Researching the Hearsay Rule: Emerging Findings, General Issues, and Future Directions

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Researching the Hearsay Rule:
Emerging Findings, General Issues,
and Future Directions

Richard F. Rakos* and Stephan Landsman**

I. THE HEARSAY RULE AS A FOCUS OF EMPIRICAL INVESTIGATION

In recent years, scholars applying social science to the law have sought to test a wide array of legal assumptions empirically. Researchers working on evidentiary issues have provided data about the use of prior convictions, coerced confessions, eyewitness identifications, expert testimony, post-hypnotic

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The order in which the authors' names are listed is the result of an agreement growing out of previous coauthorships, and should not be taken as an indication of relative contributions to this article. The authors thank William Danko, Administrator of the Cuyahoga County (Ohio) Court of Common Pleas, Brenda Johnson and Maureen Klemens for their research assistance in the preparation of this Article, and Professor Michael Saks for his comments and advice.


3. For a recent summary of this large body of research, see LAWRENCE S. WRIGHTSMAN, PSYCHOLOGY AND THE LEGAL SYSTEM 137 (2d ed. 1991).

testimony,5 and judicial limiting instructions.6 The hearsay rule has been notably absent from the list of topics studied. This exclusion is particularly striking in light of John Henry Wigmore’s assertion that hearsay is the “most characteristic rule of the Anglo-American law of evidence.”7 Why has the hearsay rule been ignored as a subject of investigation?

One possibility is that the absence of hearsay research is simply a function of the limitations of an emerging discipline. Another possible explanation is a lack of legal sophistication among researchers. However, one of the major justifications for the hearsay rule—that hearsay compromises the fairness of a trial—suggests a third possibility. In a recent article in Law and Human Behavior, Gary Melton suggested that the social-science-in-law movement is scientifically conservative but politically liberal.8 Unlike the evidentiary rules that experimenters have investigated, the hearsay rule appears to protect a defendant from the power of the state. Therefore, critical examination of the rule may have seemed less attractive to social scientists.9 Hesitating on this basis to scrutinize hearsay is unwarranted, both because its impact on the fairness of a trial is amenable to empirical evaluation and because the rule may on occasion work to the detriment of criminal defendants, as, for example, when it bars exculpatory hearsay from admission at trial.10

The first results of empirical research on the effect of hearsay evidence on jurors’ verdicts were presented by two independent research groups at the 1990 meeting of the American Psychological Association. Using undergraduate subjects and videotape trial reenactments, a group including

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9. Another possible reason, recently suggested by Richard Lempert, is that there is no central, organizing, psychological theory in the hearsay area of the sort to be found in the case of such carefully examined subjects as eyewitness testimony. Richard O. Lempert, Presentation at the Hearsay Reform Conference (Sept. 7, 1991). A substantial body of social science research has been done, however, in a number of areas where no such principle appears to exist. See, e.g., materials cited supra note 1.
Meine, Park, Borgida, and Anderson assessed juror evaluative competence by comparing four evidentiary conditions: one involving only circumstantial evidence; a second involving circumstantial evidence and eyewitness testimony; a third involving circumstantial evidence and hearsay; and a fourth involving circumstantial evidence, eyewitness testimony, and hearsay.11 These researchers found that the combination of inculpatory hearsay testimony and circumstantial evidence resulted in only a few more guilty verdicts than when the jurors considered the circumstantial evidence by itself. Moreover, the jurors who considered the hearsay testimony and the circumstantial evidence gave substantially fewer guilty verdicts than those who were also presented with eyewitness testimony. In fact, the mock jurors appeared to discount the hearsay testimony; the conviction rate actually went down when eyewitness testimony was coupled with hearsay. The mock jurors judged the hearsay testimony to be less important and less reliable than eyewitness evidence, suggesting that jurors may adequately weigh and evaluate some types of hearsay.

The authors of this Article conducted the second study, in which undergraduates read twelve-page hypothetical criminal transcripts that contained either weak, moderate, or strong prosecution evidence. In addition, the transcripts offered either no hearsay or hearsay that was strong, moderate, or weak in its inculpatory implications. The hearsay was admitted without attorney objection or judicial limiting instruction. The results suggested that jurors do indeed note the presence of strong and moderate hearsay. Subjects who reported a higher reliance on hearsay in forming judgments about people in daily life ascribed greater importance to the hearsay information presented at trial. Despite this, guilty verdicts did not vary as a function of hearsay testimony. Rather, the overall strength of the case determined verdicts. Thus, hearsay that was not highlighted as inappropriate, and that was introduced within the context of a substantial amount of other evidence, appeared to influence the ultimate outcome of the trial only minimally.12

Recently, Regina Schuller reported a third study in a paper


presented at the June, 1991, meeting of the Law and Society Association. She had undergraduates from psychology classes read a summary of a criminal trial transcript based on an actual case. The transcript included exculpatory hearsay from an expert witness describing certain facts and the defendant’s state of mind at the time of the crime. As in the case of the studies on inculpatory hearsay, Schuller found that exculpatory hearsay did not affect the verdicts of mock jurors, despite clear data indicating that the mock jurors noted and processed the hearsay material.

II. THE PRESENT STUDY

The consistency of the findings of the three initial and independent exploratory studies on hearsay is provocative. Science, however, demands a wide convergence of data from different sources before a finding is accepted as a reflection of reality. Additionally, the legal community insists on experimentation that evaluates the circumstances that exist in actual courtroom settings before it will even consider modifications based on empirical studies. Therefore, much additional work remains to be done before the findings of these studies can be useful to either scholarly community. In recognition of this requirement, we have conducted a second hearsay study and intend to mount a series of increasingly sophisticated and refined investigations of the topic in the future.

A major criticism of many past empirical studies of legal processes has centered on their use of college undergraduates as subjects. A number of scholars have raised objections to generalizations based upon findings obtained from subjects who


may be different from actual jurors in important ways. Consequently, in our second study we employed a hypothetical criminal trial transcript, similar to that used in our first experiment, but assessed the reactions of actual members of the Cuyahoga County (Ohio) Common Pleas Court jury pool. We invited people waiting to be called for voir dire to participate; a total of 142 males and females volunteered to read the transcript and complete the three questionnaires.

We subjected the trial transcript to extensive pilot testing with this population in an effort to create a balanced vehicle that would allow inculpatory hearsay testimony to influence the outcome. Eventually, we achieved a situation where verdicts were evenly split between guilty and not guilty. We then used this transcript to develop seven conditions that varied the nature of the eyewitness identification of a perpetrator alleged to have stolen a coat from a restaurant coat rack. The seven conditions were:

1) **No eyewitness identification**: Bystander waitress said: “I was very busy and didn’t see anything.”

2) **Vague eyewitness identification**: Bystander waitress said: “I thought I saw someone in a flowered shirt look at some coats.”

3) **Vague hearsay testimony**: Bystander waitress said another waitress told her that “she thought she saw someone in a flowered shirt look at some coats.”

4) **Vague hearsay testimony with counsel objection and judicial instruction to disregard**.

5) **Specific eyewitness identification**: Bystander waitress said: “A man with a flowered shirt, whom I now recognize as the defendant, carefully checked the pockets of eight to ten raincoats. From the last he pulled a wallet, looked around nervously, put the coat on, and ran out.”

6) **Specific hearsay testimony**: Bystander waitress said another waitress told her that “she had seen a man with a flowered shirt, whom she later recognized as the defendant, carefully check the pockets of eight to ten raincoats, pull a wallet

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18. The original sample included 175 members of the jury pool. Before completing the materials, 33 subjects were called for voir dire.
from the last one, look around nervously, put the coat on and run out of the restaurant.”

7) **Specific hearsay testimony with counsel objection and judicial instruction to disregard.**

The transcripts for the seven conditions were identical in all other respects except that the two eyewitness conditions presented four witnesses for the prosecution, while the four hearsay conditions and the no eyewitness identification condition presented three prosecution witnesses. The extra witness in the two eyewitness conditions was, of course, the waitress whose words otherwise were turned into hearsay and quoted by a co-worker. All conditions included identical introductory remarks by the judge, opening and closing statements by counsel, several dozen evidentiary statements by the witnesses, cross-examination, and final instructions detailing the applicable Ohio law and the jury's duty to apply the law to the facts.

We escorted subjects in groups to the room used to sequester juries and instructed them to read the transcript carefully, assume the role of jurors who would return a verdict, and then respond to three short questionnaires in the following order:

* **Trial Decision Questionnaire:** verdict (guilty/not guilty) and confidence in verdict rated on a seven-point scale.

* **Trial Reaction Questionnaire:** one item assessing defendant's character; thirteen items assessing the importance ascribed to specific evidentiary statements in the order they were introduced at the trial, including the variable eyewitness/hearsay statement; and five items concerning the integrity of the trial, including fairness of the trial and the judge, competence of each attorney, and extent of justice achieved. All were to be rated on seven-point scales.

* **Sources of Personal Judgment Scale:** seven items rated on a seven-point scale assessing the importance attached to specific sources of information when forming judgments about others. Sources included first-hand information acquired through actual interactions, second-hand information involving what others said had been their experience with another person and what others said they had heard about another person, and four social/cultural sources included as “fillers” such as newspaper advice columns, television documentaries, and films.

One-way analyses of variance revealed no significant differences among the seven conditions for any item on the Sources of Personal Judgment Scale. Thus, across the seven conditions, subjects reported similar use in everyday life of hearsay and di-
rect experience. Looking at the overall means, all subjects reported direct experience to be more important than either what others said had been their experience with another person or what they heard about another person. 19

We analyzed guilty and not guilty verdicts by a chi-square. This revealed no significant differences in the frequencies of verdicts among mock jurors reading the seven different transcripts. 20 Of interest, however, is the fact that only twenty-seven percent of the jurors presented with specific hearsay testimony returned guilty verdicts, whereas fifty-two percent of the jurors receiving specific hearsay testimony with a limiting instruction returned a guilty verdict. The difference suggests the possibility of a boomerang effect, i.e., that the limiting instruction caused jurors to emphasize, rather than disregard, the hearsay. 21

We also performed one-way analyses of variance on the mock jurors' confidence in their verdicts, their assessment of the character of the defendant, the importance they ascribed to thirteen specific evidentiary statements, and their evaluation of the five items concerning the integrity of the trial. The only significant difference resulted from the importance the groups attributed to the eyewitness/hearsay testimony. 22 Further analysis of this finding, summarized in Table 2, 24 revealed

19. With 0 = least important and 6 = most important, the means were 5.3 (direct experience), 2.8 (what others said had been their experience with another person), and 1.6 (what was heard about another person). These means are virtually identical to those reported by the undergraduate subjects in our previous study, suggesting that the two populations share important characteristics. See Landsman & Rakos, supra note 12, at 82.
20.

<table>
<thead>
<tr>
<th>Table 1</th>
<th>Mock Juror Verdicts As a Function of Condition</th>
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<tbody>
<tr>
<td>Condition</td>
<td>Guilty</td>
</tr>
<tr>
<td>1.</td>
<td>No Eyewitness ID</td>
</tr>
<tr>
<td>2.</td>
<td>Vague Eyewitness ID</td>
</tr>
<tr>
<td>3.</td>
<td>Vague Hearsay ID</td>
</tr>
<tr>
<td>4.</td>
<td>Vague Hearsay + Instruction</td>
</tr>
<tr>
<td>5.</td>
<td>Specific Eyewitness ID</td>
</tr>
<tr>
<td>6.</td>
<td>Specific Hearsay ID</td>
</tr>
<tr>
<td>7.</td>
<td>Specific Hearsay + Instruction</td>
</tr>
</tbody>
</table>

Chi-Square: Not Significant
22. F (6, 134) = 9.96, p < .0001.
23. Tukey B post-hoc comparisons.
that mock jurors judged the specific eyewitness testimony to be significantly more important than the testimony in any other condition except specific hearsay. Furthermore, the jurors judged specific hearsay testimony to be significantly more important than the testimony in all other conditions except the specific eyewitness and the vague hearsay conditions. Of additional interest was the mock jurors' report that they disregarded hearsay evidence in the specific hearsay with limiting instructions condition, as indicated by the exceedingly low importance attached to the specific hearsay when an instruction was given. Nevertheless, as noted earlier, mock jurors in this condition were somewhat more likely to find the defendant guilty.

Our interpretation of these data is highly tentative. On the one hand, the introduction of specific, presumably damaging, hearsay testimony that was clearly processed by subjects did not increase guilty verdicts. On the other hand, the same can be said for specific, damaging, admissible eyewitness testimony. At least two explanations for these findings are plausible. First, our methodology may have been weak. This is a possibility because a number of studies indicate that eyewitness identification is a powerful—perhaps the most powerful—type of evidence, despite its unreliability. Alternatively, we may have been examining a weak phenomenon, in which case no single piece of evidence could, by itself, significantly affect the

<table>
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<tr>
<th>Condition</th>
<th>Mean*</th>
</tr>
</thead>
<tbody>
<tr>
<td>Specific Hearsay + Instruction</td>
<td>0.87</td>
</tr>
<tr>
<td>Vague Hearsay + Instruction</td>
<td>1.58</td>
</tr>
<tr>
<td>No Eyewitness ID</td>
<td>1.79</td>
</tr>
<tr>
<td>Vague Eyewitness ID</td>
<td>2.13</td>
</tr>
<tr>
<td>Specific Hearsay ID</td>
<td>2.50b</td>
</tr>
<tr>
<td>Specific Eyewitness ID</td>
<td>3.59bc</td>
</tr>
<tr>
<td>Specific Hearsay ID</td>
<td>4.43c</td>
</tr>
</tbody>
</table>

* Scale: 0 = least important, 6 = most important.
Means sharing the same superscript do not differ significantly from each other at p < .05 (Tukey B test).

24. See Wrightsman, supra note 3, at 145.
verdict, at least under most circumstances. Support for this hypothesis is provided by research that suggests that jurors construct a "story" as a trial unfolds and thereby incorporate and interpret new testimony within the context of previous testimony and the story line as a whole.\textsuperscript{29}

An analysis of the importance the mock jurors ascribed to the thirteen pieces of evidentiary testimony assessed in the Trial Reaction Questionnaire further supports the weak phenomenon possibility. Reactions to twelve of these pieces of evidence were constant across all seven conditions, as predicted. The only one to which reactions differed was the variable eyewitness/hearsay testimony. From Table 3,\textsuperscript{30} it is clear that jurors viewed the specific eyewitness testimony as no more important, and perhaps less important, than most of the other evidence. This is particularly true for evidence presented before the eyewitness testimony. These data suggest that eyewitness testimony may be less critical to juror decision making than previously thought\textsuperscript{31} or that the order of the "story" is de-

\textsuperscript{30} Table 3
Importance of Evidentiary Statements

<table>
<thead>
<tr>
<th>Statement Mean*</th>
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<tbody>
<tr>
<td>1. Victim described wallet</td>
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<tr>
<td>2. Victim noticed missing coat</td>
</tr>
<tr>
<td>3. Victim described distinctive features of coat</td>
</tr>
<tr>
<td>4. Victim and others noticed numerous coats on rack</td>
</tr>
<tr>
<td>5. Arresting officer found defendant wearing coat</td>
</tr>
<tr>
<td>6. Arresting officer encountered no resistance</td>
</tr>
<tr>
<td>7. Arresting officer found wallet in defendant's pants pocket</td>
</tr>
<tr>
<td>8. Waitress recalled noticing defendant looking over coat rack</td>
</tr>
<tr>
<td>9a. Specific eyewitness condition: Waitress described defendant's actions when taking coat</td>
</tr>
<tr>
<td>9b. Specific hearsay condition: Waitress described another waitress's observations of defendant's actions when taking coat</td>
</tr>
<tr>
<td>10. Waitress checked for defendant's coat at end of day</td>
</tr>
<tr>
<td>11. Defendant could not find his coat</td>
</tr>
<tr>
<td>12. Defendant placed strange wallet in pants pocket for safekeeping</td>
</tr>
<tr>
<td>13. Defendant failed to notice wrong coat</td>
</tr>
</tbody>
</table>

\textsuperscript{* Scale: 0 = unimportant, 6 = very important
\textsuperscript{31} See materials summarized in WRIGHTSMAN, supra note 3.
terminative, or both. Further, since jurors judged the specific hearsay testimony to be somewhat less important than the specific eyewitness testimony, these data strongly suggest that the mere introduction of hearsay testimony may not disproportionately influence juror decisions.

At this point, neither our research nor the initial work of other investigators substantiates the hypothesized dangers of hearsay testimony. In fact, the clear consistency in the findings of independent researchers employing different methodologies is provocative. Mock jurors do not appear to be unduly swayed by such evidence, nor do they appear to perceive that it affects the integrity of the trial process. Thus, jurors may be competent to weigh hearsay testimony appropriately and resist the inclination to see it as an automatic blot on the fairness of proceedings. Nonetheless, a host of questions and issues remain to be investigated.

III. THE FUTURE OF EMPIRICAL INQUIRY CONCERNING HEARSAY

If we truly wish to understand the purpose and operation of the hearsay rule, or any other evidence rule for that matter, we must commit ourselves to a project of longer duration and broader scope than has previously been undertaken. Such a study of the hearsay rule may both inform us about hearsay and establish a useful methodology for examining other evidentiary rules.

Three critical and interrelated issues must be addressed as hearsay research moves forward. First, we must determine the real nature and functions of the hearsay rule as a legal mechanism. Second, we must determine when, in the course of the research, sufficient data have been accumulated to warrant advocacy of reform. Finally, we must consider whether the risks attendant to reform have been adequately explored. Only then should we be willing to embrace definitive changes.

A. THE LEGAL NATURE AND FUNCTION OF HEARSAY

A clear understanding of the functioning and objectives of a procedural rule is critical to successful social science analysis. Only when such an understanding exists can empirical tests be properly designed to ascertain whether the rule's un-
derlying assumptions are sound and its goals effectively served. Unfortunately, this is not an easy task. The three obvious sources of guidance are the text of the rule, scholarly interpretation of that text, and practice in the courtroom. All too frequently these sources present strikingly inconsistent insights vis-a-vis each other and within themselves.

Today there is a lively scholarly debate about the meaning and function of the hearsay rule. Some writers have emphasized its importance in guaranteeing the reality and appearance of procedural fairness by requiring in-court confrontations. Others have presented it as a means of guarding judgments from subsequent attack. Still others have stressed the rule's role in maintaining the adversarial nature of judicial proceedings. These theories notwithstanding, a number of observers have pointed out that in modern American courts the hearsay rule is mostly honored in the breach and that little "important" hearsay is excluded from consideration. While it will be of substantial importance in the long-term study of hearsay to sort out precisely why and when we use the rule, for the purposes of beginning our research, a less complete analysis will suffice.

The following is a thumbnail sketch of the history of the use of the hearsay rule. It is included to suggest that the rule is a complex legal creation with a range of not entirely consistent objectives. The research described in the first part of this paper focused on only one of the issues addressed by the rule—juror incompetence. Even a cursory examination of the history of hearsay makes it clear that exploration of the incompetence question will not suffice as a measure of the rule's operation or importance. Other questions must be considered to ensure a thorough empirical examination. These include questions about the appearance of fairness and the value of cross-examination.

The hearsay rule was not the creation of some clever legal

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35. E.g., 5 WIGMORE, supra note 7, § 1364, at 20-23; Edmund M. Morgan, Application of the Hearsay Concept, 62 HARV. L. REV. 177, 181-84 (1948); Swift, supra note 33, at 1389-76.
philosopher or rules-drafting committee. Rather, it was a by-product of jury-based common law adjudication. It was molded and remolded over the course of more than four centuries by lawyers pursuing the business of representing clients and by judges seeking to ensure proper verdicts. As a consequence of its incremental development, the rule, like so much in Anglo-American jurisprudence, does not have a single goal or express a single viewpoint. It reflects a variety of objectives sought at different times by participants in the courtroom contest.

Medieval English jury adjudication was, in essence, based upon hearsay. Juries in the thirteenth and fourteenth centuries decided cases on the basis of the rumor, gossip, and community opinion to which they were exposed before the trial commenced. While reservations about hearsay were articulated as early as 1202, it was not until the latter half of the 1500s that serious concerns were voiced about its use in litigation.

Dubious second-hand evidence was used in attempts to convict two popular political figures, Sir Nicholas Throckmorton in 1554 and Sir Walter Raleigh in 1603. Both defendants decried the unfairness of this tactic, as did commentators who later discussed their trials. Sir Walter Raleigh's execution, on the basis of hearsay statements from two men, at least one of whom the government had in its power and refused to produce at trial, became a rallying point for sentiment against the use of hearsay evidence. During the turbulent half century from the rise of Cromwell to the Glorious Revolution, use of the hearsay doctrine grew, as the rule came to be viewed as one means of blunting the prosecutorial zeal of rulers bent on dominating wealthy and powerful members of the aristocracy. In this setting, the hearsay proscription served as a means of enhancing

38. 2 Pollock & Maitland, supra note 37, at 622.
40. See Regina v. Raleigh, 2 State Tr. 1 (1603).
41. Raleigh is said to have complained, "[If witnesses are to speak by relation of one another, by this means you may have any man's life in a week; and I may be massacred by mere hearsay as Sir Nicholas Throckmorton was like to have been in Queen Mary's time." 9 William Holdsworth, A History of English Law 445 (3d ed. 1944).
the integrity of the judicial process, at least in the case of those of means and social prominence.

Despite these developments, hearsay was not a widely utilized concept in the early 1700s. The proceedings of London's central criminal court, the Old Bailey, reveal that the hearsay rule was virtually never invoked before the 1730s. Prosecutors and prisoners alike employed out-of-court words. In the 1730s, this began to change. The central objective of the new approach seemed to be to ensure that courts and juries were provided, whenever possible, with first-hand sworn testimony subject to cross-examination. During the course of the eighteenth century, this principle, particularly with regard to cross-examination, grew substantially.

Hearsay was dramatically reconceptualized in the nineteenth century. From as early as the time of Lord Mansfield, judges and treatise writers began to emphasize the incompetence of jurors as a justification for the hearsay prohibition. This new argument was strikingly different from arguments based on procedural fairness or the production of witnesses for cross-examination. As the 1800s wore on, lawyers increasingly argued that the untutored members of the jury were incompetent to assess the value of second-hand information. As the influential treatise writer Thomas Starkie put it:

If it were to be assumed, that one who had been long enured to judicial habits might be able to assign to such evidence just so much, and no greater credit than it deserved, yet, upon the minds of a jury unskilled in the nature of judicial proofs, evidence of this kind would frequently make an erroneous impression.

As a result of such views, limitations on the use of hearsay increased. However, these limits were likely to be ignored when judges concluded that a particular type of hearsay was safe for jurors to consider. This approach produced a complex rule with a range of exceptions.

Around the beginning of the present century, the two giants of American evidence law, James Bradley Thayer and John Henry Wigmore, turned their attention to the hearsay rule. Both embraced variants of the juror incompetence hypothesis in conjunction with the cross-examination and fairness

44. Id. at 539-42, 548-64.
45. 1 THOMAS STARKIE, A PRACTICAL TREATISE OF THE LAW OF EVIDENCE 45 (1824).
principles.\textsuperscript{46} This meant the perpetuation of a chimerical hearsay rule utilizing a rigid exclusionary scheme with respect to some categories of material. Although generations of reformers have tried to slay this monster, the rule remains an amalgam of concerns about juror competence, cross-examination, and fairness. Researchers must keep this multiplicity of objectives in mind as they pursue further empirical research.

B. WHEN HAVE SUFFICIENT DATA BEEN GATHERED TO WARRANT CONSIDERATION OF REFORM?

Social scientists who examine legal procedures face particularly tough questions when deciding whether their findings warrant a call for reform. Lawyers have been deeply skeptical of such calls, often with good reason.\textsuperscript{47} The dispute that arose in 1908 after the publication of Professor Hugo Muensterberg's \textit{On the Witness Stand} presents a fine example of the sort of reception social science reformers have received. In his book, Muensterberg argued:

Yes, it can be said that, while the court makes the fullest use of all the modern scientific methods when, for instance, a drop of dried blood is to be examined in a murder case, the same court is completely satisfied with the most unscientific and haphazard methods of common prejudice and ignorance when a mental product, especially the memory report of a witness, is to be examined. No jurymen would be expected to follow his general impressions in the question as to whether the blood on the murderer's shirt is human or animal. But he is expected to make up his mind as to whether the memory ideas of a witness are objective reproductions of earlier experience or are mixed up with associations and suggestions. The court proceeds as if the physiological chemistry of blood examination had made wonderful progress, while experimental psychology, with its efforts to analyze the mental faculties, still stood where it stood two thousand years ago.\textsuperscript{48}

\textsuperscript{46} See JAMES B. THAYER, A PRELIMINARY TREATISE ON EVIDENCE AT COMMON LAW 47, 518-23 (1898); Edmund M. Morgan & John M. Maguire, \textit{Looking Backward and Forward at Evidence}, 50 HARV. L. REV. 909, 919 (1937) ("Mr. Wigmore's basic assumption in treating hearsay [was] a judicial conviction that the jury must be protected from testimony which it [could not] properly evaluate.").

\textsuperscript{47} On the other hand, some lawyers and judges have too frequently been eager consumers of dubious data and conclusions based upon them. An example of this inclination was Wigmore's virtually blind willingness to accept the allegedly scientific claims of handwriting "experts" like Albert Osborn. See D. Michael Risinger et al., \textit{Exorcism of Ignorance as a Proxy for Rational Knowledge: The Lessons of Handwriting Identification "Expertise"}, 137 U. PA. L. REV. 731, 764-71 (1989).

\textsuperscript{48} HUGO MUENSTEBERG, ON THE WITNESS STAND 44-45 (1908).
The professor then claimed that psychology had developed several mechanisms appropriate for immediate judicial adoption. Among these were a method for assessing testimonial certitude by examining various witness reactions and a means of appraising a defendant’s guilt through associational techniques.

Wigmore unmercifully attacked Muensterberg’s proposals. He challenged each in the context of six questions: “(1) Does it offer something new? (2) Are its rules exact? (3) Are its tests concrete, not merely abstract? (4) Are the conditions of its use practical? (5) Are its results yet even agreed upon? (6) Has it yet taken all available means of verifying its relative efficiency?”

Wigmore's first three questions focused on the translation of allegedly new psychological insights to the courtroom. Wigmore argued that many of Muensterberg's ideas were not novel. He demonstrated that judges and legal scholars had been aware of and had taken into account the sorts of phenomena Muensterberg mentioned long before the professor wrote his book. Wigmore next argued that the psychological data in question were far from exact and were couched in terms that could provide no clear, let alone definitive, answers to questions from the courtroom. He then demonstrated that the general principles espoused by Muensterberg and his colleagues could not deal with “the infinite variation of idiosyncrasy” presented by real witnesses in actual courtroom proceedings. In sum, the “new” ideas were either known to the legal profession already or were so vague as to be worthless.

Turning his attention to the quality of the research itself, Wigmore pointed out that it was the product of experimental settings wildly different from the courtroom and that its legal applicability was open to serious question. Beyond this, Wigmore demonstrated that there was substantial disagreement among psychologists about many of the propositions set forth in On the Witness Stand. Finally, Wigmore emphasized that the material used by Muensterberg had never been verified by ex-

49. Id. at 63-69.
50. Id. at 73-110.
52. Id. at 417-20.
53. Id. at 420-21.
54. Id. at 421.
55. Id. at 424-25.
56. Id. at 425-26.
periments using different methodologies.\textsuperscript{57}

Wigmore succeeded in painting Muensterberg as an imprudent and hasty reformer whose data were not reliable and whose methods were open to question. To survive such rigorous scrutiny requires a far more effective research program than that undertaken by Muensterberg and his colleagues. In outline, such a program should have at least the following three characteristics: critical studies should be demonstrably valid in internal design and external applicability, the resulting data and conclusions should be subjected to searching and effective review, and the most important results should be replicated by studies using a variety of methodologies. While these research criteria have been discussed in other settings,\textsuperscript{58} it is useful to review their implications in the hearsay context. As a preliminary matter it should be noted that jury proceedings are, by law, confidential.\textsuperscript{59} Therefore, researchers will generally have to rely on simulations if they wish to scrutinize the nature of juror deliberations regarding hearsay. Reliance on simulation techniques has a number of important implications. These will be explored in the next section along with other, more general, methodological issues.

1. Research Validity.

Research validity is an important concern of social scientists working on legal questions. In a recent article, John Monahan and Laurens Walker sought to outline what social science findings deserve to be treated as authoritative.\textsuperscript{60} Monahan and Walker argued that the key to internal validity is controlling for "competing hypotheses that may account for an observed state of affairs."\textsuperscript{61} Although there are a range of internal validity problems,\textsuperscript{62} two are likely to be particularly

\textsuperscript{57} Id. at 426-27.

\textsuperscript{58} See generally Symposium, Simulation Research and the Law, 3 LAW & HUM. BEHAV. 1 (1979) (articles discussing research methodology in simulating legal phenomena).

\textsuperscript{59} Jury secrecy has been protected both by judicial decision and legislative enactment. See, e.g., Stein v. New York, 346 U.S. 156, 178 (1953); 18 U.S.C. § 1508 (1988); CAL. PENAL CODE § 167 (West 1988).

\textsuperscript{60} See John Monahan & Laurens Walker, Social Authority: Obtaining, Evaluating, and Establishing Social Science in Law, 134 U. PA. L. REV. 477 (1986).

\textsuperscript{61} Id. at 502.

\textsuperscript{62} See DONALD T. CAMPBELL & JULIAN C. STANLEY, EXPERIMENTAL AND QUASI-EXPERIMENTAL DESIGNS FOR RESEARCH 5-6, 13-22, (1963) (detailed discussion of internal validity problems); see also Richard Lempert, Strategies of
prominent in evidence simulations. The first is the possibility that a confounding factor will be inadvertently inserted into the stimulus materials. The second is that evaluative instruments will call undue attention to the items being tested.

When the target of research is a particular method of delivering evidence, for example hearsay, or a single substantive category of proof, for example the fact that one of the parties has a criminal record, it is extremely difficult to ensure that alternative motivations do not cloud juror reactions to the evidentiary item being tested. The source presenting the critical material, whether it is a witness or a document, may improperly affect the outcome as, for example, when a witness of unexpectedly high or low credibility is used. Alternatively, the form of the targeted evidence, that is, the length or method of delivery, may stimulate an inappropriately intense response from jurors. Finally, the uniqueness of the material may provoke inappropriate scrutiny and defeat the experimental objective of assessing how jurors in trial conditions react to unhighlighted evidence of a defined sort.

The second particularly thorny internal validity problem in evidentiary simulations is designing sensitive evaluative techniques, such as questionnaires, that do not call special attention to the targeted evidence but allow assessment of jurors' reactions to it. This problem is especially serious if the experimenters seek to assess individual juror attitudes before the same jurors join in group deliberations.

These two problems may be even more serious if one presently popular theory about the way jurors decide cases is correct. The "Story Model" suggests that jurors decide cases by creating stories that draw on patterns or schemas that they have previously established. Nancy Pennington and Reid Hastie describe the Story Model in the following way:

"The story is established by inferring events not included in the testimony from frames of world knowledge matching events already es-

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63. See Lippa, supra note 14, at 258.

64. See Richard O. Lempert, Uncovering 'Nondiscernible' Differences: Empirical Research and the Jury-Size Cases, 73 Mich. L. Rev. 643, 703 (1975) (suggesting that obtaining written commitments from individual jurors before they deliberate may "affect the extent to which [they] are influenced by groups" and citing Harold B. Gerard, Deviation, Conformity, and Commitment, in Current Studies in Social Psychology 263, 266-67 (Ivan D. Steiner & Martin Fishbein eds., 1965)).

65. See Pennington & Hastie, supra note 29, at 243-45.
Inferences are evaluated by simulating one's own behavior in similar situations, by checking for contradictions with other plausible conclusions and by checking for inconsistencies with other plausible conclusions and by checking for inconsistencies with the current form of the story.\textsuperscript{66}

If this model is accurate, jurors do not really focus on individual pieces of evidence but instead try to fashion all the evidence into a single plot. They filter out or assign substantially reduced significance to things that do not fit. This theory implies that individual items of evidence are generally of little intrinsic importance to jurors and that teasing out their effects will depend on exactly how they are related to larger currents in juror thinking. In order to stir any reaction, the targeted evidence may have to have substantial relevance to preexisting juror schemas. Of course, identifying such evidence may prove difficult. Its use may attract undesired attention to the experimental manipulation or become confounded with the appeal of the underlying story.

External validity problems may also prove serious in evidence simulations. Wayne Weiten and Shari Diamond have carefully analyzed the sorts of difficulties experimenters may face when using simulations in this type of applied research.\textsuperscript{67} They suggest six problems of particular concern: inadequate sampling, inadequate trial simulation, lack of jury deliberation, inappropriate dependent variables, lack of corroborative field data, and disparity of motivation between real and simulated jurors.\textsuperscript{68}

As Weiten and Diamond point out, experimenters should take care in selecting their subject sample. Although students are generally easy to use as subjects, they have a variety of attitudes and attributes that differ from those of the general public. Therefore, it is best to conduct applied research simulations with subjects drawn from the wider community.\textsuperscript{69} This advice seems to make as much sense in the hearsay area as elsewhere, but it should not be construed as a flat ban on student-focused research. Student subjects may prove extremely useful in preliminary experiments, especially because they help keep costs

\textsuperscript{66.} \textit{Id}. at 254.

\textsuperscript{67.} Researchers will generally conduct such simulations without specific theoretical objectives and, therefore, they should be classified as applied research. See Weiten & Diamond, \textit{supra} note 16, at 75.

\textsuperscript{68.} \textit{Id}. at 75-83.

\textsuperscript{69.} \textit{Id}. at 75-77.
to a minimum while researchers develop their techniques and materials.

The evidentiary simulations utilized in hearsay research should be as much like real trials as possible. Stripped-down simulations like the once-popular 400-word summary are likely to produce misleadingly exaggerated results. The key in hearsay and other evidence work is determining "whether... manipulations would be effective if embedded in the vastly greater flow of information more typical of an actual trial." Whether the stimulus materials are presented in writing or by a means more closely approximating the trial experience, such as videotape, is also important. Written materials may be appropriate for a beginning inquiry, especially because of the substantial costs associated with videotaped presentations. However, because hearsay usually consists of one witness repeating the out-of-court words of another, a body of experimentation that failed to test juror-subjects' reactions to oral hearsay would be incomplete. The best approach may be to begin with written experiments and then proceed to videotaped replications as promising trends and effective experimental vehicles are discovered.

Weiten and Diamond urge experimenters to use group deliberations rather than rely on the reports of single jurors. They hasten to point out, however, that "[i]ndividual prediscussion positions are important determinants of the deliberation proceedings and are certainly related to the final verdict." For this reason, preliminary research may rely upon individual juror evaluations. Definitive results, however, must incorporate group deliberations. Such assessments will require very large sample populations in order to create a set of data powerful enough to warrant reform proposals. An interesting additional question is whether researchers ought to monitor and analyze the deliberations themselves to trace the precise effects of the targeted evidentiary material. This sort of scrutiny has

70. Id. at 77.
71. Id. However, as in testing for potential carcinogens, there may be some utility in initially examining extreme situations that can be manufactured only in the laboratory.
72. See id. at 77 (noting that a change in the mode of information input, such as reading as opposed to listening, deprives the simulated juror of a source of information available to real jurors, who pay attention to the nonverbal behavior of trial participants).
73. Id. at 78-79.
74. Id. at 79.
been advocated in cognate areas such as jury size research.\textsuperscript{75} It
seems warranted for hearsay research so long as there are sufficient
resources to sustain the elaborate and labor-intensive effort.

Two other concerns are the use of appropriate dependent
variables and the pursuit of corroborative field data.\textsuperscript{76} With res-
pect to dependent variables, jurors generally should be asked
to give a guilty/not guilty\textsuperscript{77} verdict rather than a less realistic
decision. Of course, verdicts are not particularly well focused
on the target evidentiary material, but supplementary question-
naires can provide more precise data. As discussed above, the
questionnaires must be carefully designed to minimize any
skewing of juror thinking or deliberation. On the issue of real
world corroboration, experimenters should explore as many
methodologies as possible in order to get a sense of the effect of
specific evidentiary rules in real trials. This is likely to be an
extremely complex task in light of the huge volume and wide
range of materials produced at trials. The gathering of judicial
reports or evaluations like those solicited by Harry Kalven and
Hans Zeisel for \textit{The American Jury} may be helpful.\textsuperscript{78} Alterna-
tively, experimenters may attempt to collect post-verdict assess-
ments from jurors.\textsuperscript{79}

The final problem highlighted by Weiten and Diamond is
the potential disparity of motivation between real jurors and
those simply playing the juror role in a simulation.\textsuperscript{80} This is
the most troublesome of the six external validity problems be-
cause it is virtually impossible to solve in a simulation.\textsuperscript{81} In
light of the legally enforced secrecy that shrouds real jury de-
liberations, there may be no alternative to relying on simula-

\textsuperscript{75} See Lempert, supra note 64, at 702 (noting that the ability to monitor
jury deliberation processes is an advantage of using mock juries).
\textsuperscript{76} See Weiten & Diamond, supra note 16, at 79-81.
\textsuperscript{77} This should not be taken as implying that only criminal cases are ap-
propriate for experimentation.
\textsuperscript{78} HARRY KALVEN, JR. \& HANS ZEISEL, THE AMERICAN JURY 33-54
(1966).
\textsuperscript{79} A number of commentators have pointed out the questionable value
of such data. See, e.g., Lempert, supra note 64.
\textsuperscript{80} Weiten & Diamond, supra note 16, at 81-83. Actual jurors display a
range of motivation, as was well demonstrated by the Public Broadcasting Sys-
tem's Frontline videotape of actual jury deliberations that was aired on April
8, 1986. See Margaret E. Guthrie, \textit{Film Takes an Inside Look at Deliberations
\textsuperscript{81} It might be possible to arrange a simulation in which the subjects are
tricked into believing that their decision has actual significance, but the ethical
questions posed by such deception are substantial.
tions. Risk of distortion due to role-playing may decline, however, as verisimilitude is heightened, providing an extra incentive for attention to jury composition, simulation realism, and group deliberations.

2. The Need for Effective Review

Individual investigators must consider the foregoing methodological principles, but social scientists as a body must participate if hearsay research, or any other evidentiary inquiry, is to establish its persuasiveness. Monahan and Walker have urged that claims to social science authority be judged, at least in part, by various forms of peer review. These include refereed journals, federal funding panels, and task forces asked to assess the value of specific bodies of data. Researchers should subject hearsay and evidence research to all of these processes. More important, a working group, perhaps under the aegis of the Association of American Law Schools or Division 41 of the American Psychological Association, ought to be formed to facilitate the review process and serve as a clearinghouse for the research effort. Conferences similar to the Minnesota Hearsay Reform Symposium ought to be held regularly and their results published so that scholars can collectively consider refinements in research methods and questions raised by research data.

3. Replication of Results

The ultimate goal of hearsay rule research is to produce a substantial body of experiments that use different methodologies and replicate one another’s findings. Such a body of research would create a strong basis for evaluating the propriety of reform. Monahan and Walker stress this point and compare it to the respect a court’s decision receives when it is followed by a substantial number of other courts.

Even if such a foundation were established, caution would still be warranted. Writing in 1979, Vladimir Konečni and Ebbe Ebbesen described a set of experiments in which they used six

82. Monahan & Walker, supra note 60, at 500.
83. Id. at 500-01.
84. See Campbell & Stanley, supra note 62, at 3 (describing 12 factors jeopardizing the validity of various experimental designs).
85. Monahan & Walker, supra note 60, at 507-08; see also William Gardner et al., Asserting Scientific Authority, 1989 Am. Psychol. 895, 899 (1989) (advocating the use of a variety of methodologies and replication of findings to substantiate the difference between adults' and adolescents' decisions on whether to have an abortion).
different methods to assess the nature of the sentencing process in criminal courts. These ranged from "journalistic" interviews and experimental simulations to archival analysis.86 They determined that, at least as to sentencing, different methods did not produce uniformity of findings, and that one method, archival analysis, seemed far more reliable than others.87 Indeed, partial agreement between other methods appeared to distort the real nature of the process.88 Consequently, Konečni and Ebbesen argued that the multiple method principle must be qualified "when one studies an intact functioning social network—such as the criminal justice system."89 They stressed the need for archival analysis of real world decisions.90 Still, the appropriate sorts of real world data are virtually impossible to obtain in the evidence context. In such situations, Konečni and Ebbesen urged: "[R]ather than automatically assume that simulations are useful, one ought to collect sufficient evidence to test whether they have captured the necessary details of the real world to be real simulations."91

There has not yet been enough hearsay research to warrant a discussion of reform on empirical grounds. Before entertaining reform proposals, we must be satisfied that we have fashioned "real simulations" that cover the critical aspects of the hearsay rule. The research described above addresses only one facet of the rule, juror incompetence. Although it may take only a modest showing to discredit such a judicial hypothesis,92 doing so does not satisfy the need for empirical research. It remains necessary to address other concerns associated with the hearsay concept, including the appearance of fairness, the use of cross-examination, and perhaps even the maintenance of an adversarial approach to adjudication.

86. Konečni & Ebbesen, supra note 17, at 46.
87. Id. at 62-64.
88. [L]iterally all methods indicated that the severity of the crime and the offender's prior criminal record are highly important. If all of our studies, except for the archival analysis, had been carried out, the conclusion about the major importance of these two variables in the sentencing process would presumably have been made with a great deal of confidence, due to the fact that it would have been based on the results which represent a point of convergence of many quite different methods. Yet, such a conclusion would be entirely wrong. Id. at 64 (citation omitted).
89. Id. at 64-65.
90. Id. at 65-66. Real world data, however, lack the internal validity of laboratory results and thus present their own problems.
91. Id. at 68.
92. See Gardner et al., supra note 85, at 900.
C. THE RISK OF REFORM

To urge reform without the broadest sort of research program is both dangerous and foolhardy—dangerous because injurious reforms may be adopted and foolhardy because substantial, justified criticism may be leveled at those who prematurely call for restructuring. At the request of the Chief Justice of the Supreme Court of the United States, and the Federal Judicial Center, a panel was established in 1978 “to identify, define, analyze, and recommend resolution of issues bearing on the propriety, value and effectiveness of controlled experimentation for evaluating innovations in the justice system.” Although this group’s main concern was experimentation involving actual litigants, its report provided criteria useful for assessing when reform ought to be considered. The Committee’s report stressed the need to be cognizant of the ethical obligation owed to those affected by proposed innovations. The report stated that “uncertainties regarding either the risks of adverse consequences or the possibility that the innovation will be ineffective” make it essential that careful and ethically sensitive experimental work be carried out before major changes are made.

In the hearsay setting, following this principle militates against hasty reform. Nothing should be done until experimental work reveals that the admission of hearsay poses little threat to the actual or perceived integrity of jury deliberation. Testing should not stop when these propositions are established; researchers should assess the efficacy of any new mechanism proposed to deal with hearsay, whether it be a blanket rule of admission, reliance on judicial discretion, or something else. It is crucial to determine not only how any replacement rule would work, but also whether it would unacceptably discourage the production of live witnesses or pose other risks.

Reform has never been approached this carefully. In fact, courts and legislatures have frequently acted on the basis of the most unreliable data. Such a methodology can yield damaging results. The best example of the pitfalls of a careless and hasty approach is the Supreme Court’s decision in Williams v. Flor-

94. Id. at 7.
95. Id. at 2.
ida.\textsuperscript{96} and its aftermath. In \textit{Williams}, the Court permitted a reduction in the size of the jury from twelve to six.\textsuperscript{97} While it was not incumbent on the Court to adduce any empirical support for its willingness to overthrow hundreds of years of legal tradition, the Court went out of its way to find scientific endorsement.\textsuperscript{98} Unfortunately, the Court embraced the few available studies without scrutinizing their internal or external validity. It did not search for a real confluence of findings but rather twisted what few data there were to meet its objectives. It did not seek data about the likely consequences of the reform it was adopting. Predictably, subsequent commentary made it clear that the Court’s empirical basis for its change was absolutely unconvincing,\textsuperscript{99} that studies cited by the Court were grievously flawed,\textsuperscript{100} that the change exposed the system to greater jury unpredictability\textsuperscript{101} and that it undermined the jury’s ability to represent minority points of view.\textsuperscript{102} Eventually, the Court retreated from the logic of its own reform, and in \textit{Ballew v. Georgia}\textsuperscript{103} implicitly turned its back on the jury studies it had been so keen to utilize just eight years before. The Court did not acknowledge, however, that the very basis of its original decision in \textit{Williams} was invalid. The Supreme Court’s hasty and ill-supported action has resulted in substantial and lasting damage to the jury process.\textsuperscript{104}

\textbf{IV. WHERE DO WE GO FROM HERE?}

The assumptions underlying social science research make it virtually impossible to prove that admitting hearsay testimony into evidence will never alter the outcome of a trial in a way that will concern the legal community.\textsuperscript{105} Research can demon-

\textsuperscript{96} 399 U.S. 78 (1970).
\textsuperscript{97} \textit{Id.} at 103.
\textsuperscript{98} \textit{Id.} at 101.
\textsuperscript{100} See Zeisel & Diamond, \textit{supra} note 99, at 282-90.
\textsuperscript{101} \textit{Id.} at 294.
\textsuperscript{102} See Lempert, \textit{supra} note 64, at 668-81.
\textsuperscript{103} 435 U.S. 223 (1978).
\textsuperscript{104} Evidence from Los Angeles courts confirms that using smaller juries does exclude minority points of view. See G. Thomas Munsterman et al., A Comparison of the Performance of Eight- and Twelve-Person Juries 35 (1990) (unpublished manuscript, on file with the \textit{Minnesota Law Review}).
\textsuperscript{105} See JOHN M. NEALE \& ROBERT M. LIEBERT, SCIENCE AND BEHAVIOR: AN INTRODUCTION TO METHODS OF RESEARCH 94 (3d ed. 1986).
strate only that admitting hearsay into evidence does or does not make a difference in the settings created in particular experiments. This means that research of the sort described in this Article will never provide the definitive answer to all questions about the hearsay rule. Researchers will be able to do no more than pursue those questions which seem most lively or pressing. Even such a limited project will be complex, expensive, and beyond the resources of any single laboratory. Nevertheless, the research necessary for a satisfying empirical understanding of hearsay is not beyond our reach.

One basic concern must be the method of presenting the hearsay to subjects. The oral and visual impact of hearsay evidence investigated in studies must be representative of the impact of hearsay in courtroom situations. This will require more use of videotaped materials of the sort used in the experiments conducted by Peter Meine and his associates.\textsuperscript{106}

A second issue involves the quantity of hearsay evidence that jurors can successfully process. Our materials presented a single statement embedded in a relatively long trial; those of Meine et al. appear to do the same.\textsuperscript{107} However, Schuller introduced at least four pieces of hearsay evidence, two describing factual occurrences and two attesting to the defendant’s state of mind at the time of the crime. Still, she found that the hearsay had no effect on verdicts.\textsuperscript{108} Further research ought to be pursued to determine how large a ratio of hearsay to nonhearsay statements jurors are able to handle effectively.

A third issue relates to the factual context in which the hearsay testimony is set. Does the type of case matter? This is a reasonable possibility since, for example, the impact of prior convictions\textsuperscript{109} and jury nullification instructions\textsuperscript{110} are powerfully altered by the nature or severity of the crime being prosecuted. Does the substance of the hearsay testimony itself change its impact? For example, would jurors judge a witness’s

\textsuperscript{106} Meine et al., supra note 11, at 4.
\textsuperscript{107} Id.
\textsuperscript{108} Schuller, supra note 13, at 17.
\textsuperscript{109} Cornish & Sealy, supra note 1, at 222 (noting that the admission of previous convictions increases the likelihood of a guilty verdict but only if the convictions are for offenses similar to the offense charged); Wissler & Saks, supra note 1, at 43-47.
second-hand description of what someone said he or she did differently than they would a witness's hearsay about what someone else said he or she saw? Does the inculpatory or exculpatory nature of the hearsay alter its effect on jurors? Schuller's findings begin to speak to this point, but many questions remain. Similarly, whether the timing or placement of the hearsay within the trial sequence affects verdicts is an important question. Hearsay offered early in a trial, when jurors are just beginning to construct their story, might be more of a threat to the integrity of the process. Finally, the effect of factors such as the length, complexity, and overall strength of a case on the evaluation of hearsay testimony deserve to be assessed.

A fourth issue inevitably raised in research on jury processes is how jurors handle hearsay in group deliberations. Evidence from actual observation of deliberations, mock juror deliberations, and memoirs of juror experiences all suggest that the majority of jurors take their task seriously and weigh the evidence carefully. Despite this evidence, are there particular circumstances under which jurors would misuse hearsay testimony? For example, do jurors misuse hearsay when the evidence is less carefully weighed, as in the cases of juries that are "verdict-driven" or operating under a majority decision rule? Instructions to disregard testimony may be important if research demonstrates that hearsay influences juror decisions under some circumstances. Is the instruction to "disregard" effective? Mock jurors in our experiment and in Schuller's study reported that they complied with this instruction, but such self reports do not necessarily signal actual compliance. In fact, data on such instructions as well as so-

111. Schuller, supra note 13, at 13-18.
113. The authors found no interaction between overall strength of case and hearsay testimony. Landsman & Rakos, supra note 12, at 76.
114. See KALVEN & ZEISEL, supra note 78, at 482-91; Guthrie, supra note 80.
116. See, e.g., MELVYN B. ZERMAN, CALL THE FINAL WITNESS (1977) (memoirs of a juror at a murder trial).
117. See HASTIE ET AL., supra note 115.
118. Id. at 228-29; see KALVEN & ZEISEL, supra note 78, at 461.
119. Schuller, supra note 13, at 15.
120. See id.
cial psychological theory\textsuperscript{121} suggest that compliance is not so easily achieved. As an alternative, would a "cautionary" instruction urging close scrutiny of the questioned evidence better serve desired goals?\textsuperscript{122} Or do all types of instructions highlight and thereby increase the undesired effect of hearsay? This is a possibility strongly suggested by other research on limiting instructions.\textsuperscript{123}

Is blanket admission the best remedy for the hearsay problem? Assuming it is, several procedural questions arise. First, can voir dire procedures be developed to identify jurors unable to weigh the second-hand nature of hearsay competently? If such strategies were available, particularly vulnerable jurors might be eliminated from the panel. Second, can cross-examination of hearsay witnesses produce results comparable to cross-examination of witnesses with identical admissible testimony? In other words, if the hearsay is "shaky," can cross-examination of a sponsoring witness\textsuperscript{124} expose its weaknesses in a manner similar to that achieved by cross-examination of other questionable testimony?

These issues and questions are all products of an inductive approach to the topic. In closing, we should mention that an alternative, deductive strategy might stimulate a different range of research questions and designs. A detailed discussion of a deductive approach is beyond the scope of this Article, but a provocative possibility is offered by Richard Petty and John Cacioppo's dual route theory of central and peripheral persuasion.\textsuperscript{125} This theory asserts that the central route of decision

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\textsuperscript{121} Lisa Eichhorn, Social Science Findings and the Jury's Ability to Disregard Evidence Under the Federal Rules of Evidence, 52 LAW & CONTEMP. PROBS. 341, 344-50 (1989).

\textsuperscript{122} Schuller, supra note 13, at 17 n.6, tested this and found that mock jurors receiving a cautionary instruction reported more use of the hearsay evidence than those receiving a disregard instruction; nevertheless, the type of limiting instruction did not affect the verdicts.

\textsuperscript{123} Id. at 13-18.

\textsuperscript{124} Cross-examination requires the availability of such a witness and his or her preparation to speak about the origins of the hearsay evidence. For a proposal mandating the production of such witnesses, see Swift, supra note 33, at 1358-61, 1378-83.

making is employed by involved listeners who analyze and then logically evaluate the contents of the messages they receive.\textsuperscript{126} Such listeners are said to undertake comprehensive scrutiny of the issue-relevant arguments presented by the source, while avoiding distractions such as irrelevant features of the source, the way the information is packaged, or their affective responses to that information.\textsuperscript{127} Cognitive skills and motivation are necessary for effective utilization of the central route.\textsuperscript{128} When these attributes are absent, decision makers are likely to rely on the peripheral route, in which superficial characteristics assume greater importance and critical analysis is neglected.\textsuperscript{129} The theory implies that hearsay statements will be of greatest concern when jurors fail to employ the central route in an effective manner. Furthermore, it offers hypotheses concerning the conditions under which decision makers are likely to increase the use of each of the persuasion routes, such as prior knowledge, existing opinion, forewarning of content, and persuasive intent. These are variables that are obviously relevant to, and could be tested within, the hearsay context. Clearly, a hearsay research program that is based in theory is likely to generate hypotheses and experimental designs that produce very different but substantially useful data.

\textsuperscript{126} Petty \& Cacioppo, \textit{supra} note 125, at 11.
\textsuperscript{127} Id.
\textsuperscript{128} Id. at 20.
\textsuperscript{129} Id. at 11, 141-72; see also id. at 204-15 (discussing variables that serve as peripheral cues).