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On the Requirements of Proof: The Timing of Judicial Instruction and Mock Juror Verdicts

Saul M. Kassin and Lawrence S. Wrightsman
University of Kansas

At the close of a trial, the judge instructs the jury that the defendant is presumed innocent, that the burden of proof is on the prosecution, and that guilt must be established beyond a reasonable doubt. In view of criticisms that the judge's charge has no effect on jury decisions, the present study examined whether the timing of judicial instruction mediates its efficacy. One hundred seven mock jurors watched a 1-hour videotape of a trial and were instructed by the judge either before the evidence, after the evidence, or not at all. Results for posttrial measures indicated that although the timing manipulation had no significant effect on the standards of reasonable doubt adopted by subjects, those who were instructed before the evidence viewed the defendant as less likely to have committed the crime and demonstrated a lower conviction rate than subjects in the instructions-after and no-instructions groups. Results for a series of mid-trial judgments of 54 subjects further indicated that pre-instructed subjects were less likely to convict throughout the trial. These findings are discussed, and their implications for procedural reform in the courtroom are noted.

In trials by jury, the judge is obligated to instruct jurors in both general and case-specific matters of law. These instructions serve a number of functions: The judge orients jurors in their task, outlines the undisputed facts and issues of the case, explains the relevant law, and informs jurors about procedural matters (McBride, 1969). From the defendant's perspective, perhaps the most crucial of the mandatory instructions is that concerning the "requirements of proof" (LaBuy, 1963). Specifically, the accused is entitled to the instruction that he or she is presumed innocent, that the burden of proof is on the prosecution, and that all elements of the crime must be proven to a constitutional standard of "beyond a reasonable doubt."

Although the potential importance of the judge's charge is widely recognized (McCart, 1964), its actual effectiveness is a subject of controversy. On the one hand, the courts assume that jurors understand their instructions, use them in making decisions, and are acutely sensitive to even minor variations in wording. Thus, attorneys often request specific instructions, appellate courts have occasionally reversed verdicts on the basis of an improperly worded instruction, and many states currently favor the practice of having judges recite a preapproved pattern instruction in order to ensure standardization. Experimental support for the effect of variations in instructional content has recently been obtained. Kerr et al. (1976) presented mock jurors with one of three definitions of reasonable doubt: one that described a lax criterion of reasonableness (i.e., "you need not be absolutely sure that the defendant is guilty to find him guilty," p. 286), one that described an extremely stringent criterion (i.e., "if you are not sure and certain of his guilt, you must

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Requests for reprints should be addressed to Saul M. Kassin, who is now at the Department of Psychological Sciences, Purdue University, West Lafayette, Indiana 47907.

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find him not guilty," p. 286), and one in which reasonable doubt was not defined. As it turned out, these varying definitions significantly influenced both individual and group verdicts—a lax criterion resulted in a high conviction rate, whereas a stringent one produced a low rate of conviction. Subjects for whom reasonable doubt was not explicated fell between these extremes. This study thus demonstrated that verdicts are indeed influenced by the reasonable doubt element of the requirements-of-proof instruction.

On the other hand, it has been suggested by legal scholars (Frank, 1949) and researchers (Sealy & Cornish, 1973) that these instructions have little effect on jurors' verdicts. One common criticism is that because they are written in statutory language, the instructions are often confusing to laypersons untrained in the law (Elwork, Sales, & Alfini, 1977). In fact, one study revealed that 40% of 375 sampled jurors reported that they did not understand their judge's instruction (Hervey, 1947). A second criticism is aimed at the timing of the judge's charge. Although the procedure is not fixed by law, the jury is typically instructed at the close of the trial presentation, that is, after the evidence has been presented. Although it is possible that this sequence increases the salience of the instruction and its availability for recall during deliberation,[1] a number of sources (e.g., McBride, 1969) have questioned the utility of an instruction that is given at a stage where jurors might have already decided on a verdict. Kalven and Zeisel (1966) noted that jurors often form very definite opinions about a defendant's guilt or innocence before the close of the trial. Accordingly, Judge E. Barrett Prettyman (1960) argued the following:

> It makes no sense to have a juror listen to days of testimony only then to be told that he and his conferees are the sole judges of the facts, that the accused is presumed to be innocent, that the government must prove guilt beyond a reasonable doubt, etc. What manner of mind can go back over a stream of conflicting statements and alleged facts, recall the intonations, the demeanor, or even the existence of the witnesses, and retrospectively fit all these recollections into a pattern of evaluation and judgment given him for the first time after the events; the human mind cannot do so. . . . Why should not the judge, when the jury is sworn, then and there tell them the rules of the game. (p. 1066)

In view of the flexibility in courtroom procedure and the potential importance of a judge's charge to the jury, the absence of research on temporal factors associated with the instructions is surprising (Elwork, Sales, & Alfini, 1977, is an exception). After all, order effects in the perception of unfolding behavior sequences have repeatedly been noted in both person perception (Jones et al., 1968) and jury contexts (Walker, Thibaut, & Andreoli, 1972). In the latter study, Walker et al. (1972) varied the order in which the prosecution and defense presented their cases but did not address the issue of judicial instruction.

The present study was designed to investigate the relationship between the timing of a judge's instructions and mock juror verdicts for a criminal case. In particular, the aims of the research were twofold. First, we sought to determine whether or not a prototypical, ecologically valid instruction on the requirements of proof influences jurors' decisions and whether the timing of that instruction mediates its efficacy. In order to achieve these goals, requirements-of-proof instructions were gleaned from those currently employed (DeVitt & Blackmar, 1977; LaBuy, 1963; McBride, 1969) and were introduced to subjects before testimony, after testimony, or not at all. At the conclusion of the trial presentation, subjects rendered their individual verdicts and answered a number of other case-related questions. Since the requirements of proof are defendant oriented, their effectiveness should be manifested in a lowered rate of conviction. Based on jurors' tendencies to make early decisions about guilt or innocence and Judge Prettyman's (1960) reasoning that instructions can have their intended effects only when they are delivered before jurors have made up their minds, a kind of primacy or inoculation effect was predicted. That is, the present instructions should produce fewer guilty verdicts when presented before the evidence than when presented after it.

The second general aim of the present study was to explore the mechanism or cog-

[1] Jones and Goethals (1971) have termed this the "recall readiness" hypothesis for recency effects.

nitive process that underlies the proposed order effect. Requirements-of-proof instructions may operate either by decreasing the belief that the defendant committed the crime (i.e., a lowered probability-of-commission estimate) or by increasing the standard or threshold to which that likelihood is compared (i.e., a stringent interpretation of reasonable doubt). Consequently, both were assessed. A collateral issue addressed here was whether or not subjects who are instructed prior to the presentation of evidence evaluate evidentiary information differently as it unfolds from those who do not receive prior instruction. Accordingly, half the subjects in each instruction group made judgments of guilt–innocence at various points during the trial. It was hypothesized that instructed subjects would set a higher standard by which to evaluate the case against the defendant and would therefore be less influenced by the prosecutor's case throughout the trial.

Method

Subjects and Design

A total of 107 introductory psychology students (47 male, 60 female) participated in the study. The experiment was conducted in 25 small groups ranging in size from 3 to 7 and took 1 hour and 20 minutes to complete. Each group was randomly assigned to one of six cells produced by the 3 (Judge's Instructions Before Evidence, Instructions After Evidence, No Instructions) × 2 (Multiple Judgments vs. Single Judgment Only) factorial design.

Procedure

Upon entering, subjects were told that they would observe a videotaped trial simulation, after which they would be asked to render a verdict. They were further instructed that as jurors they should be attentive but should not take notes and should not converse while the trial presentation was in progress.

All subjects were then told that in order to get the entire trial presentation on one reel of tape, a few pauses and meaningless exchanges had been deleted, but that all the testimony remained intact. Those groups who were to receive the judge's instruction, however, were informed that this instruction had inadvertently been deleted but would be read to them at the appropriate time from the original transcript. Finally, subjects in the multiple-judgment cells were informed of the fact that at certain points the tape would be stopped and their judgments would be assessed. The trial was then presented.

The Trial

The stimulus trial was one that had previously been employed (Juhnke et al., 1979). Stylistically, the simulation was presented on a 1-hour (black and white) videotape in which a number of law students retried a case in a realistic courtroom setting. It was filmed from a juror's perspective: the judge, attorneys, and witness stand were all in view.

Substantively, the trial was based on an actual criminal case in which the defendant, Ronald Oliver, was charged with stealing a car and with transporting it across state lines.[2] Though continuously presented, the trial consisted of the following three distinct phases: (a) opening statements by prosecutor and defense; (b) direct examination, cross-examination, and redirect examination of two prosecution witnesses (the salesman from whom the vehicle was stolen and the arresting officer) and one defense witness (the defendant); and (c) closing arguments of counsel (prosecution, defense, prosecution). The judge's instructions on the requirements of proof represented a fourth phase whose presence–absence and timing were varied.

In one condition, these instructions appeared prior to the introduction of evidence (i.e., between the first and second phases). In a second condition, they appeared after the closing arguments (i.e., after the third phase). In a third condition, no instruction was given. The specific instruction employed was neither strong nor weak. Rather, it was patterned after the approved instructions designed to convey each element of the requirements of proof: presumption of innocence, burden of proof, and reasonable doubt (see DeVitt & Blackmar, 1977; McBride, 1969). The instruction read as follows:

> Ladies and gentlemen of the jury—at this point I want to emphasize that the law presumes the defendant, Ronald Oliver, to be innocent unless proven otherwise. A defendant begins the trial with a "clean slate" with no evidence against him.
>
> This presumption places the burden *not* upon the defendant to prove his innocence but, on the contrary, the burden is on the prosecution to convince you beyond any reasonable doubt that the defendant, Ronald Oliver, committed the crime. That burden never shifts at any stage of the proceeding to the defendant. Ronald Oliver has no obligations of any kind to go forward and prove that he is innocent.
>
> You have now heard the term "reasonable doubt."

[2] A transcript taken verbatim from the videotape and a summary of the trial are available upon request.

Table 1
Pattern of Final Verdicts in Each of the Six Cells

Instruction and verdict	Multiple judgments		Single judgments	
	No.	%	No.	%
Before				
Guilty	7	39	6	35
Not guilty	11	61	11	65
After				
Guilty	12	63	10	56
Not guilty	7	37	8	44
None				
Guilty	12	71	10	56
Not guilty	5	29	8	44

> What is it? It is a doubt based upon reason and common sense—the kind of doubt that would make a reasonable person hesitate to act in important matters. To summarize, the defendant is presumed innocent, so the prosecution must prove to your satisfaction beyond any reasonable doubt that Ronald Oliver is guilty. If two conclusions can reasonably be drawn from the evidence—one of innocence and one of guilt—the jury should adopt the one of innocence.

Dependent Measures

At the close of the trial, all subjects responded individually and without deliberation to a two-page questionnaire in which they first rendered a dichotomous judgment (guilty–not guilty) and indicated their confidence in that verdict on a 0–8-point scale. They then rated the strength of the evidence as well as their interest and involvement in the case (all on 0–8-point scales).

Since verdicts are a function of the perceived probability that the defendant committed the crime and of the standard of proof deemed necessary for conviction, both of these variables were also assessed. All subjects were thus asked, "What is the likelihood that the defendant committed the crime?" to which they responded by circling a number from 0 to 100 (in multiples of 5), and "A defendant should be found guilty if there is at least a _____% chance that he committed the crime." Finally, subjects answered 16 short-answer recall questions that pertained to the major facts of the case (e.g., "On what highway was Ron Oliver stopped?"). The total number of correctly recalled items served as a measure of fact recall.

In addition to providing these outcome data, half the subjects indicated their judgments (guilty–not guilty), confidence values, and probability-of-commission estimates at six distinct points during the trial—after both the direct examination and cross-examination of each witness. Specifically, they were asked, "If the trial ended now, would you vote that the defendant is *guilty* or *not guilty*?" "How confident are you in this judgment?" (0–8) "What is the likelihood that the defendant committed the crime?" (0–100). It was thus possible to trace beliefs as the trial unfolded and to examine whether subjects who were already instructed on the requirements of proof (instructions before) evaluated the evidence differently from those who had not yet been instructed (instructions after and no instructions). Previous research employing these multiple judgments (e.g., Weld & Danzig, 1940) has been criticized on the ground that such a procedure may bias final verdicts (Davis, Bray, & Holt, 1977). For that reason, only half the subjects in the present study made these on-line responses. A comparison of their posttrial responses with those of single-judgment subjects thus provided a test for the (non)reactivity of the procedure.

Results

Outcome Measures

Overall, 53% of the subjects voted guilty and 47% voted not guilty. The pattern of verdicts in each cell appears in Table 1.

In order to test for all main and interaction effects and to determine which model best fits these categorical data, these dichotomous judgments were analyzed with a likelihood ratio (goodness of fit) chi-square (Fienberg, 1977). Results indicated that the simplest model that described the observed frequencies was the two-way interaction between instructional set and verdicts, $\chi^2(6) = 1.21$, $p > .97$. None of the more complicated models (i.e., those involving the multiple vs. single judgments factor) contributed any explanatory power to this Instructional Set × Verdict model. Put another way, the pattern of verdicts was accounted for by the main effect for timing of instruction. Table 1 shows that the instructions-before condition produced 37% guilty verdicts, compared to 59% in the instructions-after and 63% in the no-instructions conditions.

A scalar variable was defined by combining subjects' verdicts with their confidence ratings (confidence itself was unaffected by the independent variables). Specifically, positive confidence values were assigned to guilty verdicts and negative values to verdicts of not guilty. Scores could thus range from −8 (maximum confidence in not-guilty verdict)

to 8 (maximum confidence in guilty verdict). A 2 × 3 analysis of variance on this measure revealed one marginally significant effect for timing of instruction, $F(2, 101) = 2.90$, $p < .06$. Duncan's multiple-range test further indicated that subjects in the instructions-before condition were less likely ($p < .05$) to convict the defendant than were noninstructed subjects (means of −.94 and 2.35, respectively). Instructions-after ($M = 1.79$) and noninstructed conditions did not so differ. An analysis of variance on the probability-of-commission estimates yielded results that closely paralleled those for the verdict–confidence measure—the only significant effect was for timing of the judge's instruction, $F(2, 101) = 2.92$, $p < .06$. Only subjects who received instructions before the evidence viewed the defendant as less likely to have committed the crime ($p < .05$) than did the noninstructed subjects (mean percentage estimates of 64% and 77.6%, respectively); those who were instructed after the reception of evidence ($M = 73.1\%$) did not lower their probability-of-commission estimates.

Contrary to predictions, no significant differences were obtained on evaluations of evidence strength, interpretations of reasonable doubt, or self-ratings of interest and involvement. Interestingly, a close look at the reasonable-doubt data indicates that the overall estimate of reasonable doubt ($M = 86.07$) was almost identical to that previously reported for college students (Simon & Mahan, 1971, obtained a general estimate of 87% and an estimate of 85% when the crime was auto theft). Subjects thus believed that there should be at least an 86% chance that the defendant committed the crime in order to vote for conviction.

Finally, subjects demonstrated a moderately high level of recall, averaging 12.15 correctly recalled items from a total of 16.[3] Further analysis indicated that a main effect was obtained for timing of instruction on the number of case-related facts recalled, $F(2, 101) = 3.26$, $p < .05$. Surprisingly, subjects in the instructions-after condition recalled fewer items ($M = 11.30$) than those in either the instructions-before ($M = 12.51$) or noninstructed ($M = 12.69$) conditions. The presence of the instruction between the evidence and response assessment apparently inhibited recall.

Table 2
Pattern of Mid-Trial Verdicts for the Three Instruction Groups at Each of the Six Decision Points

Instruction and verdict	Decision point 1	2	3	4	5	6
Before						
Guilty	8	8	8	7	5	8
Not guilty	10	10	10	11	13	10
After						
Guilty	16	16	17	16	13	15
Not guilty	3	3	2	3	6	4
None						
Guilty	15	14	15	14	14	12
Not guilty	2	3	2	3	3	5

Note. Decision points 1–6 immediately follow the direct examination and cross-examination of the prosecution's two witnesses and the defendant.

In sum, the mere timing of instructions had an impact on jurors' final judgments. Results for the verdict–confidence measure tended to support the major hypothesis that instructions on the requirements of proof would reduce subjects' tendencies to convict (i.e., relative to no instructions) only when delivered prior to the introduction of testimony. Although the timing variable had no significant effect on interpretations of reasonable doubt, preinstructed subjects actually viewed the defendant as less likely to have committed the crime than either the instructions-after or no-instructions subjects. Those who received instructions at the end of the trial did not respond differently on the verdict and probability-of-commission measures from those who were never instructed. In fact, they even recalled fewer of the case-related

[3] Only the major facts in the case were tested. Moreover, these facts were typically repeated throughout the trial proceedings (e.g., the highway on which Ron Oliver was stopped was first mentioned during the prosecutor's opening statement and was reiterated during the examination of both the arresting officer and the defendant and during the closing arguments). As a result, the facts tested could not be classified for further analysis as proprosecution or prodefense, nor could they be located at a single point in the trial.

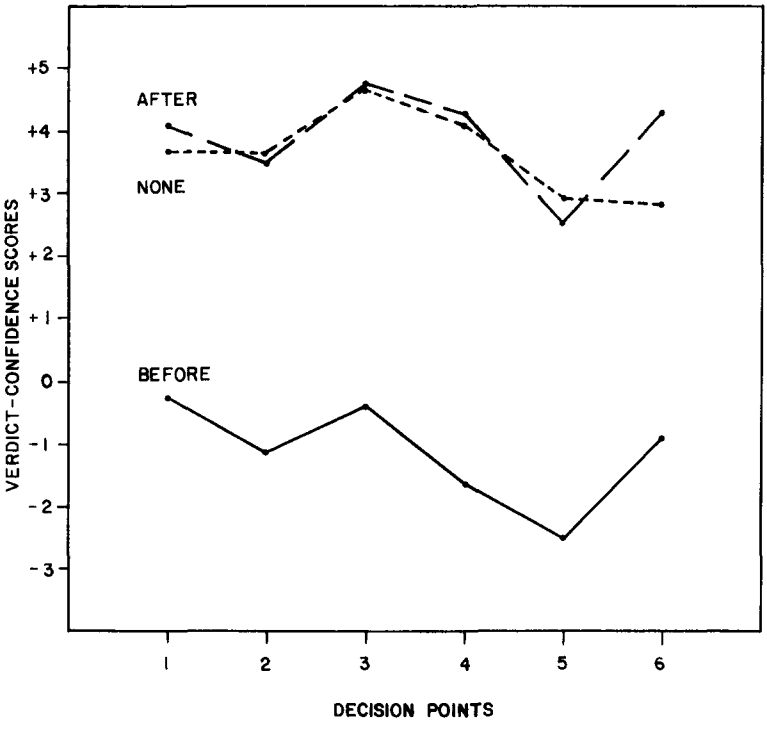

Figure 1. The pattern of verdict-confidence scores for the three groups at each of six decision points. (Higher scores indicate confidence in guilt.)

facts than all other subjects did. Finally, whether or not subjects made mid-trial judgments did not affect any of the above variables. The nonreactive nature of this multiple-judgment procedure was thus confirmed.

Process (Mid-Trial) Measures

Overall, 68% of the mid-trial responses were guilty verdicts. For the 54 subjects who made these judgments, the pattern of verdicts is presented in Table 2. As with the outcome data, these verdicts were analyzed with a likelihood ratio goodness-of-fit chi-square. Again, the simplest model that explained the observed frequencies was the two-way interaction between instructional set and verdicts, $\chi^2(30) = 6.50$, $p = 1.0$. Although a higher order model comprised of both Instruction × Verdict and Decision Point × Verdict interactions also fits the data, $\chi^2(20) = 3.13$, $p = 1.0$, it did not contribute significantly to the predictions made by the simpler model. Mid-trial verdicts were thus accounted for by the effect for instructional set. Table 2 shows that across all decision points the instructions-before group yielded 41% guilty verdicts, compared to 82% in both the instructions-after and no-instructions groups.

A verdict-confidence measure was again created and was this time submitted to a 3 (Instructions) × 6 (Decision Point) analysis of variance. Figure 1 illustrates the strong main effect for the instruction factor, $F(2, 51) = 10.15$, $p < .001$, on these verdict scores. As before, this analysis clearly indicated that subjects who were instructed before the evidence ($M = -1.14$) were consistently less likely to convict the accused ($p < .001$) than were either the instructions-after or noninstructed subjects (Ms = 3.91 and 3.65, respectively). These latter groups did not differ. An additional main effect for the decision point factor, $F(5, 255) = 4.26$, $p < .001$, indicated, as expected, that verdict scores fluctuated widely as the trial pro-

Table 3
Correlation Coefficients of Mid-Trial and Final Verdict–Confidence Scores in Each Group and Across All Groups

Group	Verdict–confidence score					
	1	2	3	4	5	6
Before	.14	.42	.57**	.46**	.42	.54*
After	.22	.22	.18	.15	−.03	−.15
None	.31	.30	.37	.42	.45	.86***
Overall	.28*	.41**	.47***	.44**	.36**	.52***

Note. Correlations are based on *n*s of 18, 19, and 17 ($N = 54$).
* $p < .05$. ** $p < .01$. *** $p < .001$.

gressed. As might be anticipated, the conviction rate was highest at the third point—right after the prosecutor examined his second witness ($p < .01$), and lowest at the fifth point—right after the direct examination of the defendant ($p < .01$). The interaction between instructional set and decision point did not approach significance ($F < .1$).

On the question of whether jurors' early impressions were predictive of their final predeliberation verdicts, Table 3 presents the correlations between subjects' mid-trial and final verdict–confidence scores. It can be seen that over all multiple-judgment groups the average correlation between mid-trial and posttrial verdict scores was .44 ($p < .01$). In fact, the average correlation between the verdict scores rendered at even the first decision point and those given at the end of the trial was .28 ($p < .05$). To some extent, then, subjects' decisions were substantially formed very early in the trial presentation.

Results for the repeated probability-of-commission measures followed a similar pattern (see Figure 2). A main effect for timing of instruction, $F(2, 5) = 6.58$, $p < .005$, re-

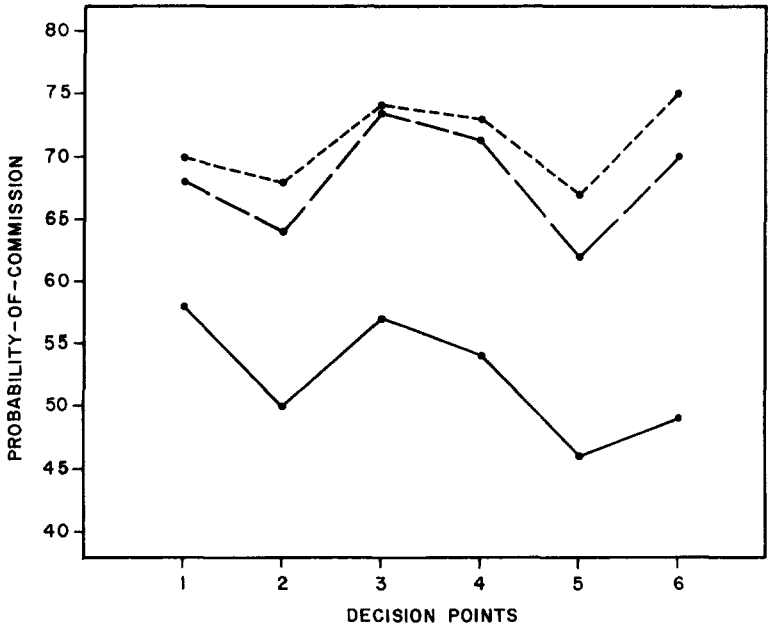

Figure 2. The pattern of probability-of-commission estimates for the three groups at each of six decision points.

vealed that during the trial, the instructions-before group ($M = 52.5\%$) viewed the defendant as less likely to have committed the crime ($p < .01$) than either the instructions-after or no-instructions groups (68.16% and 71.67%, respectively), who again did not differ from each other. Moreover, a main effect for the decision point factor, $F(5, 255) = 3.08$, $p < .01$, indicated that the perceived probability of commission was lowest at the fifth point—right after the defendant testified in his own behalf.

In sum, subjects' mid-trial judgments fluctuated in the predicted directions (i.e., guilty judgments after the prosecutor's examination and not-guilty judgments after the defense's examination). More important, these judgments were influenced largely by whether or not subjects had been instructed on the requirements of proof. That is, although instructional set and decision point did not interact (i.e., their curves were almost perfectly parallel, thereby indicating that pre-instructed subjects were neither less influenced by the prosecutor's testimony nor more influenced by the defendant's testimony), subjects who had been instructed before the evidence immediately and continually viewed the defendant as less likely to have committed the crime.

Discussion

The present study demonstrated what might be described as a primacy effect. A judge's instruction to the jury was effective when delivered prior to but not after the presentation of evidence. That is, mock jurors who were instructed on the presumption of innocence, burden of proof, and reasonable doubt before observing the testimony were ultimately less likely to vote for conviction. It was noted earlier that verdicts are a function of the perceived likelihood that the defendant committed the crime and of the standard or threshold to which that likelihood is compared. In the present experiment, variations in the timing of the instruction affected estimates of the probability of commission but not interpretations of reasonable doubt. Subjects who received the instructions before the evidence thus demonstrated a low rate of conviction because they actually viewed the defendant as less likely to have committed the crime.

Although the present experiment was not theoretically guided, it now appears that an information integration model of juror judgments (Kaplan & Kemmerick, 1974) might well describe the results. Briefly, each piece of evidentiary and nonevidentiary information possesses some scale value along a dimension of guilt–innocence and a weight that determines the importance of that information. A trial judgment is then formed on the basis of a weighted-average combination of stimulus components. In the context of the present study, subjects received two global categories of information: the trial presentation and judicial instruction. From the probability-of-commission responses of noninstructed subjects, the scale value of the trial information may be estimated at .78 (i.e., .78 is the marginal mean for noninstructed subjects). Although the scale value of the requirements of proof instruction was not assessed in the present study, it should—by legal ideal—have implied a scale value or initial probability of commission of 0 (Ostrom, Werner, & Saks, 1978). On the assumption that the scale values of the instruction and trial presentation remained constant (these assumptions are supported indirectly by the lack of a timing effect on interpretations of reasonable doubt and ratings of evidence strength, respectively), to what do we attribute the effects for the timing manipulation? Within an impression formation paradigm, Anderson (1965) found that later adjectives keep a fixed scale value but decrease in their weight. This mechanism describes the present results—the prodefendant instruction received a greater weight when delivered before than when delivered after the evidence.[4]

What *process* underlies these findings? At least two plausible mechanisms are worth evaluating. It was hypothesized that pre-instructed subjects would demand a greater burden of proof when evaluating the strength

[4] Note that although the present analysis treats judges' instructions as information that has a scale

of the prosecutor's case as it unfolds during the trial. This critical, evaluative "schema" was expected to manifest itself in different fluctuating patterns in the responses of the instructed (instructions-before) and noninstructed (instructions after and no instructions) mid-trial judgment groups. However, the absence of an Instruction × Decision Point interaction on verdicts and probability-of-commission ratings ruled out this hypothesis. In fact, the three instruction groups demonstrated remarkably similar mid-trial shifts in judgment (see Figure 1). Those who had been instructed were neither more influenced by the defense nor less influenced by the prosecutor than the others were. Instructed subjects thus did not reject or distort the prosecutor's evidence to fit an initial impression or presumption of innocence. And why should they? Luchins (1957) has demonstrated that by forewarning observers of the imminence of additional information, the proactive effects of early information on subsequent information are suppressed. Subjects in the present study and jurors in general *expect* to be confronted with inconsistent information. Early information in this setting (i.e., judicial instruction) is thus unlikely to produce the strong bias that would stimulate the processes of discounting or assimilation.

The process that did appear to operate was considerably less complex. Subjects who were instructed before the evidence were more likely than the others were to vote for acquittal and indicated a lower probability of commission *even from the first decision point.* Although they responded similarly to testimony that followed, their initial leanings resulted in fewer guilty verdicts at the conclusion of the trial. The impact of this initial reaction is reflected in the significant correlation between first and final verdict scores. Simply put, most preinstructed subjects "presumed innocent," whereas the others "presumed guilty."

The *practical* implications of the present study are straightforward. Mandatory code provisions do not forbid or discourage judges from using their discretion in instructing the jury before the trial. A number of states (e.g., Indiana, 1966), individual judges (e.g., Prettyman, 1960), and legal scholars (e.g., McBride, 1969) have thus adopted or advocated delivery of preliminary instructions prior to the presentation of evidence. In Missouri (1964), for example, the Supreme Court Committee on Jury Instructions recommended that "the jury be instructed before the trial begins. . . . The committee believes that it is better to draw the jury's attention to these matters before the trial rather than waiting until after the jurors may have reached a decision" (quoted in McBride, 1969, p. 62). This latter example is unfortunately an exception rather than the rule, which is that preliminary instructions are vastly underutilized (DeVitt & Blackmar, 1977). Viewed in this context, the present study provides firm support for proponents of procedural reform in the courtroom. The Anglo-American system of justice has traditionally favored the accused on the philosophy that acquitting a truly guilty person is better than convicting a truly innocent one (Ploscowe, 1935). Yet the present results suggest that the accused may not benefit from this protective instruction unless it is delivered to jurors *before* the trial. In this regard, perhaps the requirements of proof might well be included in the currently popular "juror handbooks" that are distributed to jurors as part of a pretrial orientation (National Institute of Law Enforcement and Criminal Justice, 1975) and might perhaps be presented at both the beginning and the end of the proceedings (Elwork et al., 1977).

From a methodological standpoint, the multiple-judgment procedure initially employed by Weld and Danzig (1940) merits increased consideration in future research. The present study confirmed that it does not bias jurors' ultimate verdicts or any other posttrial measures (also see Pyszczynski, Note

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value and weight, Kaplan and his colleagues are quick to point out that strictly speaking, because instructions do not pertain to the specific defendant or crime, they do not relate to information scale value (S_I). Instead, instructions should affect an initial impression of the defendant that exists prior to jurors' receiving information about him or her (S_O), that is, the impression of defendants in general (see Kaplan & Miller, 1978).

1). Moreover, it appears to be valuable for examining jurors' reactions to the trial as it unfolds. Questions concerning the relative impact of different trial phases (e.g., opening statements and closing arguments, direct examination vs. cross-examination) and different kinds of evidence (e.g., tangible exhibits, expert or eyewitness testimony) may be fruitfully investigated through the mid-trial assessment of verdicts.

Finally, some of the limitations of the present results deserve mention. As with other nonevidentiary factors, the timing of an instruction cannot be expected to influence the outcome of a one-sided case (i.e., an extremely strong or weak case against the defendant). Rather, the effects should be considered limited to close, ambiguous cases. Note also that the present experiment assessed the verdicts of nondeliberating jurors. Whether or not deliberation provides enough of a corrective strategy to erase the timing effect upon juries remains open to question. Finally, the trial used in the present study was much shorter than the average criminal case. Whether or not the timing manipulation would affect jurors' responses in realistically longer trials remains to be seen.

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