Journal of Applied Psychology
1981, Vol. 66, No. 6, 688-696

0021-9010/81/6606-0688$00.75

The Tractability of Eyewitness Confidence and Its Implications for Triers of Fact

Gary L. Wells
University of Alberta
Edmonton, Canada

Tamara J. Ferguson
Catholic University of Nijmegen
The Netherlands

R. C. L. Lindsay
University of Manitoba, Winnipeg, Manitoba Canada

A theft staged for 80 unsuspecting eyewitnesses was followed by a picture lineup that did or did not contain the thief. In an attempt to see if eyewitness confidence is tractable after the identification, half of the eyewitnesses who identified the thief (accurate witnesses) and half who identified an innocent suspect (inaccurate witnesses) were briefed by a "prosecutor" who suggested they rehearse answers to potential questions that would be asked under cross-examination. Cross-examinations of 10 accurate briefed witnesses, 10 accurate nonbriefed witnesses, 9 inaccurate briefed witnesses, and 9 inaccurate nonbriefed witnesses were viewed by 152 subject-jurors in groups of 4. Briefed eyewitnesses rated themselves as more confident that they had identified the thief than did nonbriefed witnesses. This increase was primarily due to inaccurate eyewitnesses increasing their confidence, and the briefing manipulation served to eliminate the confidence–accuracy relationship. Subject-jurors were significantly more likely to vote guilty in conditions in which the eyewitness had been briefed than in the nonbriefed conditions. It is argued that briefing eyewitnesses, although legal, simply serves to increase the eyewitnesses' confidence in their memory, not the accuracy of memory. It is also argued that an accurate eyewitness may have memories that are already associated with high confidence, and therefore, briefing may primarily inflate the confidence of inaccurate eyewitnesses.

The confidence of an eyewitness has begun to play a central role in eyewitness research. Eyewitness confidence has been shown to be poorly related to eyewitness identification accuracy at best (Wells, Lindsay, & Ferguson, 1979), unrelated much of the time (Leippe, Wells, & Ostrom, 1978), and sometimes negatively related (Loftus, Miller, & Burns, 1978). Confidence of eyewitnesses takes on further import in its demonstrated role as a primary determinant of the perceived credibility of a witness (Lindsay, Wells, & Rumpel, 1981; Wells et al., 1979; Wells, Lindsay, & Tousignant, 1980). Confidence also has a formal role in the criminal justice system as the United States judiciary has declared confidence a valid criterion for use in deciding the accuracy of eyewitness testimony[1] (Neil v. Biggers, 1972).

The current study had several objectives.[2] First, although confidence has been a focus of research in decision making (e.g., Einhorn & Hogarth, 1978; Fischhoff, Slovic, & Lichtenstein, 1977), there has been little theoretical treatment of confidence in eyewitness memory. How can we represent the cognitive processes that give rise to judgments of confidence in memory? Second, confidence of eyewitnesses has been examined only as a correlate of eyewitness accuracy. Empirical

The authors would like to thank Julie Laramée, Carolyn Rumpel, and Lynn Hazen for their roles in the experiment.

Requests for reprints should be sent to Gary L. Wells, Department of Psychology, University of Alberta, Edmonton, Alberta T6G 2E9, Canada.

[1] Neil v. Biggers, 409 U.S. 188 (1972).

[2] The current study was also designed to expand the Wells, Lindsay, & Ferguson (1979) paradigm wherein witnesses to staged crimes are cross-examined. Previous research in this paradigm used individual judgments of subject-jurors (rather than group-deliberated judgments), required judgments that were strictly believe–not believe the witness (rather than guilty/not guilty verdicts), and eyewitness evidence that was the only evidence given.

work has not examined other factors that might affect confidence. In fact, empirical studies suggest that over 90% of the variance in eyewitness confidence is determined by factors other than eyewitness accuracy (Wells et al., 1979). Finally, research showing that eyewitness confidence affects jurors' judgments of eyewitness accuracy has not actually manipulated confidence. Instead these studies have simply measured the correspondence between confidence of eyewitnesses to staged crimes and their believability under cross-examination. Numerous possibilities exist for third-variable types of involvement. That is, it might be that confident individuals possess other qualities that in turn serve the true causal role. An independent manipulation of eyewitness confidence would help establish its causal role in the confidence–credibility phenomenon.

Is Confidence Tractable?

Eyewitness confidence is a belief in the accuracy of one's memory and as such should be subject to the processes inherent in belief acquisition and belief change. Two lines of research are particularly relevant to the issue of eyewitness confidence. Tesser and his colleagues have found that thought tends to result in a polarization of beliefs (Tesser, 1976; Tesser & Conlee, 1975; Tesser & Cowan, 1975, 1977). Tesser's research shows that thought changes a set of cognitions so that it becomes more consistent with one's predispositions. This appears to be due to the person's tendency to search for cognitions consistent with their initial belief (Sadler & Tesser, 1973), to generate new cognitions that follow logically from initial belief (Tesser & Cowan, 1975), and to reinterpret inconsistent cognitions to make them more consistent (Tesser & Cowan, 1977). Through these processes beliefs become more extreme with increased thought. An eyewitness who has enough belief in the guilt of a lineup member to choose that member might show an enhancement of the strength of that belief, therefore, by being induced to think about it after the fact.

Tesser's important research parallels recent research of Koriat, Lichtenstein, & Fischhoff (1980). Koriat et al. have found that confidence in a decision is primarily dependent on thoughts consistent with the decision. Koriat et al. have shown that confidence is more highly correlated (positively) with the number of consistent thoughts than it is (negatively) with the number of inconsistent thoughts. Furthermore, Koriat et al. showed that people naturally generate a biased memory search when reviewing evidence, a bias marked by a process of "gradually biasing the search toward evidence supporting a tentatively preferred answer" (Koriat et al., 1980, p. 108). Thus thought is biased toward consistency (also see McGuire, 1960; Wyer, 1974), and confidence is determined by the quantity of consistent cognitions.

This research on human strategies in cognitive search is further corroborated by research showing the difficulties people have in accepting the relevance of negative evidence in logical inference tasks (Johnson-Laird & Wason, 1977). In general it leads to the expectation that the more thought an eyewitness gives to his or her identification of a lineup member, the more he or she will come to believe that the choice of that lineup member was correct. Specifically, the cognitive-search literature suggests that the eyewitness will think of reasons supporting the chosen alternative (the chosen lineup member) and not think of reasons supporting the rejected alternative (e.g., the other lineup members or the alternative of rejecting the entire lineup). As a result increased thought should produce increased confidence.

Leippe (1980) has argued a similar position in an article devoted to the relationship between eyewitness confidence and eyewitness accuracy. Leippe points out the possibility that confidence in memory and accuracy in memory can be under the control of different mechanisms. Leippe notes that several studies have manipulated accuracy of memory (e.g., Lindsay et al., 1981; Loftus et al., 1978) without having corresponding effects on confidence in memory. Without empirical support Leippe makes a strong theoretical argument for a class of events that may alter confidence in memory without affecting accuracy in memory. This latter class of events is social events that include such factors as commitment (Brehm, 1966),

self-perception effects (Bem, 1965), and mere thought. Leippe notes that "the somewhat certain witness should become more certain . . . the more he or she thinks about the matter [because] consistent aspects of memory should be more readily retrieved than inconsistent aspects" (Leippe, 1980, p. 270).

Affecting Confidence but Not Accuracy

The previous section can serve as a theoretical framework for examining confidence. Confidence in memory is considered, therefore, to be a tractable or malleable judgment that can be affected independently of memory. Our choice of possible manipulations was numerous, but we felt that an ecologically valid manipulation would be most informative. Thus we chose a systems variable, that is, something over which the criminal justice system has potential control (Wells, 1978), namely the pretrial interaction between an attorney and his or her witness. The manipulation operationalized in the current study is not only legal but, we understand, common. We are referring specifically to the situation in which an attorney briefs the witness on the type of questions that the witness could expect under cross-examination. The witness is told what these potential questions might be and is encouraged to rehearse how she or he might answer the questions. The witness may also be warned that the cross-examiner will try to catch inconsistencies in what the witness says.

If our hypothesis is correct, briefings of this type should elicit increased thought on the part of the eyewitness in a way that is biased toward confirming the choice made by the witness. This should enhance witnesses' confidence and, in turn, should powerfully affect jurors' perceptions of witness credibility. In fact the briefing manipulation should serve to elevate the confidence of inaccurate eyewitnesses, thereby insuring a task for the trier of fact that is at best difficult (and potentially impossible) for discerning eyewitness accuracy.

Method

Overview

Thefts were staged individually for witnesses, who then attempted to identify someone from a six-person picture array. The array included either the thief or an innocent replacement. Those identifying the thief (hereafter called accurate witnesses) and those identifying the innocent suspect (hereafter called inaccurate witnesses) were then randomly assigned to a briefing or no-briefing condition. These witnesses were later cross-examined, and the cross-examinations were videotaped using high quality color equipment. A separate sample of subjects (groups of four) in the role of jurors watched the testimony on a 6-foot color video screen, heard other evidence on the case, made individual judgments as to whether they believed that the witness identified the thief or an innocent suspect, and finally, deliberated to reach a group verdict of guilt or innocence.

Phase 1

Eighty undergraduate males and females participated. Subjects arrived individually and were taken to a cubicle to fill out certain pieces of identification prior to what they believed would be a questionnaire study. Approximately 120 sec after the subject entered the cubicle, another person, actually the confederate-thief but posing as a coparticipant, entered the cubicle. The thief sat for 30 sec, then asked the subject the following two questions: "How long do we have to stay here?" and "Where is the experimenter?" After the second question the thief "discovered" a calculator under her chair, remarked that she could use one like that, quickly slipped it in her purse, and exited. The entire encounter lasted approximately 190 sec. Sixty seconds later the female experimenter reentered the cubicle and inquired about the calculator. Witnesses never failed to report that it was taken by another person. Though occasionally referring to the calculator as having been "stolen," most witnesses simply indicated that it was "taken." The experimenter then indicated that the event was a theft staged for benefit of the witness and that she would now like to see if the witness could identify the thief. A six-person picture array was then brought to the witness with instructions to: "try to identify the person who stole the calculator. The thief may or may not be in this set of pictures." Witnesses viewed one of two photo lineups; half of the eyewitnesses viewed a photo lineup that included the thief, and half viewed an identical lineup except that the thief was replaced by an innocent person.

After attempting the identification witnesses who identified the thief or the replacement were asked if they would be willing to be cross-examined.[3] Willing wit-

[3] We did not cross-examine those who chose a foil (e.g., someone other than the thief or the innocent replacement). This is consistent with our view of the lineup as being composed of only one suspect. The remaining lineup members are like police detectives or otherwise known-innocent distractors whose identification is not

nesses were then randomly assigned to wait 25 minutes for cross-examination or to be briefed by someone described as the prosecuting attorney. The briefing manipulation took approximately 7 minutes. It began with a warning that "the defense attorney is probably going to act very antagonistically toward you [in the upcoming cross-examination]." It went on to note things such as "She [the cross-examiner] will do her utmost to discredit your testimony in the eyes of the jury. . . . The cross-examiner will probably ask you questions about the person you saw at the scene. What was she wearing? How tall was she? What color was her hair? . . . Rehearse several times how you would answer questions along these lines . . . the cross-examiner will try to catch inconsistencies in what you say." The prosecuting attorney delivering this briefing was blind to the witnesses' accuracy. Briefed witnesses were given 18 minutes to think about the upcoming cross-examination.

The witnesses were then taken to a large room that included a witness stand in which they were seated and sworn in. A high quality color videotape camera filmed the cross-examination. The cross-examiner, blind to the actual accuracy of the eyewitness, delivered a standard cross-examination used in previous studies (Lindsay et al., 1981). There were 15 questions for the witness that included controlled narratives (e.g., "Describe what the person was wearing") as well as interrogating questions (e.g., "How long was the person in the room?"). The final question asked the witness how confident she or he was that she or he had identified the thief rather than an innocent suspect. The witness responded on a 7-point scale.

Phase 2

One hundred fifty-two male and female subjects participated in groups of 4. Each group was randomly assigned to view one of 38 cross-examinations. Eighteen of the cross-examinations were of inaccurate witnesses, 20 were of accurate witnesses. Half of the accurate witnesses and half of the inaccurate witnesses were recipients of the briefing manipulation.

Subjects were told that their role would be that of a juror. These subject-jurors were then told that

> a crime was committed approximately 90 days ago. The crime was a theft of a valuable calculator. The accused was found in the area and was detained because she resembled the general description given by a witness to the crime. Although the calculator was not found on the accused and has not to date been recovered, the accused was found in possession of a strap that was attached to the calculator. The accused claims that she found the strap on the sidewalk outside of the building where the theft occurred. The accused was then placed in a photo array along with five other females and the eyewitness identified the accused as the person who stole the calculator. We'd like you to view the testimony of the eyewitness.

The subject-jurors then viewed the cross-examination on a 6-foot color video-projection system.

Four dependent measures were taken from the subject-jurors. First, they were asked to indicate individually whether they believed the witness identified the actual thief or an innocent suspect. Second, they were asked to indicate how confident they perceived the witness to be. Third, they were asked to deliberate for 15 minutes and try to reach consensus on whether the suspect was guilty or innocent of the charge. After a single deliberation session, secret ballots were given to the experimenter. Finally, they were asked whether there was anything about the witness's testimony that led them to suspect that the witness had been coached or rehearsed prior to cross-examination (yes or no). Following this subject-jurors were plenarily debriefed and dismissed.

Results

Table 1 presents the percentage who made identifications of the thief, the thief's replacement, and foils and the percentage who made no identification. There were 5 witnesses among those identifying the thief who refused to be cross-examined. There was 1 witness who identified the innocent suspect who refused to be cross-examined. The remaining 20 witnesses who chose the thief and the 18 witnesses who chose the innocent replacement constituted the sample who were cross-examined.[4]

Prior to presenting the videotapes to subject-jurors, each of the 38 tapes was scored for witness confidence as indicated by the witnesses' responses to the last cross-examination question. These scores were subjected to an analysis of variance in a 2 (briefed, not briefed) × 2 (accurate witness, inaccurate witness) design. The means are presented in Table 2. The analysis of variance (ANOVA) produced a significant main effect for briefing, $F(1, 34) = 7.19, p < .025$, no main effect for accuracy, $F(1, 34) > 1.0$ *ns*, and a significant interaction, $F(1, 34) = 6.63, p < .025$. Thus there is statistically significant evidence that confidence was tractable as a function of the briefing treatment. However, the effect of the briefing

incriminating. We also did not cross-examine those witnesses who made no choice even though a recent account of eyewitness behavior shows a logic for including such witnesses in court (Wells & Lindsay, 1980).

[4] This attrition increases rather than decreases the ecological validity of the study (see Wells, 1978).

Table 1
Percentages of Witnesses Identifying the Thief, the Thief's Replacement, Foils, and Making No Identification as a Function of the Thief's Presence/Absence

Lineup condition	Choice of thief		Choice of suspect		Choice of foil		No choice	
	%	Frequency	%	Frequency	%	Frequency	%	Frequency
Thief present	57	25		NA	25	11	18	8
Thief absent		NA	48	19	33	13	20	8

Note. NA = Not applicable.

treatment is almost completely nested in the inaccurate witnesses, perhaps because of a ceiling effect for accurate witnesses.

The subject-jurors' belief data were scored by assigning a number from zero to four for each of the 38 witnesses, depending on how many of the four subject-jurors believed the witness had identified the actual thief. In other words, each group of four subject-jurors constituted one data-point in a 2 (witness accuracy) × 2 (briefed, not briefed) design. An ANOVA revealed marginally significant main effects for witness accuracy, $F(1, 34) = 3.95$, $p \simeq .08$, and for the briefing treatment, $F(1, 34) = 4.08$, $p \simeq .06$. The interaction was not significant. A linear conversion of these data yields the data in Table 3, which represents the percentage of subject-jurors believing the briefed and not briefed witnesses who were accurate and inaccurate. These data show that the effect of witness accuracy is in the direction of believing inaccurate witnesses more than believing accurate witnesses. This odd result has some precedent (see Wells et al., 1979; Wells et al., 1980; Wells & Leippe, 1981), although its magnitude is surprising and inexplicable at this point. Table 3 shows that the effect of the briefing manipulation is clearly in the expected direction, that is, more belief of the eyewitness who was briefed than of the eyewitness who was not briefed.

Verdicts were treated in three ways. First, the number of individual guilty votes was analyzed by a 2 (witness accuracy) × 2 (briefed, not briefed witnesses) chi-square analysis. The results showed a significant main effect for briefing, $\chi^2(1) = 5.37$, $p < .05$, but no effect for witness accuracy and no interaction. The percentages of guilty votes by condition is presented in Table 4. Second, we calculated the percentage of sessions that would have yielded guilty verdicts if a majority decision rule would have applied (i.e., three or more votes of guilt). This analysis was subjected to a 2 × 2 chi-square analysis. There was no main effect for briefing, no effect for witness accuracy, and no interaction. These percentages are presented in Table 4, by condition. Finally, we calculated the percentage of sessions that would have yielded guilty verdicts if a unanimous

Table 2
Mean Witness Confidence at End of Cross-Examination as Functions of Witness Accuracy and the Briefing Manipulation

Witness	Not briefed	Briefed
Accurate	5.25_a	5.33_a
Inaccurate	3.83_b	6.08_a

Note. Higher numbers represent greater levels of confidence on the 7-point scale. Means not sharing a common subscript differ at $p < .05$, using the Newman-Keuls procedure.

Table 3
Percentage of Subject-Jurors Believing That the Witnesses Made Accurate Identifications as Functions of Witness Accuracy and the Briefing Manipulation

Witness	Not briefed	Briefed
Accurate	40	45
Inaccurate	44	72

decision rule would have applied (i.e., all four subject-jurors vote guilty). A 2 × 2 chi-square on this data yielded no significant effects. These percentages are also presented in Table 4.

Subject-jurors' estimations of the witnesses' confidence were analyzed as a 2 (witness accuracy) × 2 (briefed, not briefed witnesses) ANOVA. There was no effect for witness accuracy, $F(1, 148) > 1$, *ns*, nor any interaction, $F(1, 148) = 1.27$, *ns*. There was a marginally significant main effect for the briefing treatment, $F(1, 148) = 3.56$, $p \simeq .07$. Witness confidence was perceived to be higher for briefed witnesses ($M = 5.22$) than for nonbriefed witnesses ($M = 4.47$).[5]

The percentage of subject-jurors who indicated that they thought the witness had been coached or rehearsed averaged 34.2% across conditions. A 2 × 2 ANOVA produced no effects on this measure.

Table 4
Percentages of Guilty Votes and of Sessions Yielding Guilty Verdicts Under Majority and Unanimous Decision Rules as Functions of Witness Accuracy and the Briefing Manipulation

Witness	Not briefed	Briefed
Accurate		
Overall guilty votes	33	40
Sessions yielding a majority of guilty votes	20	40
Sessions yielding unanimous votes of guilt	10	30
Inaccurate		
Overall guilty votes	28	61
Sessions yielding a majority of guilty votes	11	56
Sessions yielding unanimous votes of guilt	11	22

Ancillary Analyses

Perceived witness confidence (estimated by subject-jurors) was significantly correlated with subject-jurors' belief of the witness ($r = .58$, $n = 152$, $p < .001$) and was significantly correlated with the witnesses' self-rated confidence ($r = .53$, $n = 34$, $p < .01$). Perceived witness confidence was unrelated to suspicions of subject-jurors that the witness had been coached or rehearsed ($r = .04$, $n = 152$, *ns*). Subject-jurors' suspicions that the witness had been coached was unrelated to their decisions to believe the witness ($r = -.08$, $n = 152$, $p > .1$). Postdeliberation judgments of guilt on the part of individual subject-jurors were primarily a function of the individual subject-juror's initial belief of the eyewitness. There were a total of only 27 subject-jurors who initially believed the witness and then voted not guilty after deliberation. A total of 12 subject-jurors initially disbelieved the witness and then voted guilty after deliberation. The remaining 113 subject-jurors voted guilty if they believed the witness and voted not guilty if they disbelieved the witness. There was slightly more change away from the majority (13 cases) than toward the majority (8 cases), and most change occurred in cases where initial belief of the witness was equally split among the subject-jurors (18 cases).

Discussion

Eyewitness confidence following an identification of a suspect is tractable. The current study further suggests that the inflation of confidence may be greater for inaccurate witnesses than it is for accurate witnesses. Perhaps this is due to the fact that the inaccurate witnesses began with a lower level of confidence, thereby giving the inaccurate witnesses more room for elevating their confidence. Stated another way, the accurate eyewitness's confidence in memory may already be high, producing a ceiling effect. Therefore, it is the inaccurate eyewitnesses who will show the greatest increase in confidence as a function of the briefing manip-

[5] Although subject-jurors saw the eyewitnesses' self-ratings of confidence on the TV monitors, subject-jurors did not simply mimic these values when asked to rate the witnesses' confidence (see Lindsay, Wells, & Rumpel, 1981). The correlation between witnesses' self-ratings and subject-jurors' estimations was .53, demonstrating that other cues are also used in estimating witness confidence.

ulation. Regardless of the processes involved, the data show clearly that confidence in a false memory can be enhanced. Thus, confidence is affected by things other than accuracy, which makes confidence a dubious criterion to be used in judging the trustworthiness of testimony. This stands in contrast to common assumptions of the lay observer who uses confidence to infer witness accuracy and the United States judiciary, which has formally designated confidence as a reliable cue to accuracy in a major United States Supreme Court case.[6]

Inflating eyewitness confidence requires nothing on the order of high-powered persuasion techniques. A simple instruction to rehearse the witnesses' account, sample questions that might be asked by a cross-examiner, and warnings that the cross-examiner will look for inconsistencies in the testimony are sufficient to inflate the witnesses' confidence in his or her memory. The effect is apparently more than just enhancing the confidence of the witness as perceived by subject-jurors. The witnesses seem to convince themselves of their accuracy, perhaps because the rehearsal involves a biased search for consistent supporting evidence (Koriat et al., 1980; Tesser, 1976).

The process of rehearsing a witness's recollections and briefing the witness on potential questions had a significant impact on verdicts rendered by subject-jurors in this experiment. The percentage of guilty votes increased from an average of 30.5% to an average of 50.5% as a function of briefing the witnesses. The percentage of jury sessions yielding a majority, or better, of guilty votes increased from an average of 15.5% to 48% as a function of the briefing manipulation. The percentage of jury sessions yielding a unanimous vote of guilty increased from an average of 10.5% to 26% as a function of the briefing manipulation. There was absolutely no benefit of the briefing manipulation in terms of subject-jurors *improving* their abilities to separate accurate from inaccurate witnesses. Instead the briefing manipulation seems to exacerbate the detection problem (see Table 3).

The practice of briefing eyewitnesses, although totally legal, seems to muddy an already difficult problem in the criminal justice system. Few psychologists should be surprised that confidence in memory has been shown to be easily inflatable. Confidence is a subjective judgment based on imperfect heuristics and, therefore, is responsive to factors that are unrelated to accuracy (see Koriat et al., 1980). The dangers of tractability in eyewitness confidence without changes in accuracy may be at least as important as its counterpart phenomenon showing tractability in accuracy without corresponding changes in confidence (e.g., Lindsay et al., 1981; Loftus et al., 1978; Putnam, 1979).

Is the practice of briefing or coaching eyewitnesses something that is done but never openly discussed in the criminal justice system? Clearly not. In fact the practice is openly advocated. A recently released film for law enforcement officers is solely devoted to stressing the importance of thorough witness preparation. It stresses the initial meeting between witness and prosecuting attorney; the use of role playing; discussion of appropriate courtroom demeanor for witnesses, including proper posture, avoidance of nervous gesture, good eye contact with jurors, giving concise and complete answers, and pausing before answering (Anderson, 1979). These openly advocated procedures for rehearsing witnesses would seem to be considerably more robust than the briefing manipulation we used. These may be effective ways to achieve one's goal of increasing eyewitness impact but may not be in the best interests of justice. At the very least the practice of coaching eyewitnesses goes against the general argument that eyewitness testimony should have less rather than more impact in court (Goldstein, 1977); the current study also suggests that it may serve to destroy any already-existing confidence–accuracy relationship.

Are there practical solutions to this problem? Is there a systems variable that might reduce the magnitude of the problem? Perhaps there is. Doob (Note 1) likens the criminal justice system's use of lineups to an experiment for which our knowledge of

[6] Neil v. Biggers, 409 U.S. 188 (1972).

experimental design and procedure may apply. This suggests at least one possibility. Solomon (1949), for example, was one of the first to acknowledge formally that a pretest–posttest design can reduce the efficacy of an intervening manipulation. Although experimental psychologists usually consider this a reason to avoid pretests, it suggests that an early measure of confidence (i.e., prior to the witnesses' exposure to a briefing treatment) may dampen the confidence-inflation effect.

Finally, we must acknowledge that our manipulation of briefing was a compound manipulation. We do not know, therefore, whether it was the warning of the upcoming cross-examination, the information about what kinds of questions may be asked, or the request of the witness to rehearse that accounts for the confidence-inflation effect. Without intervening measures of thought generation (e.g., Petty, Wells, & Brock, 1976; Tesser, 1976), we cannot be certain that the biased cognitive search was responsible for our effects.

Although we cannot precisely specify the cognitive processes operative in the confidence-inflation effect observed here, we have gained some knowledge of the theoretical aspects of the confidence–accuracy relationship. At the least the results of this study suggest that the confidence–accuracy relationship issue is more complex than previously thought. Deffenbacher (1980), for example, suggested that the strength of the confidence–accuracy relationship is a positive function of the level of accuracy. We now know, however, that manipulations of eyewitness accuracy do not necessarily affect confidence or the accuracy–confidence relationship (Lindsay et al., 1981) and that both confidence and the confidence-accuracy relationship are changeable when accuracy is held constant (current study). Theoretical models of the confidence–accuracy relationship, therefore, must recognize that confidence and accuracy have some orthogonal causes. That is, some things cause changes in accuracy but not confidence, whereas other things cause changes in confidence but not accuracy. It is the presence or absence of events in this orthogonal-cause category that produce noncorrespondence of confidence and accuracy.

Reference Note

1. Doob, A. N. *Police procedures and psychological knowledge in eyewitness identification.* Paper presented at the Psychology of Eyewitness Testimony Conference, Edmonton, Alberta, Canada, June 1980.

References

Anderson, D. (Writer and producer). *Courtroom performance.* Schiller Park, Ill.: Woroner Films, 1979. (Film)

Bem, D. J. An experimental analysis of self-persuasion. *Journal of Experimental Social Psychology,* 1965, *1,* 199–218.

Brehm, J. W. *A theory of psychological reactance.* New York: Academic Press, 1966.

Deffenbacher, K. Eyewitness accuracy and confidence: Can we infer anything about their relationship? *Law and Human Behavior,* 1980, *4,* 243–260.

Einhorn, H. J., & Hogarth, R. M. Confidence in judgment: Persistence in the illusion of validity. *Psychological Review,* 1978, *85,* 395–416.

Fischhoff, B., Slovic, P., & Lichtenstein, S. Knowing with certainty: The appropriateness of extreme confidence. *Journal of Experimental Psychology: Human Perception and Performance,* 1977,*3,* 522–564.

Goldstein, A. G. The fallibility of the eyewitness: Psychological evidence. In B. D. Sales (Ed.), *Psychology in the legal process.* New York: Spectrum, 1977.

Johnson-Laird, P. N., & Wason, P. C. A theoretical analysis of insight into a reasoning task. In P. N. Johnson-Laird & P. C. Wason (Eds.), *Thinking.* Cambridge, England: Cambridge University Press, 1977.

Koriat, A., Lichtenstein, S., & Fischhoff, B. Reasons for confidence. *Journal of Experimental Psychology: Human Learning and Memory,* 1980, *6,* 107–118.

Leippe, M. R. Effects of integrative and memorial cognitive processes on the correspondence of eyewitness accuracy and confidence. *Law and Human Behavior,* 1980, *4,* 261–274.

Leippe, M. R., Wells, G. L., & Ostrom, T. M. Crime seriousness as a determinant of eyewitness accuracy. *Journal of Applied Psychology,* 1978, *63,* 345–351.

Lindsay, R. C. L., Wells, G. L., & Rumpel, C. Can people detect eyewitness identification accuracy within and across situations? *Journal of Applied Psychology,* 1981, *66,* 79–89.

Loftus, E. F., Miller, D. G., & Burns, H. J. Semantic integration of verbal information into visual memory. *Journal of Experimental Psychology: Human Learning and Memory,* 1978, *4,* 19–31.

McGuire, W. J. A syllogistic analysis of cognitive relationships. In M. J. Rosenberg, C. I. Hovland, W. J. McGuire, R. P. Abelson, & J. W. Brehm (Eds.), *Attitude organization and change.* New Haven, Conn.: Yale University Press, 1960.

Petty, R. E., Wells, G. L., & Brock, T. C. Distraction can enhance or reduce yielding to propaganda: Thought disruption vs. effort justification. *Journal of Personality and Social Psychology,* 1976, *34,* 874–884.

Putnam, W. H. Hypnosis and distortions in eyewitness memory. *International Journal of Clinical and Experimental Hypnosis,* 1979, *27,* 437–448.

Sadler, O., & Tesser, A. Some effects of salience and time upon interpersonal hostility and attraction during social isolation. *Sociometry,* 1973, *36,* 99–112.

Solomon, R. L. An extension of the control group design. *Psychological Bulletin,* 1949, *46,* 137–150.

Tesser, A. Thought and reality constraints as determinants of attitude polarization. *Journal of Research in Personality,* 1976, *10,* 183–194.

Tesser, A., & Conlee, M. C. Some effects of time and thought on attitude polarization. *Journal of Personality and Social Psychology,* 1975, *31,* 262–270.

Tesser, A., & Cowan, C. L. Some effects of thought and number of cognitions on attitude change. *Social Behavior and Personality,* 1975, *3,* 165–173.

Tesser, A., & Cowan, C. L. Some attitudinal and cognitive consequences of thought. *Journal of Research in Personality,* 1977, *11,* 216–226.

Wells, G. L. Applied eyewitness testimony research: System variables and estimator variables. *Journal of Personality and Social Psychology,* 1978, *36,* 1546–1557.

Wells, G. L., & Leippe, M. R. How do triers of fact infer the accuracy of eyewitness identifications? Using memory for peripheral detail can be misleading. *Journal of Applied Psychology,* 1981, *66,* 682–687.

Wells, G. L., & Lindsay, R. C. L. On estimating the diagnosticity of eyewitness nonidentifications. *Psychological Bulletin,* 1980, *88,* 776–784.

Wells, G. L., Lindsay, R. C. L., & Ferguson, T. J. Confidence, accuracy, and juror perceptions in eyewitness identification. *Journal of Applied Psychology,* 1979, *64,* 440–448.

Wells, G. L., Lindsay, R. C. L., & Tousignant, J. P. Effects of expert psychological advice on human performance in judging the validity of eyewitness testimony. *Law and Human Behavior,* 1980, *4,* 275–286.

Wyer, R. S. *Cognitive organization and change: An information processing approach.* Potomac, Md.: Erlbaum, 1974.

Received December 29, 1980 ■