998).

In this article, we focus on a proposal that is closely related to the proposed distinction between beliefs about objective probability and intuitive perceptions of certainty. The proposal is inspired in part by Teigen and Brun's (2000) assertion that numeric probability forecasts can be ambiguous in an important way. When a communicator says that an event has a probability of .18, it is unclear whether this number expresses a possibility, a doubt, or neither. Verbal likelihood forecasts, on the other hand, are relatively unambiguous in this respect. For example, the term *small chance* is interpreted as expressing a possibility, albeit small, whereas the term *highly unlikely* is interpreted as expressing a doubt. We extend Teigen and Brun's (2000) line of reasoning and propose that a numeric probability—whether it is a communicated forecast, an internal belief regarding an event, or external information on which a belief is based—can be ambiguous from an intuitive perspective even though it is numerically precise. Imagine that a newspaper reader learns that women have a 12% chance of suffering from Disease X. This 12% estimate may determine the reader's belief about a woman's chances of getting the disease, but it does not necessarily carry with it an intuitive interpretation; it is not clear whether the estimate suggests optimism or pessimism. We propose that this intuitive assessment of the 12% depends substantially on what standard the estimate is compared against.

What comparison standards do people use to interpret numeric probabilities, and how do the standards influence judgments? Social judgment and social cognition research has addressed analogous questions about how people make judgments based on behavioral information about an individual. Such judgments require that people compare a given behavior with some standard or norm for judgment (see, e.g., Biernat, Manis, & Nelson, 1991; Fiske & Taylor, 1991; Higgins, 1996; Higgins & Lurie, 1983; Kahneman & Miller, 1986; Wyer & Srull, 1989). When a perceiver learns about a target person's behavior, the perceiver can compare the behavior with some standard based on stored knowledge, such as a category norm based on the distribution or central tendency for behaviors in a given setting or a postcomputed norm based on exemplars recruited for the judgment (see discussion by Kahneman & Miller, 1986). However, when immediate context information is salient, such as information about how another person behaved in the target's situation, a perceiver can use this context information as an immediate standard for judgment, placing less weight on the role of stored knowledge.

An apt example comes from research on the change-of-standard effect (Higgins & Lurie, 1983; see also Clark, Martin, & Henry, 1993; Higgins & Stangor, 1988). Participants in one study read about prison sentences rendered by several judges (Higgins & Lurie, 1983). In two conditions, the mean sentence length of the target judge, Judge Jones, was constant (5 years), but the mean sentence length of context judges varied between conditions (2.1 or 7.9 years). Higgins and Lurie found that Judge Jones was categorized differently in the two conditions—as harsh in the lenient context condition and as lenient in the harsh context condition. Moreover, they proposed and supported a dual-representation model for their effects. They concluded that a participant's contextually influenced categorization of a target stimulus, which constituted one representation, could affect future

recall, judgments, and behaviors independently of the participant's separately stored representation of the original target information.

Our reasoning regarding the interpretation of a numeric probability value, such as a 12% chance that women will acquire a specified disease, bears resemblance to the above-mentioned research on social judgment and social cognition. In the absence of context information, the primary standard of comparison for the 12% estimate could take many forms. People may compare this estimate with some sort of category norm (e.g., a norm based on knowledge about women's vulnerability to other diseases), with a highly accessible exemplar (e.g., a known vulnerability estimate for breast cancer), or with a highly salient yet arbitrary point on a probability scale (e.g., 10%). We suspect, however, that when people are also given immediate context information, such as information that men have a 20% chance of acquiring the same disease, this information becomes the primary comparison standard and results in a robust contrast effect. Analogous to how Higgins and Lurie (1983) proposed that a contextually influenced categorization of a target stimulus is distinct from the stored representation of the original target information, we propose that a contextually influenced representation of intuitive certainty is semi-independent from a belief about objective probability, which would not be influenced by context in the same manner. Furthermore, we hypothesize that contextually influenced representations of intuitive certainty do not play a role that is merely peripheral to that of beliefs in objective probability. Instead, intuitive representations can have an important mediating influence on decisions and judgments under uncertainty, even in situations in which a person's intuitive perceptions and beliefs in objective probability conflict.

We tested these novel hypotheses in four experiments, using an information-processing task that resembled what people do on a frequent basis when they see newspaper or other media stories about diseases or other health conditions. Participants read a set of information about diseases that included, for each disease, symptom information as well as prevalence rates for two specified social groups—a target group as well as a context group. Various types of dependent measures were used in the experiments to assess how the prevalence information about the context group influenced participants' intuitive perceptions of target-group vulnerability as well as participants' beliefs about the objective probability of disease for target-group members.

## Experiment 1

Experiment 1 was an initial test of whether prevalence rates for a context group would, in fact, lead to a detectable contrast effect for the perceived vulnerability of the target group. This hypothesis was tested twice within the same design; half of the key diseases tested whether prevalence rates for men would influence vulnerability ratings for women, and half tested whether prevalence rates for African Americans would influence vulnerability ratings for European Americans. For a disease of the former type, participants read that 12% of women had the disease and either 4% or 20% of men had the disease. We expected that participants' intuitive perceptions of women's likelihood of acquiring the disease would be higher if they learned that 4% rather than 20% of men had the disease, because the prevalence rate for men would serve as an immediate comparison standard for judging the 12% rate for

women. Participants' intuitive perceptions of women's likelihood of acquiring the disease were assessed with 7-point Likert-type vulnerability rating scales.<sup>1</sup> Similarly, we expected that the immediate comparison standards provided by the context information would influence decisions related to the key diseases. Hence, a diagnosis question in Experiment 1 forced participants to choose which of two diseases was the more likely diagnosis for a described woman named Anna. The two diseases were both described as having a 12% prevalence rate for women, but we expected that participants would exhibit a significant tendency to diagnose Anna with the disease that had a relatively low prevalence rate among men. Finally, we included numeric recall measures to test our assumption that memories for the target-group prevalence rates would not be significantly influenced by the context information.

## Method

**Participants and procedure.** The participants were 188 students (102 women and 86 men) from introductory psychology courses at the University of Iowa. Tested in groups of 2–10, participants first read a health information form that described four key diseases and two fillers. After that form was collected and participants had completed a filler questionnaire, they were given a questionnaire containing all of the dependent measures in the following order: diagnosis questions, perceived vulnerability questions, and numeric prevalence recall questions.

**The health information.** Instructions on the health information form indicated that participants would be reading about a set of diseases that they would be asked about later. The form described six diseases in the same order for all participants. Each description included numeric prevalence estimates for two social groups and information about symptoms (see Appendix). The disease information was fabricated to minimize unwanted influences of participants' prior knowledge on responses. We used unusual nicknames to increase the memorability of the names.

Two of the diseases, wine-breath disease (WBD) and Vitamin B12 aversion (VB12), were fillers. Two of the key diseases, stomach churn discomfort (SCD) and gluten bloat condition (GBC), had similar symptoms and were always described as having a 12% prevalence rate for women. The stated prevalence rates for men, however, were manipulated in a combined within- and between-subjects fashion. Specifically, one group of participants (Group A) read that 4% of men have SCD and 20% have GBC. Another group (Group B) read that 20% of men have SCD and 4% have GBC. The prevalence information read by Groups A and B is summarized in Table 1. The two other key diseases, body toxin condition (BTC) and blue-elbow disease (BED), also had similar symptoms and were always described as having a 1 out of 900 prevalence rate for European Americans. The stated prevalence rates for African Americans, however, were manip-

ulated in a manner analogous to how men's estimates were manipulated for SCD and GBC (see Table 1).<sup>2</sup>

**Diagnosis questions.** The first diagnosis question described a woman who was suffering from either SCD or GBC, and it forced participants to pick the more likely of those diagnoses.

Anna is a 36-year-old female who shows symptoms of both stomach churn discomfort (which involves an embarrassing allergic reaction that causes churning noises) and gluten bloat condition (which involves sensations of a painfully bloated stomach). Her doctor has determined that she must be suffering from one of these two conditions but not both. Based on the prevalence information that you encountered earlier, which condition is more likely to be the correct diagnosis for Anna?

The second question, which was similar in structure to the first, forced participants to indicate whether it was more likely that a described European American named Robert had BTC or BED.

**Perceived vulnerability questions.** The perceived vulnerability questions were worded in the following manner: "How vulnerable are Xs to Y?" The X refers to a social category (men, women, African Americans, European Americans), and the Y refers to a disease. The first set of questions asked about women's vulnerability to the three relevant diseases, the second set asked about men's vulnerability, the third set asked about European Americans' vulnerability to the three relevant diseases, and the fourth set asked about African Americans' vulnerability. The response scale for each question was a 7-point Likert-type scale anchored by *not very vulnerable* (1) and *highly vulnerable* (7). The scales also included a *don't know* option that participants were instructed to use only if they had no idea how to respond. "Don't know" responses were rare, and we omitted them from the data analyses reported below.

**Prevalence estimate recall questions.** The last dependent measures asked participants to recall the exact numeric prevalence estimates they had seen earlier for the various social groups.

## Results and Discussion

**The diagnoses.** According to a strictly objective analysis of the provided health information, the woman described in the first diagnosis question (Anna) had an equal likelihood of suffering from SCD and GBC (12%). However, as predicted, Anna tended to be diagnosed with the disease that was said to affect only 4% of men (which was SCD for participants in Group A but GBC for those in Group B). In Group A, 71.0% of the participants selected the SCD diagnosis, and 29.0% selected GBC. In Group B, only 43.6% of the participants selected SCD, and 56.4% selected GBC. A chi-square analysis indicated that the manipulation of prevalence rates for the context group caused a significant shift in the diagnoses for Anna,  $\chi^2(1, N = 187) = 13.20, p < .001$ . Similar

Table 1  
Summary of the Key Prevalence Rates Read by Participants in Groups A and B in Experiment 1

Disease and target (context) group	Group A	Group B
SCD among women (men)	12% (4%)	12% (20%)
GBC among women (men)	12% (20%)	12% (4%)
BTC among EA (AA)	1/900 (1/300,000)	1/900 (1/200)
BED among EA (AA)	1/900 (1/200)	1/900 (1/300,000)

**Note.** The values in the two bottom rows reflect relative frequencies (e.g., "1/900" can be read as 1 out of 900). EA = European Americans; AA = African Americans; SCD = stomach churn discomfort; GBC = gluten bloat condition; BTC = body toxin condition; BED = blue-elbow disease.

<sup>1</sup> Like a judgment of risk, a person's judgment of vulnerability may be a function not only of perceived likelihood but also of perceived severity. However, because none of our experiments manipulated symptom severity, we assumed that any differences in rated vulnerability reflected a change in the perceived likelihood rather than in the perceived severity of the disease.

<sup>2</sup> Readers might note that we did not use questionnaires in which men and African Americans were target groups. We decided to use women and European Americans as our target groups because we initially suspected that the contrast effects on the perceived vulnerability of a target group would be most pronounced (and easiest to detect) for participants who were members of the target group itself. Our participant pool consists of more women than men and an overwhelming majority of European Americans.

significant results were found for the diagnoses of the European American named Robert,  $\chi^2(1, N = 187) = 9.91, p < .01$ . In Group A, 63.4% of the participants selected BTC, and 36.6% selected BED. In Group B, only 39.4% selected BTC, and 60.6% selected BED. In summary, participants tended to diagnose Anna with the disease that was relatively rare for men, and they diagnosed Robert with the disease that was relatively rare for African Americans.

*Perceived vulnerability questions.* The critical question regarding the vulnerability ratings was whether manipulations of the prevalence rates for context groups affected vulnerability ratings for the target groups (see relevant means in Table 2).<sup>3</sup> As predicted, women's vulnerability to SCD was rated significantly lower by the participants who read that the rate of SCD among men was 20% (Group B) rather than 4% (Group A),  $t(172) = 3.04, p < .01$ . Similarly, women's vulnerability to GBC was rated lower by the participants who read that the rate of GBC among men was 20% (Group A) rather than 4% (Group B),  $t(163) = 2.10, p < .05$ .<sup>4</sup> In an analogous fashion, the vulnerability of European Americans to BTC was rated significantly lower by the participants who read that the rate of BTC among African Americans was 1 in 200 (Group B) rather than 1 in 300,000 (Group A),  $t(164) = 3.32, p = .001$ . The same type of effect was found for the perceived vulnerability of European Americans to BED,  $t(170) = 2.75, p < .01$ .

*Prevalence estimate recall questions.* Our earlier arguments suggest that the observed contrast effects had their impact on intuitive perceptions of certainty, whereas beliefs in objective probability were largely unaffected. We assume, for example, that learning that 12% of women and 4% of men were affected by a disease did not lead participants to quickly revise their objective belief for women upward to 18%. However, it is conceivable that, in the present study, the contrast effects had their impact through biased memory for objective probabilities. When answering the diagnosis and vulnerability measures, participants might have attempted to recall the exact numeric prevalence rates that they read earlier but unwittingly recalled rates that were biased by contrast effects. The results from the prevalence recall questions address this possibility.

Across all recall questions about the four key diseases, participants accurately recalled the exact prevalence rate 27.6% of the time. Also, as would be expected, recall of prevalence rates for the context groups was significantly affected by the manipulations of

those rates; Mann-Whitney tests of these effects were significant on all four key diseases ( $p < .01$ ). More critical is whether the context manipulations affected recall of the rates for the target groups. A Mann-Whitney analysis revealed that the recalled rates for women having SCD were significantly lower when participants had read that the SCD rate for men was 20% (Group B  $Mdn = 10\%$ ) rather than 4% (Group A  $Mdn = 12\%$ ),  $z(N = 184) = 2.82, p < .01$ . However, the recalled rates for women having GBC were not significantly affected by the same manipulation (both  $Mdns = 12\%$ ),  $z(N = 183) = 0.50, p > .05$ . Also, the recalled rates of BTC in European Americans were not significantly affected by the manipulation of rates for African Americans (both  $Mdns = 1$  in 900),  $z(N = 182) = 1.25, p > .05$ , and the recalled rates for BED in European Americans were not significantly affected by the same manipulation ( $Mdns = 1$  in 900),  $z(N = 184) = 1.19, p > .05$ .

*Internal analyses.* The recall data reveal that biases in recall were not the primary mediators of the contrast effects detected on the diagnoses and Likert-type vulnerability measures; for three of the four diseases (i.e., GBC, BTC, BED), recall estimates were not significantly influenced by the context manipulation. In the case of SCD, mediation tests following Baron and Kenny's (1986) procedures revealed significant partial mediation by prevalence-rate recall. We also conducted a set of internal analyses that provide compelling evidence that the context manipulations had a robust influence on perceptions of vulnerability even when recall estimates were accurate. In each of these internal analyses, we used data only from those participants who had accurately recalled the target group's prevalence rate for the disease in question. For example, our first analysis included only those participants who accurately recalled that the rate for SCD in women was 12%. For these participants, as was the case for the full sample, vulnerability ratings for women to SCD were significantly lower when the prevalence rate for SCD in men was said to be 20% (Group B:  $M = 3.53, SD = 1.36, n = 30$ ) rather than 4% (Group A:  $M = 4.22, SD = 1.40, n = 36$ ),  $t(64) = 2.02, p < .05$ . Analogous internal analyses for the ratings of target-group vulnerability to GBC and BED were also significant,  $t(42) = 2.88, p < .01$ , and  $t(67) = 6.19, p < .001$ , respectively. The same type of analysis for the BTC disease was not significant,  $t(63) = 0.87, p > .05$ . In summary, three of the four analyses indicate that the context information influenced the perceived vulnerability of a target group even when participants were aware of the prevalence rate for the target group.

Internal analyses on the diagnosis data suggest a similar conclusion, but the number of participants who met the relevant memory-accuracy criteria (on both diseases in a pair) was lower, making the conclusion more tentative. Of the 24 participants who

Table 2  
Mean Ratings of the Vulnerability of Target Groups in Experiment 1 as a Function of the Prevalence Rates Read by Participants (Group A or B)

Disease and target group	Group A		Group B	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
SCD for women	4.08	1.41	3.43	1.44
GBC for women	3.80	1.41	4.27	1.47
BTC for EA	4.14	1.64	3.28	1.71
BED for EA	3.32	1.80	4.07	1.78

*Note.* The vulnerability ratings were scored on a 1–7 scale. SCD = stomach churn discomfort; GBC = gluten bloat condition; BTC = body toxin condition; EA = European Americans; BED = blue-elbow disease.

<sup>3</sup> A precursory but less interesting question was whether vulnerability ratings for the context groups were affected by the manipulations. Between-subjects tests of these effects were significant on all four of the relevant dependent measures: the perceived vulnerability of men to SCD and to GBC as well as perceived vulnerability of African Americans to BTC and to BED (all  $p < .01$ ).

<sup>4</sup> We also conducted ANOVAs that treated the ratings for pairs of diseases (e.g., SCD and GBC or BTC and BED) as repeated measures. These analyses supported conclusions identical to those of the between-subjects *t* tests that we report in the *Results* section.

accurately recalled 12% for both SCD and GBC, 15 diagnosed Anna with the disease that was said to have a 4% prevalence rate for men. Of the 37 participants who accurately recalled 1 in 900 for both BTC and BED, 28 diagnosed Robert with the disease that had a 1 in 300,000 rate for African Americans.

*At what level do the comparison standards have their influence?* Our account of the contrast effects detected on vulnerability ratings suggests that the context information provided immediate comparison standards, which in turn influenced intuitive perceptions of certainty. However, classic work on social judgment and psychophysics indicates that although contrast effects can sometimes reflect changes in the perception, evaluation, or internal representation of a stimulus, they can instead reflect merely changes in the way response options are used or semantically interpreted (see, e.g., Campbell, Lewis, & Hunt, 1958; Krantz & Campbell, 1961; Manis, 1967; Manis & Armstrong, 1971; Simpson & Ostrom, 1976; Stevens, 1958; Upshaw, 1969).<sup>5</sup> Furthermore, in research on judgment standards in stereotyping, Biernat and her colleagues have argued and provided supporting evidence that response options on subjective measures such as Likert-type measures are particularly susceptible to being flexibly interpreted depending on the comparison standards that are used for judgment (see discussion in Biernat et al., 1991; see also Biernat & Manis, 1994; Biernat, Manis, & Kobryniewicz, 1997). Hence, they suggested that contrast effects on these measures are more likely to be caused by changes in response use rather than changes in internal representations.

Although we agree that Likert-type and other subjective measures can be susceptible to shifts in response-option use, the diagnosis questions from Experiment 1 provide some support for the assumption that the contrast effects on the vulnerability measures were not merely response based. Because the diagnosis measures asked respondents to pick which of two diseases was the more likely cause of the target person's symptoms, the issue of semantic interpretation and scale use is irrelevant. However, one could argue that the results of the diagnosis measure demonstrate only a minimal influence of context; the measure may have assessed only some form of a tie breaker between two diseases that had an equal objective probability. Given the importance of determining whether the context information prompted a change in some component of the internal representation of certainty rather than merely a change to the interpretation and use of response options, we designed Experiments 2 and 3 to specifically test our change-in-intuitive-perceptions hypothesis. In other words, we sought to investigate contrast effects under conditions in which the response-use interpretation could be completely ruled out.

## Experiment 2

Experiments 2 and 3 used different solutions to achieve this goal. In Experiment 2, female participants read symptom information about a disease and answered an initial question about how vulnerable they would feel to the disease on the basis of only that symptom information. Then they read prevalence information for the context group (men) and the target group (women) and indicated the extent to which this prevalence information might change their initial feelings of vulnerability. All participants read the same prevalence rates for women for a given disease (e.g., 22% get GBC). However, we predicted that the participants would have a

more relaxed reaction to prevalence rates (i.e., they would be more likely to indicate a reduced feeling of vulnerability) when the prevalence rate for men was said to be high (30%) versus low (5%). This type of contrast effect could not be plausibly explained by a response-use account because the personal change in perceived vulnerability scale explicitly specified a fixed comparison point (the participant's answer on the previous question), and the interpretation of the response options on that scale would not shift as a function of the context information.

## Method

The participants, tested in groups of 1–6, were 36 women who were students in introductory psychology courses at the University of Iowa. They were given a short questionnaire containing information and questions about four diseases. For each disease, participants first read a set of symptoms and indicated, on a 21-point scale, how vulnerable they would feel to that disease on the basis of the symptom information alone. Then they read prevalence information about the disease among men and women and answered a question about how this information might influence their perceptions of vulnerability. More specifically, this information and question were worded as follows: "Now imagine you read that X% of men and Y% of women get this disease. How would this additional information affect your feeling of vulnerability to the disease?" (–4 = *It would make me feel much less vulnerable*, 0 = *It would have no influence*, 4 = *It would make me feel much more vulnerable*). Table 3 contains summary information about the prevalence information read by participants. As is evident from that table, the prevalence rates for a disease in the target group (women) remained constant, but the rates for the disease in the context group (men) varied between Group A and B.

## Results and Discussion

The key issue was whether the manipulation of the prevalence rates for men influenced the way the participants (all women) reacted to prevalence rates for women. Table 4 contains the mean responses for the questions that asked participants whether a given set of prevalence information would make them feel less or more vulnerable to a disease. As expected, the participants tended to have more concerned reactions to the prevalence information (i.e., they were more likely to indicate it would make them feel more vulnerable) when the prevalence rate for men on a given disease was low than when it was high. This effect was robust and statistically significant for GBC,  $t(34) = 2.85$ ; WBD,  $t(34) = 3.49$ ; and BED,  $t(34) = 2.77$  ( $p$  values < .01). The effect was directional but not significant for VB12,  $t(34) = 1.53$ ,  $p = .14$ .<sup>6</sup>

Explaining these contrast effects observed on the change-in-vulnerability scales would be difficult using a classic response-use account. Such an account would need to assume that the participants' interpretations of the response options were influenced by an implied or anticipated experience of having to indicate how a

<sup>5</sup> To illustrate the general nature of the response-use interpretation, we note that a person rating the size of a horse might select the rating *quite large* in the context of small items like a VCR and a hat, but the person might interpret and use the rating *quite large* differently in the context of a dump truck and radio tower.

<sup>6</sup> We also conducted a repeated-measures ANOVA on these data, which supported conclusions identical to those based on the between-subjects  $t$  tests that we report in the *Results* section.

Table 3  
Summary of the Prevalence Rates Read by Participants in Groups A and B in Experiment 2

Disease and target (context) group	Group A	Group B
GBC among women (men)	22% (5%)	22% (30%)
WBD among women (men)	500 (12,000)	500 (60)
VB12 among women (men)	15% (29%)	15% (2%)
BED among women (men)	1,300 (90)	1,300 (18,500)

*Note.* The values for WBD and BED are frequencies per million (e.g., "500" can be read as 500 per million). GBC = gluten bloat condition; WBD = wine-breath disease; VB12 = Vitamin B12 aversion; BED = blue-elbow disease.

male participant's personal vulnerability would change as a function of the prevalence rates. That participants would anticipate this experience is possible, but it seems relatively implausible. Even if they did anticipate this experience, the response options themselves contain hard constraints that cannot be flexibly interpreted. Answers below 0 clearly indicate reduced concern relative to the response on the initial question, whereas answers above 0 clearly indicate increased concern. These characteristics of Experiment 2 suggest that the contrast effects are better explained by changes in perception or representation than by changes in the interpretation and use of response options. Additional evidence for this claim was sought in Experiment 3.

### Experiment 3

Experiment 3 used the same basic procedures and materials as Experiment 1, but it included two modifications that made it a strong test of the hypothesis that comparisons between the target and context groups can change internal representations about the perceived vulnerability of the target group. First, we slightly changed the prevalence information so that one of the key diseases had prevalence rates of 11% for women and 4% for men, whereas another disease had prevalence rates of 12% for women and 20% for men. An analogous change was made for the European American/African American diseases. Second, the main dependent measures, which we call the direct-comparison measures, required that participants indicate which of two key diseases the target group was more vulnerable to. For example, the SCD/GBC question asked whether and to what extent (on a 9-point scale) women were more vulnerable to SCD or GBC.

We expected that, if prevalence information about a context group can change the internal representation of the vulnerability of the target group, participants should, under some conditions (discussed more below), indicate that women were more vulnerable to the disease (either SCD or GBC) that affects 11% of women than to the disease that affects 12% of women. At an intuitive level, the perceived certainty of the 11% disease would be relatively high because the 11% rate would be contrasted against a 4% standard; the perceived certainty of the 12% disease would be relatively low because the 12% rate would be contrasted against a 20% standard. This predicted finding on the direct-comparison measures would constitute strong evidence that prevalence statistics for context groups produce contrast effects that go beyond mere response-use effects.

Although one goal of Experiment 3 was to test whether the contrast effects examined in this paradigm go beyond response-use effects, a second goal was to test our proposal that the influence that these contrast effects have on intuitive perceptions of certainty is independent of participants' beliefs about objective probability. If contrast effects operate on intuitive perceptions independently of beliefs about objective probability, participants would—when faced with the SCD/GBC direct-comparison question—hold two sets of internal representations that suggest conflicting responses. Their intuitive perceptions of certainty would indicate that women are more vulnerable to the 11% disease than to the 12% disease, whereas their beliefs in objective probability would indicate that women are slightly more vulnerable to the 12% disease. To test whether participants can simultaneously hold these contradictory sets of representations, we manipulated the conditions under which participants provided responses on the direct-comparison questions. Half of the participants were instructed to provide their intuitive gut reactions on these questions, whereas the other half were instructed to think carefully and to recall the earlier learned prevalence information when answering. To further encourage the former group to rely on their intuitive reactions, we put that group under time pressure when responding. We expected that the former group would indicate that women are more vulnerable to the 11% disease than to the 12% disease but that the latter group would indicate the reverse pattern.

### Method

*Participants and design.* The participants, tested in groups of 2–8, were 91 students (68 women and 23 men) from introductory psychology courses at the University of Iowa. The design was a Prevalence Information (Group A or B) × Instruction (gut or careful) factorial.

*Procedure.* As in Experiment 1, participants first read information about six diseases and completed a short, unrelated questionnaire at their own pace. To strengthen participants' encoding of the disease information, the experimenter told participants that they should reread the disease information after completing the unrelated questionnaire. When a participant finished rereading the disease information, he or she began working on another short, unrelated questionnaire and then a filler puzzle. When all participants in a given session had reached the filler puzzle, they were interrupted and given both written and oral instructions for the direct-

Table 4  
Participants' Ratings of How the Prevalence Information Would Affect Their Feeling of Vulnerability for Four Diseases in Experiment 2

Disease	Group A		Group B	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
GBC	1.16	1.83	-0.43	1.42
WBD	-1.47	1.74	0.47	1.59
VB12	-0.89	1.66	-0.06	1.60
BED	0.79	1.55	-0.53	1.28

*Note.* Responses were coded on a -4 to 4 scale. Negative values indicate that respondents (who were all women) thought that the prevalence rates would make them feel less vulnerable, and positive values indicate that the respondents thought the prevalence rates would make them feel more vulnerable. GBC = gluten bloat condition; WBD = wine-breath disease; VB12 = Vitamin B12 aversion; BED = blue-elbow disease.

comparison questions. The instructions for the *gut-instruction condition* stressed that participants should provide their intuitive or gut reactions to the questions. Part of those instruction were as follows:

Okay, as you read, for the next set of questions we want you to give us your quick first impressions. We don't want you to labor over each question. Instead you should read each question and immediately indicate your first impression by circling a response. You'll have a 2-min time limit to finish all of the questions.

The instructions for the *careful-instruction condition* stressed careful reflection before responding. Part of those instruction were as follows:

Okay, as you read, we are interested in how well you can remember the relevant information you read earlier and use it to answer the questions you are about to see. For each question, first try to recall the relevant numeric information that you read about earlier, and then decide on your response. Please try to work very carefully and take as much time as you need.

After receiving instructions, participants completed the 12 direct-comparison questions. In the gut-instruction condition, the experimenter conspicuously started a timer at the onset and informed participants at the end of a 2-min period (by which time most participants had already finished). Finally, all participants completed the prevalence recall questions at their own pace.

*The health information.* As in Experiment 1, the health information described four key diseases and two fillers. There were four relatively minor differences and one major difference between the sets of health information that were used in Experiments 1 and 3. First, the word *while* in the SCD description (see Appendix), which could be construed as an inherently comparative word, was replaced by the more neutral word *and*. Second, the disease name *body toxin condition* was changed to *sun toxin condition* to increase the ease with which this disease name could be associated with its description. Third, the order in which the diseases were listed and described was reversed for half of the participants. This counterbalance manipulation had no significant effects on the main results, and therefore is not discussed further. A fourth minor but more notable change concerned the type of frequency information described for the European and African Americans. Pilot testing suggested that a small portion of participants were finding the "1 out of X" expressions confusing, presumably because large numbers for X meant less rather than more vulnerability. To avoid this complication, we created a new set of prevalence estimates for these diseases and expressed them in the form of "Y per million."

The major change to the health information concerned the prevalence estimates quoted for the target groups (women and European Americans). In Experiment 1, the prevalence rates for the context groups—but not for the target groups—were manipulated. In Experiment 3, the prevalence rates for both the target and the context groups were manipulated in a linked fashion, making it possible that, with respect to key pairs of diseases (e.g., SCD and GBC), participants' intuitive perceptions of target-group vulnerability and their beliefs about objective probability might conflict. Table 5 provides a summary of all the key prevalence rates read by the participants.

*Direct-comparison questions.* The direct-comparison questions took the following form: "Are women more vulnerable to stomach churn discomfort or gluten bloat condition?" Respondents answered on a 9-point scale (4, 3, 2, 1, 0, 1, 2, 3, 4) with endpoint anchors of *much more vulnerable to stomach churn discomfort* and *much more vulnerable to gluten bloat condition*. The left/right position of the endpoint anchors and their ordering in the question stem were counterbalanced between subjects. Although there were 12 direct-comparison questions, only two were critical—the SCD/GBC question for women and the STC/BED question for European Americans. The remaining questions asked participants to com-

Table 5  
Summary of the Key Prevalence Rates Read by Participants in Groups A and B in Experiment 3 and Experiment 4

Disease and target (context) group	Group A	Group B
SCD among women (men)	11% (4%)	12% (20%)
GBC among women (men)	12% (20%)	11% (4%)
STC among EA (AA)	1,500 (4)	1,600 (18,000)
BED among EA (AA)	1,600 (18,000)	1,500 (4)

*Note.* The values in the two bottom rows reflect frequencies per million (e.g., "1,600" can be read as 1,600 per million). SCD = stomach churn discomfort; GBC = gluten bloat condition; STC = sun toxin condition; EA = European American; AA = African American; BED = blue-elbow disease.

pare a key disease with a filler and/or asked participants about the vulnerability of the context group.

For analyses of the SCD/GBC question, responses indicating greater vulnerability to SCD (GBC) were coded as negative (positive). For analyses of the STC/BED question, responses indicating greater vulnerability to STC (BED) were coded as negative (positive). Finally, the order of the questions was counterbalanced such that, for half of the participants, the critical SCD/GBC question and the critical STC/BED question appeared in the first and fourth position, respectively. The other participants saw the two questions in the fourth and first slot, respectively.

*Prevalence estimate recall questions.* The recall questions asked participants to recall the exact numeric prevalence estimates they had seen earlier for each of the various social groups.

## Results

*The direct-comparison questions.* The central issue for this experiment was how the prevalence-rate manipulations would influence the direct-comparison ratings regarding the target groups. The relevant means are displayed in Table 6.<sup>7</sup> We expected that the careful-instruction group would provide ratings that reflected sensitivity to objective prevalence rates but that the gut-instruction group would provide ratings that were shaped by contrastive comparisons with the context group (causing, e.g., women to be rated as more vulnerable to the 11% disease than to the 12% disease). In other words, we expected a Prevalence Information (Group A or B) × Instruction Type (gut or careful) crossover interaction with no main effects. To our surprise, however, an analysis of variance (ANOVA) on ratings of women's vulnerability to SCD versus GBC revealed a robust main effect for prevalence information,  $F(1, 87) = 38.37, p < .001$ , and this main effect was not qualified by a significant interaction,  $F(1, 87) = 2.91, p > .05$ . Simple-effects analyses revealed that not only did participants in the gut-instruction group exhibit significant contrast effects but participants in the careful-instructions group did as well,  $ps < .05$ . Participants in both groups tended to rate women as more vulnerable to the objectively less likely disease. The main effect for instruction was, as expected, not significant,  $F(1, 87) < 1.00$ .

<sup>7</sup> Regarding the precursory issue of how the manipulations affected ratings for context groups, we note that, as one would expect, participants tended to rate a given context group as more vulnerable to the disease that had a high rather than a low prevalence rate. These effects did not interact with the instructions manipulation ( $F_s < 1.00$ ).

Table 6  
*Mean Direct-Comparison Ratings Regarding Target Groups as a Function of the Prevalence Rates Read by Participants (Group A or B) and the Instructional Manipulation (Gut or Careful) in Experiment 3*

Target group and instructions	Group A		Group B	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Women				
Gut	-1.17	2.58	1.09	2.76
Careful	-1.57	2.43	2.41	1.65
European Americans				
Gut	-1.58	3.01	0.45	3.29
Careful	-1.61	3.03	0.73	2.99

*Note.* The values in the top two rows are based on the question of whether women are more vulnerable to stomach churn discomfort (SCD) or gluten bloat condition (GBC). These responses were coded on a -4 to 4 scale such that negative values reflect greater vulnerability to SCD and positive values reflect greater vulnerability to GBC. The values in the bottom two rows are based on the question of whether European Americans are more vulnerable to sun toxin condition (STC) or blue-elbow disease (BED). These responses were coded on a -4 to 4 scale such that negative values reflect greater vulnerability to STC and positive values reflect greater vulnerability to BED.

Likewise, the ANOVA on ratings of European Americans' vulnerability to STC versus BED revealed a robust main effect for prevalence information,  $F(1, 87) = 11.46, p = .001$ , and this main effect was not qualified by a significant interaction,  $F(1, 87) < 1.00$ . Simple-effects analyses revealed that participants in both the gut-instruction and the careful-instruction groups exhibited significant contrast effects,  $ps < .05$ . Participants in both groups tended to rate European Americans as more vulnerable to the objectively less likely disease. The main effect for instruction was, as expected, not significant,  $F(1, 87) < 1.00$ .

*Prevalence estimate recall questions.* Across all responses to recall questions about the four key diseases, participants accurately recalled the exact prevalence estimate 31.7% of the time. We conducted Mann-Whitney tests to assess the effect of the prevalence-information manipulation (Group A vs. Group B) on the recall of the prevalence estimates for the target group of the four key diseases. None of the four tests revealed a significant effect on the recall estimates (all  $ps > .05$ ).

## Discussion

The results of Experiment 3 provide clear support for our proposal that the prevalence rates for a context group can produce contrast effects that go beyond mere response-use or semantic effects. The response-use explanations that might apply to contrast effects on Likert-type measures cannot account for the effects detected on the direct-comparison questions used in this experiment. The influence that the contrast processes had on perceptions of vulnerability worked against the direction of the objective prevalence information—causing participants to indicate that women have a greater vulnerability to a disease with an 11% prevalence rate than to a disease with a 12% rate. This type of response pattern was observed not only for participants who were under time pressure and instructed to provide their intuitive re-

sponses but also for those who were instructed to think carefully and to recall relevant information before responding. This finding for the careful-instruction group was unexpected, but it nevertheless speaks to the robust influence that the objectively irrelevant context information can have on judgments about vulnerability.

The contrast effects observed on the direct-comparison measures were not mediated by recall estimates. As a requisite for mediation, the manipulation of prevalence rates would need to have a significant influence on recall estimates (see Baron & Kenny, 1986), but none of the recall estimates for target groups were significantly influenced by the manipulation.<sup>8</sup> It is also the case that participants' direct-comparison ratings and recalled prevalence rates tended not to be strongly related. To illustrate, we computed recall difference scores, which, for the SCD/GBC pair, reflected the extent to which participants' recall estimates were higher for GBC than for SCD. The correlation between these difference scores and direct comparison responses for the SCD/GBC question (coded in a similar direction) was not significant for the gut-instruction group ( $r = .19, p > .05$ ); the correlation was significant but only moderate in size for the careful-instruction group ( $r = .39, p < .01$ ). The analogous correlations for the STC and BED diseases were not significant for the gut-instruction ( $r = .13, p > .05$ ) or the careful-instruction group ( $r = .06, p > .05$ ).<sup>9</sup>

These findings regarding the gut-instruction group provide, as expected, evidence of a partial dissociation between the responses on the direct-comparison and recall questions. However, the unexpected findings from the careful-instruction group raise the following question: Why would participants who were instructed to think carefully about prevalence information fail to provide direct-comparison answers that were more closely related to the relevant prevalence rates that they recalled from memory? One possibility is that participants essentially missed or ignored the instructions to think carefully about prevalence information before providing the direct-comparison responses. We think this is unlikely given the clarity of the instructions and the fact that they were delivered both on paper and orally. Instead, we suspect that participants did attempt to think carefully about prevalence information, but when their thoughts about the information lacked clarity or salience or when participants were not sufficiently confident about the numeric prevalence estimate, they relied on their more intuitive perceptions of certainty when responding on the direct-comparison questions. Even participants who provided completely accurate estimates on recall questions may have lacked complete confidence in those estimates. For example, even if a given participant's best recollection was that 11% of women get SCD and 12% get GBC, that participant may have had little confidence in these numeric beliefs and instead rated women as more vulnerable to SCD than GBC because of a rather salient impression that was initially shaped by contrastive comparisons with disease rates for men.

<sup>8</sup> In addition, the differences between recalled estimates for key pairs of diseases (e.g., the difference in rates recalled for SCD and GBC) did not vary significantly as a function of the prevalence-rate manipulation, which means these differences in recall cannot be considered a mediator of responses on the direct-comparison questions.

<sup>9</sup> Spearman correlation analyses produced identical conclusions.



### Experiment 4

If this speculation regarding the results from the careful-instruction group is valid, then we would expect that the results from a careful-instruction group in a subsequent experiment could change dramatically if the encoding of the numeric prevalence information was enhanced. That is, if the knowledge of the numeric prevalence information was highly accessible and held with good confidence because of strong encoding conditions, then participants motivated to use that information on direct comparison questions could easily do so (even if the more intuitive perceptions of certainty conflicted with this knowledge). With this in mind, we conducted another experiment that was identical to Experiment 3, with the exception that all participants first learned the numeric prevalence information to a recall-accuracy criterion of nearly 100% before completing the filler task and the direct-comparison questions. As we had originally expected for Experiment 3, we predicted that the direct-comparison ratings of participants in the careful-instruction and gut-instruction groups would be in complete disagreement; the ratings of the former group would reflect beliefs in objective vulnerability, whereas the ratings of the latter group would reflect intuitive perceptions shaped by contrastive comparisons.

### Method

**Participants and design.** The participants, tested in groups of 2–8, were 81 students (53 women and 28 men) from introductory psychology courses at the University of Iowa. The design of the experiment was identical to that of Experiment 3.

**Materials and procedures.** The materials and procedures were identical to those of Experiment 3, with the following exception. After participants had finished reading the health information for the first time, the experimenter informed them that they would later be asked about the prevalence information. The experimenter then indicated that they should reread the health information, with special attention to the prevalence rates. After a 3-min time period, the participants were given a recall test, nearly identical to the test that was used in Experiment 3. On completion, participants were told to score their own test and to learn from their errors to fill in their memory with the correct information. After 2 min, the participants completed a second recall test. They were then told to score their own test and to be sure they learned all of the correct information. After 2 min, the experiment continued as it did in Experiment 3.

### Results

**The direct-comparison questions.** The central issue was whether the manipulations of prevalence rates and instructions would have an interactive influence on the direct-comparison ratings regarding the target groups (see means in Table 7). As expected, the ANOVA on ratings for the SCD/GBC question revealed a nonsignificant main effect for instructions,  $F(1, 77) < 1.00$ , and a nonsignificant main effect for prevalence information,  $F(1, 77) = 3.26, p > .05$ , but a significant interaction,  $F(1, 77) = 5.30, p < .05$ . Simple-effects analyses for the gut-instruction groups revealed an ordering of means that was in the predicted direction (Group A < Group B), but the difference was not significant,  $t(41) = 0.87, p > .05$ . The simple effect between the careful-instruction groups was significant in the opposite (but predicted) direction,  $t(36) = 3.14, p < .01$ . Participants in Group A rated women as relatively more vulnerable to GBC, whereas

Table 7

*Mean Direct-Comparison Ratings Regarding Target Groups as a Function of the Prevalence Rates Read by Participants (Group A or B) and the Instructional Manipulation (Gut or Careful) in Experiment 4*

Target group and instructions	Group A		Group B	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Women				
Gut	−1.30	2.77	−0.50	3.32
Careful	1.00	1.72	−0.78	1.77
European Americans				
Gut	−2.57	2.21	0.70	3.08
Careful	0.75	2.20	−1.61	1.79

*Note.* The values in the top two rows are based on the question of whether women are more vulnerable to stomach churn discomfort (SCD) or gluten bloat condition (GBC). These responses were coded on a −4 to 4 scale such that negative values reflect greater vulnerability to SCD and positive values reflect greater vulnerability to GBC. The values in the bottom two rows are based on the question of whether European Americans are more vulnerable to sun toxin condition (STC) or blue-elbow disease (BED). These responses were coded on a −4 to 4 scale such that negative values reflect greater vulnerability to STC and positive values reflect greater vulnerability to BED.

those in Group B rated women as relatively more vulnerable to SCD. In other words, participants in the careful-instruction groups tended to rate women as more vulnerable to whatever disease was described as the 12% disease.

Similar results were found regarding the STC/BED ratings. As expected, an ANOVA on these ratings revealed a nonsignificant main effect for instructions,  $F(1, 77) < 1.00$ , a nonsignificant main effect for prevalence information,  $F(1, 77) < 1.00$ , and a robust interaction,  $F(1, 77) = 28.23, p < .001$ . A simple-effects analysis for the gut-instruction groups revealed that the ratings from participants in Group A were, as predicted, significantly lower than those from Group B,  $t(41) = 4.03, p < .001$ . The simple-effects analyses for the careful-instruction groups revealed a significant effect in the opposite (but predicted) direction,  $t(36) = 3.61, p < .01$ . In sum, participants in the gut-instruction groups tended to rate European Americans as more vulnerable to whatever disease was described as the 1,500 disease, whereas participants in the careful-instruction groups tended to rate European Americans as more vulnerable to whatever disease was described as the 1,600 disease.

**Prevalence-rate recall questions.** We inserted the first two recall tests prior to the direct-comparison questions for participants to discover holes in their knowledge of the numeric prevalence rates. This strategy appears to have worked well; across the four key diseases on the third recall test, recall accuracy was 88.6%. Mann–Whitney tests revealed that the recalled prevalence rates for SCD and GBC among women were significantly higher when the relevant prevalence rates read for women were 12% than when they were 11%. Analogous statistically significant results were found for the recalled prevalence rates of STC and BED.

### Discussion

Consistent with our predictions, the direct-comparison data from Experiment 4 exhibit significant interactions between the type of

instructions participants were given and the prevalence-rate manipulations. Participants who were instructed to provide their gut-level impressions tended to indicate that the target group was more vulnerable to the objectively less likely disease,<sup>10</sup> whereas participants who were instructed to provide carefully considered assessments indicated that the target group was more vulnerable to the objectively more likely disease. These results support our proposal that participants held dual representations that suggested conflicting responses to the direct-comparison questions.

### General Discussion

Whereas psychological uncertainty has been commonly conceptualized and measured as a unitary construct, the present studies add to a growing body of evidence indicating that the construct of uncertainty can be fruitfully divided into two components—beliefs about the objective probability of an event and more intuitive perceptions about whether an event will occur (see, e.g., Windschitl & Weber, 1999; Windschitl & Wells, 1998; Windschitl & Young, 2001). In this work, we proposed that a numeric probability, despite its precision for shaping or defining beliefs about objective probability, can be flexibly interpreted from an intuitive perspective. We argued that context can play a critical role in shaping intuitive perceptions of certainty by providing an immediate comparison standard by which numeric probability information can be judged. Experiment 1 demonstrates that an objectively irrelevant prevalence rate for a context group can serve as an immediate comparison standard and produce robust contrast effects on judgments of a target group's vulnerability to a disease. Experiments 2 and 3 establish that the influence of context could be attributed to a change in the cognitive representation of the vulnerability of the target group, not merely a change in the way response options on a subjective scale are interpreted or used. Experiment 4 provides evidence for a dual-representation proposal, showing that participants' intuitive perceptions of certainty—shaped by contrast effects—and beliefs about objective probability could simultaneously suggest conflicting responses regarding which of two diseases is more likely.

To what extent is variation in intuitive perceptions important? In other words, how useful is it to conceptualize and measure intuitive perceptions of certainty separately from beliefs about objective probability? Our answer to this question has three components. First, variability in intuitive perceptions of certainty is systematically related to factors that vary in real-world settings. It is not the case that differences between intuitive perceptions and beliefs about the objective probability of an event reflect merely random variability or merely differences related to respondent characteristics. The present findings regarding the contrastive influence of context information add to a list of findings demonstrating that various factors—such as additivity constraints (Windschitl, 2000), distributions of alternative outcomes (Windschitl & Wells, 1998), ratio biases (Kirkpatrick & Epstein, 1992), and associative/schematic influences (Windschitl & Weber, 1999)—can have differential effects on intuitive perceptions of certainty and beliefs about objective probability.

Second, intuitive perceptions of certainty play an important mediational role in decisions (e.g., Kirkpatrick & Epstein, 1992; Windschitl & Wells, 1996, 1998), and this role is not redundant with that of beliefs about objective probability. In our Experi-

ment 1, the context information not only influenced intuitive perceptions of vulnerability but also caused a significant shift in diagnosis decisions. Although Experiment 3 used judgments instead of decisions as dependent measures, its results provide a quite compelling case for the consequential impact of intuitive perceptions of certainty. The context manipulation caused participants in both the gut- and the careful-instruction groups to indicate that an objectively less likely disease was more threatening to the target group than was an objectively more likely disease, even though recall of the target-group prevalence rates was unaffected by the manipulation. If the contrast effect operating on intuitive perceptions can cause participants in a careful-instruction group to respond in this manner to a question that explicitly pits the two diseases against each other, it seems reasonable to assume that the contrast effect would have a mediating influence on a variety of consequential decisions related to these diseases (e.g., diagnoses, whether to get screened).

Third, it is notable that our distinction between intuitive perceptions and beliefs about objective probability shares commonalities with other theories in social and cognitive psychology that posit that implicit associations and nondeliberative processes—as distinct from explicit beliefs and controlled processes—play a central role in many important decisions and behaviors (e.g., Chaiken & Trope, 1999; Devine, 1989; Epstein, 1990, 1994; Fazio, 1990; Greenwald & Banaji, 1995; Slovic, 1996; Smith & DeCoster, 2000; Trope & Gaunt, 1999). Many of these theoretical approaches, accompanied by empirical work, have yielded novel insights by exploring in detail the more implicit and nondeliberative types of processes. For example, researchers have learned a great deal about how implicit associations and stereotypes influence judgment and behavior under time pressure or lack of deliberative control (see, e.g., Devine, 1989; Fazio & Towles-Schwen, 1999; Greenwald, McGhee, & Schwartz, 1998). Given the recent advances achieved by studying the more intuitive sides of many of the dichotomies cited above, we think there is reason to assume that there is important potential in studying the more intuitive half of the key dichotomy that this article discusses.

### *Intuitive Perceptions and Risk as Feelings*

Our work and arguments are related to but distinct from notions that perceptions of risk are not simply reducible to anticipated outcomes and their presumed likelihoods (see, e.g., Fischhoff, Lichtenstein, Slovic, Derby, & Keeney, 1981; Loewenstein, Weber, Hsee, & Welch, 2001; Peters & Slovic, 1996, 2000). The recently proposed risk-as-feelings hypothesis, for example, suggests that behaviors are mediated not only by cognitive evaluations of what outcomes might occur and how likely they are to occur but also by affect-related factors (e.g., time between behavior and outcomes, hazard observability) that do not necessarily bear on those consequentialist evaluations (see Loewenstein et al., 2001). Like the risk-as-feelings hypothesis, our work suggests that traditional expected-utility type models are too narrowly defined. The risk-as-feelings hypothesis focuses on how affect influences be-

<sup>10</sup> As reported in the *Results* section, this simple effect was highly reliable for the STC/BED question ( $p < .001$ ); it was directional but not significant for the SCD/GBC question.

havior through conceptual pathways that are completely outside of pathways comprising the consequentialist evaluations (i.e., through pathways not influencing expected-utility evaluations). Our work focuses on components within this consequentialist evaluation. We assume that both intuitive perceptions of certainty and belief about objective probability need to be appreciated as possible mediators of many types of expected-utility evaluations.

### *Comparison Standards and Social Comparisons*

Although the context information in the present experiment consistently influenced perceptions of target-group vulnerability in the predicted direction of contrast, it is notable that context information could, under some circumstances, influence the interpretation of probability information in the direction of assimilation. We do not review the ever-growing literature on the multiple factors that can determine the direction of context effects (see, e.g., Bless & Wänke, 2000; Eiser, 1990; Herr, 1986; Lombardi, Higgins, & Bargh, 1987; Manis & Paskewitz, 1984; Manis, Biernat, & Nelson, 1991; L. L. Martin, 1986; Mussweiler & Strack, 2000; Stapel & Winkielman, 1998), but we briefly speculate on what might have been some key factors leading to contrast rather than to assimilation in the present studies. As discussed earlier, a basic assumption in psychophysics and social judgment research is that judging a stimulus requires a comparison standard (see, e.g., Brown, 1953; Biernat et al., 1991; Helson, 1964; Higgins & Lurie, 1983; Kahneman & Miller, 1986; Poulton, 1989; Sherif & Hovland, 1961; Wyer & Srull, 1989). Previous research suggests that when a comparison standard provided by immediate context is an exemplar (not an indistinct concept) that is sufficiently extreme and viewed as distinct and relevant to the judgment, it often results in contrast effects (e.g., Brown, 1953; Helson, 1964; Herr, 1986; Mussweiler & Strack, 2000; Stapel & Winkielman, 1998; Stapel, Koomen, & Van der Pligt, 1997). We think these characteristics—exemplar, distinct, extreme, relevant—apply to the context information used in our experiments. The 4% estimate for men provided respondents with an exemplar that was, by designation, distinct from the rate for women; context that simply primes an indistinct concept like *rare* or *low prevalence* may be more likely to lead to assimilation (see Windschitl & Weber, 1999, for a related finding involving assimilation). It is difficult to determine what numeric prevalence values would qualify as extreme in the present paradigm, but the difference between 12% and 4% does seem extreme enough to be notable to a perceiver. Finally, the fact that the 4% value informs respondents about the prevalence rates for a major social group is likely to give it some assumed relevance for judging the 12% value.

Additional research is required to determine whether a change in any of these four characteristics would reduce contrast effects or even result in assimilation. We suspect, for example, that if the context information that was juxtaposed with the target information was less relevant (e.g., if it indicated that 4% of men play a musical instrument), the resulting contrast effect would be weakened or eliminated. However, we did not address this issue empirically, as our focus has been on the ecologically frequent juxtaposition of disease rates for different social groups. Relatedly, we did not seek to test whether the social-comparative nature of the context information enhanced or was a necessary component of the contrast effect observed in the present experiments. The jux-

taposition of 4% and 12% may lead to a contrast effect even when those values refer to the proportions of fat in a baked ham and fried bacon—two nonsocial stimuli. If so, then the conclusions of the present experiments could be extended appropriately.

Whether or not the processes revealed in the present work apply to nonsocial comparisons, the present findings add to a growing body of research investigating the importance of immediate social comparisons in determining people's perceptions of vulnerability to disease (e.g., Klein, 1997; Rothman, Klein, & Weinstein, 1996; Weinstein & Klein, 1995). In a recent study, Klein (1997) presented participants with hypothetical information about their own chances of experiencing a negative life event as well as the average person's chances. Participants were told to imagine that they had either a 30% or a 60% chance of causing a car accident some time in their life and that this rate was either 20% lower or 20% higher than average. Participants' responses on several dependent measures (e.g., intention to wear seat belts) were significantly affected by the relative-risk manipulation but not the absolute-risk manipulation. Klein's (1997) interpretation of this effect and related effects focused primarily on how comparative information, beyond absolute information, influences evaluative self-assessments, which mediate the effects detected on the dependent measures. A related but distinct idea based on the present findings is that changes in intuitive perceptions of certainty partially mediated the effects detected by Klein; a 30% probability of anything can seem intuitively large or small depending on whether the immediate comparison standard is 10% or 50%.

### *Are the Contrast Effects Dependent on the Respondents' Group Membership?*

When initially designing these studies, we speculated that participants who were members of a target group would exhibit stronger contrast effects than would those who were not members. Because our participant pool included far more women and European Americans than men and African Americans and because we wished to maximize statistical power for detecting contrast effects in our experiments, we created our materials so that women and European Americans would be the designated target groups. Our expectations received some initial support in Experiment 1. Women but not men showed significant contrast effects when rating women's vulnerability for one of the two key diseases (SCD). A similar trend was found for women's and men's diagnoses of Anna. However, there were no significant interactions (or notable patterns) involving the sex of participants on the direct comparison measures in Experiments 3 and 4, suggesting that membership in the target group is not necessary for context information to produce contrast effects. Analogous interaction effects for the questions regarding European and African Americans were not explored because there were too few African Americans in our samples.

### *Implications for Health Communications*

The effects revealed in these experiments may have important implications for understanding the public's and health professionals' perceptions regarding the vulnerability of various groups to diseases. For example, heart disease is the leading cause of death among women in Westernized nations (Eaker et al., 1999). It

therefore seems that heart disease should be appreciated as a serious threat to women's health. However, recent research (R. Martin, Gordon, & Lounsbury, 1998) has suggested that women's vulnerability to heart disease is often underappreciated—a phenomenon that may be due in part to contrast-inducing comparisons between men's and women's chances of suffering from heart disease. Although men are approximately three times more likely than are women to suffer from heart disease prior to age 65 (at which point women are actually more likely to suffer a heart attack), men reporting chest pain are more than six times more likely than their female counterparts to be referred for diagnostic cardiac procedures (Council on Ethical and Judicial Affairs, American Medical Association, 1991; Tobin, Wassertheil-Smoller, & Wexler, 1987). Also, female myocardial infarction victims are significantly less likely than their male counterparts to attribute their symptoms to cardiac causes, even after symptom presentation, age, and illness severity are controlled for (R. Martin, 2000).

Relatedly, the present findings have implications for health professionals who wish to inform various groups of people about their vulnerability to a specified disease. In Experiment 3, even participants in the careful-instruction group perceived women to be more vulnerable to a disease that had an 11% prevalence rate than to one that had a 12% rate. They also perceived European Americans to be more vulnerable to a disease that strikes 1,500 per million than to one that strikes 1,600 per million. These findings suggest that the comparison processes people engage in when encoding health statistics have the potential to be more influential than the statistical values themselves. This leads to an optimistic conclusion that risk communicators may be able to creatively construct graphs, pamphlets, and news releases that take advantage of contrastive comparisons to more effectively instill a sense of vulnerability in a targeted social group. However, a notable caution is that some of the contrastive comparisons may make non-targeted social groups feel less vulnerable than they should.

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## Appendix

## Disease Information Read by Participants in Group A of Experiment 1

*Stomach churn discomfort*—About 4% of men suffer from this condition, while about 12% of women have the condition. It involves a sometimes-embarrassing allergic reaction that causes the stomach to make a loud churning noise after a moderate amount of sugar has been digested.

*Wine-breath disease*—This disease affects about 1% of men and 2% of women. It causes digestive juices to periodically produce a fermenting smell. Sometimes people mistakenly assume that sufferers of this condition have been drinking alcohol.

*Body toxin condition*—This condition affects about 1 out of every 300,000 African-Americans, and about 1 out of every 900 Americans of European descent. The glands produce a mildly toxic mix of hormones that makes the skin dry and extra-sensitive to sunlight.

*Gluten bloat condition*—This condition affects about 20% of men, and about 12% of women. It is characterized by periodic sensations of a

painfully bloated stomach after having eaten only a moderate-sized meal that includes gluten.

*Vitamin B12 aversion*—About 1 out of every 400 African-Americans and 1 out of every 600 European-Americans are affected by this condition. It requires medication to avoid rashes and hives triggered by consumption of Vitamin B12.

*Blue-elbow disease*—This condition affects Americans of African descent at a rate of 1 out of every 200. It affects Americans of European descent at a rate of 1 out of 900. It is a heritable disorder of connective tissue in which elbows and knees often take on a bluish hue and the skin is easily bruised.

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