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Expert Psychological Testimony in Rape Trials: A Social-Cognitive Analysis

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In three studies with undergraduate subjects, we investigated juror use of group probability data (base rates) in the form of expert scientific testimony in a simulated rape trial. In Study 1 we hypothesized that juror use of such evidence would be influenced by the type and timing of the expert testimony presented. As predicted, jurors made the most use of the expert testimony when it was linked explicitly to the case under consideration and presented early in the trial. Study 2 demonstrated that the observed effects were not attributable to differences in the duration of the expert testimony or to enhanced recall and suggested that use is facilitated by concrete rather than abstract presentations of expert testimony. In Study 3 juror use of expert testimony was further examined by analyzing jury deliberations from Study 1. Results show that although juries hardly discussed the expert testimony itself, the expert testimony influenced interpretations of case facts.

Over the past decade, research in social cognition on reasoning has highlighted inferential shortcomings and deficiencies. A wide variety of inferential errors and biases have been demonstrated, many if not most of which can be traced to an overreliance on heuristic thinking and theory-driven processing (Fiske & Taylor, 1984; Kahneman, Slovic, & Tversky, 1982; Nisbett & Ross, 1980; Sherman & Corty, 1984). On the basis of such findings, irrationality in human inference processes has been hotly debated (e.g., Cohen, 1981). More recently, however, some researchers have advocated a shift in focus away from cataloguing such errors toward considering task features and perceiver characteristics that may influence reasoning processes (Markus & Zajonc, 1985; Showers & Cantor, 1985; Srull & Wyer, 1986). The influence of these characteristics has been demonstrated empirically (Bargh, Bond, Lombardi, & Tota, 1986; Borgida & Howard-Pitney, 1983; Harkness, DeBono, & Borgida, 1985; Nisbett, Krantz, Jepson, & Kunda, 1983; Tversky & Kahneman, 1983).

Perhaps exemplary of this shift has been the course of research on the base rate fallacy: the tendency to underutilize base rates or distributional information in favor of singular or individuating information on various judgment and attribution tasks (Borgida & Brekke, 1981). Following the publication of a provocative set of experiments by Kahneman and Tversky (1973), numerous studies were conducted to document the prevalence of this bias and its often deleterious consequences for social judgment and decision making. It soon became clear, however, that a more fruitful approach was to study the conditions under which base rates or group probability data are likely to affect judgments about individuals. It is now evident that a variety of task and perceiver characteristics influence when base rates are likely to be used (Borgida & Brekke, 1981; Kassin, 1979; Sherman & Corty, 1984).

This more contextual approach to social cognition emphasizes the task-specific and goal-oriented nature of social information processing and further underscores the need for studies of reasoning processes that systematically take into account contextual and perceiver variables. Accordingly, a complete understanding of the influence of base rates on social judgment requires the investigation of how group probability data are used in different tasks, including some that one could expect to encounter in more naturalistic settings.

Consistent with such a perspective, our research was designed to investigate the use of group probability data in a comparatively applied, policy-relevant context. Specifically, we examined the use of expert scientific testimony in a jury simulation. The introduction of expert scientific testimony in court represents one naturalistic context in which people are increasingly confronted with group data (i.e., scientific generalizations based on groups of respondents) and are then asked to make judgments about specific individuals involved in the trial.

In rape cases, for example, prosecutors have had a difficult time obtaining convictions, even though they have tried to bring only the strongest cases to trial. The issue in rape trials too often becomes one of "his word against hers," and jurors seem reluctant to render guilty verdicts in the absence of hard facts. More-

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over, the general public maintains numerous myths (Burt, 1980), stereotypes (Heilbrun, 1980), and misconceptions (Feild & Bienen, 1980) about the phenomenon of rape that may adversely affect perceptions of the rape victim's credibility in court (Borgida & Brekke, 1985). For example, although research indicates that in about 40% of all rapes the victim is at least casually acquainted with her assailant (Amir, 1971; National Commission on the Causes and Prevention of Violence, 1969; National Institute of Law Enforcement and Criminal Justice, 1978), it is nevertheless commonly believed that true rape involves strangers. Similarly, people often view a woman's reluctance to go to the police as being indicative of a fabricated rape report, despite the fact that only an estimated 10–30% of all rapes are ever reported to the police (Amir, 1971; National Criminal Justice Information and Statistics Service, 1979).

In order to counteract the effects of these misconceptions and to corroborate the victim's testimony, attorneys have begun to introduce scientific expert testimony on behalf of the prosecution in rape cases (Ross, 1983; Rowland, 1979, 1985). The expert may testify, for example, that most women are reluctant to report that they have been raped. It is then left up to the individual juror to decide what implications these group data have for their judgments about the victim and the defendant in the case at hand. Do jurors make use of such group data when making judgments about individuals in rape trials? Can one structure the presentation of such evidence to facilitate its use? These questions are the focus of this research.

Research in social cognition suggests at least two factors that should affect the extent to which group data influence judgments about individuals. First, research on the base rate fallacy indicates that group probability data are more likely to be used when they are made relevant to the specific judgment context (Bar-Hillel, 1980), causally relevant (Ajzen, 1977), or more concrete (Manis, Dovalina, Avis, & Cardoze, 1980). It appears that jurors may find it difficult to grasp the connection between data about groups and judgments about specific individuals unless the link is somehow made fairly explicit.

Second, research on belief perseverance suggests that once beliefs and causal theories are formed, their influence on subsequent judgments is difficult to disconfirm (Lord, Ross, & Lepper, 1979; Ross & Anderson, 1982; Ross & Lepper, 1980). Therefore, one may expect that once jurors have interpreted the case facts in a courtroom trial in particular ways (e.g., once they have decided that the victim is lying or that the victim deserved to be raped), scientific evidence about rape victims in general may have relatively little impact on subsequent juror judgments. Accordingly, we hypothesized that jurors would make the most use of group data in the form of expert testimony when it was presented early in the trial proceedings and when it was linked explicitly to the case at hand.

Study 1

Method

Subjects

Subjects were 208 undergraduates (98 men and 110 women) enrolled in general psychology classes at the University of Minnesota who volunteered to participate in the experiment in return for extra course credit.

Design

Type of expert testimony (standard expert and specific hypothetical) and timing of presentation (expert first and expert last) were varied in a 2×2 factorial design with an independent no-expert-testimony control group. Six mock juries of balanced sex composition were randomly assigned to each of the five cells, thereby facilitating the assessment of sex of juror effects.[1,2]

Procedure

Four men and 4 women were allowed to sign up for each session. Those who arrived at the scheduled time were seated around a table and given consent forms explaining the nature of the experimental task. The experiment was described as a study of jury decision making in which jurors would be asked to listen to an audiotaped reenactment of a rape trial, deliberate to a unanimous verdict, and complete a final questionnaire. After signing consent forms, jurors were given copies of a brief trial outline designed to help them keep track of the various witnesses as they listened to the case. The experimenter then instructed jurors not to talk or take notes during the trial and started the tape. The experimenter remained in the room throughout the presentation of the trial stimulus.

The duration of the trial varied according to the type of expert testimony presented (no-expert-testimony control, 65 min; standard expert, 84 min; and specific hypothetical, 102 min), but all versions included the following elements: opening remarks from the judge; opening arguments from the prosecuting and defense attorneys; the victim's testimony and cross-examination; two prosecution witnesses (both of whom were cross-examined); the defendant's testimony and cross-examination; closing arguments; and the judge's final charge to the jury.

In the no-expert-testimony control condition, the trial consisted of an abbreviated, audiotaped version of the *State of Minnesota v. McNamara* case previously used by Borgida (1981; Borgida & White, 1978). In that case the defendant was charged with third-degree sexual misconduct for allegedly forcing the complainant to have sexual intercourse against her will. The basic fact pattern exemplifies what is commonly referred to as casual acquaintance rape. Both parties knew each other prior to the evening in question and both agreed that sexual intercourse had taken place. The major point of dispute was over the issue of consent.[3]

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[1] Thirty-three juries were originally run, but 3 juries (16 subjects) were dropped from the study because of the following irregularities: (a) unbalanced sex composition, (b) failure to follow deliberation instructions properly, and (c) disruption of a session by a juror who left in the middle of the deliberation.

[2] In light of previous research involving rape cases that indicated sex differences in juror judgments, it was considered important to balance the sex composition of deliberating juries. In order to accomplish this, it was sometimes necessary to reduce the jury size. The final sample consisted of twenty 6-person juries and ten 4-person juries. Because jury size did not appear to be related to jury verdicts, $\chi^2(2, N = 30) = 2.93$, $p = .23$, this variable was ignored in data analysis and will not be discussed further.

[3] Briefly, the case scenario was as follows: The victim and the defendant, casual acquaintances who had attended some of the same parties in the past, ran into each other at their local bar one evening. The victim had a few hours to kill before meeting her boyfriend, so she agreed to accompany the defendant to a friend's trailer to play Foosball for awhile. On their way out of the trailer park after playing Foosball, the defendant parked the car in a dead end on a dark street and they had sexual intercourse. The victim charged that she had been raped; the defendant contended that the victim had been a willing participant.

The victim testified that she had made it clear to the defendant that

In addition to the basic trial presented in the control condition, jurors in experimental cells heard one of two versions of expert testimony, in which the expert was either the first prosecution witness (expert first) or the last prosecution witness (expert last). The expert was identified as a psychiatrist from the university who had conducted research in the area of rape, counseled rapists and rape victims, and taught courses in human sexuality and sexual dysfunction.

In the standard expert condition, the prosecuting attorney asked a series of leading questions that enabled the expert to dispense his testimony in essentially a lecture-style format. The expert addressed himself to the low level of public awareness regarding sexual assault and attempted to debunk a number of widely held misconceptions about rape. For example, he testified that (a) few women falsely accuse men of rape, (b) rape is a highly underreported crime, (c) a large proportion of rapes involves casual acquaintances of the victim rather than strangers, (d) rape is a crime of violence rather than a crime of passion, and (e) it is often better for a woman to submit than to risk the additional violence that could result from ineffective attempts to fight back. On cross-examination the expert admitted that he was being paid for testifying by the county and that he had discussed the facts of the case with the prosecuting attorney prior to his court appearance.

In the specific hypothetical condition, jurors listened to the standard expert testimony, followed by an explicit attempt to point out the connection between the expert testimony and the case under consideration. The prosecuting attorney posed a legally permissible hypothetical example to the expert into which the critical features of *State of Minnesota v. McNamara* had been incorporated. The expert was then asked to comment on the hypothetical victim's behavior in light of the research literature on rape. The expert highlighted features of the hypothetical example that were typical of casual acquaintance rapes and argued that the victim's behavior was easily understandable when viewed in light of scientific knowledge regarding typical behavior in such circumstances. Note that the inclusion of the hypothetical example provided no additional information beyond that already given to jurors; it merely enabled the expert to comment more directly on the case at hand. For a comparison of standard expert and specific hypothetical expert testimony, see Appendix A.

On completion of the trial tape, extra jurors were excused from the session so that all deliberating juries would be composed of equal numbers of men and women.[4] To expedite the deliberation process, the experimenter chose a jury foreperson[5] from among the remaining jurors and gave that person a list of deliberation instructions to be read aloud to the rest of the jury. The experimenter then left the room. Prior to discussing the case, jurors filled out predeliberation ballots indicating their beliefs as to the guilt or innocence of the defendant. The foreperson tallied the votes and set them aside in a sealed envelope. In juries in which the straw vote was unanimous, the supervisor called the experimenter and she distributed the final questionnaire. All other juries proceeded to deliberate, governed by a unanimous decision rule. If agreement had not been reached after 30 min, the experimenter terminated the discussion, a final vote was taken, and the jury was classified as hung. All deliberations were audiotaped.

After the deliberation each juror completed a final questionnaire assessing verdict, recommended sentence, and evaluations of various trial characters and pieces of evidence. Jurors were instructed to fill out the questionnaires independently, without conferring with their fellow jurors. As soon as the entire jury finished their questionnaires, jurors were debriefed and excused from the experiment.

Juror Questionnaire

Verdict. Jurors rendered verdicts (guilty or not guilty of rape) both prior to deliberating (predeliberation verdict) and after deliberating (final verdict).

Sentence. Jurors chose the sentence that they would recommend for the defendant from six alternatives, ranging in severity from "*Allow him to go free*" to "*Five to ten years in prison.*"

Likelihood of consent. The likelihood that the victim voluntarily agreed to have sex with the defendant was rated on a 10-point scale (1 = *not at all likely that she agreed,* 10 = *very likely that she agreed*).

Attributions of responsibility. Jurors rated the defendant on 4 items and rated the victim on 13 items concerning the extent to which each of them was responsible for, to blame for, or had control over the events on the night in question. Each juror's responses were averaged across the items in each set separately, resulting in two scale scores: victim responsibility (Cronbach's alpha = .93) and defendant responsibility (Cronbach's alpha = .83). Values on both scales ranged from 1 to 7, with higher values indicating greater attributed responsibility or blame.

Evaluation of the litigants. Jurors rated both the defendant and the victim on a set of 24 bipolar adjectives. Principal factoring with iterations (varimax rotation) of each adjective set yielded two factors for each litigant with eigenvalues greater than 1.0. For each of these factors, items with substantial loadings were unit weighted and averaged for each juror, resulting in the following four scales (scale values ranged from 1 to 7, with higher values indicating greater amounts of the attribute): (a) victim credibility (e.g., credible–not credible, honest–dishonest); (b) victim's moral character (e.g., respectable–unrespectable, moral–immoral); (c) defendant credibility (e.g., credible–not credible, honest–dishonest); and (d) defendant likability (e.g., likable–dislikable, very desirable as a friend–not at all desirable as a friend). Cronbach's alphas for the four scales were .95, .81, .93, and .89, respectively.

Usefulness of the expert testimony. Jurors exposed to expert testimony rated the extent to which each of 13 points made by the rape expert were useful to them in reaching their verdicts (0 = *not at all useful—did not influence my verdict,* 5 = *very useful—influenced my verdict*). Responses to the items (e.g., "Most women are reluctant to

she was not interested in any sort of sexual involvement with him and that she had repeatedly asked the defendant to take her home. She claimed that only after resisting to the point of extreme fatigue and only out of fear for her well-being did she finally submit.

The prosecution introduced two additional witnesses. First, the victim's girlfriend testified that the victim arrived at her house, appearing very upset and claiming to have been raped, but that no direct evidence of physical harm to the victim was visible at that time. She eventually took the victim to the bar to get her boyfriend, to the police to report the incident, and finally to the hospital for a physical examination. Second, the examining physician from the hospital testified that there was physical evidence that intercourse had occurred but that there was no way of knowing whether it had been consensual. He further testified that there was a 5-in. bruise on the victim's left temple and a small cut on her lip.

The defendant's only witness was the defendant himself, who testified that the victim had been a willing participant in the sexual encounter and that she did not appear to be injured or upset when he dropped her off at her girlfriend's house.

[4] Extra jurors were taken to another room where they completed the final questionnaire, were debriefed, and then were excused from the experiment. Nondeliberating jurors were included only as backups, to ensure deliberating juries of proper size and sex composition. Thus, the responses of these 32 jurors were not analyzed and will not be discussed further.

[5] The person seated at the head of the table was appointed foreperson by the experimenter unless that chair happened to be empty. In those instances, another juror was chosen at random.

report rape, it is not an indication that they are lying") were averaged for each juror (Cronbach's alpha = .90).

Usefulness of the hypothetical. Jurors in the specific hypothetical condition also rated the extent to which each of seven aspects of the expert's comments regarding the hypothetical example were useful to them in reaching their verdicts. Responses to these items were averaged for each juror (Cronbach's alpha = .89). Scale values ranged from 0 to 5, with higher numbers indicating greater perceived usefulness.

Evaluation of the attorneys and the expert witness. Each attorney and the expert witness were rated on a set of six bipolar adjectives: competent–incompetent, respectable–not respectable, believable–not believable, convincing–unconvincing, honest–dishonest, and interesting–boring. Responses to the items in each set were averaged for each juror, yielding an overall evaluative scale score for each of the above trial characters (Cronbach's alphas = .86, .81, and .84 for evaluations of the defense attorney, the prosecuting attorney, and the expert witness, respectively). Scale values ranged from 1 to 7, with higher numbers indicating more favorable evaluations.

Interest in the trial. Jurors rated their level of interest in the trial on a 6-point scale (1 = *extremely disinterested,* 6 = *extremely interested*).

Results

Analysis of Juror Questionnaire Data

Predeliberation verdicts were analyzed by means of a 2 (sex of juror) × 5 (specific hypothetical-expert first, specific hypothetical-expert last, standard expert-expert first, standard expert-expert last, and no-expert-testimony control) analysis of variance (ANOVA) in which effects were partitioned into nine orthogonal, single degree of freedom contrasts. Six effects involved comparisons among groups exposed to expert testimony: (a) a type of expert testimony main effect (i.e., specific hypothetical vs. standard expert); (b) a timing of presentation main effect (i.e., expert first vs. expert last); (c) a Type × Timing interaction; (d) a Type × Sex of Juror interaction; (e) a Timing × Sex of Juror interaction; and (f) a Type × Timing × Sex of Juror interaction. Three effects involved the entire sample, including no-expert-testimony controls: a sex of juror main effect, a treatment main effect (i.e., expert testimony vs. no expert testimony), and a Sex of Juror × Treatment interaction.

The remaining dependent measures were completed following deliberations, thus creating nonindependence due to groups (Kenny & Judd, 1986). Accordingly, all postdeliberation measures were analyzed by means of a 2 (sex of juror) × 5 (condition) × 6 (jury, nested within condition) ANOVA (cf. Anderson & Agar, 1978).[6] Effects were again partitioned as described earlier.

Sex of Juror Effects

Table 1 shows the sex of juror main effects for each of the dependent measures. As expected, women were more favorably disposed toward the victim and less favorably disposed toward the defendant on a variety of measures. Women rendered significantly more guilty verdicts than did men, although this sex difference was somewhat less pronounced after deliberation. Women attributed significantly less responsibility to the victim, considered it significantly less likely that she consented to have sex, and showed a marginally significant tendency to attribute more responsibility to the defendant. Female jurors also viewed the victim as being a significantly more credible witness and the defendant as being a significantly less credible witness than did male jurors.

Male and female evaluations of the litigants did not differ on all dimensions, however. Both sexes rated the victim's moral character equally, viewed the defendant as being equally likable, and recommended comparable sentences. Similarly, although there was a significant tendency for women to evaluate the prosecuting attorney more favorably, there were no sex differences in ratings of the defense attorney or in general levels of interest in the trial.

Some of the strongest sex differences emerged on evaluations of the expert witness and his testimony. Compared with men, women evaluated the rape expert more favorably and considered his testimony to be more useful to them in reaching their verdicts. Female jurors exposed to specific hypothetical expert testimony also rated the information conveyed via the hypothetical example as being significantly more useful to them than did their male counterparts.

Although women appeared to be somewhat more accepting of the expert testimony and its implications than were men, the pattern of evidentiary use was generally consistent across the sexes. There was only one significant interaction: a Sex of Subject × Timing × Type of Testimony interaction on ratings of the victim's credibility, $F(1, 25) = 4.50$, $p = .04$. Newman-Keuls contrasts indicated that women in the specific hypothetical-expert first cell perceived the victim to be significantly more credible than did all other jurors.

Type of Expert Testimony Effects

As predicted, the type of expert testimony to which jurors were exposed significantly affected their judgments on a wide range of measures. Table 2 shows a summary of these effects. Jurors exposed to the specific hypothetical expert testimony were significantly more likely to vote for conviction (both prior to and after deliberating) and to recommend harsher sentences than were jurors who received standard expert testimony. These two groups also differed significantly in their perceptions and evaluations of the litigants. Compared with jurors who heard standard expert testimony, those who heard the specific hypothetical example considered it significantly less likely that the victim consented to have sexual intercourse with the defendant and viewed her as being more credible. There was also a marginal tendency for specific hypothetical jurors to view the victim as being more moral and to attribute less responsibility to her for the events on the night in question. The defendant, by contrast, was viewed as being significantly less credible and less likable by specific hypothetical jurors.

Type of expert testimony did not, however, influence juror

[6] Evaluation of the expert and usefulness of the expert testimony ratings were not, of course, made by no-expert-testimony control jurors and thus were analyzed by means of a 2 (sex of juror) × 2 (type of testimony) × 2 (timing of presentation) × 6 (jury, nested within type by timing) analysis of variance (ANOVA). Similarly, the usefulness of the hypothetical ratings were made only by specific hypothetical jurors and thus were analyzed by means of a 2 (sex of juror) × 2 (timing of testimony) × 6 (jury, nested within timing) ANOVA.

Table 1
Mean Sex of Juror Effects: Study 1

Dependent variable	*df*	*F*	*p*	Women	Men
Predeliberation verdict (proportion guilty)	1, 150	9.98	.002	.64	.40
Final verdict (proportion guilty)	1, 25	7.60	.01	.49	.38
Sentence	1, 25	0.26	.61	3.10	2.91
Likelihood of consent	1, 25	5.37	.03	4.73	5.51
Victim responsibility	1, 25	10.88	.003	3.49	4.02
Defendant responsibility	1, 25	3.98	.06	4.98	4.69
Victim credibility	1, 25	7.20	.01	4.19	3.77
Victim's moral character	1, 25	0.74	.40	3.51	3.61
Defendant credibility	1, 25	4.18	.05	3.67	4.01
Defendant likability	1, 25	2.18	.15	3.38	3.65
Evaluation of expert	1, 20	23.54	.0001	6.43	5.86
Usefulness of expert testimony	1, 20	8.66	.008	2.74	2.26
Usefulness of hypothetical	1, 10	23.88	.0006	3.80	2.81
Evaluation of prosecutor	1, 25	5.38	.03	5.12	4.80
Evaluation of defense attorney	1, 25	0.08	.78	4.88	4.88
Interest in trial	1, 25	0.35	.56	4.95	4.91

perceptions of all aspects of the trial. Evaluations of the attorneys and the expert witness as well as judgments regarding the defendant's responsibility, the usefulness of the expert testimony, and jurors' general level of interest in the trial remained constant, regardless of the type of expert testimony that jurors heard.

Table 2
Type of Expert Testimony Effects: Study 1

Dependent variable	*df*	*F*	*p*	Specific hypothetical	Standard expert
Predeliberation verdict (proportion guilty)	1, 150	3.89	.05	.64	.47
Final verdict (proportion guilty)	1, 25	4.84	.04	.65	.38
Sentence	1, 25	8.12	.009	3.69	2.56
Likelihood of consent	1, 25	9.27	.005	3.94	5.90
Victim responsibility	1, 25	3.37	.08	3.27	3.92
Defendant responsibility	1, 25	2.74	.11	5.20	4.69
Victim credibility	1, 25	5.83	.02	4.51	3.75
Victim's moral character	1, 25	3.31	.08	3.91	3.41
Defendant credibility	1, 25	7.50	.01	3.34	4.20
Defendant likability	1, 25	4.23	.05	3.16	3.68
Evaluation of expert	1, 20	0.82	.38	6.21	6.07
Usefulness of expert testimony	1, 20	0.27	.61	2.52	2.47
Evaluation of prosecutor	1, 25	0.39	.54	5.23	5.08
Evaluation of defense attorney	1, 25	0.29	.59	4.83	4.96
Interest in trial	1, 25	2.14	.16	4.83	5.09

Table 3
Dunnett Comparisons of Expert Testimony Cells With the No-Expert-Testimony Control Cell

	Specific hypothetical		Standard expert		No-expert-testimony control
Dependent variable	First	Last	First	Last	
Predeliberation verdict (proportion guilty)	.72*	.56	.44	.50	.39
Final verdict (proportion guilty)	.75**	.56*	.38	.38	.11
Sentence	4.32**	3.12	2.44	2.60	2.55
Likelihood of consent	3.41*	4.44	6.25	5.46	6.03
Victim responsibility	2.92**	3.62	4.10	3.69	4.35
Defendant responsibility	5.32*	5.09	4.62	4.78	4.40
Victim credibility	4.86**	4.17	3.76	3.74	3.38
Victim's moral character	4.06*	3.76	3.48	3.32	3.13
Defendant credibility	3.13*	3.54	4.25	4.13	4.20
Defendant likability	2.99*	3.33	3.70	3.65	3.88
Evaluation of prosecutor	5.48**	5.11	5.00	5.05	4.27
Evaluation of defense attorney	4.42	5.21	5.19	4.68	4.84
Interest in trial	4.78	4.88	5.13	5.04	4.86

* $p < .05$. ** $p < .01$.

Timing of Presentation

The only effect for timing of presentation was a marginally significant effect on specific hypothetical jurors' judgments regarding the usefulness of the information conveyed in the hypothetical example, $F(1, 10) = 4.01$, $p = .07$. This information was considered to be more useful when the expert was the first prosecution witness ($M = 3.57$) than when he was the last ($M = 3.08$).

There were no significant Timing of Presentation × Type of Testimony interaction effects on any of the dependent measures.

Expert Testimony Effects

Comparisons between jurors exposed to expert testimony and no-expert-testimony control jurors revealed statistically significant main effects on 5 of the 13 total-group dependent measures. Effects on 4 additional measures approached significance ($p < .10$), and means on all but 1 of the measures were in the expected direction. Thus, there is some evidence that, on the average, the inclusion of expert testimony affects juror judgments. It is of greater interest, however, to determine which modes of expert testimony presentation are effective. Accordingly, Dunnett comparisons of each of the four expert testimony conditions with the no-expert-testimony control were computed for each of the dependent measures. Results reveal a highly consistent pattern (see Table 3). The expert testimony with the greatest impact was always the specific hypothetical version when it was presented as the first piece of prosecution evidence. Contrasts between specific hypothetical-expert first and control jurors were significant on all measures except evalu-

ations of the defense attorney and interest in the trial. Specific hypothetical-expert last responses differed significantly from control responses only on the final verdict, and the standard expert testimony groups never differed from controls.

Discussion

The inclusion of expert scientific testimony on behalf of the prosecution does appear to counteract the otherwise pervasive effects of rape myths and misconceptions on juror judgments in a simulated rape trial. When such testimony was linked directly to the case by means of a hypothetical example and presented early in the trial, it resulted in higher conviction rates, harsher recommended sentences, and more favorable perceptions of the victim. These effects were obtained despite the finding that jurors did not rate the expert testimony as being particularly useful to them in reaching their verdicts. These results suggest that jurors do make use of group data in the form of expert scientific testimony to make judgments about individuals when such information is linked directly to the case under consideration and presented before beliefs and causal theories to the contrary have had the opportunity to develop.

One could question, however, whether it was actually the specific hypothetical technique and its presumed linking function that accounted for the increased use of the expert testimony. Other features of the specific hypothetical expert testimony could have accounted for its observed effects. It may be recalled, for example, that duration of the trial stimulus and type of expert testimony were confounded. Thus, it could be that the observed effects were due to the length differences and any associated demand characteristics rather than to the testimony itself.

The specific hypothetical technique also gave the prosecuting attorney an additional opportunity to present his version of the case facts and to reinforce various aspects of the expert's testimony. It is thus possible that the superiority of the specific hypothetical testimony was due to differential recall of case facts or the expert testimony itself rather than the hypothetical highlighting the connection between the expert testimony and the case under consideration.

Finally, the specific hypothetical expert might have conveyed something beyond the simple relevance of the group data to the individual case. Although the specific hypothetical expert did not comment directly on the victim's credibility, nor did he try to convey sympathy for the victim, jurors might have inferred from his comments about the hypothetical victim that he believed the actual victim. Thus, he could have influenced jurors' judgments without necessarily increasing their reliance on the scientific evidence he presented.

We conducted Study 2 to examine these alternative explanations and to further clarify the nature of the specific hypothetical technique's influence.

Study 2

To assess the viability of the recall explanation, we replicated the specific hypothetical-expert first and standard expert-expert first cells from Study 1, including measures of recognition recall. To the extent that the influence of the specific hypothetical technique is due to its recall-enhancing properties, specific hypothetical jurors should exhibit better recall of the case and the expert's testimony than should standard expert jurors.

In order to evaluate the length explanation, we constructed another version of the *State of Minnesota v. McNamara* trial. Specifically, we lengthened the standard expert version of the case in three ways: (a) The expert was allowed to elaborate considerably on his credentials; (b) the expert prefaced his answers to some questions about the research literature with reinforcing phrases such as "Yes, now here's something we know a lot about"; and (c) the expert decreased slightly the speed at which the testimony was delivered. This *length control* version of the trial was thus equal in duration to the specific hypothetical version but lacked the hypothetical example linking the testimony to the case under consideration. If mere length of the expert testimony is an important determinant of its effects, the longer length control and specific hypothetical expert testimony should have a significantly greater impact than the shorter standard expert testimony.

In order to further clarify the means by which the specific hypothetical technique influences juror judgments, we constructed another version of the expert testimony. The *concrete control* version of expert testimony was constructed by interspersing a number of concrete examples throughout the standard expert testimony (see Appendix B).[7] This concrete expert testimony, although lacking the hypothetical example, should serve to make more obvious the relevance of the abstract group-level data to the case at hand. In addition, it should establish this link without implying that the expert is sympathetic to the alleged victim's claims. Thus, to the extent that the specific hypothetical also functions by highlighting the link between the scientific evidence and the case at hand (i.e., by rendering the abstract scientific evidence more concrete), the concrete control and specific hypothetical versions of expert testimony should be equally effective.

Method

Subjects

Subjects were 144 undergraduates (72 men and 72 women) enrolled in general psychology classes at the University of Minnesota who volunteered to participate in the experiment in return for extra course credit.

Design

Study 2 was a partial replication and extension of Study 1. Type of expert testimony (standard expert, specific hypothetical, length control, and concrete control) was varied between subjects. Timing of presentation was held constant; the expert was always presented as the first prosecution witness. As in Study 1, six 6-person juries of balanced sex composition were randomly assigned to each of the four conditions, thereby facilitating the examination of sex of juror effects.

[7] The concrete control version of expert testimony also included the elaborated credentials used in the length control condition. Although this manipulation was not optimal, it was nonetheless interpretable, because these credentials were not associated with increased use of expert testimony in the length control expert testimony (see the Results section).

Procedure

The procedure in Study 2 was virtually identical to that used in Study 1.[8] Each juror listened to one of four versions of the *State of Minnesota v. McNamara* trial, which varied according to the type of expert testimony introduced at the beginning of the prosecution's case.

Dependent Measures

In addition to the dependent measures from Study 1, jurors in Study 2 answered 14 multiple-choice questions designed to assess their recall of the case. Answers were scored (1 = *correct*, 0 = *incorrect*) and combined to form the following two indexes of recognition recall: expert testimony and case facts.

Recall of expert testimony. Responses to the 5 items that focused on the expert testimony (e.g., "Dr. Rowland, the rape expert, testified that _____ percent of all rapes are committed by acquaintances of the victim: less than 10%, 10–25%, 26–50%, over 50%") were averaged, thus indexing the proportion of expert testimony items correctly recalled by each juror.

Recall of case facts. The remaining 9 items concerned specific case facts (e.g., "Cheryl's testimony mentioned the acquisition of a bruise on her left cheek on the night in question. Which witnesses verified the existence of this bruise? Pat Anderson; Dr. Aronson; Dr. Rowland, the rape expert; Dr. Aronson and Pat Anderson"). Again, responses were averaged for each juror, yielding an index of the proportion of case fact questions correctly answered.

Results[9]

Recall Effects

Contrary to the recall explanation, there were no significant differences between specific hypothetical (M = .68, SD = .15) and standard expert jurors (M = .72, SD = .12) in recognition recall of case facts, $F(1, 20) = 1.28$, $p = .27$, nor were there significant differences between specific hypothetical (M = .88, SD = .17) and standard expert jurors (M = .92, SD = .12) in recognition recall of expert testimony, $F(1, 20) = 0.59$, $p = .45$. Correlations between final verdicts and recall of case facts, $r(71) = -.07$, $p = .28$, and recall of expert testimony, $r(71) = -.04$, $p = .36$, were also nonsignificant.

Length of Expert Testimony Effects

A planned comparison between the longer (specific hypothetical and length control) versions of expert testimony and the shorter (standard expert) version was computed for each of the dependent measures. To the extent that the differential impact of the specific hypothetical and standard expert testimony in Study 1 was due to the duration of the expert testimony rather than to its content, longer expert testimony should be associated with greater use than shorter expert testimony. Contrary to this explanation, the planned comparison was nonsignificant on all dependent measures.[10]

Concrete Versus Abstract Expert Testimony

A planned comparison between the concrete (specific hypothetical and concrete control) expert testimony and the more abstract (standard expert and length control) expert testimony was computed for each of the dependent measures. Results of this contrast as well as the means for each condition separately are presented in Table 4. As predicted, this contrast was significant on most dependent measures; concrete expert testimony had more impact on juror judgments than did abstract expert testimony. Compared with jurors exposed to abstract expert testimony, jurors exposed to concrete expert testimony viewed the victim as being significantly more credible and moral, as well as significantly less responsible for the event and less likely to have consented. They viewed the defendant, instead, as being significantly more responsible and less credible. Concrete expert testimony jurors also rendered significantly more guilty predeliberation verdicts, and means on postdeliberation verdicts and sentence, although not significant, were in the expected direction.

Residual contrasts were, by and large, nonsignificant. That is, the specific hypothetical and concrete control cells differed significantly only on evaluations of the defense attorney, $F(1, 20) = 4.91$, $p = .04$. Evaluations of the defense attorney by specific hypothetical jurors were significantly more negative than evaluations by concrete control jurors. The standard expert and length control cells differed significantly on only three measures. Compared with standard expert jurors, length control jurors were less interested in the trial, $F(1, 20) = 5.13$, $p = .03$; they evaluated the expert witness less favorably, $F(1, 20) = 9.36$, $p = .006$; and they exhibited poorer recall of the expert's testimony, $F(1, 20) = 7.21$, $p = .01$.

Discussion

It appears that neither the recall nor the length explanations are sufficient to account for the superior use of specific hypo-

[8] The only differences in procedure were minor: First, to ensure juries of sufficient size and proper sex composition, up to 6 subjects of each sex were allowed to sign up for each experimental session. Thus, if attendance rates happened to be high, subjects might have listened to the trial in slightly larger groups than in Study 1. Because subjects always deliberated in groups of 6, however, there was no reason to expect that this change in procedure would have had systematic effects on subjects' responses. Second, because of an oversight by an experimenter, trial outlines were not made available to subjects as they listened to the trial. Although the precise effects of this omission are unknown, it seems unlikely that they would have interacted with the type of expert testimony presented and thus were assumed to be constant across experimental conditions.

[9] The sex of juror effects in Study 2 are generally consistent with those obtained in Study 1, so they will not be discussed in detail. Compared with men, women were significantly more conviction prone, they evaluated the expert and the victim more favorably, and they evaluated the defendant less favorably.

[10] Even though we made no explicit attempt to equate the duration of the concrete control version with the specific hypothetical version, both were approximately equal in duration. It could be argued, therefore, that the planned comparison should have included the concrete control cell as well (i.e., specific hypothetical, length control, and concrete control vs. standard expert). Results of this comparison were essentially equivalent. The contrast was nonsignificant on all but two dependent variables: (a) defendant credibility, $F(1, 20) = 4.62$, $p = .04$, in which longer expert testimony was associated with lower ratings (M = 3.68) than was shorter expert testimony (M = 4.28); and (b) evaluation of the expert, $F(1, 20) = 4.72$, $p = .04$, in which longer expert testimony

Table 4
Influence of Concrete Versus Abstract Expert Testimony: Study 2

Dependent variable	*df*	*F*	*p*	Concrete		Abstract	
				Specific hypothetical	Concrete control	Standard expert	Length control
Predeliberation verdict (proportion guilty)	1, 136	4.24	.04	.61	.69	.58	.39
Final verdict (proportion guilty)	1, 20	1.60	.22	.67	.69	.53	.39
Sentence	1, 20	0.96	.34	4.17	3.69	3.36	3.64
Likelihood of consent	1, 20	5.34	.03	3.64	3.83	4.89	5.58
Victim responsibility	1, 20	4.54	.05	3.05	3.24	3.83	3.92
Defendant responsibility	1, 20	5.48	.03	5.10	5.25	4.57	4.60
Victim credibility	1, 20	8.47	.009	4.61	4.58	3.82	3.75
Victim's moral character	1, 20	5.49	.03	3.79	3.86	3.41	3.42
Defendant credibility	1, 20	13.49	.002	3.25	3.46	4.28	4.32
Defendant likability	1, 20	6.71	.02	2.91	3.29	3.68	3.86
Evaluation of expert	1, 20	1.69	.21	6.31	5.83	6.23	5.50
Usefulness of expert testimony	1, 20	0.04	.85	2.66	2.65	2.73	2.79
Evaluation of prosecutor	1, 20	3.80	.07	4.94	5.32	4.64	4.57
Evaluation of defense attorney	1, 20	5.67	.03	3.96	4.59	4.67	4.93
Interest in trial	1, 20	0.39	.54	4.47	4.57	4.67	4.11
Recall of case facts	1, 20	0.26	.61	.68	.70	.72	.69
Recall of expert testimony	1, 20	0.08	.78	.88	.84	.92	.79

thetical expert testimony. Recall was no higher by jurors exposed to the specific hypothetical than it was by jurors exposed to standard expert testimony, and recall was unrelated to final verdicts. Moreover, there was no evidence that the use of expert testimony was a function of the testimony's duration. Instead, the use of expert testimony appeared to be enhanced when the implications of the group data for individual judgments were made more obvious, either by means of a hypothetical example linked directly to the case or by means of concrete examples interspersed throughout the standard expert testimony. When expert testimony was more abstract, jurors seemed less able or less willing to apply it to the case, and juror judgments seemed to be relatively unaffected by its presence.

Analyses of postdeliberation juror judgments, however, provide limited insight into the processes by which expert testimony influences juror judgments. Additional information can be gained by examining the influence of expert testimony on jury deliberations. When expert testimony is perceived to be germane to the case, this should be reflected in the jury's discussion. Juries may use the expert testimony explicitly to support arguments about case facts, or the expert testimony may subtly influence the ways in which juries discuss various aspects of the trial. These subtle influences on evidence interpretation should be particularly likely when the expert testimony is presented early in the trial. In contrast, when expert testimony is abstract and its implications for the case are less clear, one may expect juries to engage in debate about the expert testimony or to ignore the expert testimony completely. Either way, the abstract expert testimony should have little impact on the manner in which juries discuss other case issues. Jury deliberation analyses could thus help to further elucidate the impact of expert testimony on juror information processing. Such analyses could also address the current legal controversy concerning the admissibility of expert psychological testimony (cf. McCloskey, Egeth, & McKenna, 1986). Important concerns have been raised about the effects of expert testimony on jury decision making. It has been suggested, for example, that expert testimony may carry too much weight, that jurors may accept it uncritically. Alternately, it has been argued that discussions of the expert testimony will come to dominate the deliberation at the expense of careful discussion of other pieces of evidence. Analyses of outcome variables do little to address such concerns. Accordingly, we examined the impact of expert testimony on jury discussion of the case in Study 3 by analyzing the content of audiotaped jury deliberations from Study 1.

was associated with less favorable evaluations (M = 5.87) than was shorter expert testimony (M = 6.23).

Study 3

Method

Deliberation Coding

Two distinct sets of questions were examined with reference to the deliberation analyses. The first set concerned explicit discussion of the expert testimony. For example, how much of the deliberation was devoted to discussion of the expert and his testimony? What kinds of things did jurors say? Did the amount and nature of this discussion vary with the particular expert testimony to which a jury had been exposed?

The second set of questions concerned the impact of expert testimony on discussion of other issues. That is, excluding discussion of the expert and his testimony, did the presence of expert testimony affect the jury deliberation process? Did expert testimony influence the manner or degree of jury deliberation on other issues? Did the effect of the expert on the deliberation vary across particular types of expert testimony?

In order to address both types of questions, undergraduate research assistants coded jury deliberations for discussion of two types of topics:

(a) discussion of the expert and his testimony (expert helpfulness, expert content, expert support, and expert credibility); and (b) discussion of other case issues (victim's perception, victim's credibility, victim's moral character, defendant's perception, defendant's credibility, consent, force, and resistance). (See Appendix C for a description of each coding category.)

The audiotaped deliberations of all Study 1 juries except those that had a unanimous verdict on the first straw vote were coded by 4 undergraduates who were blind to the experimental hypotheses. Two people coded each deliberation tape. Choosing from the 12 content codes, the first coder who listened to a tape wrote down the two most prominent themes of the discussion at approximately 30-s intervals throughout the entire deliberation. (A 30 ± 10-s interval was used so that a coder would not be forced to decide on a code when a juror was in midsentence.) These content codes allowed an assessment of whether expert testimony influenced the frequency with which juries discussed particular topics.

In addition, coders indicated the valence of the discussion for each content code, that is, whether it was favorable to the prosecution, favorable to the defense, or neutral. These codes allowed an assessment of whether expert testimony influenced the tone of the discussion on each issue.[11] Throughout the coding process, the first coder recorded the tape counter numbers corresponding to the end of each interval so that the second coder could use the same intervals. Overall, intercoder reliability was relatively high (average kappas = .70 for content codes and .64 for valences).

Results

Overview of Data Analysis

Data analysis varied somewhat for expert- and non-expert-oriented codes. For both types of codes, the frequency of each content code–valence combination was summed for each deliberation. Because there were no significant differences across conditions in either deliberation length or total number of content codes recorded, analyses of the four expert codes (expert helpfulness, expert content, expert support, and expert credibility) were performed on these raw frequencies. Thus, means on these variables reflected the average number of times an expert code constituted one of the prominent themes of the deliberation discussion.

On each of the remaining eight (nonexpert) codes, raw score frequencies were converted to proportion scores by dividing them by the total number of nonexpert codes recorded for a given deliberation. Proportion scores were then subjected to arc sin transformations (Winer, 1971), and analyses were performed on the transformed proportions. For ease of interpretation, we report means in terms of the original (untransformed) metric; means reflect the percentage of the total deliberation in which a given topic was a dominant theme, excluding discussion of the expert and discussion of uncodable topics.

To assess differences in the manner in which various topics were discussed across conditions, we first analyzed all measures by means of a 5 (condition) × 3 (valence) ANOVA with repeated measures on the latter variable. We examined the effects of expert testimony on the frequency with which issues were discussed via three orthogonal, single degree of freedom contrasts—corresponding to a type of testimony main effect, a timing of testimony main effect, and a Type × Timing interaction—and a comparison of each of the expert testimony cells with the no-expert-testimony-control cell using Dunnett's procedure.

Discussion of the Expert

Valence effects. There was a highly significant valence effect on expert support, $F(2, 44) = 9.25$, $p = .0004$. Not surprisingly, juries were significantly more likely to use the expert's testimony to support proprosecution arguments ($M = 0.70$) than prodefense ($M = 0.11$) or neutral ($M = 0.04$) arguments. There were no significant valence effects on the other expert codes, nor were there any significant Condition × Valence interactions.

Expert testimony effects. Juries exposed to the specific hypothetical were significantly more likely than those given only standard expert testimony to discuss the expert's helpfulness, $F(1, 22) = 4.60$, $p = .04$. This effect, however, was subsumed by a significant Type × Timing of Testimony interaction, $F(1, 22) = 8.04$, $p = .01$. Juries in the specific hypothetical-expert first condition were significantly more likely to discuss the expert's helpfulness ($M = 1.20$) than were juries in the remaining conditions (Ms = 0.0, 0.167, and 0.333 for standard expert-expert first, specific hypothetical-expert last, and standard expert-expert last conditions, respectively). Effects for type of expert testimony on other expert codes were nonsignificant, as were effects for timing of presentation.

We performed tests separately to determine whether the frequency of expert discussion differed significantly from zero for each expert code in each experimental condition. The comparisons revealed that significant discussion of the expert occurred only in some experimental conditions and that even this discussion was limited. Discussion of the expert's helpfulness was significant only in specific hypothetical-expert first juries, $t(5) = 2.25$, $p = .038$ ($M = 1.0$). Significant use of the expert's testimony to support arguments occurred only in standard expert-expert last juries, $t(5) = 3.05$, $p = .028$ ($M = 1.83$). Both specific hypothetical-expert first juries, $t(5) = 2.44$, $p = .029$ ($M = 1.77$), and standard expert-expert last juries, $t(5) = 3.03$, $p = .015$ ($M = 3.17$), engaged in significant discussion of the content of the expert's testimony. The same pattern held for discussion of the expert's credibility: $t(5) = 2.24$, $p = .038$ ($M = 0.50$), for specific hypothetical-expert first juries, and $t(5) = 2.91$, $p = .017$ ($M = 1.67$), for standard expert-expert last juries. All other contrasts were nonsignificant, indicating relatively little discussion of the expert. This may reflect the low power associated with only six juries per cell, but the average frequency with which expert testimony was discussed was low in absolute terms as well. When the four expert codes were collapsed and averaged across all four types of expert testimony, the mean number of intervals in which the expert or his testimony was discussed was only 3.13. Translated into deliberation time, this represented less than 2 min of deliberation discussion devoted to the expert.

Discussion of Other Issues

Valence effects. There were no significant valence effects on discussion of the victim's moral character or the defendant's

[11] If the topic of discussion did not fall under any of the coding categories, an interval received codes of 0 and was omitted in data analysis. Generally, discussion during these uncodable intervals focused on procedural issues such as the deliberation instructions and vote taking. The number of uncodable intervals did not vary significantly across experimental conditions.

Table 5
Jury Discussion of Victim Resistance by Valence and Condition: Study 3

Condition	Valence		
	Proprosecution	Prodefense	Neutral
Specific hypothetical-expert first	0.9%[a,b]	0.5%[a]	0.4%[a]
Standard expert-expert first	1.5%[a,b]	2.7%[a,d]	0.3%[a]
Specific hypothetical-expert last	3.1%[a,b]	3.8%[b,d]	0.7%[a]
Standard expert-expert last	3.3%[b]	6.1%[b,d]	0.3%[a]
No-expert-testimony control	3.4%[a,b]	11.6%[c,d]	0.5%[a]

Note. Means sharing different superscripts within a single column or within a single row are significantly different at $p < .05$, using the Newman-Kuels procedure.

perception, indicating that discussion of these topics was fairly evenly split among proprosecution, prodefense, and neutral statements.

The valence main effect was highly significant on jury discussion of the victim's perception, $F(2, 44) = 6.30$, $p = .004$, victim credibility, $F(2, 44) = 44.99$, $p < .00001$, consent, $F(2, 44) = 13.21$, $p = .00003$, and force, $F(2, 44) = 8.81$, $p = .0006$. These topics were highly controversial; the amount of proprosecution and prodefense discussion did not differ, but there was significantly less neutral discussion on these topics.

Defendant credibility, instead, was most often discussed in a proprosecution manner ($M = 7.5\%$), followed by significantly less prodefense discussion ($M = 3.2\%$) and even significantly less neutral ($M = 0.8\%$) discussion, $F(2, 44) = 32.24$, $p < .00001$.

Finally, the issue of resistance was characterized by mostly prodefense discussion, significantly less proprosecution discussion, and significantly less neutral discussion, $F(2, 44) = 19.84$, $p < .00001$. Moreover, discussion of resistance evidenced the only significant Valence × Condition interaction, $F(8, 44) = 2.44$, $p = .028$. Cell means for this interaction are presented in Table 5. Tests of simple effects within levels of valence indicated that the degree of proprosecution and neutral discussion did not vary across conditions, whereas the amount of prodefense discussion varied significantly, $F(4, 22) = 6.85$, $p \leq .01$. There was less prodefense discussion of resistance in specific hypothetical-expert first conditions than in control, standard expert-expert last, and specific hypothetical-expert last conditions. Tests of simple main effects within levels of condition indicated that the valence of the discussion did not differ within either of the expert first conditions. However, in the control condition, $F(2, 44) = 15.50$, $p \leq .01$, the specific hypothetical-expert last condition, $F(2, 44) = 3.36$, $p \leq .05$, and the standard expert-expert last condition, $F(2, 44) = 9.09$, $p \leq .01$, valence varied considerably. In the control condition, prodefense discussion was significantly more frequent than either proprosecution or neutral discussion. In the standard expert-expert last condition, instances of both prodefense and proprosecution discussion significantly outnumbered neutral discussion, and in the specific hypothetical-expert last condition, prodefense discussion was significantly more frequent than neutral discussion.

Expert testimony effects. There were no significant effects attributable to the type of expert testimony on jury discussion of nonexpert issues.

There were, however, significant timing of testimony effects. Discussion of victim resistance was significantly less frequent in expert first conditions than in expert last conditions, $F(1, 22) = 6.37$, $p = .02$. Dunnett contrasts indicated that only specific hypothetical juries discussed the issue significantly less than control juries. (These effects were subsumed by the Valence × Condition interaction discussed earlier [see Table 5].)

Discussion of victim credibility, instead, was significantly more frequent in expert first conditions than in expert last conditions, $F(1, 22) = 10.98$, $p = .003$. The Timing × Type of Testimony interaction was also highly significant, $F(1, 22) = 9.46$, $p = .006$. Standard expert-expert last juries ($M = 23.2\%$) discussed victim credibility significantly less than other expert testimony juries (Ms = 40.4%, 50.2%, and 39.6%, for specific hypothetical-expert first, standard expert-expert first, and specific hypothetical-expert last juries, respectively). Dunnett comparisons indicated that all expert testimony cells differed significantly from the no-expert-testimony control ($M = 37.8\%$). Dunnett comparisons revealed that standard expert-expert last juries ($M = 4.3\%$) were also significantly more likely to discuss the defendant's perception than were control juries ($M = 0.0\%$).

General Discussion

Taken together, these studies suggest that people can use group probability data in the form of expert testimony if it is presented so that the implications of the expert's testimony for a specific case are fairly clear. Without this link, jurors appear less able or less willing to make the connection between the group data and their judgments about individual cases. With this link, the expert testimony is used and serves to counteract the otherwise detrimental effects of rape myths and misconceptions on juror evaluations of the victim and judgments about the case.

The consistent pattern of means that emerged on most measures suggests that the timing of presentation of expert testimony is also important. Jurors tended to make the greatest use of the specific hypothetical expert testimony when it was presented early in the trial. As expected, once jurors had the opportunity to interpret the case facts in light of their own preconceptions, their resulting beliefs and intuitive theories were less influenced by expert testimony. It appears, therefore, that the use of expert scientific testimony will be enhanced when (a) the information is somehow linked explicitly to the case under consideration and (b) one is able to reduce the likelihood that other beliefs and causal theories will have developed such that they conflict with or contradict the group data presented by the expert.

The results are also suggestive regarding the processes underlying the use of group probability data in this form. The absence of recall effects implies that the use of expert testimony was related neither to recall of case facts nor to recall of what the expert said. Judgments by jurors exposed to the hypothetical

example indicated much greater use of the expert testimony than judgments by jurors exposed to standard expert testimony, despite equivalent levels of recall. The hypothetical example seems to have influenced juror's interpretations of the case rather than their memories for specific pieces of information.

How might interpretations of case facts change when jurors make use of expert testimony? The legal intent behind the introduction of expert scientific testimony in rape trials is to corroborate the victim's testimony (i.e., to lend scientific credence to her claim of rape). If this is so, then the effects of expert testimony should be mediated by perceptions of victim credibility (see Borgida & Brekke, 1985, for a discussion of this issue). Inclusion of expert testimony with the hypothetical example did lead to higher ratings of victim credibility and to greater discussion of the victim's credibility during the jury deliberation. Discussion of victim credibility, however, was equally likely in two of the less effective expert testimony conditions. More important, there was no evidence that expert testimony influenced the tone of jury discussion about victim credibility; jury discussion remained evenly split on the issue of whether the victim was credible.

It has also been argued that expert testimony shifts the jury's focus to a consideration of the victim's perspective throughout the experience and that this leads to very different interpretations of her behavior (Rowland, 1979). According to this line of reasoning, the expert may be increasing identification with or empathy for the rape victim. Yet, data from Study 3 showed that specific hypothetical juries were no more likely than control juries to discuss the victim's perception during the deliberation and that expert testimony had no impact on the manner in which the victim's perception of the incident was discussed. Expert juries were just as likely to express skepticism about the victim's viewpoint as they were to accept her interpretation. The expert testimony, therefore, did not seem to facilitate juror empathy with the victim.

Still others have speculated that the impact of such testimony may be on perceptions of the defendant, leading to considerable concern that expert testimony on behalf of the prosecution may be unduly prejudicial to the defendant. In this research, expert testimony with the hypothetical did in fact lead to less favorable evaluations of the defendant on questionnaire measures. Such effects were not evident, however, in jury deliberations. Jury discussion of the defendant's perception was generally positive across all conditions. Expert testimony had no impact on the way in which juries discussed the defendant and little impact on the amount of time that they discussed him. The negative evaluations expressed on questionnaire measures, then, might have been a consequence of the guilty verdicts rendered in expert conditions rather than a cause of them.

Study 3 suggests that the most substantial change in interpretation of case facts resulting from expert testimony concerns the issue of resistance. In the absence of expert testimony, victim resistance was a dominant theme during more than 15% of the deliberation, and this discussion tended to be defense oriented. That is, juries spent considerable time noting that the victim did not resist enough. Specific hypothetical-expert first juries, by contrast, devoted less than 2% of the deliberation to the issue of resistance, and their discussion of this issue was generally prosecution oriented. By presenting evidence that many rape victims do not resist their attackers and by explaining that resistance can have extremely undesirable consequences, the expert might have altered jurors' expectations about typical victim behavior, thereby altering their interpretations of other case facts as well. Although our research was not designed to distinguish between these plausible mediators, our speculations are consistent with previous research on juror and jury decision making. Pennington and Hastie (1986), for example, have found that jurors try to organize trial evidence into plausible scenarios or stories and that these stories are systematically related to juror verdicts. Similarly, Holstein (1985) has provided evidence to suggest that jury deliberations involve discussion of jurors' competing case interpretations, in an effort to agree on a consensual account of the event in question. Thus, expert testimony may influence jurors' interpretations of the evidence, providing them with a shared story or account of the case. Future research should address more directly the role of mediators in the processes by which expert testimony influences juror judgments, perhaps through protocol analyses of decision-making processes (cf. Pennington & Hastie, 1986).

From a broader perspective, this research has both theoretical and applied implications. On the more theoretical level, our data contribute to the understanding of the conditions under which people are likely to apply group probability data to judgments about individuals. Although the jury simulation context differs markedly from most experimental settings within which this topic has been investigated, the observed effects associated with the hypothetical example are generally consistent with previous research in social cognition (Borgida & Brekke, 1981) and with previous research in psychology and law on expert testimony (Hosch, 1980). The results of Study 2 suggest that the specific hypothetical technique may serve to "concretize" the more abstract group-level data presented by the expert, thereby increasing its apparent connection to the case. Similar concretizing manipulations have proven effective in increasing base rate use in less complex tasks (e.g., Manis et al., 1980). The precise mechanism by which concrete expert testimony facilitates use is worthy of further investigation. Expert testimony is designed to educate jurors about rape myths, but concrete expert testimony may also educate jurors about statistical reasoning. It may teach jurors by example the proper way to apply generalizations based on groups to judgments about individuals (cf. Fong, Krantz, & Nisbett, 1986). Alternatively, jurors may already know how to make the connection but may not see the relevance of the group data to this particular judgment task. If so, concrete expert testimony may function to increase the accessibility of the relevant prediction rule (Ginossar & Trope, 1987).

The moderating effects of timing of presentation also extend previous research by identifying yet another variable that influences use of group probability data, at least within the context of a jury decision-making task. The finding that expert testimony had little impact when presented late in the trial questions the law's implicit model of decision making, which assumes that people passively accumulate information throughout the course of the trial, evaluating it and interpreting it only after all of the facts of the case are in. In social psychological terms, these results suggest that jurors approach the case with an impression set instead of a recall set (cf. Hamilton, Katz, &

Leirer, 1980; Sherman, Zehner, Johnson, & Hirt, 1983). That is, rather than storing case facts one by one, to facilitate recall of specific pieces of evidence, jurors may try to organize the information as they receive it into a consistent, meaningful whole. This strategy makes intuitive sense, given the juror's information load and ultimate decision-making task, but it does have implications for the processing of expert testimony. Data gathered via impression sets are resistant to later reinterpretation (Lingle & Ostrom, 1981) and subject to belief perseverance effects (Lord et al., 1979); one must discredit not only the initial data but also all of the impressions based on those data (Ross & Anderson, 1982). Under impression set conditions, the timing of presentation of new information, then, becomes a critical variable (Sherman et al., 1983). Expert testimony, when presented early in the trial, may serve as a powerful organizing theme or basis for a juror's initial impression of the case. When presented later in the trial, by contrast, the expert testimony may be treated merely as additional information to be integrated into an existing, well-organized impression.

Finally, our research corroborates findings in other areas of social psychology demonstrating the effectiveness of direct educational interventions in counteracting the influence of rape myths (e.g., Malamuth & Check, 1984). Further research is necessary, however, to determine whether expert testimony produces the long-term changes reported by other researchers or whether jurors exposed to expert testimony simply hold their preconceived notions in abeyance while making judgments about the case at hand.

From a legal policy perspective, this research has a number of implications. One must, of course, be cautious about generalizing too broadly from the results of jury simulations (see Kerr & Bray, 1982, for a discussion of the limitations of simulation research); nonetheless, our data do pertain to several interesting policy-relevant issues. It appears that the introduction of expert testimony on behalf of the prosecution may be an effective strategy for increasing conviction rates in rape trials (cf. Frazier & Borgida, 1985). This strategy would be particularly well suited for casual acquaintance rapes, for which statutory and procedural reforms appear to be less effective (Borgida, 1981; Borgida & Brekke, 1985; Borgida & White, 1978). Contrary to many jurists' fears, however, jurors do not seem to automatically accept and apply the testimony of an expert witness. Jurors seem reluctant to use expert testimony about group probabilities, and it may be necessary for attorneys to present it so as to enhance its use.

If presented correctly, jurors may make considerable use of scientific testimony by an expert witness, even though when queried about it they may report that it had little impact on their decisions and, as the jury deliberation analysis in Study 3 suggests, they may not spend a great deal of deliberation time discussing the expert testimony. Jurors may be unaware of the extent to which they were influenced by an expert or they may simply be reluctant to acknowledge its impact. Whichever the case may be, results of posttrial interviews with jurors should be viewed with some skepticism, and a more accurate method of assessing the true impact of such evidence should be sought.

Moreover, the possibility that expert scientific testimony could have such a substantial impact on juror judgments raises ethical and legal issues regarding its admission in court. To the extent that jurors rely heavily on this evidence, for example, experts have an especially strong obligation to present only information that is well supported by empirical data. When is the database in a given area complete enough to justify its introduction into the courtroom? This question has proven to be highly controversial within both the scientific and the legal communities (Gianelli, 1980; Loftus, 1983; McClosky & Egeth, 1983). Legal concerns have also been voiced regarding juror understanding of evidence presented by experts, the possible prejudicial effects associated with its use in court, and the degree to which jurors already know what experts are trying to explain to them.

Adequate resolution of such issues will require research that examines not only when jurors are likely to use expert testimony but also how they are likely to use it. Do jurors overweight testimony by experts? Are jurors sensitive to the limitations as well as the strengths of the data they are exposed to? What constitutes the proper use of expert scientific testimony? How do jurors make use of conflicting testimony by multiple experts in a given field? Can the presentation of expert scientific testimony be structured so as to discourage its misuse? Psychologists of the law have finally begun to address these questions empirically (e.g., Brekke, in press) and to consider the ethics of expert testimony (McCloskey et al., 1986). Psycholegal research on these questions will no doubt be critical to psychology's future in the courtroom.

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Appendix A

Topical Comparison of Expert and Specific Hypothetical Expert Testimony

Topic: Casual Acquaintance Rapes

Standard

". . . rapes by casual acquaintances accounted for more than 50% of the cases surveyed, and some rape experts estimate the actual percentage may be as high as 70%, since rape, especially by casual acquaintances, is so underreported. . . . The term refers to a nodding acquaintance, a previous knowledge of a particular person, not necessarily on a personal level such as a friend, but just that there was some prior knowledge of that person. However, sometimes it can be a person whom the victim thought she knew well, someone she trusted. This makes casual acquaintance rape more difficult to avoid than stranger rape, since it's a crime committed by everyday male companions. The predominant thinking is that 'If I know him, he couldn't possibly rape me.' "

Hypothetical

". . . the situation as you have presented it to me is a casual acquaintance rape which, according to statistics, is one of the most common forms of rape. In this case, the woman knew the man beforehand on an acquaintance basis, she was on familiar ground when she accepted the ride, she'd taken rides home from him before and he had posed no danger to her in the past; therefore, she did not feel in any danger when she accepted the invitation to go play Foosball."

Topic: Victim Reactions: Frozen Fright Syndrome

Standard

"The behavior of the vast majority of women during their contact with rapists demonstrates this frozen fright syndrome, in which they submit, and their consequent helplessness makes it appear to the outsider that the victim's behavior was friendly, even cooperative. However, the individual submits, in her perception, in order not to be killed, or otherwise harmed physically. Sometimes these victims close their eyes and try to leave their body to the rapist. This response is based on fright and a desire to remove oneself from the situation in the only avenue left possible."

Hypothetical

"A fifth point would concern her statement after she ceased struggling, saying, "Go ahead, get it over with." This comes very close to what I mentioned about victims experiencing frozen fright syndrome in which they leave their body to the rapist, removing themselves from the situation by detaching themselves from their body and what is happening to it."

Topic: Failure to Report Rape

Standard

"There is a myth that if a woman doesn't report or is reluctant to report rape, she probably wasn't raped. Actually, most women are reluctant to report rapes to the authorities. It is estimated that only 10–15% of all sexual assault victims report the crime to the police, and the FBI [Federal Bureau of Investigation] has termed it the most underreported crime. This is especially true with respect to casual acquaintance rapes."

Hypothetical

". . . it was very normal and not to be taken as sign of the victim's lying that she wanted to see her girlfriend and boyfriend before reporting to the authorities. . . . The fact that she had to be talked into reporting the incident is not at all unusual or hard to understand."

Topic: Casual Acquaintance Rapists

Standard

"The casual acquaintance rapist only uses as much force as is necessary. Rape is his means of conquering and controlling another person. He frequently denies that the sexual encounter was forcible because he needs to believe that the victim wanted and enjoyed it. He'll even attempt to redefine the situation for the victim, making it seem as if nothing out of the ordinary happened."

Hypothetical

". . . the behavior of the young man, the attacker in this situation, which fits the pattern of casual acquaintance rapists in their attempt to redefine the situation for the victim. . . . His apparent contriteness after the rape, saying that she must now hate him and his willingness to take her anywhere she wanted to go after the rape, represent a further attempt on his part to redefine the situation. Realizing what he has done, he tries to undo or assuage the woman, making her feel sorry for him rather than herself. His behavior also fits the pattern of casual acquaintance rapists in that he threatened her and maneuvered her into a position of relative helplessness and used only as much violence as was necessary to get the victim's compliance."

(*Appendixes continue on next page*)

Appendix B

Examples From Concrete Control Expert Testimony

Topic: Casual Acquaintance Rapes

Standard Expert Testimony

"The term refers to a nodding acquaintance, a previous knowledge of a particular person, not necessarily on a personal level such as a friend, but just that there was some prior knowledge of that person."

Concrete Example

"For example, it could be someone who works in the same office building as the victim, someone who rides the same bus in the morning, or even someone she occasionally sees at the grocery store."

Topic: Rapists

Standard Expert Testimony

"Men from virtually every profession and every walk of life have been arrested for committing rape."

Concrete Example

"Police officers, doctors, lawyers, men from virtually every profession one could think of have been arrested for committing rapes. Rapes have been committed by men in all walks of life, by acquaintances, bosses, friends, relatives, boyfriends, and dates."

Topic: Verbal Resistance

Standard Expert Testimony

"Many women employ some sort of verbal resistance."

Concrete Example

"For instance, they may cry or feign inability to understand the rapist's instructions; they may even try to convince him that they are pregnant, diseased, or injured."

Topic: Planned Rapes

Standard Expert Testimony

"In fact, 60–70% of all rapes are planned in advance."

Concrete Example

"The majority of rapists actually watch for a victim and then approach her with the rape in mind."

Topic: Physical Resistance

Standard Expert Testimony

"Common folk wisdom is to 'kick your attacker in the balls.' However, self-defense experts in the area of rape do not recommend that particular move as it is one of the easiest ways of being thrown off balance and eventually overwhelmed; all the attacker has to do is to simply grab the woman's leg as she attempts to kick him."

Concrete Example

"Generally, the rapist is several inches taller than the victim, so to hit the target area, the victim must lift her leg fairly high. This is quite an awkward position to be in for a woman untrained in self-defense."

Appendix C

Deliberation Content Codes: Study 3

1. *Expert helpfulness.* Discussion of how helpful (or useless) the expert's testimony was to jurors.
2. *Expert content.* Discussion of the content of the expert's testimony (e.g., quoting or paraphrasing of his testimony). The expert need not have been mentioned explicitly.
3. *Expert support.* Use of the expert's testimony to support or bolster jurors' arguments.
4. *Expert credibility.* Discussion of the credibility of the expert or his testimony (including references to the expert's credentials, his motivation to testify, the empirical support for his testimony, etc.).
5. *Victim's perception.* References to the victim's perception of the situation before, during, and shortly after the alleged rape.
6. *Victim's credibility.* Direct or indirect references to the victim's credibility, including discussion of her motive for crying rape.
7. *Victim's moral character.* Discussion of the victim's moral character, including her attitudes about sexual relations and past sexual behavior.
8. *Defendant's perception.* References to the defendant's perception of the situation before, during, and shortly after the alleged rape.
9. *Defendant's credibility.* Direct or indirect references to the defendant's credibility.
10. *Consent.* Discussion of whether or not the victim consented to have sexual intercourse with the defendant.
11. *Force.* Discussion of whether the defendant forced or coerced the victim to have sexual intercourse, including all references to injuries.
12. *Resistance.* Discussion of the victim's resistance (or lack thereof) to the defendant's advances.

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