Rape Myth Acceptance and Judgments of Vulnerability to Sexual Assault: An Internet Experiment

Gerd Bohner,[1] Unna N. Danner,[2] Frank Siebler,[1] and Gary B. Samson[3]

[1] University of Bielefeld, Germany, [2] Free University, Amsterdam, The Netherlands, and [3] University of Kent, Canterbury, UK

Abstract. Processing strategies in risk assessment were studied in an Internet experiment. Women (N = 399) who were either low or high in rape myth acceptance (RMA) were asked to recall either two or six behaviors that either increase or decrease the risk of being sexually assaulted. Later they judged their personal vulnerability to sexual assault under either no time pressure (no response deadline) or time pressure (response deadline of 5 s). Without time pressure, the results were opposite to previous research: Women low in RMA relied on ease of recall and reported higher vulnerability after recalling few rather than many risk increasing behaviors, or many rather than few risk-decreasing behaviors; women high in RMA relied on the amount of information recalled, which resulted in an opposite pattern of vulnerability judgments. No influences of ease of recall or amount recalled on vulnerability judgments were detected under time pressure.

Key words: rape, judgment under uncertainty, heuristic processing

Rape is a serious problem worldwide. In The Netherlands, for example, one in seven women has been sexually abused (Over seksueel geweld, 2001), and in the United States even one in six women has been a victim of a completed or attempted rape (Tjaden & Thoennes, 2000). Rape and sexual assault are crimes that can hurt people deeply, mentally and physically. Most reported cases concern female victims and male perpetrators (e.g., Poppen & Segal, 1988). Women who have been raped will seek social support to help them deal with the sexual violence, but often they find themselves as targets of negative social reactions (e.g., Feldman, Ullman, & Dunkel-Schetter, 1998).

At the core of such reactions are widely held beliefs that have been termed *rape myths* (Burt, 1980). They may be defined as "descriptive or prescriptive beliefs about rape … that serve to deny, trivialize or justify sexual aggression by men against women" (Bohner, 1998, p. 14; see also Lonsway & Fitzgerald, 1994). Some of these myths are clearly empirically false (e.g., "It is usually only women who dress suggestively that are raped"), whereas others are impossible to falsify (e.g., "Many women secretly desire to be raped") – what they have in common is that they create a hostile climate towards rape victims (Burt, 1980). For women, one function of endorsing rape myths is to keep rape at a distance, to lower one's perceived personal risk of being sexually assaulted. If rape is seen as happening only to other women who could have avoided the sexual violence by behaving differently, the risk of personal victimization

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becomes subjectively controllable (Bohner, 1998). Accordingly, women high (vs. low) in rape myth acceptance (RMA) tend to judge their own vulnerability to sexual assault as lower and interpret information about rape in a less self-threatening way (Bohner, 1998; Bohner & Lampridis, in press; Bohner, Siebler, & Raaijmakers, 1999; Bohner, Weisbrod, Raymond, Barzvi, & Schwarz, 1993).

In this paper, we will address the cognitive mechanisms by which different levels of rape myth acceptance may affect judgments of vulnerability to sexual assault. An important factor in human judgment is *how* relevant information is processed. People may process heuristically, taking into account only the most highly accessible information or subjective experience, or they may process systematically, making use of more informational detail. The processing strategy used mainly depends on an individual's motivation (e.g., personal relevance of the judgment) and processing capacity (e.g., Bohner, Moskowitz, & Chaiken, 1995; Chen & Chaiken, 1999). Applying these insights to risk perception regarding sexual victimization, Grayson and Schwarz (1999) proposed that differences in women's RMA affect the processing strategy that women use. Specifically, they predicted that women high in RMA would perceive lower personal relevance of sexual violence, and would thus form risk judgments based on heuristic processing, whereas women low in rape myth acceptance would feel more personally affected and hence use more systematic processing to arrive at a risk judgment. They tested these hypotheses in two studies where a specific heuristic strategy – judging by the *ease* with which information comes to mind – was pinpointed against the presumably more systematic strategy of using the *amount* of information available. As our present research directly follows up on these studies, we will present their underlying logic and experimental paradigm in some detail.

The Ease of Retrieval Heuristic

The frequency or likelihood of an event can be judged by the ease with which relevant instances can be retrieved from memory: The easier it is to generate examples of an event, the more frequent that event is judged to have happened. This phenomenon has been termed the "availability heuristic" (Tversky & Kahneman, 1973). Reliance on this heuristic provides accurate frequency judgments to the extent that the accessibility of instances is indeed caused by the frequency of the target category to which they belong. This need not always be the case, however. For example, individuals tend to estimate that words beginning with the letter "r" are more frequent in the English language than are words that have an "r" as their third letter, although in fact the opposite is true (Tversky & Kahneman, 1973, Exp. 3). In this case, of course, the greater accessibility of words beginning with "r" is caused by the fact that the first letter of a word serves as a more potent retrieval cue than does its third letter.

Interestingly, most early studies on the availability heuristic were ambiguous regarding the process underlying the operation of the heuristic. Do individuals use the *experienced ease of retrieval* as a cue to judging frequency, as Tversky and Kahneman (1973) suggested, or do they rely on the *amount of information they can retrieve* within a short time interval? As Schwarz and colleagues (1991) observed, these two aspects are confounded in natural situations – the easier it is to retrieve information, the more information should be brought to mind in a given time interval.

To disentangle these two aspects, Schwarz et al. (1991) introduced an ingenious paradigm: By varying the exact number of examples that participants were asked to generate, the implications of ease of retrieval on the one hand and amount of information on the other could be set in direct opposition. For example, students were instructed to recall six examples of either assertive or unassertive personal behavior (which pretests had shown to be easy), whereas others were asked to recall twelve such examples (which pretests had shown to be difficult). The students' subsequent judgments of their own level of assertiveness clearly reflected the ease with which relevant information had come to mind: Those who had recalled six assertive or twelve unassertive examples judged their own assertiveness to be higher than those who had recalled twelve assertive or six unassertive examples. A strategy that relies on the amount of information recalled would have produced the opposite pattern of results (Schwarz et al., 1991, Exp. 1). To date, several conceptual replications using the paradigm introduced by Schwarz and colleagues have demonstrated the operation of an ease of retrieval heuristic in various domains (e.g., Belli, Winkielman, Read, Schwarz, & Lynn, 1998; Grayson & Schwarz, 1999; Rothman & Schwarz, 1998; Wänke, Bohner, & Jurkowitsch, 1997; Wänke, Schwarz, & Bless, 1995; for a review, see Schwarz, 1998).

Variations in Processing Strategy: Evidence and Critique

If judgments based on experienced ease reflect a heuristic processing strategy, however, then one should expect people to switch to a more systematic strategy if they are both capable and motivated of doing so. One such strategy might be directing one's

attention to the amount of information retrieved and its implications, i.e., interpreting many examples as stronger evidence than few examples, regardless of how easy or difficult it was to generate them. To date, two papers on judgments of personal risk have demonstrated that people may rely either on experienced ease or on the amount of information recalled, depending on individual differences that are related to the personal relevance of the judgmental domain (Grayson & Schwarz, 1999; Rothman & Schwarz, 1998), although neither of these studies directly manipulated processing effort.

Rothman and Schwarz (1998) asked male participants to recall either few or many personal behaviors that either increase or decrease the risk of getting heart disease. Later they assessed their participants' judgments of personal vulnerability to heart disease. The results greatly depended on whether participants knew about cases of heart disease in their own family. Those without a family history of heart disease judged their own personal vulnerability on the basis of ease of retrieval, thus giving lower vulnerability judgments after recalling few risk-decreasing or many risk-increasing behaviors than after recalling few risk-increasing or many risk-decreasing behaviors. A fully reversed pattern, however, was found for participants who had heart disease running in the family and thus may have perceived the task as more personally relevant – their judgments reflected the amount of behaviors recalled.

Grayson and Schwarz (1999, Study 1) studied judgments of vulnerability to rape. They asked female participants to generate either four examples ("easy" condition) or eight examples ("difficult" condition) of personal behaviors that either increase or decrease the risk of being raped. Participants' rape myth acceptance (low vs. high) was included in the design as an individual difference variable. Grayson and Schwarz found that women *high in RMA* seemed to judge their own vulnerability based on ease of retrieval: These women gave higher vulnerability estimates when they had generated few risk-increasing or many risk-decreasing behaviors than when they had generated few risk-decreasing or many risk-increasing behaviors. The result pattern was reversed, however, for women *low in RMA*. Those women seemed to judge their vulnerability on the basis of recalled amount, inferring high vulnerability when they had recalled many (vs. few) risk-increasing behaviors or few (vs. many) risk-decreasing behaviors. In a second study where participants were asked only to generate risk-increasing behaviors, Grayson and Schwarz replicated the results of their first study. They concluded that low RMA, being associated with greater personal relevance of the risk of rape, instigated a systematic processing strategy (i.e., using the amount of information generated as a basis of judgment), whereas high RMA, being associated with less personal relevance, instigated a heuristic strategy (i.e., judging on the basis of subjective ease of retrieval).

Although these conclusions seem convincing at first glance, we note a few ambiguities. First, none of the studies using the ease-of-retrieval paradigm that tried to distinguish between heuristic and systematic processing included an *experimental* manipulation of processing effort. Instead, pre-existing differences in family history or individual differences in chronic beliefs were used to operationalize different levels of personal relevance. Due to the correlational nature of these designs a *causal* effect of personal relevance on processing strategy was not strongly established.

Second, it is not fully convincing to assume that judgments based on the number of examples are any more systematic (or less heuristic) than judgments based on the ease with which these examples come to mind. In the persuasion domain, for example, judgments based on the number of available arguments have been conceptualized as an example of heuristic processing (Axsom, Liberman, & Wilson, 1982, as cited in Eagly & Chaiken, 1993, pp. 334–335) and shown to predominate over content-based processing under conditions of low rather than high involvement (Petty & Cacioppo, 1984). It might thus be more expedient to conceive of the observed difference in processing strategy as reflecting the use of internal, experiential cues versus external, informational cues. Whether this reflects differences in processing effort as well, still remains to be demonstrated.

Two final points pertain specifically to the studies by Grayson and Schwarz (1999). These authors' interpretation that women low in RMA would perceive high relevance of their own personal behavior as an antecedent of potential victimization, stands in contrast to the *content* of these women's beliefs. After all, low RMA is defined partly as the rejection of a causal link between women's behavior and the risk of being raped, whereas high RMA includes the endorsement of specific links between women's behavior and their vulnerability to rape. Thus, even though women low in RMA are likely to perceive rape as a threat to all women, including themselves, they should be unlikely to perceive a strong connection between a woman's own behavior and her victimization. Conversely, even though women high in RMA do not tend to perceive rape as a threat to themselves personally, they do tend to perceive a connection between a woman's behavior and her being victimized. In fact, these differences in the perceived causality of women's behavior seem to be a logical antecedent to variations in beliefs about the degree to which rape constitutes a threat to oneself (for discussion, see Bohner, 1998). Taking the content of rape myths into account, one could therefore plausibly expect a reversed relationship between

RMA and the subjective relevance of own behavior for judgments of vulnerability. In this context it should also be noted that Grayson and Schwarz did not seem to find a positive correlation between RMA and vulnerability judgments in their sample (which would have shown as a main effect of RMA in their ANOVAs), which is at variance with other research (see Bohner, 1998, pp. 63 ff.).

Furthermore, as a caveat regarding their own hypothesis, Grayson and Schwarz (1999) argue that low-RMA women may have thought more extensively about rape in the past in an attempt to free themselves from prevailing stereotypes, whereas high-RMA women would have been able to maintain their stereotypes without expending much cognitive effort (pp. 4–5). The consequences of such a difference in previous elaboration, they continue, are not clear: On the one hand, participants' habitual processing style may generalize to the experimental situation and contribute to producing the pattern that Grayson and Schwarz predicted (see Smith, 1994); on the other hand, previous elaboration may have created well-established knowledge structures regarding rape in low-RMA women, thus limiting the extent to which they may be influenced by the temporary accessibility of both experiential and declarative information (see Schwarz & Bless, 1992). Again, although Grayson and Schwarz' data seem to be compatible with the former argument, we disagree with its central premise. Even though low-RMA women may have thought more about rape in the past, it seems unlikely that the content of such extended thinking included the detailed consideration of women's behavior as an antecedent of rape – if anything, such a link would probably have been categorically denied, thus obviating the need for considering the implications of specific behaviors in any detail. On the other hand, high-RMA women may well have established a routine of deflecting the threat of rape by blaming the victim – e.g., by condemning the victim's behavior – when confronted with particular instances of rape (for discussion see Bohner, 1998).

The Present Research

To address these critical points, we conducted an extended replication of Grayson and Schwarz (1999). Given the ambiguity of assumptions regarding the implications of RMA for levels of personal relevance, we wondered if their main result pattern would be replicable using a more diverse, multinational sample. We also tried to distinguish more clearly between low-effort and high-effort processing by including an experimental manipulation of time pressure. Specifically, some participants were asked to report their judgments within five seconds, whereas others were not given a time limit. We reasoned that the time-limit manipulation would interfere more with using a systematic processing strategy than with using a heuristic strategy (see Wilson, Lindsey, & Schooler, 2000). Therefore, if judging by the number of examples indeed reflects a systematic strategy, whereas judging by ease reflects a heuristic strategy, as proposed by Grayson and Schwarz, we should find that all respondents would fall back on judging by ease in the time pressure conditions, whereas processing differences based on level of RMA would be apparent mainly in the no time pressure conditions.

Method

An experiment was conducted via the Internet, simultaneously in three languages: English, German, and Dutch. This procedure offered the advantage of studying a larger and more diverse sample of women in comparison to the typical college student sample used in most laboratory experiments. The use of computers to deliver instructions and collect data enabled us to control the time participants actually took to arrive at a vulnerability judgment and to measure these response times unobtrusively. Another advantage of experimenting via the Internet is that it increases people's feelings of anonymity, in comparison to face-to-face interactions between participant and the experimenter. When people experience higher levels of visual anonymity, they disclose information about themselves more readily (Joinson, 2001; Locke & Gilbert, 1995). Compared to paper-and-pencil assessment or face-to-face interviews, computerized data collection has been found to elicit fewer social desirability concerns (for a meta-analysis, see Richman, Kiesler, Weisband, & Drasgow, 1999), to result in the revealing of more embarrassing information (for a meta-analysis, see Feigelson & Dwight, 2000) or risk-related behaviors (Gerbert et al., 1999), and to increase the likelihood of reporting substance use, even in the presence of bystanders (Aquilino, Wright & Supple, 2000). On the other hand, when conducting a study on the Internet, control over the experimental environment is reduced. Specifically, the laboratory's standardized setting is replaced by a range of diverse, physical as well as technical, environments selected by the participants. This increased variability is likely to reduce the impact of experimental manipulations (for a discussion see Nosek, Banaji, & Greenwald, 2002), but may at the same time increase the generalizability of findings (Reips, 2000, p. 111). In any case, when studying sensitive topics as in the present research, data collection via the Internet may be a particularly useful complement of the more traditional, laboratory-based approach.

Apparatus

All materials and instructions were first developed in English and then translated to German and Dutch by the first and second author, respectively. The experimental procedure was controlled by means of custom scripts written in the language "PHP: Hypertext Preprocessor" (PHP). Depending on experimental condition and the language selected by a participant, the scripts generated and delivered standard hypertext markup language (HTML) documents to participants' Web browsers. Different from client-side solutions like Java or JavaScript, PHP scripts are executed on the server. By choosing a server-side approach, we bypassed incompatibilities in different browsers' implementation of client-side scripting (for a discussion see, e.g., Schmidt, 2000, pp. 292–293), thereby creating a largely standardized experimental environment.

Sampling

Potential participants were either approached by e-mail or were asked to participate in the study on forums and message boards of diverse Websites in each of the three languages of the study[1]. These Websites were found through search engines and links on other Websites. One of our main search priorities was identifying "Websites for women", e.g., Websites for housewives, lesbian women, students, business women, and many more. In addition, all psychology undergraduate students at the University of Kent at Canterbury were contacted via e-mail, as well as social psychology mailing lists. Friends, relatives, and acquaintances of the researchers were also approached by e-mail. In those e-mail messages, recipients were generally informed about the purpose of the study and were asked to pass our message on to other women they knew who may want to participate. The same information was given on message boards where we also asked if people would like to participate and if they knew other possible participants. The sample thus created may be best described as a relatively diverse opportunity sample. No direct remuneration was given to participants, although they were informed that they could obtain information about the study's results as soon as it would be completed. The study site remained active for about three months.[2]

Design

Participants were randomly assigned to one of the conditions of a 2 (number of examples: few, many) × 2 (direction of examples: risk-increasing, risk-decreasing) × 2 (time pressure: no, yes) design or to one of two control conditions. Participants in the control conditions did not generate any examples, but were either placed under time pressure or not. The randomization procedure was based on an algorithm that assigned 10% of cases at random to each of these 10 conditions.

In addition, and orthogonal to these 10 conditions, participants were categorized as low versus high in rape myth acceptance, based on a median split of an RMA measure (see below). Thus, the full design was a $2 \times 2 \times 2 \times 2$ between-subjects factorial with the factors *number of examples*, *direction of examples*, *time pressure*, and *RMA*, plus four control conditions which reflected the crossing of RMA and time pressure.

Overview of Procedure

After participants had accessed the experiment's Website and selected a language, they were thanked for their interest and reminded that the authors were especially looking for female participants. They were further informed that the study was conducted to investigate people's perception of rape and sexual violence. Participants were assured of the strict confidentiality and anonymity of all responses and learned that they could leave the study at any time, with their data only being transmitted after having completed all parts of the study.[3] Then participants were familiarized with the mode of answering. Specifically, they were asked to respond to four neutral questions by ticking, with the computer mouse, an appropriate response option on a 7-point scale. To proceed with the study, they were asked to press a button. If the button was pressed before having answered each question, the same screen was presented once more. This time, however, any unanswered questions were highlighted. A brief message asked participants to respond to the highlighted questions or, should they not wish to do so, to press the "proceed" button again. In subsequent steps of the experimental procedure, we included similar reminders to elicit any initially omitted responses, but participants could always choose to proceed without completing all questions.

[1] E.g.:, http://shesgotforum.com/cgi-bin/Ultimate.cgi
http://christien.nl/discussie
http://www.gesprekvandedag.nl/forum/index.html
http://womanthink.community.everyone.net

[2] The study was run from a site at the University of Kent. It is currently still available at http://www.ukc.ac.uk/psychology/surveys/rm/oldindex.htm

[3] This procedure implies that it was not possible to determine how many participants may have started answering the questionnaire but later decided not to submit their data. However, the final distribution of participants across the ten conditions was examined to guard against the possibility of uneven drop-out rates. This analysis did not suggest any problems, $\chi^2(9; N = 399) = 9.74$, $p = .371$.

Next, participants were asked to provide the appropriate number of risk-increasing or risk-decreasing behaviors (depending on condition – for control participants, this stage was omitted), after which they were asked to provide an estimate of personal vulnerability either under time pressure or without time pressure. Later, participants (except those in the control conditions) were asked to indicate for each of the behaviors they had generated how often they engaged in it and how strongly it was related to the risk of being sexually assaulted. Then they completed a need for cognition scale[4] and a rape myth acceptance scale, were asked to provide some demographic information, and answered questions regarding any experiences of sexual assault that they may have had. Finally, they were thanked and invited to leave their e-mail address in case they wished to be informed about the study results; they were also informed that debriefing information would be made available shortly at the same Website. When a participant pressed the "continue" button on the debriefing screen, his or her responses were permanently stored. Further, in order to avoid multiple clicks on that button, the participant's browser was automatically redirected to the Psychology Department's homepage.

Materials

Variation of number and type of examples and assessment of perceived difficulty. Participants were asked to type either two or six examples of personal behaviors that they thought would "increase (decrease) the risk of a woman of being raped". Informal pilot testing had indicated that generating two behaviors was experienced as easy, whereas generating six behaviors was experienced as difficult. After listing their examples, participants were asked to indicate, as a manipulation check, how difficult it was for them to list the examples of risk-related behaviors, using a scale from 1, "*not at all difficult*", to 7, "*extremely difficult*".

Variation of time pressure and assessment of vulnerability judgment. The following two screens provided the time-pressure manipulation and served to assess the judgment of personal vulnerability, our main dependent variable. Participants in the time-pressure conditions were instructed to answer the question on the following screen within 5 seconds, giving the first answer that comes to mind. By contrast, participants in the no-time-pressure conditions were asked, without any reference to a deadline, to consider the question on the next screen thoroughly and to answer it as accurately as possible. The vulnerability question on the following screen read: "How likely do you think it is that you personally could be sexually assaulted", to be answered on a scale from 1, "*not at all likely*", to 7, "*extremely likely*".

Above this question, only participants in the time-pressure conditions were shown a "time bar" that dynamically indicated how the five seconds for answering elapsed. After five seconds, the instruction "Time up. You must answer now." appeared within the bar. Technically, the time bar was implemented in the form of an animated Graphics Interchange Format (GIF) image. Thus, the five-second response interval was timed by the participant's computer. By choosing this strategy, we avoided error variance that might otherwise contaminate the response interval by transmission delays between server and client computers, or "net lag" (Reips, 2000, p. 111). The experiment did not continue before the question was answered, and each participant's response time was recorded unobtrusively in all conditions. Specifically, our scripts recorded the time when a participant's request for the document featuring the focal screen arrived at the server. Further, they recorded the time of arrival of the next request, as triggered by the participant's response to the vulnerability question. The response time score was computed as the difference between these two points in time, with a one-second granularity. It should be noted that the method of assessing response times included transmission delays between client and server computers both before and after presentation of the vulnerability question. Thus, due to "net lag", response time scores were likely to overestimate participants' actual response latency by some amount. However, transmission delay should be a source of unsystematic error, affecting both time pressure and no time pressure conditions similarly. Further, as described above, the duration of the five-second response prompt was not affected by transmission delay.[5]

Judging examples for frequency and relevance. Next, participants (except those in the control condi-

[4] One purpose of the need for cognition (NFC) scale (see Cacioppo, Petty, Feinstein, & Jarvis, 1996) was to serve as a buffer between the experimental manipulations and the RMA scale, another purpose was that we were interested in the relationship between NFC and RMA. We will not discuss any results involving the NFC scale, as they are less relevant in the context of this article.

[5] A reviewer was concerned about the possibility that the graphics file used to induce time pressure may have downloaded noticeably after the text on the same page. However, extensive testing off-site using modem connections at 28.8 kbps as well as several slower connections revealed no problems of this kind. (In the German version, the size of the time pressure manipulation GIF file was 5.86K, whereas in the English and Dutch versions it was somewhat smaller; using a modem connected at 28.8kbps, these files would have taken less than one second to download.)

tions) were again shown the examples they had generated. For each example separately, they were asked to indicate its personal frequency ("How often do you perform this behavior?") and relevance ("How much do you believe this behavior can increase/decrease a woman's risk of being sexually assaulted"). Each question was followed by a 7-point scale ranging from 1, "*never/not much*", to 7, "*very often/very much*".

Rape myth acceptance. The remaining sequence was the same for all participants. First they were asked to complete a scale measuring the need for cognition (see Footnote 4) and then a scale measuring rape myth acceptance. For the latter purpose, we used the short form of the Illinois Rape Myth Acceptance (IRMA) Scale (Payne, Lonsway, & Fitzgerald, 1999). This scale contains 17 critical items (e.g., "Rape accusations are often used as a way of getting back at men"; "A woman who 'teases' men deserves anything that might happen") and three fillers; it is reported to have high reliability and validity (Payne et al., 1999). As there were no published German or Dutch versions of the IRMA scale available, we had it translated to German and Dutch, respectively, by native speakers and translated back to English independently to check for inconsistencies. Only few inconsistencies arose in the first round of translation, and any problematic wording was discussed and changed if necessary. Each item had to be rated on a 7-point scale, ranging from 1, "*totally disagree*", to 7, "*totally agree*", with higher scores indicating greater rape myth acceptance.

Demographics. On the two final screens, participants were asked some demographic details. They were reassured that they were not required to complete these questions, but learned that doing so would be useful for our research. Participants' sex was assessed to identify any male participants so their data could be excluded from analyses. Participants' age, nationality (open-ended) and any prior experience with sexual assault ("*no*" – "*yes*") were also assessed; participants who answered "yes" to the assault question were further asked to describe their experience in a few words in a separate box.

Results

Preliminary Analyses

Sample demographics, language chosen, and prior victimization. During three months, 440 individuals participated. The minimum age for inclusion had been pre-set to 16 years, and only female participants were to be included. Based on these criteria, the data of 41 participants were excluded from analyses (28 male; 11 who did not indicate their sex; two who did not meet the age criterion). We were thus left with 399 cases for analyses. (After the analyses on scale reliability and the manipulation checks, ten additional participants' data were excluded from the main analysis of variance because their response times for the vulnerability judgment were excessively long; see below for detail.)

Participants' mean age was 27.56 years (SD = 8.46; range = 16 to 63). The English, German, and Dutch versions of the study were completed by 197, 89, and 113 participants, respectively. Participants' most frequent nationalities were: British or Irish (109), Dutch (101), German (72), U.S. American or Canadian (34); the remaining 83 participants reported other nationalities (including Austrian, Swiss, and Belgian) or did not indicate their nationality. One hundred and twenty-four participants (31.1%) indicated that they had been raped or sexually assaulted, 242 (60.7%) reported no prior experience with sexual assault, and 33 participants (8.3%) left the respective questions unanswered. The rate of prior victimization was statistically independent of language group, $\chi^2(2; N = 366) = 2.51$, $p = .285$, and experimental condition, $\chi^2(9; N = 366) = 10.20$, $p = .335$.[6]

We did, however, find differences between language groups in terms of vulnerability judgments, $F(2, 396) = 28.84$, $p < .001$. Participants answering the English version perceived the highest risk (M = 4.12), followed by participants answering the German version (M = 3.42) and the Dutch version (M = 2.80), with all pairwise comparisons being significant at $p < .05$, Tukey's HSD test. It is presently unclear what may have caused these different levels of risk perception. Prior victimization was also related to vulnerability judgments, with participants who had previously been sexually assaulted judging their vulnerability to be higher (M = 4.06) than participants who had not been assaulted (M = 3.40), $t(364) = 3.94$, $p < .001$. Importantly, however, preliminary analyses showed that the inclusion of either language group or prior victimization as factors in the main ANOVA did not qualify the effects of rape myth acceptance and the experimental variables to be reported below.

Multiple submissions. Because clicking the "continue" button on the last screen automatically redirected a participant's browser to another page,

[6] The level of prior victimization in our sample is comparable to previous research. In a study with U.S. college students by Bohner et al. (1993, Study 2), 41.6% of the female participants reported to have been victims of either completed or attempted sexual coercion; in the two studies reported by Grayson and Schwarz (1999), this rate was 27%. In a large-scale survey of U.S. college students, 39.4% of female respondents reported to have been victims of rape, attempted rape, or sexual coercion (Koss, Gidycz, & Wisniewski, 1987).

multiple submission of data by the same participant was unlikely to occur in the present study. In fact, an inspection of responses to the NFC scale revealed 399 unique response patterns. Thus, no participant submitted her or his data more than once.[7]

Treatment of missing data. Although participants were not required to answer every single question, the overall rate of missing values was low. Our method of highlighting any unanswered questions and reminding participants to answer them seems to have been effective. Specifically, 24.5% of participants in the experimental conditions (8.5% in the two examples conditions, and 43.5% in the six examples conditions) were shown a reminder because they had not initially completed the requested number of examples. After this reminder, the final percentage of participants who did not complete the exact number of examples requested dropped to 12.4% (4.5% and 21.8%, respectively, in the two examples versus six examples conditions). Following Grayson and Schwarz (1999), we did not exclude any participants on the basis of incomplete lists of examples (see manipulation checks below for further information).

Only one participant (0.3%) did not rate all her self-generated examples for frequency and relevance – even after being reminded she rated only two out of six examples generated. This case was omitted from the analysis of the relevance manipulation check.

Finally, 18 participants (4.5%) were shown a reminder because they had failed to complete several IRMA items. In the end, only three participants (0.8%) had left up to three IRMA items unanswered; these cases were not included in the IRMA reliability analysis, but their RMA score was computed from their nonmissing item scores and used for the main analyses.

Reliability of the IRMA scale and defining groups of low versus high RMA. Reliability analyses of the IRMA scale were conducted separately for each language version as well as for the full sample. Cronbach's α was .86 for the English version, .84 for the Dutch version, and .78 for the German version. For the full sample, we found an alpha of .84. Thus, internal consistencies were at least satisfactory and comparable across subsamples. The mean across all 17 critical IRMA scale items was defined as a participant's RMA score. We further tested for differences in the mean level of RMA among the three language subsamples. A one-way analysis of variance (MSE = 0.38) indicated a marginal effect of language, $F(2, 396) = 2.61$, $p = .075$.[8] Consequently, median splits on RMA were conducted separately within each language version.

Manipulation checks

Number of examples generated and perceived difficulty. Separate 2 × 2 × 2 × 2 between-subjects analyses of variance (ANOVAs) with the factors *number of examples* (few vs. many), *direction of examples* (risk-decreasing vs. risk-increasing), *RMA* (low vs. high) and *time pressure* (no vs. yes) were performed on the number of examples generated (MSE = 0.85) and on the perceived difficulty of generating examples (MSE = 3.29), respectively. These revealed that the experimental variation of number had been successful: Participants in the "few" conditions generated a mean of 1.94 (SD = 0.31) examples, whereas those in the "many" conditions generated a mean of 5.41 (SD = 1.33) examples, $F(1, 307) = 1063.61$, $p < .001$.[9] Also, as intended, participants in the "few" conditions perceived generating examples as less difficult (M = 3.66, SD = 0.14) than did participants in the "many" conditions (M = 4.70, SD = 0.16), $F(1, 307) = 25.41$, $p < .001$. An additional main effect was found for RMA, with low-RMA participants generally reporting greater difficulty at generating examples (M = 4.39, SD = 0.15) than did high-RMA participants (M = 3.98, SD = 0.14), $F(1, 307) = 3.88$, $p = .05$. No other effects were obtained.

Effects of time pressure on response times. Prior to conducting an ANOVA on the time participants took to respond to the vulnerability question, the distribution of this variable was inspected. Ten outliers with response times greater than three standard deviations (SD = 14.13) above the grand mean (M = 11.56) were identified, i.e., with response latencies > 54 seconds. Two of these were in the time pressure conditions and eight were in the no time pressure conditions. As these response times seemed exces-

[7] Although we can exclude the possibility of *multiple submission of the same data*, we cannot exclude the possibility of *multiple participation of the same person*. However, as we did not offer incentives beyond information about the study's results (a benefit that could be gained by participating only once), multiple participation appears somewhat unlikely.

[8] According to pairwise post-hoc comparisons (Tukey's HSD test), participants in the German version had lower IRMA scores (M = 1.58, SD = 0.50) than participants in the Dutch version (M = 1.77, SD = 0.66), $p = .06$; participants in the English version lay in between (M = 1.67, SD = 0.64) without differing significantly from either other subgroup, ps > .33.

[9] This main effect was not moderated by any other independent variable. The only other effect that reached significance was the interaction of direction, RMA, and time pressure, $F(1, 307) = 4.34$, $p = .038$. As this effect is both completely independent of the effect of number and negligible in magnitude compared to the latter, it will not be further discussed.

sive and it was unclear what might have caused them (including possible hardware problems), we decided to eliminate these cases from further analyses.

A $2 \times 2 \times 2 \times 2$ ANOVA on response time (MSE = 37.02) revealed only the predicted main effect of the time pressure manipulation, with participants under time pressure responding earlier (M = 6.06 s, SD = 4.53 s) than participants not under time pressure (M = 12.35 s, SD = 7.19 s), $F(1, 297) = 79.06$, $p < .001$. No other effects approached significance, all $p > .15$. Thus, although the mean response time under time pressure was slightly above the 5-second limit (but see Method section regarding potential overestimation), the induction of this limit did cause participants to respond twice as quickly as participants who were not given a limit.

Relevance of generated behaviors for risk assessment. To check if the content of behaviors in the "many" conditions was comparable to those in the "few" conditions, an index of relevance was created for the last two behaviors (or, where appropriate, the single behavior) reported by each participant by first multiplying the rating of a behavior's personal frequency with its perceived relevance, and then averaging the products across the two behaviors. An ANOVA on the resulting index (possible range: 1 to 49; MSE = 107.47) revealed that, as intended, there was no difference in relevance between the "few" conditions (M = 18.69, SD = 11.16) and the "many" conditions (M = 17.19, SD = 11.07), $F(1, 290) = 1.02$, $p > .31$. We did find a main effect of direction, however, showing that risk-decreasing behaviors were generally rated as more relevant (M = 22.07, SD = 11.68) than risk-increasing behaviors (M = 13.82, SD = 8.74), $F(1, 290) = 40.20$, $p < .001$.

Vulnerability judgments in control conditions. As a final check before moving on to the main analysis of vulnerability judgments, we used the control condition data (N = 76) to test if there were any effects of the time pressure manipulation or level of RMA on vulnerability judgments in the absence of self-generated information regarding risk-related behaviors. Interestingly, a 2×2 ANOVA (MSE = 2.65) with factors RMA (low vs. high) and time pressure (no vs. yes) revealed a significant interaction effect, $F(1, 72) = 4.91$, $p = .03$. Under time pressure, low-RMA participants reported greater subjective vulnerability (M = 4.29, SD = 1.44) than did high-RMA participants (M = 3.11, SD = 1.71), $F(1, 72) = 4.10$, $p = .047$ for the simple effect. This difference was not present in conditions without time pressure, where low-RMA participants did not report greater subjective vulnerability (M = 3.30, SD = 1.61) than did high-RMA participants (M = 3.82, SD = 1.70), $F(1, 72) = 1.10$, $p = .299$ for the simple effect.

Thus, the content of participants' enduring beliefs about rape seemed to determine judgments under time pressure: When asked to make a snap judgment, low-RMA participants, who believe that rape poses a threat to all women, rated their own vulnerability as relatively high, whereas high-RMA participants, who are more likely to believe that specific women provoke being raped by their own behavior, rated their own vulnerability as relatively low. If there was more time to think, however, the judgments of women low vs. high in RMA became more similar. It is presently unclear what caused this interaction of RMA and time pressure, but it should be taken into account when interpreting the vulnerability results of the main design.

Judgments of Vulnerability to Sexual Assault

The main analysis addressed the question if Grayson and Schwarz' findings regarding the role of ease versus amount of information retrieved would replicate in the present, more diverse, sample. Furthermore, the effects of time pressure at the judgment stage were also tested in an ease-of-retrieval paradigm for the first time. A $2 \times 2 \times 2 \times 2$ ANOVA on judgments of vulnerability (MSE = 2.54) revealed a marginal three-way interaction of RMA, number of examples, and direction of examples, $F(1, 297) = 3.05$, $p = .082$, which was qualified by a marginal interaction of all four factors, $F(1, 297) = 3.24$, $p = .073$. No other effects emerged, all $p > .13$. The condition means are displayed in Table 1.

To diagnose the four-way interaction pattern, we conducted separate $2 \times 2 \times 2$ ANOVAs for the no-time-pressure and time-pressure conditions, respectively. *Under time pressure* (MSE = 2.69), no significant effects were found, all $p > .12$. Specifically, there was no two-way interaction of number and direction of behaviors, nor was the latter interaction qualified by level of RMA, $F(1, 148) < 1$ for each effect. Thus, it seems safe to conclude that neither experienced ease nor amount of information recalled affected judgments of vulnerability if these judgments had to be made very fast. The difference between low and high RMA that we had observed in the control conditions under time pressure also did not emerge (M = 3.49 and 3.45 for low and high RMA, respectively.), $F(1, 148) < 1$.

When *no time pressure* was induced (MSE = 2.39), more interesting results emerged. The three-way interaction of RMA, number of examples, and direction of examples was significant, $F(1, 149) = 6.64$, $p = .011$; all other $F < 1$. As can be seen from the pattern of means in Table 1, we found a double cross-over pattern that is exactly opposite to the pattern reported by Grayson and Schwarz (1999). Low-RMA participants seemed to base their judgments on experienced ease, reporting higher vulnera-

Table 1. Ratings of Vulnerability to Sexual Assault as a Function of Rape Myth Acceptance, Number of Examples Generated and Type of Examples Generated

	Low RMA		High RMA	
	2 examples	6 examples	2 examples	6 examples
No time pressure				
Risk-decreasing examples	3.24	4.08	4.05	3.75
	(1.56)	(1.72)	(1.69)	(1.18)
Risk-increasing examples	4.04	3.45	3.23	4.13
	(1.74)	(1.29)	(1.31)	(1.51)
Time pressure				
Risk-decreasing examples	3.19	3.73	3.65	3.37
	(1.44)	(1.74)	(1.37)	(1.95)
Risk-increasing examples	3.22	3.83	3.50	3.25
	(1.56)	(1.61)	(1.85)	(1.65)

Note. Mean scores of women's ratings of their personal likelihood of being sexually assaulted; standard deviations in parentheses. The possible range of scores is 1 to 7, with higher values indicating greater risk. Based on $N = 313$; number of observations per condition, from left to right, beginning in top row: 25, 24, 21, 16, 23, 11, 22, 22, 15, 21, 11, 23, 19, 18, 24, 20, 20.

bility after generating few risk-increasing behaviors or many risk-decreasing behaviors than after generating many risk-increasing behaviors or few risk-decreasing behaviors; $F(1, 149) = 3.98$, $p = .048$ for the simple interaction of number and direction. By contrast, high-RMA participants seemed to base their judgments on amount of recalled information, reporting the highest levels of vulnerability after generating many risk-increasing behaviors or few risk-decreasing behaviors, $F(1, 149) = 2.73$, $p = .100$ for the simple interaction of number and direction.

Discussion

The results of this Internet experiment show a highly interesting, though not anticipated, pattern. Based on the reasoning put forward by Grayson and Schwarz (1999) one would have expected to find a replication of their result pattern under conditions of no time pressure. If participants' processing strategy was unconstrained, high-RMA participants were expected to judge their own vulnerability to sexual assault by the ease with which relevant behaviors could be generated, whereas participants low in RMA would judge their own vulnerability by the number of behaviors generated. However, this was not the case. On the contrary, the pattern of vulnerability judgments suggests that women low in RMA judged on the basis of ease of retrieval, whereas women high in RMA judged by the number of examples generated. We will postpone the discussion of possible reasons for this difference in result patterns for a moment, and first examine another question: Does our result pattern suggest that it was the low-RMA women who processed heuristically, whereas it was the high-RMA women who processed systematically?

If so, then one should see a stronger impact of time pressure on high-RMA participants' judgments than on low-RMA participants' judgments. Considering the time pressure manipulation, we first note that it seems to have been effective: Participants who were instructed to judge within five seconds actually did answer within a much shorter period than participants who were not given a response deadline. Looking at the effect of the time pressure manipulation on judgments of vulnerability, we find that it was strong enough to wipe out the interaction pattern of number and direction of examples for both low-RMA and high-RMA groups. In fact, judgments of personal vulnerability that were given under time pressure did not show any effect of the previously recalled examples.

This leaves us with the possible interpretation that both strategies – judging by ease of retrieval and judging by the number of instances – requires a certain amount of processing effort, which people in the time pressure conditions were unable to expend. In both cases, two pieces of information must be considered and integrated: First, the direction of examples generated (i.e., their content), and secondly, the ease with which they were retrieved or their number, respectively. This mental integration bears some similarity to processes studied within the mood-as-input approach (Martin, Abend, Sedikides, & Green, 1997; Martin, Ward, Achee, & Wyer, 1993). For example, Martin et al. (1997) found that people who had been put in a negative mood evaluated a sad story more *positively* than did people who had been put in a

positive mood, whereas mood had an opposite effect on evaluating a happy story. In other words, mood is used as information, but the implications of one's mood have to be considered in conjunction with aspects of the target of judgment (in this case: its purpose). It is conceivable that subjective ease of retrieval is used as information in a similar way. This information needs to be integrated with the content of information retrieved – without considering *what* it is that comes to mind more or less easily, any judgment on the basis of subjective ease would be meaningless. The same reasoning applies to the use of amount recalled as information. Without considering *what* was retrieved in a larger or smaller amount, the amount of information itself does not provide a meaningful basis for judgment. Although this may seem obvious, it is important to emphasize that content always matters in the judgments of both high-RMA and low-RMA groups, and that it is somewhat misleading to call one of these strategies "relying on content" as opposed to "relying on ease," as do Grayson and Schwarz (1999).

While these considerations imply that the strategies used by both low-RMA and high-RMA groups may be similar in requiring a more-than-negligible amount of cognitive effort, they offer no explanation for why our result pattern diverges from that obtained by Grayson and Schwarz (1999). Of course it is possible that one set of findings may have come about by chance. Although effect sizes are somewhat higher in the studies reported by Grayson and Schwarz (e.g., for the three-way interaction of RMA, direction and number in their Study 1, we find $\eta = .29$, compared to $\eta = .21$ for the respective effect under no time pressure in the present data), each set of results is clearly significant. It therefore seems expedient to discuss possible mechanisms that might be responsible for the divergence in findings.

One possibility is that our instructions in the no-time-pressure conditions to "consider this question thoroughly and answer as accurately as possible" induced the motivation to apply a judgmental correction to any judgment that spontaneously came to mind. In other words, participants' initial, spontaneous impressions might have produced judgments similar to those observed by Grayson and Schwarz (1999), but rather than expressing these initial impressions, our participants may have judged more negatively if the initial impression was positive and vice versa (cf. Martin & Stapel, 1998). A finding that seems compatible with this interpretation is the interaction of time pressure and RMA observed in the control conditions – i.e., even in the absence of previously generated examples, judgments may have been corrected away from an initial tendency in the conditions where no time pressure was induced. This tentative explanation could be tested in future research by introducing more explicit warnings or instructions to correct for unwanted influences.

Another possibility is that the manipulation check question referring to perceived ease, which preceded the question about perceived vulnerability, may have induced a subtle experimental demand that judging by perceived ease vs. difficulty was the strategy that the researchers expected to be used. In the studies by Grayson and Schwarz (1999) this demand may have been stronger, because the two questions seem to have been presented in direct succession, whereas in our present design, they were separated by one screen that provided a new lead-in. If our interpretation is correct, women low (vs. high) in RMA in Grayson and Schwarz' studies may have been less willing to follow the experimental demand, which may have caused them to judge by the amount of information recalled instead. This interpretation is of course highly speculative and would need to be investigated systematically, for example by varying the order in which judgments of perceived difficulty and of vulnerability are assessed.

A final aspect of our results worth noting is the finding that women low (vs. high) in RMA reported greater difficulty overall in generating examples of rape-related behaviors. This casts doubt on the assumption that women low (vs. high) in RMA may have elaborated more on such behaviors in the past (Grayson & Schwarz, 1999), but in line with our alternative suggestion that women low (vs. high) in RMA may generally refuse considering women's behavior as a potential risk factor. In sum, our results seem to raise many questions about the conceptualization of judgments by ease of retrieval within a heuristic-systematic processing framework. In the absence of clear experimental evidence that this processing strategy is more likely to be used than other, presumably more effortful, strategies under conditions of low motivation or capacity, it seems to be premature to draw any firm conclusions about the effort expended by women low vs. high in RMA when judging the risk of rape.

An important feature of the present study that sets it apart from previous research using the ease-of-retrieval paradigm introduced by Schwarz et al. (1991) was our use of the Internet. Thus, we were able to reach a highly diverse sample of participants living in different parts of the world in an economic way within a relatively short period of time. In spite of this diversity and apparent lack of control, our results suggest that it is feasible to apply a time pressure manipulation via the Internet, and to use response time measurement to assess its effectiveness (for related findings see McGraw, Tew, & Williams, 2000). As a by-product of our study, Internet versions of the short Illinois Rape Myth Acceptance Scale were developed and tested in three languages. All three

versions of the IRMA scale proved to be satisfactory to good in reliability, and we are planning to continue using and validating them in future research.

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Gerd Bohner

Professor of Social Psychology
Department of Psychology
University of Bielefeld
Postbox 100131
D-33501 Bielefeld
Germany
Tel.: +49 521 106 4437
Fax: +49 521 106 6422
E-mail: gerd.bohner@uni-bielefeld.de